How does Medicaid managed care affect provider behavior? New evidence from spillovers on private health care*

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Abstract

Medicaid is increasingly provided by private managed care plans. I examine the direct effect of Medicaid privatization on health care utilization of Medicaid beneficiaries as well as the indirect effect on non-Medicaid privately insured individuals. Exploiting the staggered rollout of the Medicaid managed care (MMC) mandate across counties in New York, I find evidence of quality improvements under MMC, such as increased routine office visits and child immunizations. MMC also expanded Medicaid beneficiaries' access to physicians by increasing the number of providers treating Medicaid patients. I find that routine office visits similarly increased for non-Medicaid privately insured individuals, and the same-signed spillover effect is larger in low-income areas. My findings suggest that physicians may have updated their overall practice styles when the mandate affected a large share of their patients.

Keywords: Medicaid; managed care; private insurance; spillover; provider practice style
JEL Classification Codes: I10, I13, I18

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1 Introduction

Medicaid is the largest means-tested public health insurance program, which plays a key role in financing births in the US, covering slightly less than half of all births (MACPAC 2020). Over the past few decades, many US states have dramatically transitioned their Medicaid delivery system from government-run fee-for-service (FFS) to privately-run Medicaid managed care (MMC). Currently, MMC is the dominant way in which states deliver services to their Medicaid beneficiaries. In 2019, about 83% of Medicaid beneficiaries were enrolled in a type of managed care plan, increased from 10% in the early 1990s (Duggan and Hayford 2013).[1]

Despite the widespread adoption of MMC, existing literature documents mixed effects, with limited understanding of underlying mechanisms. In theory, welfare effects of MMC are ambiguous. While MMC may reduce program spending by incentivizing private plans to reduce costs (Lee 2020), it can also increase spending if private plans cannot negotiate lower provider prices than the public system (Duggan 2004; Duggan and Hayford 2013). In addition, while MMC may improve health care by specifying quality along contractible dimensions (Layton et al. 2019; Lee and Vabson 2024) or by increasing competition (Cabral et al. 2018), profit-maximizing private plans may provide inadequate care, which may deteriorate quality and patient health (Aizer et al. 2007; Kuziemko et al. 2018).

This paper examines the county-by-county rollout of mandates in New York that required Medicaid beneficiaries to enroll in a private managed care plan, focusing on infants and pregnant women. This setting provides multiple advantages. First, using Medicaid claims data from New York, I examine detailed health care utilization patterns of Medicaid beneficiaries under both public and private systems, including outpatient, inpatient, and emergency department visits. Second, I supplement the analysis on Medicaid beneficiaries by examining indirect effects of Medicaid privatization on the privately insured, using a large private insurance claims data. Given that Medicaid is a prominent payer for infant and maternity care, focusing on these populations provides an opportunity to understand how changes in Medicaid may influence non-Medicaid markets. Moreover, examining spillover effects allows me to investigate the mechanism through which MMC can achieve improvements in health care as well as to understand how providers navigate a fragmented health system in which they treat patients with multiple insurers.

Examining the direct effects of Medicaid privatization on Medicaid beneficiaries using

1. The 2019 Medicaid managed care enrollment records (https://data.medicaid.gov/dataset/52ed908b-0cb8-5dd2-846d-99d4af12b369/data?conditions[0][resource]=t&conditions[0][property]=year&conditions[0][value]=2019&conditions[0][operator]==) were accessed on May 26, 2022.

Medicaid claims data (2004-2010), I find that the MMC mandate expanded access to routine care. MMC increased Medicaid infants' routine office visits in the first year by 0.7 visits or by 25% when evaluated at the sample mean. The increase in total routine office visits was largely driven by increased encounters with primary care physicians (PCPs). In addition, Medicaid infants received 0.4 more immunizations following the MMC mandate in the first year (a 27% increase when evaluated at the sample mean). I find evidence that the number of routine providers in Medicaid increased and the associated market concentration for routine care in Medicaid decreased, suggesting that MMC achieved improvements in health care at least in part by expanding access to physicians and increasing competition.

As counties mandated managed care for Medicaid beneficiaries, I find clear evidence that the mandate also affected non-Medicaid patients. Examining privately insured infants from IBM® MarketScan® Commercial Database (2007-2010), I document same-signed spillovers from Medicaid on private health care. Specifically, the MMC mandate increased routine office visits with PCPs by 1.5 visits for infants with private insurance, which is statistically indistinguishable from the increase in PCP visits for infants with Medicaid. This effect was concentrated in low-income areas, suggesting that physicians might have changed their overall practice style when a large share of their patients was affected by the transition to managed care under Medicaid. Additionally, I find that the main estimates are robust to the inclusion of provider fixed effects, further supporting the primary mechanism behind the positive spillover effect as changes in within-provider practice style.

These findings provide important insights on how MMC operates: managed care directly influences physicians' practice styles rather than simply recruiting providers who meet managed care plans' needs. If managed care plans sort their MMC enrollees toward "higher-quality" providers (e.g., by using plan networks), "lower-quality" providers may sort themselves onto non-Medicaid patients, which would generate opposite-signed spillovers. Neither the number of providers nor market concentration of those participating in the private insurance market changed following the mandate, suggesting that provider sorting is unlikely to drive the spillover effect. I consider several other alternative mechanisms and provide evidence that is inconsistent with them, including changes in private prices or shifts in patient composition.

To fully capture the effects of the MMC transition on the privately insured, I further examine other outcomes. I find that the increase in routine office visits with PCPs did not translate into a change in the total number of routine office visits. Instead, I find a negative but insignificant effect on routine office visits with non-PCPs for privately insured infants. This is in contrast to the increase in the total number of routine office visits for Medicaid infants. Moreover, I do not find evidence that the MMC mandate increased immunizations for infants with private insurance, suggesting that the benefits of the increased routine visits were limited for the privately insured. Additionally, I find no evidence that increased PCP routine office visits resulted in lower costs or improved health for the privately insured. Theoretically, the improved access to routine care may reduce costs by avoiding more specialized or expensive visits, but I find no significant change in insurer spending following the MMC mandate. Further, while better preventive care may reduce avoidable inpatient visits or emergency department visits, I do not detect any changes in these types of care.

These patterns are not unique to infant care. In fact, I find very similar patterns for pregnant women. The transition to MMC increased the number of total prenatal care visits for pregnant women with Medicaid by 1.7 visits. This was driven by increases in both obstetrician-gynecologists (OB-GYN) and non-OB-GYN visits, suggesting an overall improvement in access to care for Medicaid beneficiaries. For pregnant women with private insurance, the MMC mandate also increased OB-GYN prenatal care visits, indicating the same-signed spillovers in routine care. However, I find no evidence that the increase in OB-GYN visits for privately insured women was associated with any cost savings or health benefits.

These findings contribute to the ongoing debate on MMC versus FFS by demonstrating that MMC can increase routine care among Medicaid patients. However, it is important to note that these findings may be specific to this study's setting. In New York, a quality incentive program provides bonuses to plans that perform well on specific quality metrics. Additionally, the Medicaid-to-Medicare fee ratio in New York is relatively low compared to other states,[2] suggesting that MMC may have increased reimbursement rates for providers, encouraging them to participate in Medicaid and/or deliver better care under MMC. In states or settings without similar incentives, these findings may not be generalizable.

Additionally, while I find an overall increase in routine care, the effects may vary across plans. In the spirit of Geruso et al. (2023), I compare counties with varying degrees of high-spending plan presence. I find that the increase in routine care is concentrated in areas with an above-median share of high-spending plans, suggesting that my findings may be driven by certain high-spending plans. This finding underscores the heterogeneity among plans and highlights that my results may not generalize to all managed care plans.

An important implication of my findings is that changes to the Medicaid delivery system can lead to broader impacts through positive spillover effects on non-Medicaid patients. This is relevant beyond Medicaid, given the growing popularity of Medicare Advantage (MA), a managed care option for Medicare. Currently, over half of Medicare beneficiaries are enrolled in an MA plan, a significant rise from 19% in 2007 (Freed et al. 2024). My findings suggest

2. See https://www.kff.org/medicaid/state-indicator/medicaid-to-medicare-fee-index.

that as the number of people choosing MA increases, the associated provider incentives can influence not only the treatment of MA patients but also that of non-MA patients, such as traditional FFS Medicare enrollees and other privately insured older adults. This suggests that policymakers should consider not only the direct impacts of contracting out care for targeted public beneficiaries but also the potential implications for the broader population.

This paper is closely tied to the literature on spillovers of managed care. While some papers examine spillovers of managed care in commercial insurance (Baker and Corts 1996; Glied and Zivin 2002) or Medicare Advantage (Baicker et al. 2013; Baicker and Robbins 2015), no prior work has studied spillovers of managed care in the context of Medicaid to the best of my knowledge. This paper fills this important gap by studying the effects of Medicaid privatization on private insurance using staggered implementation of MMC mandates. More broadly, this paper contributes to the literature on spillovers of various health policies. Several papers focus on spillovers of Medicare, e.g., on technology adoption (Finkelstein 2007), private physician payments (Clemens and Gottlieb 2017), discharge destinations (Einav et al. 2020), and prescribing behavior (Barnett et al. 2020). Additionally, a few papers study spillovers of Medicaid (Baicker and Staiger 2005; McInerney et al. 2017) and commercial insurance (Richards and Tello-Trillo 2019).

The remainder of the paper proceeds as follows. Section 2 provides the background on Medicaid managed care and discusses potential mechanisms through which the transition to MMC can affect patients with private coverage. Section 3 discusses data and sample. Section 4 describes the empirical strategy, difference-in-differences and event study designs, and presents summary statistics, highlighting the baseline differences in access between Medicaid and private insurance enrollees. Section 5 presents the main results for infant and maternity care and investigates different mechanisms. Section 6 conducts several robustness checks. Section 7 concludes.

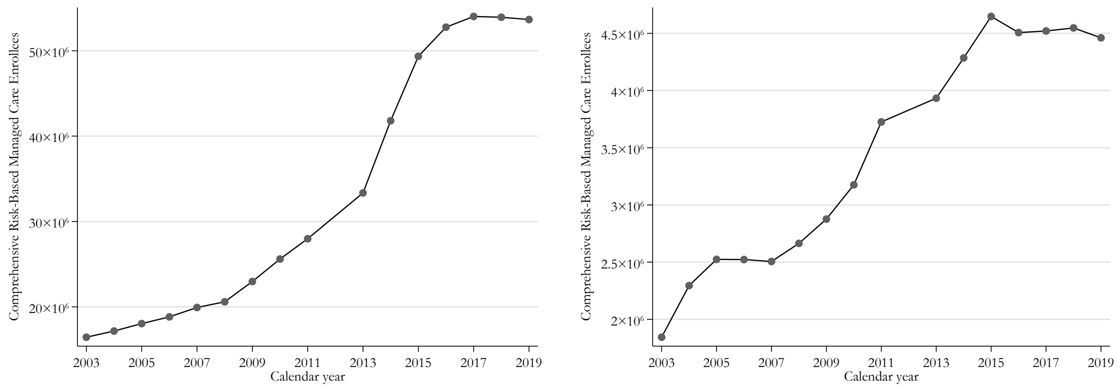
2 Background

2.1 Growth of Medicaid managed care

Many states have started implementing Medicaid managed care in 1990s, and MMC enrollment has continued to grow over the years. Figure 1(a) shows the consistent growth of MMC enrollment in the US until 2015, when the enrollment plateaued. Figure 1(b) shows a similar pattern for the state of New York. New York rolled out mandatory enrollment in MMC across counties between October 1997 and November 2012. Currently, as a result, Medicaid beneficiaries in all 62 counties of New York are generally required to enroll in a

Figure 1: Growth of Medicaid managed care

(a) United States (b) New York



Source: Kaiser Family Foundation.
Note: The 2012 data is missing from the Kaiser Family Foundation.

managed care plan.

