The Fiscal Cost of Providing Medicaid to Disabled Beneficiaries Through Private Managed Care Plans*

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Abstract

Medicaid spends 40% of its total spending on disabled beneficiaries, a sum that amounts to 6% of the U.S. national health expenditure. Over the last two decades, states have shifted the provision of Medicaid to the disabled from their public fee-for-service system to private managed care plans. To study such transitions, we use an administrative database to identify county-level mandates that lead to a sharp increase in managed care enrollment. We exploit these mandates as an instrument for individuals' enrollment in managed care plans. We find that a transition to managed care eventually increases Medicaid's fiscal spending. Although spending mostly doesn't change at the first year after the transition, it increases by 0.5% to 30% of the baseline mean in the years after that, compared to the public program. Our results suggest that spending tends to increase more in states that have lower pre-mandate payment rates to providers.

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1 Introduction

Disabled beneficiaries make up 14% of all Medicaid enrollment, but spending on the disabled constitutes 40% of Medicaid's total spending - almost $187 billion in 2014, or about 6% of the U.S. national health expenditure (Kaiser Family Foundation (2014a,b)). Medicaid was established as a public fee-for-service (FFS) insurance, but nowadays about 70% of its beneficiaries receive their care through private managed care plans, publicly-financed by capitated payments from the Medicaid program (Hinton et al. (2019)). While most states shifted children and non-disabled adults to private plans long ago, the transition of disabled beneficiaries from the public FFS system to private plans is more recent, or ongoing. A major motivation of states for making such a transition is often the belief that it will reduce the fiscal spending on Medicaid - a belief based on studies by states' Medicaid agencies and their consulting firms (Lewin Group (2009)). In this paper we provide evidence that this belief is false - we find that shifting disabled beneficiaries to private managed care plans eventually *increases* Medicaid's fiscal costs. The impact is dynamic - spending barely changes during the first year after a mandate-induced transition to managed care, but in later years increases - by 16% over the baseline mean in our main specification.

Using national data on Medicaid enrollees between 2004 and 2015, we exploit county-level enrollment mandates that swiftly shift large shares of disabled beneficiaries into private managed care plans. Within a difference-in-differences framework (DID), we compare counties that roll out a mandate to counties that remain in the public FFS system. In using enrollment mandates we follow previous papers that examine transitions to managed care in Medicaid. Duggan and Hayford (2013) conduct their analysis at the state-level, using the share of population living in a county with a mandate as an instrument for the state's penetration rate of Medicaid managed care plans. They examine mandates rolled out mostly in the 90's for the general Medicaid population and find that private plans don't reduce Medicaid's fiscal spending on average. Our empirical approach is similar to Layton et al. (2019), that examine transitions of disabled beneficiaries from FFS to Medicaid managed care plans in mid-2000 Texas. They exploit staggered introduction of county-level managed care enrollment mandates, and find that such transitions increase Medicaid fiscal spending by 11.7% relative to control counties that remain in the public system. However, while Layton et al. (2019) focus on a single state, we examine national data on all the transitions of disabled beneficiaries to managed care during our twelve-years sample period.

Our analysis uses monthly individual-level data on Medicaid enrollment and costs from an administrative database - the Medicaid Analytic eXtract (MAX). The data is described in section 2. Section 3 presents our empirical approach. We use the MAX data to calculate the penetration rate of comprehensive managed care plans among disabled beneficiaries in each county over time. We identify enrollment mandates, in the data, when the penetration rate increases sharply and swiftly - at least by 20 percentage points over at most 3 months. Our baseline analytic sample includes a treatment group of beneficiaries in 936 counties with an enrollment mandates - counties that had no significant MC penetration before the mandate. The control group includes beneficiaries in 723 counties that remain in FFS throughout our sample period. We control for individual and quarter fixed effects in all our specifications and use event studies to show that our treatment and control groups share the same spending trends during the three years before a mandate is rolled out.

Our results are presented in section 4. We find that the average county-level mandate eventually increases the share of disabled beneficiaries that are enrolled in managed care plans by 63 percentage points. At the first year after a mandate, the $387 increase in per-member-per-month (PMPM) capitated payments to the private plans is offset by a decrease in direct FFS payments to providers. In total, there is little change in Medicaid fiscal spending. After the first year, the total spending in treatment counties increases by $98 (PMPM) relative to control counties - a 8% increase over the baseline mean. This increase is driven solely by higher capitated payments to the managed care plans, while spending on FFS payments to providers continues to decrease.[1] Using the mandates as an instrument for individuals' enrollment in managed care plans we find a similar spending dynamics - there is little change in total Medicaid spending at the first year after a transition from FFS to managed care, but spending increases after that by $194 - 16% of the baseline mean.

We test the robustness of our results to alternative analytic samples. First, we examine the reliability of the MAX data in each state by comparing the aggregate spending in the MAX database to the verified spending amounts that states report annually to the federal government in CMS-64 forms. Our results are robust to using only a subsample of state-years with reliable data. Second, we repeat the estimation on a sample that includes a balanced panel of treatment counties, and find similar results. In addition to that, we test a different specification, comparing contiguous

[1] This is in contrast to the result in Layton et al. (2019), who find that the private plans in Texas increase the utilization of carved out services, that remain in the FFS system.

treatment and control counties. Lastly, we use a Stacked DID approach. First, we create a stacked dataset in which only counties that are never treated serve as controls for each treated county, avoiding a possible bias when treatment effect changes over time and already-treated counties implicitly serve as controls (Goodman-Bacon (2021)). Second, we examine a specification in which only later-treated counties serve as controls (following Deshpande and Li (2019), Fadlon and Nielsen (2021) and others). Our main results remain unchanged in all these specifications and they strongly suggest that transition from the public FFS system to private managed care plans does not save money to the Medicaid program - spending eventually increases within a range of 0.5% to 30% of the baseline mean. 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. Finding no effect supports our empirical approach and the assumption that no other concurrent shocks in our treatment counties led to the increase of medical spending for the disabled after enrollment mandates.

Since Medicaid programs are different in each state, we examine possible heterogeneity in our results in section 5. We find that after the first year of an enrollment mandate, Medicaid spending weakly increases in all our treatment states except Louisiana. We show that states with lower pre-mandate FFS payment rates, tend to have a higher spending increase when beneficiaries are shifted to managed care.[2] This suggests that private plans find it harder to decrease payments to providers when the FFS rates are already very low to begin with. Duggan and Hayford (2013) find a similar result - states with higher FFS prices (closer to the rates of commercial insurers), decrease their Medicaid fiscal spending after a transition to managed care. While Duggan and Hayford (2013) rely on the price gap for a single service - newborn delivery - our price index uses claims data on *all* outpatient services to Medicaid's disabled beneficiaries.

In section 6 we discuss possible mechanisms behind our results. Beyond higher payment rates for managed care plans (especially in states with already low rates), costs could also be higher for MMC plans if disabled beneficiaries are under-served in the FFS system. Drug caps, that limit the number of prescriptions a beneficiary can fill each month, are an example for an explicit limit on utilization within the public FFS system - a limit relaxed under managed care plans (Layton et al. (2019)).

[2]To measure states' FFS payment rates for providers, we estimate each state's fixed effects on payments for outpatient services to disabled beneficiaries.

