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THE PRIVATE PROVISION OF PUBLIC SERVICES: EVIDENCE FROM RANDOM ASSIGNMENT IN MEDICAID

Danil Agafiev Macambira Michael Geruso Anthony Lollo Chima D. Ndumele Jacob Wallace

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ABSTRACT

This paper examines the effects of privatizing social health insurance. We exploit a natural experiment in Medicaid, wherein nearly 100,000 enrollees were randomly assigned between a publicly-operated fee-for-service system and private managed care. Managed care reduced costs by 5.6% via cost-effective substitutions within prescription drugs and via lower prices for outpatient services. We present evidence that pharmacy utilization management was the key mechanism reducing overuse and encouraging substitution to lower-cost drugs without decreasing quality. In contrast, privatizing medical benefits led to only modest savings and was associated with decreased healthcare quality and consumer satisfaction.

Danil Agafiev Macambira Yale School of Public Health New Haven, CT 06520 danil.agafievmacambira@yale.edu

Michael Geruso University of Texas at Austin Department of Economics 1 University Station C3100 Austin, TX 78712 and NBER mike.geruso@utexas.edu

Anthony Lollo Yale School of Public Health New Haven, CT 06520 anthony.lollo.jr@gmail.com Chima D. Ndumele Yale University chima.ndumele@yale.edu

Jacob Wallace Yale School of Public Health Department of Health Policy and Management 60 College Street New Haven, CT 06520 and NBER jacob.wallace@yale.edu

1 Introduction

Medicaid is the largest health insurer in the United States (US)—serving over 90 million low-income families and individuals with disabilities—and the largest means-tested program in the nation.¹ The scale of the program and vulnerable population it serves have led to unresolved debates about the value of privatizing Medicaid, which now contracts-out the administration of benefits for more than two-thirds of its enrollees to private managed care plans (Kaiser Family Foundation, 2022). The key question in these debates—and a central issue in the economics of outsourcing (Hart, Shleifer and Vishny, 1997)—is whether publicly-funded programs like Medicaid should be administered directly by the government or contracted out to the private sector.

Despite extensive study, it remains unclear whether privatizing Medicaid has reduced public spending (Duggan and Hayford, 2013).² This uncertainty has persisted even as an ever-increasing share of public health insurance in the US is contracted-out to private firms (e.g., to build networks, negotiate provider payments, manage beneficiary enrollment and care, and pay providers). Estimating the impact of privatization—in place of a "fee-for-service" (FFS) program in which the government sets prices and directly pays participating providers—has been hampered by two primary empirical challenges. First, most approaches are subject to selection bias or other endogeneity concerns. When beneficiaries can self-select between public (FFS) and private (managed care) options, differences across those options may reflect patient characteristics rather than performance (Brown et al., 2014). Further, when state mandates have forced enrollees into Medicaid managed care plans a common source of variation used in the literature—these mandates are often accompanied by other policy changes (e.g., Layton et al., 2022) or enacted in response to rising spending (Perez, 2018b), making it difficult to isolate the effect of privatization. Second, data limitations have generally precluded an examination of the mechanisms managed care plans use to control cost (in Medicaid, Medicare, or any other setting). The absence of credible control groups and limited visibility into mechanisms have contributed to a lack of consensus in the literature.

¹In 2021, Medicaid spent \$728.3 billion on healthcare, an amount that exceeded the combined sum of spending on food stamps, the Earned Income Tax Credit, Supplemental Security Income, and cash welfare.

²Findings have been mixed on whether privatization reduces program costs, with some studies showing that managed care achieves significant savings (Marton, Yelowitz and Talbert, 2014; Dranove, Ody and Starc, 2021) while others find it to be cost-neutral or even cost-increasing (Duggan, 2004; Duggan and Hayford, 2013; Layton et al., 2022). Findings have also been mixed as to whether privatization harms the health of enrollees, with some studies documenting severe harms (Aizer, Currie and Moretti, 2007; Kuziemko, Meckel and Rossin-Slater, 2018; Duggan, Garthwaite and Wang, 2021) and others finding either evidence of patient benefit or no evidence of harm (Layton et al., 2022; Dranove, Ody and Starc, 2021).

This paper advances the literature by estimating the causal effect of the private versus public provision of social health insurance (operationalized as managed care versus FFS) in a setting where both models operate contemporaneously. Our study is the first to exploit person-level *randomization* between the two regimes. This natural experiment is set in Louisiana Medicaid, wherein nearly 100,000 enrollees were randomly assigned to either a managed fee-for-service plan ("FFS") in which the state directly reimbursed most providers or a Medicaid managed care plan ("MMC") run by a private insurer. The administrative data we obtain through a partnership with the Medicaid state administrator provide an opportunity to observe plans' use of utilization management via claim denials. This allows us to open the "black box" of managed care, pinpointing the services and populations affected (in the spirit of Brot-Goldberg et al., 2017) and documenting the mechanisms that shape enrollee and provider behaviors, including and beyond network formation, which has been well-studied (e.g., Gruber and McKnight, 2016; Wallace, 2023).

We find evidence that spending is 5.6% lower for enrollees randomly assigned to MMC plans in place of FFS, with mixed effects on healthcare quality. This estimate is less than half of the crosssectional relationship, indicating that, in the absence of randomization, selection bias would lead us to substantially overstate the cost savings of privatization. A formal decomposition reveals that lower negotiated prices account for some of the savings, but their role differs by the type of care. Prices account for most of the reductions in outpatient spending but relatively little of the cost reductions in pharmacy spending. For pharmacy spending, the main driver of overall cost savings is the substitution toward less expensive alternative medicines, consistent with Sacks (2018). Overall, MMC plans generated an average reduction in drug spending of 25%.³ While impacts on health outcomes (e.g., mortality) are difficult to detect and measure for the young and relatively healthy Medicaid population, we catalog potential tradeoffs by examining a wide-range of utilization measures associated with high- and low-value care. Despite the large reductions in pharmacy spending, we find evidence of *increases* in the use of drugs from some therapeutic classes—including diabetes medication, a high-value class. For medical care, on the other hand, we find evidence of reductions in primary care and preventative services that may have shifted enrollees to other settings; avoidable emergency department use was 14% higher for enrollees assigned to managed care relative to those

³The magnitude of our finding is similar to the 21.3% reduction in pharmacy spending reported in Dranove, Ody and Starc (2021), a study focused on Medicaid prescription drug spending that used aggregated data and tracked the impact of carving-in prescription drug benefits to MMC following a change generated by the Affordable Care Act (ACA).

assigned to the FFS option.

Our first set of empirical results helps to reconcile disparate and seemingly conflicting evidence in the prior literature evaluating the privatization of Medicaid. Our study, which contrasts managed care's impact on cost and patient well-being across the medical and pharmacy domains, is consistent *both* with evidence showing that managed care plans efficiently manage prescription drug benefits (Layton et al., 2022; Dranove, Ody and Starc, 2021), and simultaneously with evidence showing that managed care can lead to a reduction in quality or patient well-being that might be due to the management of non-pharmacy benefits (Aizer, Currie and Moretti, 2007; Currie and Fahr, 2005), which account for more than two-thirds of healthcare utilization. Our results suggest that private managed care plans may have *sharp tools* for managing pharmacy benefits—where they are able to reduce spending without harming access—but their effectiveness at managing medical benefits is less clear in our setting. For medical care, we observe reductions in quality and more modest cost savings.⁴

We also present new evidence that enrollee satisfaction is lower in managed care. An advantage of the administrative data we use is that it enables us to construct revealed preference measures of enrollees' satisfaction with plan quality.⁵ We find that enrollees auto-assigned to managed plans care were nearly three times as likely to switch out of their plans as those assigned to the FFS option. Despite the large difference in disenrollment rates, back of the envelope calculations comparing the savings-disenrollment elasticity (implied by enrollee behavior estimated here) to the range of premium-enrollment elasticities estimated in the literature suggest that this savings-satisfaction trade-off *could* be efficient—in the usual sense that enrollees would choose the less expensive, less desirable private option if they were the claimants on the savings.

Another important contribution of this paper is to identify the primary mechanism through which managed care plans achieve savings in our setting: real-time adjudication of pharmacy claims. In real-time adjudication, which occurs prior to service provision, if a claim is denied, then no payment is made to the pharmacy and the enrollee does not receive the prescription.⁶ Following a change

⁴We detect no impacts on inpatient spending, which may reflect the narrower scope for reducing cost via managed care among a young and healthy population that is rarely hospitalized (Duggan, Gruber and Vabson, 2018; Curto et al., 2019; Brot-Goldberg et al., 2024).

⁵In particular, we observe whether auto-assigned enrollees chose to disenroll from their plans of assignment (as allowed by the program rules), switching to another plan. Importantly, disenrollment can reveal preferences even among those enrollees with no detectable clinically-adverse outcomes and in a context where there are no consumer-facing prices.

⁶This differs from denials for a medical claim: Enrollees have generally already been treated at the point that a medical claim is submitted from a healthcare provider to an insurer. Real-time pharmacy claim adjudication is an old concept, which began in 1988 with the creation and publication of the National Council for Prescription Drug Programs (NCPDP) Telecommunication Standard Version 1.0. Today, nearly all pharmacy claims are adjudicated in real-time (National Council

in which managed care plans assumed responsibility for drug coverage,⁷ we show that the share of pharmacy claims denied rises sharply for enrollees assigned to the MMC plans but trends smoothly for those assigned to FFS. The extent of the savings across various therapeutic classes of drugs corresponds to the denial rates for those classes, and savings are primarily achieved by forcing substitutions to cost-effective alternatives, rather than generating outright reductions in the net number of prescriptions filled. We provide evidence against other potential mechanisms driving the pharmacy savings, including differences between MMC and FFS in provider networks, steering to providers, and negotiated prices with pharmacies.

We supplement our first strategy (randomization of enrollees across MMC and FFS plans) with a second identification strategy that exploits an entirely different source of variation in exposure to private managed care within the same state Medicaid program. In 2015, three years after the initial policy change and randomization, Louisiana Medicaid discontinued its FFS program. Consequently, a single plan was forced to transition from facilitating care on a FFS basis (where the state is atrisk) to acting as a full risk-bearing managed care provider. Thus the plan's incentives and structure changed, but the plan's ownership and enrollees did not. This second, complementary natural experiment rules out a potential external validity concern with our primary analyses based on person-level randomization. In the primary analysis, enrollees became part of our randomization sample only if they did not make an active health plan choice, so they may not be representative of the broader Medicaid population. The treatment group for our second identification strategy is composed of all enrollees in a single plan (a sample including enrollees that made active choices and those who were auto-assigned). Despite the different samples, time frame, and identifying variation, the findings from this plan transition are strikingly similar to the findings from randomized auto-assignment, suggesting that our estimates generalize beyond auto-assignees.

Finally, we evaluate whether pharmacy utilization management, which accounts for most of the spending reduction in managed care, also explains why consumers are less satisfied with, and more likely to switch out of, managed care plans. The delayed "carve-in" of prescription drugs, when drugs became the responsibility of the managed care plans, provides an opportunity to identify

for Prescription Drug Programs, 2009).

⁷Neither the FFS or MMC plans initially covered prescription drugs; these were carved-out and paid directly by the state. But nine months after the 2012 plan-assignment shock, a second policy change occurred: Drugs were carved into the financial responsibility of MMC plans. This secondary shock allow us to separately identify the effects of managed care operating through medical versus pharmacy management.

this. We show that the bulk of the reduction in spending generated by managed care doesn't materialize until after the prescription drug carve-in—when the plans could begin to employ real-time adjudication—but, that a large and statistically significant difference in disenrollment rates between MMC and FFS emerges prior to the pharmacy carve-in. This is important because it suggests that dissatisfaction with managed care may be linked to the management of medical benefits—where managed care achieves more modest spending reductions—or to other aspects of MMC provision that are unrelated to prescription drug management. The implication of these findings for efficient outsourcing stands in contrast to the history of Medicaid privatization, which, despite many differences across state programs, was generally characterized by the privatization of medical benefits prior to drug benefits.

Our findings contribute to several literatures concerned with healthcare and public-service contracting. First, we contribute to a strand of research on the public versus private provision of governmentsponsored healthcare in Medicare (Cabral, Geruso and Mahoney, 2018; Curto et al., 2021; Duggan, Starc and Vabson, 2016; Brot-Goldberg et al., 2024) and Medicaid (Dranove, Ody and Starc, 2021; Duggan, Garthwaite and Wang, 2021). This trend towards managed care contracting for publicly subsidized healthcare benefits is cited as one the most important changes in the US healthcare economy over the last several decades (Gruber, 2017). Recent work in the context of Medicare (e.g., Abaluck et al., 2021) and Medicaid (e.g., Geruso, Layton and Wallace, 2023; Garthwaite and Notowidigdo, 2019) has attempted to evaluate the causal impacts of competing managed care plans in a publiclyfunded, managed competition setting that did not include a FFS option for comparison. Other work has assessed tradeoffs using a limited set of measures (i.e., hospitalizations, as in Van Parys, 2017) or for targeted populations (i.e., pregnant women, as in Aizer, Currie and Moretti, 2007). Our work is most closely aligned with studies like Curto et al. (2019) and Duggan, Gruber and Vabson (2018) that have, in the context of Medicare, documented broad differences in spending patterns between Medicare FFS and Medicare Advantage by controlling for observables and exploiting plan exit, respectively. A unique advantage of our setting relative to that of the most-closely related prior work is the clean identification of causal effects via randomization here.

Second, a separate and important contribution of this study is to provide new insight into the mechanisms managed care plans use to achieve savings. In particular, we focus on a mechanism that has not been previously highlighted: Plans' capacity to affect care provision through the real-time

adjudication of pharmacy claims. Whereas medical claims are denied after care is provided, creating administrative burdens for providers without *directly* impacting care (Dunn et al., 2021), real-time adjudication in pharmacy allows plans to precisely target and interdict healthcare consumption immediately before the care would have otherwise been dispensed.⁸ While the prior literature has focused on the administrative burdens caused by the use of utilization management via claims denials (e.g., Casalino et al., 2009; Cutler and Ly, 2011; Gottlieb, Shapiro and Dunn, 2018), we show it may also be a powerful tool, in some contexts, to constrain spending. This echoes contemporaneous evidence from Brot-Goldberg et al. (2023) on the effectiveness of authorization restrictions for reducing drug spending within the elderly Medicare population. We also provide new insight into how public- and private-sector actors differ in their use of these tools. In principle, the public program could target does. In practice, however, pharmacy denials in the public program—which we show target different drugs than the managed care plans—appear to be driven more by a bureaucratic process centered on documenting medical necessity, rather than targeting cost-saving substitutions.

Third, much of the economic and policy analysis of contracting-out publicly funded health insurance has focused on designing or evaluating "high-powered" contracts that create the right incentives for firms to constrain costs (Laffont and Tirole, 1993). The prior literature on managed care outsourcing has tended to focus primarily on incentives, including the difficulty of incomplete contracting on quality (e.g., Duggan, Garthwaite and Wang, 2021; Knutsson and Tyrefors, 2021). Our results suggest that the contracting problem may also be viewed from a different perspective. In particular, strong incentives—in our context, capitation contracts, in which the plans are residual claimants on the savings they produce—may be necessary but not sufficient for generating healthcare spending reductions (or other desired outcomes). Our results, which show larger utilization impacts of managed care on pharmacy than on other services and which identify pharmacy denials as the key mechanism, suggest that plans' ability to adjudicate and deny pharmacy claims in real time may play a special role in this differential impact. This nuance somewhat contrasts with the traditional focus in the healthcare privatization literature, which has largely been on firm incentives, with less attention given to the availability of effective mechanisms to constrain costs.

⁸Retail pharmacy claims are near-universally adjudicated in real-time. In contrast, almost no medical claims are, suggesting a differential capacity constraint faced by plans. See Orszag and Rekhi (2020) for a useful summary of this pharmacy versus non-pharmacy difference in real-time adjudication.

The rest of the article proceeds as follows. Section 2 describes our empirical setting and data. Section 3 presents our empirical framework. Section 4 presents our first main estimates of the effects of assignment to MMC vs. FFS on spending and patient well-being, based on random assignment of enrollees to plans. Section 5 presents estimates from a second identification strategy, the transition of a plan from FFS to MMC. Section 6 shows that utilization management is an important channel through which managed care plans generate spending reductions. Section 7 concludes.

2 Data and Setting

2.1 The Medicaid Program

The Medicaid program, an entitlement created in 1965, has become the largest single insurer in the United States (Centers for Medicare and Medicaid Services, 2022). The third largest mandatory program in the federal budget, Medicaid provided primary or supplemental insurance coverage for over one-fourth of the US population in fiscal year 2019 and accounts for close to 7% and 30% of federal and state spending, respectively (Kaiser Family Foundation, 2021). Medicaid coverage is tradition-ally provided to low-income and disabled populations free at the point of service.

2.2 Medicaid Managed Care

For most of the program's existence, Medicaid has been administered as a "fee-for-service" program in which state governments contract directly with a set of physicians and hospitals willing to accept their reimbursement rates. As enrollment and spending in the program has grown, states have shifted to managed competition among private health plans as the dominant policy choice for the provision of services in Medicaid (Gruber, 2017). States have discretion as to which services and populations they shift to managed care provision and in what order. Historically, states have tended to contract the provision of medical (non-drug) benefits to MMC plans first, leaving prescription drugs under public provision. Prior to the Affordable Care Act (ACA), Medicaid programs were legally entitled to large rebates from drug manufacturers, but these rebate rules did not apply if drug benefits were administered by private managed care plans (Dranove, Ody and Starc, 2021). The ACA mandated identical discounts for medications dispensed to enrollees in managed care plans. Many states privatized prescription drug benefits following the passage of the ACA. However, several states, including New York and California, have recently returned to the direct public provision of prescription drugs in Medicaid or are planning to do so (Gifford et al., 2020).

The most commonly stated motivation for transitioning away from state-managed FFS towards privately administered managed care organizations is the potential for cost savings. Cost savings was the explicit goal of Louisiana's transition to managed care (Hood, 2011). The potential for competition between private plans to better satisfy heterogeneous consumer preferences is also sometimes described as a motivating factor in the trend toward managed care (Gruber, 2017). So, too, is the potential benefit in terms of budget predictability that could arise by shifting spending risk from states to private insurance carriers—though the evidence for this latter channel is mixed (Perez, 2018*a*).

One channel by which privatization is intended to generate savings is via the high-powered contracts that make private insurers residual claimants on any cost savings they can generate (Laffont and Tirole, 1993). Moreover, there is a view that private health insurers, which often also operate in the commercial and Medicare sectors, have more experience facilitating access to healthcare services and managing care than state agencies. However, the canonical literature shows that outsourcing may also have an adverse effect on quality (Shleifer, 1998), particularly in the healthcare sector where many components of quality are not verifiable or contractible (Bergman et al., 2016; Knutsson and Tyrefors, 2021), consumer choice suffers from information frictions or hassle costs (e.g., Handel and Kolstad, 2015; Abaluck et al., 2021), and investment in selection and screening by insurers may be a viable strategic alternative to investment in quality (e.g., Geruso, Layton and Prinz, 2019).

Relative to a public FFS program, managed care plans may generate productive efficiencies through several mechanisms: (i) price negotiations with providers; (ii) the selection and organization of providers (e.g., excluding inefficient providers); or (iii) utilization and care management, in various forms (Glied, 2000). For example, managed care plans may facilitate more timely access to care through the use of innovative care management practices (e.g., AI-targeted case management) or by relying on prior authorization to steer enrollees to more efficient services or medications. While some studies have investigated whether privatization has lead to greater efficiency in Medicaid overall, to date the effectiveness of the particular tools used by managed care plans has been less well studied.

2.3 Public vs. private provision of Medicaid in Louisiana

Louisiana, the setting for our study, operates a Medicaid managed care program that is similar to other Medicaid programs around the country and bears resemblance to both Medicare Advantage and private insurance markets. As in the broader United States, Medicaid is the dominant single insurer in Louisiana, covering approximately one-fourth of state residents in 2016. Louisiana, with a greater proportion of its residents living in poverty, relies more heavily on the Medicaid program than other states. Louisiana now relies largely on private MMC plans to deliver Medicaid benefits to enrollees.⁹

During our study period (i.e., 2010-2016), Louisiana was in the midst of its transition from FFS to MMC. Rather than shifting all enrollees into full-risk, managed care plans, the state offered enrollees two options: (1) a full-risk Medicaid managed care plan ("MMC"); or (2) a managed FFS plan ("FFS") akin to primary care case management (PCCM). There were three full-risk MMC plans and two FFS plans.

We conceptualize the FFS option as the "public" option because the state was bearing health spending risk, deciding on covered services, and, for most covered services, determining eligible providers and setting contracting terms with those providers. The one exception was the determination of the primary care network. The state paid FFS plans a small fee (\$11 per member per month) for maintaining a primary care network and minimally managing primary care.¹⁰ Services other than primary care were provided via the state's legacy FFS network—i.e., the set of providers willing to accept Medicaid enrollees at the FFS payment rates. Payment for services (including primary care physician services) were paid directly by the state on a FFS basis. For the FFS enrollees, the state remained responsible for both the expected cost of treatment and the spending risk for the FFS population. These FFS payments from the state directly to providers accounted for 87% of the annual spending for enrollees in the managed FFS model.

In contrast, the MMC plans received a prospective, monthly risk-adjusted capitation payment (averaging \$263 per member per month) to cover a wide range of contracted services for their Medicaid enrollees (Louisiana Department of Health and Hospitals, 2015). This monthly payment was

⁹See Appendix A for additional detail on the history of the Louisiana Medicaid programs and its use of Medicaid managed care.

¹⁰These figures are for calendar year 2013, a year in the middle of our sample period. The primary care case management fees were in addition to shared savings payouts, which amounted to \$0.61 and \$3.00 per member per month, respectively, for the two managed FFS plans.

similar to other states' capitation payment amounts (averaging \$246 per member per month) for comparable populations around this time period (Centers for Medicare and Medicaid Services, 2016). Louisiana uses competitive bidding to select a limited number of plans with the goal of maximizing the benefits of competition and ensuring enough market share for risk pooling. Risk bearing and provider contracting (including network formation) are the important features distinguishing the managed care option here as the "private" plan—though as in other settings, like in the market for individual health ACA-compliant plans or Medicare Advantage, private managed care plans in our setting were heavily regulated.

Each of the three MMC plans was administered by a for-profit insurance carrier: Amerigroup, Amerihealth, or Louisiana Health Connections. All three plans operated statewide and were subject to uniform benefit designs regarding enrollee-facing prices, with nearly all cost-sharing set to zero. Each plan used a different pharmacy benefits manager. Plans could in principle differentiate themselves according to their network of providers, customer service, and utilization management techniques. Although our main analysis considers the block of these three plans versus the two FFS plans, some analysis that follows examines each plan individually.

An important feature of our setting is that prescription drugs were "carved-in" to managed care financial responsibility starting in November 2012. Prior to this, the state paid for prescription drugs on a fee-for-service basis for enrollees in both the MMC and managed FFS plans. We use this event as an additional source of identifying variation below.

2.4 Auto-assignment policy

Our study focuses on the first region to transition to managed care, eastern Louisiana, which contains New Orleans. The transition for this region occurred in February 2012. The enrollees in this region received notification via mail in December 2011 of the upcoming transition and were given the opportunity to select one of five plans (i.e., the two FFS and three MMC plans) within 30 days.¹¹ However, if enrollees had not selected a plan within 30 days of being notified of the transition, they were automatically assigned ("auto-assigned") to one of the five plans—and, hence, to either MMC

¹¹The state and its contractors made reminder calls to encourage those who had not selected a plan to make their choices, and there were several ways to enroll. According to the Louisiana Department of Health, enrollees could call 1-855-BAYOU-48 and have an enrollment specialist assist them in choosing a plan, they could follow automated phone cues to select a plan, they could enroll online, or they could complete and mail the forms back in the envelope provided in their Enrollment Kits. See Appendix A.2 for additional details on the roll out of Medicaid managed care and the timeline for auto-assignment.

or managed FFS. Most enrollees (68.9%) were auto-assigned.¹²

The key for our study design is that many of these auto-assignments were random. For enrollees with family members in Medicaid, the state prioritized keeping those family members together. Specifically, auto-assignees whose family members had chosen plans were assigned to the plan of that family member, even if they themselves did not choose a plan. We removed all of these non-random auto-assignments from our sample.¹³ The auto-assignment algorithm was also designed to assign enrollees to a plan that contracted with their prior primary care provider, based on their utilization in Medicaid FFS prior to the transition. Hence, randomization probabilities differed across plans based on the network of providers each plan covered.¹⁴ In all analyses we control for this unit of randomization (i.e., enrollee's primary care provider prior to assignment) to preserve the conditional randomization, and we cluster at the same level to account for potential correlation between enrollees with the same prior primary care provider.

Lastly, there was imperfect compliance with auto-assignment because Medicaid enrollees could switch plans without cause within 90 days of being assigned to a health plan. This is evident in the sharp decline in compliance with assignment during the first three months in Figure 1. After this 90-day period ended, enrollees could only switch plans for "good cause" unless they waited until the next annual open enrollment, illustrated by the year-long plateaus in Figure 1. As an instrument for enrollment, auto-assignment is strong: Despite having ample opportunity to switch, compliance with plan assignment was very high. In our study, 90.2% of enrollee \times year observations are observations in which auto-assignees remained in their assigned coverage model (managed FFS or MMC) throughout the entire study period (i.e., the 35 months we observe them post-assignment). The minority of non-compliers generate a useful revealed preference measure of plan satisfaction, exploited below.

2.5 Primary sample

We construct our "auto-assignment sample" with the following restrictions. First, we limit our sample to enrollees whose eligibility categories were mandated to transition to either the MMC or man-

¹²This is calculated specifically for the study sample after all sample exclusions; e.g., 94,976 divided by 137,937, respective sample sizes of the auto-assignee and overall (auto-assignees plus active-choosers) samples.

¹³This exclusion removed 4% of auto-assignments before arriving at the final sample.

¹⁴Our dataset includes an indicator for enrollees' prior primary care provider. The vast majority of Medicaid enrollees (92%) were linked to a primary care provider.

aged FFS model. We do this because several categories of Medicaid eligibility were excluded from the transition (e.g., nursing home residents).¹⁵ Second, we exclude members who are older than 65 years of age at any point during our study period. As Medicaid is the payer of last resort, it is possible we would not observe all healthcare claims for these "dual-eligible" enrollees whose primary payer would be Medicare.¹⁶ Third, for our primary analyses, we also restrict to a balanced panel of enrollees continuously enrolled for approximately three years post-assignment (2012-2014). Although there is churn in the Medicaid program, we see no evidence of differential attrition between those assigned to MMC and managed FFS plans (Figure A1). These sample restrictions leave us with 94,976 unique enrollees. In some instances, we make comparisons to the broader Louisiana Medicaid population in the region, which includes an additional 42,961 enrollees who were not auto-assigned to a plan by virtue of making an active choice (i.e., "active choosers").

2.6 Administrative data and outcomes

To estimate the impact of managed care, we use detailed administrative data obtained from the Louisiana Department of Health (LDH). To facilitate a comparison of our results to those of prior studies examining the effects of demand-side (e.g., Manning et al., 1987; Brot-Goldberg et al., 2017) and supply-side incentives (e.g., Gruber and McKnight, 2016; Curto et al., 2019; Geruso, Layton and Wallace, 2023; Dunn et al., 2021) in healthcare, we focus analysis on the outcomes examined in those studies. The outcomes fall into three broad domains: healthcare use and spending, healthcare quality, and patient satisfaction. To better understand mechanisms, we additionally examine plans' utilization management strategies (i.e., prior authorization, step therapy, and quantity limits), for which we observe a novel proxy via claims denials.

