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

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ABSTRACT

This paper examines the effects of privatizing social health insurance in the United States. We study this question in the context of the Medicaid program, the largest health insurer in the US and the largest means-tested program in the nation — serving over 90 million low-income families and individuals with disabilities. Exploiting a natural experiment wherein nearly 100,000 Medicaid enrollees were randomly assigned between a state-administered fee-for-service system and private managed care, we find that spending was nearly 10% lower for enrollees assigned to managed care plans. These savings were concentrated in prescription drugs, where we show that prior authorization was the key mechanism plans used to reduce overuse and encourage substitution to lower-cost alternatives without reducing quality. This was distinct from the effects of privatization on medical benefits, where private plans lowered quality and abraded consumers without achieving savings. In contrast to what our findings imply for an efficient public-private division of services, Medicaid has historically favored the public provision of prescription drugs and private outsourcing of medical care.

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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.

From a theoretical perspective, high-powered managed care contracts (in the classic sense of Laffont and Tirole, 1993 where the private firms are the residual claimants on cost savings) create strong incentives to control cost. However, when competition is weak and consumer choice suffers from the types of frictions documented in health insurance (e.g., Handel and Kolstad, 2015; Abaluck et al., 2021), outsourcing may also have an adverse effect on quality (Shleifer, 1998). This concern is further heightened in the healthcare sector, where many components of quality are not verifiable or contractible (Bergman et al., 2016; Knutsson and Tyrefors, 2021).

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 (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" 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 and private options, differences may reflect patient characteristics rather than performance (Brown et al., 2014). And when state mandates have forced

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

[2] Findings 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).

enrollees into private 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, 2018*b*), 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.

This paper advances the literature by estimating the causal effect of the private versus public provision of social health insurance 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 are unusually rich, and include the remittance of services, providing insight into 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)).

In our empirical analysis, we find evidence that spending is 5%–10% lower for enrollees randomly assigned to private managed care plans in place of FFS, with mixed effects on health care quality. This estimate is less than half of the cross-sectional OLS coefficient, 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 by managed care plans account for relatively little of the cost reductions.[3] Instead, the largest impact and the main driver of overall cost savings is the substitution toward cheaper, generic options for brand drugs within narrow therapeutic classes, as well as outright quantity reductions for certain drug classes. Overall,

[3]This finding is in line with recent studies of privatized public programs (e.g., Geruso, Layton and Wallace, 2020; Curto et al., 2019), where differentiation in negotiated prices for identical products and services plays only a secondary role for explaining cost savings. This is importantly different from the context of private, employer sponsored insurance where price variation is large (e.g., Cooper et al., 2018).

MMC plans generated an average reduction in drug spending of 18%.[4] Despite the large reductions in pharmacy spending, we find no evidence of reductions in the use of drugs from high-value therapeutic classes (e.g., statins, diabetes medication, etc.), consistent with Sacks (2018). However, we find evidence of reductions in primary care in MMC that may have shifted enrollees to other settings; avoidable emergency department use was 14% higher for enrollees assigned to managed care relative to those assigned to the FFS option.

We also present new evidence that enrollee satisfaction is lower in managed care. An advantage of our administrative data is that it enables us to construct revealed preference measures of enrollees' satisfaction with plan quality.[5] We find that enrollees auto-assigned to manage 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 cheaper, 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 private managed care plans achieve savings in our setting: real-time adjudication of pharmacy claims (i.e., just-in-time denials) that lack prior authorization. Following the carve-in of pharmacy to managed care responsibility,[6] 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 cheaper alternatives, rather than generating outright reductions in the net number of prescriptions. We provide suggestive evidence against—and in some cases, clearly rule out—other potential mechanisms behind the savings, including differences between MMC and FFS in negotiated provider prices, provider networks, steer-

[4]The 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.

[5]In 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.

[6]Neither 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.

ing to providers, and case management.

We supplement our first strategy (randomization of enrollees across MMC and FFS) 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 shock and randomization, Louisiana Medicaid discontinued its managed FFS program. Consequently, a single plan was forced to transition from facilitating care on a FFS basis (where the state is at-risk) to acting as a full risk-bearing managed care provider. Thus the plan's incentives and structure changed, but not plan ownership or enrollees. 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 in magnitude to the findings from randomized auto-assignment, suggesting that our estimates generalize beyond auto-assignees.[7] Our second identification strategy is also helpful in examining the potential for spillovers across MMC and FFS patients via their shared physicians.[8]

Finally, we ask whether pharmacy utilization management, which accounts for the bulk 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 to managed care provides an opportunity to identify this. We show that the reduction in spending generated by managed care doesn't materialize until after the 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.[9] This is important because it sug-

[7] The findings from this plan transition reinforce the findings from auto-assignment. We show that, holding fixed the nominal identity of this plan and its enrollees, overall spending dropped following the conversion from FFS to risk-bearing MMC, driven primarily by substitutions to therapeutic alternatives and outright reductions in quantity rather than price, and led once again by a large (21%) reduction in pharmacy spending. Moreover, the magnitude of spending reductions by therapeutic class were strikingly similar across the two different identification strategies, suggesting different plans target similar classes of drugs for savings.

[8] For example, if physicians shift their prescribing behavior *for all enrollees* in response to utilization management by the MMC plans (Glied and Zivin, 2002), pharmacy spending could also fall somewhat for FFS enrollees due to the introduction of managed care. We do not find evidence of substantial spillovers from these two natural experiments.

[9] Further, we find that the spending reductions in managed care are concentrated among enrollees using drugs targeted by real-time adjudication whereas disenrollment rates from managed care are higher among all types of enrollees.

gests that dissatisfaction with managed care may be linked to the management of medical benefits — where managed care appears to abrade consumers but not reduce spending — or to other aspects of MMC provision that are unrelated to utilization 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 government-sponsored healthcare in Medicare (Cabral, Geruso and Mahoney, 2018; Curto et al., 2021; Duggan, Starc and Vabson, 2016) 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, 2020; Garthwaite and Notowidigdo, 2019) has attempted to evaluate the causal impacts of competing managed care plans in a publicly-funded, managed competition setting that did not include a FFS option for comparison. Other work has only been able to assess 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 on 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, our study helps to partially reconcile disparate and apparently conflicting findings in the prior literature evaluating the privatization of Medicaid. Our study, which contrasts MMCs' impact on cost and patient well-being across the medical and pharmacy domains, is consistent *both* with studies showing that MMC plans efficiently manage prescription drug benefits (Layton et al., 2022; Dranove, Ody and Starc, 2021), and simultaneously with reductions in quality or patient well-being that might be due to management of the 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 blunt tools for managing medical benefits, where we observe small cost savings and reductions in quality and satisfaction.

Third, 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 large administrative burdens for providers (Dunn et al., 2021) but not *directly* impacting care, real-time adjudication in pharmacy allows plans to precisely target and interdict healthcare consumption immediately before the care would have otherwise been dispensed.[10] While the prior literature has focused on the administrative burdens caused by the use of utilization management, we show it may also be a powerful tool, in some contexts, to constrain spending. In principle, the public FFS program could use prior authorization and real-time adjudication (pharmacy denials) in the same way as managed care does. In practice, however, pharmacy denials in the FFS system — which 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.

Finally, 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 private plans 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, 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) in the presence of a binding technological or managerial constraint. For example, while a plan may wish to curtail the intensity of care, our results suggest it may have less *capacity* to do so for medical services than it does for prescription drugs. Whereas this type of capacity constraint is a mainstream consideration in the analysis of government functioning, the focus

[10] Retail 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/non-pharmacy difference in real-time adjudication.

of the healthcare privatization literature has largely been on firm incentives, with less attention given to the mechanisms firms use and the capacity constraints they face.

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 FY 2019, and accounts for close to 7% and 30% of total federal and state annual spending, respectively (Kaiser Family Foundation, 2021*a*). Medicaid coverage is traditionally provided to low-income and disabled populations free at the point of service. Nationally, Medicaid coverage has become central for key populations in the US, covering 38% of children, 42% of births, and 45% of non-elderly individuals with a disability in FY 2019 (Kaiser Family Foundation, 2019).

2.2 Medicaid Managed Care

For most of the program's existence, Medicaid has been administered as a "fee-for-service" program in which the Government contracted directly with a set of physicians and hospitals willing to accept their reimbursement rates. As enrollment and spending in the program has grown, states, motivated by the goal of cost control and the idea that choice and competition between private plans can best satisfy heterogeneous consumer preferences, have shifted to managed competition between private health plans as the dominant policy choice for the provision of services in Medicaid (Gruber, 2017). Under typical managed care arrangements, states contract with private health plans which, in turn, assume financial risk and contract with a restricted network of physicians and hospitals that provide care for their beneficiaries. The plans are generally paid a fixed, per-enrollee payment in exchange for administering benefits and bearing the full risk for any covered health care expenses. In 1992, approximately 10% of Medicaid beneficiaries were enrolled in private managed care plans. As of 2020, 57 million beneficiaries receive primary coverage through comprehensive managed care plans, representing over 70% of the Medicaid program (Kaiser Family Foundation, 2021*b*). The expansion of Medicaid via the Affordable Care Act has only accelerated the growth of managed care, as close to 90% of non-elderly beneficiaries since 2014 have been enrolled in managed care plans (Medicaid and CHIP Payment and Access Commission, 2020).

The transition from fee-for-service to managed care creates strong incentives for cost control via the high-powered contracts that make private insurers residual claimants on any cost savings (Laffont and Tirole, 1993). This is intended to create incentives for plans to reduce wasteful spending and better coordinate services. 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 health care services and managing care than state agencies.[11] 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 fee-for-service (FFS) program, managed care plans may generate productive efficiencies through several mechanisms: (i) the selection and organization of providers (e.g., excluding inefficient providers); (ii) price negotiations with providers; or (iii) utilization and care management, in their 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 attempted to investigate whether privatization has lead to greater efficiency in Medicaid, to date the effectiveness of the tools available to managed care plans has been less well studied.

[11] Another argument that has been made in favor of Medicaid managed care is that it offers states greater budget predictability by shifting risk to private health insurers, but the evidence for this is mixed (Perez, 2018*a*).

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 by 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 Medicaid managed care (MMC) plans to deliver Medicaid benefits to enrollees.[12] However, during our study period (i.e., 2010-2016), Louisiana was in the midst of its transition from fee-for-service (FFS) to MMC, with the explicit goal of achieving cost savings (Hood, 2011). 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, a number of competitive plans largely consistent with MMC markets across the US. As in most states, Louisiana requires competitive bidding for a select number of plans to both maximize the benefits of competition and ensure enough market share to enable risk pooling. All plans operated statewide and were subject to uniform benefit designs.

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 similar to other state capitation payment amounts (averaging $246 per member per month) for comparable populations around this time period (Centers for Medicare and Medicaid Services, 2016). The two FFS plans, on the other hand, were paid small monthly management fees (averaging $11 per member per month) for narrower care coordination and only contracted directly with primary care providers (PCPs).[13] Services other than primary care were accessed via the state's legacy FFS network—i.e., the set of providers willing to accept Medicaid enrollees at the FFS payment rates—and paid directly by the state. These FFS payments from the state directly to providers accounted for 87% of the annual spending for enrollees in the managed FFS model.

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

[13]These 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.

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.[14] 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 or managed FFS. Most enrollees (68.9%) were auto-assigned.[15]

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 would be 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. 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.[16] In all analyses we control for this unit of randomization (i.e., enrollee's linked 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 primary care providers prior to the switch from FFS.

Lastly, there was imperfect compliance with auto-assignment because Medicaid enrollees could

[14] The 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 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.

[15] Calculated as 94,976 divided by 137,937, respective sample sizes of the auto-assignee and overall (auto-assignees plus active-choosers) samples in Table A1.

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

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. 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 managed FFS model. We do this because several categories of Medicaid eligibility were excluded from the transition (e.g., nursing home residents).[17] 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 health care claims for these "dual-eligible" enrollees whose primary payer would be Medicare.[18] 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").[19]

[17] Enrollees 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.

[18] Finally, we 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.

[19] Table A1 provides summary statistics for these samples.

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. 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., **?**Curto et al., 2019; Geruso, Layton and Wallace, 2020; Dunn et al., 2021) in healthcare, we focus analysis on the outcomes examined in those studies. The outcomes fall into three broad domains: health care 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 the Louisiana Department of Health (LDOH). 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 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 fee-for-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 LDOH 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.[20] 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). For example, we include as a high-value care measure an indicator for whether enrollees fill a prescription for a statin.[21] As an example of a low-value care measure, we assess the likelihood an enrollee uses the emergency department for avoidable reasons (Medi-Cal Managed Care Division, 2012). 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, 2020). Following the literature, we term this measure "willingness-to-stay" and assume that enrollees' preferences are revealed through their choices to switch plans.[22] (The traditional willingness-to-pay measure is not defined here, because there are no premiums in Medicaid.) Given the well-documented consumer choice frictions in this domain (e.g., **?**Handel, Kolstad and Spinnewijn, 2019), a useful feature of this measure is that it reflects a choice that occurs after enrollees experience their assigned plans (Israel, 2005), rather than inferring preference from an initial, possibly poorly-informed, choice.

Healthcare claims denials. The claims data include all fully adjudicated claims regardless of whether the service was ultimately paid or denied. 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

[20]We 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.

[21]We use the The Anatomical Therapeutic Chemical (ATC) Classification System to identify anti-depressant, anti-hypertensive, statin and diabetes medication prescriptions (for Drug Statistics Methodology, 2005).

[22]For the first three months after assignment enrollees may switch for any reason, after which a nine-month lock-in period begins during which they may only switch for "good cause." (Louisiana Department of Health and Hospitals, 2016)

their health plan.[23] See Appendix D.3 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).[24]

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 experiment in which one health plan was forced by the state to switch from the FFS provision 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

[23] In 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.

[24] The 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.

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. In some specifications, we include additional enrollee-level controls, X, to improve precision, though we present results with and without these to demonstrate that our point estimates are not sensitive to their inclusion, consistent with the maintained assumption of conditional random assignment. 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, as we discuss below.

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

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

where $\widehat{ManagedCare}_{it}$ 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 does 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 differences-in-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:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{\tau \neq -1} \beta_\tau AssignedManagedCare_i + v_{it}. \tag{3}$$

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 or quarters, 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_t—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 for all groups enrollment is zero. 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 $\times$ year observations have enrollees in their assigned coverage model. Pooling across 2012–2014, the first-stage coefficient (π) from Equation 1 is 0.76 (s.e. 0.03), with a first-stage F-statistic of 678 ($p < 0.001$).

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 21 baseline characteristics indicates statistically significant imbalance. (The balance test is not corrected for multiple inference.) 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 A2), underscoring the importance of our reliance on quasi-experimental variation to identify the causal effects of managed care.[25]

Monotonicity, a third key assumption in any IV, 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. Moreover, we think violations of monotonicity in this setting are highly unlikely given that enrollees spend at least one month in their assigned plan and must actively "opt out" of those assignments; hence, it seems unlikely that being assigned to MMC would make an enrollee *less likely* to enroll in MMC than being assigned to FFS.

3.3 External Validity

One potential concern for external validity is that auto-assignees may be healthier and less-engaged with the healthcare system. While the auto-assigned population spends \$400 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, 40% of overall spending comes from outpatient, 9% from inpatient, and 11% from behavioral health, 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 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.[26]

As described in detail in Section 5, we also use a second, complementary research design that

[25] The 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.

[26] For transparency, we also present estimates from OLS regressions of the effects of enrollment in MMC (relative to FFS) using the broader Louisiana Medicaid population, relying on baseline characteristics we can construct in our rich administrative data (e.g., health care 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.

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 stayed put. 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: Auto assignment to Medicaid managed care vs. FFS

4.1 Healthcare Use and Spending

Before reporting our main IV estimates of the impact of Medicaid managed care (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) 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 enrollees over nearly four years (February 2011 – December 2014). 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. The largest reduction in spending emerges after pharmacy was carved-in, which we analyze further below. 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 pattern of findings—in dollar levels in Figure 2—is robust to alternative transformations of the dependent variable to address the extreme values and skew that are common to healthcare spending (inverse hyperbolic sine and log) and to aggregating healthcare spending at the month, rather than quarter, level. See Figures A3 and A4.

Table 3 presents the main results: instrumental variable estimates of the impact of MMC enrollment in the post-assignment 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 time- and duration-independent, so it can be summarized by a single coefficient.[27] The restriction to a single coefficient estimate is a useful summary, but we also report IV results in the appendix that are separately estimated within each period (Table A10).

We find an economically and statistically significant reduction in total healthcare spending associated with managed care of roughly \$82 per year (Row 1, Column 3). 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.[28] An important contrast is that here the spending differences emerge without exposing enrollees in the different plan types to differential financial risk.

For comparison we estimate the same effects using OLS, which reflect both causal plan effects and enrollee selection. The OLS results (Table 3, Column 5) recover differences in healthcare spending that are almost 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.

Panel A of Table 3 presents our spending results by components of care. We find suggestive evidence of reductions in medical spending, driven by a reduction of \$19 in the outpatient setting with no effect of assignment to managed care (relative to FFS) on inpatient spending. The largest effects are for pharmacy spending, with managed care leading to a reduction of \$68, or 18%, in annual pharmacy spending. This is 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

[27] This is analogous to estimating the difference-in-differences specification via a single post$\times$MMC effect, rather than MMC interacted with post-treatment periods.

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

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. Below, we extend their (and our) result by demonstrating the mechanisms by which these large spending reductions are achieved, a key contribution of our paper.

Panels B and C of Table 3 present results stratified by gender and predicted healthcare spending. In every subsample, assignment to managed care was associated with economically and statistically significant reductions in healthcare spending. We find little evidence of heterogeneity in the treatment effect by gender, but large differences in levels based on health status at baseline, prior to assignment (i.e., predicted spending). Enrollees in the highest quartile of predicted spending experience spending reductions due to managed care that are nearly five times larger than the reductions in the lowest quartile, with the effect sizes progressing monotonically between these extremes. As a percentage of the mean spending within each quartile group, managed care leads to a 4-9% decrease in healthcare spending regardless of health status, consistent with the overall result.