Higher costs for private plans affect Medicaid fiscal spending due to the way their capitation payments is adjusted. CMS' rate development guidelines require states to have actuarially fair capitation rates, updated annually and based on the experience of the Medicaid population in the recent three years. Thus, past increases in plans' costs would lead to higher payments from the Medicaid program. This may reduce the saving incentive for the plans.

Our paper contributes to the literature that assesses the impacts of managed care, especially in Medicaid, and adds to the few papers that focus on the disabled population within this program. Our results are in line with the economic literature in finding no fiscal savings from Medicaid's transition to private managed care plans, and is in contrast with policy makers' believes that such savings will occur. Private provision of publicly-financed insurance is becoming more and more popular in both Medicaid and Medicare (Gruber (2017)), and is common in many developed countries (McGuire and van Kleef (2018)). Our paper highlights that the impact of private provision on spending depends on both the public system it replaces and the procurement rules of the private plans.

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. Enrollment information on Medicaid beneficiaries is taken from the MAX Personal Summary files (PS), that contain person-month enrollment status. For individuals enrolled in Medicaid, these files hold data on demographic characteristics, the basis for Medicaid eligibility, and whether the individual is enrolled in a comprehensive managed care plan. Data on Medicaid's fiscal spending for each beneficiary is included in the MAX Inpatient (IP), Other Therapy (OT), and Prescription Drug (RX) 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. 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[3]

To assess the reliability of the MAX data in each state, we compare the state's

[3]The data register the beneficiary's county of residence only at the end of the year. Thus, we can identify and exclude movers between counties only if they appear in different counties in separate years.

aggregate Medicaid spending in 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.[4] As robustness tests, we repeat our estimates for subsamples that only include data from reliable state-years, using a both a liberal and a more conservative definition of reliability.

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

Our empirical approach exploits county-level enrollment mandates, that swiftly push a large share of disabled beneficiaries into private managed care plans. We estimate the effect of a transition to managed care on Medicaid's fiscal spending within a difference-in-differences framework, comparing the treatment counties, in which we identify a mandate, to control counties that remain in the public FFS system. As mandates are not randomly assigned, differential trends in the outcomes between our treatment and control counties may pose a challenge to our identification strategy. To address this challenge we, first, control for individual fixed effects in all our specifications. This allows us to account for time invariant differences between our treatment and control counties.[5] This means that within-beneficiary changes around transitions to MMC are the source of our identification. Second, we run event studies to verify that treatment and control counties share similar trends in the fiscal spending on Medicaid before managed care enrollment mandates are rolled out. Lack of differential pre-trends would support the assumption that the outcomes in the control counties can serve

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

[5] As explained in section 2, all movers are dropped from our sample. This means that individual fixed effects account also for time invariant characteristics of their county and state of residence.

as good counterfactuals to the post-mandate outcomes in treatment counties had the treatment never occurred. Third, we run a placebo test to verify that Medicaid mandates have no effect on *Medicare's* spending for its disabled beneficiaries in our treatment counties. A lack of effect in this placebo test would reduce the concern that some other concurrent shocks affect the spending on disabled in our treatment counties after a managed care enrollment mandate.

Including individual fixed-effects helps us also to address a possible threat to identification if private managed care plans affect who becomes or remains a Medicaid beneficiary. Currie and Fahr (2005) present some evidence that Medicaid managed care plans change the composition of children enrolled in Medicaid, lowering the enrollment rates of young and black children. However, it is not clear whether managed care plans can have such an effect on the composition of disabled Medicaid beneficiaries, as the eligibility of most of them is based on their Supplemental Security Income (SSI) status, which is determined by the Social Security Administration. The inclusion of individual fixed-effects in our specifications means that our estimates measure the within-beneficiary effect of managed care on Medicaid spending, and not the overall effect, that may include the impact of composition changes.

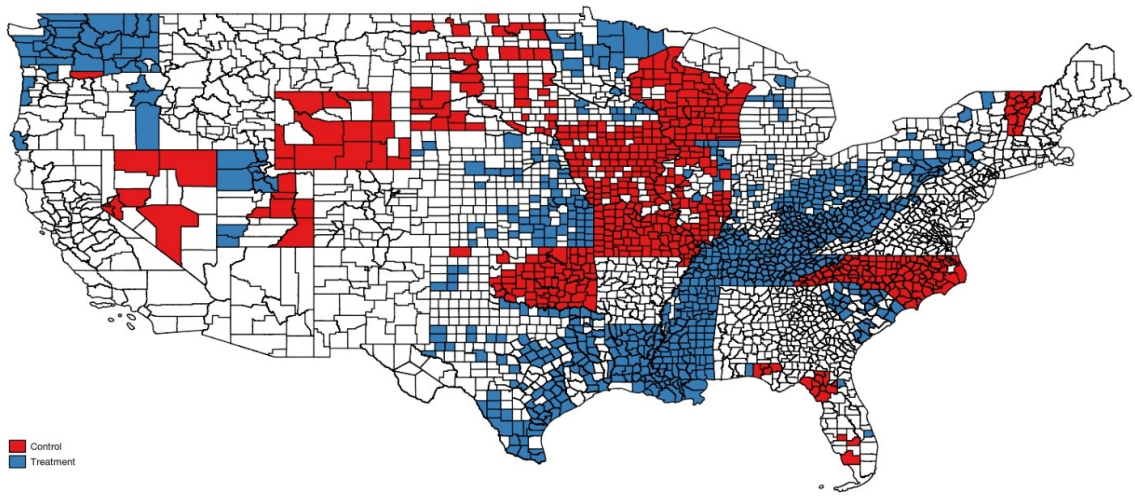
After examining the reduced form effect of managed care mandates on spending, we use mandates as an instrumental variable (IV) for individuals' enrollment in managed care. This allows us to account for the different take-up of managed care in each of the treatment counties after the mandate. Using the mandates IV we also address possible selection of Medicaid beneficiaries into managed care plans. Such selection of healthier beneficiaries into managed care could bias OLS estimates that simply compare costs in managed care and in the public FFS program. However, since all our treatment counties, by construction, experience a managed care enrollment mandate, this possible bias is less relevant in our data.

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. 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 at the first or last six months of our sample. Counties in which the managed care penetration rate never exceeds 10 percents, i.e. counties

that rely on the FFS public system throughout our sample period, are used as control counties. Our baseline sample includes only treatment counties in which managed care penetration was below 10 percents before the mandate. Figure 1 presents the map of the identified treatment (blue) and control (red) counties in our baseline sample. Most of the mandates in our sample occur in 2011 to 2013 (Figure A1 in the appendix presents the histogram of mandates by quarter throughout our sample period).

Figure 1. Treatment and Control Counties in the Baseline Analytic Sample



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

Table 1 presents summary statistics for the treatment and the control counties in our baseline sample. This sample includes 936 treatment counties in 20 states, and 723 control counties. The gender and age mix of disabled beneficiaries is almost identical in the average treatment and control county. However, treatment counties have more beneficiaries in them - there are 947 beneficiaries in the average treatment county vs. 518 in the average control county. Treatment counties also have a larger share of disabled beneficiaries eligible for Supplemental Security Income (SSI). Moreover, the total monthly Medicaid spending per beneficiary is higher in control counties, especially due to higher FFS spending on "other" services (mainly outpatient services). Some of these differences are most likely related to the higher prevalence of managed care mandates in urban areas.