Healthcare use and spending. We measure healthcare spending using administrative claims data provided by LDH. When measuring healthcare use and spending, we include the full set of Medicaid covered services, including those paid for by the Medicaid managed care plans as well as any additional "carved-out" services paid for by fee-for-service Medicaid. Our Medicaid managed care administrative data include the prices paid to providers, allowing us to observe whether our effects

¹⁵Enrollees eligible for specialty Medicaid programs and home and community-based waiver enrollees were also excluded from the transition. Hence, to be conservative, we also excluded any enrollees who had a home health claim within the year prior to February 1st 2012.

¹⁶We exclude enrollees whose prior providers covered fewer than 20 enrollees after all of the other exclusions. We do this because the fixed effects for these providers would be noisily estimated. However, sensitivity analyses indicate that our qualitative findings are robust to this exclusion.

are driven by price or quantity. The interpretation of transaction prices in the context of prescription drugs is complicated by the presence of rebates; we discuss this issue in Section 4.2.

Prior to assignment or plan choice, enrollees are covered by the publicly-operated, Medicaid feefor-service program, which allows us to observe their baseline healthcare use and spending. This enables powerful balance tests and allows us to construct a measure of enrollee health risk (uncontaminated by plan effects) using a cross-validated, LASSO regression that takes as inputs enrollee demographics, diagnoses, and spending *at baseline* to predict healthcare spending post-assignment (Appendix Section B.2). We use broad service categories provided by the LDH to disaggregate spending by type of service.

Healthcare quality. We measure healthcare quality using our administrative claims data. We construct measures of quality and access included in the Healthcare Effectiveness Data and Information Set (HEDIS) Core Set. These measures are commonly used to evaluate managed care plans in Medicaid and encompass a wide range of services including preventive care, primary care access, maternal and perinatal health, care of acute and chronic conditions, and behavioral health care.¹⁷ To mitigate concerns about multiple comparisons, we also aggregate the measures into a composite. In addition, we construct potentially high-value and low-value services identified in the literature (Wilkins, Gee and Campbell, 2012; Schwartz et al., 2014; Brot-Goldberg et al., 2017; Starc and Town, 2019). For example, we include as a high-value care measure an indicator for whether enrollees fill a prescription for diabetes medications.¹⁸ For low-value care, we assess the likelihood an enrollee uses the emergency department for avoidable reasons (Medi-Cal Managed Care Division, 2012) and create a composite "any low-value care" from several measures relevant to the Medicaid population (Charlesworth et al., 2016). The full set of quality measures are described in detail in Appendix Section B.

Consumer satisfaction. The final outcome we study is enrollee satisfaction, measured by whether or not an enrollee stays in their assigned plan (Wallace, 2023; Geruso, Layton and Wallace, 2023). Following the literature, we term this measure "willingness-to-stay" and assume that enrollees' preferences are revealed through their choices to switch plans. (The traditional willingness-to-pay measure is not defined here, because there are no premiums in Medicaid.)

¹⁷We further restrict to the subset of measures that can be constructed from administrative data and that have look-back periods of at most 2 years so that we can reliably construct them for each year in our study period. These measures include: child and adolescent annual well-child visits; child access to primary care; chlamydia screening in sexually active women; cervical cancer screening in women; and follow-up care for children prescribed ADHD medication.

¹⁸We use the The Anatomical Therapeutic Chemical (ATC) Classification System to identify asthma and diabetes medication prescriptions (WHO Collaborating Centre for Drug Statistics Methodology, 2005).

Healthcare claims denials. The claims data include all fully adjudicated paid claims as well as information on claims denials. As such, we can use a simple binary variable to define a claim as being "denied" if it was not paid by the healthcare plan. Generally, pharmacy denials differ from medical denials in at least one important respect: pharmacy claims are subject to real-time adjudication. In real-time adjudication, which occurs *prior to* service provision, if a claim is denied, then no payment is made to the pharmacy and the enrollee does not receive the prescription. This differs from denials for a medical claim: Enrollees have generally already been treated at the point that a medical claim is submitted from a healthcare provider to an insurer. Thus, pharmacy denials provide a unique opportunity for an insurer to interdict service provision. Real-time adjudication is commonly used to deny a prescription when an enrollee has not obtained prior authorization from their health plan.¹⁹ See Appendix E for more information on how we measure and analyze administrative claims denials.

Table 1 contains summary statistics. Our primary sample of randomly assigned enrollees contains 94,976 unique enrollees and 284,928 enrollee-years during the period 2012-2014. Typical of Medicaid, the sample is young, with an average age of 9.4 years old. On average, enrollees spent \$1,451 annually on health care. The largest share of spending was for outpatient care (\$590 annually), which was followed by pharmacy spending (\$381 annually).²⁰

3 Research Design

3.1 Econometric model

The main empirical goal of this paper is to estimate the causal effect of enrollment in a private, full-risk managed care plan, as opposed to enrollment in a FFS plan, on outcomes like healthcare spending and enrollee satisfaction. The key challenge historically to identifying these effects is the potential endogeneity arising from selection on unobservables across beneficiaries choosing to enroll in MMC versus FFS. Our main empirical approach leverages the random variation generated by auto-assignment of people to MMC and FFS plans, as discussed in Section 2.4. (A complementary research design, for which we defer detailed discussion to Section 5, uses an entirely separate natural

¹⁹In addition to lacking prior authorization, prescription drug claims may be denied for administrative reasons (e.g., there is a clerical error on the submitted claim, duplicate claims were submitted, etc.), when enrollees exceed plan-set quantity limits, or if a given prescription drug is not included on a plan's formulary meaning it is not covered at all.

²⁰The characteristics of the enrollees in our primary, auto-assignee sample, were similar to those of enrollees that made active plan choices. Table A1 presents summary statistics for the active choosers and full Medicaid population.

experiment in which one health plan was forced by the state to switch from the FFS to MMC coverage model, while its enrollees largely stayed put).

In the main approach, we instrument for *enrollment* in managed care with *assignment* to one of the three managed care plans. Specifically, we estimate the causal impact of managed care via two-stage least squares (2SLS) in which the first-stage takes the form:

$$ManagedCare_{it} = \gamma + \pi AssignedManagedCare_i + \phi_{p(i)} + \nu X_i + \mu_{it}, \tag{1}$$

where *AssignedManagedCare_i* is an indicator variable set to one if the auto-assignment algorithm assigned enrollee *i* to a full-risk, managed care plan at the time of the program transition in February 2012 and zero otherwise. The coefficient π captures the first-stage effect of (one-time) auto assignment to managed care on enrollment in managed care in the observation period. In our primary specification, we aggregate data up to the enrollee-year level, so that the time subscript *t* indicates years, and the dependent variable *ManagedCare_{it}* is an indicator for whether the enrollee spent the majority of the year enrolled in managed care. In other specifications, we disaggregate the time dimension to quarters or months.

Because the auto-assignment algorithm was designed to assign enrollees to a plan that contracted with their prior primary care provider (superscripted p), we include fixed effects for each enrollee's provider prior to assignment ($\phi_{p(i)}$) to preserve the structure of the conditional randomization. Intuitively, our identification comes from comparing the outcomes of enrollees with the same pre-assignment provider who are randomly assigned to different coverage models. Enrollees assigned to managed care may choose to disenroll from managed care after assignment, switching to a FFS plan. Imperfect compliance with assignment ($\pi < 1$), the motivation for our use of 2SLS, also provides an opportunity to measure enrollee satisfaction.

To estimate the impact of MMC enrollment on spending and other outcomes Y_{it} , we estimate models of the form:

$$Y_{it} = \alpha + \beta ManagedCare_{it} + \phi_{p(i)} + \delta X_i + \eta_{it},$$
(2)

where $ManagedCare_{it}$ is predicted from Equation 1, and β recovers the causal effect of managed care enrollment relative to FFS on the outcomes of interest. To account for any correlation within randomization cohorts, we cluster standard errors by enrollees' pre-assignment providers. The primary estimation sample includes observations over the entire post-assignment period, 2012–2014, though for some specifications, we estimate results for 2012, 2013, and 2014 in separate regressions.

Equation 2 is estimated over only the post-transition period, after individuals were randomly assigned to either MMC or FFS plans. Cross-sectional comparisons of the outcomes between treatment and control individuals after assignment is straightforward to interpret and unbiased given the conditional random assignment, but they do not fully exploit the panel nature of the data. Therefore, to visualize how our treatment effects evolve over time, we also estimate regressions that exploit the same fundamental variation but are operationalized as reduced-form event-study differencesin-differences regressions. These include observations prior to random assignment. These models flexibly allow for impacts to evolve over the post period, with pre-period "effects" serving as falsification tests:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{\tau \neq -1} \beta_\tau Assigned Managed Care_i + v_{it}.$$
(3)

In these regressions, the β_{τ} coefficients capture the effect of being assigned to managed care in each period τ . Event time $\tau = 0$ corresponds to the first post period, beginning February 2012. Estimates of β_{τ} for $\tau < -1$ provide opportunities for the data to reveal problematic differences in the baseline levels or pre-trends of characteristics between the individuals (eventually) assigned to MMC versus (eventually) assigned to FFS. The units of τ are either months, quarters, or years, as indicated in the event study figures. Because we observe the same enrollees over time as they move from public FFS to either managed care or privately-administered FFS, we can include an individual fixed effect α_i . Fixed effects for time periods, λ_i —which variously represent month, quarter, or year fixed effects, as indicated in results tables—are also included.

3.2 Identifying Assumptions and First Stage Results

Figure 1 shows that assignment to Medicaid managed care is a strong instrument for enrollment in Medicaid managed care. This figure plots the probability that an individual is enrolled in MMC as a function of their assignment in February 2012. Prior to this date, there was no managed care option, and enrollment was zero for all groups. Immediately at February 2012, enrollment for MMC-auto-assignees rises to nearly 100%, and over the entire 2012–2014 post-assignment period, 90.2% of enrollee × year observations have enrollees in their assigned coverage model. Pooling across 2012–2014, the first-stage coefficient (π) from Equation 1 is 0.76 (std. err. = 0.03), with a first-stage F-statistic

of 678 (*p* < 0.001). (See Table A2.)

The exclusion restriction here is that assignment to Medicaid managed care only impacts enrollee outcomes through its effect on enrollment in Medicaid managed care. The assumption is natural in this setting, but a violation would occur if assignment were correlated with unobservable enrollee characteristics that affected the outcomes we study, leading our estimates of β (or β_{τ}) to be biased. Table 2 presents *p*-values from a series of balance tests on baseline enrollee characteristics. Each row presents the result of a bivariate regression in which the baseline characteristic is the dependent variable, and an indicator for whether the enrollee was assigned to managed care is the regressor, with fixed effects for enrollees' prior primary care provider (the unit of randomization). Only one out of 18 baseline characteristics indicates statistically significant imbalance. Table 2 also presents a test of joint significance (in which the indicator for assignment to managed care is regressed on the full set of baseline characteristics), which fails to reject the null that all characteristics are jointly zero. On the other hand, the baseline characteristics for a sample of enrollees that made active choices are *highly imbalanced* between enrollees that chose MMC and FFS plans (Table A3), underscoring the importance of our reliance on quasi-experimental variation to identify the causal effects of managed care.²¹

Monotonicity, a third key assumption in any instrumental variable (IV) analysis, cannot be tested. However, Angrist, Imbens and Rubin (1996) demonstrate that the bias introduced by violations of monotonocity decreases in the strength of the first stage. Hence, given the strength of our first stage, any violations of monotonicity in our setting would introduce minimal bias.

3.3 External Validity

Our sample is younger than a modern Medicaid population because our study period, which centers on 2012, predates Louisiana's ACA expansion of Medicaid coverage to more adults in 2016. Appendix C.2 provides a detailed comparison of the age distribution pre- and post-expansion and shows how the post-ACA population differs, with an average age of 20.5 years compared to 9.4 years in our sample. This is important context for the results that follow, including with respect to interpreting null effects of managed care on utilization in subcategories of spending for which baseline utilization is low in our population. We consider this where we describe the estimates for inpatient

²¹The imbalance among the smaller sample of enrollees that made active choices also suggests the lack of balance in our auto-assignee sample does not reflect a lack of statistical power.

utilization, which is a small spending category in our setting.

Nonetheless, even after the 2016 adult expansion in Louisiana, children still comprised more than 50% of non-disabled, non-dually eligible Medicaid enrollees there, and children comprise more than 45% of this Medicaid enrollment nationally today. Thus, the main results and conclusions of the auto-assignee sample remain relevant to the Medicaid program.

One potential concern for external validity is that auto-assignees may be healthier and lessengaged with the healthcare system. While the auto-assigned population spends approximately \$370 less annually than the active chooser population (Table A1), the distribution of spending across components of care is nearly identical across the two populations. For both samples, about 40% of overall spending comes from outpatient care and 5-7% from inpatient care, with small differences for the other components of care. Further, there are only minimal differences between the two populations with respect to potentially high-value prescription drug utilization and receipt of low-value care. Lastly, we note that the auto-assignees are not a small subset of the Medicaid population in Louisiana; more than two-thirds of the enrollees in the state were auto-assigned. Thus, the estimated local average treatment effects (LATEs) we present are likely to be similar to average treatment effects (ATEs) for this population.²²

As described in detail in Section 5, we also use a second, complementary research design that exploits a separate natural experiment in which one health plan was forced by the state to switch from the FFS to MMC payment model, while its enrollees largely remained enrolled in the plan. This second strategy allows us to address subtle issues of interpretation and external validity that person-based randomization would not be able to confront.

4 Results

4.1 Healthcare Use and Spending

Before reporting our main IV estimates of the impact of MMC enrollment on healthcare spending, we begin in Figure 2 with reduced-form difference-in-differences results. The figure is useful both as an additional opportunity to falsify the identifying assumptions (via a test for parallel pre-trends)

²²For transparency, we also present estimates from OLS regressions of the effects of enrollment in MMC (relative to FFS) using the broader Louisiana Medicaid population. We rely on baseline characteristics we can construct in our administrative data (e.g., healthcare use and predicted spending) to adjust for potential, enrollee-level confounders. The (biased) OLS results based on the broader Medicaid population are larger and, as expected, more sensitive to controls.

and as a clear visual summary of how impacts evolve over the post-assignment period (via separate coefficient estimates for each calendar quarter).

The sample in Figure 2 is a balanced panel of 85,668 recipients continuously enrolled from February 2011 through December 2014. This sample is smaller than the primary sample due to the additional requirement of continuous enrollment for one year prior to assignment. The figure plots the β_t coefficients estimated via Equation 3. Time *t* is at the quarter-year resolution. The omitted interaction is for the quarter prior to assignment (*t* = -1). The leftmost vertical line indicates the start of managed care in February 2012, when auto-assignment took place. The rightmost vertical line indicates the date (November 2012) when the MMC plans became responsible for managing the pharmacy benefit—i.e the pharmacy "carve-in." Prior to the carve-in, FFS Medicaid paid directly for the prescription drugs of MMC enrollees. For further transparency, Figure A2 presents the time series of healthcare spending for the MMC and FFS groups separately, with the data residualized only on calendar quarters and the unit of randomization.

Figure 2 and Figure A2 show no evidence of differential pre-trends (and no evidence of differential levels in the pre-period in Figure A2), consistent with other evidence above that the randomization generated exogenous variation in assignment. Substantively, assignment to managed care is associated with lower spending in the post-assignment period. Figure A3 stratifies these reductions by component of care, illustrating that the outpatient reductions manifest immediately after randomization, while pharmacy reductions do not occur until pharmacy is carved in. These event study results suggest that the reductions in spending associated with managed care are not short-term effects, but rather persist for nearly three years post-assignment. The largest reduction in spending emerges after pharmacy was carved in, which we analyze further below.

Table 3 presents the main results: IV estimates of the impact of MMC enrollment in the postassignment period (2012–2014). The source of identifying variation is the same in this as in Figure 2, but the IV effects are scaled up by the first stage. The scaling is minimal because of the size of the first stage (Figure 1). This IV specification restricts the estimated impact of managed care to be timeand duration-independent, so it can be summarized by a single coefficient.²³

We find an economically and statistically significant reduction in total healthcare spending associated with managed care of roughly \$82 per enrollee per year (std. err. = \$17; Table 3, Row 1,

²³This is analogous to estimating the difference-in-differences specification via a single post×MMC effect, rather than MMC interacted with post-treatment periods.

Column 3).²⁴ This is a 5.6% reduction in spending relative to the auto-assignee sample mean. To put this estimate in context, Brot-Goldberg et al. (2017) find a 14% reduction in spending after enrollees in an employer plan were moved to a high-deductible health plan offered by the same carrier, and Curto et al. (2019) find a 9% difference in utilization between FFS Medicare and private (MCO) Medicare.²⁵ An important contrast is that, in this case, the spending differences emerge without exposing enrollees in the different plan types to differential financial risk in the form of out-of-pocket spending.

For comparison we estimate the same effects using OLS, which reflect both causal plan effects and enrollee selection. The OLS results (Table 3, Column 7) recover differences in healthcare spending that are more than three-fold our causal estimates, consistent with classic adverse selection leading sicker enrollees to sort into the FFS plans. We show below that FFS plans impose fewer hurdles to accessing care, making them plausibly more attractive to worse-health beneficiaries.

In Figure A4, we depart from our main specification and separately estimate this overall spending effect in each of the three MMC plans, relative to each of the two FFS plans. The figure illustrates that the variation in spending effects across payment models (MMC versus FFS) is far larger than the variability of individual plan effects within the same payment model (MMC or FFS).²⁶ The similarity of plan spending effects among the three for-profit MMC plans is consistent with Geruso, Layton and Wallace (2023), which examined plan-level variation in spending effects among managed care plans in New York Medicaid and found similar spending impacts among the for-profit carriers.

Panel A of Table 3 presents our annual spending results by components of care. We find reductions in medical (i.e., non-drug) spending, driven by a reduction of \$19 (std. err. = \$7) in the outpatient setting. Impacts on inpatient spending are economically small and statistically insignificant (+\$3; std. err. = \$5). That result largely reflects that 95% of our population has no prior inpatient utilization, and thus offers little scope to lower spending in this category. This is an important way in which our Medicaid sample, which is predominantly children, is not ideal for extrapolating to an

²⁴Annual total spending is winsorized at \$25,000. In supplemental analyses we estimate the effect of MMC on the raw, unwinsorized level of spending using the same IV and OLS specifications (Table A4). Estimates are similar across the two specifications: For instance, the total spending effect of MMC over the study period is -\$74 (std. err. = \$34), a 4.9% reduction in spending relative to the unwinsorized mean.

²⁵Both of those studies compare plan options with different cost-sharing, although the Curto et al. (2019) analysis was limited to medical spending.

²⁶For total healthcare spending, we test for the equality of spending effects among the two FFS plans and among the three MMC plans. We do not reject equality of effects within the groups of FFS or MMC plans (p = 0.78 and p = 0.48, respectively).

adult or elderly population.²⁷

The largest effects are for pharmacy spending, with managed care leading to a reduction of \$68 (std. err. = \$9), or 18%, in annual pharmacy spending per enrollee. Restricting to the final 2 years of the period, 2013-2014 (Table 3, Columns 5-6), which fall after the pharmacy carve-in, we find that managed care reduces pharmacy spending by \$94 (std. err. = \$10), or 25%. Because the pharmacy effects have the largest contribution to total spending, restricting the estimation of total spending impacts to this post-carve-in period increases the estimated savings from \$82 to \$100. That is despite the fact that the contribution of outpatient estimates to overall savings shrinks over the later window.

The pharmacy estimates are similar in magnitude to the 21.3% reduction in pharmacy spending reported in Dranove, Ody and Starc (2021), who examine how pharmacy spending changes when states shift pharmacy from FFS to managed care, identifying effects by comparing across state Medicaid programs in national data. The correspondence between our result and Dranove, Ody and Starc (2021) is striking, given that ours is identified off a very different natural experiment (randomization within a state) and estimated in individual claims data rather than aggregate state-level reports.

Panels B and C of Table 3 present results stratified by enrollee characteristics and predicted healthcare spending. In every subsample, assignment to managed care was associated with economically and statistically significant reductions in healthcare spending, but we find little evidence of heterogeneity in the treatment effect when stratifying by gender or race. As for predicted spending,²⁸ enrollees in the highest quartile of predicted spending experience total healthcare spending reductions nearly five times larger than those in the lowest quartile. The finding of small impacts for enrollees with low expected spending is consistent with there being little scope to reign-in spending among enrollees who would otherwise use little (or zero) care. Yet, as a percentage of the mean spending within each quartile group, the spending regardless of predicted healthcare spending, inline with the headline result of 5.6%. Finally, we stratify the results by age and baseline inpatient spending in Figure A5. The impact of MMC on overall healthcare spending is similar in magnitude for most age strata except for the oldest group (over 18, 5% of our sample) where estimates are especially imprecise. We find no significant effects on post-assignment inpatient spending for any strata

²⁷Appendix Table A5 provides estimates specifically for children aged 0-18.

²⁸To predict enrollee spending we estimate a cross-validated LASSO regression with mean annual post-assignment healthcare spending (in the 3 years after random assignment) as the outcome and use a set of demographic and baseline utilization measures as predictors. See Appendix Section B.2 for more details.

of pre-assignment inpatient spending. The point estimate on inpatient spending is negative for the subpopulation with the highest pre-randomization inpatient spending, but the estimate is imprecise (-\$102; std. err. = \$148). Because inpatient care is rare in our sample overall, the lack of a precisely estimated effect here is not evidence that managed care would have no inpatient spending effects in populations with greater inpatient care use.

4.2 Pharmacy Use and Spending

Because the largest spending reductions generated by managed care are concentrated in prescription drugs, we next examine the effects of managed care on the quantity of prescriptions filled, days supply, paid amounts per prescription, and spending—overall and separately for brand and generic drugs.

Figure 3 presents reduced-form difference-in-differences versions of these estimates. Panel A demonstrates that managed care does not reduce the quantity of filled prescriptions overall, but instead leads to a shift from brand to generic prescriptions. The effect sizes for brand and generic quantity were identical, but opposite signed, suggesting nearly one-for-one substitution from brand drugs to generics. In principle, reduced drug spending could cause offsetting increases in non-drug spending, if fewer appropriate prescriptions were filled, leading to higher demand for outpatient or inpatient care (Starc and Town, 2019). Because drug savings operate in our setting largely via substitutions to lower-cost alternatives rather than via reductions in the number of prescriptions filled, there is little scope for this channel to exert upward pressure on non-drug spending in our context.²⁹

The finding that filled prescriptions do not decline with the drug carve-in is important, given that a key concern with managed care privatization is the potential loss of access as private plans tighten restrictions in the course of pursuing savings. Taken together, these patterns corresponded to a large, 24% (std. err. = 2.6) decrease in the quantity of brand drug prescriptions and an 11% (std. err. = 2.0) increase in generic drug quantity. The effect sizes differ in percentage terms due to differing baseline levels of generic and brand utilization. While overall prescription drug quantity is unchanged in the long-term, there is a reduction in overall quantity in the first two quarters following the pharmacy "carve-in," evidence of a potential disruption during the period in which managed care

²⁹Appendix F offers a bounding exercise for the potential of such offsets.

plans aggressively deploy real-time utilization management to shift drug consumption. We return to this mechanism in Section 6.

While managed care does not generate reductions in the overall number of prescriptions, Panel B of Figure 3 reveals that managed care does reduce quantity by lowering the days supply per prescription. Here, again, reductions are concentrated among brand drugs, with a decrease of 2.2 days supply per prescription (std. err. = 0.2), or a 9% decline. We present evidence in Section 6 that utilization management (in this case, likely via quantity limits) is the tool managed care plans use to achieve this: Following a claims denial, prescriptions that get filled *for the same drug* tend to have lower days supply than the original denied claim in MMC (but not FFS).³⁰ The MMC plans enforce these quantity limits (e.g., 30 days supply) by denying claims at the pharmacy—before a prescription is dispensed. Hence, managed care plans both shift the composition of drugs from brand to generic and standardize and reduce the days supply per prescription, particularly for brand drugs. Collectively, these effects generate a reduction in brand drug spending of more than 28% that drives the overall reduction in pharmacy spending generated by MMC.

In Panel C, we demonstrate that there is also a reduction in the paid amount per prescription for brand and generic drugs after the carve-in of pharmacy to managed care. The lower paid amounts per prescription in managed care do not reflect lower unit prices paid by the managed care plans (a point we provide empirical evidence for in Section 6). Rather, the lower paid amounts per prescription in managed care reflect reductions in the days supply per prescription for brand drugs (as shown in Panel B) and a shift in the composition of generic drugs towards lower cost therapeutics.³¹

Panel D reveals that the net effects of managed care on pharmacy spending are primarily driven by a significant reduction of \$22 (std. err. = \$3) per enrollee per quarter (or \$88 annually; 26%) in brand drug spending, with no offsetting increase in generic drug spending (-\$1.6; std. err. = \$1.2). The lack of a spending increase for generic drugs despite the increase in the number of prescriptions (and days supply per prescription) is a result of the offsetting decrease in the paid amount per prescription as a result of the compositional shift within generics towards lower-cost therapeutics.

It is important to understand that the transaction prices recorded in the claims data are not in-

³⁰While the point estimates for generic drug days supply post-carve-in are positive, the effect sizes are small and generally statistically insignificant. The causal effect of managed care on the composition of prescription drugs (e.g., Panel B of Figure 3) complicates the interpretation of this conditional-on-generics measure.

 $^{^{31}}$ This finding is reinforced in a more formal decomposition in the style of Brot-Goldberg et al. (2017) presented in Appendix D.

clusive of rebates (which occur ex post and as a lump sum payment). If rebates were identical between FFS and MMC and unchanging over the study period, then the decline in drug spending that we document in Table 3, in percent terms, would be an unbiased estimate of the net-of-rebate decline. If, however, rebates were higher in MMC—for example, because private plans built their utilization management techniques around moving enrollees to drugs with differentially favorable rebates—then the results we document based on claims data would understate the total savings.

In Figure A6, we use a separate, state-level database on aggregate Medicaid rebates in Louisiana to examine how these changed over the time period of the pharmacy carve-in. The figure shows rebates increasing gradually from the pre- to post-period, on the order of 10 percentage points. This result—that transaction-level savings do not appear to be offset by a decline in rebates—is closely consistent with the only other evidence to date on this issue, from Dranove, Ody and Starc (2021).

Because we find some evidence that average rebates in the Medicaid program overall increased after a larger share of the enrollee population was moved to MMC, it implies the drug savings we report in Table 3 may be an underestimate. For example, a back-of-the-envelope calculation indicates that a 10 percentage point larger average rebate in MMC relative to FFS would mean that our observed 25% cost savings would imply rebate-inclusive savings closer to 40%.³²

4.3 Effects on High-value and Low-value Services

We next examine whether the MMC-FFS difference in outpatient spending corresponds to what could plausibly be considered targeted reductions in services where overuse is a concern (i.e., "wasteful" services). An alternative possibility that would be consistent with recent evidence is that MMC savings came from broad-based reductions in both "high" and "low" value services (e.g., Brot-Goldberg et al., 2017; Curto et al., 2019; Geruso, Layton and Wallace, 2023).