The popularity of MMC stems from its payment structure. Under MMC, state governments pay private managed care plans a fixed fee per month per enrollee (i.e., capitation) to cover certain services. In turn, private plans pay health care providers (such as physicians and hospitals) for covered medical services. In contrast to the FFS system where governments directly pay health care providers for each service provided, MMC can in theory reduce costs and better manage care through capitation payments. Fixed payments under capitation incentivize private plans to minimize costs, and as part of this effort, plans may invest in quality improvements to reduce unnecessary and expensive services (Lee 2020). Moreover, competition for enrollees and government regulation can help ensure that plans do not excessively ration care (Layton et al. 2022).

Unlike the Medicaid fee-for-service system, where healthcare providers receive reimbursement directly from the government according to a fee schedule, payment levels under Medicaid managed care may vary depending on the contract between the managed care plan and the provider. While the Medicaid fee schedule is often used as a reference point, the actual rates depend on negotiations between the two parties, which can be influenced by various factors, such as the relative bargaining power (Gaynor et al. 2015), the generosity of the baseline FFS rates (Duggan and Hayford 2013), and performance incentives (Gold 1999).

2.2 Characteristics of Medicaid managed care in New York

Most MMC beneficiaries in New York are enrolled in a health maintenance organization (HMO). As is typical for an HMO, enrollees select a primary care physician (PCP) who provides routine care and manages their health care. PCPs also act as a gatekeeper who refers patients to specialists and other health care providers when necessary. Also, MMC enrollees are generally required to find health care providers from their plan's network. By contrast, Medicaid fee-for-service gives beneficiaries flexibility in choosing health care providers and does not actively coordinate nor restrict care.

MMC plans in New York receive capitation payments in exchange for providing health care services to their enrollees.[3] During the sample period, more than half of state Medicaid programs were operating a pay-for-performance program to improve quality (Kuhmerker and Hartman 2007). New York has also operated a quality incentive program since 2001. Based on composite scores from quality and satisfaction measures, plans can receive bonuses and auto-assignment preferences (NYSDOH 2016).

These measures focus on primary care for children and women's health services, including immunizations, access to preventive care, access to PCPs, prenatal and postpartum care, and well-child visits.[4] This indicates that managed care plans in New York are strongly incentivized to improve preventive and routine care services for these populations, which are the primary outcomes analyzed in this paper. In contrast, quality bonuses are not directly tied to outcomes such as hospitalizations or emergency department visits. This implies that if health plans focus their quality improvement efforts on the metrics tied to quality bonuses, one would expect to observe improvements in routine and primary care services, but not necessarily in inpatient or emergency department visits.

2.3 Hypotheses on spillovers

The US health care market is characterized by a mix of private and public insurers. In 2020, 66.5 percent of the population had private insurance coverage and 34.8 percent had public coverage (Keisler-Starkey and Bunch 2021). Employment-based insurance was the most common type of private coverage, accounting for 54.4 percent of the population. A

3. Initially, these payments in New York were not adjusted based on the health status of enrollees. However, in April 2008, New York implemented a risk-adjusted capitation payment system for MMC plans (State of New York 2009). Under this system, plans with above-average case mixes receive higher payments, while those with below-average case mixes are compensated at lower rates. Since this paper focuses on the overall population of infants and pregnant women, the introduction of risk adjustment is unlikely to have a significant impact, as risk adjustment is generally more relevant for higher-cost enrollees.

4. For more information, see https://www.health.ny.gov/health_care/managed_care/quality_strategy.htm.

large share of the population had public coverage, predominantly through Medicare (18.4 percent) or Medicaid (17.8 percent). Consequently, most physicians in the US treat patients with various insurance arrangements, having at least some patients in each of the major payer categories. For example, according to Gillis (2017), the average patient mix in 2016 was 29.3% Medicare, 16.9% Medicaid, 43.4% commercial, 6.1% uninsured, and the remaining 4.3% with other payers.

In such a fragmented system, when one insurer changes its payment system to affect the way physicians treat its enrollees, those physicians might change how they treat all patients. For example, a payment reform that is designed to improve care for the publicly insured may have positive or negative externalities on the privately insured. The existence and the sign of such spillovers have implications for the design of healthcare payment systems. Below, I discuss potential mechanisms through which the transition to MMC may affect patients that are not enrolled in Medicaid.

First of all, managed care can influence healthcare providers' general practice styles. Healthcare providers refer to a range of entities, including individual physicians, group practices consisting of multiple individual providers, as well as facilities such as hospitals. Since managed care plans are incentivized to control utilization and quality, they may emphasize low-cost preventive and routine care while restricting high-cost specialist care. To achieve this, managed care plans may pay healthcare providers higher prices for high-value care, offer quality bonuses to encourage providers to meet certain targets (Gold 1999), or use mechanisms like prior authorization and referral requirements (Baicker et al. 2013).

Providers treat a variety of patients with different insurance arrangements, and it may be costly for providers to customize their practice style to each patient's insurer (Barnett et al. 2020). As a result, when Medicaid transitions to MMC, providers may update their general practice style, which could affect not only Medicaid patients but also non-Medicaid patients. This type of change in provider practice style would lead to a same-signed spillover effect onto other patients.

For example, the change in practice style could include actions such as physicians paying more attention to certain aspects of care (e.g., recommending vaccinations, emphasizing follow-up care) and facilities or offices putting more effort into encouraging patients to return (e.g., through reminder texts and calls), i.e., increased outreach effort at the provider level.[5] They may also involve changes during visits, such as adjustments to visit length or the

5. Note that managed care plans can also engage in outreach efforts to encourage their enrollees to seek primary and routine care, aiming to better manage their health and avoid unnecessary or expensive treatments. While this can directly affect the care received by Medicaid patients under MMC, it would not impact privately insured patients, as the plans would not be able to target them. Therefore, this is unlikely to be an underlying mechanism behind the spillover effects.

number of prescriptions issued, as studied in Glied and Zivin (2002).

In particular, the extent to which providers respond to the transition would be a function of how much of their income depends on Medicaid. Providers who have a larger share of Medicaid patients are more likely to respond to Medicaid's transition to MMC by updating their practice patterns. In other words, spillover effects are likely to be larger when providers practice in areas with many Medicaid patients. Therefore, in addition to estimating changes in care utilization in Medicaid and private insurance, I test whether spillover effects varied by local median income.

Second, the transition to MMC can affect the number and the type of providers who participate in Medicaid. MMC generally requires enrollees to use in-network providers, while there is no provider network in the FFS system. If MMC restricts the available provider pool using networks, the number of providers participating in Medicaid may decline following the transition to MMC. On the other hand, if MMC is somehow able to recruit providers that did not previously participate in Medicaid, it could increase the number of participating providers. A change in the number of providers participating in Medicaid could affect the privately insured if the provider pool overlaps. To examine this channel, I investigate whether the number of providers participating in each market changed following the transition.

In addition, managed care plans may steer patients toward low-cost, high-value providers by recruiting such providers in their network (Ho and Pakes 2014; Raval and Rosenbaum 2017). For example, managed care plans may recruit physicians who focus on preventive care, while excluding expensive providers who are prone to refer patients out to specialists. As a result, following the transition to MMC, some "lower-cost" providers may specialize in providing care to MMC beneficiaries, while "higher-cost" providers focus on other patients such as those with private insurance. Thus, this kind of patient sorting is likely to generate opposite-signed spillovers. To test the role of such patient sorting, I examine changes in the provider Herfindahl-Hirschman Index (HHI), a measure of market concentration. If MMC did sort patients toward certain types of providers, leaving other providers to the privately insured, the HHI may increase in both Medicaid and private insurance markets.

There may be additional mechanisms of spillovers. For instance, the implementation of MMC can affect the entire system by changing incentives to invest in certain technologies. For instance, managed care plans may discourage the use of high-cost technologies (e.g., MRI machines). If the transition to MMC slows the adoption of such technologies, it can affect not only MMC patients but also others. Thus, I test whether the MMC mandate decreased the use of high-cost, low-value services, such as imaging services (Curto et al. 2019), among Medicaid patients and whether same-signed spillovers were observed in private health care.

Assuming that health care providers are able to adjust their practice style based on the patient's insurer, opposite-signed spillovers can also arise. For example, given capacity constraints, an increase in office visits for Medicaid patients can lead to a decrease in office visits for privately insured patients. Alternatively, minutes per visit may decrease to accommodate the increased number of office visits. I test whether such capacity constraints play a role by examining minutes per visit as well as heterogeneous effects in spillovers by monthly number of Medicaid visits.

Finally, MMC can affect provider prices. With the transition to MMC, the payer changes from the government to private managed care plans. While providers receive administratively set prices under FFS, they negotiate prices with private plans under MMC, suggesting that prices can increase or decrease depending on several factors, such as the relative bargaining power (Gaynor et al. 2015) and baseline FFS payment rates (Duggan 2004; Duggan and Hayford 2013).

The contracts that MMC plans have with physicians vary, and I do not have access to the specific contracts or the MMC payment levels. However, previous research provides valuable insight suggesting that MMC rates might be higher than FFS rates. For example, Layton et al. (2022) found higher prices and higher quality under MMC in Texas, a pattern consistent with the findings in my paper. Moreover, Duggan and Hayford (2013) suggests that states with low Medicaid FFS prices may have room for higher reimbursement under MMC through negotiation. This is also consistent with potentially higher reimbursement under MMC in New York, where the Medicaid FFS rates are low relative to Medicare fees. In 2019, for example, New York was ranked 46th in terms of Medicaid-to-Medicare fee ratios, according to the Kaiser Family Foundation.[6]

Higher prices under MMC can affect privately insured patients through two channels. First, providers may change their practice style in response to higher prices, and by using the updated practice style for all patients, this can impact non-Medicaid patients. I consider this as part of the first hypothesis regarding changes in provider practice style. Second, there may be a change in private prices as the benchmark Medicaid prices change, e.g., by changing outside options (Clemens and Gottlieb 2017). The increase in prices can affect provider behavior and thus the care delivered to the privately insured (Clemens and Gottlieb 2014; Cabral et al. 2021). While I do not have information on provider prices under MMC, I am able to examine whether insurer payment in private insurance claims changed following the MMC mandate.

6. https://www.kff.org/medicaid/state-indicator/medicaid-to-medicare-fee-index was accessed on May 31, 2022.

3 Data and sample

To examine both direct and indirect effects of Medicaid managed care, I use two administrative datasets. First, I use the New York Medicaid Analytic eXtract (MAX) claims and enrollment files obtained from the Centers for Medicare & Medicaid Services (CMS) for 2004-2010. The New York MAX data contains all FFS claims as well as encounter data for MMC enrollees. Because states collect encounter data from multiple managed care plans, its quality may be comparably worse than the quality of FFS claims. However, New York reports usable encounter data according to policy briefs published by Mathematica (Byrd and Dodd 2012, 2015). In particular, encounter data for outpatient services, which is the main focus of this paper, is complete and of high quality throughout my study period. Other encounter data such as inpatient and prescription claims are known to be incomplete for a subset of years.

Moreover, note that incomplete encounter records would suggest that my findings of increased visits under MMC may be underestimated. My analysis based on binary indicators also helps address some concerns regarding data quality. I find the same patterns when using binary indicators as outcomes instead of the actual number of visits. Since whether someone had any visit in a given year is likely recorded more reliably than the exact number of visits under MMC, the consistent findings help alleviate concerns about data quality.