3.2 Event studies around enrollment mandates

To study the spending trends in our treatment and control groups we run event studies that examine three years before a mandate and four years after it occurs. The reduced

Table 1. Descriptive statistics for treatment and control counties in the baseline analytic sample in 2004 (pre-mandate)

	Treatment	Control
Number of beneficiaries	885,927	374,174
Number of counties	936	723
Number of states	20	15
Total Medicaid spending ($PMPM)	1,057	1,197
Total FFS spending	1,037	1,171
In-patient FFS spending	243	265
Long-term care FFS spending	119	144
Other FFS spending	459	541
Drugs FFS spending	216	221
Spending on capitated payments	20	25
Share of beneficiary-months in managed care (%)	1.3	0.9
Share of females (%)	51	51
Share under 21 years old (%)	26	25
Share 21 to 44 years old (%)	31	31
Share 45 to 64 years old (%)	43	44
Share of SSI eligibles (%)	76	66

Note: This table presents summary statistics for the baseline analytic sample of counties in the treatment and control groups in 2004. 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%. The statistics shown use data on treatment counties before any mandate occurred.

form analysis is performed at the individual level and at a quarterly duration. We control for individual fixed effects in all our specifications. As all movers are dropped from the sample, these fixed effects control for all individual, county and state time-invariant characteristics. As mandates occur at different times throughout our sample period, and all control counties serve as control for all treatment counties, we include in the specification quarter fixed effects, that control for quarter-specific common shocks that affect similarly both treatment and control counties. As mandates are rolled out at the county-level, we cluster our standard errors at this level. The

regression specification is the following:

$$
\begin{aligned}
Y_{ict} &= \alpha_0 + \sum_{j=-12}^{15} \beta_j I_{jct} + \gamma_i + \delta_t + \epsilon_{it} \\
s.t.\ I_{jct} &= \begin{cases} 1(Quarter_t - MandateQuarter_c = j) & \text{if } Treat_c = 1 \\ 0 & \text{if } Treat_c = 0 \end{cases}
\end{aligned}
\tag{1}
$$

where Y_{ict} is the examined outcome for individual i in county c at quarter t. I_{jct} are indicator dummies, that equal 1 if quarter t is j quarters after a mandate is rolled out at a treatment county c (i.e. $Treat_c$ equals 1). For control counties, I_{jct} equals 0 for all dummies. γ_i is the individual fixed effect, δ_t is the quarter fixed effects, and ϵ_{it} represents a random error term. We examine four outcome variables: managed care penetration in the county, FFS spending, spending on capitated payments, and the total Medicaid fiscal spending.

3.3 Mandates as IV for enrollment in a managed care plan

As the effect of a mandate on the penetration rate of managed care varies between our treatment counties, we present instrumental variable (IV) estimates on top of reduced form estimates. The results of the event studies guide our choice for the reduced-form and IV specification. Trying to examine a possible dynamic response of the spending, we differentiate between the first year after the mandate is rolled out and the years after that. Our reduced form specification is a difference-in-differences specification:

$$
Y_{ict} = \beta_0 + \beta_1 PostY1_{ct} + \beta_2 PostY2On_{ct} + \gamma_i + \delta_t + \epsilon_{it} \tag{2}
$$

where Y_{ict} is the outcome of interest for individual i, in county c, at quarter t. $PostY1_{ct}$ is an indicator equal to one if quarter t occurs at the first year after a mandate in treatment county c and zero otherwise. $PostY2On_{ct}$ is an indicator equal to one if quarter t is later than the first year after a mandate in treatment county c and zero otherwise. γ_i is the individual fixed effect and δ_t is the quarter fixed effect. ϵ_{it} represents a random error term.

The IV specification uses the county-level mandates as an instrument for individuals' enrollment in a managed care plan. The IV estimates are local average treatment effects (LATE) for the population of disabled beneficiaries that transition from FFS

to managed care due to the enrollment mandate in the county (i.e. "compliers"). The first stage regression is:

$$InMMC_{ict} = \beta_0 + \beta_1 Post_{ct} + \gamma_i + \delta_t + \epsilon_{it} \tag{3}$$

where $InMMC_{ict}$ indicates whether individual i in county c was enrolled in a managed care plan during quarter t. $Post_{ct}$ is an indicator equal to 1 if quarter t occurs after a mandate was rolled out in a treatment county c and zero otherwise. γ_i is the individual fixed effect and δ_t is the quarter fixed effect. ϵ_{it} represents a random error term. The IV regression specification is:

$$Y_{ict} = \theta_0 + \theta_1 In\widehat{MMC}_{ct} + \gamma_i + \delta_t + \psi_{it} \tag{4}$$

where $In\widehat{MMC}_{ct}$ is the predicted value from equation 3 and ψ_{it} is a random error. θ_1 is the LATE - for beneficiaries that were shifted from the public FFS to managed care plans due to our identified mandates (i.e. "compliers"), it represents their average difference in the outcome Y_{ict} between managed care plans and the public FFS. To examine possible dynamic responses when using the IV, we repeat the IV estimation with two subsamples - one in which we drop all observations in treatment counties that are from quarters later than the first year after the mandate, and another subsample in which we drop all observations from treatment counties during the first year after a mandate.

3.4 Contiguous treatment and control counties

To make the treatment and control groups more comparable, we also analyze contiguous treatment and control counties. This 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. In all specifications, we cluster the errors at the cohort level. Figure A2 in the appendix presents a map of the (blue) treatment counties and (red) control counties. The stacked dataset is used to examine event studies around the mandates. For event studies, we use a specification similar to the one described in equation 1, but change the value of the indicator function I_{jct} in control counties to be equal to the value of the function in the cohort's treatment county at each quarter, so timing relative to

the mandate is defined and uniform for all counties in the cohort. We also add, in all specification, a fixed effect for each interaction of cohort and quarter, allowing for cohort-specific time trends. The reduced form specification is now:

$$Y_{icht} = \beta_0 + \beta_1 PostY1_{ht} \times Treat_c + \beta_2 PostY2On_{ht} \times Treat_c + \gamma_i + \delta_t + \theta_h \times \delta_t + \epsilon_{it} \quad (5)$$

where $PostY1_{ht}$ is an indicator equal to one if in cohort h, to which individual's i's county c belong, quarter t is in the first year after the mandate in the cohort's treatment county. $PostY2On_{ht}$ is an indicator equal to one if quarter t is more than a year after the mandate in cohort h's treatment county. $Treat_c$ is an indicator equals to one if county c is a treatment county (i.e. a county with a mandate), and equals to zero otherwise. Using the mandates as an instrument for enrollment in a managed care plan, the first stage is:

$$InMMC_{ict} = \beta_0 + \beta_1 Post_{ct} \times Treat_c + \gamma_i + \delta_t + \theta_h \times \delta_t + \epsilon_{it} \quad (6)$$

and the IV regression specification remains unchanged from equation 4.