Table 4 presents our estimates of the effect of MMC on the use of potentially high- and low-value services, as well as on consumer satisfaction. Panel A focuses on primary and preventive care services

³²Define the drug cost savings of transitioning an enrollee to MMC, in percent terms relative to a FFS baseline, as $\Delta c = \frac{c_{FFS} - c_{MMC}}{c_{FFS}}$, where *c* gives the observed cost in claims data and the *FFS* and *MMC* subscripts denote the plan type. In the presence of rebates that accrue back to the plan but are not reflected in the claims (transaction data) used to estimate *c*, the true underlying cost savings is $\Delta c_* = \frac{c_{FFS}(1 - r_{FFS}) - c_{MMC}(1 - r_{MMC})}{c_{FFS}(1 - r_{FFS})}$ or $1 - \frac{c_{MMC}}{c_{FFS}} \frac{(1 - r_{MMC})}{(1 - r_{FFS})}$, where *r* is the mean drug rebate in the relevant plan type and so 1 - r deflates the observed cost to recover the actual cost. If $r_{MMC} = r_{FFS}$, then it follows that $\Delta c_* = \Delta c$, which means that the observed savings is an unbiased estimate of true savings inclusive of rebates. If $r_{MMC} > r_{FFS}$, then the true savings is larger than the savings observed in claims data by the factor $\frac{1 - r_{MMC}}{1 - r_{FFS}}$. To benchmark this, moving from a rebate of about 40% to about 50% would imply that our approximately 25% drug cost savings (drug spending in the post period equal to 75% of the pre period) would be 38% once accounting for rebates (= $1 - (.75) \frac{(1-5)}{(1-4)}$).

where *underuse* is a concern in Medicaid. Pooling outcomes across the post-assignment study period (2012-2014), column 3 reveals that assignment to MMC (relative to the FFS option) is associated with a reduction of 2.00 percentage points (pp) (std. err. = 0.70) in the likelihood of enrollees receiving recommended annual primary care visits. For other preventative measures we did not find evidence that assignment to a MMC plan led to reductions in utilization, and in the case of annual well-child visits, we can rule out effects larger than 2 percentage points, a 4% effect relative to the population average. For use of any behavioral health services, we observe no significant effect overall (-0.44 pp; std. err. = 0.24), but we do see a potentially concerning reduction in the use of behavioral health services among children (-0.58 pp; std. err. = 0.24; Appendix Table A6). We also examine follow-up care for children prescribed ADHD medication, which is an important category for this study population, and can rule out effects larger than approximately 6 percentage points. In interpreting these results, we caution against placing too much emphasis on individual coefficients, particularly given the multiple hypotheses being tested. Therefore, we rely more heavily on the primary and preventive care composite measure, where we find that MMC is associated with a modest reduction of 0.87 percentage points (std. err. = 0.36), or nearly 2%. The results in Panel B of Table 4, column 3, suggest that enrollees assigned to MMC plans were *more likely* to use diabetes medications, while there was a significant reduction in asthma medications. Figure A8 provides reduced-form difference-in-difference results for quality outcomes, illustrating that the timing of potentially high-value prescription effects coincide with the pharmacy carve-in.

Finally, Panel C of Table 4 presents our estimates of the effect of MMC on the use of potentially low-value services. We find that MMC has a negative, but insignificant effect on the rate of low valuecare defined by our catch-all measure (any low-value care) described in Section 2.6 and Appendix B. Similarly, MMC has a negative, but insignificant effect on overall rates of imaging. In contrast, we find a substantial and statistically significant increase in avoidable emergency department (ED) visits annually, with enrollment in MMC (relative to FFS) leading to a 1.17 percentage point (std. err. = 0.27) increase in enrollees receiving any care for non-emergency conditions in the ED, a 14% increase relative to the mean. Combined with the result that MMC decreases primary care visits, this result suggests that MMC may drive enrollees to seek out ED care as a substitute for office-based primary care. Taken together, the effects on high-value and low-value services are mixed.

4.4 Consumer satisfaction

MMC plans reduced spending relative to FFS enrollment. Did these savings come at the cost of observable correlates of enrollees' satisfaction in their plans? Here, we evaluate the probability that a randomly assigned enrollee remains in their assigned plan.³³ Under the typical revealed preference assumption—here, that the decision to exit a randomly-assigned plan is a revealed preference measure of plan dissatisfaction relative to the alternatives—enrollees' switches are informative of enrollee satisfaction.³⁴ We construct a time-varying indicator variable that is equal to one if an enrollee's plan matches their assigned plan, and call this *willingness-to-stay*.

Figure 4 plots willingness-to-stay over time for enrollees assigned to MMC and FFS plans. For both groups, compliance with assignment begins at 100% in month zero, but within a few months, compliance drops as people exit their assigned plans. Exit is differential, with enrollees assigned to an MMC plan more than twice as likely to switch as enrollees assigned to FFS. In particular, Table 4 shows that during the nearly 3 year follow-up period, assignment to MMC leads to a 14.5 percentage point (std. err. = 3.3), or 208%, increase in the probability of switching plans relative to assignment to the FFS option. These results imply that on average, the value of switching away from a managed care plan is much more likely to exceed the inertia and the hassle costs than is the value of switching away from the less restrictive FFS option. Importantly, large MMC-FFS differences in willingness-to-stay occur prior to the pharmacy carve-in. This suggests that dissatisfaction with managed care may be linked to the management of medical (i.e., non-pharmacy) benefits, despite that such management appears to produce relatively little cost-savings (Figure 2). Results at the individual plan level, shown in Figure A4, indicate that these satisfaction results are consistent for individual plans within a payment model as the three MMC plans are the plans with the highest switching rates.³⁵

³³For the first three months after assignment enrollees may switch plans for any reason, after which enrollees could only switch for "good cause" until the next annual open enrollment.

³⁴While this differs from a traditional ex-ante willingness-to-pay measure because there are no premiums, an ex post measure of consumer satisfaction may have some advantage given the difficulties of interpreting willingness-to-pay measures in the presence of choice and information frictions (e.g., Handel and Kolstad, 2015; Handel, Kolstad and Spinnewijn, 2019).

³⁵We cannot reject equal-sized effects among the 2 FFS plans (p = 0.37) but find significant differences across the satisfaction in the 3 MMC plans (p < 0.01). Nonetheless, all three MMC plans have much higher switching rates than either of the 2 FFS plans.

5 Second Identification Strategy: Evidence from the Discontinuation of FFS

So far, we have used variation generated by the random auto-assignment of beneficiaries across MMC and FFS plans to identify effects. One subtlety to interpreting those results as the effects of managed care *per se* is that estimates could reflect the characteristics of the particular set of insurers chosen by the state to participate as managed care plans (rather than as FFS plans). Perhaps, for example, the state selected plans for inclusion in the managed care program on the basis of their expected success in lowering costs. A second subtlety involves the auto-assignees themselves. The program beneficiaries who failed to make an active choice—and were thus randomly assigned between FFS and managed care—may differ in important but unobservable ways from the full population.

In this section, we describe a second natural experiment that speaks to external validity along both of these dimensions and provides an independent research design to complement our primary approach of using person-level randomization. In February 2015, three years after the main autoassignment event exploited above, the FFS model was discontinued by the state. The single remaining FFS plan (hereafter the "transitioned plan") was forced to switch from the FFS to MMC coverage model, including for coverage of prescription drugs.³⁶ Thus, the identity and ownership of the plan was held fixed, even as the coverage model changed, and the entire pool of then-enrolled beneficiaries in this plan were exposed to the shock.

5.1 Econometric Model

We study this second source of variation using a difference-in-differences specification estimated at the enrollee-year level in the following regression:

$$Y_{it} = \alpha + \beta \left(TransitionedPlan_i \times Post_t \right) + \gamma Post_t + \delta TransitionedPlan_i + \varepsilon_{it},$$
(4)

where Y_{it} is an outcome for enrollee *i* at time *t*; *TransitionedPlan* is an indicator variable set to one if an enrollee was continuously enrolled in the transitioning plan and zero if the enrollee was enrolled in one of the control plans; *Post*_t is an indicator for any time period in the year following the state-

³⁶The other FFS plan was acquired and exited the market prior to the forced switch. The enrollees in the acquired plan are all excluded from the difference-in-differences analysis in this section.

mandated transition to MMC for the transitioned plan (i.e., February 2015-January 2016); and β is the coefficient of interest, our measure of the effect of managed care (relative to FFS) using this alternative source of variation.

Our primary sample in this analysis is a balanced panel of enrollees continuously enrolled in the same plan (i.e., with the same insurer) for 24 months (from February 2014 to January 2016), spanning the year prior to and after the transition from FFS to MMC. The sample, described in full detail in Appendix Table A7, is comprised of 497,057 enrollees: there are 189,900 in the transitioned plan—i.e., those enrolled in the plan that shifted from FFS to MMC—and 307,157 enrollees in one of the three preexisting MMC plans that did not experience any substantial policy changes in February 2015.³⁷

5.2 Results of Discontinuation of FFS

Figure 5 plots in raw means how spending in the transitioned plan evolved before and after switching from FFS to managed care, while retaining the same plan enrollees. The figure demonstrates that there is a large, sharp reduction in overall spending—driven primarily by lower pharmacy spending—in the transitioned plan after the switch to MMC, which included a simultaneous drug carve-in for the transitioned plan. The figure shows that the spending levels in the transitioned plan converge to the levels among the existing MMC plans within a few months of the transition.

Using a difference-in-differences framework, we find a pattern of results that is strikingly similar to the analyses based on auto-assignment in the first natural experiment. For the earlier autoassignment experiment, we find MMC reduces annual pharmacy spending by \$94 (25%, 95% CI: 20-30%) after pharmacy is fully carved in (Table 3). For the plan transition experiment, we find a statistically similar percentage reduction, \$145 (30%, 95% CI: 29-31%), after re-weighing the sample to resemble the auto-assigned population (Table A9). Figure A7 shows that the plan transition leads to an approximately one-to-one substitution of brands for generics. Despite the increase in generic prescriptions, overall generic drug spending decreases, consistent with the compositional shift within generics toward lower cost therapeutics, as was seen in the auto-assignment experiment.

Similarly, we find that MMC reduces outpatient spending by 4.1% (95% CI 3.0-5.1%) in this difference-in-differences framework, which is consistent with the 3.2% (95% CI: 1.0-5.3%) reduction

³⁷To facilitate a comparison of our difference-in-differences estimates to those based on the auto-assignee sample in Section 4, we re-weight the difference-in-differences sample to balance its characteristics with those of the auto-assignee sample in our primary analyses. The re-weighted sample characteristics are shown in Appendix Table A8 (See Appendix Section C.2 for additional details).

for the randomized experiment reported in Table 3. For low- and high-value services, we find that when the transitioned plan switched from FFS to MMC, there were again reductions in measures of primary care access (e.g., child access to primary care, well-child visits) as well as a 0.5 percentage point (std. err. = 0.1) increase in the share of enrollees with avoidable ED visits in a year (Panels B and D, Column 3 of Table A9). Further, we find similar reductions in the use of any asthma medications within a year (-0.76 pp; std. err = 0.15).

Besides providing general support for the findings above, these results indicate that the effects identified via auto-assignees and the particular MMC plans in the auto-assignment experiment generalize to a wider set of plans and enrollees.

6 How Does Managed Care Do It?

Our results so far provide strong evidence that full-risk managed care reduces spending relative to FFS (Table 3), that the majority of this effect materializes only after MMC plans take responsibility for prescription drug spending (Figure 2), and that the savings are coincident with a decline in brand drug receipt (Figure 3). In this section, we discuss the potential channels through which managed care plans restrict and reshape utilization and present evidence that utilization management—via real-time adjudication and denials at the pharmacy—is the key mechanism driving the observed changes in utilization.

One advantage of our Medicaid setting is that there is little to no consumer cost-sharing (e.g., coinsurance or deductibles), and the scope of covered benefits is set by the state. This institutional feature narrows the set of possible mechanisms contributing to the observed spending differences between FFS and MMC. In particular, these differences must be driven by differences in the use of supply-side managed care tools, rather than differences in cost-sharing. Though the term *managed care* can encompass a wide range of mechanisms, Glied (2000) summarizes the key components as: (1) how plans negotiate payments to providers; (2) the selection and organization of providers (i.e., networks); (3) case management; and (4) utilization management, in its various forms. In this section, we examine each of these to the extent possible in the data.

6.1 Payments, Providers, and Case Management

We begin with a decomposition of price and quantity effects. To investigate the importance of price differences, we rerun our main auto-assignee IV analysis on a transformation of our dataset in which claims across the FFS and MMC plans have been repriced to a common price list. Because this transformation eliminates price variation at the service level, comparing coefficients in the repriced analysis to the original isolates the role of prices versus quantities. We generate the common price list as the service/procedure fixed effects from a regression of price on these fixed effects, an indicator for MMC assignment and an indicator for the year. The level of the repricing is either the procedure code, National Drug Code (NDC), or Diagnosis Related Group (DRG), depending on the service type considered.³⁸ The difference between the original IV spending estimate in Table 3 and the estimate in the repriced data gives the contribution of prices. The residual reveals the role of quantity changes, including substitutions.

Column 3 of Table 5 reports our instrumental variable estimates of the contribution of lower prices in MMC to savings, overall and within subcategories. ³⁹ The IV estimates for the non-repriced data (our main estimates) are repeated in column 2 for comparison. Price differences between the state FFS schedule and private MMC plans account for about 30% of the overall spending difference in the first row of Table 5. By service category, prices account for almost all of the reductions in outpatient spending and for 10% of the reductions in drug spending. Figure A9 contrasts the average MMC and FFS prices paid, by service. The Evaluation and Management (E&M) codes comprise the greatest category of outpatient services across our study (41% of claims). Within that category MMC pays 4.3% less on average (Table A10). In contrast, across all other non-E&M services (59.3% of services) the average price paid is only 0.84% lower. Thus, MMC is primarily achieving savings in the outpatient service category by paying lower prices for E&M procedures.

Another hypothesized mechanism for how managed care reduces spending is by steering enrollees to providers who provide and organize less intensive care. Table A11 investigates the role of providers and networks in explaining the MMC savings. Though our primary model includes fixed effects for enrollees' providers *prior to assignment*, it is possible that the enrollees assigned to managed

³⁸An ATC-4 was used if the NDC was unavailable, and the primary diagnosis code was used if the DRG was unavailable.

³⁹Drug rebates, discussed above, may be a channel by which additional pharmacy savings accrue to plans, beyond the spending effects observable in claims data and documented in Table 3. Any such potential savings are not considered here, where we decompose the Table 3 effects. See Section 4.2 for a full discussion.

care plans are steered (e.g., via provider network restrictions, provider assignment algorithms, etc.) to a different set of treating providers than the enrollees assigned to managed FFS. To assess whether this type of steering explains our results, we estimate our primary model with an additional set of fixed effects for each enrollee's primary provider in each post-assignment year (i.e., the provider responsible for the modality of their claims in that year). We also build controls for the primary care provider network breadth at the plan \times ZIP level—allowing for different breadth of *de facto* networks even within a plan, as a function of providers' locations relative to the enrollee—following the method of Wallace (2023).⁴⁰

Isolating the network channel (Table A11, Column 3) reveals that MMC plans do not constrain costs by restricting access via narrower provider networks. In fact, MMC plans have broader primary care networks (i.e., they cover more providers) in almost every geographic location (Figure A10), which would imply a significant increase of \$34.7 (95% CI: \$22.8 - \$45.7) in overall spending holding other features fixed. Further, MMC plans do not reduce spending by steering enrollees to lower-cost providers: comparing auto-enrollees assigned to MMC and FFS who shared the same primary care provider (via post-assignment provider fixed effects), we estimate small and statistically insignificant effects of provider steering for total and pharmacy spending. These results imply that MMC-FFS spending differences persist within equally restrictive networks and within primary providers.

A third potential explanation for managed care's spending effects is case management—the process of managing and coordinating the provision of healthcare for members, such as by coordinating referrals to a specialist, nurse triage lines, post-discharge planning, etc. Such investments are believed to offset costs, for example, by reducing hospitalizations (Chandra, Gruber and McKnight, 2010; Chandra, Flack and Obermeyer, 2021). Case management is not directly observable in our data (or any claims data). But our results do not appear to be consistent with MMC plans generating offsetting reductions in hospitalizations via high-value services: Table 4 did not reveal systematically greater use of high-value care among MMC plans, and Table 3 showed zero reduction in inpatient spending among MMC plans relative to FFS.

In summary, prices account for one-third of total spending reductions and the majority of the outpatient savings. The broader primary care networks in managed care plans and the fact that managed care enrollees are more likely to see providers associated with higher spending would both

⁴⁰See Appendix D.2.4 and D.2.3 for more details on identifying providers and estimating network breadth.

tend to increase spending, so neither of these explain the overall savings in managed care. Even though these features themselves could be important for cost control, MMC-FFS *differences* in these features are not the most important explanatory variables here.

6.2 Utilization Management

The key remaining category among supply-side interventions is utilization management (i.e., prior authorization, step therapy, and quantity limits), which we observe a proxy for via novel data on claims denials. Because prescription drugs account for the largest share of the causal spending difference between managed care and FFS, we focus our attention there. As noted above in Section 2.6, pharmacy denials provide a unique opportunity for an insurer to interdict service provision because, unlike medical denials (in which a service is rendered and then a dispute over payment follows), pharmacy denials are adjudicated in real-time, so that a denied claim results in a patient not receiving a prescription.

In Panel A of Figure 6, we document sharp increases in the rate of pharmacy claim denials immediately following the carve-in of prescription drugs to MMC plan responsibility. The plot shows that in the early part of our sample period, the share of claims denied is similar across the enrollees assigned to MMC and FFS and rising steadily, reflecting a secular trend in the legacy FFS system's approach to administrative claims processing. The share of claims denied remain similar after randomized auto-assignment and enrollment in MMC plans, until the carve-in of prescription drugs to MMC financial responsibility. Following the carve-in, the share of claims denied rises sharply for enrollees assigned to the MMC plans but trends smoothly for those assigned to FFS. This increased use of claim denials coincides with the sharp reduction in pharmacy spending after the carve-in, documented in Section 4. Importantly, this increase in the share of claims denied in MMC relative to FFS is short-lived, peaking in the quarter after the carve-in but ultimately falling below the FFS denial rate and stabilizing around two-thirds of the FFS level.

The spike in denials does not in itself explain *how* utilization management impacts drug spending, which could occur through insurers using denials to induce overall reductions in prescriptions filled, substitutions from brand-to-brand within a therapeutic class, substitutions from brand-togenerics within a therapeutic class, or substitutions across therapeutic alternatives. To further investigate these issues, we take three complementary approaches. First, we follow Dranove, Ody and Starc (2021) in measuring generic efficiency: the share of prescriptions filled with a generic when a generic equivalent is available. Consistent with the increases in generics noted in Figure 3, Figure A11 shows an increase in generic efficiency timed with the denials regime. This measure is useful, but it doesn't capture more complex patterns of substitution (such as from a branded drug to a generic with a different molecule in the same therapeutic class) and it doesn't indicate what share of the spending reduction generic substitution accounted for. As a second approach, we follow Brot-Goldberg et al. (2017) in generating a complete decomposition of price, quantity, and substitution effects for prescription drug spending. For that exercise, we assign each drug to an ATC-4 therapeutic class using the Anatomical Therapeutic Chemical (ATC) Classification System, which provides a way to identify drugs that are clinical substitutes (Ganapati and McKibbin, 2019; Dubois, Gandhi and Vasserman, 2019). The full details of that analysis are provided in Appendix D, but we summarize the results showing that in the first year after the pharmacy carve-in, substitutions from brands to generics within the same therapeutic class (but potentially across molecules) account for an 8.6% reduction in pharmacy spending (95% CI: 6.9-10.5), or one-third of the total 25.5% reduction in pharmacy spending. Substitutions across therapeutic classes or outright quantity reductions (the residual component in our decomposition) are responsible for an 11.6% reduction (95% CI: 6.5-15.9) in spending, or nearly one-half of the overall pharmacy spending reduction. This term of the decomposition would capture, for example, the across-class substitution of guanfacine—an antihypertensive that is FDA-approved for the treatment of ADHD—for Adderall, which is 19 times its price.⁴¹ Results in the second year after carve-in are similar. (See Figure A12 and Tables A12 and A13).

Neither the Dranove, Ody and Starc (2021) nor Brot-Goldberg et al. (2017) approaches are able to evaluate whether these substitution effects are driven by utilization management. Therefore, our third approach is more directly focused on this mechanism, examining whether differences in the claims denial rate across various therapeutic classes of drugs correspond to the quantity changes and spending effect sizes we estimate for those classes. If denials were causing spending reductions, one would expect the heterogeneity in denials to track the heterogeneity in spending reductions across classes of prescription drugs. Panel B of Figure 6 plots IV estimates of managed care's spending effects (relative to FFS) on each drug class against the share of claims denied by managed care plans

⁴¹Guanfacine costs, on average, \$12 per 30 days supply in our data, while Adderall costs, on average, \$230 per 30 days supply.

in that therapeutic class during the spike period just after carve-in.⁴² The figure shows a negative and statistically significant relationship, indicating that managed care plans generated larger spending reductions in drug classes where they managed utilization more aggressively.⁴³ In Figure A14 we verify that the drugs denials effects by therapeutic class are similar for the auto-assignee identification strategy (used to construct Figure 6) and the plan transition strategy (used to construct Figure 5). The correspondence between the two sets of estimates is very close, with the estimates from the two strategies and samples aligning closely along the 45 degree line.⁴⁴

An alternative way to show that utilization management via denials is the precise mechanism behind reduced drug spending—rather than something merely coincident with the timing of carvein—is examining the correlation between denials and drug spending at the level of individual drugs. To investigate this, in Figure A15, we group drugs using National Drug Codes (NDCs) into deciles based on the share of prescriptions denied in the first quarter after the pharmacy carve-in. The figure shows large and immediate reductions in the quantity of paid pharmacy claims in MMC following the carve-in for the most denied NDCs but no reductions (and possibly increases) in the quantity of paid claims for the least denied NDCs.

It is important to understand that denials and lower spending within a class do not necessarily imply fewer filled prescriptions in that class. Figure 7 demonstrates that, for most therapeutic drug classes, spending reductions do not correspond to outright reductions in prescription counts, and instead reflect enrollee substitution from higher- to lower-cost prescription drugs within therapeutic classes. The figure compares therapeutic class-specific denial rates on the horizontal axis to the causal effects of MMC on prescription drug *quantity* (i.e., prescriptions filled) by therapeutic class. Although

⁴²To measure the managed care claims denial rate we restrict to the first quarter after the pharmacy carve-in (with November 2012 as a wash-out month). This period best reflects differences in the managed care denial regime between therapeutic classes as denial rates are measured prior to quantities adjusting to the new utilization management regime. We avoid including the month immediately after the carve-in to allow for a modest transition period and ramp-up.

⁴³A similar dose-response relationship exists if we restrict our analyses to children (Figure A13).

⁴⁴The correspondence between the two identification strategies and, within the autoassignee sample, between the various managed care plans (as shown in Appendix Figure A4), is striking given that they all utilize different pharmacy benefits managers (Table 24.1 in https://ldh.la.gov/assets/docs/BayouHealth/2013Act212/Fiscal_Year_2015/SFY15_Draft_FINAL-08092016.pdf). This suggests that, at least in our context, the high-powered incentives associated with MMC, and the additional flexibility the private plans may have, are more important drivers of savings on pharmacy spending than differences in approaches to utilization management between the PBMs operating in this market. Most of the points lie above the 45 degree line, consistent with our estimates from the auto-assignee identification strategy generally being smaller. Despite balancing the samples on health status-by-gender-by-age bins, those who select into "not making an active plan choice" are lower spending overall (Appendix Table A1) and it is possible that their spending is less impacted by managed care. Another possibility, consistent with the literature (e.g., Geruso, Layton and Wallace, 2023), is that managed care plans differ in how aggressively they manage utilization and the focal plan in the plan transition natural experiment may be particularly restrictive.
the overall relationship in Figure 7 is negative, for therapeutic classes with claim denial rates below 40%—the vast majority of classes, and the largest classes—there is no strong statistical relationship between denials and the number of prescriptions filled (slope = -0.45, std. err. = 0.25), consistent with no substantial, long-lived MMC-FFS differences in the quantity of prescriptions (as seen in Panel A of Figure 3). However, for the drug classes targeted most aggressively by MMC plan denials (e.g., expectorants, antiallergics, agents for dermatitis, and antibiotics), we find that cost savings come with correspondingly large reductions in quantity, revealing a more complex strategy by which utilization management may be used to drive both substitution and outright quantity reductions. Together with Panel B of Figure 6, which showed that *spending* in heavily-managed drug classes was reduced, Figure 7 indicates that cost-savings are achieved via utilization management that drives within- or across-class substitution, not outright quantity reductions, for most drug classes. This is consistent with our finding that managed care does not significantly reduce drug supply (Panel A, Figure 3): managed care reduces overall pharmacy medication days supply by 3.48 days annually (std. err. = 2.45) on an average drug utilization of 114.29 days supply annually (Panel A, Table A14). As an example of substitutions, the branded prescription drug Pataday is an antihistamine typically used for the treatment of eye infections (e.g., pink eye). After the drug carve-in, we observe Pataday, which has an average unit cost of \$4.80 and is administered once a day, being denied by private plans at the point of service and replaced with a subsequent prescription of Ketotifen, a generic drug in the same ATC-4 class that has an average unit cost of \$0.30 and is administered, on average, twice a day. On occasion, we also observe a denied Pataday prescription being replaced with a subsequent prescription for Tobramycin, a broader spectrum generic antibiotic that may treat some of the symptoms of eye infections, at an average unit cost of \$2.02, but needs to be administered up to six times a day.

In principle, the public FFS program could use real-time adjudication to deny pharmacy claims in the same way as managed care does to achieve savings. In practice, however, pharmacy denials in the FFS system appear to be diffuse and driven by a bureaucratic process centered on documenting medical necessity, rather than targeting cost-saving substitutions. In Figure 8, we show that the patterns of FFS denials by class contrast substantially with the strategic denial regime of the private plans, with FFS making fewer denials of potentially lower-value antibiotics, expectorants, and antiallergics and more denials of antipsychotics, diabetes drugs, and centrally acting sympathomimetics (treating, for example, ADHD). Further, Figure A16 demonstrates that after experiencing a denial, MMC enrollees are more likely to shift from brand to generic drugs relative to the substitution patterns of FFS enrollees experiencing denials. Figure A17 reveals that the denial rates in MMC are higher for both brand and generic drugs, relative to FFS, and that MMC denial rates are much higher than FFS denials for prescriptions where the requested days supply exceeds 30 days.

The transitory spike in denied claims apparent in Figure 6, Panel A, suggests the possibility of learning: In the first months following the carve-in, denials spike while prescribers and pharmacists learn what will be allowed, but within a year, the denial rates plummet below the counterfactual (FFS) rates. However, because our primary sample is a balanced panel, an alternative explanation may be that enrollees initially face pharmacy denials which shift them to new prescriptions that do not get denied on subsequent refills. This would explain the pattern of an initial spike in denial rates and then a reduction over time to a lower level. But it would not involve behavior change by physicians.

