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THE DYNAMIC FISCAL COSTS OF OUTSOURCING HEALTH INSURANCE -EVIDENCE FROM MEDICAID

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The Dynamic Fiscal Costs of Outsourcing Health Insurance - Evidence from Medicaid
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ABSTRACT

Setting payment rates for providers contracted over multiple periods is a persistent challenge in government procurement. We study the dynamics of fiscal costs following the outsourcing of Medicaid provision to private health insurers by states. We focus on beneficiaries with disabilities who account for a third of Medicaid's spending. Using a national administrative database, we identify county-level private plan enrollment mandates and exploit them as an instrument for individuals' transition to managed care plans. These transitions, while initially slightly reducing fiscal costs, lead to a continuous increase in Medicaid's costs over subsequent years. Counties subject to mandates experience a 9.8% higher cost four years post-mandate compared to those without mandates. "Actuarially sound" endogenous payment rates, that are based on past costs in the market, may serve as mechanism underlying the rising spending.

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1 Introduction

The outsourcing of the provision of public health insurance services to private firms has grown dramatically over time in the United States (Gruber, 2017). In Medicare, over half of eligible beneficiaries are already enrolled in private Medicare Advantage (MA) plans (Biniek et al., 2023),[1] and in Medicaid the outsourcing of insurance to private health plans for over 70% of beneficiaries has left only a small parallel public program in many states (MACPAC, 2023).

Government procurement of these health insurance services from private managed care insurers is a complex and difficult process. A key part of this process is the determination of how payments to the 'winning' private insurers will be set. Due to federal regulations, payment rates are typically set administratively rather than as part of the procurement process (i.e., via the bids). These rates have to be set to achieve sufficient participation and quality, while incentivizing cost savings over time. This task becomes increasingly challenging when insurers hold private information about their true costs (Laffont and Tirole, 1993; Chalkley and Malcomson, 2000) and when those costs are endogenous to insurer decisions (Geruso et al., 2023). While a parallel public insurance program may provide an exogenous cost benchmark, the outsourcing of public health insurance to private managed care insurers has gradually rendered such benchmarks irrelevant. Endogenous cost benchmarks, based on observed historical spending of contracted insurers, often serve as an alternative in setting payment rates. Such benchmarks track market cost increases to prevent quality deterioration due to underpayment. They also allow regulators to capture a portion of any savings generated by private managed care plans. However, this payment design limits the incentive for plans to reduce spending because, in the long run, when insurers reduce costs, their payments are decreased (aka the "ratchet effect").

We study the fiscal costs of private versus public provision of health insurance services in Medicaid. Payment rates to private plans in states' Medicaid programs are set in an "actuarially sound" manner and are updated annually based on the plans' actual spending. Such endogenous rates have been required by law since 1981 and the federal regulator of Medicaid, the Centers for Medicare & Medicaid Services (CMS), has been publishing guidelines for setting these rates since 2002 (MACPAC, 2011).[2] To explore the impact of procurement under these rate-setting regulations

[1] The share in MA increased from 19% in 2007 to just over 50% in 2023.

[2] See, for example, CMS' "2021-2022 Medicaid Managed Care Rate Development Guide" (https://www.medicaid.gov/medicaid/managed-care/downloads/

we study the transitions of Medicaid beneficiaries with disabilities from the public fee-for-service program to private managed care plans that are paid an actuarially sound capitated rate.

Understanding the fiscal effects of this particular set of transitions is important in itself, as the Medicaid program is a major insurer, extending public health insurance coverage to about 30% of the U.S. population in 2022 (MACPAC, 2023). In 2021, close to one-third of the $699 billion in benefit costs was spent on beneficiaries with disabilities, despite the disabled population only making up 11% of Medicaid enrollment — 9.6 million beneficiaries. In an effort to restrain spending growth for this high-cost group and harmonize the use of managed care across different types of Medicaid enrollees, states have increasingly shifted the provision of Medicaid benefits to private managed care plans. In 2008, only 28% of disabled beneficiaries were enrolled in a comprehensive risk-based managed care plan (MACPAC, 2011), but by 2021 this share increased to 53%, representing a dramatic shift in how Medicaid works for this group. While the general Medicaid population experienced earlier transitions to private plans,[3] these transitions largely excluded disabled beneficiaries.

Previous research has provided mixed findings on the impact of Medicaid privatization on program costs (Duggan and Hayford, 2013), leaving uncertainty about whether outsourcing effectively achieves the goal of cost savings for general Medicaid beneficiaries. Moreover, it remains unclear if these findings can be generalized to individuals with disabilities. The general Medicaid population primarily comprises relatively healthy groups, such as children and parents, who may not benefit significantly from managed care tools and for whom costs are more stable and homogeneous, making the rate-setting problem more straightforward. In contrast, disabled beneficiaries, who are typically sicker individuals with complex conditions, may derive more substantial benefits from managed care tools that aid in managing chronic conditions and reducing costly hospital stays. Therefore, private plans may have greater potential for spending reductions among the disabled population, raising the possibility that rate-setting methods that adjust rates downwards as cost savings are achieved could yield savings for the Medicaid program for this group despite achieving little savings for other, healthier groups. However, the incentives to realize this potential are still limited, making the actual realized savings an important empirical question.

To study the fiscal effects of outsourcing under endogenous payment rates, we use

2022-2023-medicaid-rate-guide-03282022.pdf)

[3]Among non-disabled Medicaid beneficiaries, 60% of children and 44% of adults were enrolled in private plans already by 2008.

beneficiary-level national data on Medicaid program spending (claims paid for those in the public program and capitation payments for those in private plans) from 2004 to 2015 (the latest available year in our data source). To identify the causal effect of the shift to private plans on program costs, we exploit county-level enrollment mandates that induce rapid shifts in the share of disabled beneficiaries enrolled in private managed care plans. Mandates allow us to better address possible selection biases due to beneficiaries' voluntary enrollment in private plans. Within a stacked difference-in-differences framework (DID), we compare counties that implement an enrollment mandate to counties where disabled Medicaid beneficiaries remain in the public fee-for-service (FFS) system (never-treated or later-treated counties). This empirical approach avoids possible bias in our estimates due to heterogeneous treatment effects (Goodman-Bacon, 2021; Sun and Abraham, 2021). Our baseline sample includes a treatment group of beneficiaries in 980 counties with private plan enrollment mandates during our study period. The control group includes beneficiaries in 1,305 counties. Event studies confirm that our treatment and control groups have similar spending trends during the four years before a mandate is rolled out.

We find that while fiscal costs decrease by $29 (2.2%) per member per month (PMPM) during the first mandate year, they continuously increase afterwards. By the fourth post-mandate year, counties with an enrollment mandate experience higher fiscal costs of $132 (9.8%) PMPM, compared to counties that maintain the public FFS system for disabled beneficiaries. The average share of disabled beneficiaries enrolled in private plans in these mandate counties is higher at that time by 52 percentage points, leading to an IV (LATE) estimate of a $253 (18.8%) increase in the costs of each beneficiary making the transition to private plans due to the mandate. We show that a large part of the spending growth comes from states setting capitation payments to private plans higher than counterfactual FFS spending, with the wedge between private plan payments and counterfactual FFS spending increasing over time. We find increased spending for mandate-counties in most sample states, but some states do achieve lower spending 4 years after the mandate. We find little correlation between these changes in spending and baseline Medicaid prices in each state.[4]

We verify the robustness of our results to alternative analytic samples and empirical approaches. First, we examine the reliability of the Medicaid data in each state by comparing the aggregate spending in our dataset to the verified aggregate

[4]This is in contrast to a prior study on the transitions of the general Medicaid population to MMC, that found that private plans were more likely to reduce spending in states with more generous baseline Medicaid reimbursement of providers (Duggan and Hayford, 2013).