4.2 Pharmacy Use and Spending

Because the spending reductions generated by managed care are concentrated in prescription drugs, we next examine the effects of managed care on pharmacy quantity, 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 prescription drug quantity 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 (Figure A6). This is an important finding, 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% decrease in the quantity of brand drug prescriptions and a 10% increase in generic drug quantity. (The percent changes differ due to differences in the pre-carve-in base rates for brand and generic drug quantity.) 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 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 an approximate decrease of 2.4 days supply per prescription, or a 10% 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 managed FFS).[29] The MMC plans enforce these quantity limits (e.g., 30 tablets/month) by denying claims at the pharmacy — before a prescription is dispensed — unless there is prior authorization for a higher quantity. Hence, managed care plans both shift the composition of drugs from brand-to-generic, but also 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 26% 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.[30]

Panel D reveals that the net effects of managed care on pharmacy spending are primarily driven by a reduction of approximately \$22 per enrollee per quarter (or \$88 annually; 26%) in brand drug spending, with no statistically significant offsetting increase in generic drug spending. 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

[29] While 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.

[30] This finding is reinforced in a more formal decomposition in the style of Brot-Goldberg et al. (2017) presented in Appendix C.2.

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 inclusive of rebates (which occur ex post and as a lump sum payment). Therefore, net savings could be less than the 18% we report in Table 3 if there were a decline in overall state-level rebates that coincided with the sharp decline in spending evident in Figure 3. In Figure A5, we use a separate, state-level database on rebates to show that rebates do not, in fact, decline after the pharmacy carve-in.[31] This result—that transaction-level savings were not 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). Dranove et al. shows (using a national difference-in-difference design and aggregate, state-level data) that as states carved-in prescription drugs to managed care responsibility, rebates remained unchanged, even as the mix shifted to generics (as here) and even as the transaction-price-denominated spending fell by about 20% (as here).

4.3 Effects on High-value and Low-value Services

We next examine whether the MMC-FFC 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, 2020).

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 care services 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 (std. err. = 0.70) in the likelihood of enrollees receiving recommended annual primary care visits. However, we did not find evidence that assignment to a MMC plan led to reductions in the utilization of well child visits, chlamydia or cervical cancer screening, or dental care. We observe no reduction in the use of behavioral health services overall, but do see a potentially concerning reduction in the use of behavioral health services among children. We also examine

[31] We measure the share of point-of-sale drug spending that is returned in rebates over time. The share numerator is constructed from the Medicaid Financial Management Reports, and the denominator is constructed from the Medicaid State Drug Utilization Data, following the same procedure as in (Dranove, Ody and Starc, 2021).

follow-up care for children prescribed ADHD medication, which is an important category for this study population, and find a null effect. The results in Panel B in column 3 suggest that enrollees assigned to MMC plans were *more likely* to use potentially high-value prescription drugs (e.g., statins and diabetes drugs), despite the large reductions in prescription drug spending generated by assignment to an MMC plan.

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 value-care 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 visits, with enrollment in MMC (relative to FFS) leading to 1.17 percentage points more enrollees receiving any care for non-emergency conditions in the emergency department, 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 E.D. care as a substitute for office-based primary care.

Taken together, the effects on high-value and low-value services are mixed. Our finding of smaller effects (with varied signs) in these categories is consistent with the small overall impacts of MMC enrollment on healthcare utilization outside of pharmacy in our setting.

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.[32] 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, and may reflect experienced utility in the plan in addition to ex-ante preferences prior to enrollment (in the spirit of Israel, 2005).[33] We construct an indicator variable that is equal to one if an enrollee's plan matches their assigned plan, and call this *willingness-to-stay*.

[32]For 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.

[33]While this differs from a traditional willingness-to-pay measure because there are no premiums, an ex-post measure of consumer satisfaction has advantages given the difficulties of interpreting willingness-to-pay measures in the presence of choice frictions (e.g., **?**Handel, Kolstad and Spinnewijn, 2019).

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 14.54 pp (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 little cost-savings (Figure 2).

5 Evidence From the Discontinuation of Managed FFS

5.1 Background

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.[34] Neither concern would imply bias in our estimates of the local average treatment effect (identified via random assignment), but either could imply that our findings were not fully generalizable to the state's overall Medicaid program. They may not be informative, for example, of the spending, satisfaction, and health effects of transitioning the entire state to managed care.

In this section, we introduce a complementary research design that is not subject to these inter-

[34]For example, relative to the auto assignee sample, enrollees in the difference-in-differences sample were older and utilized more healthcare services, particularly generic drugs (Table A8).

pretation issues. It is based on a separate policy experiment that occurred three years after the main auto-assignment event exploited above: In February 2015, the managed 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.[35] 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. Using a difference-in-differences framework, we compare outcomes for enrollees in the transitioned plan before and after its change from FFS to MMC to outcomes for enrollees already enrolled in MMC plans, for which there was no policy change during this period.

5.2 Econometric model

The difference-in-differences specification for this second natural experiment is estimated at the enrollee-year level in the following regression:

$$Y_{it} = \alpha + \beta TransitionedPlan_i \times Post_t + \gamma Post_t + \lambda TransitionedPlan_i + \varepsilon_{it}, \tag{4}$$

where Y_{it} is an outcome for enrollee i at time t (quarter or year depending on specification); $TransitionedPlan$ is an indicator variable set to one if an enrollee was continuously enrolled in the transitioning plan (for the year prior to and after the plan-level transition in January 2015) and zero otherwise (i.e., zero if the enrollee was in one of the control plans); $Post_t$ is an indicator for any time period in the year following the state-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. Additional details on how we estimate Equation 4 and decompose the sources of spending reductions are available in Appendix Section C.2.

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 Feb 2014 to Jan 2016), spanning the year prior to and after the transition from FFS to MMC.The sample is comprised of 495,537 enrollees: There are 189,252 in the transitioned plan—i.e., those enrolled in the plan that shifted from FFS to MMC—and 306,285 enrollees in the control plans, who were continuously enrolled in one of the three

[35] The 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.

preexisting MMC plans that did not experience any substantial policy changes in February 2015.[36]

5.3 Results of plan transition

We begin in Panels A and B of Figure 5 by plotting raw time series of quarterly mean medical and pharmacy spending, respectively, at the enrollee level.[37] Mean spending is presented separately for enrollees in the transitioned plan (that switched from the FFS to MMC model) and in the three control plans. The "Pooled Control Plans" line is an enrollee-weighted combination of the three control plans. The vertical line in the figures represents February 2015, when the state-mandated switch to MMC for the transitioned plan occurred. Relative to the control plans, 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 half year of the transition.

Difference-in-differences regression estimates corresponding to Equation (4) are presented in Table A9. These summarize the (time-varying) effect evident in Figure 5 into a single coefficient and examine impacts on subcategories of spending. The pattern of results is similar to the analyses based on auto-assignment in the first natural experiment, including for low- and high-value services. For example, we find that when the transitioned plan switched from FFS to MMC, there were reductions in measures of primary care access (e.g., child access to primary care, well-child visits) as well as a 0.5 pp (5.3%) increase in the share of enrollees with avoidable ED visits in a year (Panels B and C, Column 3 of Table A9).

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

[36]To facilitate a comparison of our difference-in-differences estimates to those based on the auto-assignee sample in Section 4, we reweight the difference-in-differences sample to balance its characteristics with those of the auto-assignee sample in our primary analyses (See Appendix Section C.3 for additional details).

[37]The data is residualized on calendar quarters to adjust for seasonality.

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 no consumer cost sharing, 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 (e.g., copays, deductibles). 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 provide evidence that utilization management (e.g., prior authorization, step therapy, and quantity limits), for which we observe the plan's enforcement mechanism (real-time denials), is the key driver of the reductions in Medicaid spending generated by managed care plans in our context.

6.1 It's Not Payments, Providers, and Case Management

Before presenting the direct evidence in support of the utilization management mechanism, we note that we can provide suggestive evidence against—and in some cases, clearly rule out—certain other explanations. To investigate the importance of differences in provider payment, 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, NDC, or DRG, depending on the service type considered.[38] See Appendix C.2 for additional details.

Column 3 of Table 5 reports our instrumental variable estimates of the impact of MMC enrollment in the post-assignment period (2012–2014) on price-standardized spending. The IV estimates for the non-repriced data (our main estimates) are repeated in column 2 for comparison. The estimates

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

reveal that price differences paid by the state FFS schedule and private MMC plans account for a relatively small share of the overall spending difference. The coefficient of interest on total spending shrinks from -\$82 to -\$57, a statistically insignificant difference of 30%. By service category, prices account for almost all of the small reductions in outpatient spending and for almost none of the large reductions in drug spending. Although the finding that prices play only a small role in our setting contrasts with early work comparing FFS to HMOs by Cutler, McClellan and Newhouse (2000), it is consistent with contemporary work comparing FFS and managed care, including Curto et al. (2019), which evaluated managed care spending relative to FFS in the Medicare system.[39]

One hypothesized mechanism for how managed care reduces spending is by steering enrollees to more efficient providers. The remaining columns of Table 5 investigate 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 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). Full details on how we assign enrollees to providers are in Appendix D.1. 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). See Appendix D.2 for more details.

Specifications controlling for provider network breadth (Table 5, Column 4) reveal that MMC plans do not appear to constrain costs by restricting access via narrower provider networks. Nor are MMC plans saving by steering enrollees to more efficient providers: Comparing auto-enrollees assigned to MMC and FFS who shared the same primary care provider (via post-assignment provider fixed effects in column 5) does not significantly reduce the large estimated differences in spending. These results imply that MMC-FFS spending differences persist within equally restrictive networks

[39]One possible explanation for the contrast of recent findings, including ours, with Cutler, McClellan and Newhouse (2000)—which found that prices accounted for a large FFS versus HMO spending difference in the treatment of heart disease—is that the Cutler et al. result could have been affected by the history of rate setting regulation in Massachusetts (their study context), which exempted HMOs from certain surcharges. The relevant history and implications are discussed in Clemens and Ippolito (2019).

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 health care 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, networks, steering to providers, primary care, and case management do not appear to be driving the large MMC-FFS spending differences. 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 fee-for-service, 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-to-generics 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 A6 shows an increase in generic efficiency timed with the denials regime. This measure is useful, but 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 C.2, but we summarize the results here as showing that between one-fifth and one-half of the spending reduction was attributable to substitution from brands to generics within the same therapeutic class (but potentially across molecules), and about one half of the spending reduction was attributable to substitution across therapeutic classes or from outright reductions (See Figure A8 and Tables A10 and A11).[40]

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

[40]These findings are broadly consistent with evidence from Medicare Advantage that spending reductions in managed care are driven by quantity (e.g., Landon et al., 2012; Curto et al., 2019), though the quantity reductions in our context appear less broad-based and more targeted, particularly for prescription drugs.

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 these 58 classes of prescription drugs. Panel B of Figure 6 plots instrumental variable estimates of managed care's spending effects (relative to FFS) on each drug class against the share of claims denied by managed care plans in that therapeutic class during the spike period just after carve-in.[41] 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.[42] In Figure 7 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.[43] 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, 2020), 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.

It is important to understand that denials and lower spending within a class do not imply fewer filled prescriptions in that class. Figure 8 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

[41] To 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.

[42] A similar dose-response relationship exists if we restrict our analyses to children (Figure A9).

[43] The correspondence between the two identification strategies and, within the auto-assignee sample, between the various managed care plans, 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.

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. For therapeutic classes with claims denial rates below 40%—the vast majority of classes, and the largest classes—the cloud of points is centered on the horizontal line at zero, consistent with no substantial MMC-FFS differences in the quantity of prescriptions (ultimately) filled.[44] Together with Panel B of Figure 6, which showed that *spending* in heavily-managed drug classes was reduced, Figure 8 indicates that cost-savings are achieved via utilization management that drives within-class substitution, for most drug classes. Take, for example, the branded prescription drug Pataday, 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 $7.55 and is administered once a day, being denied by private plans at the point of service and replaced with a subsequent prescription of Ketotifin, a generic drug in the same ATC-4 class that has an average unit cost of $0.55 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 $1.46, 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 that lack prior authorization) in the same way as managed care does to achieve savings. In practice, 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 A10, 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 A11 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.

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 carve-in—is examining the correlation between denials and drug spending at the level of individual drugs.

[44] The drug classes targeted most aggressively by MMC plan denials (e.g., expectorants, antiallergics, agents for dermatitis, and antibiotics) were among the few classes where MMC spending reductions were generated (at least partially) by quantity reductions relative to FFS, revealing a more complex strategy by which utilization management may be used to drive both substitution and outright quantity reductions.

To investigate this, in Figure A12, 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.[45]

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. Figure A12 shows that for the drugs most intensely targeted with denials, these denial rates remain low after the spike, *even as paid claims remain steady and low*. In other words, once the chain of professionals responsible for drug prescription and delivery understand the new regime, they stop generating scripts that lead to denials. In summary, there is clear evidence that utilization management, deployed via real-time adjudication and denials, drives behavior change in prescription drug use and ultimately generates reductions in spending.

6.3 Tradeoffs

A difficult question in this setting is whether the apparent tradeoff between cost savings to the state and the beneficiary dissatisfaction coincident with those savings is appropriate. One lens through which to view this question is social efficiency—the net benefit to the recipient (as evaluated by the recipient and revealed in their market choices) minus the social resource cost of care.[46] Medicaid beneficiaries do not face premiums that could reveal valuations, but estimates of own-price premium elasticities from adjacent markets, especially the lowest-income tranche of the ACA marketplaces, suggest own-price elasticities at the insurer level ranging from about -2 to -3.[47]

In our setting, enrollees randomly assigned to managed care leave their assigned plans at an aver-

[45] In FFS, quantity for these drugs also declined, but with a lag relative to MMC, and at a slower rate. This may reflect spillovers from MMC to FFS, as providers adapt their prescribing patterns for all Medicaid enrollees.

[46] Alternatively, one might take the perspective of maximizing the regulator's objective function, but as far as we know, no studies of Medicaid have to date produced a model of the Medicaid regulator's objective function that could be fit to our micro data.

[47] Own-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.

age rate of about 16 percent. The savings to the Medicaid program of being assigned to managed care are about 6 percent, implying an savings-switching elasticity of 2.8.[48] Therefore, own-premium enrollment elasticities in the range of the literature would imply that cash rebates to Medicaid enrollees equal to government 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 the revealed policy preference for providing this care. And here, as in every context 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, in particular—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 a statistically insignificant \$3 per beneficiary per year, compared to an overall average of \$62 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 had already accrued before drugs were even carved in to managed care responsibility.

[48] In 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 -5.7% cost difference ($= -\$82/\$1,451$) effect of being assigned to managed care. This naive calculation implies an elasticity of -2.76 would mean that a rebate equal to the savings (\$82) would exactly counteract the disenrollment effects.

To interrogate this further, Panel B of the table 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, perhaps 25 percent ($= 1 - \frac{11.3}{14.54}$, 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 very little in the non-drug domain, where we document dissatisfaction but fail to find associated savings. This pattern stands in contrast to the history of Medicaid privatization, which, despite many differences across state programs, almost universally was characterized by privatization that started with the outsourcing of non-drug benefits, and outsourced drugs to managed care only later, if at all.

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 health care spending, quality, and consumer satisfaction between enrollees in a state-administered fee-for-service (FFS) system and private Medicaid managed care (MMC). 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 10% 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 prior authorization via 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 prior authorization 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. 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.

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) in the presence of a binding technological or managerial constraint for particular services. Although the *incentives* for constraining spending existed across all healthcare service domains for MMC plans in our setting, these plans appeared to have the *capacity* to directly affect care provision primarily 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 blunt tools for managing medical benefits, where we observe small cost savings and reductions in consumer satisfaction and health care quality. 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 public provision may be preferable.[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.