Figure 9, Panel A presents evidence against the provider learning hypothesis. The figure plots the patterns of pharmacy claims denials for cohorts of Medicaid enrollees that joined the managed care program before and after the pharmacy carve-in. If prescribers and pharmacists learn over time, we would expect the initial denial rates for cohorts of MMC enrollees that join Medicaid after the pharmacy carve-in to be lower, reflecting prescribing patterns that have changed to accommodate managed care plan formularies and prior authorization rules. Instead, what we observe is a pattern of high initial pharmacy denial rates for each MMC cohort (comparable to the peak denial rate for the cohorts exposed to the carve-in) and then a reduction in the denial rate within each cohort over time. The pattern is more consistent with MMC utilization management practices leading to quantity substitutions and reductions that persist within cohorts, than with large impacts on prescriber behavior for new enrollees. Panel B of Figure 9 demonstrates a different pattern for enrollees in FFS: denial rates are flat or increasing over time with no visual change in denial rates around the pharmacy carve-in.

6.3 Tradeoffs

A difficult question in this setting is whether the apparent tradeoff between cost savings and the beneficiary dissatisfaction coincident with those savings is appropriate. Whether a social planner

would evaluate the shift from FFS to MMC as welfare-improving depends on some facts that we do not observe, including how plan quality impacts health (as distinct from effects on healthcare use, observable in claims data) and how such health impacts were valued.

Clinical measures of quality are often used to inform on healthcare quality, but they cannot capture the full social value of one plan versus another. In principle, the revealed preference implicit in patterns of health plan choice in a health insurance market can reveal enrollees' private valuation of health plan features, including anticipated downstream impacts on health. But health insurance decisions are impacted by choice frictions (e.g., Handel and Kolstad, 2015; Abaluck et al., 2021), and in the case of entitlement programs like Medicaid, there is an explicit public goal of providing services that are not privately valued at their cost. These facts complicate any complete welfare analysis that relies too heavily on revealed preference as a sufficient statistic for the social marginal benefit of one plan option relative to another.

With those caveats, we can consider the value to the enrollee of FFS versus MMC enrollment, as revealed and rationalized by their choices to comply with plan assignment or to switch plans. We can ask: How does a measure of enrollee dissatisfaction (plan switching) stack up against the savings? If enrollees themselves could claim those savings, would they find the tradeoff acceptable? Such an exercise can provide insight into some of the social benefits and costs of privatization, even while falling short of a complete characterization of social welfare. It will necessarily leave out some concerns that may be first order for government, including the financial risk reduction in state budgeting that arises from contracting with private plans on a capitation basis.

To make this comparison, the plan valuations implied by switching need to be converted to a monetary equivalent. Medicaid beneficiaries do not face premiums that could reveal plan valuations in dollar terms, but estimates of own-price premium elasticities from adjacent markets can bridge the gap by relating plan switching to price increases. In particular, evidence from the lowest-income tranche of the ACA marketplaces suggests own-price elasticities at the insurer-level ranging from about -2 to -3.⁴⁵

⁴⁵Own-price *plan* elasticities in the ACA markets, summarized in Saltzman (2019), span a wide range, from around -2 to -10. Estimates of insurer elasticity (a closer analog to our setting in which each insurer offers a single Medicaid plan and provider network) are smaller—around -2 to -3 in Timmers (2022), which estimates insurer elasticities in the cost-sharing-reduction (CSR) population in the ACA Marketplace. These are the lowest-income ACA participants, whose income made them just barely ineligible for Medicaid coverage. At the plan-level (rather than the insurer-level), Timmers (2022) finds larger elasticity estimates, in the middle range of estimates for the ACA Marketplaces (see Drake, 2019, Saltzman, 2019, and Tebaldi, 2022), and similar to Curto et al. (2021) in the context of Medicare Advantage.

In our setting, enrollees randomly assigned to managed care leave their assigned plans at an average rate of about 16 percent. The lower expenditure associated with being assigned to managed care is about 6 percent, implying a savings-switching elasticity of 2.8.⁴⁶ Therefore, own-premium enrollment elasticities in the range of the literature would imply that cash rebates to Medicaid enrollees equal to plan savings would approximately compensate these enrollees for being assigned to the thriftier, managed care option. From this perspective, it is *plausible* that the savings-satisfaction tradeoff is efficient, though the framework of willingness-to-pay is an awkward fit to this market— where government-funded healthcare provision occurs precisely because of a mismatch between the low revealed valuations of health insurance among very low-income consumers (Finkelstein, Hendren and Luttmer, 2019; Finkelstein, Hendren and Shepard, 2019) and because of the revealed policy preference for providing this care. And here, as in every context in which revealed preference is used as a sufficient statistic for value, the measure ignores everything beyond the subjective evaluation of the consumer, including the type of process outcomes (e.g., the use of the emergency department or of high- and low-value services) that we measure in Section 4.3.

Beyond the gross spending-satisfaction tradeoff, we can examine the subtler issue of whether the primary source of the cost savings—shown above to be managed care's administration of drug benefits and drug denials aimed at substitution—is the primary cause of beneficiary dissatisfaction. The delayed carve-in of prescription drugs provides an opportunity to identify this. Panel A of Table 6 breaks out the spending and satisfaction results relative to the timing of the carve-in, but otherwise follows the same specifications of Tables 3 and 4. In the pre-carve-in period from February to November 2012, the reduced form effect of random assignment to managed care on spending was \$31 (std. err. = \$14) per beneficiary per year, compared to an overall average of \$62 (std. err. = \$13) per enrollee per year when the sample includes the full follow-up period. This mirrors the time patterns of overall spending impacts visible in the quarter-by-quarter coefficients plotted in Figure 2. In comparison, columns 5 and 6 in Table 6 show that in the pre-carve-in period, the impact on willingness-to-stay was already large and statistically significant. Exiting one's assigned plan is essentially an absorbing state and 11 percentage points of the eventual 15 percentage point exit rate

⁴⁶In more detail: What own-price plan premium elasticity would be required to exactly match the cost savings effects to the disenrollment effects (in a hypothetical in which enrollees were rebated the savings)? From Table 4 there is a -15.6% (= 14.54/93.02) retention effect of being assigned to managed care. From Table 3, column 3 there is a -5.6% cost difference (= -\$81.51/\$1,451.35) effect of being assigned to managed care. This naive calculation implying an elasticity of -2.76 would mean that a rebate equal to the savings (\\$82) would exactly counteract the disenrollment effects.

had already accrued before drugs were carved in to managed care responsibility.

To investigate this further, Panel B of Table 6 subsets the main analysis sample according to anticipated exposure to the drug denials regime. To avoid endogenously classifying beneficiaries according to the ex post impacts of being exposed to a managed care plan's denials, we generate a predicted exposure measure that depends only on drug and non-drug utilization data from the period prior to randomized auto-assignment. Enrollees are grouped into quartiles of ex ante exposure. Two patterns are clear from the analysis. First, willingness-to-stay declines monotonically with the level of predicted exposure, while all of the savings accumulates from the highest exposure quartile. Second, even among the lowest-exposure group, there is significant outflow (13 percentage points) among those assigned to managed care plans. Together with the results in Panel A, these results suggest that even if cost-saving drug denials and substitutions abrade beneficiaries, these plan features are responsible for at most a small fraction (22 percent = $1 - \frac{11.3}{14.5}$, columns 5 and 6 in Panel A) of the plan switching effects we observe. That is important because it implies the possibility that plans are exceptionally efficient at managing the prescription drug benefit, while offering less in the non-drug domain, where we document dissatisfaction but find smaller associated savings.

As most of the switching occurred before drugs were carved into managed care, it's unsurprising that exposure to pharmacy denials explain a small fraction of plan switching.⁴⁷ We further explore the relative importance of medical and pharmacy utilization for plan switching by stratifying the sample according to predicted healthcare utilization. Using baseline healthcare utilization and demographic data, we generate three separate predicted measures for the likelihood that a member has any healthcare utilization, medical utilization, or pharmacy utilization within 90 days after assignment.⁴⁸ Enrollees are grouped into deciles of ex ante likelihood and we examine both switching and healthcare spending effects stratified by these deciles. Figure A19 indicates that enrollees in the highest decile of any predicted use are 16.9 percentage points (std. err. = 4.1) more likely to switch from

⁴⁷Starting in September 2012, 7 months after randomization and two months before the pharmacy carve-in, the Louisiana Department of Health began informing and educating members about the pharmacy carve-in via mailings (Louisiana Department of Health and Hospitals, 2012). At this time, individual plans also informed members of this change by sending them their pharmacy benefits management ID cards so they could continue using their pharmacy benefit. The initial period in which members could freely switch plans without cause was between February and March of 2012.

⁴⁸In Figure A18 we perform a complementary analysis, examining heterogeneity in satisfaction based on recency of healthcare utilization (medical or pharmacy) prior to random assignment as a rough proxy for whether enrollees are likely to engage in healthcare post-assignment. In Figure A18, we find that enrollees who recently used care (2 weeks prior to assignment) were 10.8 percentage points (std. err. = 2.5) more likely to switch from managed care than enrollees who had no utilization in the year prior to assignment. Relative rates of switching decrease roughly monotonically as time since prior utilization increases.

managed care than enrollees in the lowest decile of predicted use, with a strong monotonic increase as the likelihood of future use increases. When we include both predicted medical and pharmacy utilization in the regressions simultaneously, to determine which is most predictive of the overall utilization heterogeneity, we see no clear relationship between switching and the deciles of predicted pharmacy utilization but the same approximately monotonic increase in switching as medical utilization becomes more likely.

7 Discussion

In this paper, we examine the effects of privatizing social health insurance in the United States. We do this in the context of the Medicaid program, where we compare healthcare spending, quality, and consumer satisfaction between enrollees in a state-administered FFS system and a private Medicaid managed care plan. A special feature of our setting is that it accommodates two complementary identification strategies—the first leveraging random assignment of Medicaid enrollees across the private and public models of provision and the second exploiting the elimination of the state-administered FFS program three years later, which caused the last remaining FFS plan to transition to operating as a risk-bearing, private managed care plan. Evidence from the two identification strategies was remarkably consistent. We find that spending was nearly 6% lower for enrollees auto-assigned to a managed care plan, with most savings arising from reducing pharmacy, rather than medical, expenses. Substitutions to lower-cost alternatives—driven by real-time claims denials at the pharmacy—accounted for a large share of savings. We also show that the reductions in spending came at the cost of revealed consumer satisfaction: enrollees assigned to managed care plans were nearly 3 times as likely to switch out of their plans as those assigned to the FFS option. Despite the large difference in disenrollment rates, back of the envelope calculations suggest that, while enrollees dislike managed care, they would select it if they were the claimants on the savings.

By shedding new light not only on the size of these effects, but also the role of utilization management as the mechanism, our findings contribute to a growing evidence base on administrative frictions in the US healthcare economy (Cutler and Ly, 2011). In particular, we focus on a new mechanism: plans' capacity to affect care provision through the real-time adjudication of pharmacy claims. Whereas medical claims may be denied after care is provided—creating large administrative burdens for providers that reduce their likelihood of participating in Medicaid (Dunn et al., 2021)—real-time adjudication in pharmacy allows plans to efficiently interdict healthcare consumption at the point of service. Although there is no in-principle reason that medical claims couldn't also be adjudicated in real-time, to date real-time adjudication, which is near-universal for pharmacy claims, is not used for medical claims (Orszag and Rekhi, 2020). Hence, our work establishes that utilization management techniques—and the well-documented administrative frictions they generate—can, in some circumstances, lead to a more efficient allocation of healthcare resources. The results also suggest that utilization management need not result in a large volume of claims denials in long-term equilibrium to shape prescribing patterns. In our analysis, after an initial spike in claims denials in managed care, denial rates in MMC plummeted, eventually settling at a denial rate below that in FFS. This finding also suggests that MMC plans may be operating the bureaucracy of utilization management more efficiently than FFS in our setting: both entities rely on claims denials (and deny a substantial share of claims), but MMC generates savings relative to FFS and, in equilibrium, has a lower claims denial rate.

Though our study period was over a decade ago, it remains the case that real-time claims adjudication remains rare outside of the pharmacy setting, so we anticipate that the divide between pharmacy and non-pharmacy utilization management that we document remains relevant. In our study, the ability of real-time adjudication to drive pharmacy savings seems closely linked to the availability of suitable lower-cost alternatives, so applicability of this mechanism today and into the future will depend on the degree of substitutability for important, expensive medications. Other utilization management techniques by managed care organizations may have changed in importance since our study period. Price negotiation, which drives the outpatient spending effects, likely remains as relevant as ever.

These findings inform an active policy landscape in Medicaid administration. State Medicaid programs continue to refine policies regarding which services are carved-in and carved-out of MMC contracts. While the prior literature on managed care outsourcing has tended to focus solely on its high-powered incentives (for e.g., Laffont and Tirole, 1993), our findings indicate that strong incentives—in our context, capitation contracts, in which the plans are residual claimants on the savings they produce—may be insufficient to generate healthcare spending reductions (or other desired outcomes). Although the incentives for constraining spending existed across all healthcare service domains for MMC plans in our setting, these plans appeared to have more capacity to affect care

provision in the context of pharmacy services, via real-time claims adjudication.

One implication of our findings is that private managed care plans may have sharp tools for managing pharmacy benefits—where they are able to reduce spending without harming access—but their effectiveness at managing medical benefits, where we observe smaller cost savings and reductions in consumer satisfaction and healthcare quality, is less clear in our setting. To put this finding in context: historically it has been more common for governments to contract provision of non-drug benefits to MMC plans, while leaving drugs carved-out and under public provision, than to do the opposite. While most managed care states have now carved-in prescription drugs to MMC provision, several states (including New York and California) have recently, or are planning to, return to the direct public provision of prescription drugs in Medicaid (Gifford et al., 2020). However, we find the clearest evidence to support outsourcing pharmacy benefits—where private firms appear to efficiently reduce cost—rather than medical benefits, where the trade-off is less clear.⁴⁹ Therefore, carving out drug benefits from managed care may forgo important opportunities for efficient cost reduction in state governments' make-or-buy decision-making. Ultimately, our findings suggest that states could benefit from selectively contracting for services where managed care proves most effective, rather than adopting an all-or-nothing approach.

⁴⁹One consideration that weighs against outsourcing drug provision to private managed care organizations is that pharmacy benefit design is a service area with significant potential to be used as a screening tool, discouraging—via pharmacy benefit design and implementation—enrollees who are predictably unprofitable from joining or staying in the organization's plan (Geruso, Layton and Prinz, 2019). It is unclear how important this consideration is in the context of Medicaid, where pharmacy benefits are more constrained by the regulator than in other settings.

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Figure 1: First stage: Medicaid managed care assignment and enrollment (raw means)

Note: Figure plots mean enrollment rates in Medicaid managed care (MMC) over time for enrollees assigned to the MMC and Fee-for-Service (FFS) delivery models. Observations are at the assigned model × quarter level. Time, in quarters, is along the horizontal axis. The leftmost vertical line indicates the start of managed care (the beginning of the treatment period); the rightmost vertical line indicates when pharmacy is carved into Medicaid managed care. The vertical axis measures the fraction of individuals who are observed to be enrolled in an MMC plan in the indicated quarter, plotted separately according to the assigned plan type in February 2012. The sample here is the main sample of 94,976 auto-assignees.



Figure 2: Main result: impact of assignment to managed care on overall healthcare spending

Note: Figure presents a difference-in-differences event study comparing health spending across assignees to MMC and FFS. The vertical axis indicates spending per-enrollee-per-quarter. Estimates are based on a balanced panel of 85,668 continuously-enrolled recipients for the 47 month (February 2011–December 2014) period depicted. Time, in quarters, is along the horizontal axis. The leftmost vertical line indicates the start of managed care (the beginning of the treatment period); the rightmost vertical line indicates when pharmacy is carved into Medicaid managed care. The figure shows the (null) effects of assignment to managed care prior to the treatment period and a large, and precisely-estimated drop in quarterly healthcare spending after assignment to MMC. Total annual spending is winsorized at \$25,000. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). 95% confidence intervals reported.



Figure 3: Main result: impact of assignment to managed care on pharmacy spending and quantity

Panel A. Number of prescriptions per-enrollee-per-quarter



Note: Figure presents difference-in-differences event studies comparing pharmacy spending and quantities across assignees to MMC and FFS. The dependent variable in each panel is indicated in the title. Panels A and B express percent change relative to the pre-period. Observations are at the assigned model \times quarter level. Time, in quarters, is along the horizontal axis. The leftmost vertical line indicates the start of managed care (the beginning of the treatment period); the rightmost vertical line indicates when pharmacy is carved into Medicaid managed care. Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details). Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). 95% confidence intervals reported.



Figure 4: Revealed preference: enrollees assigned to managed care are more likely to switch plans

Note: Figure plots mean enrollment rates in three MMC plans and two FFS plans over time. Observations are at the assigned model \times months level. Time, in months, is along the horizontal axis. The vertical line indicates when pharmacy is carved into Medicaid managed care. The vertical axis measures the fraction of individuals who are observed to be enrolled in their assigned plan in the indicated month, plotted separately according to the plan type of assignment. The sample here is the main sample of 94,976 auto-assignees.



Figure 5: Second identification strategy: the last FFS plan transitions to become a managed care plan

Note: Figure plots means of spending over time by plan. Individual plans contributing to the pooled control plan are shown as dashed grey lines. The vertical axis indicates spending per-enrollee-per-quarter. Observations are at the plan \times quarter level. Time, in quarters, is along the horizontal axis. The vertical line indicates when the treatment plan transitioned from FFS to become a full-risk managed care plan. The plans that were already full-risk managed care plans did not experience a change at that time. This event date (February 2015) is three years after the date of the auto-assignment natural experiment used in Figures 1, 2, 3, and 4. The sample is a balanced panel of 497,057 continuously enrolled recipients. Plotted means are residualized on calendar quarters to adjust for seasonality. Observations are reweighted such that the sample matches the distribution of the auto-assignee sample used in the first identification strategy on health status-by-gender-by-age bins (See Appendix C.2 for additional details). Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details).





(a) *Panel A.* After pharmacy carve-in, pharmacy denials spike





Note: Panel A presents a time series plot of overall denial rates (percent denied) separately for MMC and FFS plans. Observations are at the assigned model \times month level. Time, in months, is along the horizontal axis. The leftmost vertical line indicates the start of managed care (the beginning of the treatment period); the rightmost vertical line indicates when pharmacy is carved into Medicaid managed care. It shows a sharp increase in denials in MMC after pharmacy is carved in. Panel B compares managed care spending effects by therapeutic drug class (vertical axis) to the share of claims denied by managed care plans (horizontal axis). Markers correspond to ATC-4 therapeutic classes of drugs with sizes proportional to FFS spending. To measure the managed care claims denial rate used in Panel B, we restrict to the first quarter 1 month after the pharmacy carve-in in order to capture the peak visible in Panel A. The negative and statistically significant relationship in Panel B (slope = -1.64; std. err. = 0.31) indicates that managed care plans generated larger spending reductions in drug classes where they managed utilization more aggressively via denials. Both panels use the auto-assignment experiment and sample.

Figure 7: Mechanisms: denials caused within-class substitutions, not outright reductions for most drug classes



Note: Figure shows how overall quantity within therapeutic classes (measured as filled prescriptions) changes as a result of utilization management using a dose-response relationship similar in construction to Panel B of Figure 6. The plot compares the IV estimates for the effect of MMC enrollment on the number of paid prescriptions per ATC-4 therapeutic class (vertical axis) to the share of claims denied by managed care plans (horizontal axis; identical to Panel B of figure 6). Markers correspond to ATC-4 therapeutic classes of drugs. Marker sizes are proportional to FFS spending.

Figure 8: The share of prescription drug claims denied by therapeutic drug class are negatively correlated between MMC and FFS



Note: Figure presents evidence that pharmacy denials in the state FFS program are negatively correlated with the strategic denial regime of the full-risk MMC plans. The figure compares the pharmacy prescription denial rates in MMC — based on enrollees randomly auto-assigned to one of the three full-risk MMC plans — against the pharmacy prescription denial rates in FFS — based on enrollees randomly auto-assigned to one of the FFS plans. The prescription drug claims denial rates for each ATC-4 therapeutic class are measured by restricting to the first quarter after the pharmacy carve-in, with a one month wash-out period.



Figure 9: Limited evidence of prescriber learning: after the carve-in, pharmacy denials in MMC follow a similar pattern of high-to-low for each cohort of enrollees new to the Medicaid program

Note: Figure shows denial rates over time, for new monthly cohorts that join Medicaid for the first time, among members who join a MMC plan (top panel) or a FFS plan (bottom panel). For each cohort we require continuous enrollment from the join date through the end of the study period (Dec 31, 2024) to avoid any compositional changes within a single time series over time. Every third cohort is labeled by the month and year of the earliest data point for that cohort. The vertical line indicates when pharmacy benefits were carved-in to Medicaid Managed Care. The last cohort shown joined Medicaid in June 2014, which allows for at least 6-months of follow-up before the end of the period.

	Mean	Std Dev	Max	Min	Ν		
	(1)	(2)	(3)	(4)	(5)		
Panel A. Enrollee characteristics							
Age at baseline	9.4	7.5	63	0	284,928		
Female (%)	52.9	49.9	100	0	284,928		
Panel B. Enrollee spending, annually (\$)							
Total	1,451	2,428	25,004	0	284,928		
Medical	1,053	1,815	18,257	0	284,928		
Inpatient	97	748	9,891	0	284,928		
Outpatient	590	820	7,342	0	284,928		
Pharmacy	381	949	10,408	0	284,928		
Brand Drug	229	757	8,967	0	284,928		
Generic Drug	150	346	3,427	0	284,928		
Panel C. Any annual utilization of high- or potentially high-value	care (%)						
Annual Well-Child Visits	49.3	50.0	100	0	168,315		
Access to Primary Care	80.5	39.7	100	0	280,915		
Chlamydia Screening	59.7	49.1	100	0	10,403		
Cervical Cancer Screening	67.2	47.0	100	0	13,759		
Follow-up care after ADHD Prescription	51.2	50.0	100	0	3,881		
Behavioral	7.4	26.2	100	0	284,928		
Dental	55.2	49.7	100	0	284,928		
Asthma Medication	14.9	35.6	100	0	284,928		
Diabetes Medication	0.6	7.6	100	0	284,928		
Panel D. Any annual utilization of low- or potentially low-value care (%)							
Any Low-Value Care Composite	0.9	9.6	100	0	284,928		
Avoidable Emergency Department Visits	8.4	27.8	100	0	284,928		
Imaging	23.3	42.3	100	0	284,928		

Table 1: Summary statistics

Notes: Table reports summary statistics on enrollee demographics, utilization, and spending. The sample consists of a balanced panel of Medicaid enrollees that were randomly auto-assigned to Medicaid managed care or the managed FFS option in February 2012 and remained in Medicaid until at least December 2014. Observations are at the enrollee-year level: N = 284,928 enrollee-years. Additional details on the utilization and spending measures is available in Section 2. Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details). Components of spending do not sum up to "Total" due to winsorization. In addition, "Inpatient" and "Outpatient" spending do not sum to "Medical" spending because of Medicaid-specific services omitted from this summary statistics table, for example, behavioral health and dental care.

	Mean	Coef. on Managed Care Assignment	p-value
	(1)	(2)	(3)
Panel A. Enrollee characteristics			
Age at baseline	9.36	0.02	0.89
Female (%)	52.92	0.04	0.91
Panel B. Pre-assignment enrollee health conditions			
Asthma	6.18	-0.02	0.89
Serious Mental Illness	2.71	0.02	0.90
Diabetes	0.63	0.03	0.59
Pregnancy	1.22	0.01	0.87
Cardiovascular conditions	1.23	0.10	0.18
Panel C. Pre-assignment enrollee spending, monthly (\$)			
Total	153.82	11.36	0.11
Medical	117.83	11.06	0.10
Pharmacy	35.99	0.31	0.81
Panel D. Any pre-assignment use of potentially high-value care (%)			
Annual Well-Child Visits	24.92	0.24	0.47
Access to Primary Care	76.18	0.09	0.86
Chlamydia Screening	0.85	0.03	0.67
Asthma Medication	2.59	-0.06	0.37
Diabetes Medication	0.17	0.04	0.06
Panel E. Any pre-assignment use of potentially low-value care (%)			
Any Low-Value Care Composite	0.79	-0.07	0.29
Avoidable Emergency Department Visits	6.28	0.17	0.50
Imaging	24.31	0.91***	0.00
Joint Test			0.25
Ν		94,976	

Table 2: Balance: auto-assignee characteristics across the assignment groups (MMC vs FFS)

Notes: Table presents tests for balance of predetermined characteristics among enrollees who were auto-assigned to FFS or MMC. Each row corresponds to a separate regression. The characteristics tested for balance include predetermined recipient demographics and pre-assignment utilization and diagnoses. To construct column 2, each baseline characteristic is regressed on an indicator for assignment to managed care with controls for, and clustering on, the unit of randomization (i.e., recipient's prior provider). Large p-values are expected with random assignment, as they indicate baseline characteristics are comparable between enrollees assigned MMC and FFS. The estimates are based on a balanced panel of 94,976 continuously-enrolled enrollees that were auto-assigned to Medicaid managed care or managed FFS in February 2012 and remained in Medicaid until, at least, December 2015. The joint test reports the p-value of the F statistic that all baseline characteristics are jointly zero from a regression of MMC assignment status on all baseline characteristics. The p-value on the joint test is calculated using randomization inference where treatment labels within each unit of randomization are randomly permuted 1000 times.

	Auto-Assignee Sample				Full Sample		
	\overline{Y}	RF	2SLS 2SLS Post Pharmacy Car		rmacy Carve-in	OLS	
		Level Effect	Level Effect	% Change	Level Effect	% Change	Level Effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total Spending (\$)	1,451	-62***	-82***	-5.6%	-100***	-6.9%	-266***
		(13)	(17)		(20)		(22)
Panel A. Spending by components of care (\$)							
Inpatient Spending	97	3	3	3.4%	1	1.5%	0
		(4)	(5)		(6)		(3)
Outpatient Spending	590	-14^{**}	-19**	-3.2%	-9	-1.5%	-82***
		(5)	(7)		(7)		(8)
Pharmacy Spending	381	-52***	-68***	-18.0%	-94***	-24.7%	-166***
		(7)	(9)		(10)		(14)
Panel B. Spending by enrollee characteristics (\$)							
Female	1,484	-65**	-85**	-5.7%	-102***	-6.9%	-237***
		(20)	(26)		(30)		(23)
Male	1,415	-63***	-84***	-5.9%	-102***	-7.2%	-297***
		(18)	(23)		(25)		(27)
Black	1,280	-53**	-67**	-5.2%	-82**	-6.4%	-186***
		(16)	(21)		(25)		(23)
White	1,812	-54*	-76*	-4.2%	-95*	-5.3%	-329***
		(26)	(36)		(41)		(30)
Panel C. Spending by quartiles of predicted spending (\$)							
0-25%	683	-40**	-47**	-6.8%	-57**	-8.3%	-104***
		(13)	(15)		(19)		(17)
26-50%	941	-33*	-42*	-4.4%	-70**	-7.4%	-93***
		(16)	(21)		(25)		(16)
51-75%	1,331	-84***	-115***	-8.6%	-126***	-9.4%	-171***
		(21)	(30)		(33)		(23)
76-100%	2,851	-126**	-185**	-6.5%	-206**	-7.2%	-363***
		(39)	(56)		(65)		(38)

Table 3: Main results: IV estimates of the effect of managed care on spending

Notes: Table presents sample means, and OLS and IV regression coefficients corresponding to Equation 2, where the regressor of interest, an indicator for enrollment in managed care, is in some specifications instrumented with assignment to managed care. Each row corresponds to a separate regression. In Panel A, the variables listed indicate the dependent variable in the regression. In Panels B and C, the dependent variable is total spending, and the variables listed specify the subsample the regression restricts to. The sample consists of auto-assignees for columns (1) through (6) and adds the active-choosers to the sample for column (7). Percentage columns (4) and (6) are calculated by dividing the effects in (3) and (5) by the average reported in (1). Only post-assignment observations are included (February 2012 to December 2014). Observations are at the enrollee \times year level: N = 284,928 for auto-assignees and N = 413,811 overall. Number of auto-assignees: 94,976. Number of active-choosers: 42,961. All regressions adjust for the unit of randomization (i.e., recipient's prior provider). Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details). Components of spending do not sum up to "Total" due to winsorization. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). * p < 0.05, ** p < 0.01, *** p < 0.001.