The MAX data also contains information on provider identifiers as well as provider specialty. The available provider identifiers are different from the National Provider Identifiers (NPIs) for most of my sample period and cannot be linked to external datasets. The MAX data also includes information on Medicaid spending but does not contain insurer payments to providers under MMC. Additionally, the enrollment data provides beneficiaries' basic demographic characteristics as well as monthly enrollment status in Medicaid, along with additional indicators for whether a beneficiary is in FFS or MMC.

Second, I use IBM® MarketScan® Commercial Databases for 2007-2010. This dataset contains private health insurance claims for a large population of individuals and their dependents with employer-provided commercial insurance. While the MarketScan data does not capture the full population of the privately insured, it represents the most common type of private coverage, employment-based insurance. Moreover, the data is substantially large, covering more than 20 million people annually (Butler et al. 2021). The MarketScan data also provides provider identifiers along with provider specialty. However, provider identifiers in MarketScan have a lot of missing values and are known to be unreliable. Thus, provider-level analyses based on the MarketScan data may be subject to measurement error and need to be interpreted with caution. Finally, the MarketScan data also provides claims records on inpatient and outpatient care including ED visits. Moreover, the MarketScan data contains information on insurer payments to health care providers.

From each dataset, I focus on two groups of people—infants under 1 year old and pregnant women. I use age to identify infants, specifically those aged between 0 and 1 year. For pregnant women, I identify beneficiaries who appear in the inpatient claims with diagnosis codes related to childbirth and pregnancy (Major Diagnostic Category 14 and ICD-9 codes 640-680). To further refine the identification of pregnancy periods, I restrict the sample to observations within 10 months prior to delivery. This way, I identify about 140,000 infants and 200,000 pregnant women each year from Medicaid, representing roughly 7% of the total Medicaid population in New York.[7]

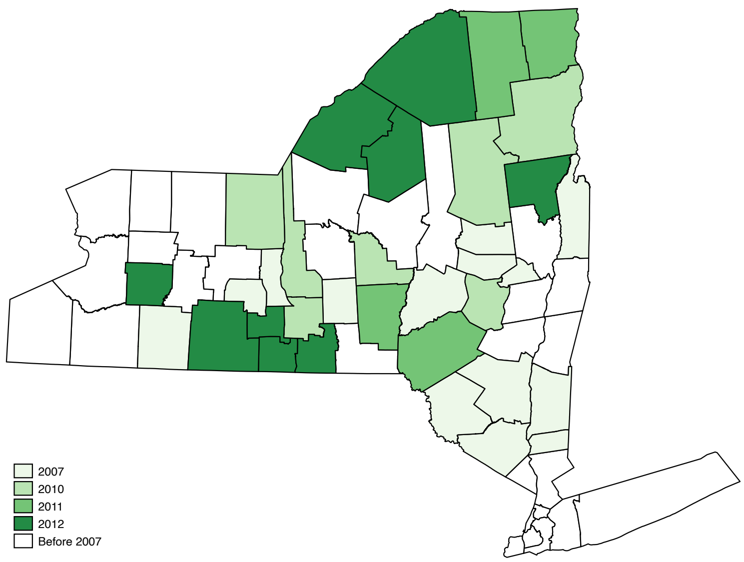
Focusing on these two groups offers two main advantages. First, a large share of these groups is covered by Medicaid. New York has the most generous income eligibility levels for infants and pregnant women. For example, in 2022, the Medicaid income eligibility level for a family of four in New York was $61,883 (223% FPL) for infants under 1 year old and for pregnant women, whereas it was $42,735 (154% FPL) for children between ages 1-18 and $38,295 (138% FPL) for adults under 65.[8] During my sample period, the Medicaid income eligibility level was 200% FPL for pregnant women and infants under the age of 1. It was lower for other groups (e.g., 133% FPL for children ages 1-5 and 100% for children ages 6-18). As a result, infants under age 1 tend to be the largest single-year age group among children. Similarly, women of childbearing age represent a disproportionately large group among Medicaid enrollees.

Second, these two groups have continuous Medicaid eligibility in New York. This policy allows infants and pregnant women to stay enrolled in Medicaid for a specified period unless they voluntarily withdraw or move out of state. Infants have 12 months of continuous eligibility, and women have continuous eligibility from pregnancy through 60 days postpartum. Without the continuous eligibility policy, many beneficiaries may lose Medicaid coverage due to changes in circumstances or administrative burdens even before the renewal period (Williams et al. 2022). If enrollees frequently switch between Medicaid and private insurance, their health care utilization patterns may be influenced by both programs, making it difficult to distinguish between spillovers from providers and convergence in utilization patterns among enrollees. By focusing on these two groups with continuous eligibility over a relatively short period, I minimize concerns about churning or switching between Medicaid and private insurance. For the main analysis, I thus do not impose any restrictions regarding

7. For the main analysis, I exclude counties that were already treated at the start of the data period (which includes New York City) to avoid using always-treated counties as controls in my TWFE model. This restriction reduces the sample size to roughly 15,000 infants and 20,000 pregnant women each year.

8. https://www1.nyc.gov/assets/ochia/downloads/pdf/all_populations_medicaid.pdf

Figure 2: New York counties by the year of managed care mandate



churn. That is, I allow enrollees to churn in and out of Medicaid. However, I test whether the results are robust to focusing on a subset of enrollees who are continuously enrolled.

Moreover, managed care organizations may have incentives to provide high-quality care to infants and pregnant women because of the quality incentive program. As described in Section 2.2, this program places a large emphasis on primary care for children and women's health services. Further, New York had kick payments for both maternity and newborn services during the study period (New York State Office of the State Comptroller 2004, 2014; Courtot et al. 2012). In addition to the capitation payment, managed care plans receive these one-time supplemental payments to cover costs associated with maternity care and newborn medical care. During the 2001-2010 analysis period of one study (Courtot et al. 2012), the maternity kick payment was reported in the $5,000 to $10,000 range. Between 2012 and 2014, kick payments for newborn services ranged from $2,277 to $6,700 per newborn in New York (New York State Office of the State Comptroller 2014). These potentially high payments may have incentivized plans to offer high-quality services to both newborns and pregnant women to keep them enrolled in their plans.

4 Empirical strategy

To estimate the effects of the transition from FFS to MMC on Medicaid and private insurance, I exploit the county-by-county rollout of the MMC mandate in New York state using a difference-in-differences (DD) framework. 21 counties implemented the mandate during the sample period. After the study period, 13 counties implemented the mandate either in 2011 or 2012. The remaining 28 counties had the mandate in place at the beginning of my sample period. Figure 2 shows the map of the counties by the year of the MMC mandate.

With differential treatment timing, a conventional two-way fixed effects (TWFE) model may be inappropriate due to negative weights on treatment effects when using already-treated groups as controls (Borusyak and Jaravel 2017; De Chaisemartin and d'Haultfoeuille 2020; Goodman-Bacon 2021). Thus, I drop counties that were already treated at the start of the data period from the analysis (Baker et al. 2022; De Chaisemartin and d'Haultfoeuille 2023)[9] and estimate the following regression:

$$y_{ict} = \alpha + \beta Mandate_{ct} + \gamma_c + \lambda_t + \epsilon_{ict} \tag{1}$$

where i indicates an enrollee, and c is county. t indicates the year of birth for infants and the year of conception for pregnant women, assuming 40 weeks of pregnancy for all childbirths. Thus, the unit of observation is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. For dependent variables, y_{ict}, I examine various health care utilization measures aggregated at the enrollee level (e.g., total number of office visits[10] for an infant i born in county c in year t). In addition to the number of visits, I consider extensive margin measures, such as an indicator for any office visit in a given year. α is a constant. $Mandate_{ct}$ is an indicator for whether the MMC mandate is in place for county c and year t. γ_c is a county fixed effect, and λ_t is a year fixed effect. β is the coefficient of interest, which captures the weighted average treatment effect of the MMC mandate. I cluster the standard errors at the county level. In Appendix C, I discuss my choice of using the TWFE specification as the main model and provide robustness checks using alternative methods.

Moreover, to understand how the treatment effects vary over time and test the parallel-trends assumption of the difference-in-differences design, I estimate an event study model where I replace the indicator for the MMC mandate ($Mandate_{ct}$) with the sum of indicators for each relative year from the county-specific year of the mandate.

$$y_{ict} = \alpha + \sum_{j \neq -1} \beta_j 1[YearToMandate_{ct} = j] + \gamma_c + \lambda_t + \epsilon_{ict} \tag{2}$$

where j indicates year relative to the mandate. I drop one year before the mandate as

9. I use the *twowayfeweights* Stata package to estimate the weights attached to equation (1) (De Chaisemartin and d'Haultfoeuille 2020) and find that the weights are all positive, suggesting that my TWFE model is not subject to the concerns regarding the negative weights. To further address the concerns regarding the TWFE model, I use the Callaway and Sant'Anna 2021 method to test robustness in Section 6.

10. The number of visits is based on the number of days with a positive number of claims.

a reference group and include counties that were never treated by 2010 as controls. By estimating β_js before the mandate, I test whether pre-trends are balanced between treatment and control counties. Examining β_js after the mandate will show how the treatment effects evolve over time. I report robust standard errors clustered at the county level.

4.1 Gap in health care between Medicaid and private insurance

Table 1 compares the sample means of key variables between Medicaid and private insurance. The unit of observation is an enrollee. To capture baseline differences, I compute the sample means for the periods prior to the MMC mandate implementation, between 2007 and 2010, in both datasets. Columns (1)-(2) summarize the sample means from the Medicaid claims data, while columns (3)-(4) summarize the sample means from the private insurance claims data. Standard deviations are presented in brackets. In column (5), I report the difference in means between Medicaid and privately insured individuals, along with the corresponding t-statistic in parentheses.

While the Medicaid records are derived from the full population of Medicaid enrollees, the private insurance records come from a non-representative sample of privately insured individuals in MarketScan. As a result, the number of observations for private insurance is much smaller. To facilitate a comparison between the two groups, I weight all estimates from MarketScan by the ratio of the county-year population to the sample size in the corresponding county-year. Note, however, that the true values for the entire privately insured population may differ from my findings.

Panel A shows health care utilization patterns of infants. I examine Evaluation and Management (E&M) codes for office visits (CPT codes 99201-99215) to focus on preventive medicine services. Infants with Medicaid have much fewer E&M office visits than infants with private coverage in the first year, with 2.9 and 4.9 visits on average, respectively. I also find a stark difference in the types of health care professionals that provide these visits between insurers. While infants with private coverage are more likely to receive these services from primary care physicians (defined as physicians whose specialty is family practice, pediatrics, or internal medicine), infants with Medicaid are more likely to see other professionals for these services. Moreover, infants with Medicaid receive a fewer number of immunizations than infants with private coverage in the first year (1.3 versus 3.5). Infants with Medicaid have more ED visits (0.5 versus 0.4 visits) and more inpatient visits than infants with private coverage (0.1 versus 0.07).