spending amounts that states report annually to the federal government in CMS-64 forms.[5] Our baseline dataset uses only a subsample of state-years where these measures match reasonably well, and our results are robust to using stricter matching criteria, as well as to using the full sample. Second, we repeat the estimation on a sample that includes a balanced panel of treatment counties. Third, we exclude from our sample control counties that implemented a mandate at a later date, using only never-treated counties as controls. Fourth, we estimate a specification where only contiguous counties serve as controls. Across all samples and specifications, our main results consistently demonstrate that the transition to private plans does not lead to fiscal cost savings. Four years after a transition, Medicaid costs are higher for shifting beneficiaries by 16% to 35% of baseline spending levels. In addition to these robustness tests, we run a placebo test in which we examine the effect of Medicaid enrollment mandates on *Medicare's* spending on its non-elderly disabled beneficiaries who are not dually enrolled in Medicaid, a similar group for whom Medicaid policies should have no effect. We estimate a precise zero, indicating that spending levels in treatment and control counties were trending similarly in the Medicare program around the Medicaid private plan enrollment mandates. This finding suggests that our control counties represent good counterfactuals for spending trends for disabled individuals in our treatment counties.

Finally, we discuss how the current guidelines mandating actuarially-sound payment rates in Medicaid Managed Care may lead to states setting capitation rates higher than counterfactual FFS costs, leading to a dynamic increase in spending, in contrast to the stated reason for outsourcing. Whether annual rates are based on plans' bids within fairly narrow (actuarially sound) bounds, or are administratively set at a fixed level for all insurers, they are mostly based on observed spending patterns at the market level in preceding years. Since in many MMC markets procurement rules result in only a few insurers participating in any given year, a significant portion of each insurer's cost increase can be offset in subsequent years through the updated rates, thereby limiting the incentive for managed care plans to reduce spending.[6]

[5] These spending amounts are more likely to be accurately reported, as they are used to determine federal matching (FMAP) payments to states.

[6] Geruso and McGuire (2016) define the concept of "power" in a health plan payment system as the extent to which payers or regulators compensate plan expenditure on the margin. While Geruso and McGuire (2016) examine power in a static sense of a single year, our focus lies on "dynamic power" resulting from the scheme of updating capitation rates over time (i.e., to what extent future rates will compensate insurers' permanent increase in spending over the next years). A tradeoff between fit and power persists also in the dynamic sense, with low "dynamic fit" possibly leading to lower non-contractible quality, to re-negotiations and exits, and exacerbating the incentives for

An alternative payment mechanism could involve a 'yardstick competition' strategy (Schleifer, 1985) where payments for a specific plan are based on observed spending patterns for all *other* plans in the market in previous years, or on patterns in related MMC markets. Such a mechanism for partly-endogenous payments may deviate from full actuarial soundness, as it excludes spending for a share of the relevant population from the calculation, but it may create stronger incentives for plans to reduce spending and allow states to capture most of those savings. Other proposals for an updating mechanism called for exogenous updating of capitation rates based on the medical component of the Consumer Price Index,[7] or based on pre-determined national predictions by the Congressional Budget Office for the growth in Medicaid spending.[8] Other countries use a combination of national price and wage indices to introduce an exogenous component to the updating scheme.[9] Ultimately, however, it is unclear that lower spending is a desirable outcome in the Medicaid context, as reduced spending may harm access to care and lower quality for beneficiaries (Layton et al., 2022; Geruso et al., 2023). Nevertheless, if lower spending is indeed the goal of outsourcing to private insurers, it is unlikely to be achieved under the current payment mechanism.

Our paper contributes to the literature that assesses the impacts of private versus public provision of public health insurance benefits. Private provision has become increasingly common across US social health insurance programs (Gruber, 2017), yet there are still many open questions about the trade-offs involved. Prior work on private provision in Medicaid has produced mixed results. Evidence from earlier transitions in Medicaid suggested that in some cases managed care led to increases in access and quality and in other cases decreases in these outcomes (Sparer, 2012). Prior work also suggests mixed effects on program costs (Duggan and Hayford, 2013; Marton et al., 2014; Perez, 2018; Dranove et al., 2021; Macambira et al., 2022). The limited research on the effects of private provision for sicker populations has mostly focused on single states and has also produced mixed results. Recent work studying

"static" risk selection.

[7]CPI-based rate updating was included in the Healthy Adult Opportunity initiative by the Centers for Medicare & Medicaid Services. See: https://www.cms.gov/newsroom/press-releases/trump-administration-announces-transformative-medicaid-healthy-adult-opportunity

[8]This suggestion was included in Tennessee's block grant proposal. See: https://www.tn.gov/content/dam/tn/tenncare/documents2/TennCareAmendment42.pdf

[9]For example, Israel's formula for updating the base rate paid to health plans in the National Health Insurance scheme includes the Consumer Price Index, the Price Index of Inputs in Residential Building, national indices for wages in the health care sector and in the public sector, and the change in the national minimum wage.

the transition of non-inpatient and non-drug healthcare services for disabled Medicaid beneficiaries in Texas indicates improvements in access to care but also increases in program costs (Layton et al., 2022). Research examining a more comprehensive transition in California, however, suggests negative health effects of private provision (Duggan et al., 2021). Our paper contributes to this literature by providing a national comprehensive estimate of the fiscal costs of private provision for the disabled Medicaid population as well as estimates of the variation in effects across states. Beyond Medicaid, a long literature examines the effects of private provision in Medicare, i.e., the Medicare Advantage (MA) program, generally finding lower utilization and similar or better quality compared to traditional Medicare (TM) (e.g., Duggan et al. (2018); Curto et al. (2019); Landon et al. (2023)). However, it also seems to be the case that the Medicare program pays more to MA plans than counterfactual TM costs, due to high legislatively-determined MA benchmark payments in some time periods, plus some combination of advantageous selection (conditional on risk scores) and risk adjustment upcoding. Our paper adds to this literature by showing that increases in fiscal costs under private provision are common across public health insurance programs.

Beyond the United States, many other countries around the world also use 'managed competition' among private plans to provide social insurance benefits. This is the case in the Netherlands, Germany, Switzerland, Israel, and many more. A long literature studies the design of these programs and the effects of design choices on program costs and quality of care (see McGuire and van Kleef (2018) for a review). Our paper contributes to this literature by highlighting the fiscal consequences of the decision to provide social health insurance benefits through private plans and also by emphasizing that procurement and payment rules most likely impact these consequences.

The paper is organized as follows: Section 2 describes the national administrative dataset we use, section 3 lays out our empirical approach, and Section 4 describes our results. We discuss our findings in section 5. Section 6 concludes.

2 Data

Our main data source is the Medicaid Analytic eXtract (MAX) - an administrative dataset managed by the Centers for Medicare and Medicaid Services (CMS). We use

data for the years 2004 to 2015.[10] Enrollment information on Medicaid beneficiaries is taken from the MAX Personal Summary files (PS), which contain person-month Medicaid enrollment status as well as indicators for monthly enrollment in private Medicaid managed care plans. These files also include data on demographic characteristics and the basis for Medicaid eligibility (including eligibility due to disability). Data on Medicaid's fiscal spending for each beneficiary is included in the MAX Inpatient (IP), Other Therapy (OT), Prescription Drug (RX), and Long Term Care (LT) files. These files track claims for services provided by the public FFS system. They also include information on the capitated premium payments to managed care plans.

We use these data to construct individual-level program costs. For beneficiaries not enrolled in a private plan, program costs are equal to the sum of all fee-for-service medical and drug claims. For beneficiaries enrolled in a private plan, program costs are equal to the capitation payment paid to the private plan plus any residual fee-for-service spending for services carved out of the private plan contract.