3.5 Stacked DID with limited control groups

As additional robustness tests we estimate the fiscal effect of Medicaid Managed Care in a stacked difference-in-differences framework. Following this approach, used by Deshpande and Li (2019), Fadlon and Nielsen (2021), and others, we create a separate dataset for each cohort of treated counties, that includes the treated counties that have a mandate at a certain quarter, and all the control counties. All the counties in the dataset are assigned the same timing variables relative to the quarter of the mandate in the cohort's treatment counties. These cohort-by-cohort data sets are then stacked together to create the analytic sample. We examine two groups of control counties. First, we use counties that have a MMC mandate in the future, at least three years after the treated cohort's mandate. This option may make the group of control counties more similar to the treatment group, further supporting the assumption of parallel trends required for the identification. With later-treated controls, results are identified off the timing of mandates, rather than their occurrence. Second, we use a control group that only includes counties that are never treated, i.e. have no mandate over the whole sample period. This specification shuts down a possible bias when using two-way fixed effects DID methods. As Goodman-Bacon (2021) demonstrates, these methods implicitly use already-treated counties as con-

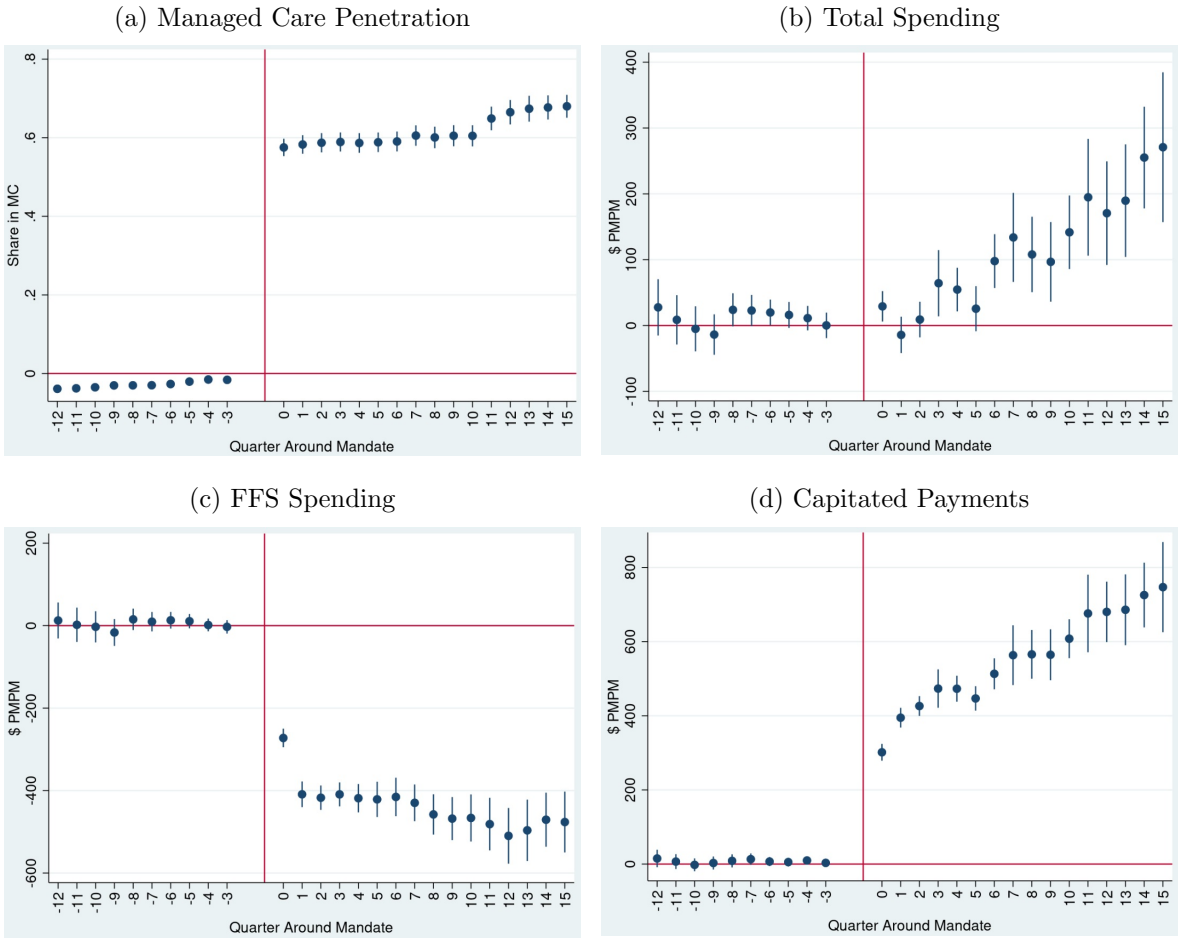
trols, leading to a biased estimate when treatment effects change over time. The estimated equations are identical to the specification in the contiguous counties case, in which we also create a stacked dataset of cohorts (Equations 5 and 6). All estimations include individual and quarter fixed effects, as well as a fixed effect for each interaction of cohort and quarter, allowing for separate time trends for each cohort of treated counties.

4 Results

4.1 Event studies around mandates

Figure 2 presents event studies examining the difference in outcomes between our treatment and control groups around a managed care enrollment mandate. For all the examined outcomes, the event studies show no significant differential trends between treatment and control counties in the three years before a mandate is rolled out. After a mandate, panel A shows an increase of 50 to 60 percentage points in the penetration rate of managed care plans in treatment counties relative to control counties. Panel B examines the changes in total Medicaid spending around a mandate. The total spending in treatment counties, after a mandate, shows little consistent change during most of the first year after a mandate, but then starts to increase. Spending rises modestly at first, but increases more and more as time goes by. This result shows no support to the claim that managed care mandates save money to the Medicaid program, and it indicates that mandates lead to dynamics of increasing spending. The differences in total spending are broken down to differences in Medicaid FFS spending (panel C) and differences in capitated payments (panel D). As expected, a mandate that shifts a large share of enrollees from the public FFS system to managed care plans decreases the FFS spending and increases the amount of capitated payments.

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 two quarters before the mandate (quarters -1 and -2). Quarter zero is the first quarter in which the mandate is in place. The sample includes only treatment counties with low pre-mandate MC penetration. 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

Table 2 presents the reduced form estimates of the effects of a managed care enrollment mandate. After a mandate, the share of disabled beneficiaries enrolled in managed care increases by 60 percentage points in treatment counties (column 4). At the first year after a mandate there is no significant change in the total fiscal spending

of the Medicaid program. However, in the period after the first year, the monthly spending per beneficiary increases in treatment counties by $98 relative to control counties (Column 1). This is a 8 percent increase over the pre-mandate mean spending in the treatment counties. At the first mandate year, the monthly FFS spending per beneficiary decreases in these counties by $375 (column 2), while spending on capitated payment rises by a similar amount of $387 (column 3). FFS spending continues to decrease in later years, by additional $66, but this decrease in spending is overwhelmed by a $151 increase in capitated payments to the managed care plans.