The gap in health care utilization is also evident in the probability of having any visit. While the majority (89.9%) of privately insured infants have at least one E&M office visit,

Table 1: Summary statistics before the MMC mandate, 2007-2010

	(1)	(2)	(3)	(4)	(5)
	Medicaid		Private insurance		Difference in
	Observations	Mean [SD]	Observations	Mean [SD]	means (t-statistics)
Panel A. Infants					
Number of visits					
E&M* office visits	25311	2.854 [3.671]	2267	4.894 [4.558]	-2.040 (-20.7)***
With a PCP+	25311	0.600 [1.708]	2267	3.797 [4.322]	-3.197 (-35.0)***
With a non-PCP	25311	2.261 [3.316]	2267	1.110 [2.581]	1.151 (19.8)***
Immunizations	25311	1.317 [1.607]	2267	3.477 [2.095]	-2.160 (-47.8)***
ED visits	25311	0.495 [1.069]	2267	0.356 [0.805]	0.139 (7.6)***
Inpatient visits (excluding births)	25311	0.096 [0.378]	2294	0.071 [0.295]	0.025 (3.79)***
Probability of any visit					
Any E&M office visit	25311	0.670 [0.470]	2267	0.899 [0.301]	-0.229 (-32.8)***
With a PCP	25311	0.200 [0.400]	2267	0.729 [0.445]	-0.529 (-54.7)***
With a non-PCP	25311	0.579 [0.494]	2267	0.296 [0.457]	0.283 (28.1)***
Any immunization	25311	0.523 [0.499]	2267	0.888 [0.316]	-0.365 (-49.7)***
Any ED visit	25311	0.280 [0.449]	2267	0.229 [0.420]	0.051 (5.5)
Any inpatient visit (excluding births)	25311	0.078 [0.269]	2294	0.061 [0.239]	0.017 (3.2)***
Patient characteristics					
Female	25310	0.483 [0.500]	2267	0.466 [0.499]	0.017 (1.6)
Zip code level median income	25175	44571.0 [9176.6]	2267	47031.7 [4568.7]	-2460.7 (-22.0)***
Panel B. Pregnant women					
Number of visits					
Prenatal care† visits	16967	5.038 [4.287]	3078	6.630 [5.261]	-1.592 (-15.9)***
With an OB-GYN	16967	1.267 [2.296]	3078	3.063 [4.082]	-1.796 (-23.7)***
With a non-OB-GYN	16967	3.931 [3.587]	3078	3.907 [4.087]	0.024 (0.3)
ED visits	16967	1.052 [1.913]	3078	0.867 [2.465]	0.185 (4.0)***
Inpatient visits	16967	0.073 [0.330]	3025	0.040 [0.229]	0.033 (6.8)***
Probability of any visit					
Any prenatal care visit	16967	0.898 [0.302]	3078	0.949 [0.219]	-0.051 (-11.1)***
With an OB-GYN	16967	0.440 [0.496]	3078	0.629 [0.483]	-0.189 (-19.9)***
With a non-OB-GYN	16967	0.869 [0.337]	3078	0.830 [0.376]	0.039 (5.4)***
Any ED visit	16967	0.440 [0.496]	3078	0.281 [0.450]	0.159 (17.7)***
Any inpatient visit	16967	0.059 [0.235]	3025	0.033 [0.178]	0.026 (7.0)***
Patient characteristics					
Age	16964	24.8 [5.1]	3064	29.184 [5.687]	-4.384 (-39.9)***
Zip code level median income	16905	44534.9 [8998.1]	3040	47199.0 [5327.6]	-2664.1 (-22.4)***

Notes: The unit of observation is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. Standard deviations (SD) are in brackets. With no restrictions on missing values, the number of observations varies across certain variables. The last column shows the difference in means along with its t-statistic in parentheses. Significant at 10%, ** significant at 5%, *** significant at 1%.

* E&M: Evaluation and Management services (CPT codes 99201-99215).

+ PCP: physicians whose speciality is family practice, pediatrics, or internal medicine.

† The following CPT codes are used to identify prenatal care: 59025, 59400, 59425, 59426, 59430, 59510, 59610, 59618, 76801-76817, 76818, 82105, 82106, 88271-88275, 88291.

a much lower share of Medicaid infants (67%) have a positive number of E&M office visits. The discrepancy is driven by the difference in the probability of having an E&M office visit with a PCP (20% for Medicaid infants versus 72.9% for privately insured infants). The probability of receiving any immunization is also higher for privately insured infants (52.3% versus 88.8%). Comparing observable characteristics, the share of girls is similar between the two groups, and the zip code level median income is on average lower in the Medicaid group.[11]

Further, I find large gaps in coverage between pregnant women with Medicaid and those with private insurance. Panel B summarizes these health care utilization patterns during pregnancy. I focus on prenatal care visits[12] and find that women with Medicaid have on average 5 visits during pregnancy, while women with private coverage have 6.6 visits on average. Appendix Table B.2 shows that the gap is the largest in the first trimester, suggesting that pregnant women with Medicaid may start prenatal care later in the pregnancy. This gap in the first trimester exists even for women who are continuously enrolled throughout the 10 months of pregnancy, suggesting disparities in access even conditional on coverage. Moreover, the difference in prenatal care visits is mainly driven by the gap in prenatal care visits with an OB-GYN. Pregnant women with Medicaid have more ED and inpatient visits than pregnant women with private coverage during pregnancy. I observe the same patterns for the probability of these visits. The mean maternal age is lower for pregnant women with Medicaid (24.8) compared to those with private coverage (29.2). The zip code level median income is also lower in the Medicaid group.

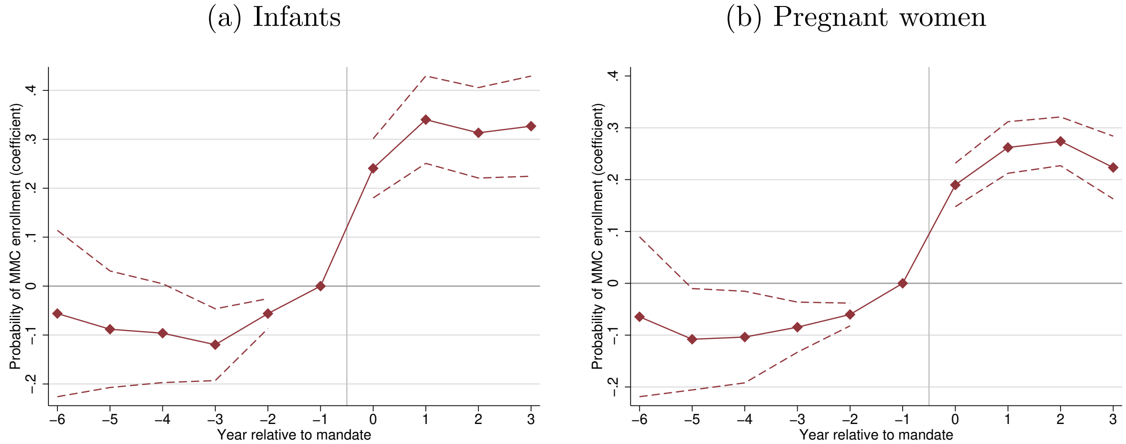
Overall, these summary statistics highlight the baseline gaps in access to care and health between enrollees in Medicaid and private insurance, suggesting that MMC's emphasis on quality improvements can be beneficial for Medicaid enrollees. At the same time, an inability to specifically target Medicaid enrollees may also generate low-value spillovers to the privately insured.

Additionally, in Appendix Table B.3, I report average provider specialties. For this analysis, I construct a provider panel by calculating means at the provider×county×year level using outpatient claims. Note that provider identifiers in MarketScan have a lot of missing values. I thus suggest caution in interpreting these results. Medicaid infants are less likely to be treated by pediatricians and more likely to be treated by OB-GYNs than privately insured infants. The provider composition is similar between Medicaid and private insurance for pregnant women, with OB-GYN being the most common provider.

11. In Appendix Table B.1, I report race/ethnicity compositions for Medicaid patients. A vast majority of them are white (91.5% for infants and 92.8% for pregnant women). Patient race/ethnicity is not available in the MarketScan sample.

12. The following CPT codes are used to identify prenatal care: 59025, 59400, 59425, 59426, 59430, 59510, 59610, 59618, 76801-76817, 76818, 82105, 82106, 88271-88275, 88291.

Figure 3: Event study estimates for the probability of MMC enrollment



Notes: Each panel plots β_js from equation (2) with the probability of MMC enrollment as a dependent variable. The unit of observation used in the analysis is the enrollee, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women.

5 Main results

5.1 Effects of the Medicaid managed care mandate on enrollment

In this section, I examine the first stage effect of the MMC mandate on enrollment. Figure 3 plots the event study estimates for the probability of MMC enrollment separately for infants under age 1 and pregnant women. The figure shows sharp increases in MMC enrollment following the mandate for both groups, with around a 30-percentage-point increase in the probability of MMC enrollment in three years from the mandate. The corresponding DD estimate (standard error) is 0.346 (0.033) for infants, with a baseline mean of 0.183. The corresponding DD estimate for pregnant women is 0.283 (0.024), with a baseline mean of 0.155. Note, however, that the event study estimates indicate that there may be small anticipatory or early treatment effects starting from two years prior to the mandate. This suggests that the magnitudes of the first stage effects on MMC enrollment should be interpreted with caution.

Moreover, while the sharp increases in MMC enrollment following the mandate provide evidence of strong first stage effects, they also show that not all Medicaid beneficiaries switched to MMC following the mandate. The lack of full compliance is partly because some subgroups of Medicaid enrollees were excluded or exempt from participating in the mandate and also because of limited enforcement due to administrative shortcomings. Nevertheless, these results highlight that a substantial share of Medicaid enrollees transitioned to MMC during my sample period.

In an alternative specification, I examine whether the increase in MMC enrollment is robust to the inclusion of provider fixed effects. Specifically, I construct a provider panel by calculating the share of patients in MMC at the provider×county×year level and examine how the share changed following the MMC mandate using the difference-in-differences design. Column (1) of Appendix Table B.4 shows that following the mandate, the share of Medicaid infants in MMC increased by 30 percentage points, which is similar to the first stage effect on MMC enrollment based on the main sample (i.e, the beneficiary×county×year panel). Importantly, column (2) shows that the estimate changes little when I add provider fixed effects, suggesting that the MMC mandate changed the patient composition *within* Medicaid providers. Columns (3)-(4) show the same pattern for pregnant women. These results suggest that providers who treated FFS patients before the mandate also treated MMC patients after the mandate. In fact, there was a significant overlap in the provider pools between FFS and MMC, with roughly 86% of outpatient providers treating both groups. In other words, the provider pool generally remained constant as Medicaid transitioned to MMC.

5.2 Effects of the Medicaid managed care mandate on infants

5.2.1 Health care utilization

This section presents the effects of the MMC mandate on infants. Table 2 presents the difference-in-differences estimates (βs from equation 1), and Figures 4-5 report event study graphs (β_js from equation 2) for various utilization measures.

Panel A1 of Table 2 summarizes the DD estimates on the number of visits for infants with Medicaid. I find that the MMC mandate increased Medicaid infants' E&M office visits by 0.7, or by 25% when evaluated at the sample mean. This increase was entirely driven by PCP visits, consistent with managed care's emphasis on preventive medicine. I also consider the effect of the MMC mandate on the number of non-PCP visits and find a small but insignificant decrease.

The increased number of E&M office visits following the mandate can be driven by an extensive margin response (i.e., new visits by infants who otherwise would have not have had any visit), an intensive margin response (i.e., more visits by infants who would have had some visits), or both. To further investigate which type of response is driving the results, I consider extensive margin measures by examining indicators for different types of visits as outcomes. Panel A2 in Table 2 shows these estimates. I find large and significant effects on the extensive margin. The probability of having any E&M office visit increased by 14 percentage point, or by 22%. The probability of any E&M office visit with a PCP essentially

Table 2: Effects of the MMC mandate on infants' health care utilization

	(1) E&M office visits	(2) PCP E&M office visits	(3) Non-PCP E&M office visits	(4) Immunizations	(5) ED visits	(6) Inpatient visits
Panel A1. Medicaid, number of visits						
MMC mandate	0.740***	0.820***	-0.059	0.363***	0.124**	-0.009
	(0.271)	(0.124)	(0.228)	(0.118)	(0.059)	(0.007)
Observations	101522	101522	101522	101522	101522	101522
Mean before mandate	2.923	0.638	2.290	1.331	0.620	0.109
Panel A2. Medicaid, probability of any visit						
MMC mandate	0.138***	0.222***	-0.048	0.161***	0.034	-0.006
	(0.044)	(0.028)	(0.030)	(0.038)	(0.021)	(0.006)
Observations	101522	101522	101522	101522	101522	101522
Mean before mandate	0.637	0.209	0.525	0.510	0.294	0.089
Panel B1. Private insurance, number of visits						
MMC mandate	0.664	1.490***	-0.834	-0.259	-0.004	0.014
	(0.667)	(0.507)	(0.791)	(0.329)	(0.061)	(0.019)
Observations	2477	2477	2477	2477	2477	2497
Mean before mandate	4.894	3.797	1.110	3.477	0.356	0.071
Panel B2. Private insurance, probability of any visit						
MMC mandate	0.064	0.208*	-0.131	-0.039	-0.003	0.013
	(0.050)	(0.116)	(0.099)	(0.042)	(0.042)	(0.019)
Observations	2477	2477	2477	2477	2477	2497
Mean before mandate	0.899	0.729	0.296	0.888	0.229	0.061

Notes: Each cell shows an estimated β from equation (1) for each dependent variable. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. * Significant at 10%, ** significant at 5%, *** significant at 1%.

doubled from 21% to 43%. This highlights that the extensive margin response played an important role in increasing the number of PCP E&M office visits for Medicaid infants.