Our full sample includes all non-elderly beneficiaries that are eligible for full benefits from Medicaid due to disability, and are not enrolled in Medicare. We exclude all beneficiaries that ever moved between states or counties.[11]

To assess the reliability of the MAX data in each state, we compare the state's aggregate Medicaid spending in the MAX to the Medicaid spending that the state reports to CMS in CMS-64 forms. These forms report actual quarterly expenditures for which all supporting documentation has been compiled, and are used to determine the federal reimbursement to states.[12] Our main analytic sample includes data from state-years we deem to have reliable data. As a robustness test, we also examine a subsample of state-years, using a more conservative definition of reliability.[13]

In addition to Medicaid data, for a placebo test we also use data on Medi-*care* enrollment and the claims of non-elderly disabled beneficiaries from CMS. The base segment of the Master Beneficiary Summary File (MBSF) includes information on Medicare enrollment, enrollment in Medicare Advantage and in a Prescription Drug

[10] 2015 is the latest year for which MAX data is available. Medicaid claims and encounter data from later years are available only in the Transformed Medicaid Statistical Information System Analytic Files (TAF). Data from MAX and TAF are not directly comparable.

[11] We can identify and exclude movers between counties only if they appear in different counties in separate years.

[12] https://www.medicaid.gov/medicaid/financial-management/state-expenditure-reporting-medicaid-chip/index.html

[13] We construct two subsamples - a 'liberal' subsample and a 'conservative' subsample. The liberal sample requires MAX and CMS-64 spending trends to match closely but allows the levels to vary somewhat. The conservative sample requires MAX and CMS-64 spending levels and trends to match.

Program plan, and information on the basis of eligibility. Spending information is gathered from the MedPAR file, that contains information on inpatient hospital and skilled nursing facility stays, the outpatient file, and the Carrier file, that holds claims submitted by professional providers. We use data on disabled beneficiaries that joined traditional (FFS) Medicare before 2004 - the first sample year of our Medicaid data.

3 Empirical Framework

3.1 Identifying managed care enrollment mandates

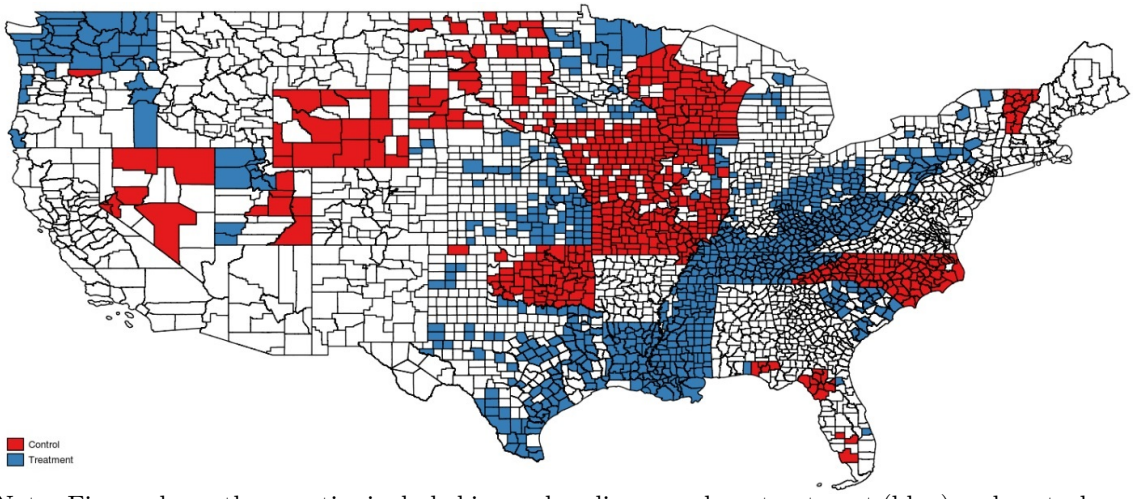
To identify county-level managed care enrollment mandates, we use the MAX data to calculate the penetration rate of managed care plans among the disabled beneficiaries in each county over time.[14] We define a county-level mandate as a sharp and swift increase in the penetration rate — an increase of at least 20 percentage points in the rate over at most 3 months. We use counties with a mandate as treatment counties, excluding mandates that occur in the first or last six months of our sample (where we would not have sufficient pre- or post-mandate periods). We also restrict to treatment counties in which managed care penetration was below 10 percent prior to the mandate. Counties in which the managed care penetration rate never exceeds 10 percent, i.e. counties that rely almost entirely on the FFS public system throughout our sample period, are used as control counties. Our baseline analysis also uses as controls counties in which a mandate occurs at least 5 years after the examined mandate in the cohort's treatment counties.

Figure 1 presents the map of the identified treatment (blue) and control (red) counties in our full sample. Most of the mandates in our sample occur between 2011 and 2013 (Appendix Figure A1).

While in some cases every county in a state has a mandate, in other cases only a subset of counties has a mandate during our sample. This is consistent with prior work showing that states often roll out these mandates in a staggered fashion (Layton et al., 2022).

[14] Alternatively, one could identify mandates from state policy announcements. However, we found that (1) announcements of these mandates are often difficult to find and (2) stated mandates are often not *de facto* binding. We thus prefer this empirical approach.

Figure 1. Treatment and Control Counties in the Full Sample



Note: Figure shows the counties included in our baseline sample as treatment (blue) and control (red) counties.

3.2 Specification

To estimate the effects of private provision on program costs we exploit county-level private plan enrollment mandates. We restrict to mandates that swiftly push a large share of disabled beneficiaries into private managed care plans, generating sharp variation in private plan enrollment. We estimate the effect of the transition to managed care on Medicaid's fiscal spending using a stacked difference-in-differences (DID) framework, comparing changes in program costs for beneficiaries residing in counties in which there was a mandate (treatment counties) to changes in program costs for beneficiaries residing in counties where beneficiaries remain in the public fee-for-service plan throughout our sample period (never-treated), or for at least 5 years after a mandate in the examined treatment county (later-treated). Following the stacked DID approach, used by Deshpande and Li (2019), Fadlon and Nielsen (2021), and others, we create a separate dataset for each quarterly-cohort of treated counties with a mandate during the quarter, and all of the control counties. These datasets have an annual frequency and include up to four years before and after the mandate. All of the counties in each cohort's dataset are assigned the same timing variables relative to the quarter of the mandate in the cohort's treatment counties. These cohort-by-cohort datasets are then stacked together to create the analytic sample.

We estimate both standard DID specifications as well as event study specifications to illustrate both the evolution of treatment effects over time and the comparability of spending trends in treatment and control counties during the pre-mandate period.

All regressions include individual fixed effects, interacted with cohort fixed effects to ensure within-cohort comparisons, and calendar-year fixed effects. We include the individual fixed effects also to ensure that potential changes in the composition of disabled Medicaid beneficiaries do not bias our results. While such changes are unlikely for this population, where eligibility is related to disability, they have been shown in other populations (Currie and Fahr, 2005), so we include them to preclude this alternative explanation for our results. As a robustness test, to control for potential cohort-specific time trends, we also estimate equations that include year fixed effects interacted with the cohort fixed effects, finding similar results (Appendix Tables A2 and A3).

Our event study regression specification is as follows:

$$Y_{icth} = \sum_{j=-4}^{3} 1(t = j)(\alpha_j + \beta_j isTreated_{ic}) + \gamma_{ih} + \delta_y + \epsilon_{icth} \quad (1)$$

where Y_{icth} is the examined outcome for individual i in county c at year t around the mandate in cohort h. $isTreated_{ic}$ indicates whether individual i who resides in county c has an enrollment mandate in her county within the cohort's time frame. γ_{ih} are the individual fixed effects interacted with cohort fixed effects, δ_y are the calendar year effects, and ϵ_{it} represents a random error term. β_j's are the coefficients of interest and are presented in the event studies charts. Using the year before the mandate as our base period, each coefficient indicates the change in the examined outcome in treated counties compared to the control counties, j years before/after the mandate. The outcomes we examine include: the share of individuals in MMC plans, total Medicaid spending, fee-for-service Medicaid spending, and Medicaid spending on capitated payments.