Table 2. The effects of a managed care enrollment mandate (reduced form)

	(1) Total Spending	(2) FFS Spending	(3) Capitated Payments	(4) MC Penetration
First year after a mandate	12.00	-374.96***	386.96***	0.60***
	(12.15)	(14.38)	(11.12)	(0.01)
After the First year	97.63***	-440.57***	538.20***	0.63***
	(22.64)	(26.11)	(23.97)	(0.01)
Baseline Mean	1,227	1,166	61	0.04
# of beneficiary-quarter obs.		38,887,578		
# of beneficiaries		3,034,342		
# of counties		1,663		

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

Note: Table shows estimates of the impact of a managed care enrollment mandate on the examined outcomes in treatment counties, relative to control counties. All specification include also a constant, individual fixed effects and quarter fixed effects. Standard errors are clustered at the county level. Column (1)-(3) show the effect on total/FFS/Capitated spending, accordingly, all measured in dollars per beneficiary per month (PMPM). Column 4 presents the effect of a mandate on the share of disabled beneficiaries enrolled in a managed care plan. Baseline mean values are calculated for the quarter before a mandate. All coefficients are from estimating Equation (2). For more details see Section 3

Table 3 presents the IV estimates of the effect of individuals' enrollment in a managed care plan on Medicaid's monthly spending for them. Enrollment is instrumented using county-level managed care enrollment mandates. Columns (2) and (3) present the estimates for the first year after a mandate and for the years after that, accordingly. The IV estimates confirm the dynamics observed in the reduced form estimation - change in Medicaid spending is insignificant at the first year after an enrollment in a managed care plan (due to a mandate). In later years, the monthly Medicaid spending per beneficiary increases by $194 relative to beneficiaries that re-

Table 3. The effect of enrollment in a managed care plan on total spending (IV)

	(1) All Post-Mandate Years	(2) First Year	(3) After the First Year
In a MMC Plan	86.28***	-2.18	194.40***
	(26.25)	(21.49)	(37.60)
Baseline Mean	1,227	1,227	1,227
# of beneficiary-quarter obs.	38,887,578	31,013,833	35,308,346
# of beneficiaries	3,034,342	2,661,490	2,977,712
# of counties	1,663	1,663	1,663

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

Note: Table shows instrumental variable estimates of the impact of individuals' managed care enrollment on the Medicaid spending on them. Individual's MC enrollment is instrumented by MC enrollment mandates in the individual's county. All specification include also a constant, individual fixed effects and quarter fixed effects. Standard errors are clustered at the county level. Column (1) is estimated on the baseline sample. Column (2) is estimated on a sample that drops all post-mandate observations after the first year. Column (3) show estimates for a sample that drops all observations from the first year after a mandate. These coefficients are from estimating Equation (4). For more details see Section 3

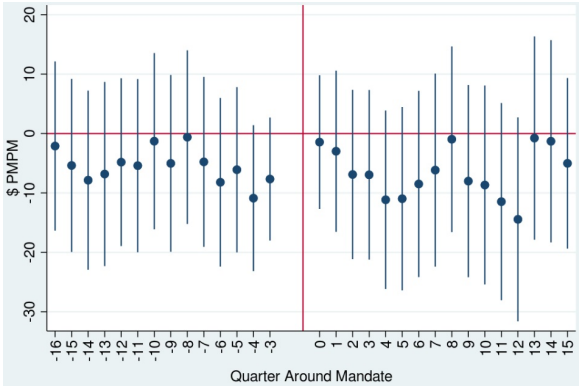
main in FFS (in control counties) - a 16% increase over the baseline spending in treatment counties.

4.3 Placebo test: Spending on Medicare's disabled

For our IV estimates to be valid, the mandate instrument should satisfy the exclusion restriction, i.e. Medicaid managed care enrollment mandates should affect Medicaid spending only through their effect on enrollment in managed care plans. To support the assumption that this requirement is satisfied, we run a placebo test in which we examine the effect of the Medicaid mandates on *Medicare*'s spending on non-elderly disabled beneficiaries.[6] The event study graph in figure 3 shows no difference in Medicare's spending on the disabled between treatment and control counties after Medicaid enrollment mandates. This result reduces the concern that other concurrent shocks in our treatment counties affect medical spending for the disabled after enrollment mandates, and it provides support for the assumption that the exclusion restriction holds for the instrument.

[6]To prevent overlapping, we examine only beneficiaries that joined Medicare before the beginning of our Medicaid sample period, i.e. before 2004. The Medicare sample includes 297,198 disabled beneficiaries.

Figure 3. Placebo test: effect of Medicaid mandates on Medicare spending on disabled



Note: Figure shows an event study for *Medicare*'s spending on disabled beneficiaries around *Medicaid* managed care enrollment mandates. Y-axis shows the monthly PMPM spending in dollars. X-axis shows the number of quarters before or after a mandate.

4.4 Robustness I: Alternative samples

In this section we explore the robustness of our estimates to restricting the analytic sample to state-years with more reliable MAX data. We measure reliability by the difference between aggregate Medicaid spending in the MAX database and the aggregate spending reported in the state's CMS-64 reports. We use a liberal criteria of reliability and a conservative (more restrictive) criteria and repeat our estimation for the two resulting subsamples. Columns (1) and (2) in Table 4 present the IV estimates when these county selection criteria are applied. Column (3) presents the results of an estimation on a third sample that includes a balanced panel of treatment counties, for which reliable data (under the "liberal" definition) is available for the three years before and after the enrollment mandate. In all alternative estimations, like in our baseline results, enrolling in a MMC plan (due to a mandate) has no significant effect on Medicaid's spending during the first year, but leads to a 13% to 21% increase in spending over the years after that.

4.5 Robustness II: Contiguous treatment and control counties

Table 5 presents IV estimations for contiguous treatment and control counties (Table A1 in the Appendix presents the reduced form estimates). Similar to the baseline estimation, monthly spending for beneficiaries that enroll in MMC (due to a mandate) is higher after the first year of the enrollment mandate - by $457 (30% of the baseline mean). During the first year, spending seems to decrease, by $90, but this decrease is not statistically different than zero.

Table 4. IV estimates for alternative samples

	(1) Liberal Criteria		(2) Conservative Criteria		(3) Balanced Panel (3Y)	
	First Year	After the First Year	First Year	After the First Year	First Year	After the First Year
In MMC	-15.86	192.33***	6.72	258.68***	-6.66	156.80***
	(16.17)	(32.26)	(18.79)	(34.31)	(16.09)	(29.70)
Baseline Mean	1,211	1,211	1,207	1,207	1,211	1,211
# of beneficiaries	1,979,742	2,243,216	1,588,167	1,787,917	1,841,917	2,022,826
# of counties	1,414	1,420	1,245	1,251	1,149	1,155

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 the Medicaid spending on them. Individual's MC enrollment is instrumented by MC enrollment mandates in the individual's county. All specification include also a constant, individual fixed effects and quarter fixed effects. Standard errors are clustered at the county level. Column (1) and Column (2) apply a "liberal" and "conservative" reliability criteria, accordingly. Column (3) uses a sample restricted by the "liberal" reliability criteria, that includes a balanced panel of treatment counties for which data is available 3 years before and after an enrollment mandate.

Table 5. IV estimates for the contiguous counties sample

	(1) First Year	(2) After the First Year
In a MMC Plan	-90.04	456.91***
	(87.95)	(89.52)
Baseline Mean	1,503	1,503
# of beneficiaries	406,446	434,182
# of counties	239	239

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 Medicaid spending. Individual's MC enrollment is instrumented by MC enrollment mandates in the individual's county. All specification include also a constant, individual fixed effects, quarter fixed effects, and fixed effects for cohort-quarter interactions. Standard errors are clustered at the county level.