	Auto-Assignee Sample				Full Sample	
	\overline{Y} (1)	RF (2)	2SLS (3)	N (4)	OLS (5)	
Panel A. Any primary care access and preventive care in year (%)						
Primary Care and Preventative Composite	48.16	-0.67* (0.27)	-0.87* (0.36)	284,928	-3.35*** (0.40)	
Annual Well-Child Visits	49.34	-0.56 (0.72)	-0.73 (0.94)	168,313	-4.01*** (1.14)	
Access to Primary Care	80.46	-1.52** (0.54)	-2.00** (0.70)	280,915	-4.60*** (0.64)	
Chlamydia Screening	59.67	0.02 (1.33)	0.03 (1.72)	10,395	-0.52 (1.02)	
Cervical Cancer Screening	67.19	-0.13 (1.52)	-0.16 (1.97)	13,758	-1.74 (1.18)	
Follow-up Care after ADHD Prescription	51.17	1.23 (2.22)	1.77 (3.22)	3,864	-1.38 (1.70)	
Behavioral	7.40	-0.34 (0.18)	-0.44 (0.24)	284,928	-1.54*** (0.23)	
Dental	55.18	-0.13 (0.49)	-0.17 (0.64)	284,928	-3.74*** (0.63)	
Panel B. Any potentially high-value care drug utilization in a year (%)						
Asthma Medication	14.87	-0.66** (0.25)	-0.86** (0.33)	284,928	-2.52*** (0.39)	
Diabetes Medication	0.58	0.14* (0.06)	0.18* (0.07)	284,928	-0.01 (0.04)	
Panel C. Any potentially low-value care in a year (%)						
Any Low-Value Care Composite	0.92	-0.07 (0.04)	-0.09 (0.06)	284,928	-0.12** (0.04)	
Avoidable Emergency Department Visits	8.43	0.89*** (0.19)	1.17*** (0.27)	284,928	0.93*** (0.14)	
Imaging	23.33	-0.15 (0.28)	-0.19 (0.38)	284,928	-1.90*** (0.29)	
Panel D. Consumer satisfaction (relative to FFS)						
Share of enrollees in their assigned plan (%)	93.02	-14.54*** (3.28)				

Table 4: Impacts on quality and consumer satisfaction

Notes: Table presents sample means, and OLS and IV regression coefficients corresponding to Equation 2, where the regressor of interest, an indicator for enrollment in managed care, is in some specifications instrumented with assignment to managed care. Each row corresponds to a separate regression, with the dependent variable listed in the row label (left). The sample size, listed in column (4), differs across rows because only a subset of the sample would be clinically eligible or "at risk" for certain outcomes. Sample consists of auto-assignees for columns (1) through (4) and adds the active-choosers to the sample for column (5). Only post-assignment observations are included (February 2012 to December 2014). All regressions adjust for the unit of randomization (i.e., recipient's prior provider). Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). * p < 0.05, ** p < 0.01, *** p < 0.001.

	Main Effect		Decomposition				
	\overline{Y}	2SLS	Price	Quantity			
	(1)	(2)	(3)	(4)			
Panel A. Spending by components of care (\$)							
Total Spending	1,451	-81.5	-24.7	-56.8			
		(-117.9, -43.9)	(-34.2, -17.4)	(-91.0, -20.0)			
Inpatient Spending	99	3.1	1.9	1.2			
		(-6.7, 14.9)	(-1.0, 4.4)	(-8.2, 13.7)			
Outpatient Spending	590	-18.6	-16.4	-2.2			
		(-30.9, -4.9)	(-20.6, -12.0)	(-15.0, 12.3)			
Pharmacy Spending	380	-68.7	-7.2	-61.4			
		(-89.6, -51.3)	(-12.7, -3.3)	(-80.4, -44.3)			
Danal P. Dhanmaan anandina bu tuma of duna (¢)							
Brand Drug Spending	228 22	-65.8	1 4	-67 2			
bland blug spending	220	(-80.7, -51.5)	(-1.5, 4.3)	(-81.8, -53.1)			
Generic Drug Spending	149	-3.5	-8.6	5.1			
		(-11.8, 2.8)	(-12.0, -6.1)	(-1.6, 11.3)			

Table 5: Price quantity decomposition

Notes: Table presents sample means and IV regression coefficients corresponding to Equation 2, where the regressor of interest, an indicator for enrollment in managed care, is instrumented with assignment to managed care. Column 1 lists means of dependent variables. Each cell in columns 2-4 corresponds to a separate regression, with the dependent variable listed in the row label. The IV specification for the non-repriced data (from column 3 of Table 3) is repeated in column 2 for comparison; small differences in the estimates reflect the additional, minor, sample restriction here to claims with enough information to standardize prices used in columns 3–4. The identification of price and quantity components are described in Appendix Section D.2.1. The sample consists of auto-assignees. Only post-assignment observations are included (February 2012 to December 2014). Observations are at the enrollee-year level: N = 284,716. Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details). Components of spending do not sum up to "Total" due to winsorization. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). 95% confidence intervals are obtained using 1000 cluster bootstrapped replicates and shown below estimates in parentheses (See Appendix D.3).

	Ν		Spending (\$)		Willingness-to-Stay	
	FFS	MMC	\overline{Y}	RF	\overline{Y}	RF
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Heterogeneity in effects before and after the carve-in of ph	armacy to i	managed ca	ıre			
Pre-carve-in period (9 months post-assignment, annualized)	354,231	500,553	1,390	-31*	93.9	-11.3***
				(14)		(3.0)
Full study period (3 years post-assignment)	118,077	166,851	1,451	-62***	93.0	-14.5***
				(13)		(3.3)
Panel B. Effects by auartile of enrollee exposure to managed care denial regime based on enrollee-level drug utilization pre-carve-in						
0-25%	21,018	29 <i>,</i> 529	974	-30	92.9	-12.6***
				(18)		(3.1)
26-50%	21,594	28,950	1,222	-13	92.6	-15.3***
				(20)		(4.2)
51-75%	21,012	29,529	1,668	-49	92.4	-19.3***
				(30)		(4.0)
76-100%	20,628	29,913	3,321	-192***	90.9	-20.5***
				(50)		(4.2)

Table 6: Tradeoffs: heterogeneity in the effects of managed care on spending and satisfaction

Notes: Table presents sample means and RF regression coefficients related to Equation 2, where the regressor of interest is an indicator for assignment to managed care. Columns 1 and 2 lists the number of observations; Row 1 of Panel A uses monthly data whereas all other rows use yearly data. Each cell in columns 4 and 6 corresponds to a separate regression, with the dependent variable listed in the row label. Column 3 presents overall means for spending as in Table 3 whereas column 5 presents FFS means as in Panel D of Table 4. Panel A compares time. Panel B creates an "exposure to MMC denial regime" using pre carve-in pharmacy spending per ATC-4 multiplied by the corresponding MMC denial rates (from the peak period, just like for our dose-response figures, see Panel B in Figure 6), broken into four quartiles. We see that most of the spending effect is concentrated in the highest quartile, whereas the WTS effects is monotonically decreasing. The sample consists of auto-assignees. Only post-assignment observations are included (February 2012 to December 2014). Observations are at the enrollee-year level: N = 284,716. Total annual spending is winsorized at \$25,000. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). * p < 0.05, ** p < 0.01, *** p < 0.001.

For Online Publication

Appendix for: The Private Provision of Public Services: Evidence from Random Assignment in Medicaid

A Medicaid managed care in Louisiana

A.1 Public vs. private provision of Medicaid

In 2012, the state of Louisiana transitioned its Medicaid fee-for-service program to a mandatory Medicaid managed care (MMC) program with a blend of full-risk Medicaid manage care and a managed fee-for-service (FFS) program known as enhanced primary care case management (ePCCM). There were three full-risk MMC plans and two ePCCM plans, which we refer to as "FFS" plans.

The MMC plans received a prospective, monthly risk-adjusted capitation payment (averaging \$263 per member per month) to cover a wide range of contracted services for their Medicaid enrollees. The FFS plans receive a small monthly fee (approximately \$11 per member per month) to cover the costs of coordinating care and contracting directly with primary care providers (PCPs). However, services other than primary care were coordinated by the FFS plan but provided via the state's legacy FFS network and paid directly by the state. The FFS plans were technically eligible to share up 20% of savings depending on performance, but in practice both plans received less than \$5 per member per month in shared savings payouts.

Payment to the full-risk and shared savings plans could be affected by plan performance on five quality measures focusing on adult access to preventive/ambulatory health services, diabetes care, chlamydia screening, and well child and adolescent visits. For the full-risk plans, the state could deduct up to 0.5% of the monthly capitation payment for each of the measures that did not meet a benchmark.

A.2 Auto assignment in Louisiana

Mandatory MMC was required only for specific Medicaid eligibility categories within Louisiana at the time of transition. The two major eligibility groups mandated into MMC during our study period were Low-Income Families & Children (LIFC) and Families & Children. The LIFC category provides eligibility to children and families that meet the eligibility requirements defined by the Aid to Families with Dependent Children (AFDC) state plan that was in effect since July 16th, 1996. Through different programs, the Families and Children eligibility category provides coverage to pregnant women, parents or caregivers of a child under age 19, children under age 19, or women who need treatment for cervical or breast cancer with coverage allowable for income up to 250% of the federal poverty level depending upon the specific program.

Mandatory MMC was phased in region-by-region in Louisiana. Eastern Louisiana (which contains New Orleans), the first region to use MMC, is the subject of our study. This region underwent the transition in February 2012. On December 15, 2011, enrollees in this region received written notice of the switch to MMC and were given 30 days to choose a plan. A series of outbound calls were made to enrollees to remind them to make a decision (if they had not already done so). A person was automatically allocated to one of the five active plans if a decision was not made within 30 days of the initial packet being provided.

At the time of the switch to MMC, the state's auto-assignment algorithm gave priority to three goals: preserving existing provider relationships, keeping families together, and balancing auto-

assignee across plans. Because of this, not every auto-assignment was random. For instance, beneficairies with family members in a plan at the time of assignment were automatically assigned to their family members' plan (rather than at random). We remove these non-random assignments from our sample. The second goal, maintaining previous provider ties, also creates a challenge. To account for this conditional randomization (enrollees' providers did not necessarily participate in all plans), all models control for the unit of randomization, an enrollee's 2011 linked PCCM provider, and cluster standard errors at that level to allow for correlation among enrollees with the same 2011 PCCM provider.

B Data

B.1 Administrative data and outcomes

We use our administrative data to construct a series of outcomes including enrollee spending, utilization of medical services and drugs, healthcare quality (including avoidable hospitalizations) and plan satisfaction through a "willingness-to-stay" measure. We briefly describe the details of these outcomes below.

- Winsorized spending outcomes. Total annual healthcare spending is winsorized at \$25,000. For the auto-assignment population this corresponds to the 99.77th percentile of spending. For consistency, when examining different components of care (inpatient, outpatient, pharmacy) or spending aggregated to shorter time intervals (monthly or quarterly) we winsorize each at the spending level defined by the 99.77th percentile of that distribution. For the plan transition experiment we also winsorize total annual spending at \$25,000. This corresponds to the 98.83rd percentile of the spending distribution for this population, so any different aggregations of spending for this experiment are winsorized at the 98.83rd percentile of that distribution.
- Annual well-child visits. Percentage of children (3-6 years old) and adolescents (12-21 years old) who had at least 1 comprehensive well-care visit with a PCP during the measurement year.
- Access to primary care. This is modeled on the children and adolescents access to primary care Healthcare Effectiveness Data and Information Set (HEDIS) measure. It is the percentage of children and adolescents ages 12 months to 19 years who had a visit with a primary care practitioner (PCP). The HEDIS measure reports four separate percentages, stratified by age, which we combine into a single outcome:
 - Children ages 12 to 24 months and 25 months to 6 years who had a visit with a PCP during the measurement year.
 - Children ages 7 to 11 years and adolescents 12 to 19 years who had a visit with a PCP during the measurement year or the year prior to the measurement year.
- **Preventive care measures.** We followed the HEDIS measure sets commonly used to evaluate health plan performance in Medicaid to evaluate the receipt of recommended services for preventative care and acute and chronic conditions:
 - *Cervical cancer screening.* Percentage of women ages 24 to 64 who were screened for cervical cancer. Eligible population: women 24 -64 years old. Excludes women who have a history of hysterectomy with no residual cervix, cervical agenesis, or acquired absence of cervix.
 - *Chlamydia screening in women.* Percentage of women ages 16 to 24 who were identified as sexually active and who had at least one test for chlamydia. Eligible population: women 16 to 24 years old who are identified as sexually active during the year.

- Follow-up care for children prescribed ADHD medication. Percentage of children newly prescribed attention-deficit/hyperactivity disorder (ADHD) medication who had at least three follow-up care visits within a 10-month period, one of which was within 30 days of when the first ADHD medication was dispensed.
- **Drug classification.** We assign each drug to an ATC-4 therapeutic class using the Anatomical Therapeutic Chemical (ATC) Classification System, which provides a way to identify drugs that are clinical substitutes (Ganapati and McKibbin, 2019; Dubois, Gandhi and Vasserman, 2019). The ATC system classifies the active ingredients of drugs according to the organ or system on which they act as well as their therapeutic, pharmacological, and chemical properties. Drugs are classified at five different levels. We use the ATC 4th level (e.g., fast-acting insulins and analogues for injection) to classify drugs into a therapeutic class. For asthma medications we exclude R03BA, glucocorticoids.
- **Behavioral and dental healthcare utilization.** We evaluate whether enrollees have any utilization of behavioral health or dental services within a year. We rely on a state-specific typology to identify claims associated with these services and create indicator variables for enrollees set to one if they have at least one claim for a particular service in a given year, and zero otherwise.
- Avoidable emergency department use. This measure captures emergency department (ED) utilization for low-acuity services that could be treated in another ambulatory setting (Medi-Cal Managed Care Division, 2012).
- Low-value care composite We create a monthly composite measure of low-value care, which measures if there was any instance of low-value among the following categories: head imaging for uncomplicated headaches, head imaging for syncope, simultaneous brain and sinus CT scan, thorax CT combined studies, CT scan for acute uncomplicated rhinosinusitis, abdomen CT combined studies, arthroscopic surgery for knee arthritis, EEG for headaches, imaging for non-specific low back pain, spinal injections for back pain and imaging for diagnosis of plantar fasciitis. We invert the HEDIS measure "appropriate treatment for upper respiratory tract infections" to obtain a low-value care measure of inappropriate prescribing of antibiotics for treatment of upper respiratory tract infections.
- **Imaging.** We identify healthcare claims that involve imaging using Berenson-Eggers Type of Service (BETOS) codes.
- **Primary care and preventative composite.** We create a composite member for each enrollee, which measures the overall share of primary care and preventative outcomes that an enrollee receives. The composite averages binary outcomes across annual well-child visits, access to primary care, cervical cancer screening, chlamydia screening in women, follow-up care for children prescribed ADHD medication, behavioral utilization and dental utilization. As some measures only apply to specific populations, for each enrollee × year the denominator is the set of eligible measures for that enrollee × year.

B.2 Predicting enrollee health spending (i.e., risk) using enrollee baseline characteristics

To predict enrollee health status we estimate a cross-validated LASSO regression with mean annual post-assignment healthcare spending (in the 3 years after random assignment) as the outcome and use a set of demographic and baseline utilization measures as predictors. For demographics, we use enrollees' Medicaid eligibility category, ZIP code, race, five year age by gender bins, and an indicator for whether they were an "auto assignee" or "active chooser." In addition to these predictors, we

use indicators for the 700 most common baseline diagnosis codes (those obtained by enrollees at any time in the 12 months prior to assignment), baseline medical spending, and baseline pharmacy spending. The baseline spending variables are z-score normalized because they are continuous and on a different scale than the binary indicators which can lead to problems in LASSO estimation.

C Robustness and external validity

In this section we present three separate analyses to explore the robustness and external validity of the main results. First, we present plan transition estimates for a different sample which allows for plan switching after the plan transition. Second, for external validity we re-weight the autoassignee sample to better represent the observational Medicaid population which selected plans, and the generally older population of the plan transition experiment. Finally, we re-weight the autoassignees to resemble a much older Medicaid expansion population to extrapolate the effects to this important population which did not exist at the time of the initial experiment.

C.1 Robustness of plan transition estimates

The primary plan transition sample excludes enrollees that switched between plans during the 2-year study period. We assess the robustness of our results to an alternative sample of enrollees continuously enrolled from Feb 2014 to Jan 2016 who were permitted to switch plans after the plan transition occurred in February 2015. For this alternative sample, we define the treatment assignment based on their pre-transition plan.

Figure A20 illustrates the rate of switching within this sample after the plan transition. 12 months after the plan transition most enrollees remain in the plan they were enrolled with before the transition (89.0% in the transitioned plan and 93.4% on average across the three control plans). Though the rate of switching is greater within the transitioned plan, given the overall low rate of switching among enrollees who remain in the same plan for a year before the transition, the potential impact of this exclusion in our main specification is greatly minimized.

Table A15 replicates the plan transition estimates within this sample which allows for switching and reports those estimates alongside the re-weighted estimates in Table A9 from the sample where switchers were excluded. Results between the two samples are entirely consistent, though the level of significance of some estimates changes (inpatient spending, primary care and preventative composite, and annual well-child visits).

C.2 Re-weighting samples

C.2.1 Active choosers and plan transition populations

In order to investigate external validity and facilitate a comparison of our plan transition (PT) experiment and auto-assignment (AA) experiment, in some analyses we re-weight our samples on three dimensions: gender, age-buckets (0-5, 6-18, 18+) and 3M Clinical Risk Group (CRG), which use an enrollee's prior claims history to categorize their severity of illness.

- Auto-assignment experiment. In the auto-assignee experiment (presented in Section 4) we examine the external validity of our estimates based on the auto-assignee sample by re-weighting the auto-assignee population to balance its characteristics with those of the active chooser population. Table A16 presents our re-weighted results.
- **Plan transition experiment.** In the plan transition natural experiment, we lead with estimates that re-weight the PT sample to balance its characteristics with those of the AA sample.

Because of strong joint support between the different samples, and the coarseness of our re-weighting cells, only 4 (0.004%) of the auto-assignee enrollees cannot be assigned a weight when re-weighting to match the characteristics of the active choosers and fewer than 0.2% of the enrollees in the PT natural experiment cannot be assigned a weight when re-weighting to match the characteristics of the auto-assignee sample.

C.2.2 Post-expansion Medicaid population

The main study period of this paper was prior to the Affordable Care Act's (ACA) Medicaid expansion. In this section, we describe the age distribution in more detail and discuss external validity with respect to a current Medicaid population that would include more adults.

Louisiana expanded coverage to its adult population in July 2016, after our study period. We identify the post-ACA population as all beneficiaries who are enrolled in December 2016 (the latest period post-ACA for which we have administrative enrollment data) and belong to the following eligibility groups: Families & Children, Low-Income Families & Children, or Expansion. Thus, this population includes the same eligibility categories as the auto-assignee population, with the sole addition of the ACA expansion members. As in the auto-assignee sample we limit to enrollees age 65 and younger who have no history of home-health claims.

Figure A21 shows the distribution of age and gender for this post-ACA population relative to the auto-assignee population. On average, the post-ACA population is older than the auto-assignees (20.5 years compared to 9.4 years) and the post-ACA population has a much larger share of enrollees over the age of 18 – 44.5% of the population compared with just 5% for the auto-assignees. We reweight the auto-assigned sample on three dimensions: sex, 3M Clinical Risk Group, and age-buckets. We bin age into 5-year intervals between 0-65 to better capture the older post-ACA population distribution.

Table A17 reproduces the results of Table 3 with the auto-assignees re-weighted to the post-ACA population. After re-weighting the average annual spending increases to \$2171 annually from \$1451 in the unweighted sample, a 50% overall increase, with inpatient spending having the largest percent increase due to re-weighting. However, none of the estimated effects of MMC are significant. We attribute this to lack of precision. For example, in our main analysis for total spending the 2SLS standard errors are 1.4% of the population average, allowing us to detect relatively small and precise differences. In this re-weighted analysis the 2SLS standard errors are 8.2% of the overall population average, reducing our ability to detect significant effects by nearly a factor of 6.

Ultimately, because our auto-assignee sample is young and we lack significant overlap with the age distribution of an older post-ACA expansion population, the reported estimates are too noisily estimated to draw strong conclusions.

D Decomposition of spending reductions

D.1 Decomposition of pharmacy spending

Both sources of identifying variation (across Sections 4 and 5) showed that spending reductions were largely associated with prescription drug coverage. And Figure 3 showed that substitution from brand to generic drugs was important. But exactly how much do price, quantity, and substitution effects—in drugs and elsewhere—account for in the overall spending differences between MMC and FFS? In this section, we decompose managed care's impact on spending into four mutually-exclusive components. The first component is provider price differences, which applies to all products and services. The second and third are focused on drug spending. These are steering *within* brand or generic drugs to lower cost therapies (within narrow therapeutic classes), and steering *from* brand *to* generic

drugs (also within narrow therapeutic classes). A residual captures outright quantity reductions and quantity substitutions to lower-cost procedures or drug therapies.

D.1.1 Decomposition of pharmacy spending framework

Our decomposition approach follows Brot-Goldberg et al. (2017). We begin by restricting to services we observe at least 5 times in both MMC and FFS in each year. We also exclude any cost associated with claim lines that are missing service codes.¹ This ensures that we are examining a consistent set of procedures and drugs for which we can measure price in both MMC and FFS. With these restrictions, we retain 86% of the spending represented in our main auto-assignee analysis sample.²

To explain the observed reductions in spending for enrollees assigned to managed care relative to FFS ($\Delta TS_{MMC,FFS}$), we decompose the total spending differential into price and quantity terms:

$$\Delta TS_{MMC,FFS} \equiv \Delta P_{MMC,FFS} + \Delta Q_{MMC,FFS}.$$
(5)

The total spending differential, $\Delta TS_{MMC,FFS}$, is defined to be equal to our IV estimate, $\hat{\beta}^{TS}$, which is expressed in Equation 2 and reported in Table 3. $\hat{\beta}^{TS}$ is the expected spending difference in dollars between a person randomly assigned to MMC in place of FFS. The superscript *TS* is added to the coefficient to make explicit that the estimate corresponds to a regression in which total spending is the dependent variable.³ The price term $\Delta P_{MMC,FFS}$ captures the extent to which spending differences are driven by MMC plans paying lower prices than FFS for the same service at the same provider or by MMC plans steering enrollees toward lower priced providers for the same services. The quantity term, $\Delta Q_{MMC,FFS}$, is the causal effect of managed care on overall quantity (i.e., pricenormalized healthcare consumption), which includes outright quantity reductions and changes to the composition of services.

We start by isolating the price term, and then further decompose the quantity term. To estimate the price component, we reprice claims so that all claims that share the same service code \times year have the same price. We assign these prices using estimated service code fixed effects v_{dt} from a regression run at the claim-line level in which price is the dependent variable:

$$P_{dct} = \alpha + \nu_{dt} + \pi Assigned Managed Care_{ct} + \mu_{dct}$$
(6)

 P_{dct} indicates the price per unit paid⁴ in year *t* for service code *d* (i.e., individual procedure codes, NDCs, RxCUIs, ATC-4 therapeuric classes) on individual claim record *c* in our data. The above regression is weighted by the units on each claim⁵. *AssignedManagedCare_{ct}* indicates the relative price level of MMC to the FFS option in year *t*. If the data generating process underlying prices consisted of each model determining prices as a constant-multiple markup for all services relative to some

¹Some claims are paid very small amounts, i.e. \$0.01. Our estimates are unchanged if we remove any claims that cost less than \$1.00 as these represent a very small number of claims and spending.

²For these analyses, we use our primary sample of 94,976 enrollees randomly assigned on February 1st 2012. We omit January 2012 as treatment starts in February and annualize the remaining 11 months. We include calendar year 2011—i.e., data from one year prior to assignment to MMC or managed FFS when all enrollees were in legacy FFS—as an additional balance check.

³The decomposition can be recast in terms of percentage reductions relative to FFS spending by dividing all terms by the FFS spending level: $\frac{TS_{MMC}-TS_{FFS}}{TS_{FFS}}$. \overline{TS}_{FFS} is mean total spending for enrollees *assigned* to the FFS option.

⁴For inpatient claims, each claim is assigned a single unit. For outpatient and pharmacy claims, the number of units is defined as the number of services per claim and number of days supplied per claim respectively.

⁵As a robustness check for differential reporting of units across models, we reprice claims at the claim level and use analogous regressions to the "per-unit" version. We do not see any significant differences relative to the "per-unit" version and conclude that our decomposition results are robust to differential reporting of units. Results available upon request.

common index price for each service (such as the FFS Medicaid price), then *AssignedManagedCare*_{ct} would exactly recover that markup.

To reprice the claims, we use predicted values from this regression, assigning a common price across models for each code group. This common price is set to equal $(\alpha + v_{dt}) \times units_c$ — the code group fixed effect plus the constant, multiplied by the number of units in each claim *c*. This ensures that all per year per unit prices within each code group are identical within and across models such that the only difference between models is the number of units they administered, i.e. quantity.

Note, the difference-in-differences setting from Section 5 uses the following repricing regression:

$$P_{dct} = \alpha + \nu_{dt} + \pi TransitionedPlan_{ct} + \mu_{dct}$$
(7)

where $Treatment_{ct}$ is an indicator variable set to one if claim c is part of the treatment group in year t and zero otherwise. This difference is to account for the difference-in-differences variation and specification. The rest of decomposition is identical for both experiments (Plan Transition and Auto-Assignment).

After repricing all claims in our data, we regress the new price-standardized⁶ version of the healthcare spending variable $(Y_{ijt}^{\overline{P}})$ using Equation 2 and recover $\hat{\beta}^{\overline{P}}$, where the superscript \overline{P} indicates a regression that holds prices fixed. In this estimate of the spending difference between MMC and FFS, prices are equalized, so the total spending differences can only be attributable to differences in the number and composition of services—i.e. quantity.