For the DID analysis, we estimate both reduced form and IV specifications, where we use enrollment mandates as an instrument for enrollment in a private plan, effectively scaling the reduced form estimates by the level of compliance with the mandate. As we show below, mandates are sharp and have major effects on enrollment, but they are imperfect in that not all beneficiaries ultimately comply. To examine dynamics, we estimate a specific first-stage and IV regression for each post-mandate year, including in all the equations four separate indicators for each of these years. Our first stage regressions are thus:

$$InMMC_{icth} = \sum_{j=1}^{4} 1(t = j)(\alpha_j + \beta_j isTreated_{ic}) + \gamma_{ih} + \delta_y + \epsilon_{icth} \tag{2}$$

where $InMMC_{icth}$ indicates the share of quarters in year t after the mandate ($t \in \{1, 2, 3, 4\}$) in cohort's h dataset in which individual i in county c was enrolled in a managed care plan. $1(t = j)$ is an indicator equal to 1 if t occurs in the jth year after a mandate was rolled out in cohort h. γ_{ih} are the individual fixed effects interacted with cohort fixed effects, δ_y are the calendar year fixed effects. ϵ_{icth} represents a random error term. Our reduced form specification is given by:

$$Y_{icth} = \sum_{j=1}^{4} 1(t = j)(\alpha_j + \beta_j isTreated_{ic}) + \gamma_{ih} + \delta_y + \epsilon_{icth} \tag{3}$$

where Y_{icth} is the outcome of interest for individual i, in county c, at year t around the mandate in cohort h.

Finally, The IV specification, for each post-mandate year, is given by

$$Y_{icth} = \lambda \widehat{InMMC}_{icth} + \gamma_{ih} + \delta_y + \psi_{icth} \tag{4}$$

where $\widehat{InMMC}_{icth}$ is the predicted value for year $t \in \{1, 2, 3, 4\}$ post-mandate from equation 2 and ψ_{icth} is a random error. λ represents the effect of private provision on the outcome, while β represents the effect of the mandate on the outcome.

Ultimately, the IV regression will generate a local-average treatment effect (LATE) that applies to those beneficiaries who comply with the mandate. Generally, this is the policy-relevant population, as these are the individuals who ultimately enroll in private plans. Non-compliers are either beneficiaries in their first months of Medicaid enrollment, where they generally enroll in the public FFS plan for a few months prior to being shifted to private plans, or they are excluded from private plan mandates. Always-takers, who enroll in private plans even before a mandate, are excluded from the analytic sample. As seen in the first stage regression below, the compliers group is also very large, indicating that the LATE here is actually the parameter of interest.

The identifying assumption behind the DID strategy is that outcomes in the treatment counties would have followed the same trend as outcomes in the control counties through the post-mandate period if the enrollment mandate had not occurred. In Appendix Table A1, we compare beneficiaries residing in treatment and control counties, finding similar overall spending and mostly similar demographic composition in the

pre-mandate period. To validate the identifying assumption, we include event studies explicitly investigating whether program costs in treatment and control counties trended similarly prior to the mandate. While this does not prove that trends would have been similar post-mandate, it does increase our confidence that this will be the case.

As an additional robustness test, We analyze contiguous treatment and control counties, that could be more comparable. The restricted sample includes treatment counties (i.e. counties with an identified mandate), only if they have contiguous control counties (i.e. with managed care penetration that never exceeds 10 percents). Each treatment county and its contiguous control counties form a cohort. The analytic sample is constructed by stacking all the different cohorts together. Figure A2 in the appendix presents a map of the (blue) treatment counties and (red) control counties. The empirical analysis of the contiguous counties stacked dataset is similar to the analysis of the stacked dataset in our baseline approach.

4 Results

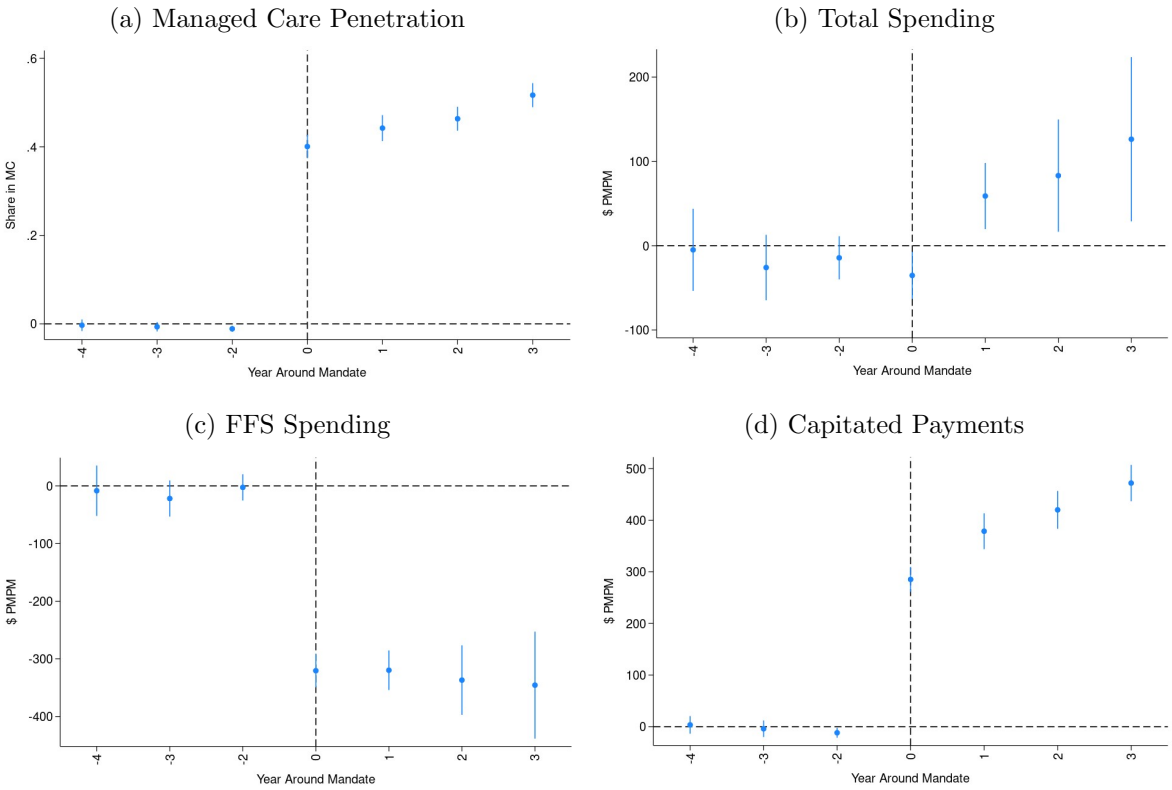
4.1 Event studies around enrollment mandates

Figure 2 presents event studies examining the difference in outcomes between our treatment and control groups surrounding a managed care enrollment mandate. In the four years leading up to a mandate implementation, the event studies reveal no significant differential trends between treatment and control counties in spending per Medicaid beneficiary or its components. Following the mandate, we find a substantial increase of 40 to 50 percentage points in the penetration rate of managed care plans in treatment counties relative to control counties (Panel A). While total Medicaid spending in treatment counties (Panel B) is lower at the first year post-mandate, it demonstrates a continuous and monotonic rise in each subsequent year. These results provide no support to the claim that managed care mandates save costs for the Medicaid program, outside of the first implementation year. Instead, they suggest that mandates lead to a dynamic pattern of increasing spending.