4.6 Robustness III: Stacked Difference-In-Differences

Table 6 presents the results of the IV estimations when using the stacked DID approach. All counties in the sample come from states with reliable MAX data (using the "Liberal" definition for reliability), and all treatment counties have low pre-mandate penetration rate of managed care. In column (1) we estimate the effect of an MMC

enrollment mandate when the control group is restricted to counties that during our sample period never experience an MMC enrollment mandate. Similarly to our main results, there is little change in Medicaid's spending in the first year after the mandate, but spending increases significantly in later years - here, by $172 PMPM (14% increase relative to the mean in the baseline period). Column (2) presents the estimation's result when the control group for each treated county comprises counties that have a mandate later on, at least three years in the future. The estimates show a decrease of $109 in spending at the first year after a mandate - 9% relative to the baseline mean. However, this decrease doesn't hold in later years. After the first year, there is a $6 increase in spending (0.5%), though this increase is statistically insignificant. This is the only specification we use in which the increase in spending after the first year is so small and not different than zero in a statistically significant way. Even in this specification there is no evidence that a transition to MMC reduces Medicaid's spending beyond the first year.

Table 6. IV estimates for stacked DID estimations

	(1) Never-Treated Controls		(2) Later-Treated Controls	
	First Year	After the First Year	First Year	After the First Year
In a MMC Plan	-7.99	172.72***	-109.15***	5.81
	(18.84)	(33.59)	(17.27)	(26.54)
Baseline Mean	1,211	1,211	1,223	1,223
# of beneficiaries	1,979,742	2,243,216	1,593,221	1,819,425
# of counties	1,414	1,420	885	891

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

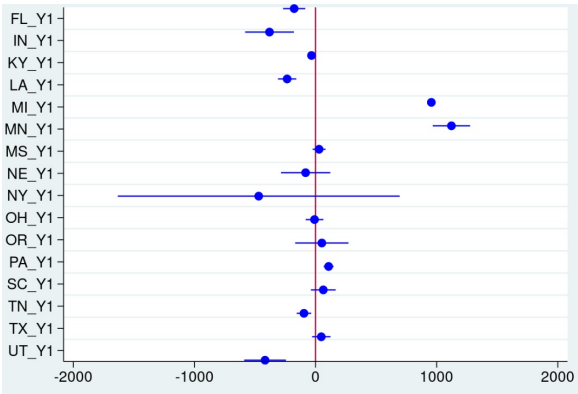
Note: Table shows IV estimates of the impact of individuals' managed care enrollment on Medicaid's spending, using county enrollment mandates as an instrument. The estimation dataset is constructed by stacking a separate dataset for each treatment cohort and its controls. Column (1) presents estimations that uses only control counties that never have a mandate. Column (2) uses as control only counties that have a mandate in the future, at least 4 years after the treatment cohort's mandate quarter. The sample includes only counties from states that have reliable MAX data (using our "liberal" reliability definition), and only treatment and later-control counties in which the managed care penetration was low before the mandate. All specification include also a constant, individual fixed effects, quarter fixed effects, and cohort-quarter interaction fixed effects. Standard errors are clustered at the county level.

5 Heterogeneity Between States

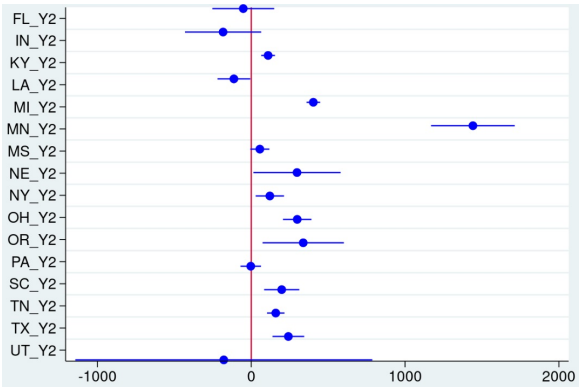
All previous results have estimated the *average* effect of managed care mandates and managed care enrollment on Medicaid spending. However, since the Medicaid program differs from state to state, we turn now to examine heterogeneity in the effect of managed care on spending. Figure 4 presents the IV estimates of the effect of enrollment in a managed care plan for each state with treatment counties. The estimates use data from treatment counties in which the penetration rate of managed care is below 10% before the mandate is rolled out.

Figure 4. State by state IV estimates of the effect of MMC on Medicaid spending

(a) First year after a mandate



(b) After the first year



Note: Figures show for each state the IV estimates of the effect of enrollment in a managed care plan on Medicaid spending. The estimation uses county enrollment mandates as instruments for individuals' enrollment. Panel A presents the estimates for the first year after a mandate is rolled out, panel B shows the IV estimates for the years after the first year (for states with data on these years within our sample period). The estimates use data from treatment counties in which the penetration rate of managed care is below 10% before the mandate is rolled out.

Panel A presents the IV estimates for the first year after the mandate. Out of 16 states with treatment counties, Medicaid spending decreases for six states, doesn't change in a statistically significant way for other six states, and increases in four states. Panel B shows the IV estimates for the period after the first year - Medicaid spending increases during this period in ten states, and decreases only in one state - Louisiana. The rest of the treatment states show no significant change in Medicaid spending. This supports our conclusion that beyond the first year of enrollment in MMC, shifting disabled beneficiaries from the public FFS system to private managed care plans does not reduce costs for the Medicaid program. Moreover, most states

that mandated beneficiaries to enroll in MMC experience eventually a significant increase in spending.

5.1 Heterogeneity by pre-mandate FFS prices

The ability of managed care plans to reduce costs by reducing payment rates to providers may depend on the baseline payment rates in the public FFS system they replace. If rates are very low to begin with, managed care plans may face higher rates than the public FFS system, increasing their costs. Duggan and Hayford (2013) find that in states where Medicaid FFS rates are higher and closer to those of commercial insurers, a transition to managed care reduces the total Medicaid spending (while on average they find no effect). Their estimation relies on the price gap for a single service - newborn delivery (using data collected by Schwartz et al. (1991)). We use our rich data to construct state-specific price measures that account for *all* outpatient FFS services to disabled beneficiaries. We estimate the states' fixed effect on the prices of claims for FFS outpatient services delivered in 2004 to the Medicaid disabled. The specification is:

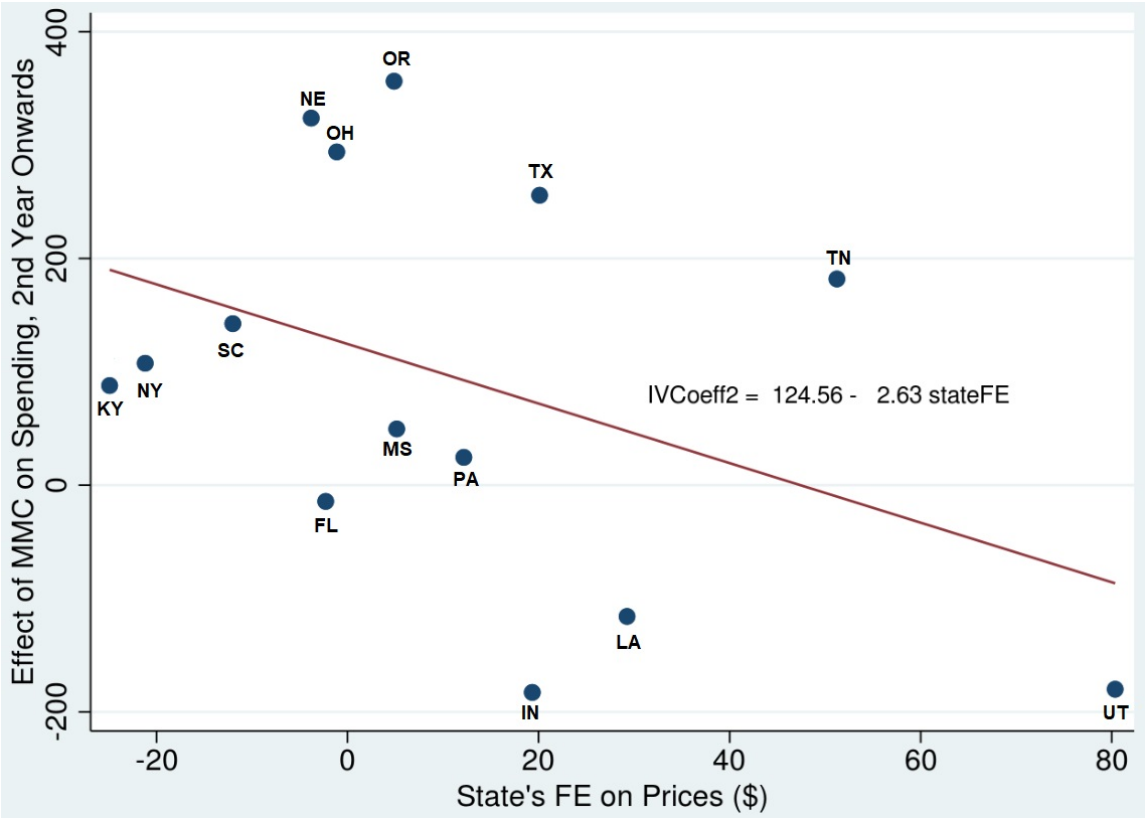
$$Price_{ips} = \beta_0 + \gamma_p + \delta_s + \epsilon_{ips} \tag{7}$$

where $Price_{ips}$ is the payment for procedure p in claim i in state s. γ_p is the procedure fixed-effects, δ_s is the state fixed-effects, and ϵ_{ips} is a random error. Figure 5 presents, for each state, the IV estimate for the effect of managed care enrollment on Medicaid spending after the first year, relative to the state's fixed effect on FFS prices in 2004. The scatter plot suggests that states with lower pre-mandate FFS prices tend to experience higher increases in spending after their beneficiaries transition from FFS to private managed care plans.

6 Discussion

We find that shifting Medicaid's disabled beneficiaries from the public FFS system to managed care plans eventually increases Medicaid fiscal spending, and creates a dynamics of spending increases. For such an outcome to occur, two conditions are required: first, costs treating Medicaid beneficiaries should be higher in managed care plans than in the FFS system; Second, Medicaid's capitated payments to these plans should increase as a result of the plans' higher spending. We now discuss possible mechanisms that may lead to these two conditions being satisfied and thus lead to our result.

Figure 5. The effect of managed care on Medicaid spending in a state and the state's fixed effect on Medicaid FFS prices



Note: Figure shows the IV estimate in each state for the effect of enrollment in a managed care plan on Medicaid spending, and the state's fixed effect on Medicaid FFS prices (prices of outpatient services that appear in FFS claims from 2004 of Medicaid disabled beneficiaries). The line is a linear trend of the points included in the scatter plot. Minnesota - an outlier at (16.73, 1,543) was excluded from the scatter plot.

6.1 Mechanism I: Higher costs for managed care plans

Managed care plans may see higher costs for treating disabled Medicaid beneficiaries if, first, utilization is higher in managed care comparing to the public FFS system. This may be the case if indeed disabled beneficiaries are under-served in Medicaid's public FFS system (KFF (2012)), and have needs that are met when they transition to private managed care plans. Layton et al. (2019) provide an example for such unmet needs in Texas, where the FFS system limited the number of prescriptions that beneficiaries could fill to three per month. This drug cap was eliminated after a transition to managed care, increasing the utilization of prescription drugs and the spending on them. Second, cost in managed care plans may be higher than the FFS system if the plans pay higher prices to providers. This may happen if the

state's FFS payment rates are already very low before the transition to managed care, and plans need to pay higher prices to accommodate the higher utilization. For example, Layton et al. (2019) find an increase in outpatient prices in Texas after a transition from FFS to managed care, alongside an increase in the utilization of these services. The association we find between lower pre-mandate prices in a state and higher Medicaid spending post-mandate provides another support for this price mechanism. Third, costs could be higher under managed care if plans' administrative costs are higher than in the FFS system. Lastly, disruptions in care during the initial period after a mandated transition to managed care may hurt individuals' health and lead to higher costs later on (see chapter 1 of this dissertation).

6.2 Mechanism II: Dynamic increase in Medicaid spending

Higher costs for managed care plans don't necessarily lead to higher costs for the Medicaid program. However, the procurement process of managed care plans in many states could lead to such a connection. The contracts signed with MC plans include the capitation rates paid for each beneficiary and the rules for updating the rates. These rules are affected by the CMS guidelines for the development of capitation payment rates to managed care plans. The guidelines (CMS (2019)) direct states to set actuarially sound rates based on the experience of the Medicaid's population in the recent three years. These rates should be updated annually. Such rules make sure that past increases in plans' costs will lead to higher payments by the Medicaid program. These procurement rules may decrease plans' incentives to save costs, leading to dynamics of continuous increases in spending. Many state follow CMS guidelines, requiring 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 is required to achieve higher (absolute) profits.

7 Conclusion

We exploit county-level enrollment mandates, that transition disabled beneficiaries in Medicaid from the FFS program into managed care plans, to estimate the fiscal effects of these transitions. We find that while total Medicaid spending doesn't change much at the first year after a transition, the spending increases significantly in later years. Procurement rules of states' Medicaid programs may serve as a mechanism for this cost increase, but further research is required to directly determine their effects.

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A Appendix Tables

Appendix Table A1. Contiguous counties: The effect of a managed care enrollment mandate (reduced form)

	(1) Total Spending	(2) FFS Spending	(3) Capitated Payments	(4) MC Penetration
First year after a mandate X isTreated	-36.14	-312.53***	276.39***	0.45***
	(41.16)	(32.77)	(35.85)	(0.02)
After the First year X isTreated	185.16***	-306.28***	491.44***	0.50***
	(46.44)	(49.04)	(50.15)	(0.03)
Baseline Mean	1,503	1,304	199	0.05
# of beneficiaries		442,878		
# of counties		239		

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

Note: Table shows estimates of the impact of a managed care enrollment mandate on the examined outcomes in treatment counties, relative to control counties. It uses a sample of contiguous treatment and control counties. All specification include also a constant, individual fixed effects, quarter fixed effects, and cohort-quarter fixed effects. Standard errors are clustered at the cohort level, i.e. the groups of a single treatment county and its control counties. Column (1)-(3) show the effect on total/FFS/Capitated spending, accordingly, all measured in dollars per beneficiary per month (PMPM). Column 4 show the effect of a mandate on the share of disabled beneficiaries enrolled in a managed care plan.