In panel B1 of Table 2, I report the DD estimates on the number of visits from the private insurance claims data. As shown in the second column, I find an increase in PCP E&M office visits for infants with private coverage by 1.5 visits, or by 32% when evaluated at the baseline mean number of E&M office visits. The estimated effect is larger but statistically indistinguishable from the increase in PCP E&M office visits for infants with Medicaid. Panel B2 shows that the probability of having any PCP E&M office visit also increased by 21 percentage point, or by 28.5%, and the estimate is marginally significant. Together, these results suggest that MMC's emphasis on preventive care affected both Medicaid and non-Medicaid infants in a similar manner.

However, the total number of E&M office visits for privately insured infants did not change significantly following the mandate, with the 95% confidence interval (CI) of [-0.643, 1.971]. This is because of a large but insignificant reduction in non-PCP E&M visits (95% CI [-2.384, 0.716]). These results suggest that the same-signed spillover in routine care with PCPs did not translate into a significant increase in the total amount of routine care that privately insured infants received.

Figure 4 plots the corresponding event study estimates for three E&M office utilization measures, separately for Medicaid and privately insured infants. Event study estimates show consistent patterns with the DD estimates. Panel (a) shows significant increases in E&M office visits following the mandate for Medicaid infants. However, while insignificant, a steady increase in E&M office visits leading up to the year of mandate may suggest a pre-trend. Figure 5(a) reports the event study estimates for the binary version of this outcome (i.e., the probability of having any E&M office visit), and it shows no evidence of a pre-trend and a sharp increase following the mandate. This provides convincing evidence that the MMC mandate increased the probability of receiving routine care for Medicaid infants.

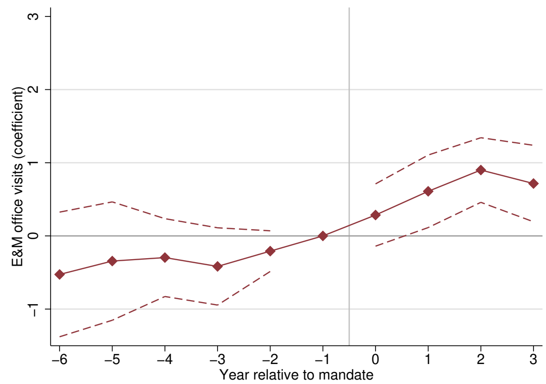
Panel (b) of Figure 4 shows no changes in the overall number of visits for infants with private coverage. Panels (c) and (e) confirm that the increase in visits among Medicaid infants was driven by PCP visits. Panel (d) similarly shows an increase in PCP visits for infants with private coverage following the mandate, with no evidence of pre-trends. Panel (f) shows insignificant changes in non-PCP visits following the mandate for infants with private coverage, consistent with the DD estimate.

Figure 5 plots event study estimates for the binary versions of these outcomes. It generally shows a consistent pattern of increased routine PCP visits for both groups of infants, although the event study estimates for privately insured infants (panel (d)) are less precise. This is likely because of the high baseline probability of these visits for privately insured infants. Since a large share (72%) of privately insured infants have at least one E&M office visit with a PCP prior to the mandate, the increased number of PCP E&M office visits is likely driven by an intensive margin response among privately insured infants. Given that the main goal of this paper is to estimate the spillover effects on the private market, I focus on the intensive margin measures (i.e., the number of visits) in the remainder of the paper, while consistently discussing the extensive margin responses.

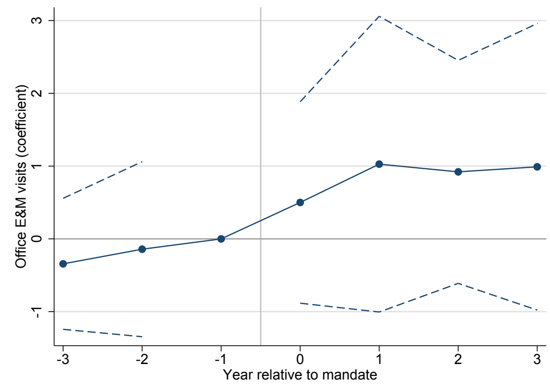
The event study estimates based on Medicaid infants show that the post-mandate effects are the smallest in year 0, which is partly because of the lowest rate of MMC enrollment in year 0 (Figure 3). It also suggests that it may take time to adopt the new delivery system or to invest in routine care. The effects stay roughly the same during the next two years

Figure 4: Event study estimates, infants

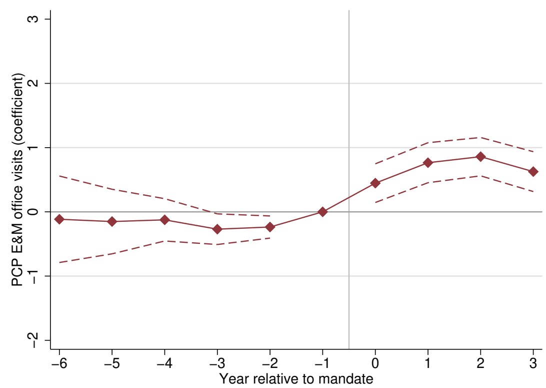
(a) E&M office visits, Medicaid



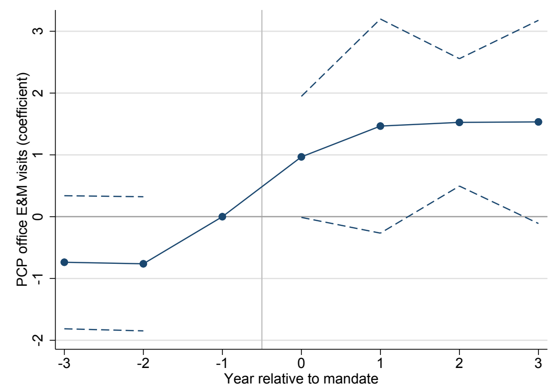
(b) E&M office visits, private



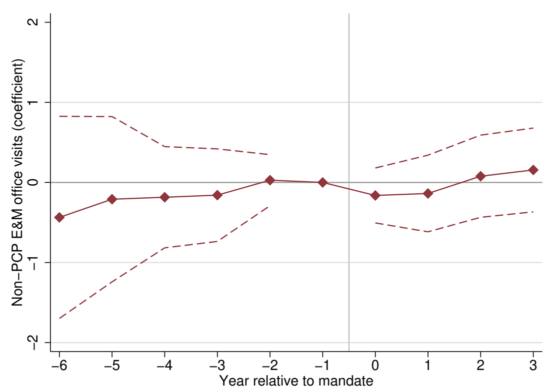
(c) PCP E&M office visits, Medicaid



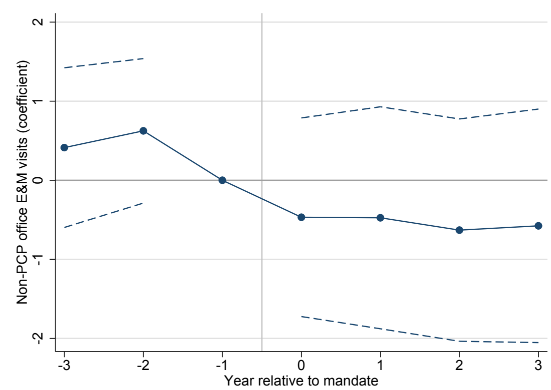
(d) PCP E&M office visits, private



(e) Non-PCP E&M office visits, Medicaid



(f) Non-PCP E&M office visits, private

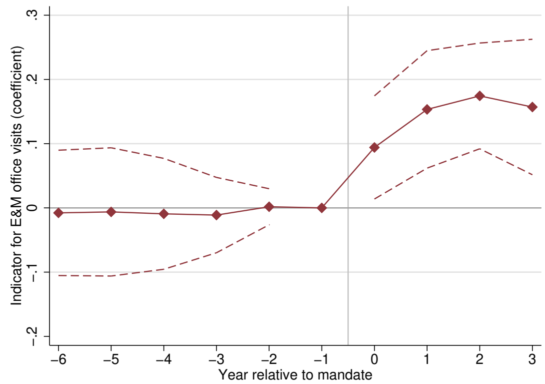


Notes: Each panel plots β_js from equation (2) for each dependent variable. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women.

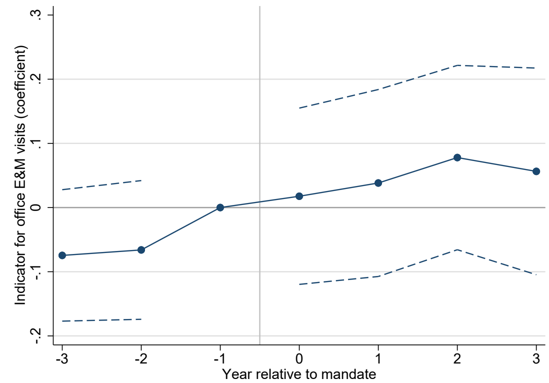
(years 1 and 2) but decline slightly the following year. While the reductions in year 3 may suggest that the effects of MMC may be short-lived, I am unable to investigate medium- to

Figure 5: Event study estimates, infants, extensive margin measures

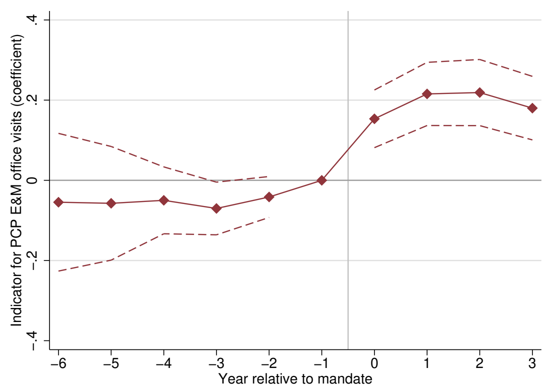
(a) Probability of an E&M office visit, Medicaid



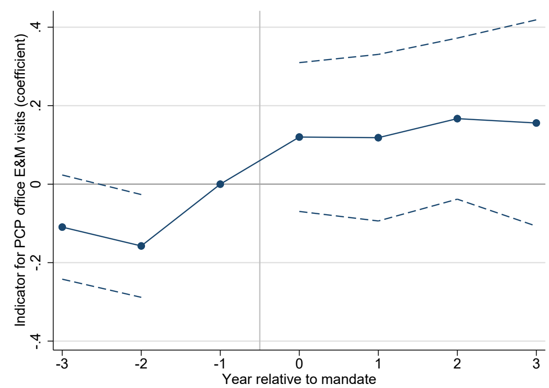
(b) Probability of an E&M office visit, private