We break down the changes into differences in Medicaid FFS spending (Panel C) and differences in capitated payments (Panel D). As expected, a mandate that shifts a considerable proportion of enrollees from the public FFS system to managed care plans initially leads to a decrease in FFS spending and an increase in capitated payments. In the years following the mandate, capitated payments increase, while

FFS payments remain mostly unchanged, despite the gradually increasing penetration of managed care. Such patterns are consistent with a gradual divergence between actual FFS spending (observed for those in mandate counties who do not move to managed care) and capitation payments to private plans.

Figure 2. Event studies around managed care enrollment mandates



Note: Figures show event studies around managed care enrollment mandates, i.e. the difference in the examined outcome between treatment counties and control counties, relative to the year before the mandate. Year zero is the first year in which the mandate is in place (denoted by a vertical dashed line). Panel A presents the managed care penetration in the county, i.e. the share of disabled beneficiaries enrolled in a managed care plan. Panel B shows the dollar differences between treatment and control in the total Medicaid spending per beneficiary per month (PMPM). Panels C and D break the total into differences in FFS spending (panel C), and in capitated payments to managed care plans (panel D).

4.2 Reduced form and IV estimates

The top panel of Table 1 presents reduced form estimates of the dynamic effects of a managed care enrollment mandate over the four years following its implementation. Examining the first stage, we find a continuous rise in managed care penetration

during this period, with an increase of 41 percentage points in the first year, rising to 52 percentage points by the fourth year. Initially, total Medicaid spending decreases in the first year after a mandate by $29 (2.2%). This suggests that states set initial post-mandate capitation rates to private plans slightly lower than the pre-mandate FFS spending level. However, total Medicaid spending steadily rises in the subsequent years. Compared to control counties, it is higher by 4.9% of the baseline mean ($65 PMPM) in the second year after the mandate, 6.8% ($91) in the third year, and 9.8% ($132) in the fourth year post-mandate. The IV estimates, presented in the bottom panel of Table 1, also demonstrate a dynamic pattern of increasing costs for Medicaid enrollees after transitioning to a private plan due to a mandate. By the fourth year post-mandate, total Medicaid spending on transitioning beneficiaries is $253 PMPM higher (18.8%) compared to those remaining in FFS.

Table 2 presents the IV estimates from our robustness tests for the fourth-year effect of transitioning to managed care. Across all alternative samples and specifications, we consistently find that the transition eventually leads to an increase in Medicaid's total costs. Almost all of the alternative specifications (conservative data quality criteria, balanced panel, never-treated controls, county FEs, and contiguous counties) produce larger effects, suggesting that our main estimates may be lower-bounds for the true effect of managed care on fiscal costs. Further, as the specification in column (6) restricts to contiguous treatment and control counties and finds an overall estimate close to the main results in column (1) of Table 1, it boosts confidence that treatment/control county differences do not explain our estimates. Overall, these robustness tests boost confidence in the conclusion that managed care does not decrease program costs and likely increases them.

4.3 Heterogeneity between states by pre-mandate prices

Given the variations in the Medicaid program across states, we now explore potential heterogeneity in the effect of managed care on spending. We find that for most states in our baseline sample, transitioning to a managed care plan following an enrollment mandate did not lead to lower Medicaid program spending four years after the mandate (Figure 3).[15] However, five states – Louisiana, Mississippi, Pennsylvania, South Carolina, and Utah — exhibit a significant decrease in spending in the fourth

[15] We could estimate a fourth-year effect for 15 states. In 8 out of 15, the estimated effect was positive. The estimate was negative, but not different than zero in a statistically significant way, in two more states.

Table 1. The effects of a managed care enrollment mandate (reduced form) and enrollment in a managed care plan (IV)

Year post-mandate	(1) Total Spending	(2) FFS Spending	(3) Capitated Payments	(4) MC Penetration
Reduced form:				
Year 1	-29.12**	-318.360***	289.18***	0.41***
	(18.17)	(14.64)	(12.21)	(0.01)
Year 2	65.42***	-317.73***	383.15***	0.45***
	(22.30)	(16.97)	(16.67)	(0.02)
Year 3	90.81**	-334.07***	424.88***	0.47***
	(39.03)	(34.30)	(17.56)	(0.01)
Year 4	132.19**	-346.03***	478.22***	0.52***
	(56.24)	(53.42)	(16.99)	(0.01)
IV:				
Year 1	-71.86	-785.42***	713.56***	
	(45.52)	(37.91)	(20.16)	
Year 2	146.16***	-709.89***	856.05***	
	(47.56)	(40.73)	(19.01)	
Year 3	193.59**	-712.14***	905.73***	
	(82.01)	(73.13)	(21.89)	
Year 4	252.52**	-660.99**	913.51***	
	(107.60)	(99.34)	(20.77)	
Baseline Mean	1,344	1,305	39	0.0
# of cohorts		32		
# of beneficiaries		2,343,468		
# of counties		1,432		

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

Note: Table shows the reduced form estimates of the impact of a managed care enrollment mandate on the examined outcomes in treatment counties, relative to control counties (top panel). It also presents the IV estimates of the effect of individuals' transition to a managed care plan, instrumented by enrollment mandated in their county (bottom panel). All specifications include also individual fixed effects interacted with cohort fixed effects, and year fixed effects. Robust standard errors, clustered at the county level, are shown in parenthesis. Column (1)-(3) show the effect on total/FFS/Capitated spending accordingly, all measured in dollars per beneficiary per month (PMPM). Column 4 presents our first stage - the effect of a mandate on the share of disabled beneficiaries in the county enrolled in a managed care plan (among beneficiaries not already enrolled in a managed care plan pre-mandate). Baseline mean values are calculated for the year before a mandate. For more details see Section 3

Table 2. The effect of MMC enrollment on total costs, 4 years post-mandate - IV estimates for alternative samples and specifications

	(1) Full Sample	(2) Conservative Criteria	(3) Balanced Panel	(4) Never-Treated Controls	(5) County FEs	(6) Contiguous Counties
Year 4	213.69**	447.23***	370.64***	447.84***	346.50***	332.71***
	(90.41)	(129.80)	(131.55)	(73.75)	(85.94)	(107.01)
% of baseline	15.0	36.1	39.2	26.7	42.1	18.9
Baseline Mean	1,308	1,283	1,583	1,311	1,312	1,243
# of cohorts	34	31	5	32	32	62
# of beneficiaries	3,002,386	1,880,418	1,383,816	2,032,383	2,343,482	178,332
# of counties	1,663	1,276	896	1,431	1,432	148

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

Note: Table shows IV estimates of the impact of individuals' managed care enrollment on total Medicaid spending on them. Individual's MC enrollment is instrumented by MC enrollment mandates in the individual's county. Robust standard errors, clustered at the county level, are shown in parenthesis. Column (1) presents estimates from examining the full sample of all states. In column (2) we apply a "conservative" reliability criteria for states - a more strict criteria than the one used in our baseline specification and all other samples. Column (3) uses a sample that includes a balanced panel of treatment counties for which data is available four years before and after an enrollment mandate. Column (4) presents the estimates when using as controls only counties that never have an enrollment mandate during the sample period. Column (5) presents the results of an alternative specification in which county-level fixed effects are included instead of individual-level fixed effects. Lastly, column (6) presents the IV estimate from a specification that compares contiguous treatment and control counties.