[49]One 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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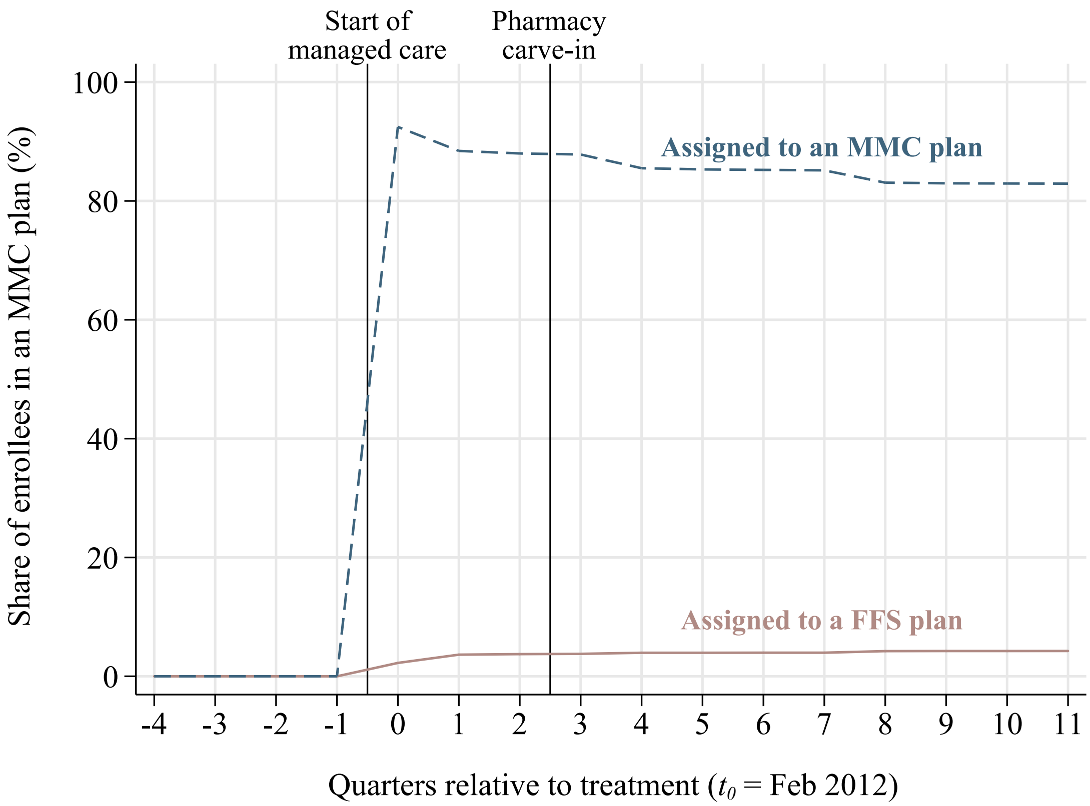
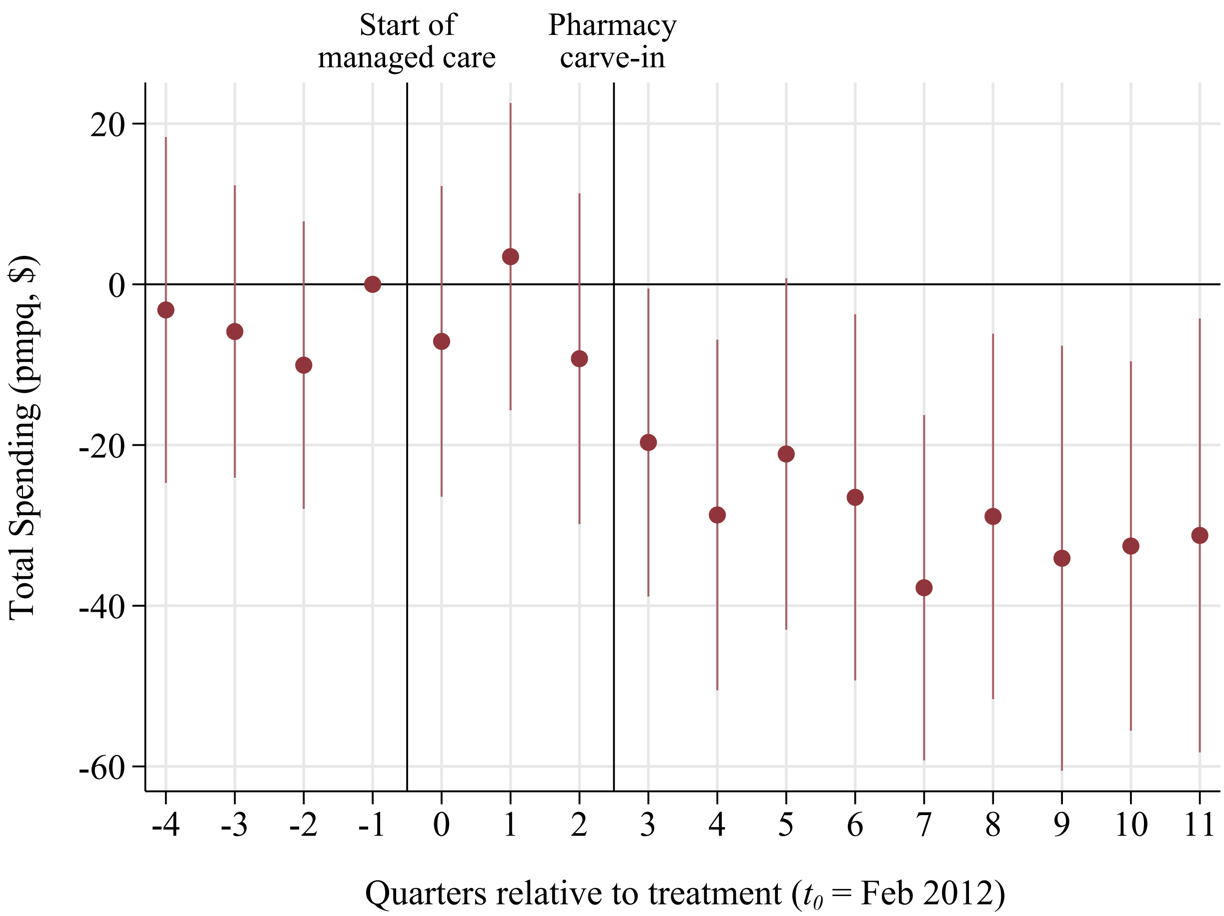
Figure 1: First Stage: Medicaid managed care assignment and enrollment (raw means)



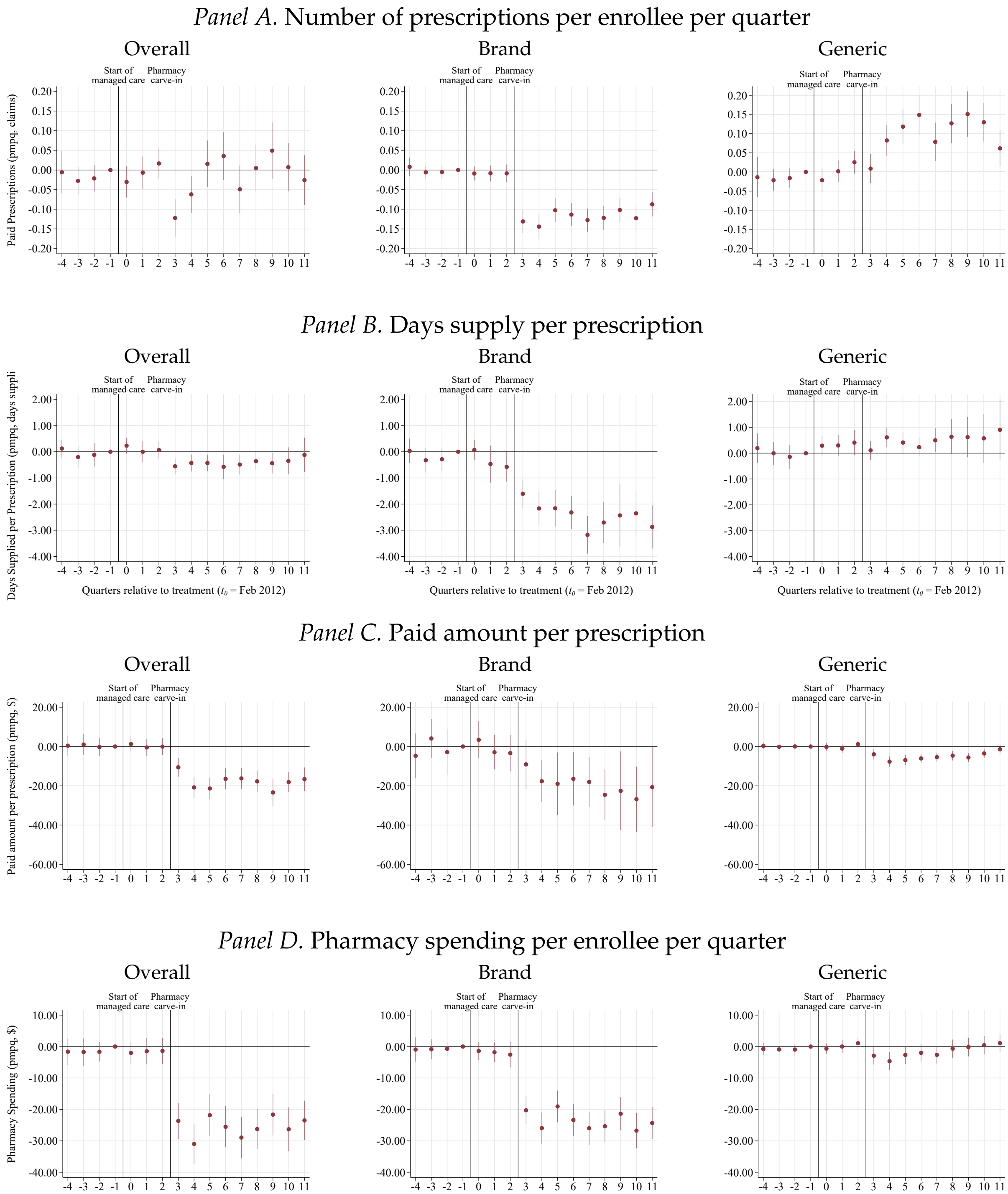
Note: Figure plots mean enrollment rates in Medicaid managed care (MMC) over time for enrollees assigned to the MMC and managed Fee-for-Service (FFS) delivery models. Observations are at the assigned model × quarters level. Time, in quarters, is along the horizontal axis. The leftmost vertical line indicates 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 plan type of assignment in February 2012. The sample here is the same balanced panel of enrollees that forms the main analysis. See Section 2.6 for additional detail regarding the sample construction.

Figure 2: Main Result: Impact of assignment to managed care on overall healthcare spending



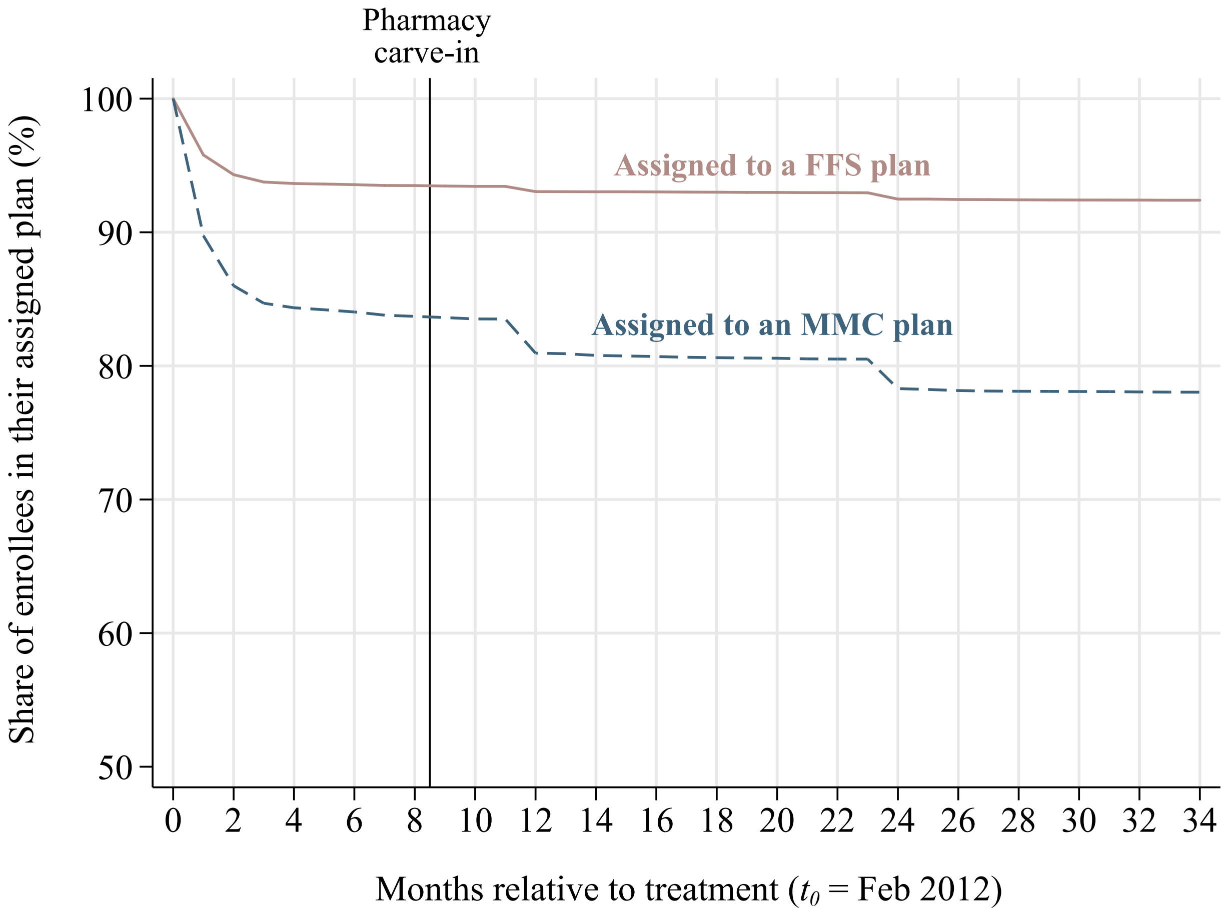
Note: Figure presents a difference-in-differences event study comparing health spending across assignees to MMC and FFS. Estimates are based on a balanced panel of 85,668 continuously-enrolled recipients for the 47 month (February 2011–December 2014) period depicted. Time, in quarters, is along the horizontal 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. Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 99.77^{th} percentile. 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 reported. See main text and Appendix Section C.1 for additional detail on variable construction and specification.

Figure 3: Main Result: Impact of assignment to managed care on pharmacy spending and quantity



Note: Figure presents a series of 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 panel title. The first column reports the overall effect, and the remaining columns narrow attention to generics and brands separately. Overall, these plots show a reduction in pharmacy spending arising primarily from brand drugs. See Figure 2 notes for additional detail.

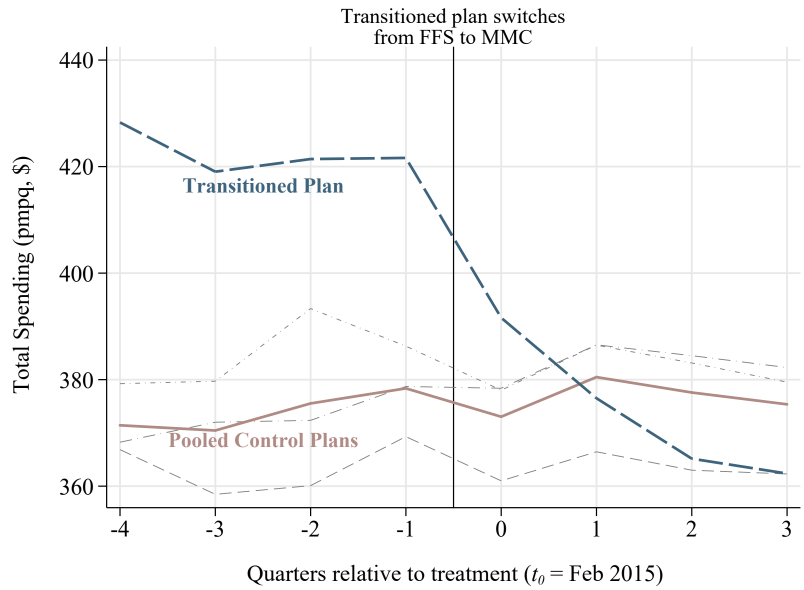
Figure 4: Revealed preference: Enrollees assigned to managed care are more likely to switch plans



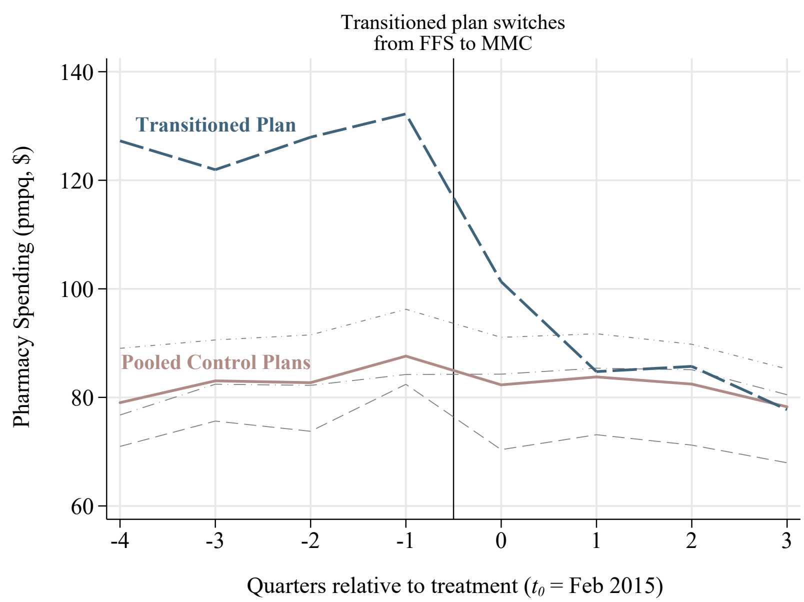
Note: Figure plots mean enrollment rates in three Medicaid managed care (MMC) plans and two FFS plans over time. Observations are at the assigned model × months level. Time, in months, is along the horizontal axis. The vertical line indicates when pharmacy is carved into Medicaid managed care. The vertical 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 (i.e., the MMC or FFS model). The sample here is the same balanced panel of enrollees that forms the main analysis. See Section 2.6 for additional detail regarding the sample construction.

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

Panel A. Overall Spending



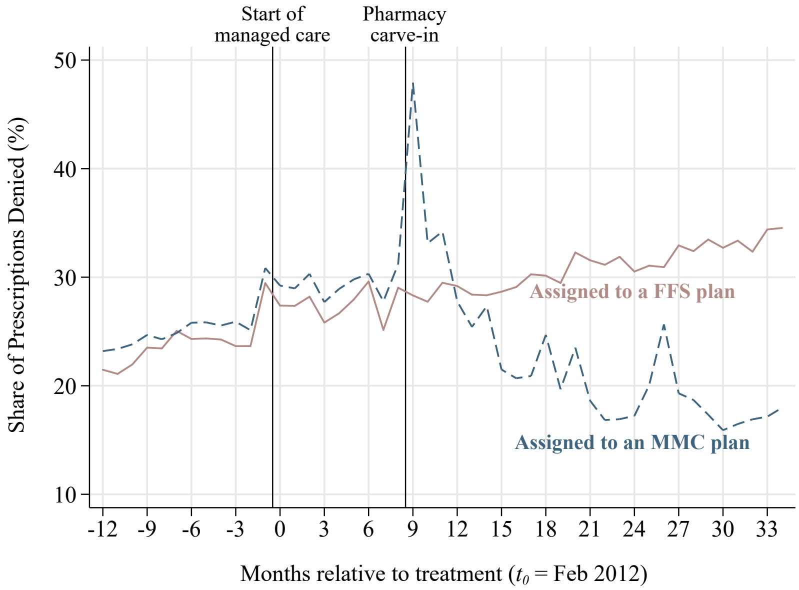
Panel B. Pharmacy Spending



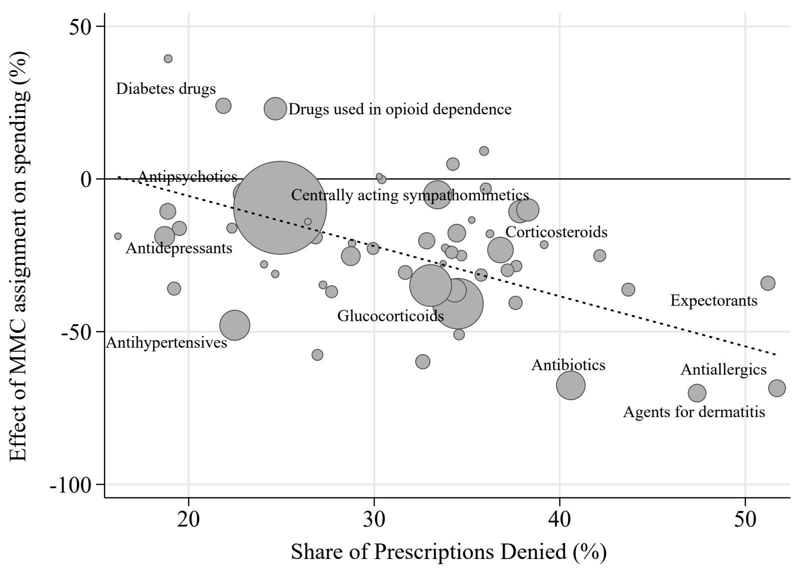
Note: Figure plots means of spending over time by plan. Observations are at the plan × quarters level. Time, in quarters, is along the horizontal axis. The vertical line indicates when the treatment plan transitioned from managed FFS to become a full-risk managed care plan. The plans that were already 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 beneficiary-months among continuously-enrolled beneficiaries. 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.3 for additional details.) Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 98.83th percentile. Components of spending do not sum up to "Total" due to Winsorization. See Section 5.1 for additional detail regarding the sample construction.