Following Brot-Goldberg et al. (2017), the difference between the main estimate (without repricing), $\hat{\beta}^{TS}$, and the coefficient from the repriced regression, $\hat{\beta}^{\overline{P}}$, yields the contribution of price differences to overall spending differences. Rearranging Equation 5 and substituting gives:

$$\Delta P_{MMC,FFS} = \Delta T S_{MMC,FFS} - \Delta Q_{MMC,FFS} = \hat{\beta}^{TS} - \hat{\beta}^{P}.$$
(8)

Focusing on prescription drug utilization—which drives the overall spending effects and for which substitutes are clinically well-defined—we further decompose the quantity effect $(\hat{\beta}^{\overline{P}})$ into three mutually exclusive components. These are defined precisely below and represent (i) a drug steering effect $\Delta Q_{MMC,FFS}^{Steering}$, which captures substitutions among the brand drugs in a therapeutic class or among the generic drugs in a therapeutic class; (ii) a brand-generic drug substitution effect $\Delta Q_{MMC,FFS}^{Generic}$, which captures substitutions between brand and generic drugs within a therapeutic class; and (iii) a residual $Q_{MMC,FFS}^{R}$. This last term includes outright quantity reductions (or increases) and other substitutions unaccounted for by within-class substitutions (or substitutions away from drugs towards medical therapies). For example, this term would capture spending differences due to substitution between ACE inhibitors and beta-blockers in the treatment of high blood pressure. The four terms decompose $\Delta TS_{MMC,FFS}$ as follows:

$$\Delta TS_{MMC,FFS} = \underbrace{\Delta P_{MMC,FFS}}_{\text{Price diffs. in}} + \underbrace{\Delta Q_{MMC,FFS}}_{\text{Steering within}} + \underbrace{\Delta Q_{MMC,FFS}}_{\text{Substitution from}} + \underbrace{\Delta Q_{MMC,FFS}}_{\text{Residual quantity}}$$
(9)

To recover the terms of this decomposition, we sequentially estimate our main IV specification (Equation 2) on alternative constructions of the dependent variable. To recover the steering component, we assign each drug to an ATC-4 therapeutic class using the Anatomical Therapeutic Chemical (ATC) Classification System, which provides a way to identify drugs that are clinical substitutes

⁶As in Table 3, overall enrollee-year spending is Winsorized at \$25,000 whereas other spending measures are winsorized at the corresponding percentile.
(Ganapati and McKibbin, 2019; Dubois, Gandhi and Vasserman, 2019).⁷ We then reprice all pharmacy services at the therapeutic class × brand/generic level, so that all generic drugs within an ATC-4 are assigned the same price and all brand drugs within an ATC-4 are assigned the same price. We then re-aggregate the repriced claims to construct an alternative measure of repriced enrolleeyear level spending, $Y_{ijt}^{\overline{Steering}}$. We use this as the dependent variable in the Equation 2 regression to recover $\hat{\beta}^{\overline{Steering}}$, the reduction in spending due to managed care that is *not* due to steering towards substitutes within generics or brands in a therapeutic class (i.e., that is not due to shifts from high to low-cost brand or generic drugs within an ATC-4). Note that $\hat{\beta}^{\overline{Steering}}$ also zeroes-out any MMC-FFS price difference for the same product because it zeroes out MMC-FFS price differences for the *entire set* of products in the same ATC-4 × brand/generic grouping. With this estimate, we can isolate the effect of drug steering as the difference between the overall (price-normalized) quantity effect ($\hat{\beta}^{\overline{P}}$) and the estimate that additionally zeros-out the contribution of steering ($\hat{\beta}^{\overline{Steering}}$):

$$\Delta Q_{MMC,FFS}^{Steering} \equiv \hat{\beta}^{\overline{P}} - \hat{\beta}^{\overline{Steering}}.$$
(10)

Reductions in pharmacy spending may also come from enrollees in managed care substituting from brand to generic drugs. This may either be for an identical molecule or a related drug within the same narrow therapeutic class. To assess this contribution, we reprice all pharmacy claims within an ATC-4 (brand and generic) to equal the average price within an ATC-4. From this we construct an alternative measure of repriced enrollee-year level spending, $Y_{ijt}^{\overline{Generic}}$. Estimating Equation 2 with this as the dependent variable recovers $\hat{\beta}^{\overline{Generic}}$, which is the reduction in spending caused by managed care that is *not* due to price differences for the same product, drug substitutions within brand/generic groups in a therapeutic class, or brand-generic substitutions within a therapeutic class. Subtracting this from $\hat{\beta}^{\overline{Steering}}$ (which zeroes-out price differences price differences for the same product, drug substitutions within brand/generic groups in a therapeutic class. but *not* brand-generic substitutions within a therapeutic class) isolates the effect of brand-generic substitution:

$$\Delta Q_{MMC,FFS}^{Generic} \equiv \hat{\beta}^{\overline{Steering}} - \hat{\beta}^{\overline{Generic}}.$$
(11)

Finally, the $\hat{\beta}^{\overline{Generic}}$ coefficient—considered alone—measures the final term of the quantity decomposition, $\Delta Q^R_{MMC,FFS}$. This is a residual that captures both outright quantity reductions and quantity substitutions between services.

D.1.2 Decomposition of pharmacy spending results

Table A12 presents the decomposition results for the auto-assignee experiment and sample. The overall effects to be decomposed are similar to the instrumental variable results on overall spending in the first row of Table 3, except that we generate results separately for each year from 2011 to 2014, estimating Equation 2 over sub-samples defined by year to document how the effects of managed care evolve over time.⁸ Recall that the carve-in of prescription drugs occurred in November 2012. Hence, the first full year that MMC plans managed prescription drugs was 2013 (t_1). We present results for the year prior to assignment (i.e., t_{-1}) to illustrate that enrollees assigned to managed care did

⁷The ATC system classifies the active ingredients of drugs according to the organ or system on which they act as well as their therapeutic, pharmacological, and chemical properties. Drugs are classified at five different levels. We use the ATC 4th level (e.g., fast-acting insulins and analogues for injection) to classify drugs into a therapeutic class.

⁸Results for t_0-t_2 (i.e., 2012-2014) use the instrumental variables approach in Table 3. Because 2011 (i.e., t_{-1}) is a preassignment period, estimates for that year are based on estimating a reduced form version of Equation 2, comparing the outcomes for enrollees *eventually assigned* to MMC versus FFS, but who have not yet been assigned or enrolled.

not have lower healthcare spending prior to assignment. To facilitate interpretation of magnitudes, results in Table A12 are scaled as percentage changes by dividing the estimates from Equations 8 through 11 by the mean FFS spending in the indicated category (total, medical, or pharmacy).

The first column presents differences in total healthcare spending (in percentage terms) between managed care and the FFS option that are consistent with analyses presented in Figure 2 and Table 3. After prescription drugs were carved into managed care we find that the MMC plans generated substantial reductions in total healthcare spending, ranging from about 7.49 to 8.52% over 2013–2014. Managed care generated a smaller reduction of 4.8% in spending in 2012 (t_0) when managed care plans were only responsible for prescription drugs for two months of that year. Consistent with evidence in Section 4.1, we observe large reductions in pharmacy spending after carve-in and modest reductions in medical spending throughout the post-assignment period.

The second column, $\Delta P_{MMC,FFS}$, examines the role of prices paid to providers. The effect of provider prices on total healthcare spending (medical and pharmacy together in *Panel A*) is fairly small for each year of the post-assignment period, ranging from -0.67% in t_2 to -2.62% in t_1 . The effect of drug steering within sets of generic or brand substitutes ($\Delta Q_{MMC,FFS}^{Steering}$) is also modest, at most -2.44% of pharmacy spending in t_2 . By comparison, the contribution of steering away from brands towards generics in the fourth column ($\Delta Q_{MMC,FFS}^{Generic}$), demonstrates that one of the main reasons for the reduction in pharmacy spending in managed care was quantity substitutions to generics within narrow therapeutic classes. In the period after pharmacy was carved into managed care, there were large quantity substitution effects for drugs, ranging from -8.61% in t_1 to -11.22% in t_2 of pharmacy spending, about half of the overall pharmacy effect (-25.48%).⁹ The final column ($\Delta Q_{MMC,FFS}^R$) is the residual; it captures both outright quantity reductions and quantity substitutions to lower-cost drugs in different therapeutic classes and to other procedures.

Figure A12 summarizes the decomposition in Table A12 and adds the analogous results decomposing estimates from the plan transition identification strategy. The results are qualitatively similar between the two distinct identification strategies and samples: Spending reductions are driven primarily by quantity substitutions and outright reductions, rather than price, and, are concentrated in pharmacy spending. Figure A12 demonstrates that, within pharmacy, spending reductions by therapeutic class were strikingly similar across the two different identification strategies.

D.2 Decomposition of components of care

Similar to the pharmacy decomposition, we provide two formal decompositions of managed care's impact on spending. The first decomposition, shown in Table 5, decomposes the overall effect into price and quantity components. A second decomposition, shown in Table A11, decomposes the overall effect into network and provider components.

D.2.1 Price-quantity decomposition framework

For each component of care, *S*, we decompose the spending differential between managed care and FFS into a price and quantity term:

$$\Delta TS^{S}_{MMC,FFS} \equiv \Delta P^{S}_{MMC,FFS} + \Delta Q^{S}_{MMC,FFS}.$$
(12)

The spending differential for each component of care, $\Delta TS^{S}_{MMC,EFS}$, is defined to be equal to our

⁹These effects—which comprise the largest component of the decomposition—capture shifts in utilization to generic drugs via two channels: (1) shifts from brand drugs to chemically identical generic drugs within narrow therapeutic classes (e.g., the statin Zocor to its generic equivalent simvastatin); and (2) shifts from brand drugs to chemically-distinct generic drugs within the same narrow therapeutic class (e.g., Zocor to rosuvastatin, which is the generic equivalent of Crestor).

IV estimate, $\hat{\beta}^{S}$, which is expressed in Equation 2. The price term $\Delta P_{MMC,FFS}^{S}$ captures the extent to which spending differences are driven by MMC plans paying lower prices than FFS for the same service at the same provider or by MMC plans steering enrollees toward lower priced providers for the same services. Claims are assigned prices using the same methodology for the decomposition of pharmacy spending (Equation 6)¹⁰. After repricing claims, we regress the new price-standardized version of the spending variable using Equation 2 and recover $\hat{\beta}^{\overline{PS}}$, where the superscript \overline{P} indicates a regression that holds prices fixed. The difference between the main estimate for each component (without repricing), $\hat{\beta}^{S}$, and the coefficient from the repriced regression, $\hat{\beta}^{\overline{PS}}$, yields the contribution of price differences to overall spending differences.

$$\Delta P^{S}_{MMC,FFS} = \hat{\beta}^{S} - \hat{\beta}^{\overline{PS}}.$$
(13)

The quantity term, which is equivalent to the residual after estimating the price channel, is defined by the coefficient from the price standardized regression,

$$\Delta Q^S_{MMC,FFS} = \hat{\beta}^{\overline{PS}}.$$
(14)

D.2.2 Network-Provider decomposition framework

For each component of care, *S*, we decompose the spending differential between managed care and FFS into a network term, a provider term, and a residual term

$$\Delta TS^{S}_{MMC,FFS} \equiv \Delta N^{S}_{MMC,FFS} + \Delta D^{S}_{MMC,FFS} + \Delta R^{S}_{MMC,FFS}.$$
(15)

The network term, $\Delta N^S_{MMC,FFS}$, captures the extent to which differences in the network of contracted primary care providers result in differences in spending (e.g., constraining cost by having a narrower set of allowed providers). $\Delta D^S_{MMC,FFS}$ is a provider term, which measures whether steering to particular providers (responsible for the plurality of an enrollee's care), who may have particular treatment patterns (e.g., referrals or quantity and types of services rendered) results in spending differences. The final term is a residual term, to make the decomposition exact.

To isolate the network component, we use equation 2 with the addition of a linear adjustment for the primary care network breadth in the ZIP code in which the enrollee resides. From this regression we obtain $\hat{\beta}^{\overline{NS}}$ where the superscript \overline{N} indicates a regression adjustment for network breadth. The difference between the main estimate for each component, $\hat{\beta}^S$, and the coefficient from the network adjusted regression, $\hat{\beta}^{\overline{NS}}$, yields the contribution of network to overall spending differences.

$$\Delta N^{S}_{MMC,FFS} = \hat{\beta}^{S} - \hat{\beta}^{\overline{NS}}.$$
(16)

To identify the provider component, we add fixed effects for the individual NPI responsible for the plurality of an enrollee's care and re-estimate equation 2 to obtain $\hat{\beta}^{\overline{DS}}$ where the superscript \overline{D} indicates a regression that includes an adjustment for the enrollee's primary doctor. The provider component is calculated as

$$\Delta D^{S}_{MMC,FFS} = \hat{\beta}^{S} - \hat{\beta}^{\overline{DS}}.$$
(17)

The residual term accounts for price and quantity effects beyond those captured by the network and provider terms and any interactions between network and provider effects that aren't accounted for independently. This term makes the decomposition exact,

¹⁰For inpatient claims we group services by the Diagnosis Related Group (DRG) and for non-inpatient medical claims we use the HCPCs or CPT code on each claim line.

$$\Delta R^{S}_{MMC,FFS} = \hat{\beta}^{\overline{DS}} + \hat{\beta}^{\overline{NS}} - \hat{\beta}^{S}.$$
(18)

D.2.3 Construction of primary care provider network breadth

To measure the breadth of a plan's primary care provider network in each ZIP code, we take into account the number of in-network primary care providers for the plan, where those providers are located, and what the distribution of patient preferences over those (and other) providers looks like. To do this, we build on the pioneering work of Ericson and Starc (2015) and Wallace (2023). A key insight in these papers is that enrollee preferences over providers lead to patient flows which, when observed in the data, allow researchers to recover enrollee preferences and use them to model provider demand. Another insight of these papers is that simple models of provider network breadth based on realized patient flows yield very similar measures of network breadth to more complex methods that estimate provider demand systems and recover measures of provider network breath (Wallace, 2023). In light of this insight, we opt for the simpler approach in this paper and construct a measure of primary care provider network breadth at the plan-by-year-by-ZIP code level as the fraction of primary care visits—with primary care visits defined as visits involving primary care providers (i.e., internal medicine, family medicine, pediatrics, obstetrics and gynecology, or general practice physicians)for enrollees living in a given ZIP code covered by each plan's network. We pool healthcare claims for the period (01/2012 to 12/2014) to construct this measure. Intuitively, the measure varies across plans and ZIP codes based on systematic differences in where enrollees in different ZIP codes seek primary care and which providers are in network for each plan.

D.2.4 Provider attribution used in network-provider decomposition

We allow enrollees to be attributed to a different provider each year. To satisfy joint-support requirements, we restrict to the set of providers that have at least 5 enrollees attributed to them in both the MMC and FFS models during the the post period (35 months from Feb 2012 to Dec 2014). We attribute each enrollee to their modal provider (each year) based on the number of claims they have with each provider. If two or more modal providers exist, we keep the first provider in our data. If an *enrollee* × *year* observation does not have a mode (i.e., enrollee *i* has no claims in year *t*), we forward and then backward fill within enrollee.¹¹ We are able to attribute 97.1% of auto-assigned enrollees to one or multiple providers in the post period. For enrollees with no claims during the post period and hence no attributed providers—we create a unique fixed effect for the group and include them in our regressions.

In order to verify that attribution to a current provider is not a function of treatment itself, we estimate the following reduced form model:

$$HasProvider_{i} = \alpha + \pi Assigned Managed Care_{i} + \phi_{i}^{p} + \mu_{i}$$
(19)

where $HasProvider_i$ is an indicator variable set to one if enrollee *i* was attributed to a provider; *AssignedManagedCare_i* is an indicator variable set to one if the auto-assignment algorithm assigned enrollee *i* to a full-risk, managed care plan at the time of the program transition in February 2012 and zero otherwise; and ϕ_i^p are fixed effects for each enrollee's provider prior to assignment. In results available upon request, we find that the coefficient on $\hat{\tau}$ is not statistically significant, and small relative to its mean. This suggests that the likelihood we are able to attribute an enrollee to a provider is not correlated with whether they were assigned to managed care or FFS.

¹¹Our findings are not sensitive to the imputation method used. Results available upon request.

D.3 Confidence intervals

We obtain 95% confidence intervals for each component of the decomposition using a bootstrap resampling method. The data has 167 units of randomization so we cluster resample 167 units of randomization with replacement for each of the replicates. Within each replicate, we re-estimate all regressions to obtain the price, quantity, network, provider and residual terms. We define the 95% confidence interval by the 2.5^{th} and 97.5^{th} quantiles of the distribution of each decomposition term estimated from the replicates. The number of replicates is indicated in table notes where bootstrapped confidence intervals are provided.

E Denials matching strategy

The nature of pharmacy denials differs from medical denials in that pharmacy denials are subject to real-time adjudication, i.e., if a claim is denied, the enrollee does not receive the prescribed drug and the claim is usually resolved instantaneously. This means that there is real scope for plans to use denials as utilization management tools.

To dig into this potential mechanism, we create a matching strategy that allows us to trace the path of care, from the initial claim that was denied until a final paid claim, if it exists. Starting with a denied claim in the the three month period following the carve-in of pharmacy benefits (we "washout" November 2012 as it is the transition month) – this is the same study period as for Panel B of figure 6 – we match it to a paid claim, if it exists, using the enrollee's ID, the drug NDC, and the claims' dates. Claims are matched for up to 7 days after their denial.¹². In cases where enrollees have multiple paid claims within 7 days of a denial, the match gives precedence in the following order: (1) same NDC; (2) same ATC-4; (3) same ATC-3; (4) same ATC-2; (5) same ATC-1. That is, we preferentially match denials to paid claims which are for the most similar drug.

Using this matching strategy, we can categorise claims as the following:

- 1. Administrative denials: denied claims that result in a paid claim within 7 days that have the same NDC. Panel B in Figure A16 further conditions by imposing units to differ between the denied and paid claim.
- 2. Substitution denials: these are denied claims that result in a paid claim within 7 days that have a different NDC. No restriction is applied for the units. Panel C in Figure A16 uses these.
- 3. Walk-away denials: these are denied claims for which we were not able to find a paid matching claim.

The number of denials in each category varies as a function of the time elapsed between denied and paid claims. This variation is due to the limitations of our matching strategy: we can not say with certainty if a particular paid claim is indeed a result of the denied claim or if the paid claim just happens to have matched but for a completely different healthcare episode. However, our results are similar when limiting the time elapsed to the same day. It is also the case that most of these subsequent paid claims are at the same pharmacy.

F Medical offsets due to pharmacy utilization management

Starc and Town (2019) "find substantial medical care offsets in MA-PD [Medicare Advantage Prescription Drug] plans: a \$1 increase in prescription drug spending reduces non-drug expenditure by

¹²Because of this, paid claims can be found in the month following the end of the study period, a period for which we still have complete pharmacy data.

approximately 27 cents," so it is possible that the smaller medical spending reductions we find in this paper are the result of a much larger medical reduction which is partially offset by pharmacy utilization management. In this section, we incorporate offsets estimated by Starc and Town to determine what level of (counterfactual) medical spending effects might have been observed if there had not been a large reduction in pharmacy spending.

It is important to caveat that this analysis relies on two main assumptions: (1) that we can transport the results across the different populations, and (2) that the mechanisms by which reductions occur are similar in both settings. Regarding (1), Starc and Town study an older Medicare population with heavy prescription drug utilization (76.87 years old and 1,302 days supply annually), while we study a younger Medicaid population with relatively low prescription drug utilization (9.36 years old and 119 days supply annually). It is possible that the offsets generated across these populations are different given the differing compositions of medical services and drug utilization across the two populations. Regarding (2), the 27% offset in Starc and Town is calculated through an offset of \$0.59 per day supply (with an average prescription cost of \$2.20 per day in their setting). In our context the main mechanism by which managed care reduces pharmacy spending is through substitutions to generic equivalents, not outright quantity reductions. Given this, we translate between their parameters and ours according to reductions in quantity. In particular, we apply their offset of \$0.59 per day supply to the reduction in days supply that we calculate as a causal effect of MMC enrollment.

We calculate the effect of managed care on annual days supply for all drugs and separately for the potentially high-value drug classes using our main 2SLS specification including estimates subset to the two years post pharmacy carve-in (this is to ensure we do not understate managed care effects by including 2012 when pharmacy was only carved-in during the last quarter). As Starc and Town estimate an offset of \$0.59 per day supply (their Section 4.3.3), we multiply these effects by the offset of \$0.59 per day supply to calculate the amount by which our spending results are diminished given this offset. Table A14 shows that managed care has a statistically insignificant negative effect on overall days supply (-3.48 days annually; std. err. = 2.45) when compared to FFS which corresponds to an expected increase in medical costs of \$2.05. This offset is extremely small; by incorporating it we would estimate MMC's true medical effect (absent pharmacy offsets) to be -\$18.72, 12.3% larger than we currently report, and still statistically insignificant and small relative to the annual level of non-pharmacy spending (\$1053). Similarly, using estimates only after pharmacy is carved in we would estimate a \$2.21 offset, such that MMC's true medical effect would be -\$12.41. This estimate is 22% larger than we currently report, but is still statistically insignificant and extremely small relative to the annual level of non-pharmacy spending.

G Additional Figures and Tables



Appendix Figure A1: Assignment to Medicaid managed care vs. FFS did not lead to differential attrition from the Medicaid program

Note: Figure reports on the probability of continued enrollment in Medicaid—in MMC or in FFS—as a function of the coverage model of assignment (i.e., MMC vs. FFS). The sample is restricted to 141,223 enrollees auto-assigned to plans in February 2012. We impose the same sample restrictions as for our primary sample (described in Section 2.6), with the exception of our continuous enrollment restriction, which is not imposed here (hence the larger number of unique enrollees relative to our primary sample). Attrition out of the Medicaid program would imply attrition out of our sample. The figure displays quarterly regression coefficients of the impact of assignment to MMC (relative to FFS) on the probability of continued enrollment in Medicaid. The dependent variables are indicators set to 1 for enrollee-month observations as long as the enrollee is still enrolled in Medicaid, and 0 for all months following an exit from Medicaid, even if the enrollee churns back into the program. Time, in quarters relative to assignment, is along the horizontal axis. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). 95% confidence intervals reported.



Appendix Figure A2: Time series plot of raw spending levels for enrollees assigned to MMC and FFS

Note: Figure presents quarterly enrollee spending, adjusted for prior provider and calendar quarters, for a 4-year period spanning 11 months prior to, and three years after, assignment to managed care for a balanced panel of 85, 668 enrollees. Observations are at the assigned model \times month level. Time, in months, is along the horizontal axis. The leftmost vertical line indicates the start of managed care (the beginning of the treatment period); the rightmost vertical line indicates when pharmacy is carved into Medicaid managed care. The figure shows that spending levels were similar between the groups prior to the assignment to managed care but diverged sharply after pharmacy was carved in to MCO responsibility. Plotted means are residualized on the unit of randomization (i.e., recipient's prior provider), and calendar quarters to adjust for seasonality.



Appendix Figure A3: Impact of assignment to managed care on spending by category

Note: Figure presents difference-in-differences event studies comparing health spending across assignees to MMC and FFS for different categories of service. The vertical axis indicates spending per-enrollee-per-quarter. Estimates are based on a balanced panel of 85,668 continuously-enrolled recipients for the 47 month (February 2011–December 2014) period depicted. Time, in quarters, is along the horizontal axis. The leftmost vertical line indicates the start of managed care (the beginning of the treatment period); the rightmost vertical line indicates when pharmacy is carved into Medicaid managed care. The figure shows the (null) effects of assignment to managed care prior to the treatment period and a large, and precisely-estimated drop in quarterly healthcare spending after assignment to MMC for total, outpatient and pharmacy spending. Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details). Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). 95% confidence intervals reported.



Appendix Figure A4: Individual plan effects on healthcare spending and enrollee satisfaction

Panel A. Plan effects on total spending

Panel B. Plan effects on enrollee satisfaction

Note: Figure presents individual plan effects and associate 95% confidence intervals for total annual health care spending (left) and consumer satisfaction (right) estimated from the auto-assigned sample. Observations are at the enrollee \times year level: N = 284,928 for auto-assignees. Number of auto-assignees: 94,976. Spending effects are estimated using the IV-specification using the randomly assigned plan as an instrument for a member's actual plan. Satisfaction effects are estimated using a reduced form specification using only an enrollee's assigned plan. To improve precision when estimating individual plan effects we include the following enrollee characteristics which are determined at baseline (the year prior to randomization): sex, race, indicators for age coarsened to 5-year increments and baseline health status measured through clinical risk groups. Total annual spending is winsorized at \$25,000. All regressions adjust for the unit of randomization (i.e., recipient's prior provider). Standard errors clustered on the unit of randomization (i.e., recipient's prior provider).



Appendix Figure A5: 2SLS spending results stratified by inpatient utilization and age

Note: Figure presents two coefficient plots of the causal effects of MMC, and associate 95% confidence intervals, on inpatient spending (left-hand side) and total spending (right-hand side). The left-hand groups the auto-assignee population by baseline inpatient spending. The right-hand side groups by age. IV regression coefficients for each group in both panels are estimated separately and correspond to Equation 2, where the regressor of interest, an indicator for enrollment in managed care, is instrumented with assignment to managed care. Only post-assignment observations are included (February 2012 to December 2014). Observations are at the enrollee \times year level: N = 284,928 for auto-assignees. Number of auto-assignees: 94,976. All regressions adjust for the unit of randomization (i.e., recipient's prior provider). Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details). Standard errors clustered on the unit of randomization (i.e., recipient's prior provider).



Appendix Figure A6: Rebates share

Note: Figure presents a time series of the share of point-of-sale drug spending that is returned in rebates (vertical axis) for the study state, Louisiana, over the study period. The numerator is constructed from the Medicaid Financial Management Reports, while the denominator is constructed from the Medicaid State Drug Utilization Data. Construction of the measure follows the same steps as in (Dranove, Ody and Starc, 2021). Observations are annual. The left vertical line indicates the start of managed care (the beginning of the treatment period); the right vertical line indicates when pharmacy is carved into Medicaid managed care. The figure shows that rebates do not decline after the pharmacy carve-in, which could have otherwise offset the lower spending resulting from the drug carve-in shown in Figure 2.



Appendix Figure A7: Plan transition time series

Panel A. Spending reductions





Note: Figure plots means of spending over time by plan. Individual plans contributing to the pooled control plan are shown as dashed grey lines. The vertical line indicates when the treatment plan transitioned from FFS to become a full-risk managed care plan. The plans that were already full-risk managed care plans did not experience a change at that time. This event date (February 2015) is three years after the date of the auto-assignment natural experiment used in Figures 1, 2, 3, and 4. The sample is a balanced panel of 497,057 beneficiaries. Plotted means are residualized on calendar quarters to adjust for seasonality. Observations are reweighted such that the Plan Transition sample matches the distribution of the auto-assignee sample used in the first identification strategy on health status-by-gender-by-age bins (See Appendix C.2 for additional details).

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Appendix Figure A8: Impact of assignment to managed care on utilization

Note: Figure presents difference-in-differences event studies comparing indicators of any utilization across assignees to MMC and FFS for different types of utilization. Utilization is measured per time period (annually for the top row, quarterly for the bottom row). Estimates are based on a balanced panel of 85,668 continuously-enrolled recipients for the 47 month (February 2011–December 2014) period depicted. Time, in quarters or years, is along the horizontal axis. The leftmost vertical line indicates the start of managed care (the beginning of the treatment period); the rightmost vertical line indicates when pharmacy is carved into Medicaid managed care. The figure shows the (null) effects of assignment to managed care prior to the treatment period. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). 95% confidence intervals reported.