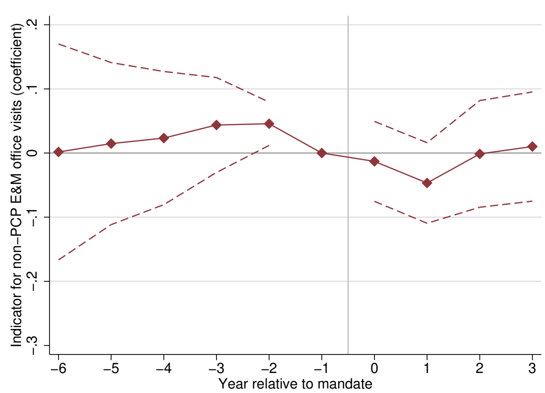
(c) Probability of a PCP E&M office visit, Medicaid



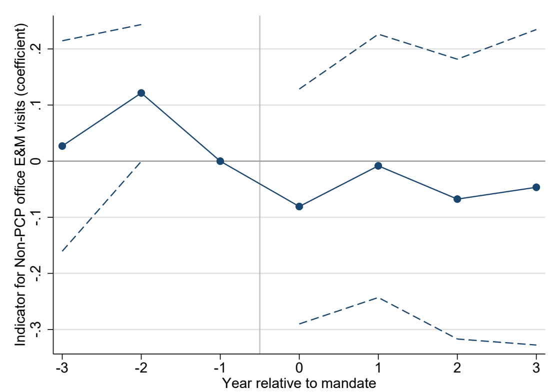
(d) Probability of a PCP E&M office visit, private



(e) Probability of a non-PCP E&M office visit, Medicaid



(f) Probability of a non-PCP E&M office visit, private



Notes: Each panel plots β_js from equation (2). The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women.

long-run effects without additional years of data. The event study estimates based on the privately insured are noisier but reveal a similar pattern: a growth in the first year and a plateau in the following years.

In an additional analysis, I consider two broader measures of care: (1) any office visit with a PCP and (2) any office visit with a specialist. I define a specialist as a provider whose specialty is something other than family practice, pediatrics, internal medicine, or obstetrics and gynecology. Column (1) of Appendix Table B.5 shows that both the number and the probability of PCP office visits increased following the mandate for Medicaid infants, further highlighting that the transition to MMC improved access to routine care. However, the number and the probability of specialist office visits decreased (column (2)), suggesting an overall decrease in encounters with specialists following the MMC mandate. These patterns are consistent with managed care plans emphasizing routine care while restricting specialty care. I find the same signed spillovers to privately insured infants (columns (3)-(4)), but the estimates are generally insignificant. With an increase in PCP care but a decrease in specialty care, welfare effects of the MMC mandate are ambiguous.

Moreover, I examine whether managed care influenced other aspects of provider practice, such as adjustments to visit lengths or the number of prescriptions (Glied and Zivin 2002). Appendix Table B.6 shows no significant changes in these outcomes for both Medicaid and privately insured individuals, suggesting that the adoption of managed care in Medicaid did not affect some of the measures previously found to be impacted by the overall penetration of managed care organizations (Glied and Zivin 2002). Instead, I find that the transition to MMC increased the number of routine outpatient visits, consistent with a previous study that found increased outpatient utilization under MMC in Texas (Layton et al. 2022). To further understand the impact of managed care on patients, I explore several measures of quality and health in Section 5.2.2.

5.2.2 Measures of quality and health

Building on these utilization outcomes, I further investigate changes in care quality or health following the transition to MMC. I consider three outcomes: immunizations, ED visits, and inpatient visits. Table 2 and Appendix Figures A.1-A.2 summarize the findings. I find 0.4 more immunizations, or a 27% increase in immunizations, among Medicaid infants, suggesting an improvement in quality following the MMC mandate. Appendix Figure A.1(a) plots the corresponding event study estimates, which show little evidence of pre-trends. Additionally, I investigate the probability of receiving any immunization as an outcome and find a large increase following the transition (column (4) of panel A2 in Table 2). The corresponding event study (Appendix Figure A.2(a)) also shows a large increase in the probability of receiving immunizations following the mandate, suggesting that the transition to MMC did result in some meaningful improvement in infants' health care as measured by increased immunizations.

I then examine whether higher quality translated into fewer ED visits or fewer inpatient visits. I find that the transition to MMC *increased* ED visits by 0.124, or by 20%, which is inconsistent with an improvement in quality. However, the corresponding event study graph (Appendix Figure A.1(c)) shows that the increase in ED visits is driven by fewer ED visits in treatment counties relative to control counties before the mandate, followed by small and insignificant increases after the mandate. This suggests caution in interpreting the increase in ED visits as causal. Additionally, I find no significant change in the probability of having any ED visit (column (5) of panel A2 in Table 2 and Appendix Figure A.2(c)). Appendix Section D further investigates ED visits, including the types of ED visits (e.g., emergent, non-emergent), and finds that the estimates are highly sensitive to model specifications. Lastly, while an increase in PCP visits may prevent avoidable hospitalizations, I find no evidence that the MMC mandate changed the number of inpatient visits in the first year of Medicaid infants' lives.

For infants with private coverage, I do not find any evidence that the MMC mandate increased immunizations or reduced the number of ED or inpatient visits, suggesting no change in quality or measurable health. The lack of increased immunizations for infants with private insurance, unlike the increase in routine visits, is likely due to the already high baseline immunization rates among the privately insured. As shown in Table 1, the average number of immunizations among privately insured infants is 3.5, significantly higher than the 1.3 observed among Medicaid infants. Since infants require a set number of immunizations each year, it is likely that more privately insured infants are already fully up to date, leaving little room for additional immunizations.

Additionally, I test whether the increase in PCP visits for privately insured children affected total provider payments. Examining total pay as an outcome, I find no significant impact of the transition to MMC on total pay. The DD estimate for total pay is -$286.4 with a standard error of $388.1. The baseline mean of total pay for infants less than 1 year old is $2,142. This suggests that the increase in routine visits for privately insured children did not impact the overall payment made by private insurers.

Taken together, these results suggest that while there is evidence of same-signed spillovers in routine care, infants with private insurance did not benefit from increased emphasis on preventive medicine. The existing gap in coverage at the baseline offers a potential explanation. At baseline, privately insured infants have almost twice as many E&M office visits as Medicaid infants, and they receive more than twice as many immunizations, suggesting

Table 3: Effects of the MMC mandate on pregnant women's health care utilization

	(1) Prenatal care visits	(2) OB-GYN prenatal care visits	(3) Non-OB-GYN prenatal care visits	(4) ED visits	(5) Inpatient visits
Panel A. Medicaid					
MMC mandate	1.742***	1.040**	0.762***	0.160	-0.005
	(0.451)	(0.422)	(0.141)	(0.104)	(0.007)
Observations	79452	79452	79452	79452	79452
Mean before mandate	4.152	1.124	3.141	1.007	0.083
Panel B. Private insurance					
MMC mandate	0.776	0.961*	-0.188	0.012	0.010
	(0.723)	(0.479)	(0.518)	(0.192)	(0.023)
Observations	3200	3200	3200	3200	3263
Mean before mandate	6.630	3.063	3.907	0.867	0.040

Notes: Each cell shows an estimated β from equation (1) for each dependent variable. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. * Significant at 10%, ** significant at 5%, *** significant at 1%.

that the marginal return to additional PCP visits or further improvement in preventive care is low.

5.3 Effects of the Medicaid managed care mandate on pregnant women

In this section, I examine the effects of the transition from FFS to MMC on pregnant women. Table 3 presents the DD estimates for various utilization measures, separately for Medicaid and private insurance. Panel A shows that the MMC mandate increased prenatal care visits by 1.7 (or 42% when evaluated at the sample mean) for pregnant women with Medicaid.

I further assess whether this increase in prenatal care visits is clinically meaningful using two commonly used measures of adequacy, which evaluate care based on the timing of the initial prenatal visit and the total number of visits during pregnancy. The Kessner Index (Kessner 1973) classifies prenatal care as adequate if it begins in the first trimester with nine or more visits. Similarly, the Kotelchuck Index (Kotelchuck 1994), also known as the Adequacy of Prenatal Care Utilization (APNCU) Index, defines adequate prenatal care as receiving 80-109% of the expected visits. For a full-term birth (40 weeks) with prenatal care initiated in the first trimester, 11 to 15 visits (out of the recommended 14) are considered adequate according to the Kotelchuck Index. To evaluate whether my findings align with improvements in prenatal care adequacy, I construct three indicators, each representing the number of prenatal visits exceeding 9, 11, and 14. For simplicity, I assume full-term births and do not incorporate the timing of the initial prenatal care visit. Across all three measures, I find significant increases in adequacy prenatal care visits for pregnant women on Medicaid (Appendix Table B.7). This suggests that the transition to MMC may have contributed to improving prenatal care adequacy.

The large increase in the total number of visits was driven by visits to both OB-GYNs and non-OB-GYNs, suggesting that MMC broadened access to care for pregnant women with Medicaid. The increase in non-OB-GYN visits also highlights that managed care did not bluntly reduce specialist care and may have provided improved access to specialists for certain populations.

In panel B, I test whether MMC also affected privately insured pregnant women. For pregnant women with private coverage, the MMC mandate increased prenatal care visits with OB-GYNs by 1, or by 31%. The increase is similar in magnitude to the effect on pregnant women with Medicaid, and the difference is not statistically significant. However, I do not find a significant change in the total number of prenatal visits for pregnant women with private coverage following the mandate. This is in part due to a negative but insignificant effect on non-OB-GYN visits. These results suggest that the same-signed spillover in prenatal care with OB-GYNs did not affect the total amount of prenatal care that pregnant women with private coverage received, which is the same pattern that I observe for infants with private coverage.

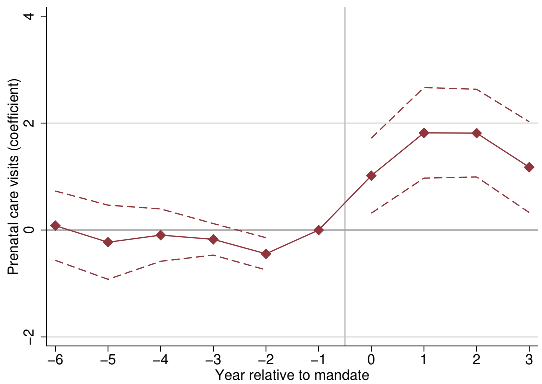
Figure 6 shows event study estimates for prenatal care visits. Panel (a) shows no evidence of a pre-trend but a sharp increase in prenatal visits for pregnant women with Medicaid. Panels (c) and (e) confirm that the mandate increased prenatal care visits with both OB-GYNs and non-OB-GYNs for pregnant women with Medicaid coverage. Panel (b) shows no sign of a pre-trend and provides suggestive evidence that prenatal care visits also increased for pregnant women with private coverage. The increase was driven by OB-GYN visits (panel (d)), consistent with DD estimates.

Columns (4)-(5) of Table 3 show the effects of the MMC mandate on ED and inpatient visits separately for pregnant women with Medicaid and those with private insurance. For both groups, I find that the MMC mandate had no effect on ED visits or hospitalizations, suggesting that increased access to prenatal care did not have significant impacts on observable health.