Figure 6: Mechanisms: Utilization management (denials) drive spending reductions

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

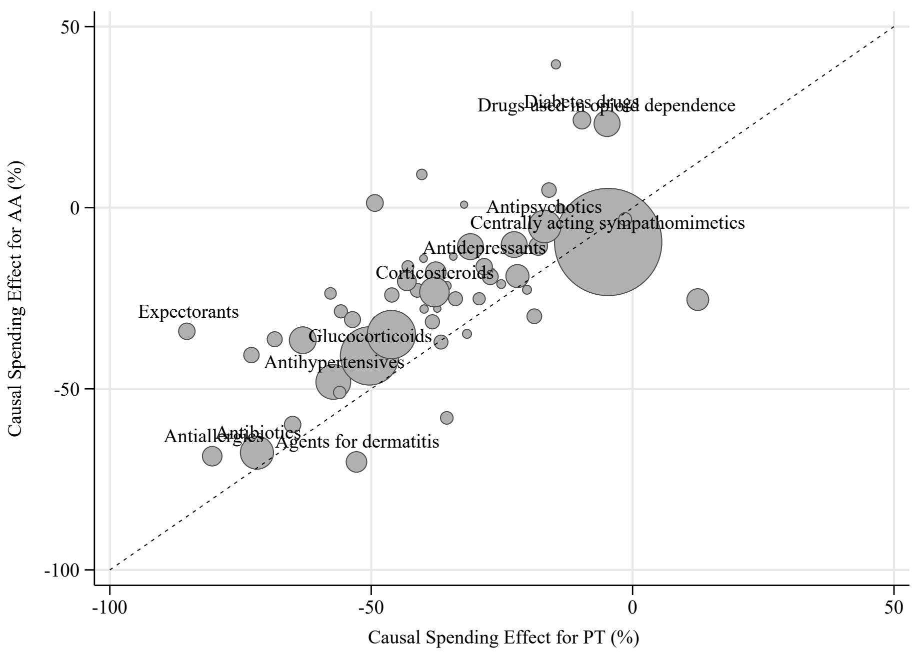


(b) *Panel B.* Spending reductions by therapeutic class align with denial rates



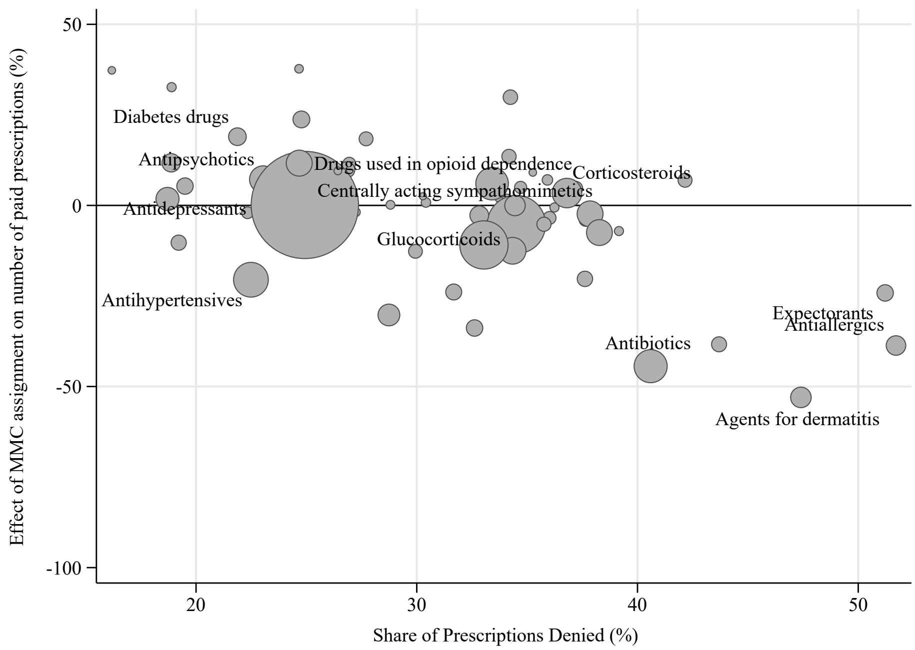
Note: Figure presents evidence that pharmacy denials are a key mechanism driving the managed care spending effects. Panel A presents a time series plot of overall denial rates (percent denied) separately for MMC and FFS plans. Observations are at the assigned model $\times$ months level. Time, in months, 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 (i.e., Anatomical Therapeutic Chemical Classification, level 4). Marker sizes are proportional to spending. To measure the managed care claims denial rate for 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 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. See Section 2.6 for additional detail regarding the sample construction.

Figure 7: 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 our 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.3 for additional details.) See Section 5.1 for additional detail regarding the Plan transition sample construction and Section 2.6 for the Auto-assignee sample.

Figure 8: Mechanisms: Denials caused within-class substitutions, not outright reductions for most drug classes



Note: Figure shows how quantity (measured as filled prescriptions) changes as a result of utilization management. The plot presents 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 (i.e., Anatomical Therapeutic Chemical Classification, level 4). Marker sizes are proportional to spending. See Figure 6 notes for additional detail.

Table 1: Summary statistics

	Mean (1)	Std Dev (2)	Max (3)	Min (4)	N (5)
Panel A. Enrollee Characteristics					
Female (%)	52.92	49.92	100.0	0	284,928
Age at baseline	9.36	7.49	63.0	0	284,928
Panel B. Enrollee-year spending ($)					
Total	1 451.35	2 427.61	25,004.0	0	284,928
Medical	1 052.74	1 815.46	18,257.0	0	284,928
Inpatient	97.48	747.79	9,891.0	0	284,928
Outpatient	590.29	820.12	7,342.0	0	284,928
Pharmacy	381.45	948.76	10,408.0	0	284,928
Brand Drug	229.30	757.06	8,967.0	0	284,928
Generic Brand	149.63	345.53	3,427.0	0	284,928
Panel C. Any Annual Utilization of High- or Potentially High-Value Care (%)					
Annual Well-Child Visits	49.34	50.00	100.0	0	168,315
Access to Primary Care	80.46	39.65	100.0	0	280,915
Chlamydia Screening	59.67	49.06	100.0	0	10,403
Cervical Cancer Screening	67.19	46.96	100.0	0	13,759
Any Follow-up care after ADHD Rx	51.17	49.99	100.0	0	3,881
Behavioral	7.40	26.18	100.0	0	284,928
Dental	55.18	49.73	100.0	0	284,928
Statins	0.30	5.51	100.0	0	284,928
Anti-Hypertensives	2.73	16.31	100.0	0	284,928
Anti-Depressants	3.55	18.52	100.0	0	284,928
Diabetes Medication	0.58	7.56	100.0	0	284,928
Panel D. Any Annual Utilization of Low- or Potentially Low-Value Care (%)					
Any Low Value Care	0.92	9.57	100.0	0	284,928
Avoidable ED Visits	8.43	27.78	100.0	0	284,928
Imaging	23.33	42.29	100.0	0	284,928

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. Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 99.77^{th} percentile. Components of spending do not sum up to "Total" due to Winsorization. In addition, "Inpatient" and "Outpatient" spending due not sum to "Medical" spending because of Medicaid-specific services omitted from this summary statistics table, for example, behavioral health and dental care.

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

	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-month Spending ($)			
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
Statins	0.07	0.00	0.90
Anti-Hypertensives	0.80	0.08	0.17
Anti-Depressants	0.81	−0.04	0.38
Diabetes Medication	0.17	0.04	0.06
Panel E. Any Pre-assignment Use of Potentially Low-Value Care (%)			
Any Low-Value Care	0.79	−0.07	0.29
Avoidable ED Visits	6.28	0.17	0.50
Imaging	24.31	0.91^{***}	0.00
N		94,976	

Notes: Table presents tests for balance of predetermined characteristics among enrollees who were auto-assigned to FFS or managed care plans (MMC). Each row corresponds to a separate regression. The characteristics tested for balance include predetermined recipient demographics and *pre-assignment* utilization and diagnoses. Each recipient is observed for at least one year prior to assignment (or prior to self-sorting into a plan). 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 do not predict assignment to managed care. 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. Additional details on the recipient-level outcomes are described in Section 2.

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

	Auto-Assignee Sample			Full Sample
	$\overline{Y}$	RF	2SLS	OLS
	(1)	(2)	(3)	(4)
Total Spending	1 451.35	−62.24***	−81.51***	−265.87***
		(13.12)	(17.28)	(21.92)
Panel A. Spending by components of care ($)				
Inpatient Spending	97.48	2.50	3.27	0.29
		(3.63)	(4.74)	(2.92)
Outpatient Spending	590.29	−14.19**	−18.58**	−81.86***
		(5.02)	(6.60)	(7.93)
Pharmacy Spending	381.45	−52.29***	−68.48***	−166.25***
		(7.02)	(8.86)	(14.45)
Panel B. Spending by enrollee characteristics ($)				
Female	1 484.00	−65.34**	−84.82**	−237.03***
		(19.83)	(26.36)	(23.34)
Male	1 414.65	−63.24***	−83.67***	−296.74***
		(18.09)	(23.20)	(27.15)
Black	1 280.27	−52.53**	−66.84**	−185.50***
		(16.24)	(20.93)	(23.46)
White	1 811.79	−54.35*	−76.34*	−329.29***
		(26.40)	(36.34)	(30.26)
Panel C. Spending by quartiles of predicted enrollee health spending ($)				
0-25%	682.61	−39.90**	−46.61**	−100.03***
		(13.22)	(15.46)	(15.12)
26-50%	940.68	−32.52*	−41.77*	−106.98***
		(15.97)	(20.51)	(12.94)
51-75%	1 331.36	−83.97***	−114.70***	−115.28***
		(21.03)	(29.71)	(22.47)
76-100%	2 850.94	−126.40**	−185.00**	−262.53***
		(39.19)	(56.40)	(40.43)

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 (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. Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 99.77^{th} percentile. 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); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Impacts on quality & consumer satisfaction

	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 (%)					
Annual Well-Child Visits	49.34	−0.56	−0.73	168,313	−4.01***
		(0.72)	(0.94)		(1.14)
Access to Primary Care	80.46	−1.52**	−2.00**	280,915	−4.60***
		(0.54)	(0.70)		(0.64)
Chlamydia Screening	59.67	0.02	0.03	10,395	−0.52
		(1.33)	(1.72)		(1.02)
Cervical Cancer Screening	67.19	−0.13	−0.16	13,758	−1.74
		(1.52)	(1.97)		(1.18)
Any Follow-up care after ADHD Rx	51.17	1.23	1.77	3,864	−1.38
		(2.22)	(3.22)		(1.70)
Behavioral	7.40	−0.34	−0.44	284,928	−1.54***
		(0.18)	(0.24)		(0.23)
Dental	55.18	−0.13	−0.17	284,928	−3.74***
		(0.49)	(0.64)		(0.63)
Panel B. Any Potentially High-Value Care Drug Classes in a Year (%)					
Statins	0.30	0.06*	0.08*	284,928	−0.05
		(0.03)	(0.03)		(0.03)
Anti-Hypertensives	2.73	0.05	0.07	284,928	−0.45***
		(0.11)	(0.14)		(0.11)
Anti-Depressants	3.55	0.05	0.07	284,928	−0.32**
		(0.14)	(0.18)		(0.12)
Diabetes Medication	0.58	0.14*	0.18*	284,928	−0.01
		(0.06)	(0.07)		(0.04)
Panel C. Any Potentially Low-Value Care in a Year (%)					
Any Low Value Care	0.92	−0.07	−0.09	284,928	−0.12**
		(0.04)	(0.06)		(0.04)
Avoidable E.D.	8.43	0.89***	1.17***	284,928	0.93***
		(0.19)	(0.27)		(0.14)
Imaging	23.33	−0.15	−0.19	284,928	−1.90***
		(0.28)	(0.38)		(0.29)
Panel D. Consumer Satisfaction (Relative to FFS)					
Share of enrollees in their assigned plan (%)	93.02	−14.54***			
		(3.28)			

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 (3) and adds the active-choosers to the sample for column (5). Only post-assignment observations are included (February 2012 to December 2014). See Table 3 notes for additional detail. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Mechanisms: Prices, primary care management, and networks explain little of managed care's savings

	Original Spending		Repriced Spending			
	$\overline{Y}$	2SLS	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Spending by components of care ($)						
Total Spending	1 451.37	-81.51^{***}	-56.78^{***}	-82.92^{***}	-72.58^{***}	-90.81^{***}
		(17.28)	(16.90)	(17.65)	(14.85)	(16.28)
Inpatient Spending	98.61	3.12	1.20	1.58	−3.96	−3.30
		(4.85)	(5.08)	(5.71)	(4.51)	(5.11)
Outpatient Spending	590.17	-18.58^{**}	−2.15	−6.29	−4.92	−7.78
		(6.60)	(6.92)	(6.43)	(6.55)	(6.66)
Pharmacy Spending	380.19	-68.66^{***}	-61.45^{***}	-71.24^{***}	-56.13^{***}	-66.45^{***}
		(8.79)	(8.23)	(9.46)	(8.56)	(9.97)
Panel B. Pharmacy spending by type of drug ($)						
Brand Drug Spending	228.07	-65.84^{***}	-67.24^{***}	-72.90^{***}	-64.58^{***}	-71.08^{***}
		(7.25)	(7.06)	(8.26)	(7.72)	(8.97)
Generic Brand Spending	149.32	−3.54	5.08	1.04	7.85^{**}	4.10
		(3.58)	(3.18)	(3.32)	(2.80)	(3.03)
Repriced Claims		No	Yes	Yes	Yes	Yes
Plan Network Breadth		No	No	Yes	No	Yes
Provider Fixed Effects		No	No	No	Yes	Yes

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 2–6 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 sample restriction here to observations with enough information to construct all variables used in columns 3–6. Columns 3–6 reprice all claims according to a common price list, as described in the text. Columns 4–6 variously include controls for plan network breadth and fixed effects for primary care providers, as described in the text. The sample consists of auto-assignees. Only post-assignment observations are included (February 2012 to December 2014). Observations are at the enrollee-year level: $N = 284,716$. Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 99.77^{th} percentile. 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); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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

	N		Spending		WTS	
	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 pharmacy to managed care						
Pre-carve-in period (first 9 months post-assignment, annualized)	354,231	500,553	1 389.99	−31.03*	93.91	−11.30***
				(14.21)		(2.99)
Full study period (3 years post-assignment)	118,077	166,851	1 451.35	−62.24***	93.02	−14.54***
				(13.12)		(3.28)
Panel B. Effects by quartile of enrollee exposure to managed care denial regime based on enrollee-level drug utilization pre-carve-in						
0-25%	21,018	29,529	973.77	−29.52	92.85	−12.57***
				(18.09)		(3.09)
26-50%	21,594	28,950	1 221.87	−12.74	92.64	−15.26***
				(20.31)		(4.19)
51-75%	21,012	29,529	1 668.09	−49.11	92.43	−19.31***
				(30.39)		(4.01)
76-100%	20,628	29,913	3 321.01	−191.74***	90.86	−20.47***
				(49.50)		(4.19)

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. Column 1 and 2 lists the number of observations; Row 1 of Panel A uses monthly data whereas all other rows use the same yearly data as other table. 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). Once this dollar measure cerated, we break the sample into 4 quartlies. From this, we see that most of the spending effect is concentrated in the highest quartile, whereas the WTS effects is monotonically increasing. The sample consists of auto-assignees. Only post-assignment observations are included (February 2012 to December 2014). Observations are at the enrollee-year level: $N = 284,716$. Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 99.77^{th} percentile. 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); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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 "managed FFS" plans. The MMC plans received a prospective, monthly risk-adjusted capitation payment (averaging over $250 per member per month) to cover a wide range of contracted services for their Medicaid enrollees.

The managed FFS plans receive a small monthly fee (approximately $10 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 managed FFS plan but provided via the state's legacy FFS network and paid directly by the state. The ePCCM 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 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 an MMC 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, beneficiaries 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 blocking, 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.

- **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). Four separate percentages are reported:
  - 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.
- **Behavioral and Dental healthcare utilization.** We evaluated whether enrollees had any utilization in a year of behavioral health or dental services. We relied on a state-specific typology to identify claims associated with these services and created indicator variables for enrollees set to one if they had at least one claim for a particular service in a given year, and zero otherwise.

- **Avoidable emergency department use.** This measure that captures emergency department (ED) utilization for low-acuity services that could be treated in another ambulatory setting (Medi-Cal Managed Care Division, 2012). A high rate of avoidable ED utilization is generally considered a marker of poor access to ambulatory care.

- **Low Value Care.** We create a monthly catch-all 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. Additionally, we create monthly measures of lab tests and imaging visits.

- **Imaging.** To identify health care claims that involve imaging we used Berenson-Eggers Type of Service (BETOS) codes.

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 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 Identification and Robustness

C.1 Event Study Specifications

This section describes the regression specification for our event studies (e.g., Figures 2 and 3). Let i index enrollees. Let t indicate event-time, defined as months/quarters/bi-annual/years relative to auto assignment. For a given outcome, Y_{ijt}, our event study regression specification takes the form:

$$Y_{ijt} = \alpha_i + \alpha_t + [\sum_{t \neq -1} \beta_t \times AssignedManagedCare_i] + \mu_{ijt} \tag{5}$$

where α_i are enrollee fixed effects, α_t are event-time fixed effects, $AssignedManagedCare_i$ is an indicator variable set to one if an enrollee was assigned to full-risk, managed care and zero otherwise, and β_t are coefficients on assigned model that vary by event-time. We omit the month prior to assignment $\beta_{t=-1}$, so that the point estimates for the other event-times can be interpreted relative to the pre-assignment baseline period. Standard Errors are clustered at the prior provider.