Appendix Figure A9: Scatter plot of average CPT/HCPCS prices in MMC and FFS

Note: Figure presents evidence that the reduction in outpatient spending is partly driven by a reduction in prices for the same outpatient services with an overall reduction in prices of 3.06% in MMC, relative to FFS. This reduction is primarily driven by Evaluation and Management codes in red (CPT codes between 99201-99499, HCSPS codes T1015) with a reduction in MMC prices of 4.34%. Scatter plot includes 90.37% of overall outpatient spending (MMC and FFS) used in the mechanisms analysis (See Section 6 for additional details). For additional details on the composition of the reduction in prices, see Table A10.



Appendix Figure A10: Comparison of MMC and FFS primary care network breadth

Note: Figure shows the primary care network breadth of FFS and MMC plans. We restrict to primary care as the FFS plans were only responsible for creating a primary care network, and thus have no other provider types within their stated networks. Each point represents a specific ZIP code within the first region of Louisiana to transition to Medicaid (the region of the main auto-assignment experiment). For each ZIP code, network breadth is calculated by identifying the physicians responsible for care of enrollees who reside in that ZIP code and determining what percentage of those claims are covered by PCPs listed in each plan's network. We create a single average over the 2 FFS plans and the 3 MMC plans to summarize the average breadth for each payment system. In nearly every ZIP code, the MMC primary care network is broader than the FFS primary care network.



Appendix Figure A11: Impact of assignment to MMC vs. FFS on generic usage

Note: Figure presents difference-in-differences event studies comparing the usage of generic drugs (Panel A) and the efficiency of this usage (Panel B) across assignees to MMC and FFS. Estimates are based on a balanced panel of 85,668 continuously-enrolled recipients for the 47 month (February 2011–December 2014) period depicted. Time, in quarters, is along the horizontal axis. The leftmost vertical line indicates the start of managed care (the beginning of the treatment period); the rightmost vertical line indicates when pharmacy is carved into Medicaid managed care. Generic penetration is the share of generic drugs among all drugs used in an enrollee-quarter. Generic efficiency is the share of drug claims that are "efficient", i.e., a pharmacy claim is said to be generic efficient if there exists a generic counterpart to the drug used, and this generic is used. Generic penetration and efficiency rise substantially and statistically significantly following the pharmacy carve-in, consistent with enrollees randomly assigned to MMC plans increasing their use of generic drugs relative to brand drugs after the carve-in of prescription drugs to managed care plan responsibility. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). 95% confidence intervals reported.



Appendix Figure A12: Decomposition of spending by type of spending and sample in t_1

Note: This figure presents the decomposition results for $t_1 = 2013$ for the Auto-Assignment (AA) experiment and $t_1 = 2015$ the Plan Transition (PT) experiment. Additional details are available in Appendix D. Observations are reweighted such that the Plan Transition sample matches the distribution of the auto-assignee sample used in the first identification strategy on health status-by-gender-by-age bins (See Appendix C.2 for additional details).



Appendix Figure A13: Dose-response for ATC-4 therapeutic classes for kids

Note: Figure presents evidence restricted to children (aged 0 to 19 excluded) that pharmacy denials are a key mechanism driving the managed care spending effects. The figure compares managed care spending effects by ATC-4 therapeutic drug class (vertical axis – i.e., Anatomical Therapeutic Chemical (ATC) Classification, level 4) to the share of claims denied by managed care plans (horizontal axis). The estimates are based on the auto-assignment experiment and sample, but restricted to children. To measure the managed care claims denial rate, we restrict to the first quarter 1 month after the pharmacy carve-in to capture the peak visible in Panel A of Figure 6. The negative slope indicates that managed care plans generated larger spending reductions in drug classes where they managed utilization more aggressively via denials.



Appendix Figure A14: Generalizability: similar estimates from two identification strategies

Note: Figure compares spending reductions for various ATC-4 therapeutic classes of drugs (i.e., Anatomical Therapeutic Chemical Classification, level 4) across two identification strategies. Results from the auto-assignment (AA) quasi-experiment are plotted along the vertical axis, and results from the plan transition (PT) quasi-experiment are plotted along the horizontal axis. A 45 degree line is plotted for ease of comparison. Observations are reweighted such that the the Plan Transition sample matches the distribution of the auto-assignee sample used in the first identification strategy on health status-by-gender-by-age bins (See Appendix C.2 for additional details).



Appendix Figure A15: Denials at the NDC-level

Panel A. Quantity reductions concentrated in drugs targeted by utilization management



Note: Figure presents four time series plots of the number of paid and denied claims respectively for the most (left) and least (right) denied NDCs, by decile. Observations are at the assigned model \times month level. Time, in months, is along the horizontal axis. The leftmost vertical line indicates the start of managed care (the beginning of the treatment period); the rightmost vertical line indicates when pharmacy is carved into Medicaid managed care.. Plotted means are residualized on the unit of randomization (i.e., recipient's prior provider).

Appendix Figure A16: Denial analysis: fewer units and substitutions

Panel A. Decomposing MMC denials - Conditions and shares





Note: These results use a matching strategy where a denial is matched to a subsequently paid claim within 7 days, if this paid claim exists. The period studied is identical to that of the dose-response Figure 6 (See Appendix E for additional details). Panel B conditions on a denial resulting in a subsequent paid claim with same NDC claim within 7 days. Panel C conditions on a denial resulting in a subsequent paid claim with different NDC. Note that the stacked barplots add up to 100, but the levels are different, especially between MMC and FFS, and between generic and brand drugs.



Appendix Figure A17: Pharmacy denials within the first month

Note: Figure shows denial rates within the first month of joining for MMC and FFS stratified by characteristics of the prescriptions (days supply in the left column, branded versus generic in the right column). The sample uses the cohorts of new to Medicaid enrollees identified in Figure 9. Because pharmacy benefits were not carved-in until Nov 2012, we only consider MMC cohorts which joined beginning in Nov 2012. The first row shows raw levels of denial rates stratified by prescription characteristics for MMC and FFS. The second row provides estimates of the denial rate in MMC relative to FFS with adjustments for the specific drug and prescribing provider, which may contribute to differences in the raw-denial rates. Standard errors are clustered at the cohort level. The last row shows the distribution of paid claims by the prescription characteristic to illustrate compositional differences between MMC and FFS in fulfilled scripts.



Appendix Figure A18: Enrollee switching based on use of care prior to randomization

Note: Figure presents the difference in the share of enrollees who remain in their assigned plan (MMC relative to FFS) grouped by the time since their most recent health care interaction (medical or pharmacy) prior to randomization. Effects are measured relative to the difference between MMC and FFS for enrollees who had no claims in the year prior to assignment. Enrollees who sought care just prior to randomization are the most likely to switch from their assigned MMC plan.



Appendix Figure A19: MMC spending and switching effects by decile of predicted utilization postassignment

Note: Figure presents spending and switching effects of MMC relative to FFS stratified by the predicted probability of utilization post assignment. Effects are measured relative to the MMC effect within the lowest decile of predicted post-assignment utilization. Predictions are based on a logistic regression with a L1 regularization using baseline de-mographic characteristics and binary indicators for the 450 most common diagnoses and individual NDCs utilized in the year prior to assignment as predictors with a binary outcome indicating whether a member had any care in the 90 days immediately following random assignment. We fit three separate models where the outcome is (1) any medical or pharmacy utilization, (2) any medical utilization, and (3) any pharmacy utilization. We find large heterogeneity in switching with respect to predicted utilization, while there is no significant heterogeneity in spending. When controlling for both predicted medical and pharmacy spending concurrently, we find that the heterogeneity is predominantly driven by predicted medical utilization 90 days post-assignment, which is expected as pharmacy is not carved into MMC contracts until November 2012.



Appendix Figure A20: Enrollee switching surrounding the 2015 plan transition

Note: Figure presents the share of enrollees who remain in their pre-transition plan after one of the plans (transitioned plan) switches from FFS to MMC in January 2015. We consider a balanced panel of enrollees who are enrolled in Medicaid for 24 months (12 months pre- and post-transition) and who are consistently enrolled in the same plan for 12 months prior to the plan transition. The control plans remain MMC throughout the entire 24 months.



Appendix Figure A21: Demographic comparison of auto-assignees to post-expansion Louisiana Medicaid

Note: Figure shows the distribution of age and gender within the auto-assigned sample used in the main study and the post-expansion population within the same state. For auto-assignees, age is calculated as of Dec 2012. The post-expansion population is comprised of the same eligibility categories with the addition of the expansion eligibility group and takes all individuals enrolled in December 2016 (expansion occurred on June 1, 2016). In both samples we remove individuals who are aged > 65. Because age is calculated at the end of the first study year and individuals needed to be already enrolled in Medicaid to be auto-assigned, there are no enrollees aged 0 in the auto-assignee sample.

	Overall		Auto-A	ssignees	Active Choosers	
	$\overline{\overline{Y}}$	Std Dev	\overline{Y}	Std Dev	$\overline{\overline{Y}}$	Std Dev
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Enrollee characteristics						
Age at baseline	9.07	7.39	9.36	7.49	8.44	7.13
Female (%)	52.79	49.92	52.92	49.92	52.51	49.94
Panel B. enrollee spending, annually (\$)						
Total	1,565.65	2,497.55	1,451.35	2,427.61	1,818.35	2,628.03
Medical	1,110.01	1,825.78	1,052.74	1,815.46	1,236.61	1,842.09
Inpatient	97.32	749.95	97.48	747.79	96.96	754.69
Outpatient	639.63	849.07	590.29	820.12	748.70	900.26
Pharmacy	436.44	1,024.41	381.45	948.76	557.99	1,165.33
Brand Drug	265.12	816.94	229.30	757.06	344.31	930.94
Generic Drug	168.14	370.10	149.63	345.53	209.05	416.45
Panel C. Any annual utilization of high- or	potentially h	igh-value ca	are (%)			
Annual Well-Child Visits	52.65	49.93	49.34	50.00	60.19	48.95
Access to Primary Care	83.46	37.15	80.46	39.65	90.12	29.84
Chlamydia Screening	58.69	49.24	59.67	49.06	56.19	49.62
Cervical Cancer Screening	68.93	46.28	67.19	46.96	74.11	43.81
Follow-up after ADHD Prescription	53.07	49.91	51.17	49.99	57.10	49.51
Behavioral	7.64	26.56	7.40	26.18	8.16	27.38
Dental	59.01	49.18	55.18	49.73	67.48	46.84
Asthma Medication	16.44	37.06	14.87	35.58	19.91	39.93
Diabetes Medication	0.61	7.80	0.58	7.56	0.69	8.29
Panel D. Any annual utilization of low- or	potentially lo	w-value car	e (%)			
Any Low-Value Care Composite	0.95	9.72	0.92	9.57	1.02	10.05
Avoidable Emergency Department	8.20	27.44	8.43	27.78	7.71	26.68
Imaging	24.23	42.85	23.33	42.29	26.21	43.98

Appendix Table A1: Summary statistics for "auto-assignee" population

Notes: Table reports summary statistics on enrollee demographics, utilization, and spending. The sample consists of a balanced panel of Medicaid enrollees that were in Medicaid from February 2012 and remained until at least December 2014. Observations are at the enrollee \times year level: N = 413,811 enrollee-years. Additional details on the utilization and spending measures is available in Section 2. Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details). Components of spending do not sum up to "Total" due to winsorization.

	Coefficient (1)	Standard Error (2)	F-Statistic (3)
Full 3-year study period	0.76	0.03	678
Last 2 years (post pharmacy carve-in)	0.75	0.03	635

Appendix Table A2: First stage regression of plan enrollment on assigned plan

Notes: Table presents the first stage coefficient for enrollment in Medicaid managed care against assignment to Medicaid managed care. Regressions are adjusted for the unit of randomization. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider).

	Mean	Coef. on Managed Care Enrollment	p-value
	(1)	(2)	(3)
Panel A. Enrollee characteristics			
Age at baseline	8.44	0.27*	0.04
Female (%)	52.51	1.49***	0.00
Panel B Enrollee health conditions			
Asthma	7.61	-0.11	0.79
Serious Mental Illness	3.15	0.15	0.51
Diabetes	0.81	0.06	0.59
Pregnancy	0.98	0.32*	0.02
Cardiovascular Conditions	1.35	-0.08	0.53
Panel C. Enrollee spending, annually (\$)			
Total	201 84	-0.05	1.00
Medical	148.60	1 49	0.89
Pharmacy	53.24	-1.53	0.41
Panel D Any annual utilization of high- or notentially high-value care (%)			
Annual Well-Child Visits	28 71	-1 52**	0.00
Access to Primary Care	80.31	-2.07**	0.01
Chlamydia Screening	0.64	0.23**	0.01
Asthma Medication	3.69	0.07	0.62
Diabetes Medication	0.27	0.05	0.18
Panel F Any annual utilization of low- and notentially low-value care (%)			
Any Low-Value Care Composite	0.90	0.08	0.31
Avoidable Emergency Department Visits	5.63	0.38	0.15
Imaging	26.89	0.00	1.00
Joint Test			0.03
N		42,961	

Appendix Table A3: Imbalance among active choosers

Notes: Table presents the results of a test for balance of predetermined characteristics among enrollees who made an active plan choice. Each row corresponds to a separate regression. The characteristics tested for balance include recipient demographics and *pre-assignment* utilization and diagnoses. To construct column 2, each baseline characteristic is regressed on an indicator for assignment to managed care with controls for, and clustering on, the unit of randomization (i.e., recipient's prior provider). Self-sorter characteristics were *highly imbalanced*, consistent with selection on observables into managed care. The estimates are based on a balanced panel of 42,961 continuously-enrolled recipients that made an active plan choice to MMC or FFS in February 2012 and remained in Medicaid until, at least, December 2015. The joint test reports the p-value of the F statistic that all baseline characteristics are jointly zero from a regression of MMC assignment status on baseline characteristics. The p-value on the joint test is calculated using randomization inference where treatment labels within each unit of randomization are randomly permuted 1000 times.

Appendix Table A4: Robustness: IV estimates of the effect of managed care using raw, unwinsorized, spending outcomes auto-Assignee Sample Full Sample $\frac{\overline{Y} \quad RF \quad 2SLS \quad 2SLS-Post \\ Pharmacy Carve-in }$ Full Sample \\ OLS OLS

	Ŷ	RF	2SLS	2SLS-Post	OLS
	(1)	(2)	(3)	(4)	(5)
Total Spending	1,516	-56* (26)	-74* (34)	-95* (37)	-285*** (30)
Panel A. Spending by components of care (\$) Inpatient Spending	137	-12 (19)	-15 (24)	-18 (24)	-25 (22)
Outpatient Spending	607	-9 (7)	-12 (9)	-5 (11)	-81*** (9)
Pharmacy Spending	409	-37** (13)	-49** (17)	-74*** (19)	-165*** (17)
Panel B. Spending by enrollee characteristics (\$) Female	1,564	-77 (41)	-99 (54)	-118* (59)	-259*** (34)
Male	1,463	-36 (31)	-47 (41)	-71 (48)	-312*** (50)
Black	1,343	-67 (45)	-86 (58)	-107 (61)	-187*** (39)
White	1,891	-22 (37)	-31 (52)	-54 (68)	-399*** (63)
Panel C. Spending by quartiles of predicted health care spending (\$) 0-25%	696	-45^{*} (18)	-52* (21)	-65* (33)	-108*** (21)
26-50%	959	-45* (21)	-58* (26)	-100** (33)	-106*** (20)
51-75%	1,376	-67* (29)	-91* (41)	-95 (50)	-153*** (32)
76-100%	3,035	-106 (101)	-155 (148)	-174 (158)	-358*** (93)

Notes: Table presents sample means, and OLS and IV regression coefficients corresponding to Equation 2 using raw, unwinsorized spending as the dependent variable. Each cell in columns (2) through (4) corresponds to a separate regression, displaying the coefficient on an indicator for assignment to or enrollment in managed care. In Panel A, the variables listed indicate the dependent variable in the regression. In Panels B and C, the dependent variable is total spending, and the variables listed specify the subsample the regressing restricts to. The sample consists of auto-assignees for columns (1) through (3) and adds the active-choosers to the sample for column (4). Only post-assignment observations are included (February 2012 to December 2014). Observations are at the enrollee-year level: N = 284, 928 for auto-assignees and N = 413, 811 overall. Number of auto-assignees: 94,976. Number of active-choosers: 42,961. All regressions control for provider prior to the auto-assignment period. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). * p < 0.05, ** p < 0.01, *** p < 0.001.

	Auto-Assignee Sample			Full Sample	
	\overline{Y} (1)	RF (2)	2SLS (3)	OLS (4)	
Total Spending	1,335	-73*** (11)	-96*** (15)	-266*** (22)	
Panel A. Spending by components of care (\$) Inpatient Spending	100	2 (9)	3 (12)	-20 (21)	
Outpatient Spending	548	-19*** (5)	-25*** (6)	-84*** (8)	
Pharmacy Spending	345	-54*** (7)	-71*** (8)	-162*** (15)	
Panel B. Spending by enrollee characteristics (\$) Female	1,264	-80*** (15)	-104*** (21)	-232*** (22)	
Male	1,406	-67*** (17)	-88*** (22)	-297*** (27)	
Black	1,170	-62*** (14)	-79*** (18)	-190*** (22)	
White	1,656	-62* (25)	-88* (34)	-320*** (29)	
Panel C. Spending by quartiles of predicted health care spending (\$) 0-25%	672	-39** (13)	-45** (15)	-98*** (15)	
26-50%	929	-34* (16)	-44* (20)	-111*** (12)	
51-75%	1,295	-93*** (21)	-128*** (30)	-112*** (24)	
76-100%	2,625	-167*** (37)	-248*** (53)	-277*** (36)	

Appendix Table A5: IV estimates of the effect of managed care on spending for children, aged 0-18

Notes: Table presents results of estimating Equation 2. Each cell in columns (2) through (4) corresponds to a separate regression, displaying the coefficient on an indicator for assignment to or enrollment in managed care. In Panel A, the variables listed indicate the dependent variable in the regression. In Panels B and C, the dependent variable is total spending, and the variables listed specify the subsample the regression restricts to. The sample consists of children (aged less than 19) with columns (1) through (3) containing the sample mean and regression results for the auto-assignees and column (4) containing OLS estimates based on the full sample. Only post-assignment observations are included (February 2012 to December 2014). Observations are at the enrollee-year level: N = 263,640 for auto-assignees and N = 384,915 overall. There were 87,880 unique auto-assignees and 40,425 unique active choosers. All regressions control for provider prior to the auto-assignment period. Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details). Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). * p < 0.05, ** p < 0.01, *** p < 0.001.

Appendix Table A6: Impacts on quality and consumer satisfaction for children, aged 0-18

	Auto-Assignee Sample				Full Sample
	\overline{Y}	RF	2SLS	N	OLS
	(1)	(2)	(3)	(4)	(5)
Panel A. Any primary care access and preventive care in year (%)					
Primary Care and Preventative Composite	48.90	-0.69* (0.28)	-0.91* (0.38)	263,640	-3.43*** (0.42)
Annual Well-Child Visits	49.91	-0.46 (0.72)	-0.60 (0.95)	163,330	-3.88*** (1.15)
Access to Primary Care	80.54	-1.60** (0.56)	-2.10** (0.73)	261,245	-4.75*** (0.68)
Chlamydia Screening	55.23	1.34 (1.50)	1.76 (1.97)	7,011	-0.94 (1.30)
Follow-up care after ADHD Prescription	51.17	1.23 (2.22)	1.77 (3.22)	3,864	-1.38 (1.70)
Behavioral	7.12	-0.44* (0.19)	-0.58* (0.24)	263,640	-1.57*** (0.23)
Dental	58.71	-0.08 (0.53)	-0.11 (0.69)	263,640	-3.77*** (0.70)
Panel B. Any potentially high-value care drug classes in a year (%)					
Asthma Medication	15.15	-0.72** (0.26)	-0.94** (0.34)	263,640	-2.63*** (0.42)
Diabetes Medication	0.26	0.05 (0.03)	0.07 (0.04)	263,640	0.00 (0.03)
Panel C. Any potentially low-value care in a year (%)					
Any Low-Value Care Composite	0.65	-0.05 (0.05)	-0.06 (0.06)	263,640	-0.10** (0.03)
Avoidable Emergency Department	7.67	0.92*** (0.20)	1.20*** (0.29)	263,640	0.90*** (0.14)
Imaging	21.06	-0.15 (0.29)	-0.20 (0.38)	263,640	-1.91*** (0.28)
Panel D. Concumer caticfaction (relative to EES)					
Share of enrollees in their assigned plan (%)	93.32	-15.15*** (3.44)			

Notes: Table presents sample means, and OLS and IV regression coefficients corresponding to Equation 2, where the regressor of interest, an indicator for enrollment in managed care, is in some specifications instrumented with assignment to managed care. Each row corresponds to a separate regression, with the dependent variable listed in the row label (left). The sample size, listed in column (4), differs across rows because only a subset of the sample would be clinically eligible or "at risk" for certain outcomes. The sample consists of auto-assignee children (aged less than 19) for columns (1) through (4) and adds the active-choosers to the sample for column (5). Only post-assignment observations are included (February 2012 to December 2014). Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). * p < 0.05, ** p < 0.01, *** p < 0.001.

	Overall		Pooled	Control	Transitioned Plan	
	\overline{Y}	Std Dev	\overline{Y}	Std Dev	\overline{Y}	Std Dev
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Enrollee characteristics						
Female (%)	52.92	49.91	52.92	49.92	52.92	49.92
Age at baseline	10.92	9.23	11.07	9.40	10.67	8.93
Panel B. enrollee spending, annually (\$)						
Total	1,584.09	2,745.78	1,510.87	2,683.35	1,702.53	2,839.87
Medical	1,179.09	2,053.76	1,173.16	2,069.63	1,188.68	2,027.78
Inpatient	90.30	613.46	92.56	617.74	86.65	606.46
Outpatient	650.30	880.59	638.11	876.24	670.01	887.22
Pharmacy	370.82	935.98	305.09	829.97	477.13	1,077.32
Brand Drug	204.90	729.19	145.95	626.48	300.24	861.53
Generic Drug	158.06	336.34	151.81	337.77	168.17	333.78
Panel C. Any annual utilization of high-or	r potentially	ı high-value	care (%)			
Annual Well-Child Visits	50.73	49.99	49.97	50.00	51.97	49.96
Access to Primary Care	80.52	39.61	79.85	40.11	81.60	38.75
Chlamydia Screening	54.02	49.84	53.60	49.87	54.67	49.78
Cervical Cancer Screening	57.89	49.37	56.66	49.56	60.02	48.99
Follow-up after ADHD Prescription	52.34	49.95	53.07	49.91	51.12	50.00
Behavioral	7.98	27.10	7.92	27.01	8.08	27.25
Dental	50.76	49.99	48.94	49.99	53.71	49.86
Asthma Medication	15.68	36.36	14.85	35.56	17.03	37.59
Diabetes Medication	0.61	7.77	0.60	7.74	0.62	7.82
Panel D. Any annual utilization of low- or	potentially	low-value c	are (%)			
Any Low-Value Care Composite	1.10	10.43	1.10	10.44	1.09	10.40
Avoidable Emergency Department	10.85	31.10	11.36	31.74	10.01	30.01
Imaging	25.41	43.54	24.86	43.22	26.32	44.03

Appendix Table A7: Summary statistics for the plan transition experiment (pre-period)

Notes: Table reports summary statistics on enrollee demographics, utilization, and spending. The sample consists of a balanced panel of Medicaid enrollees that were in Medicaid continuously between February 2014-2016 and did not switch plans. Observations are at the enrollee \times year level: N = 497,057 enrollees. Observations are reweighted such that the sample matches the distribution of the auto-assignee sample used in the first identification on health status-by-gender-by-age bins (See Appendix C.2 for additional details). Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details). Components of spending do not sum up to "Total" due to winsorization.

	Auto-A	ssignees	Plan Tra	ansition
	\overline{Y}	Std Dev	\overline{Y}	Std Dev
	(1)	(2)	(3)	(4)
Panel A Enrollee characteristics				
Female (%)	52.92	49.91	52.92	49.92
Age at baseline	9.36	7.49	10.67	8.93
0				
Panel B. enrollee spending, annually (\$)				
Total	1,451.35	2,427.61	1,702.53	2,839.87
Medical	1,052.74	1,815.46	1,188.68	2,027.78
Inpatient	97.48	747.79	86.65	606.46
Outpatient	590.29	820.12	670.01	887.22
Pharmacy	381.45	948.76	477.13	1,077.32
Brand Drug	229.30	757.06	300.24	861.53
Generic Drug	149.63	345.53	168.17	333.78
Panel C. Any annual utilization of high- or	potentially	hioh-value d	care (%)	
Annual Well-Child Visits	49.34	50.00	51.97	49.96
Access to Primary Care	80.46	39.65	81.60	38.75
Chlamydia Screening	59.67	49.06	54.67	49.78
Cervical Cancer Screening	67.19	46.96	60.02	48.99
Follow-up after ADHD Prescription	51.17	49.99	51.12	50.00
Behavioral	7.40	26.18	8.08	27.25
Dental	55.18	49.73	53.71	49.86
Asthma Medication	14.87	35.58	17.03	37.59
Diabetes Medication	0.58	7.56	0.62	7.82
Densi D. Annound utilization of low on	a a tara ti a 11. 1		(0/)	
Any Low Volue Core Core coile		ow-ouiue ca	1 00	10.40
Any Low-value Care Composite	0.92	7.57 77 70	1.09	10.40
Avoidable Emergency Department	ð.43	27.78 42.20	10.01	30.01 44.02
Imaging	23.33	42.29	26.32	44.03

Appendix Table A8: Comparing auto-assignee and plan transition samples

Notes: Table reports summary statistics on enrollee demographics, utilization, and spending for both the auto-assignee and plan transition samples. The auto-assignee sample consists of a balanced panel of Medicaid enrollees that were randomly auto-assigned to Medicaid managed care or FFS in February 2012 and remained in Medicaid until at least December 2014. Observations are at the enrollee \times year level: N = 284,928 enrollee-years. The plan transition sample consists of a balanced panel of Medicaid enrollees that were in the Medicaid plan that transitioned in February 2015, from February 2014 and remained until at least February 2016. Observations are at the enrollee \times year level: N = 189,900 enrollees. Observations are reweighted such that the sample matches the distribution of the auto-assignee sample used in the first identification on health status-by-gender-by-age bins (See Appendix C.2 for additional details). Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details). Components of spending do not sum up to "Total" due to winsorization.