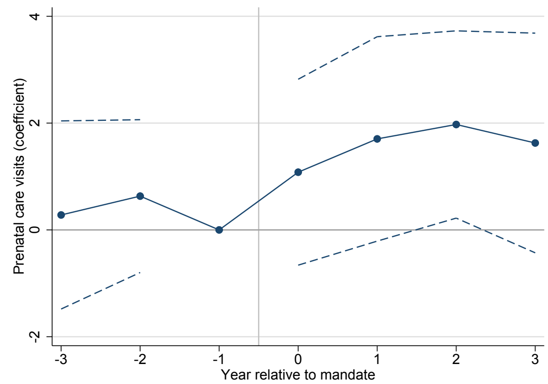
Appendix Table B.8 and Appendix Figure A.3 present the regression estimates and event study figures for binary versions of the prenatal care outcomes, respectively. While the

Figure 6: Event study estimates on prenatal care visits, pregnant women

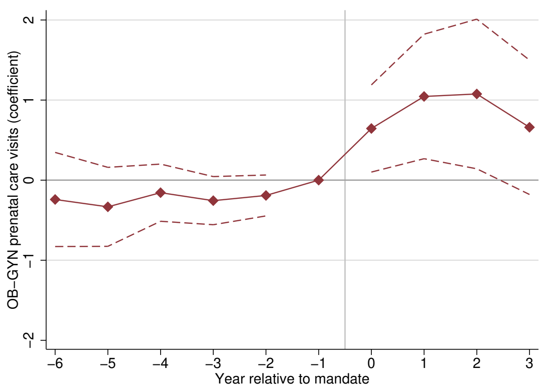
(a) Prenatal care visits, Medicaid



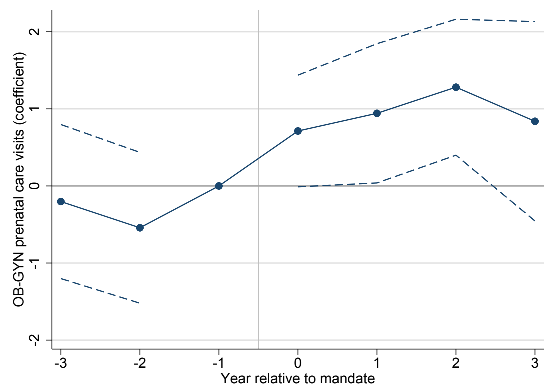
(b) Prenatal care visits, private



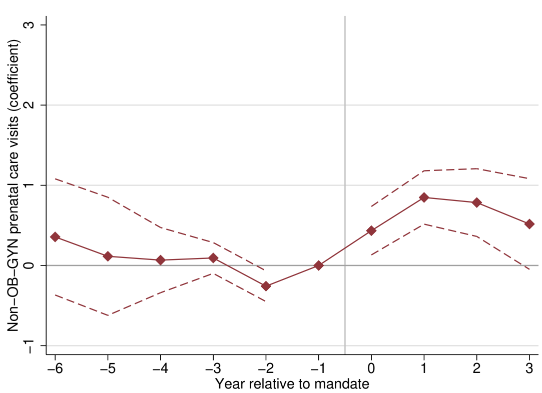
(c) OB-GYN prenatal care visits, Medicaid



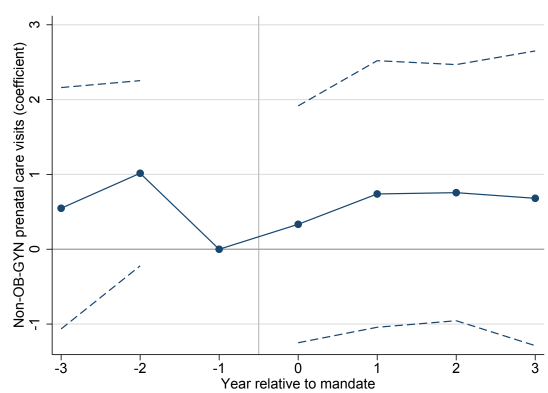
(d) OB-GYN prenatal care visits, private



(e) Non-OB-GYN prenatal care visits, Medicaid



(f) Non-OB-GYN prenatal care visits, private



Notes: Each panel plots β_js from equation (2) for each dependent variable. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women.

Table 4: Heterogeneous effects of the MMC mandate on the privately insured

	(1)	(2)	(3)	(4)
	Infants' PCP E&M office visits		Women's OB-GYN prenatal care visits	
	Low income	High income	Low income	High income
Panel A. By local median income				
MMC mandate	2.488***	1.248**	1.593***	0.546
	(0.240)	(0.514)	(0.318)	(0.590)
Observations	1128	1343	1432	1730
Mean before mandate	3.619	3.577	2.933	3.108

Notes: Each cell shows an estimated β from equation (1) for each dependent variable. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. * Significant at 10%, ** significant at 5%, *** significant at 1%.

extensive margin responses for pregnant women with Medicaid are large and significant, I find insignificant effects for pregnant women with private coverage. Similar to the findings for infants, this further suggests that intensive margin responses tend to drive the spillovers on the private market.

To summarize, there is evidence of the same-signed spillovers in preventive medicine (e.g., OB-GYN prenatal care visits), with no change in the total amount of private health care. I also find no effect of the MMC mandate on total provider reimbursements for pregnant women with private coverage. With an average total reimbursement of $3,679, the effect of the MMC mandate on total pay is $210.7 with a standard error of $513.9.

5.4 Mechanisms

I examine several mechanisms behind the same-signed spillovers in this section. First, to test whether the transition to MMC affected providers' general practice style, I examine heterogeneous effects by local median income at the three-digit zip code level.[13] If the similar increase in routine care between Medicaid and private insurance is due to MMC shifting provider practice style, the effect should be concentrated in low income areas where physicians see many Medicaid patients.

Columns (1) and (2) of Table 4 compare the effects of the MMC mandate on PCP E&M office visits for infants with private coverage between below and above median income areas.

13. I obtain median income at the five-zip code level from the 2006-2010 American Community Survey. Because the most granular level of geographic information available in MarketScan is the three-digit zip code, I calculate the average median income at the three-digit zip code level for this heterogeneity analysis with MarketScan.

I find that the increase in PCP visits was larger in low-income areas, which is consistent with physicians updating their practice style when they see a lot of Medicaid beneficiaries. Similarly, columns (3)-(4) examine how the effects on OB-GYN prenatal care visits for pregnant women with private coverage varied by local income. Consistent with spillovers arising through providers' general practice style, the increase in OB-GYN visits during pregnancy was also concentrated in low-income areas.

In an alternative specification, I use the size of the Medicaid population to examine whether the spillover effect on private insurance is driven by areas with a large Medicaid population. Appendix Table B.9 shows that the increase in routine care is concentrated in counties with larger Medicaid populations, further supporting the idea that providers update their practice style when a large share of their patients are affected by the transition to MMC.

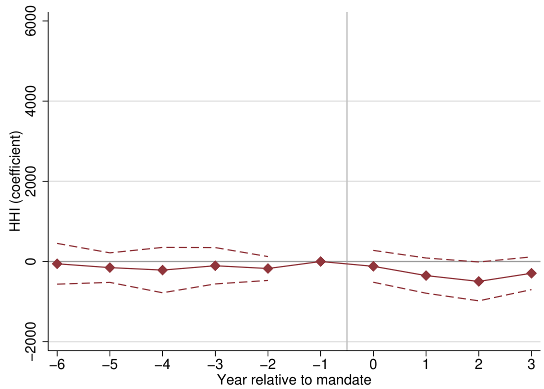
Second, the transition to MMC may affect the allocation of patients across providers. For a subgroup of observations with a non-missing provider identifier, I examine how the provider Herfindahl-Hirschman Index (HHI) changed following the mandate. HHI is a common measure of market concentration with a higher value indicating a more concentrated market. Figure 7(a) provides suggestive evidence that HHI for E&M office providers for Medicaid infants decreased following the mandate, indicating a lower level of market concentration. In other words, this suggests that Medicaid infants saw a broader group of providers following the transition to MMC. Consistently, panel (c) shows that the number of new E&M office providers increased in the Medicaid claims data, suggesting that more providers treated Medicaid infants following the transition. That is, this suggests that MMC infants gained access to physicians who typically would not have provided care to Medicaid patients. Columns (1)-(2) of Appendix Table B.10 show the corresponding DD estimates.

It is possible that a change in the way provider identifiers are reported after the mandate may explain the increase in the number of providers, even without an actual increase in providers. To investigate this issue, I identify a subgroup of providers who show up in both FFS and MMC claims. I find that most providers (86%) have both FFS and MMC claims, suggesting that FFS and MMC are unlikely to report provider identifiers differently. I then estimate the effect of the MMC mandate on the number of such providers who treat both types of Medicaid patients. Point estimates suggest increases in the number of these providers who treat both types of Medicaid patients (Appendix Table B.11), although they are insignificant. However, the magnitudes are similar to the main estimates reported in the second column of Appendix Table B.10. This suggests that a change in how provider identifiers are reported is unlikely to drive the increase in the number of providers.

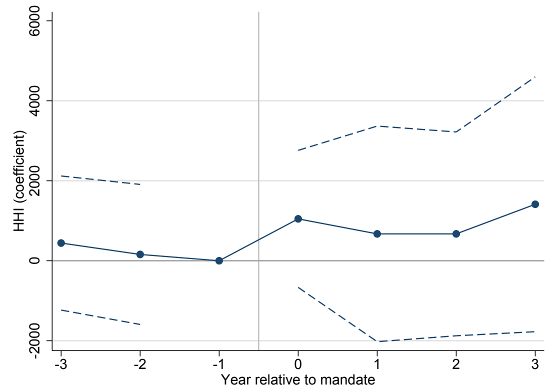
Turning to the provider market under private insurance, Figure 7(b) shows that provider

Figure 7: Event study estimates for HHI and provider counts, infants

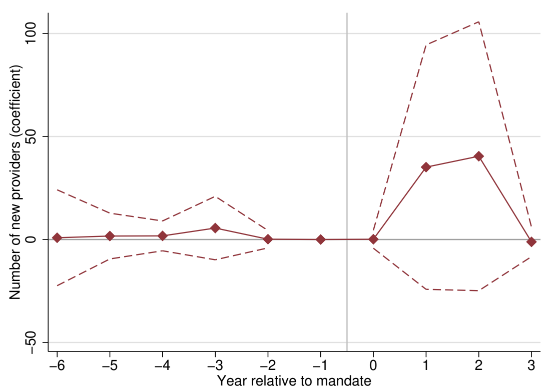
(a) HHI for E&M office providers, Medicaid



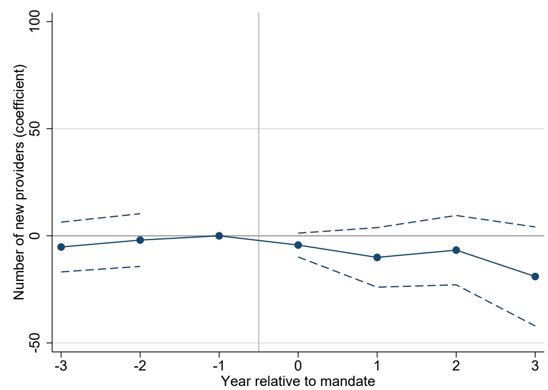
(b) HHI for E&M office providers, private



(c) Number of new E&M office providers, Medicaid



(d) Number of E&M office providers, private



Notes: Each panel plots β_js from equation (2) for each dependent variable. HHI refers to the Herfindahl–Hirschman Index, which is a common measure of market concentration. The unit of observation used in the analysis is at the county-year level.

HHI did not change for infants with private coverage following the mandate, indicating that MMC was unlikely to affect where privately insured patients got care. Panel (d) shows that the size of the provider pool also did not change for infants with private coverage. Appendix Figure A.4 shows similar patterns in HHI and provider counts for pregnant women. Columns (3)-(4) of Appendix Table B.10 summarize the corresponding DD estimates. These results provide suggestive evidence that patient sorting is unlikely to be the primary mechanism behind the increase in routine care for the privately insured. However, note that provider identifiers in MarketScan have a lot of missing values and the null results may be due to measurement error. Moreover, I am unable to completely rule out changes in providers and patients that are not captured by HHIs. To see if patient sorting explains the spillover effect, I also examine changes in patient composition in Section 6.1.