Appendix Table A2. Reduced form estimates for stacked DID estimations - effect on total spending

	(1) Never-Treated Controls	(2) Later-Treated Controls
First year after a mandate X isTreated	3.39	-25.25***
	(9.70)	(9.16)
After the first year X isTreated	82.53***	12.51
	(18.79)	(13.16)
Baseline Mean	1,211	1,223
# of beneficiaries	2,277,616	1,850,431
# of counties	1,420	891

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

Note: Table shows reduced form estimates of counties' MC thresholds on Medicaid spending. All specification include also a constant, individual fixed effects, quarter fixed effects, and cohort-quarter interaction fixed effects. Standard errors are clustered at the county level. Column (1) presents estimations that uses only counties that have no threshold as control counties. Column (2) uses as control counties only counties that have a threshold in the future, at least 3 years after the cohort's threshold quarter. The sample includes only counties from states that have reliable MAX data (using our "liberal" criteria), and only treatment and later-control counties in which the managed care penetration rate doesn't exceed 10% (pre-threshold).

Appendix Table A3. Heterogeneity: Separately examining treatment counties with a 2007 mandate, and with a post-2007 mandate.

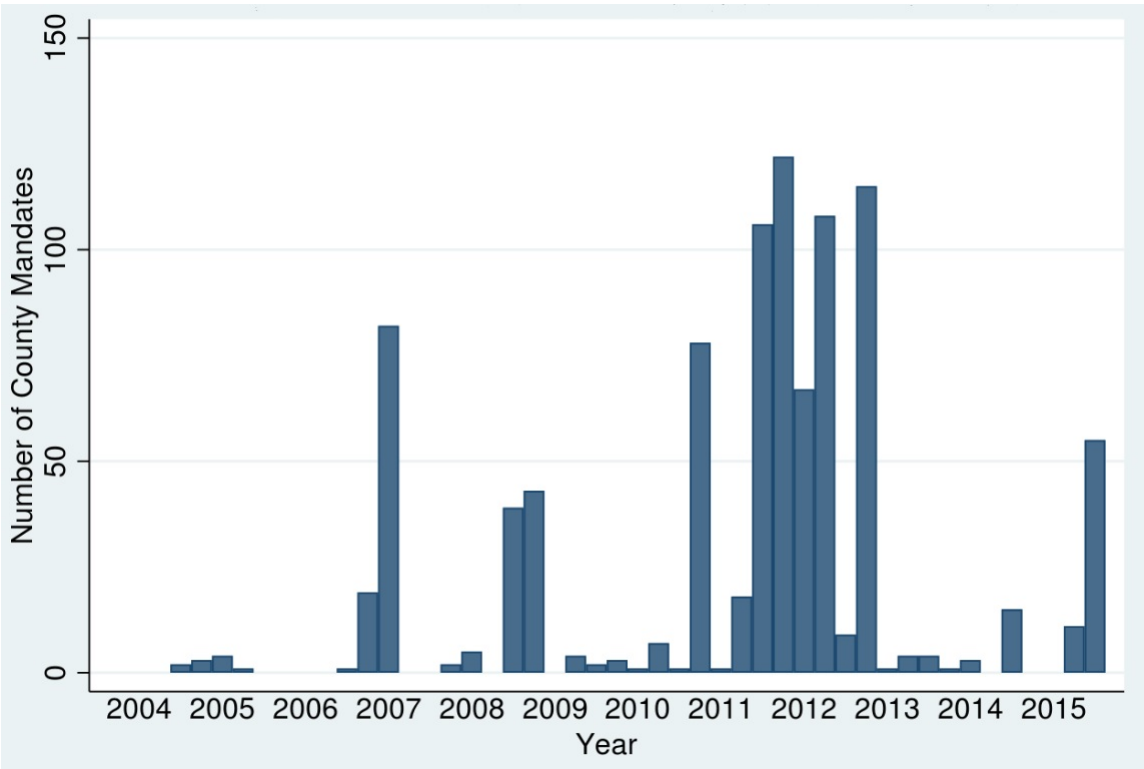
	(1) 2007 Mandates			(2) Post-2007 Mandates		
	Reduced Form	IV (First Year)	IV (After First Year)	Reduced Form	IV (First Year)	IV (After First Year)
First year	-17.63			15.30		
	(11.83)			(14.55)		
After the first year	122.54***			98.56***		
	(24.70)			(28.26)		
In a MMC Plan		-78.39***	256.87***		9.13	185.94***
		(24.58)	(42.77)		(24.64)	(46.03)
Baseline Mean	1,305	1,305	1,305	1,209	1,209	1,209
# of beneficiaries	1,550,044	1,471,722	1,542,561	2,709,620	2,415,090	2,660,473
# of counties	837	837	837	1,551	1,551	1,551

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

Note: Table shows reduced form estimates for the effect of an enrollment mandate and instrumental variable estimates of the impact of individuals' managed care enrollment on Medicaid's spending on them. Individual's MC enrollment is instrumented by the period post MC enrollment mandates in the treatment counties. All specification include also a constant, individual fixed effects, quarter fixed effects, and cohort-quarter interaction fixed effects. Standard errors are clustered at the county level. The sample includes only counties from states that have reliable MAX data (using our "liberal" criteria), and only treatment counties in which the managed care penetration rate doesn't exceed 10% pre-mandate.

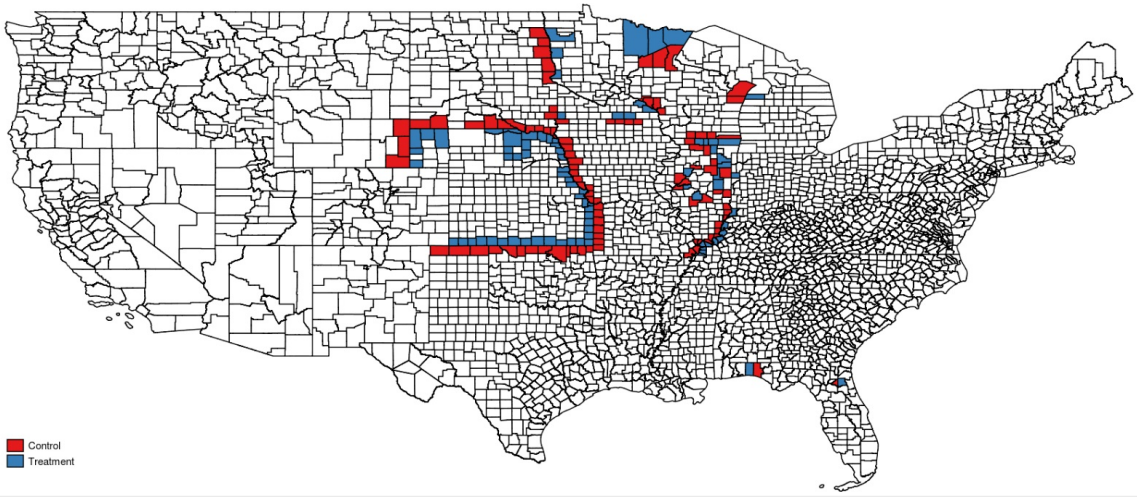
B Appendix Figures

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 over our 2004 to 2015 sample period.

Appendix Figure A2. Map of contiguous treatment and control counties



Note: Figure shows the contiguous treatment and control counties in our full sample.