C.2 Decomposing the Spending Reduction

Both sources of identifying variation (across Sections 4 and 5) showed that spending reductions were largely associated with prescription drug coverage rather than medical services. 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.

C.2.1 Framework

Our decomposition approach follows Brot-Goldberg et al. (2017). We begin by restricting attention 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:

$$\Delta TS_{MMC,FFS} \equiv \Delta P_{MMC,FFS} + \Delta Q_{MMC,FFS}. \tag{6}$$

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., price-normalized 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 × year have the same price. We assign these prices using estimated service code fixed effects ν_{dt} from a regression run at the claim level in which price is the dependent variable:

$$P_{dct} = \alpha + \nu_{dt} + \pi AssignedManagedCare_{ct} + \mu_{dct} \tag{7}$$

[1] Some 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.

[2] For 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.

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

P_{dct} indicates the price per unit paid[4] 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 claim[5]. $AssignedManagedCare_{ct}$ 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 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 $(\alpha + \nu_{dt}) \times units_c$ — 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:

$$P_{dct} = \alpha + \nu_{dt} + \pi TransitionedPlan_{ct} + \mu_{dct} \tag{8}$$

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-standardized[6] 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 6 and substituting gives:

$$\Delta P_{MMC,FFS} = \Delta TS_{MMC,FFS} - \Delta Q_{MMC,FFS} = \hat{\beta}^{TS} - \hat{\beta}^{\overline{P}}. \tag{9}$$

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 $\Delta Q^{Steering}_{MMC,FFS}$, 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 $\Delta Q^{Generic}_{MMC,FFS}$, which captures substitutions between brand and generic drugs within a therapeutic class; and (iii) a residual $Q^{R}_{MMC,FFS}$. 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:

[4]For 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.

[5]As 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.

[6]As in Table 3, overall enrollee-year spending is Winsorized at \$25,000 whereas other spending measures are winsorized at the 99.77th percentile.

$$\Delta TS_{MMC,FFS} = \underbrace{\Delta P_{MMC,FFS}}_{\substack{\text{Price diffs. in}\\ \text{identical products}}} + \underbrace{\Delta Q^{Steering}_{MMC,FFS}}_{\substack{\text{Steering within}\\ \text{brand/generic groups}}} + \underbrace{\Delta Q^{Generic}_{MMC,FFS}}_{\substack{\text{Substitution from}\\ \text{brands to generics}}} + \underbrace{\Delta Q^{R}_{MMC,FFS}}_{\substack{\text{Residual quantity}\\ \text{differences}}} . \tag{10}$$

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 (Ganapati and McKibbin, 2019; Dubois, Gandhi and Vasserman, 2019).[7] We then reprice all pharmacy services at the therapeutic class $\times$ 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 reaggregate the repriced claims to construct an alternative measure of repriced enrollee-year 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 zeros-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 $\times$ 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 ($\hat{\beta}^{\overline{Steering}}$):

$$\Delta Q^{Steering}_{MMC,FFS} \equiv \hat{\beta}^{\overline{P}} - \hat{\beta}^{\overline{Steering}}. \tag{11}$$

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 zeros-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:

$$\Delta Q^{Generic}_{MMC,FFS} \equiv \hat{\beta}^{\overline{Steering}} - \hat{\beta}^{\overline{Generic}}. \tag{12}$$

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.

[7]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.

C.2.2 Decomposition Results

Table A10 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, running the Equation 2 over subsamples 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 not have lower health care spending prior to assignment. To facilitate interpretation of magnitudes, results in Table A10 are scaled as percentage changes by dividing the estimates from Equations 9 through 12 by the mean FFS spending in the indicated category (total, medical, or pharmacy).

The first column presents differences in total health care 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 health care 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 health care spending (medical and pharmacy together in *Panel A*) is fairly small for each year of the post-assignment period, ranging from -0.7% in t_2 to -2.6% in t_1. The effect of drug steering within sets of generic or brand substitutes ($\Delta Q^{Steering}_{MMC,FFS}$) is also modest, at most -2.4% of pharmacy spending in t_2. By comparison, the contribution of steering away from brands towards generics in the fourth column ($\Delta Q^{Generic}_{MMC,FFS}$), 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 -8.6% in t_1 to -11.1% in t_2 of pharmacy spending, about half of the overall pharmacy effect (-25.3%).[9] The final column ($\Delta Q^{R}_{MMC,FFS}$) 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 A8 summarizes the decomposition in Table A10 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 A8 demonstrates that, within pharmacy, spending reductions by therapeutic class were strikingly similar across the two different identification strategies.

C.3 Reweighting samples

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 reweight our samples on three

[8]Results 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 pre-assignment 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.

[9]These 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).

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 reweighting the auto-assignee population to balance its characteristics with those of the active chooser population. Table A4 presents our reweighted results.
- **Plan Transition experiment.** In the plan transition (PT) natural experiment, we lead with estimates that reweight the PT sample to balance its characteristics with those of the AA sample (unless stated otherwise).

Because of strong joint support between the different samples, and the coarseness of our reweighting cells, only 4 (0.004%) of the auto-assignee enrollees cannot be assigned a weight when reweighting 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 reweighting to match the characteristics of the auto-assignee sample.

D Mechanisms

D.1 Assessing the role of steering to more efficient providers

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

$$Y_{it} = \alpha + \beta \widehat{ManagedCare}_{it} + \phi_i^p + \delta X_{it} + \eta_{it}, \tag{13}$$

where $\widehat{ManagedCare}_{it}$ is predicted from Equation 1 in Section 3, and β recovers the causal effect of managed care enrollment relative to FFS on the outcomes of interest. Because the auto-assignment algorithm was designed to assign enrollees to a plan that contracted with their prior provider (superscripted p), we include fixed effects for each enrollee's provider prior to assignment (ϕ_i^p) 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.

One hypothesized mechanism for how managed care reduces spending is by steering enrollees to more efficient providers. Though our primary model includes fixed effects for enrollees' prior providers, it is possible that the enrollees assigned to managed care plans are steered (e.g., via provider networks, 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 (i.e., Equation 2) with an additional set of fixed effects for each enrollee's post-assignment provider.

For purposes of this analyses, 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). This leaves us with 2,284 providers. 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* $\times$ *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.[10] We are able to attribute 97.1% of auto-assigned enrollees to one or multiple providers

[10] Our findings are not sensitive to the imputation method used. Results available upon request.

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.

Once we attribute enrollees to providers, we then estimate models of the form:

$$Y_{it} = \alpha + \beta \widehat{ManagedCare}_{it} + \phi_i^p + \rho_i^P + \delta X_i + \eta_{it}, \tag{14}$$

where we have added an additional set of fixed effects based on enrollees' attributed providers (superscripted P), designated by the term (ρ_i^P) to assess whether the differences between managed care and FFS in our outcomes (e.g., spending) persist within providers.

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

$$HasProvider_i = \alpha + \pi AssignedManagedCare_i + \phi_i^p + \mu_i \tag{15}$$

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. We find that $\hat{\pi}$ is equal to -0.0019 with a standard error of 0.0019. Hence, the estimate is not statistically significant, and small relative to the mean of 0.97 (i.e. 97.1% of enrollees were attributed a provider). This suggests that the likelihood of an enrollee being attributed to a provider is not related to whether they were assigned to managed care or FFS.

D.2 Assessing the role of primary care provider network breadth

To measure the breadth of a plan's primary care provider network in each zip code, one must 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 managed care or managed FFS 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. Once constructed, we assess the sensitivity of our primary IV estimates to the inclusion of enrollee's assigned provider network breadth (based on their plan of assignment) and present the results of that analysis in Table 5.

D.3 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, and has motivated us to dig further in how these denials are potentially decreasing pharmacy spending.

In order to do so, we set up 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 "wash-out" 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 and the claims' dates. The paid claim is within 7 days of the denied claim. Because of this, paid claims can be found in the month following the end of the study period.

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

1. Administrative denials: these are denied claims that result in a paid claim within 7 days that have the same NDC. Panel B in Figure A11 further conditions by imposing units to differ between the denied and paid claim.

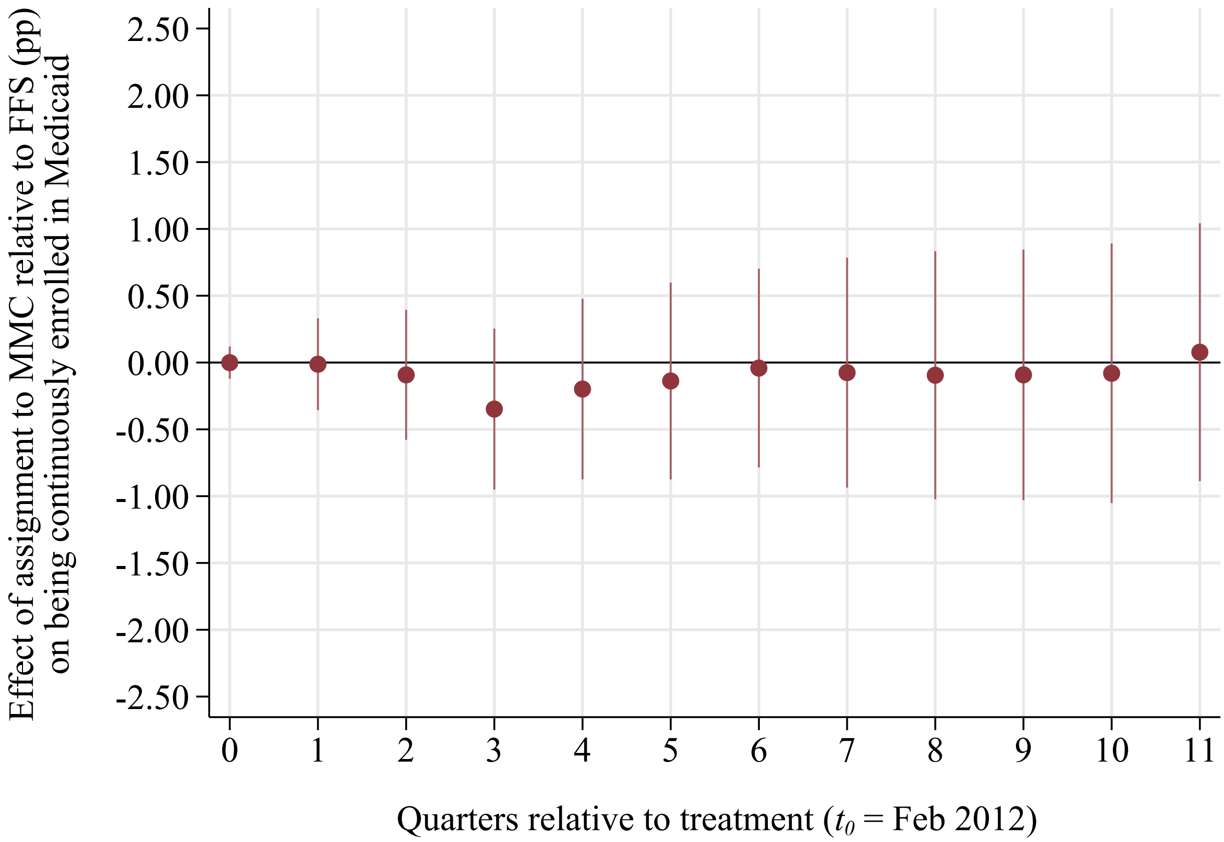
2. Substitution denials: these are denied claims that result in a paid claim within 7 days that have a different NDC. No restriction is applied for the units. Panel C in Figure A11 uses these.

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. Combined with the real-time adjudication of pharmacy denials, we can safely conclude that, despite the matching strategy being approximate, the direction of the results are correct.

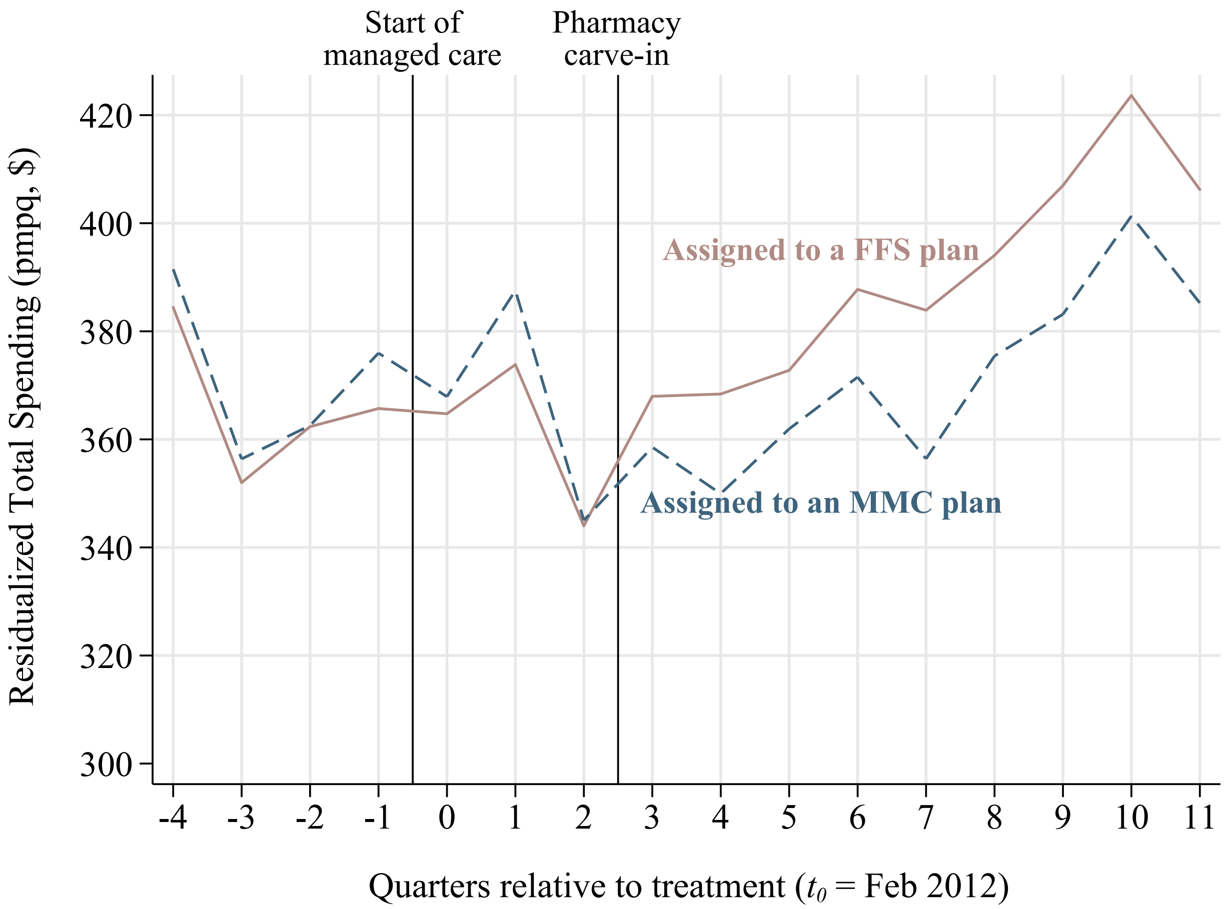
E Additional Figures and Tables

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 any managed care plan or in managed fee-for-service—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. Observations are enrollees. 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.

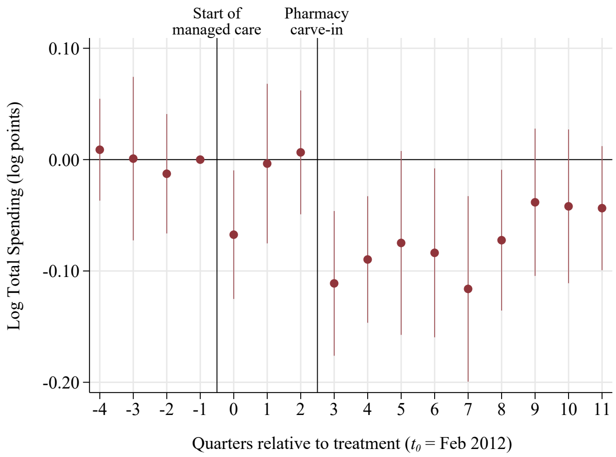
Appendix Figure A2: Time Series Plot of Raw Spending Levels for Enrollees Assigned to MMC and Managed FFS



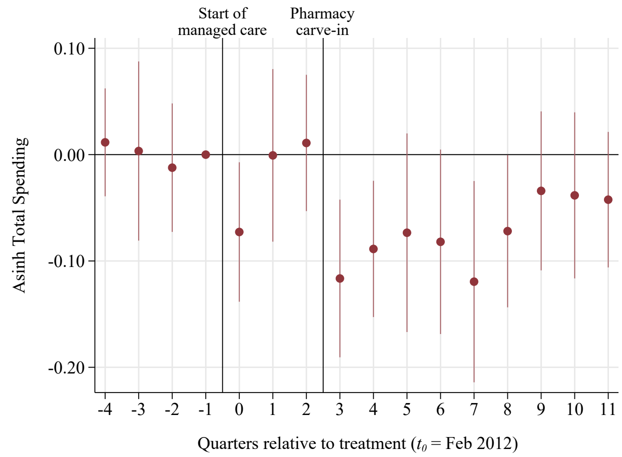
Note: This figure presents quarterly enrollee spending, adjusted for prior provider and calendar quarters, for a 4-year period spanning 11 months prior to, and three years after, assignment to managed care for a balanced panel of 85,668 enrollees. Observations are at the assigned model × months level. 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. For additional details on the construction of enrollee-level spending refer to Section 2.