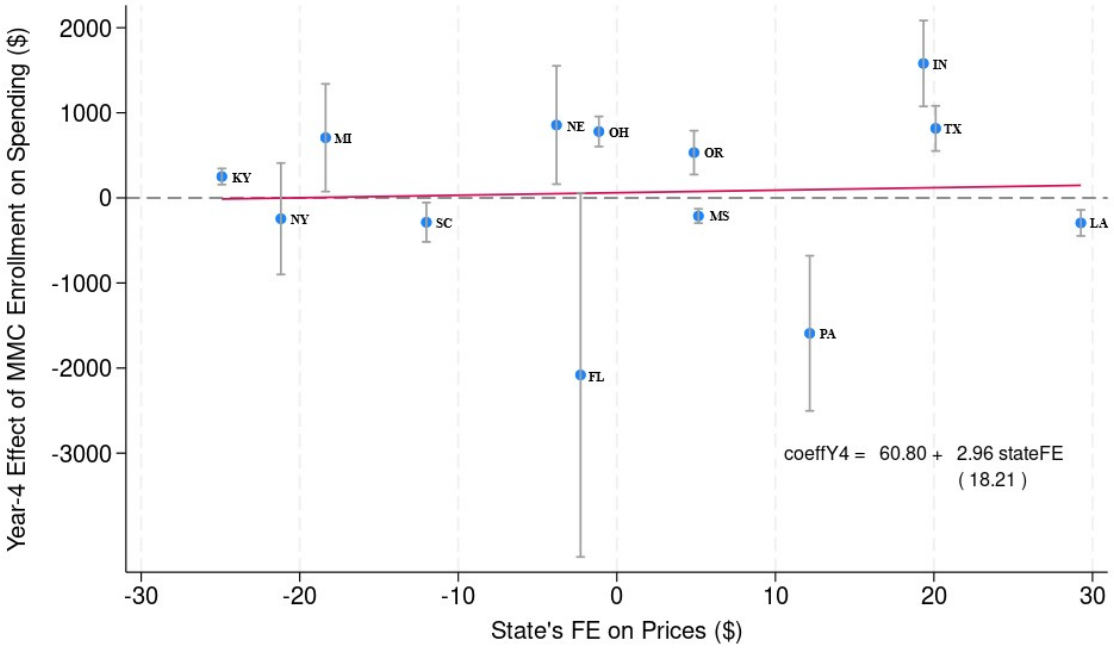
year. For Louisiana, this finding aligns with with the analysis in Macambira et al. (2022).

The effectiveness of managed care plans in reducing costs by adjusting payment rates to providers may depend on the baseline payment rates in the public FFS system they replace. If the initial rates are notably low, managed care plans may face higher rates than the public FFS system, leading to increased costs. However, our data does not support this hypothesis, contrary to the findings of Duggan and Hayford (2013). While their estimation relies on prices for a single service — newborn delivery (using data collected by Schwartz et al. (1991)) — we construct state-specific price measures that account for *all* outpatient FFS services provided to Medicaid beneficiaries with disabilities. States' fixed effect on prices are estimated based on 2004 claims. The specification is:

$$Price_{ips} = \beta_0 + \gamma_p + \delta_s + \epsilon_{ips} \tag{5}$$

where $Price_{ips}$ is the payment for procedure p in claim i in state s. γ_p is procedure fixed-effect, δ_s is the state fixed-effect, and ϵ_{ips} is a random error. Figure 3 presents, for each state, the IV estimate for the effect of managed care enrollment on spending during the fourth post-mandate year, relative to the state's fixed effect on FFS prices in 2004. Estimating the linear trend, we find no significant correlation between lower pre-mandate FFS prices and higher increases in total spending post-mandate.[16]

Figure 3. States' pre-mandate fixed effect on Medicaid FFS prices and the effect of MMC enrollment on Medicaid spending



Note: For each state, the figure shows: a. IV estimate for the effect of enrollment in a managed care plan on total Medicaid spending during the fourth post-mandate year (y-axis), and b. The state's fixed effect on Medicaid FFS prices, measured using prices of outpatient services that appear in 2004 FFS claims of Medicaid disabled beneficiaries (x-axis). Red line is a linear trend of the points included in the scatter plot and the equation for this line is presented. Standard error for the coefficient of interest is shown in parentheses.

[16]Two outlier states, Minnesota and Utah, are excluded from the figure. The correlation between the IV estimates and the states' prices FEs remain statistically insignificant also when including these two states (Appendix Figure A3).

5 Discussion

We find that in most states shifting Medicaid's disabled beneficiaries from the public FFS system to managed care plans increases fiscal spending in the Medicaid program, creating a dynamic pattern of spending increases. One potential mechanism behind this result is the way that capitation payments to managed care plans are set. States' reimbursement rules are constrained by CMS' guidelines for the development of capitation payment rates to managed care plans (CMS, 2019). These guidelines direct states to set and annually update actuarially sound rates based on the experience of the Medicaid population over the past three years. While CMS allows actuaries to use data from similar populations and to consult other sources when developing rates and setting the trends for medical costs, CMS maintains that actual experience of the relevant Medicaid population should be a primary and important consideration (Armstrong et al., 2016). Such update rules make sure that past increases in plans' costs lead to higher payments by the Medicaid program, reducing plans' incentives to save costs. Moreover, CMS' guidelines require plans to comply with a Medical Loss Ratio (MLR) of 85% or higher. This requirement may further decrease plans' incentives to save, as higher spending helps achieve higher profits in absolute terms.

These rules simultaneously (1) limit managed care plan incentives to reduce spending and (2) generate an explicit link between plan spending and plan capitation rates. If plans spend more, endogenously or exogenously to the payment rates, plan payments and program spending will increase. While we've already established ways in which this way of setting payment rates could endogenously increase plan spending, there are also exogenous reasons why private plans may spend more than the public system. First, utilization may increase in managed care compared to the public FFS system if disabled beneficiaries are under-served in Medicaid's public FFS system (KFF, 2012). Unmet need may be addressed after the transition to private managed care plans, increasing utilization. For example, in Texas, the FFS system limited the number of prescription drug fills to three per month, and this restriction was eliminated after transitioning to managed care, resulting in higher utilization and spending on prescription drugs (Layton et al., 2022). Second, managed care plans may face higher costs if they pay higher prices to providers. Each plan has a lower market power than the state's entire Medicaid program, possibly leading to higher prices. Moreover, plans may pay higher prices to broaden their network of providers either because they have to comply with network adequacy rules or simply to compete with other plans in the market. While this effect may be more pronounced if the

state's FFS payment rates were already very low before the transition, our analysis does not support this hypothesis. Third, administrative costs in multiple managed care plans may be higher than in the FFS system, contributing to overall increased costs. Additionally, disruptions in care during the initial period following the mandated transition to managed care could adversely affect individuals' health and lead to higher costs later on (Politzer, 2024).

While some factors contributing to higher costs may involve one-time adjustments after a transition (e.g., removal of FFS rationing), the gradual feeding of higher costs into the updated capitation rates over the initial years after the transition may lead to a dynamic increase in capitation spending. Other factors contributing to higher costs may have more permanent effects (e.g., plan competition), leading to a continuous increase in costs in the long run, extending beyond the scope of the period examined in this paper.

6 Conclusion

We exploit county-level enrollment mandates to examine the fiscal consequences of transitioning disabled beneficiaries in Medicaid from the fee-for-service program to managed care plans. We find that the outsourcing leads to a continuous increase in Medicaid fiscal spending over four years following the transition. We highlight the potential role of states' procurement rules and the rules for the endogenous updating of plans' capitation rates as potential mechanisms contributing to these cost increases. While decisions on rate updating are mostly left today in the hands of actuaries, further economic research is required to understand the effects of these decisions on the incentives that Medicaid managed care plans face to save costs and improve quality, as well as their impact on social welfare.