	\overline{Y}_{i_1,t_0} (1)	OLS (2)	OLS (3)	N (4)
Panel A. Spending by components of care, annually (\$)				
Total Spending	1,702.53	-292.25*** (15.60)	-204.39*** (10.52)	993,990
Inpatient Spending	86.65	-11.19*** (3.33)	-6.55** (2.38)	993,990
Outpatient Spending	670.01	-54.42*** (4.54)	-27.17*** (3.45)	993,990
Pharmacy Spending	477.13	-191.64*** (5.22)	-144.91*** (3.47)	993 <i>,</i> 990
Panel B. Any primary care access and preventive care in year (%)				
Primary Care and Preventative Composite	48.10	-0.33** (0.10)	-0.61*** (0.11)	993,990
Annual Well-Child Visits	51.97	-1.10^{***}	-1.30^{***}	508,723

Appendix Table A9: Yearly differences in spending and utilization of potentially high- or low-value care for plan transition experiment

Yes	No	Yes	
26.32	-0.39* (0.19)	0.19 (0.18)	993,990
9.85	0.38** (0.14)	0.52*** (0.13)	971,339
1.09	0.07 (0.05)	0.09* (0.04)	993,990
0.62	-0.07 (0.06)	-0.02 (0.03)	993,990
17.03	-1.03*** (0.16)	-0.76*** (0.15)	993,990
53.71	0.38 (0.21)	-0.11 (0.22)	993,990
8.08	-0.84*** (0.12)	-0.82*** (0.12)	993,990
51.12	1.41 (1.77)	0.72 (1.78)	13,121
60.02	0.01 (0.68)	0.09 (0.73)	101,236
54.67	0.34 (1.01)	0.02 (1.05)	43,065
81.60	-0.58*** (0.16)	-0.60*** (0.17)	975,918
51.97	-1.10*** (0.29)	-1.30*** (0.29)	508,723
48.10	-0.33** (0.10)	-0.61*** (0.11)	993,990
	48.10 51.97 81.60 54.67 60.02 51.12 8.08 53.71 17.03 0.62 1.09 9.85 26.32 Yes	$\begin{array}{cccc} 48.10 & -0.33^{**} & (0.10) \\ 51.97 & -1.10^{***} & (0.29) \\ 81.60 & -0.58^{***} & (0.16) \\ 54.67 & 0.34 & (1.01) \\ 60.02 & 0.01 & (0.68) \\ 51.12 & 1.41 & (1.77) \\ 8.08 & -0.84^{***} & (0.12) \\ 53.71 & 0.38 & (0.21) \\ \end{array}$	$\begin{array}{c ccccccccccccccccccccccccccccccccccc$

Notes: Observations are at the enrollee \times year level pooled over two years: N = 994, 114. N reported in (4) is for the re-weighted OLS regression and does not match the number of enrollee \times year observations due to lack of joint support for <0.2% of enrollees. Observations are reweighted such that the sample matches the distribution of the auto-assignee sample used in the first identification strategy on health status-by-gender-by-age bins (See Appendix C.2 for additional details). Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details). Components of spending do not sum up to "Total" due to winsorization. Robust standard errors reported in parentheses. * p < 0.05, ** p < 0.01, *** p < 0.001.
Share of MMC Share of FFS Mean spend per Mean spend per Difference in Perc. Difference in CPT/HCPCS Category Code Description Code claims (%) claims (%) claim in MMC (\$) claim in FFS (\$) mean spend (\$) mean spend (%) (2)(5) (1)(3)(4)(6) E&M 99213 Established patient office visit, 20-29 minutes 11.3 12.0 50.85 55.05 -4.20-7.62 E&M 99283 Emergency department visit for the evaluation and management of a patient (low level) 5.2 4.3 78.21 78.72 -0.51-0.6599214 Established patient office visit, 30-39 minutes 4.44.875.94 80.31 -4.37 -5.44E&M 3.7 4.1 30.43 32.99 -2.55 -7.74E&M 99212 Established patient office visit, 10-19 minutes E&M T1015 Clinic visit/encounter, all-inclusive 3.1 1.9 126.60 124.80 1.77 1.42 Unspecified Services 3.0 2.7 25.06 27.00 -1.94-7.1790471 2.7 3.0 16.90 17.78 -0.88-4.95 Medicine Immunization administration E&M 99211 Established patient office visit, time not accounted for 2.6 3.0 19.01 20.29 -1.28-6.3199284 E&M Emergency department visit for the evaluation and management of a patient (moderate level) 2.0 1.6 120.30 120.90 -0.69-0.5799173 1.8 2.2 1.96 1.93 0.03 1.66 Medicine Screening test of visual acuity 1.7 12.57 87880 1.812.68 -0.11-0.85Pathology and Laboratory Strep a assay w/optic 92551 1.7 2.2 7.57 7.49 0.08 1.10 Medicine Audiologic screening test E&M 99282 Emergency department visit for the evaluation and management of a patient (straightforward level) 1.5 1.4 99.05 102.40 -3.35 -3.27 Complete CBC (Complete blood count) w/auto diff wbc 1.5 1.7 7.82 1.25 Pathology and Laboratory 85025 7.72 0.10 Other V2020 Frames, purchases 1.5 1.4 15.04 14.97 0.07 0.44 Medicine 90472 Immunization administration 1.4 1.5 18.36 18.72 -0.36 -1.9471020 Radiologic examination, chest, two views, frontal and lateral 1.2 1.1 27.84 26.87 0.97 3.59 Radiology 99392 -7.77 E&M Established patient, periodic comprehensive preventive medicine (age 1-4 years) 1.2 1.4 76.01 82.41 -6.41E&M 99393 Established patient, periodic comprehensive preventive medicine (age 5-11 years) 1.1 1.4 81.51 88.40 -6.89 -7.80Pathology and Laboratory 81025 Urine pregnancy test 1.0 0.7 6.20 6.28 -0.08-1.23New patient office visit, 30-44 minutes E&M 99203 0.9 0.8 68.38 69.89 -1.51-2.17Pathology and Laboratory 87804 Influenza assay w/optic 0.9 0.7 14.08 14.02 0.06 0.42 Spherocylinder, single vision, plano to plus or minus 4.00d sphere, .12 to 2.00d cylinder, per lens Other V2103 0.7 0.8 16.97 16.50 0.46 2.80 Medicine 92014 Ophthalmological services for established patient 0.7 0.8 81.21 80.22 0.99 1.23 0.7 0.9 E&M 99394 Established patient, periodic comprehensive preventive medicine (age 12-17 years) 86.61 95.63 -9.03 -9.44Pathology and Laboratory 80053 Comprehensive metabolic panel 0.7 0.6 11.98 11.54 0.44 3.80 100.0 45.52 46.96 -1.44Overall Weighted Average (with unspecified services) 100.0 -3.06 Weighted Average for non-E&M 56.2 56.8 31.16 31.42 -0.26-0.84Weighted Average for E&M 40.9 40.5 67.07 70.12 -3.05-4.34Weighted Average for Laboratory 18.1 15.7 11.67 11.27 0.40 3.55 Weighted Average for Radiology 7.3 6.6 56.90 56.57 0.34 0.60 2.8 2.6 144.50 149.40 -4.88-3.27 Weighted Average for Surgery

Appendix Table A10: Price summary statistics of main CPT and HCPCS procedures

Notes: This table presents summary statistics for the top 20 CPT and HCPCS codes (out of 2724 codes with at least 1 claim and \$1 of spend in both MMC and FFS simultaneously) used in the mechanisms analysis (See Section 6 for additional details). The table is sorted by share of MMC claims, column (1). The bottom rows present weighted averages (weighted by the number of FFS claims over the analysis period) overall, and separtely with and without Evaluation and Management codes (CPT codes between 99201-99499, HCSPS codes T1015), Laboratory codes (CPT codes between 80047-89398), Radiology codes (CPT codes between 70010-79999) and Surgery codes (CPT codes between 10021-69990).

44

	Main Effect		Decomposition				
	\overline{Y} (1)	2SLS (2)	Network (3)	Provider (4)	Residual (5)		
Panel A. Spending by components of care (\$)							
Total Spending	1,451.4	-81.5 (-117.9, -43.9)	34.7 (22.8, 45.7)	14.8 (-6.7, 33.2)	-131.0 (-161.9 <i>,</i> -95.6)		
Inpatient Spending	98.6	3.1 (-6.7, 14.9)	1.8 (-2.7, 6.0)	5.5 (1.7, 10.1)	-4.1 (-14.1, 6.2)		
Outpatient Spending	590.2	-18.6 (-30.9, -4.9)	7.0 (2.0, 12.2)	0.9 (-6.8, 7.6)	-26.5 (-39.2, -11.8)		
Pharmacy Spending	380.2	-68.7 (-89.6, -51.3)	12.9 (6.7, 19.0)	-4.8 (-13.4, 1.0)	-76.8 (-98.3, -56.0)		
Panel B. Pharmacy spending by type of drug (\$)							
Brand Drug Spending	228.1	-65.8 (-80.7, -51.5)	7.1 (2.4, 11.6)	-2.2 (-8.4, 1.5)	-70.7 (-87.3, -53.0)		
Generic Drug Spending	149.3	-3.5 (-11.8, 2.8)	5.9 (3.8, 8.3)	-2.6 (-5.3, 0.0)	-6.8 (-15.3 <i>,</i> -0.3)		

Appendix Table A11: Mechanisms: broader primary care networks tend to increase managed care spending

Notes: Table presents sample means and IV regression coefficients corresponding to Equation 2, where the regressor of interest, an indicator for enrollment in managed care, is instrumented with assignment to managed care. Column 1 lists means of dependent variables and Column 2 lists the overall effect reported in the main paper. Each cell in columns 3–5 corresponds to a separate regression, with the dependent variable listed in the row label. The network component, (3), adds a linear adjustment for primary care network breadth at the ZIP level and subtracts that estimate from the main estimate in (2). The provider component, (4), adjusts for an enrollee's attributed primary care provider, and subtracts that estimate from the main estimate in (2). By definition, the residual component, (5), is the remainder so that the decomposition is exact, and equals the overall effect. The sample consists of auto-assignees. Only post-assignment observations are included (February 2012 to December 2014). Observations are at the enrollee-year level: N = 284,716. Total annual spending is winsorized at \$25,000. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). 95% confidence intervals are obtained using 1000 cluster bootstrapped replicates and shown below estimates in parentheses (See Appendix D.3).

	Total Effect	Components					
	Overall Change	Change in Prices	Sub. to Cheaper Generics/Brands	Sub. from Brands to Generics	Residual		
	$\Delta TS_{MMC,FFS}$ (1)	$\Delta P_{MMC,FFS}$ (2)	$\Delta Q^{Steering}_{MMC,FFS}$ (3)	$\Delta Q^{Generic}_{MMC,FFS}$ (4)	$\Delta Q^R_{MMC,FFS}$ (5)		
Panel A: Total Spending							
t_{-1}	0.85	0.22	0.11	0.02	0.51		
	(-1.5, 3.2)	(-0.5, 1.0)	(-0.1, 0.3)	(-0.2, 0.2)	(-1.5, 2.7)		
t_0	-4.80	-1.53	-0.27	0.00	-3.00		
	(-6.8, -2.5)	(-2.3, -0.6)	(-0.6, -0.0)	(-0.3, 0.3)	(-5.0, -0.7)		
t_1	-8.52	-2.62	-0.63	-2.39	-2.88		
	(-11.7, -5.3)	(-3.6, -1.8)	(-1.0, -0.3)	(-2.9, -1.9)	(-5.6, 0.6)		
t_2	-7.49	-0.67	-0.72	-3.13	-2.97		
	(-10.0, -4.4)	(-1.3, -0.1)	(-1.1, -0.4)	(-3.7, -2.6)	(-5.4, 0.6)		
Panel B: Medical Spending							
t_{-1}	1.61	0.04			1.57		
	(-0.4, 4.1)	(-0.8, 0.9)			(-0.6, 4.2)		
t_0	-3.67	-1.92			-1.75		
	(-6.1, -0.6)	(-3.0, -0.7)			(-4.3, 1.1)		
t_1	-2.28	-2.48			0.22		
	(-5.7, 1.2)	(-3.5, -1.6)			(-2.8, 3.8)		
t_2	-0.61	-0.23			-0.35		
	(-3.7, 2.9)	(-0.9, 0.4)			(-3.3, 3.2)		
Panel C: Pharmacy Spending	1.10	2.20	0.44	0.0 -	1.05		
t_{-1}	-1.19	(0.29)	0.44	(0.05)	-1.97		
	(-3.0, 2.0)	(-0.0, 1.2)	(-0.2, 1.1)	(-0.8, 0.7)	(-3.3, 1.8)		
t_0	-7.11 (-11 3 -3 3)	-0.71	-0.76	-0.15	-5.49		
	(-11.5, -5.5)	(-1.9, 0.4)	(-1.7, -0.0)	(-1.0, 0.7)	(-9.0, -0.9)		
t_1	-25.42 (-31.1 -19.8)	-3.00 (-4.71.6)	-2.19 (-3.4 -1.0)	-8.61 (-10 5 -6 9)	-11.61		
L.	05.1, -17.0)	1.40	(-0.44	(-10.0, -0.7)	10.29		
<i>L</i> 2	-23.33 (-30.6, -20.2)	-1.49	-2.44 (-3.7, -1.3)	-11.22 (-13.0, -9.4)	-10.38 (-14.5, -5.8)		

Appendix Table A12: Decomposition of spending reductions caused by managed care

Notes: Table presents a mutually exclusive and collectively exhaustive decomposition of the spending reduction due to assignment to managed care into four effects: (column 2) a price inflation index ΔP , (column 3) drug steering effect within an ATC-4 therapeutic class - brand/generic cell $\Delta Q^{Steering}$, (column 4) a brand-generic drug steering effect within ATC-4 therapeutic classes $\Delta Q^{Generic}$, and (column 5) a quantity effect ΔQ^R which captures steering across ATC-4 therapeutic classes (brand-brand or brand-generic) and outright quantity effects. Numbers presented are percent changes relative to FFS spending. Additional details are available in Appendix D.1. 95% confidence intervals shown below estimates in parentheses and are obtained using 400 cluster bootstrapped replicates (See Appendix D.3). Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details).

Sub. to Sub. from Overall Change Change in Prices Residual Cheaper Generics Brands to Generics $\Delta Q_{MMC,FFS}^{Steering}$ $\Delta TS_{MMC,FFS}$ $\Delta P_{MMC,FFS}$ $\Delta Q_{MMC,FFS}^{Generic}$ $\Delta Q^R_{MMC,FFS}$ (1)(2) (3)(4)(5) (6) (7)A10 0.8 22 36.0 26.0 -0.40.0 10.0 N07 1.7 25 32.0 16.0 6.9 0.8 8.7 N03 0.8 19 10.0 3.2 -1.3 0.9 7.2 N05 1.8 23 -6.11.8 -7.10.7 -1.42.7 33 R03 -6.5 -6.1 -0.7-0.30.6 34 J01 0.5 -6.79.6 -16.00.1 -0.2L02 0.5 42 -9.1 -12.0-0.3-0.13.4 N06 29 25 -11.03.0 -1.5-9.9 -2.6 38 105 1.7 -11.0-3.6 0.3 0.0 -7.9J01 1.7 38 -12.0-9.5 0.3 0.0 -2.9S01 1.1 35 -13.0-7.00.1 -2.2 -3.819 S01 1.3 -22.0-19.0-20.020.0 -2.727 N02 0.6 -23.0-35.0-10.0-0.6 22.0 R06 0.9 33 -24.0-12.0-0.6-0.6-11.0R03 1.2 29 -25.05.7 2.2 0.0 -33.037 S01 2.2 -27.0-25.00.5 0.0 -2.4R06 0.5 34 -29.0-29.0-3.8-2.55.8 25 N05 0.7 -30.016.0 -44.02.9 -4.8A04 0.4 38 -30.0-34.01.2 0.0 2.5 37 P03 0.5 -33.0 -21.01.1 0.0 -13.0N06 0.7 20 -33.0 -7.8-2.7-18.0-4.6R03 32 -34.0-3.3 -2.20.6 -0.8-28.0G G03 0.6 19 -35.04.0 -21.0-5.0-13.0S02 0.5 36 -37.0 -27.05.8 0.0 -16.030 J01 0.5 -38.0-3.1 -0.1-11.0 -24.0R05 0.7 51 -40.00.2 0.0 -0.8-39.0S01 0.6 44 -41.02.5 1.4 -0.9-44.0S01 38 -42.0-8.9-21.00.6 4.0-17.0S03 -19.01.8 34 -42.0-3.4-4.8-16.00.5 28 -44.0-6.2 -46.07.7 A02 1.4 R03 8.5 35 -45.03.3 2.2 -38.0 -12.05.9 33 0.1 -41.0-45.0-2.9 -1.123 C02 3 -56.03.7 -0.8-37.0-22.0B03 0.7 33 -63.0-4.00.9 -27.0-33.02.7 41 -27.0-0.1-49.0I01 -78.0-3.1D11 1.1 47 -79.0-1.7-13.0-0.2-64.0S01 52 -0.8-53.0 1 -82.0-1.5-26.0

Decomposition of spending effect

Appendix Table A13: Utilization management summary statistics of main ATC-4 therapeutic classes

ATC2

Share of Managed

FFS Spending (%)

Denial rate in

MMC (%)

S01GX Antiallergics Notes: Table presents summary statistics for the top ATC-4 therapeutic classes (i.e., Anatomical Therapeutic Chemical (ATC) Classification, level 4) used in the dose-response analysis, i.e. ATC-4s that have at least 0.15% of overall pharmacy spending and claims, excluding ATC-4s that have FFS enrollee per-year spending less than \$1. Column (1) presents data for 2013-2014. Column (2) presents average denial rates for the quarter one month after the pharmacy carve-in in November 2012. The decomposition in columns (3) - (7) uses the same claims data as for the decomposition exercise in Appendix D.1 but combines result from 2013 and 2014. The table is sorted by the overall percentage change, column (3). Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details).

ATC4

A10AD

N07BC

N03AX

N05AX

R03CC

J01FF

L02AB

N06BA

J05AH

J01CA

S01AA

S01EA

N02BE

R06AE

R03AC

S01BA

R06AX

N05AH

A04AA

P03AC

N06AX

R03DC

G03AC

S02BA

J01AA

R05CA

S01AE

S01AD

S03BA

A02BC

R03BA

C02AC

B03AD

I01DD

D11AH

Diabetes drugs

Antipsychotics

Lincosamides

Progestogens

Antibiotics

Anilides

Other Antiepileptics

Drugs used in opioid dependence

Centrally acting sympathomimetics

Penicillins With Extended Spectrum

Sympathomimetics In Glaucoma Therapy

Selective Beta-2-Adrenoreceptor Agonists

Other Antihistamines For Systemic Use

Pyrethrines, Incl. Synthetic Compounds

Leukotriene Receptor Antagonists

Iron In Combination With Folic Acid

Antibiotics (from dose-response figure)

Diazepines, Oxazepines, Thiazepines And Oxepines

Neuraminidase Inhibitors

Piperazine Derivatives

Serotonin (5Ht3) Antagonists

Corticosteroids

Antidepressants

Progestogens

Tetracyclines

Expectorants

Antivirals

Corticosteroids

Fluoroquinolones

Corticosteroids

Glucocorticoids

All other NDCs

Antihypertensives

Agents for dermatitis

Proton Pump Inhibitors

Selective Beta-2-Adrenoreceptor Agonists

Description

	Auto-Assignee Sample		
	\overline{Y}	2SLS	2SLS-Post
	(1)	(2)	(3)
Panel A. Annual medical cost and drug utilization			
Medical Cost (\$)	1,053	-16.7 (13.3)	-10.2 (16.2)
Total Days Supply	114.29	-3.48 (2.45)	-3.75 (2.78)
Panel B. Annual days supply for potentially high-value drug classes			
Total High-Value Days Supply	10.71	-0.79 (0.41)	-1.02* (0.46)
Asthma Medications	9.42	-1.13** (0.37)	-1.39** (0.43)
Diabetes Medications	1.29	0.34 (0.18)	0.37 (0.20)

Appendix Table A14: IV effects of managed care on drug utilization

Notes: Table presents sample means and IV regression coefficients corresponding to Equation 2 using days supply as the dependent variable, where the regressor of interest, an indicator for enrollment in managed care, is instrumented with assignment to managed care. Each cell in columns (2) & (3) corresponds to a separate IV regression. Panel B looks at the effect for specific potentially high-value drug classes. The sample consists of auto-assignees and column (3) includes only post-carve-in observations (January 2013 to December 2014). Observations are at the enrollee × year level. All regressions adjust for the unit of randomization (i.e., recipient's prior provider). Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). * p < 0.05, ** p < 0.01, *** p < 0.001.

	No Plan Switching		Allows Plan Switching	
	$\overline{\overline{Y}_{i_1,t_0}}$ (1)	OLS (2)	$(3) \overline{\overline{Y}_{i_1,t_0}}$	OLS (4)
Panel A. Spending by components of care, annually (\$)				
Total Spending	1,702.53	-204.39*** (10.52)	1,716.67	-198.65*** (10.10)
Inpatient Spending	86.65	-6.55** (2.38)	86.36	-5.54* (2.25)
Outpatient Spending	670.01	-27.17*** (3.45)	674.62	-25.11*** (3.32)
Pharmacy Spending	477.13	-144.91*** (3.47)	481.72	-141.90*** (3.34)
Panel B. Any primary care access and preventive care in year (%)				
Primary Care and Preventative Composite	48.10	-0.33** (0.10)	48.30	-0.59*** (0.11)
Annual Well-Child Visits	51.97	-1.30*** (0.29)	51.74	-0.86** (0.28)
Access to Primary Care	81.60	-0.60*** (0.17)	82.02	-0.71*** (0.17)
Chlamydia Screening	54.67	0.02 (1.05)	54.52	-0.18 (1.00)
Cervical Cancer Screening	60.02	0.09 (0.73)	60.57	-0.30 (0.69)
Follow-up care after ADHD Prescription	51.12	0.72 (1.78)	51.49	0.68 (1.71)
Behavioral	8.08	-0.82*** (0.12)	8.18	-0.83*** (0.11)
Dental	53.71	-0.11 (0.22)	53.99	0.09 (0.21)
Panel C. Any potentially high-value care drug classes in a year (%)				
Asthma Medication	17.03	-0.76*** (0.15)	17.09	-0.77*** (0.15)
Diabetes Medication	0.62	-0.02 (0.03)	0.61	-0.02 (0.03)
Panel D. Any potentially low-value care in a year (%)				
Any Low-Value Care Composite	1.09	0.09* (0.04)	1.11	0.09* (0.04)
Avoidable Emergency Department	9.85	0.52*** (0.13)	9.84	0.49*** (0.13)
Imaging	26.32	0.19 (0.18)	26.49	0.04 (0.18)
Re-weighted	Yes	Yes	Yes	Yes

Appendix Table A15: Sensitivity of plan transition results to allowing for plan switching

Notes: No Plan Switching requires that enrollees remain in the same plan for the 24-month sample period, while Allows Plan Switching only requires enrollees to remain in the same plan for the 12-month period prior the the plan transition. No Plan Switching repeats the results of Table A9 for ease of comparison. Observations are reweighted such that the sample matches the distribution of the auto-assignee sample used in the first identification strategy on health status-by-gender-by-age bins (See Appendix C.2 for additional details). Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details). * p < 0.05, ** p < 0.01, *** p < 0.001. Components of spending do not sum up to "Total" due to winsorization. Robust standard errors reported in parentheses.

	Auto-Assignee Sample			Full Sample
	\overline{Y} (1)	RF (2)	2SLS (3)	OLS (4)
Total Spending	1,558	-71*** (15)	-94*** (20)	-266*** (22)
Panel A. Spending by components of care (\$)				
Inpatient Spending	103	2 (4)	3 (6)	0 (3)
Outpatient Spending	626	-16** (5)	-21** (7)	-82*** (8)
Pharmacy Spending	419	-60*** (8)	-79*** (10)	-166*** (14)
Panel B. Spending by enrollee characteristics (\$)				
Female	1,547	-79*** (20)	-103*** (27)	-237*** (23)
Male	1,570	-68** (21)	-91** (27)	-297*** (27)
Black	1,374	-64** (19)	-82** (25)	-186*** (23)
White	1,936	-57* (28)	-81* (40)	-329*** (30)
David C. Snowding by quartiles of predicted health care opending (*)				
0-25%	739	-41** (15)	-48** (18)	-100*** (15)
26-50%	996	-38 (19)	-48 (25)	-107*** (13)
51-75%	1,397	-102*** (24)	-140*** (35)	-115*** (22)
76-100%	2,956	-130** (46)	-191** (67)	-263*** (40)

Appendix Table A16: External validity: IV estimates of the effect of managed care on spending with sample re-weighted to match the characteristics of Medicaid enrollees that made active choices

Notes: Table presents results of Equation 2 after re-weighting the "auto-assignee" sample to match the measured health status, gender, and age of the active chooser sample. Each cell in columns (2) through (4) corresponds to a separate regression, displaying the coefficient on an indicator for assignment to or enrollment in managed care. In Panel A, the variables listed indicate the dependent variable in the regression. In Panels B and C, the dependent variable is total spending, and the variables listed specify the subsample the regression restricts to. Columns (1) through (3) contain the sample mean and regression results for the auto-assignee sample and column (4) contains OLS estimates based on the full sample, including active choosers. Only post-assignment observations are included (February 2012 to December 2014). Observations are at the enrollee-year level: N = 284,928 for auto-assignees and N = 413,811 overall. Number of auto-assignees: 94,976. Number of active-choosers: 42,961. Observations are reweighted such that the auto-assignee sample matches the distribution of the active chooser sample on health status-by-gender-by-age bins (See Appendix C.2 for additional details). All regressions adjust for provider prior to the auto-assignment period. Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details). Components of spending do not sum up to "Total" due to winsorization. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). * p < 0.05, ** p < 0.01, *** p < 0.001.

	Auto-Assignee Sample			
	$\overline{\overline{Y}}$ (1)	RF (2)	2SLS (3)	2SLS-Post (4)
Total Spending	2,171	98 (112)	127 (144)	135 (178)
Panel A. Spending by components of care (\$) Inpatient Spending	231	15 (23)	19 (29)	14 (37)
Outpatient Spending	848	50 (39)	65 (50)	84 (61)
Pharmacy Spending	666	-2 (47)	-2 (61)	-26 (72)
<i>Panel B. Spending by enrollee characteristics</i> (\$) Female	2,369	14 (82)	18 (106)	34 (128)
Male	1,885	182 (255)	237 (331)	219 (377)
Black	1,773	12 (83)	15 (105)	1 (138)
White	2,954	214 (274)	291 (369)	349 (430)
Panel C. Spending by quartiles of predicted enrollee health spending (\$) 0-25%	717	-36 (50)	-41 (58)	-36 (86)
26-50%	1,037	55 (106)	69 (133)	98 (200)
51-75%	1,520	27 (59)	36 (77)	65 (81)
76-100%	3,819	115 (232)	161 (324)	169 (394)

Appendix Table A17: IV estimates of the effect of managed care on spending, reweighted to resemble the post-ACA Medicaid population

Notes: Table presents results of Equation 2 after re-weighting the "auto-assignee" sample to match the measured health status, gender, and age of the post-ACA Medicaid population (See Appendix Section C.2.2). Each row corresponds to a separate regression. In Panel A, the variables listed indicate the dependent variable in the regression. In Panels B and C, the dependent variable is total spending, and the variables listed specify the subsample the regression restricts to. The sample consists of auto-assignees for columns (1) through (3) and adds the active-choosers to the sample for column (4). Only post-assignment observations are included (February 2012 to December 2014). Observations are at the enrollee-year level: N = 284,928 for auto-assignees and N = 413,811 overall. Number of auto-assignees: 94,976. Number of active-choosers: 42,961. All regressions control for provider prior to the auto-assignment period. Total annual spending is winsorized at \$25,000. All other sub-components are winsorized at the same percentile of their distribution (See Appendix B for additional details). Components of spending do not sum up to "Total" due to winsorization. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider). * p < 0.05, ** p < 0.01, *** p < 0.001.