Appendix Figure A3: Impact of assignment to managed care vs. FFS on healthcare spending by quarter relative to plan assignment (arcsinh and log dependent variable)

Panel A. Log spending



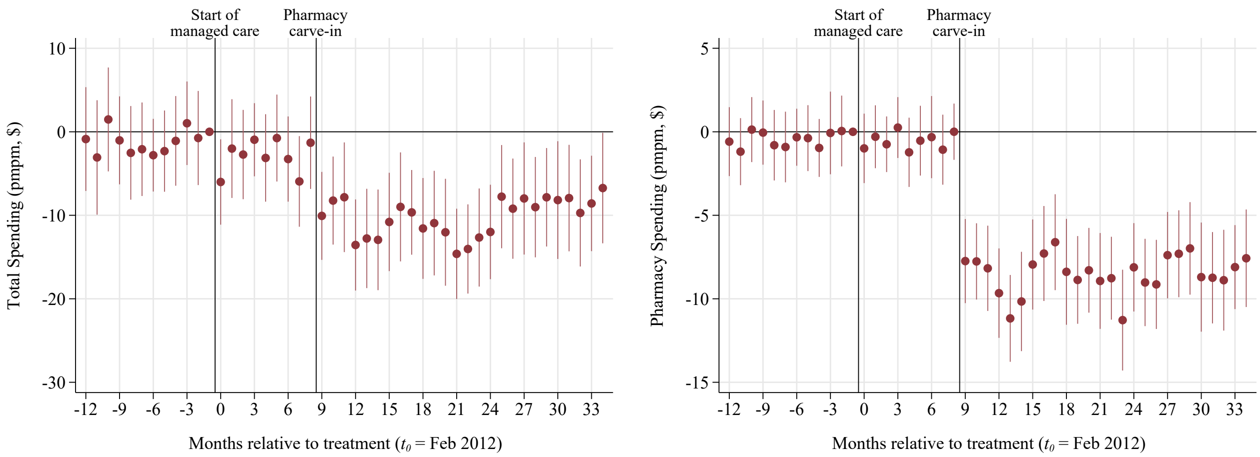
Panel A. Asinh spending



Note: Figure presents difference-in-differences event studies comparing respectively log and inverse hyperbolic sine of health spending across assignees to MMC and FFS (as in figure 2). 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. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider); 95% confidence intervals reported. See main text and Appendix Section C.1 for additional detail on variable construction and specification.

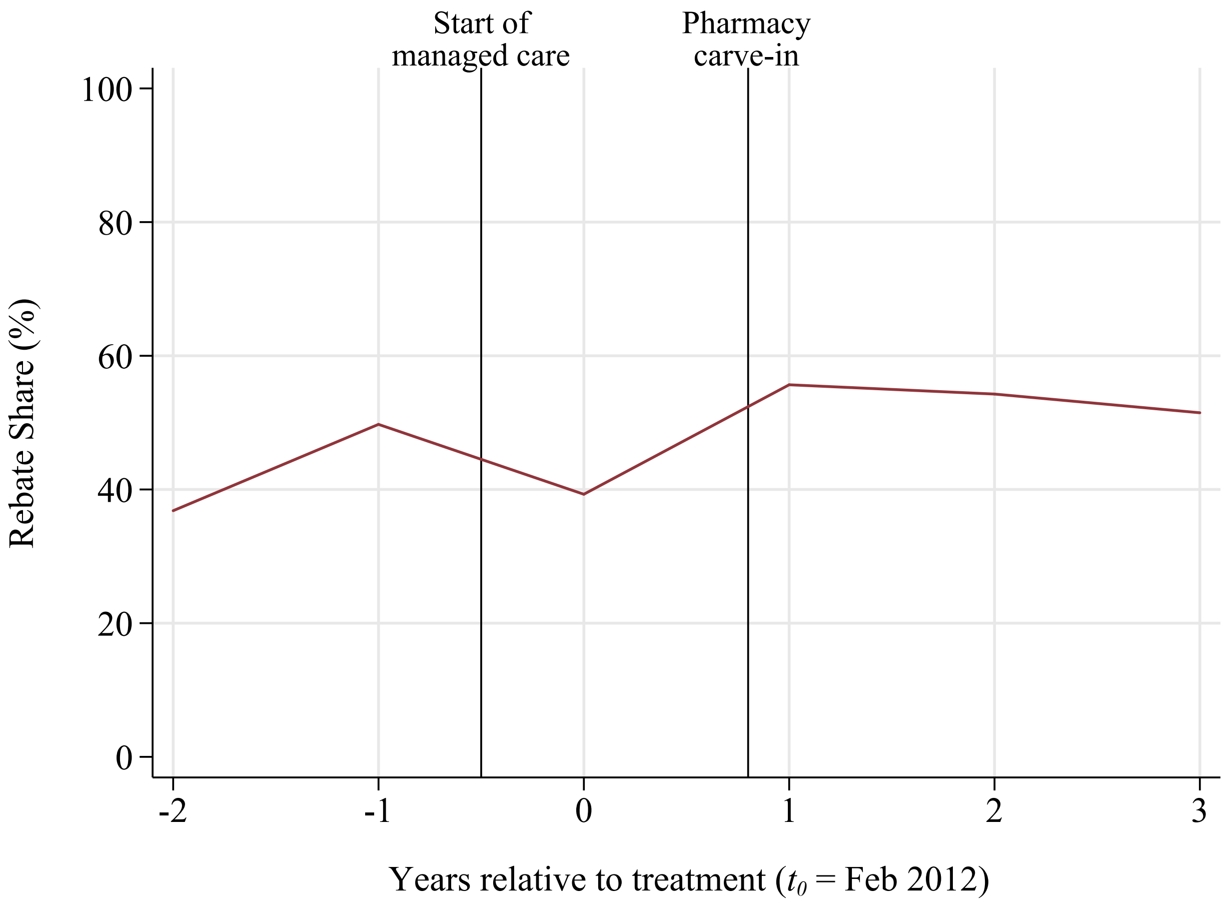
Appendix Figure A4: Impact of assignment to managed care vs. FFS on Winsorized levels of healthcare spending by time relative to plan assignment

Panel A. Overall spending (monthly) *Panel B.* Pharmacy spending (monthly)



Note: Figure presents difference-in-differences event studies comparing health and pharmacy spending spending across assignees to MMC and FFS (as in figure 2).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 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 the (null) effects of assignment to managed care prior to the treatment period and a large, and precisely-estimated drop in monthly healthcare spending after assignment to MMC, and another precisely-estimated drop in monthly pharmacy spending after the pharmacy carve-in in MMC. Standard errors clustered on the unit of randomization (i.e., recipient's prior provider); 95% confidence intervals reported. See main text and Appendix Section C.1 for additional detail on variable construction and specification.

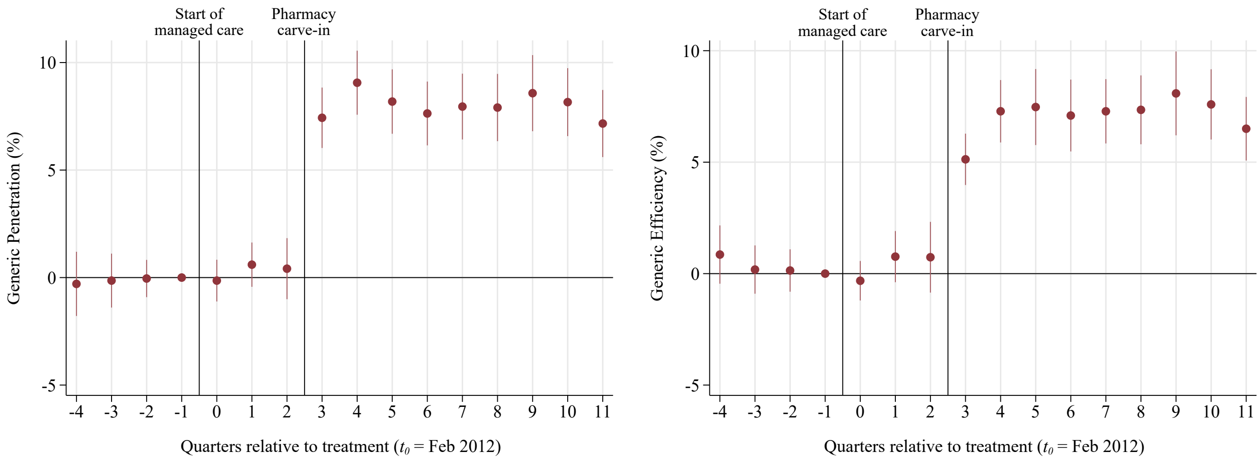
Appendix Figure A5: Rebates Share



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. 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 at the annual level. 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 A6: Impact of assignment to managed care vs. FFS on generic usage

Panel A. Generic Penetration *Panel B.* Generic Efficiency

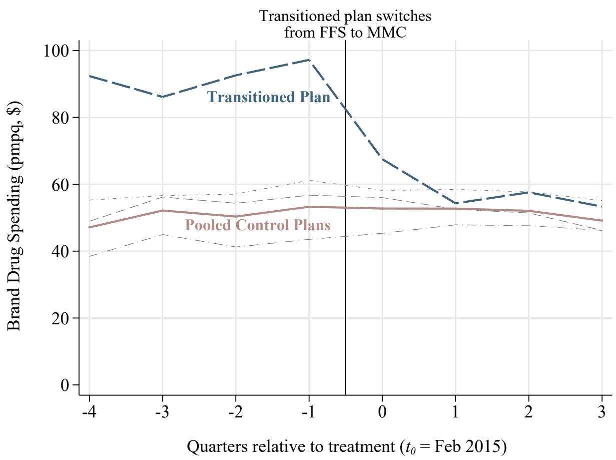


Note: Figure presents difference-in-differences event studies comparing the usage of generic drugs (Panel A) and the efficiency of this usage (Panel B) across assignees to MMC and FFS. Estimates are based on a balanced panel of 85,668 continuously-enrolled recipients for the 47 month (February 2011–December 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 by 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 random assigned to Medicaid managed care 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. See main text and Appendix Section C.1 for additional detail on variable construction and specification.

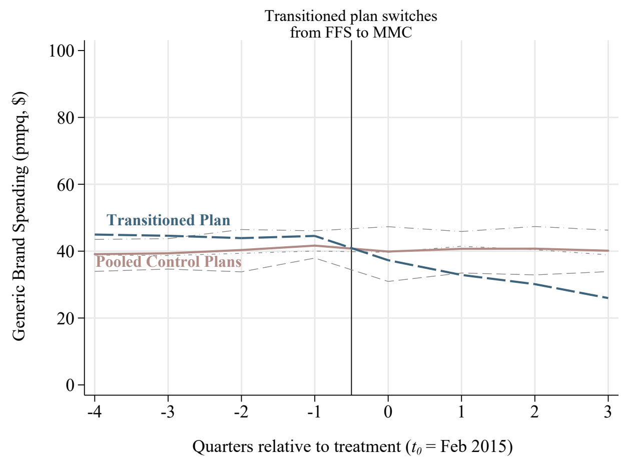
Appendix Figure A7: Plan Transition timeseries

Panel A. Spending reductions

Brand Drug Spending

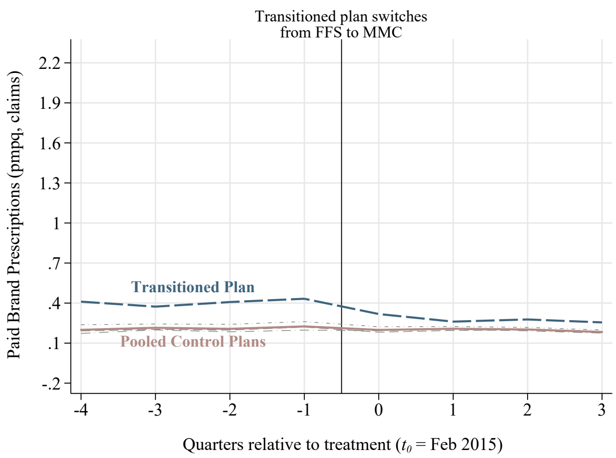


Generic Drug Spending

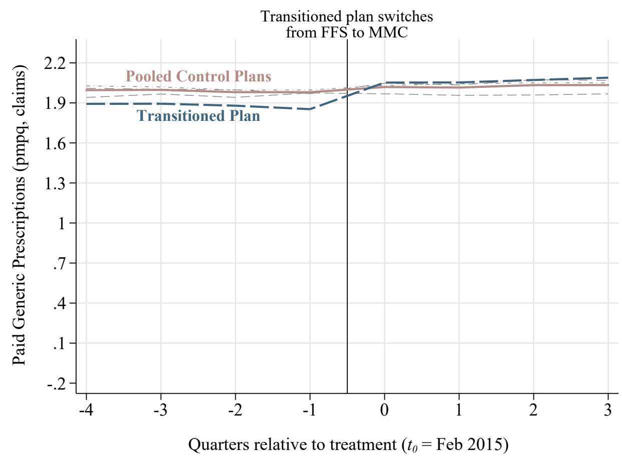


Panel B. Quantity reductions

Brand Drug Paid Prescriptions

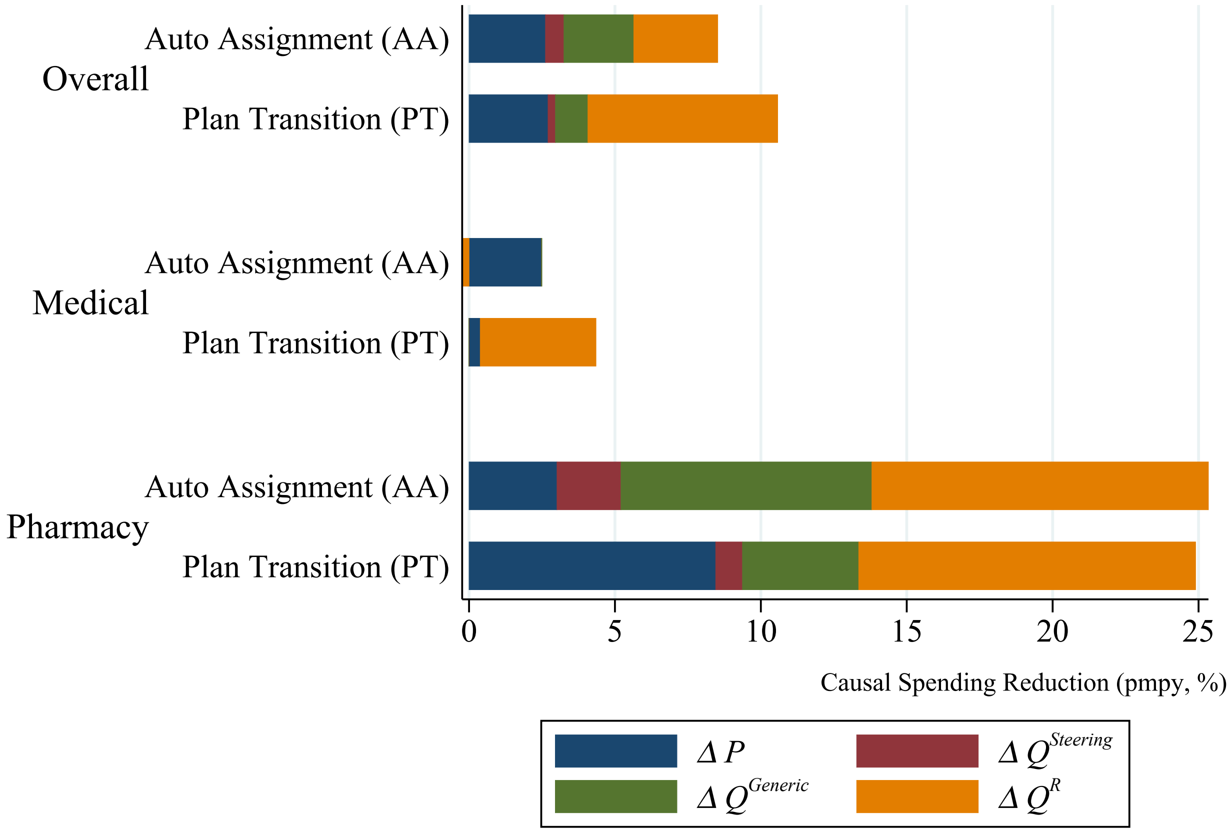


Generic Drug Paid Prescriptions



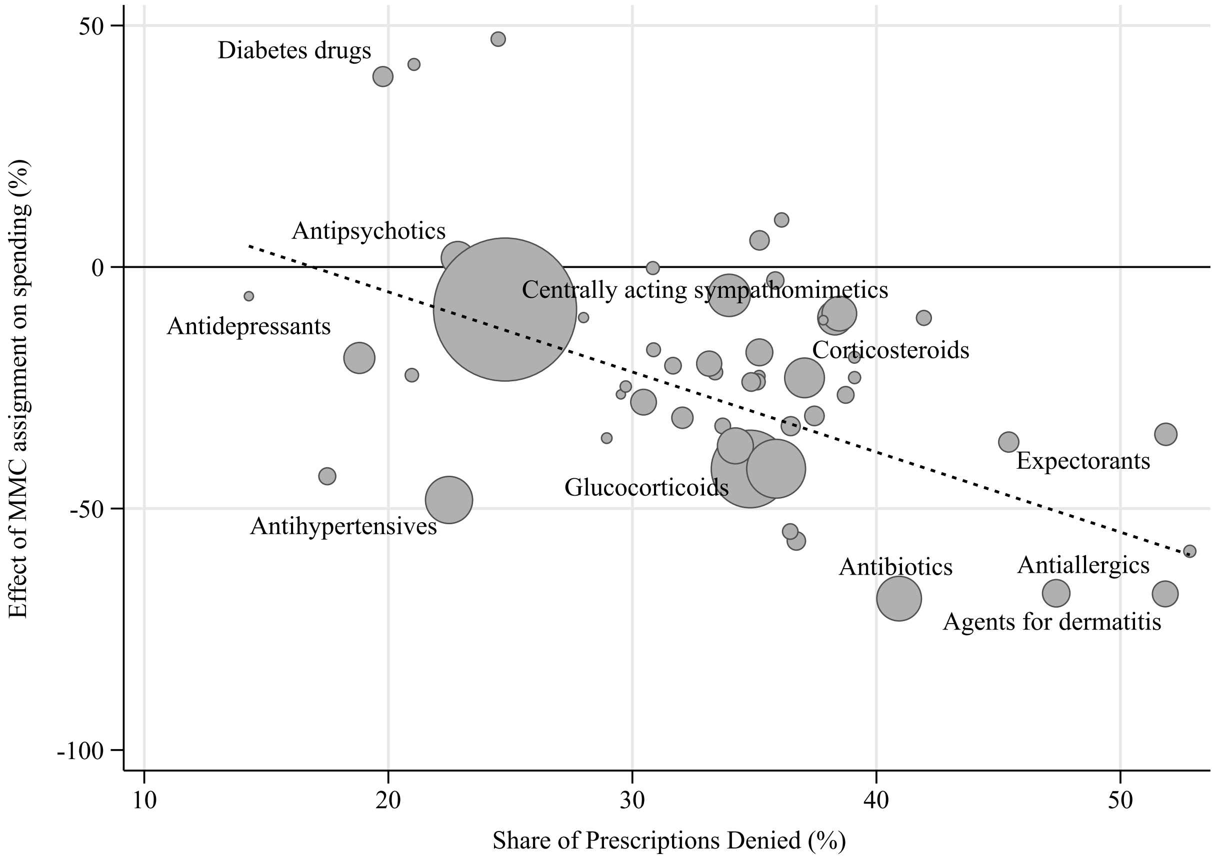
Note: Figure plots means of spending over time by plan. The vertical line indicates when the treatment plan transitioned from managed FFS to become a full-risk managed care plan. The plans that were already full-risk managed care plans did not experience a change at that time. This event date (February 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.3 for additional details.)