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A Appendix Tables

Appendix Table A1. Descriptive statistics for treatment and control counties in the baseline analytic sample, one year before each cohort's mandate

	Treatment	Control
Number of beneficiaries	406,217	1,168,071
Number of counties	980	1,305
Number of states	21	24
Total Medicaid spending ($PMPM)	1,344	1,347
Spending on capitated payments	39	51
Share of females (%)	50	51
Share under 21 years old (%)	30	29
Share 21 to 44 years old (%)	32	28
Share 45 to 64 years old (%)	38	43
Share of SSI eligibles (%)	84	77

Note: This table presents summary statistics for the baseline analytic sample of counties in the treatment and control groups, one year before the mandate in each cohort. The treatment counties are counties in which we identify a managed care enrollment mandate between 2004 and 2015, and the managed care penetration rate before the mandate doesn't exceed 10%. Control counties are counties in which managed care penetration never exceeds 10%, or counties with a mandate at least five years after the quarter of the cohort's mandate. This means that some counties appear both as treatment and controls. The sample includes only beneficiaries not in an MMC plan before the mandate in their cohort.

Appendix Table A2. The effects of a managed care enrollment mandate (reduced form) and enrollment in a managed care plan (IV) - including year-by-cohort fixed effects

Year post-mandate	(1) Total Spending	(2) FFS Spending	(3) Capitated Payments	(4) MC Penetration
Reduced form:				
Year 1	-35.62**	-311.62***	276.0***	0.40***
	(16.80)	(16.45)	(11.25)	(0.01)
Year 2	49.35***	-305.14***	354.49***	0.43***
	(18.93)	(19.84)	(14.94)	(0.01)
Year 3	70.90**	-328.86***	399.77***	0.45***
	(30.05)	(31.10)	(17.49)	(0.01)
Year 4	140.41***	-298.37***	438.78***	0.49***
	(42.77)	(41.17)	(18.48)	(0.01)
IV:				
Year 1	-89.57**	-783.56***	694.00***	
	(42.54)	(39.71)	(21.86)	
Year 2	113.95***	-704.52***	818.46***	
	(42.73)	(44.68)	(21.44)	
Year 3	156.46**	-725.65***	882.11***	
	(66.07)	(63.43)	(22.10)	
Year 4	285.02***	-605.67***	890.70***	
	(87.20)	(78.42)	(21.05)	
Baseline Mean	1,344	1,305	39	0.0
# of cohorts		32		
# of beneficiaries		2,343,468		
# of counties		1,432		

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

Note: Table shows the reduced form estimates of the impact of a managed care enrollment mandate on the examined outcomes in treatment counties, relative to control counties (top panel). It also presents the IV estimates of the effect of individuals' transition to a managed care plan, instrumented by enrollment mandated in their county (bottom panel). All specifications include also individual and year fixed effects, both interacted with cohort fixed effects. Robust standard errors, clustered at the county level, are shown in parenthesis. Column (1)-(3) show the effect on total/FFS/Capitated spending accordingly, all measured in dollars per beneficiary per month (PMPM). Column 4 presents our first stage - the effect of a mandate on the share of disabled beneficiaries in the county enrolled in a managed care plan (among beneficiaries not already enrolled in a managed care plan pre-mandate). Baseline mean values are calculated for the year before a mandate. For more details see Section 3

Appendix Table A3. The effect of MMC enrollment on total costs, 4 years post-mandate - IV estimates for alternative samples and specifications, including year-by-cohort fixed effects

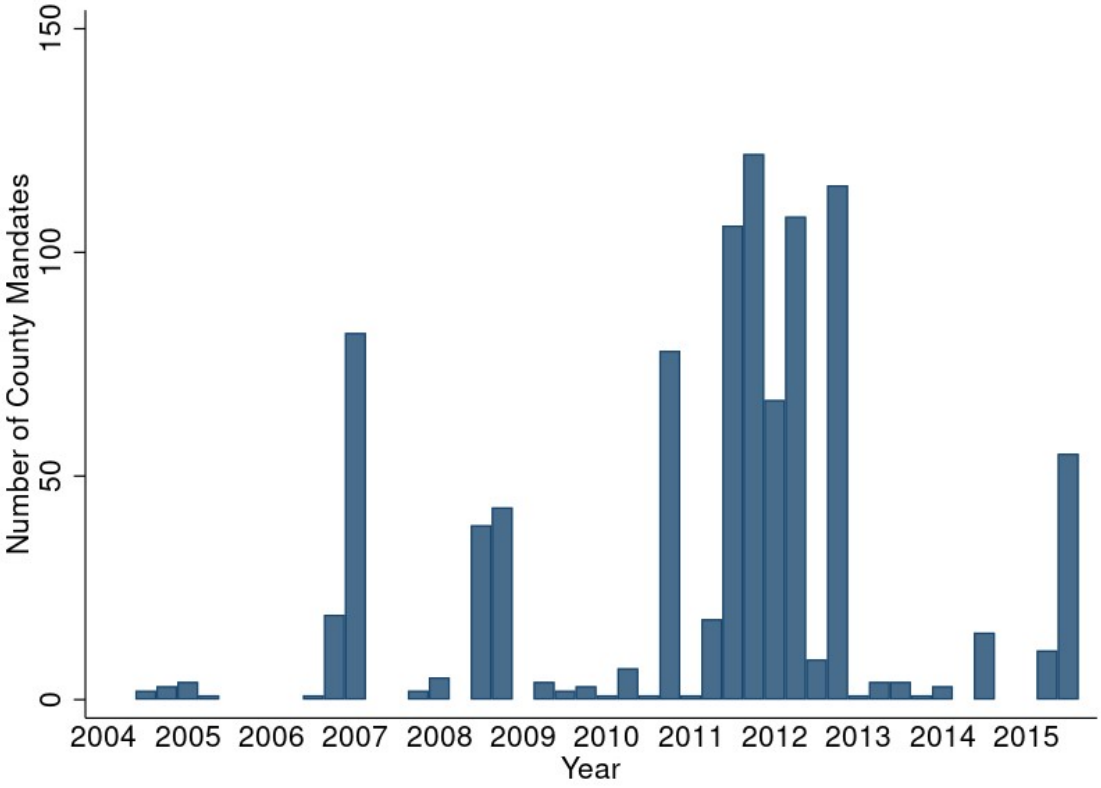
	(1) Full Sample	(2) Conservative Criteria	(3) Balanced Panel	(4) Never-Treated Controls	(5) County FEs	(6) Contiguous Counties
Year 4	196.48**	462.89***	620.59***	406.25***	551.77**	234.52**
	(79.49)	(98.09)	(101.00)	(66.71)	(82.81)	(98.88)
% of baseline	15.0	36.1	39.2	26.7	42.1	18.9
Baseline Mean	1,308	1,283	1,583	1,311	1,312	1,243
# of cohorts	34	31	5	32	32	62
# of beneficiaries	3,002,386	1,880,418	1,383,816	2,032,383	2,343,482	178,332
# of counties	1,663	1,276	896	1,431	1,432	148

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

Note: Table shows IV estimates of the impact of individuals' managed care enrollment on total Medicaid spending on them. Individual's MC enrollment is instrumented by MC enrollment mandates in the individual's county. Robust standard errors, clustered at the county level, are shown in parenthesis. Column (1) presents estimates from examining the full sample of all states. In column (2) we apply a "conservative" reliability criteria for states - a more strict criteria than the one used in our baseline specification and all other samples. Column (3) uses a sample that includes a balanced panel of treatment counties for which data is available four years before and after an enrollment mandate. Column (4) presents the estimates when using as controls only counties that never have an enrollment mandate during the sample period. Column (5) presents the results of an alternative specification in which county-level fixed effects are included instead of individual-level fixed effects. Lastly, column (6) presents the IV estimate from a specification that compares contiguous treatment and control counties.

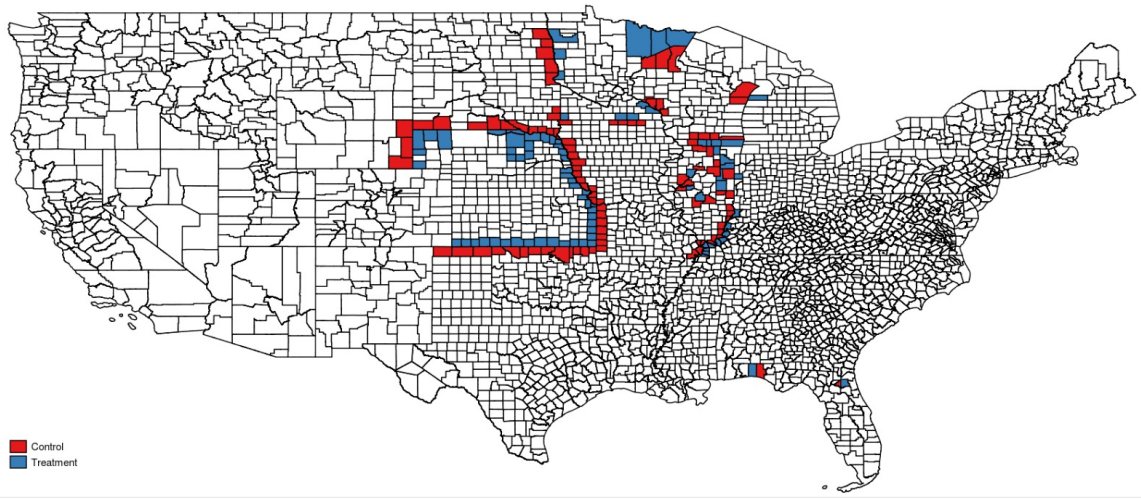
B Appendix Figures

Appendix Figure A1. The number of counties with a MMC mandate, by quarter of mandate



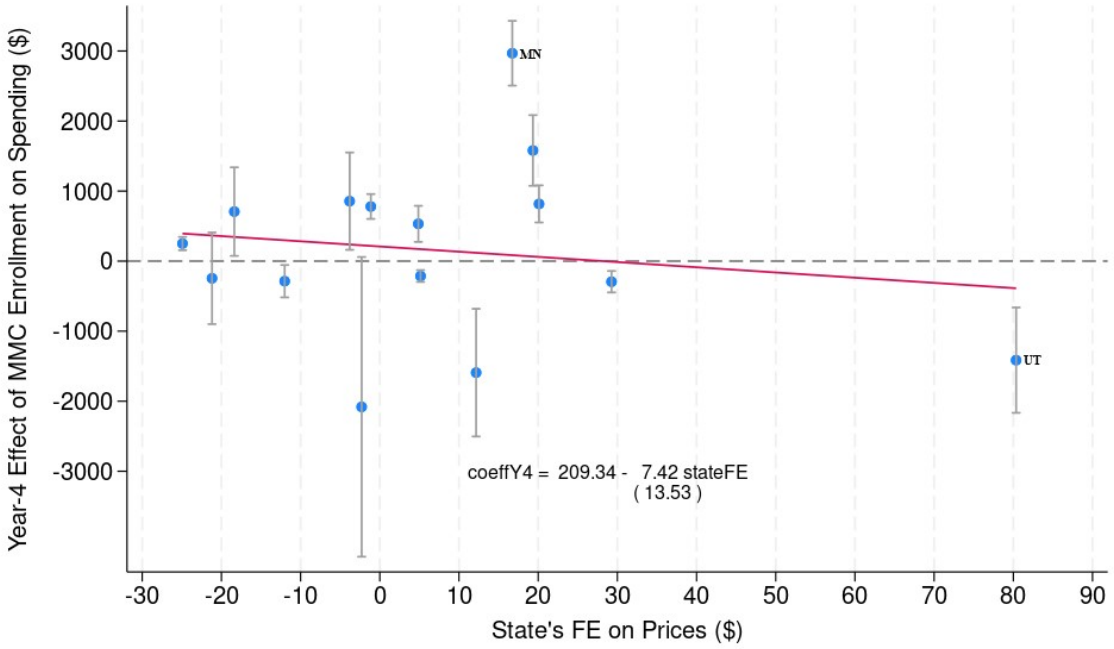
Note: Figure presents an histogram of the number of counties with a Medicaid Managed Care enrollment mandate at each quarter in our full sample, over 2004 to 2015.

Appendix Figure A2. Map of contiguous treatment and control counties



Note: Figure shows the contiguous treatment (blue) and control (red) counties.

Appendix Figure A3. States' pre-mandate fixed effect on Medicaid FFS prices and the effect of MMC enrollment on Medicaid spending, including outliers

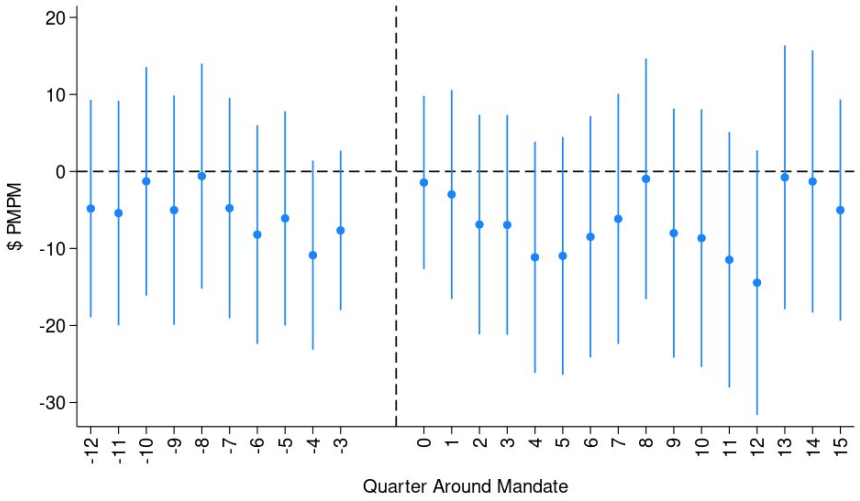


Note: For each state, the figure shows: a. IV estimate for the effect of enrollment in a managed care plan on total Medicaid spending during the fourth post-mandate year (y-axis), and b. The state's fixed effect on Medicaid FFS prices, measured using prices of outpatient services that appear in 2004 FFS claims of Medicaid disabled beneficiaries (x-axis). Red line is a linear trend of the points included in the scatter plot and the equation for this line is presented. Standard error for the coefficient of interest is shown in parentheses.

C Placebo test: Spending on Medicare's disabled

To ensure the validity of our IV estimates, it is essential that the Medicaid managed care enrollment mandates satisfy the exclusion restriction, implying that their impact on Medicaid spending is solely through their effect on enrollment in managed care plans. To assess this assumption, we conduct a placebo test, examining the effect of these Medicaid mandates on *Medicare's* spending on non-elderly disabled beneficiaries.[17] The quarterly event study graph in Figure A4 presents the results, demonstrating no difference in Medicare spending on the disabled between treatment and control counties after the implementation of Medicaid enrollment mandates. This null placebo result alleviates concerns regarding concurrent shocks in our treatment counties that could affect medical spending for the disabled post-mandates, thereby providing support for the assumption that the exclusion restriction holds for our instrument.

Appendix Figure A4. Placebo test: effect of Medicaid mandates on Medicare spending on disabled beneficiaries



Note: Figure shows an event study for *Medicare*'s spending on non-elderly disabled beneficiaries, who are not dually enrolled in Medicaid, around *Medicaid* managed care enrollment mandates. Y-axis shows the average monthly per-member-per-month (PMPM) Medicare spending in dollars. X-axis shows the number of quarters before or after a Medicaid managed care mandate.

[17] To prevent sample overlap, we focus solely on beneficiaries who enrolled in Medicare before the start of our Medicaid sample period (prior to 2004), resulting in a Medicare sample of 297,198 disabled beneficiaries.