Appendix Figure A8: Decomposition of spending by type of spending and Sample in t_1



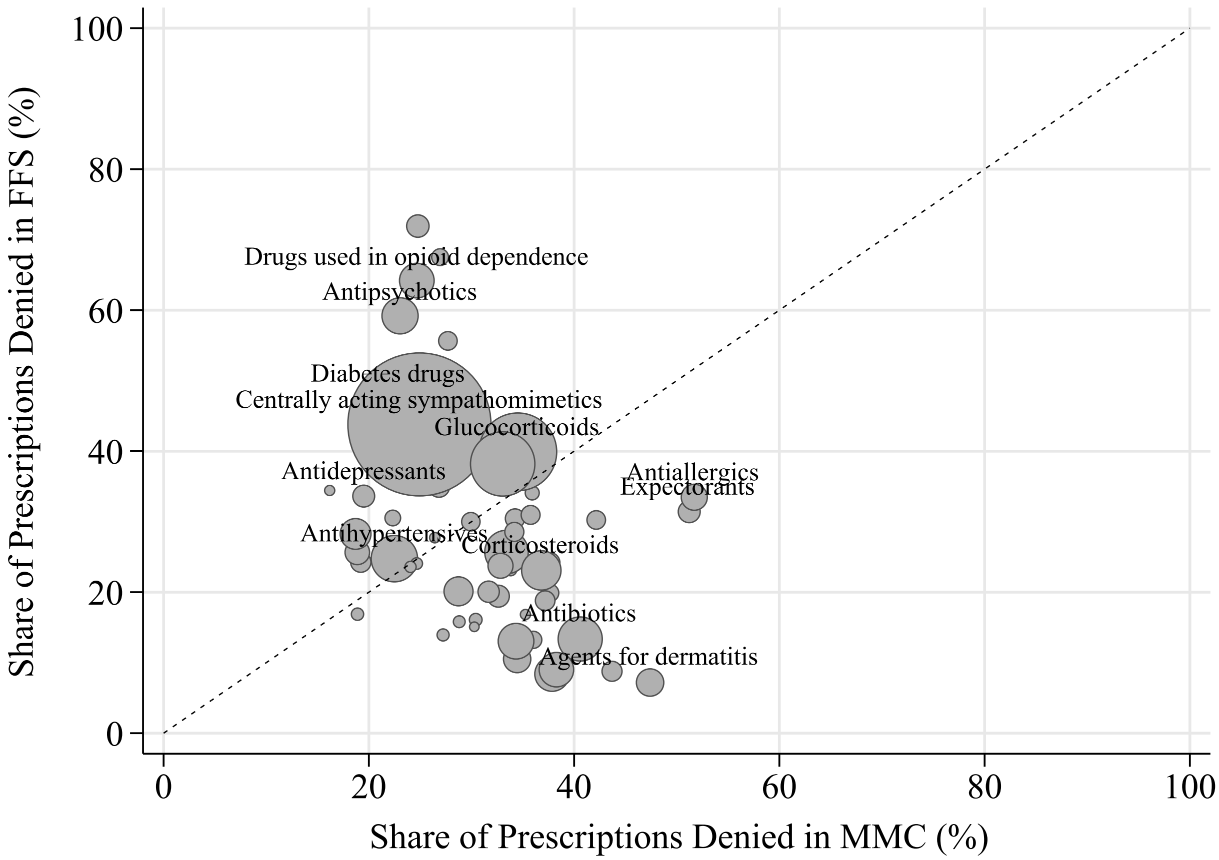
Note: This figure presents the decomposition results at the unit level for $t_1 = 2013$ for the Auto-Assignment (AA) experiment and $t_1 = 2015$ the Plan Transition (PT) experiment. Additional details are available in Appendix C.2. Observations are reweighted such that the Plan Transition sample matches the distribution of the auto-assignee sample used in the first identification strategy on health status-by-gender-by-age bins. (See Appendix C.3 for additional details.)

Appendix Figure A9: 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 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 for 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 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.

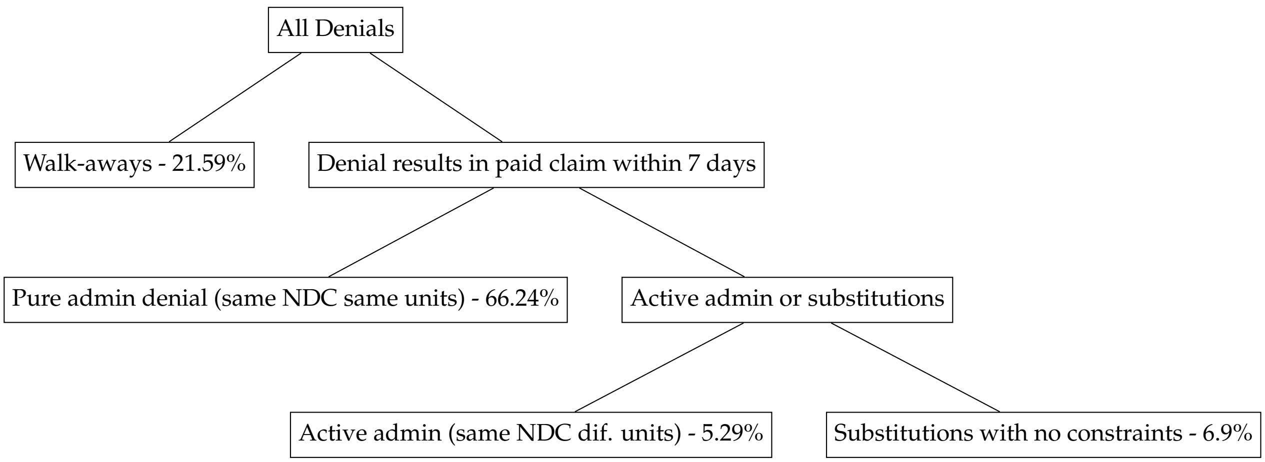
Appendix Figure A10: The share of prescription drug claims denied by therapeutic drug class are negatively correlated between MMC and FFS



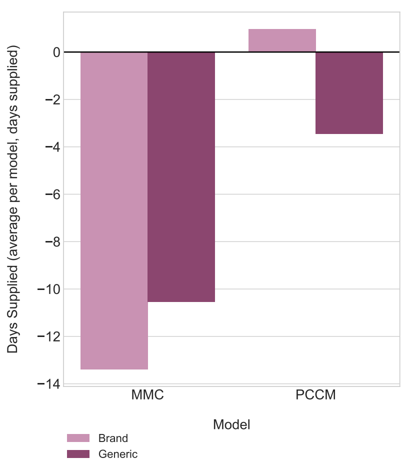
Note: Figure presents evidence that pharmacy denials in the state fee-for-service (FFS) program are negatively correlated with the strategic denial regime of the full-risk Medicaid managed care (MMC) plans. The figure compares the pharmacy prescription denial rates in MMC — based on enrollees randomly 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 Managed FFS plans. The prescription drug claims denial rates for each ATC-4 therapeutic class (i.e., Anatomical Therapeutic Chemical (ATC) Classification, level 4) are measured by restricting to the first quarter after the pharmacy carve-in (with a one month wash-out period). Additional details are available in Section 6 and Figure 6.

Appendix Figure A11: Denial Analysis: less units and substitutions

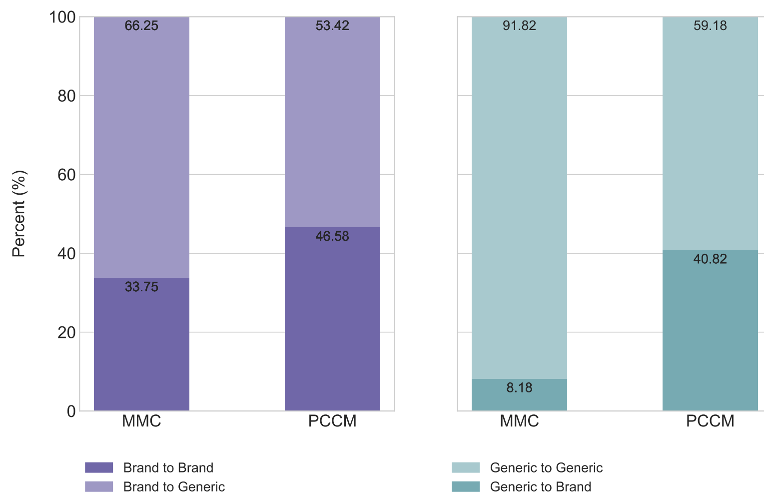
Panel A. Decomposing all of the denials - Conditions and shares



Panel B. MMC reduces days supplied



Panel C. MMC substitutes brands for generics and restricts generics to brand substitutions

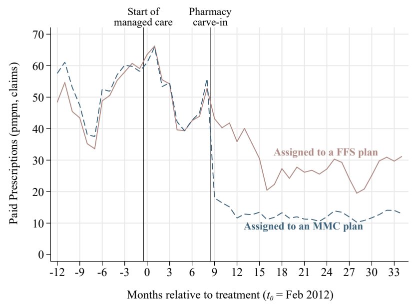


Note: These results use a matching strategy where a denial is matched to a subsequently paid claim within 7 days, if this paid claim exists. The period studied is identical to that of the dose-response figure from section 6, the three month period following the carve-in of pharmacy benefits (we "wash-out" November 2012 as it is the transition month). See Appendix D.3 for additional details. Panel A conditions on a denial resulting in a subsequent paid with same NDC claim within 7 days. Panel B 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 PCCM, and between generic and brand drugs.

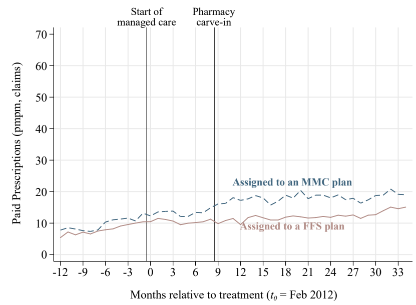
Appendix Figure A12: Denials at the NDC-level

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

NDCs in top decile of denials

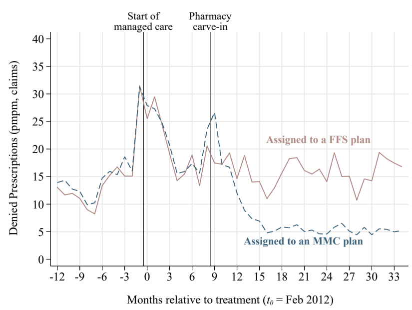


NDCs in bottom decile of denials

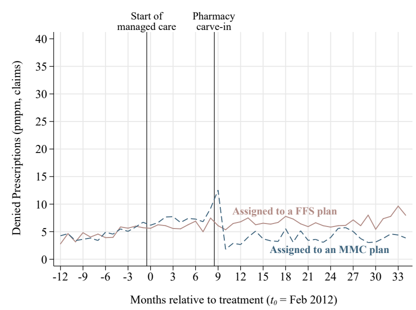


Panel C. Denied pharmacy claims first rise and then fall

NDCs in top decile of denials



NDCs in bottom decile of denials



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. Denial rates are measured at the peak of denials (3 months post carve-in). Observations are at the assigned model $\times$ months 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.

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

	Overall		Auto-Assignees		Active Choosers	
	$\overline{Y}$ (1)	Std Dev (2)	$\overline{Y}$ (3)	Std Dev (4)	$\overline{Y}$ (5)	Std Dev (6)
Panel A. Enrollee Characteristics						
Female (%)	52.79	49.92	52.92	49.92	52.51	49.94
Age at baseline	9.07	7.39	9.36	7.49	8.44	7.13
Panel B. enrollee-year spending ($)						
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	147.52	5 503.95	137.49	2 378.47	169.69	9 206.42
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 Brand	168.14	370.10	149.63	345.53	209.05	416.45
Panel C. Any Annual Utilization of High- or Potentially High-Value Care (%)						
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
Any Follow-up care after ADHD Rx	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
Statins	0.32	5.65	0.30	5.51	0.35	5.94
Anti-Hypertensives	2.88	16.72	2.73	16.31	3.20	17.60
Anti-Depressants	3.62	18.68	3.55	18.52	3.76	19.03
Diabetes Medication	0.61	7.80	0.58	7.56	0.69	8.29
Panel D. Any Annual Utilization of Low- or Potentially Low-Value Care (%)						
Any Low Value Care	0.95	9.72	0.92	9.57	1.02	10.05
Avoidable E.D.	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

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-year level: $N = 413,811$ enrollee-years. Additional details on the utilization and spending measures is available in Section 2. Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 99.77^{th} percentile. Components of spending do not sum up to "Total" due to Winsorization.

Appendix Table A2: Imbalance among Active Choosers

	Mean (1)	Coef. on Managed Care Enrollment (2)	p-value (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-year Spending ($)			
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 Potentially 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
Statins	0.12	0.02	0.67
Anti-Hypertensives	1.08	−0.14	0.15
Anti-Depressants	0.94	0.05	0.58
Diabetes Medication	0.27	0.05	0.18
Panel E. Any Annual Utilization of Low- and Potentially Low-Value Care (%)			
Any LVC	0.90	0.08	0.31
Avoidable ED	5.63	0.38	0.15
Imaging	26.89	0.00	1.00
N		42,961	

Notes: Table presents the results of a test for balance of predetermined characteristics among enrollees who made an active plan choice. The characteristics tested for balance include recipient demographics and *pre-assignment* utilization and diagnoses. Each recipient is observed for at least one year prior to assignment (or prior to self-sorting into a plan). 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 Medicaid managed care or managed FFS in February 2012 and remained in Medicaid until, at least, December 2015. Additional details on the recipient-level outcomes are available in Section 2.

Appendix Table A3: Robustness: IV estimates of the effect of managed care using Arcsinh-transformed spending outcomes

	Auto-Assignee Sample			Full Sample
	$\overline{Y}$	RF	2SLS	OLS
	(1)	(2)	(3)	(4)
Total Spending	6.75	−0.08**	−0.11**	−0.33***
		(0.03)	(0.03)	(0.04)
Panel A. Spending by components of care (%)				
Inpatient Spending	0.21	0.01	0.01	0.01
		(0.01)	(0.01)	(0.01)
Outpatient Spending	5.70	−0.07*	−0.09*	−0.32***
		(0.03)	(0.04)	(0.04)
Pharmacy Spending	3.99	−0.22***	−0.28***	−0.64***
		(0.03)	(0.04)	(0.04)
Panel B. Spending by enrollee characteristics (%)				
Female	6.79	−0.09**	−0.11**	−0.30***
		(0.03)	(0.04)	(0.04)
Male	6.70	−0.08*	−0.10*	−0.36***
		(0.03)	(0.05)	(0.05)
Black	6.61	−0.08**	−0.10**	−0.25***
		(0.03)	(0.04)	(0.04)
White	7.02	−0.04	−0.06	−0.36***
		(0.05)	(0.06)	(0.06)
Panel C. Spending by enrollee health status (%)				
0-25%	5.39	−0.12*	−0.14*	−0.30***
		(0.05)	(0.05)	(0.05)
26-50%	6.55	−0.08**	−0.11**	−0.16***
		(0.03)	(0.04)	(0.03)
51-75%	7.10	−0.09***	−0.13***	−0.14***
		(0.02)	(0.03)	(0.02)
76-100%	7.95	−0.07***	−0.11***	−0.16***
		(0.01)	(0.02)	(0.02)

Notes: Table presents sample means, and OLS and IV regression coefficients corresponding to Equation 2 using inverse hyperbolic sine transformation for 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. Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 99.77th percentile. 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); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix Table A4: 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

	Auto-Assignee Sample			Full Sample
	$\overline{Y}$ (1)	RF (2)	2SLS (3)	OLS (4)
Total Spending	1 558.19	−71.38***	−94.18***	−265.87***
		(14.66)	(19.68)	(21.92)
Panel A. Spending by components of care ($)				
Inpatient Spending	148.71	−10.77	−14.21	−25.30
		(18.41)	(24.30)	(21.74)
Outpatient Spending	626.14	−16.07**	−21.20**	−81.86***
		(5.21)	(6.97)	(7.93)
Pharmacy Spending	419.34	−59.74***	−78.81***	−166.25***
		(7.62)	(9.74)	(14.45)
Panel B. Spending by enrollee characteristics ($)				
Female	1 484.00	−78.68***	−102.76***	−237.03***
		(20.20)	(27.27)	(23.34)
Male	1 414.65	−67.93**	−90.66**	−296.74***
		(20.69)	(27.16)	(27.15)
Black	1 280.27	−64.40**	−82.35**	−185.50***
		(19.48)	(25.45)	(23.46)
White	1 811.79	−56.94*	−80.92*	−329.29***
		(28.48)	(39.89)	(30.26)
Panel C. Spending by enrollee health status ($)				
0-25%	682.61	−40.92**	−47.88**	−100.03***
		(14.92)	(17.53)	(15.12)
26-50%	940.68	−37.63	−48.43	−106.98***
		(19.13)	(24.56)	(12.94)
51-75%	1 331.36	−102.09***	−139.79***	−115.28***
		(24.29)	(34.55)	(22.47)
76-100%	2 850.94	−129.64**	−191.34**	−262.53***
		(45.85)	(66.64)	(40.43)

Notes: Table presents results of estimating 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 regressing restricts to. Columns (1) through (3) contain the sample mean and regression results for the auto-assignee sample 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: $N = 284,928$ 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. 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.3 for additional details.) Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 99.77^{th} percentile. 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); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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

	Auto-Assignee Sample			Full Sample
	$\overline{Y}$ (1)	RF (2)	2SLS (3)	OLS (4)
Total Spending	1 335.14	−73.28***	−96.10***	−265.52***
		(11.19)	(14.68)	(21.82)
Panel A. Spending by components of care ($)				
Inpatient Spending	100.30	2.42	3.17	−19.84
		(8.87)	(11.64)	(21.14)
Outpatient Spending	548.46	−18.91***	−24.80***	−84.35***
		(4.69)	(6.17)	(8.30)
Pharmacy Spending	345.18	−54.35***	−71.27***	−161.58***
		(6.72)	(8.43)	(14.74)
Panel B. Spending by enrollee characteristics ($)				
Female	1 264.30	−80.37***	−104.42***	−231.77***
		(14.90)	(20.56)	(21.66)
Male	1 406.45	−66.79***	−88.43***	−296.98***
		(17.45)	(22.11)	(26.80)
Black	1 169.87	−62.15***	−79.19***	−189.69***
		(14.16)	(18.37)	(22.18)
White	1 655.96	−62.38*	−87.75*	−320.00***
		(24.68)	(33.65)	(29.38)
Panel C. Spending by enrollee health status ($)				
0-25%	671.77	−38.79**	−45.36**	−98.46***
		(13.22)	(15.45)	(14.72)
26-50%	928.91	−33.87*	−43.54*	−110.67***
		(15.82)	(20.26)	(12.31)
51-75%	1 294.95	−92.91***	−127.88***	−112.33***
		(21.06)	(30.07)	(23.74)
76-100%	2 624.88	−167.14***	−248.08***	−276.89***
		(37.10)	(52.90)	(35.81)

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. Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 99.77^{th} percentile. 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); * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix Table A6: Children-only sample: Quality & consumer satisfaction

	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 (%)					
Annual Well-Child Visits	49.91	−0.46	−0.60	163,330	−3.88***
		(0.72)	(0.95)		(1.15)
Access to Primary Care	80.54	−1.60**	−2.10**	261,245	−4.75***
		(0.56)	(0.73)		(0.68)
Chlamydia Screening	55.23	1.34	1.76	7,011	−0.94
		(1.50)	(1.97)		(1.30)
Any Follow-up care after ADHD Rx	51.17	1.23	1.77	3,864	−1.38
		(2.22)	(3.22)		(1.70)
Behavioral	7.12	−0.44*	−0.58*	263,640	−1.57***
		(0.19)	(0.24)		(0.23)
Dental	58.71	−0.08	−0.11	263,640	−3.77***
		(0.53)	(0.69)		(0.70)
Panel B. Any Potentially High-Value Care Drug Classes in a Year (%)					
Statins	0.02	0.02	0.02	263,640	0.00
		(0.01)	(0.01)		(0.01)
Anti-Hypertensives	1.71	−0.09	−0.12	263,640	−0.54***
		(0.09)	(0.12)		(0.08)
Anti-Depressants	2.15	−0.05	−0.06	263,640	−0.33***
		(0.11)	(0.15)		(0.09)
Diabetes Medication	0.26	0.05	0.07	263,640	0.00
		(0.03)	(0.04)		(0.03)
Panel C. Any Potentially Low-Value Care in a Year (%)					
Any Low Value Care	0.65	−0.05	−0.06	263,640	−0.10**
		(0.05)	(0.06)		(0.03)
Avoidable ED	7.67	0.92***	1.20***	263,640	0.90***
		(0.20)	(0.29)		(0.14)
Imaging	21.06	−0.15	−0.20	263,640	−1.91***
		(0.29)	(0.38)		(0.28)
Panel D. Consumer Satisfaction (Relative to FFS)					
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, Each cell in columns (2), (3) and (5) corresponds to a separate regression, displaying the coefficient on an indicator for assignment to or enrollment in managed care. 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 (3) and adds the active-choosers to the sample for column (5). Only post-assignment observations are included (February 2012 to December 2014). See Table A5 notes for additional detail. 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 A7: Summary statistics for the plan transition experiment (pre-period)

	Overall		Pooled Control		Transitioned Plan	
	$\overline{Y}$ (1)	Std Dev (2)	$\overline{Y}$ (3)	Std Dev (4)	$\overline{Y}$ (5)	Std Dev (6)
Panel A. Enrollee Characteristics						
Female (%)	52.92	49.91	52.92	49.92	52.92	49.92
Age at baseline	10.91	9.22	11.06	9.39	10.67	8.93
Panel B. enrollee-year spending ($)						
Total	1 548.05	2 609.81	1 475.10	2 545.73	1 666.05	2 706.10
Medical	1 149.48	1 940.88	1 143.46	1 957.73	1 159.21	1 913.27
Inpatient	90.26	613.33	92.54	617.69	86.58	606.20
Outpatient	643.43	855.50	631.14	850.58	663.31	863.03
Pharmacy	369.67	930.56	304.23	825.40	475.50	1 070.74
Brand Drug	204.74	728.17	145.81	625.46	300.05	860.48
Generic Brand	156.32	331.52	150.73	334.65	165.36	326.21
Panel C. Any Annual Utilization of High- or Potentially High-Value Care (%)						
Annual Well-Child Visits	50.74	49.99	49.98	50.00	51.97	49.96
Access to Primary Care	80.52	39.60	79.85	40.11	81.61	38.74
Chlamydia Screening	54.01	49.84	53.57	49.87	54.69	49.78
Cervical Cancer Screening	57.89	49.37	56.65	49.56	60.02	48.99
Follow-up care after ADHD Rx	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	50.77	49.99	48.95	49.99	53.72	49.86
Statins	0.63	7.93	0.66	8.11	0.59	7.63
Anti-Hypertensives	3.37	18.04	3.23	17.68	3.59	18.61
Anti-Depressants	4.30	20.29	4.21	20.08	4.45	20.63
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 Care (%)						
Any Low Value Care	1.10	10.43	1.10	10.44	1.09	10.40
Avoidable ED	10.85	31.10	11.36	31.73	10.01	30.01
Imaging	25.41	43.54	24.86	43.22	26.31	44.03

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 2014 and remained until at least February 2016. Observations are at the enrollee-year level for the pre-period (February 2014 - February 2015): $N = 497,057$ enrollees. Additional details on the utilization and spending measures is available in Section 2. 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.3 for additional details.) Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 98.83^{th} percentile. Components of spending do not sum up to "Total" due to Winsorization.

Appendix Table A8: Comparing Auto-Assignee and Plan Transition samples

	Auto-Assignees		Plan Transition	
	$\overline{Y}$	Std Dev	$\overline{Y}$	Std Dev
	(1)	(2)	(3)	(4)
Panel A. Enrollee Characteristics				
Female (%)	52.92	49.92	52.92	49.92
Age at baseline	9.36	7.49	10.67	8.93
Panel B. enrollee-year spending ($)				
Total	1 451.35	2 427.61	1 666.05	2 706.10
Medical	1 052.74	1 815.46	1 159.21	1 913.27
Inpatient	97.48	747.79	86.58	606.20
Outpatient	590.29	820.12	663.31	863.03
Pharmacy	381.45	948.76	475.50	1 070.74
Brand Drug	229.30	757.06	300.05	860.48
Generic Brand	149.63	345.53	165.36	326.21
Panel C. Any Annual Utilization of High- or Potentially High-Value Care (%)				
Annual Well-Child Visits	49.34	50.00	51.97	49.96
Access to Primary Care	80.46	39.65	81.61	38.74
Chlamydia Screening	59.67	49.06	54.69	49.78
Cervical Cancer Screening	67.19	46.96	60.02	48.99
Follow-up care after ADHD Rx	51.17	49.99	51.12	50.00
Behavioral	7.40	26.18	8.08	27.25
Dental	55.18	49.73	53.72	49.86
Statins	0.30	5.51	0.59	7.63
Anti-Hypertensives	2.73	16.31	3.59	18.61
Anti-Depressants	3.55	18.52	4.45	20.63
Diabetes Medication	0.58	7.56	0.62	7.82
Panel D. Any Annual Utilization of Low- or Potentially Low-Value Care (%)				
Any Low Value Care	0.92	9.57	1.09	10.40
Avoidable ED	8.43	27.78	10.01	30.01
Imaging	23.33	42.29	26.31	44.03

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 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. 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-year level for the pre-period (February 2014 - February 2015): $N = 189,900$ enrollees. Additional details on the utilization and spending measures is available in Section 2. Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 99.77^{th} percentile. Components of spending do not sum up to "Total" due to Winsorization. Plan Transition respective winsorization percentile is 98.83^{th}.

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

	$\overline{Y}_{i_1,t_0}$ (1)	OLS (2)	OLS (3)	N (4)
Panel A. Spending by components of care ($)				
Total Spending	1 666.05	−286.89***	−200.15***	993,990
		(14.91)	(9.97)	
Inpatient Spending	86.58	−11.19***	−6.53**	993,990
		(3.33)	(2.37)	
Outpatient Spending	663.31	−54.95***	−28.36***	993,990
		(4.41)	(3.34)	
Pharmacy Spending	475.50	−189.68***	−143.91***	993,990
		(5.20)	(3.46)	
Panel B. Any Potentially High-Value Care in a Year (%)				
Annual Well-Child Visits	51.97	−1.10***	−1.30***	508,723
		(0.29)	(0.29)	
Access to Primary Care	81.61	−0.58***	−0.60***	975,918
		(0.16)	(0.17)	
Chlamydia Screening	54.69	0.34	−0.03	43,065
		(1.01)	(1.05)	
Cervical Cancer Screening	60.02	0.01	0.09	101,236
		(0.68)	(0.73)	
Follow-up care after ADHD Rx	51.12	1.41	0.72	13,121
		(1.77)	(1.78)	
Behavioral	8.08	−0.84***	−0.82***	993,990
		(0.12)	(0.12)	
Dental	53.72	0.38	−0.10	993,990
		(0.21)	(0.22)	
Statins	0.59	−0.06	0.00	993,990
		(0.06)	(0.02)	
Anti-Hypertensives	3.59	−0.27*	−0.18**	993,990
		(0.11)	(0.07)	
Anti-Depressants	4.45	0.01	0.03	993,990
		(0.11)	(0.08)	
Diabetes Medication	0.62	−0.07	−0.02	993,990
		(0.06)	(0.03)	
Panel C. Any Potentially Low-Value Care in a Year (%)				
Any Low Value Care	1.09	0.07	0.09*	993,990
		(0.05)	(0.04)	
Avoidable ED	10.01	0.32*	0.53***	993,990
		(0.14)	(0.13)	
Imaging	26.31	−0.39*	0.19	993,990
		(0.19)	(0.18)	
Re-weighted	Yes	No	Yes	

Notes: Sample consists of ’Control Group’ (comprised of the three MCOs) and ’Treatment Group’ (Managed FFS turned MCO): Feb14-Feb16 inclusive. Observations at enrollee-year level pooled over two years: $N = 994,114$. Number of enrollees: 494,31. 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.3 for additional details.) Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 98.83^{th} percentile. Components of spending do not sum up to "Total" due to Winsorization. Robust standard errors reported in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix Table A10: Decomposition of spending reductions caused by managed care

	Total Effect	Components			
	Overall Change	Change in Prices	Sub. to Cheaper Generics/Brands	Sub. from Brands to Generics	Residual
	$\Delta TS_{MMC,FFS}$	$\Delta P_{MMC,FFS}$	$\Delta Q^{Steering}_{MMC,FFS}$	$\Delta Q^{Generic}_{MMC,FFS}$	$\Delta Q^{R}_{MMC,FFS}$
	(1)	(2)	(3)	(4)	(5)
Panel A: Total Spending					
t_{-1}	0.87	0.10	0.13	0.02	0.61
t_0	−4.80	−1.53	−0.27	0.00	−3.00
t_1	−8.52	−2.62	−0.63	−2.39	−2.88
t_2	−7.49	−0.67	−0.72	−3.13	−2.97
Panel B: Medical Spending					
t_{-1}	1.93	0.07			1.86
t_0	−3.65	−1.91			−1.74
t_1	−2.26	−2.48			0.24
t_2	−0.56	−0.19			−0.34
Panel C: Pharmacy Spending					
t_{-1}	−1.40	0.14	0.48	0.10	−2.11
t_0	−7.06	−0.71	−0.75	−0.13	−5.48
t_1	−25.34	−3.01	−2.19	−8.60	−11.53
t_2	−25.28	−1.49	−2.41	−11.13	−10.25

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 (i.e., Anatomical Therapeutic Chemical (ATC) Classification, level 4) $\Delta Q^{Generic}$, 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 C.2. Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 99.96th percentile. Components of spending do not sum up to "Total" due to Winsorization.

Appendix Table A11: Utilization Management Summary Statistics of main ATC-4 therapeutic classes

ATC4	Description	ATC2	Share of Managed FFS Spending (%)	Denial rate in MMC (%)	Decomposition of spending effect				
					Overall Change	Change in Prices	Sub. to Cheaper Generics	Sub. from Brands to Generics	Residual
					$\Delta TS_{MMC,FFS}$	$\Delta P_{MMC,FFS}$	$\Delta Q^{Steering}_{MMC,FFS}$	$\Delta Q^{Generic}_{MMC,FFS}$	$\Delta Q^{R}_{MMC,FFS}$
			(1)	(2)	(3)	(4)	(5)	(6)	(7)
A10AD	Diabetes drugs	A10	0.8	22	36.0	26.0	−0.4	0.0	10.0
N07BC	Drugs used in opioid dependence	N07	1.7	25	32.0	16.0	6.9	0.8	8.7
N03AX	Other Antiepileptics	N03	0.8	19	10.0	3.2	−1.3	0.9	7.2
N05AX	Antipsychotics	N05	1.8	23	−6.1	−1.4	1.8	−7.1	0.7
R03CC	Selective Beta-2-Adrenoreceptor Agonists	R03	2.7	33	−6.5	−6.1	−0.7	−0.3	0.6
J01FF	Lincosamides	J01	0.5	34	−6.7	9.6	−16.0	0.1	−0.2
L02AB	Progestogens	L02	0.5	42	−9.1	−12.0	−0.3	−0.1	3.4
N06BA	Centrally acting sympathomimetics	N06	29.0	25	−11.0	3.0	−1.5	−9.9	−2.6
J05AH	Neuraminidase Inhibitors	J05	1.7	38	−11.0	−3.6	0.3	0.0	−7.9
J01CA	Penicillins With Extended Spectrum	J01	1.7	38	−12.0	−9.5	0.3	0.0	−2.9
S01AA	Antibiotics	S01	1.1	35	−13.0	−7.0	0.1	−2.2	−3.8
S01EA	Sympathomimetics In Glaucoma Therapy	S01	1.3	19	−22.0	−19.0	−20.0	20.0	−2.7
N02BE	Anilides	N02	0.6	27	−23.0	−35.0	−10.0	−0.6	22.0
R06AE	Piperazine Derivatives	R06	0.9	33	−24.0	−12.0	−0.6	−0.6	−11.0
R03AC	Selective Beta-2-Adrenoreceptor Agonists	R03	1.2	29	−25.0	5.7	2.2	0.0	−33.0
S01BA	Corticosteroids	S01	2.2	37	−27.0	−25.0	0.5	0.0	−2.4
R06AX	Other Antihistamines For Systemic Use	R06	0.5	34	−29.0	−29.0	−3.8	−2.5	5.8
N05AH	Diazepines, Oxazepines, Thiazepines And Oxepines	N05	0.7	25	−30.0	2.9	16.0	−44.0	−4.8
A04AA	Serotonin (5Ht3) Antagonists	A04	0.4	38	−30.0	−34.0	1.2	0.0	2.5
P03AC	Pyrethrines, Incl. Synthetic Compounds	P03	0.5	37	−33.0	−21.0	1.1	0.0	−13.0
N06AX	Antidepressants	N06	0.7	20	−33.0	−7.8	−2.7	−18.0	−4.6
R03DC	Leukotriene Receptor Antagonists	R03	0.6	32	−34.0	−3.3	−2.2	−0.8	−28.0
G03AC	Progestogens	G03	0.6	19	−35.0	4.0	−21.0	−5.0	−13.0
S02BA	Corticosteroids	S02	0.5	36	−37.0	−27.0	5.8	0.0	−16.0
J01AA	Tetracyclines	J01	0.5	30	−38.0	−11.0	−3.1	−0.1	−24.0
R05CA	Expectorants	R05	0.7	51	−40.0	0.2	0.0	−0.8	−39.0
S01AE	Fluoroquinolones	S01	0.6	44	−41.0	2.5	1.4	−0.9	−44.0
S01AD	Antivirals	S01	0.6	38	−42.0	4.0	−17.0	−8.9	−21.0
S03BA	Corticosteroids	S03	1.8	34	−42.0	−3.4	−4.8	−16.0	−19.0
A02BC	Proton Pump Inhibitors	A02	0.5	28	−44.0	−6.2	1.4	−46.0	7.7
R03BA	Glucocorticoids	R03	8.5	35	−45.0	3.3	2.2	−38.0	−12.0
	All other NDCs		5.9	33	−45.0	−2.9	0.1	−1.1	−41.0
C02AC	Antihypertensives	C02	3.0	23	−56.0	3.7	−0.8	−37.0	−22.0
B03AD	Iron In Combination With Folic Acid	B03	0.7	33	−63.0	−4.0	0.9	−27.0	−33.0
J01DD	Antibiotics (from dose-response figure)	J01	2.7	41	−78.0	−3.1	−27.0	−0.1	−49.0
D11AH	Agents for dermatitis	D11	1.1	47	−79.0	−1.7	−13.0	−0.2	−64.0
S01GX	Antiallergics	S01	1.0	52	−82.0	−1.5	−0.8	−26.0	−53.0

Notes: This 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 (See Section 6 for additional details). This table however does not include ATC-4s that have FFS enrollee per-year spending less than $1. This leaves us with 37 ATC-4s. Column (1) presents data for 2013-2104. 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 C.2 but combines result from 2013 and 2014 (See that section for more details on the claims data used). Overall enrollee-year spending is winsorized at $25,000 whereas other spending measures are winsorized at the 99.96th percentile. Components of spending do not sum up to "Total" due to Winsorization.