As a third mechanism, I examine whether the MMC mandate affected the use of imaging services to test spillovers through changes in technology adoption. Because I do not have a direct measure of health care investment or technology adoption, I examine utilization of imaging services as an indirect test of this mechanism. Appendix Table B.12 show the effects of the MMC mandate on imaging claims. For both infants and pregnant women with Medicaid coverage, I do not find evidence that the MMC mandate reduced the number of imaging claims. If anything, the number of imaging claims increased for pregnant women with Medicaid, which is not consistent with the prediction that managed care would reduce the use of high-cost technologies. Moreover, the MMC mandate did not affect the number of imaging claims for infants and pregnant women with private coverage.

Fourth, I test whether opposite-signed spillovers arise due to capacity constraints. In months where many Medicaid patients saw physicians for routine care, was the spillover effect attenuated for patients with private coverage? Panel A of Appendix Table B.13 compares the effect of the MMC mandate on PCP E&M office visits for infants with private coverage by the monthly number of Medicaid visits. Specifically, I calculate the number of monthly E&M office visits by Medicaid infants for each county. I then divide the sample of infants with private coverage into two based on the median number of monthly Medicaid visits. Note that this is a crude proxy of crowdedness given that I do not have information on full capacity. My approach differs from previous literature that, for instance, uses the number of empty beds (Freedman 2016) or changes in staffing shortages (Harris et al. 2020) to measure capacity constraints. I find that the increase in PCP E&M office visits was larger in "more crowded" months, i.e., months with above-median Medicaid visits, indicating no evidence of opposite-signed spillovers due to capacity constraints. Columns (3)-(4) of panel A similarly compare the effect of the MMC mandate on OB-GYN office visits for pregnant women with private coverage by crowdedness. I find a similar increase in OB-GYN prenatal care visits between less crowded and more crowded months, suggesting that capacity constraints played a minimal role.

To see if seasonality drives the increased number of visits among the privately insured when there are more Medicaid visits, I conduct a similar heterogeneity analysis by crowdedness in "non-flu months" defined as March through August. As shown in panel B of Appendix Table B.13, I still find no evidence of a negative spillover effect in crowded non-flu months. Moreover, panel A of Appendix Table B.6 test and find little evidence that minutes per office visit decreased following the mandate. Taken together, I find no evidence of opposite-signed spillovers due to capacity constraints.

Lastly, I examine if prices changed following the MMC mandate. Because I do not observe prices under MMC, I am unable to test direct effects on prices. Instead, I indirectly test whether prices in private insurance changed following the mandate. Appendix Table B.14 presents the DD estimates for the average per-procedure insurer payments. Column (1) examines the average prices for procedures provided at PCP E&M office visits for infants with private coverage. I find no significant changes in prices for these visits. Column (2) presents the effect of the MMC mandate on the average prices for procedures provided at OB-GYN prenatal care visits during pregnancy for women with private coverage. Again, I do not find any evidence that prices changed following the mandate. That said, I acknowledge that it is plausible the change in Medicaid prices affected provider behavior and could have contributed to the increase in routine care for Medicaid populations following the mandate. However, the lack of change in private prices suggests that they are unlikely to explain the positive spillover effect.

Taken together, my analyses confirm two key channels through which MMC increased routine care among Medicaid enrollees: changes in provider practice style and an increase in the number of providers participating in Medicaid. Additionally, an increase in provider prices likely influenced this outcome for Medicaid patients, although I am unable to directly test this hypothesis due to a data limitation. However, the only channel that also explains the same-signed spillover effect is the within-provider change in practice style.

Consistent with the primary mechanism—within-provider changes in practice style—I find that the main results for Medicaid beneficiaries remain robust to the inclusion of provider fixed effects.[14] For this analysis, I restructure the dataset into a provider-level panel by aggregating visit counts at the provider×beneficiary×county×year level. In columns (1) and (3) of Table 5, I present the difference-in-differences estimates without provider fixed effects using this provider panel for infants and pregnant women. Notably, these estimates are smaller than the main estimates based on the beneficiary-level panel, which is expected since beneficiaries see multiple providers. Columns (2) and (4) report the difference-in-differences estimates controlling for provider fixed effects. The estimates do not change much with the inclusion of provider fixed effects, suggesting that the increase in routine care is indeed driven by within-provider changes. In other words, the increase in routine care is not merely due to patients seeing more physicians or switching to a different set of physicians who provide more routine care. Instead, routine visits increase within individual providers, consistent with a change in provider practice style following the transition to MMC. Overall, these results confirm that the primary mechanism behind the positive spillovers from Medicaid to private insurance is the shift in within-provider practice style.

14. Since provider identifiers in MarketScan have a lot of missing values and are known to be unreliable, I do not conduct this exercise for the privately insured.

Table 5: Effects of the MMC mandate on Medicaid patients, using a provider-level panel

Sample:	Infants		Pregnant women	
Dependent variable:	PCP E&M office visits		OB-GYN prenatal care visits	
	(1)	(2)	(3)	(4)
MMC mandate	0.106***	0.121***	0.052**	0.049**
	(0.018)	(0.024)	(0.025)	(0.018)
Observations	591133	543078	929381	873007
Mean before mandate	0.115	0.115	0.110	0.110
Provider fixed effects	N	Y	N	Y

Notes: I construct a provider panel by collapsing the raw outpatient records at the provider × beneficiary × county × year level. Odd-numbered columns show an estimated β from equation (1) for each dependent variable. Even-numbered columns report the difference-in-differences estimates controlling for provider fixed effects. * Significant at 10%, ** significant at 5%, *** significant at 1%.

6 Robustness checks

6.1 Changes in patient composition

In this section, I examine a potential threat to identification by examining changes in patient composition. First, I test whether the transition to MMC changed the number of enrollees in each insurance type. If the transition to MMC made Medicaid more or less appealing to certain families, the mandate might have changed who enrolled in Medicaid (Currie and Fahr 2004). Then, some of the effects that I estimate may be due to changes in patient composition rather than changes in physician practice style. I thus test this hypothesis by examining the number of enrollees in each insurance type. Appendix Table B.15 shows the effects of the MMC mandate on the county-year level number of enrollees in Medicaid and private insurance for both infants and pregnant women. I find no evidence that the number of enrollees changed for any of the groups, suggesting that there is little concern that my results are driven by compositional changes in enrollees.

However, the characteristics of enrollees can change even without a change in the number of enrollees. I thus consider various characteristics of enrollees as outcomes in the difference-in-differences framework. Specifically, I examine infant's sex, maternal age, maternal utilization of inpatient and ED care in the year prior to pregnancy as a proxy for baseline health, and zip-code level income. The results are shown in Appendix Table B.16. I find no changes in prior health utilization of mothers and zip-code level median income following the mandate. However, I find that the MMC mandate increased the share of girls in Medicaid by 1.1 percentage point (or by 2%) and the average age of pregnant women in Medicaid by 0.14 year (or by 0.5%). The magnitudes are small but the significant effects in patient

Table 6: Robustness to different specifications

	(1)	(2)	(3)	(4)
	Infants PCP E&M office		Pregnant women OB-GYN prenatal care	
	Medicaid	Private	Medicaid	Private
Panel A. Main specification				
MMC mandate	0.820***	1.490***	1.040**	0.961*
	(0.124)	(0.507)	(0.422)	(0.479)
Observations	101522	2477	79452	3200
Mean before mandate	0.638	3.797	1.124	3.063
Panel B. Adding controls				
MMC mandate	0.821***	1.481**	1.046**	0.913*
	(0.124)	(0.544)	(0.422)	(0.444)
Observations	101522	2477	79452	3200
Mean before mandate	0.638	3.797	1.124	3.063
Panel C. Restricting to those who are continuously enrolled				
MMC mandate	1.431***	1.946**	1.362***	1.328**
	(0.342)	(0.768)	(0.451)	(0.524)
Observations	7601	1616	32254	1976
Mean before mandate	1.273	4.543	1.759	3.550
Panel D. Callaway and Sant'Anna (2021)				
MMC mandate	0.739***	1.525***	0.973**	1.194***
	(0.137)	(0.622)	(0.305)	(0.537)

Notes: In panel A, each cell shows an estimated β from equation (1) for each dependent variable. Panel B adds patient characteristics to equation (1) . Panel C restricts the sample to those who are continuously enrolled and estimates equation (1). Panel D shows an estimated average treatment effect using the Callaway and Sant'Anna 2021 method. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. * Significant at 10%, ** significant at 5%, *** significant at 1%.

characteristics warrant a further investigation. I thus estimate event studies in Appendix Figure A.5 to examine pre-trends and dynamic effects. These figures show an increase in the estimates leading up to the mandate, suggesting pre-trends, and the estimates are small and generally insignificant after the mandate.

I further examine whether the main results for Medicaid are robust to including these various controls. Panel A of Table 6 re-produces the main estimates for reference. Columns (1) and (3) in panel B of Table 6 show that the estimates barely change with the controls, suggesting that observable changes in patient composition do not drive my results for Medicaid.

Similarly, I examine changes in enrollee composition for the privately insured. Panel B of

Appendix Table B.16 shows no change in any infant or maternal characteristics following the mandate for those enrolled in private insurance. To test whether potential changes in patient composition among the privately insured affect my main results, I include these controls in the estimations and find that the main results for the privately insured are robust (columns (2) and (4) in panel B of Table 6), again suggesting that observable changes in patient composition are unlikely to drive my findings.

Relatedly, Medicaid enrollees frequently churn in and out of Medicaid (Williams et al. 2022), and if the rate of churn varies across FFS and MMC, it might explain the difference in utilization. While the two groups—pregnant women and infants under age 1—have continuous Medicaid eligibility, not all stay on Medicaid during the entire of period of continuous eligibility. I calculate that the average length of enrollment for infants in Medicaid was 8.6 during the first 12 months. The average length of enrollment for pregnant women in Medicaid was 7.3 months during 10 months of pregnancy. As a robustness check, I restrict the sample to a subgroup of Medicaid enrollees who are continuously enrolled during the specified periods of continuous eligibility. Panel C of Table 6 reports the estimates for the subset of the continuously enrolled. I find that the results are robust even when I restrict to those who are continuously enrolled during the first year for infants and during pregnancy for pregnant women. Similarly, I estimate the main models for a subgroup of privately insured populations who are continuously enrolled during the same time periods. I also find that the estimates are robust to this restriction, suggesting that a potential difference in lengths of enrollment does not affect my results.

6.2 Alternative specification

To address remaining concerns regarding the TWFE specification, I examine the key outcome variables using the Callaway and Sant'Anna (2021) method. Using the "never-treated" (i.e., counties that adopted the mandate after 2010) as the comparison group, I compute difference-in-differences estimators. Panel D of Table 6 summarizes the results. Columns (1)-(2) show that the MMC mandate increased PCP E&M office visits for both infants with Medicaid and private coverage, and the estimates are very similar to my main TWFE estimates. Columns (3)-(4) examine the main outcomes for pregnant women using the the Callaway and Sant'Anna (2021) method. Consistent with the main results, I find that the MMC mandate increased OB-GYN prenatal care visits for both pregnant women with Medicaid and private coverage. These results together support the robustness of the same-signed spillovers in preventive medicine following the MMC mandate. In Appendix C, I further discuss the robustness of the results to using alternative methods, including an imputation-based approach and a stacked difference-in-differences approach.

6.3 Alternative hypotheses

One potential explanation for the increase in outpatient visits under MMC is differences in claim denial rates between FFS and MMC. Gottlieb et al. (2018) find that FFS tends to deny more claims than MMC. Since I only observe paid claims, an increase in claims could reflect fewer denials rather than more patient encounters. Unlike the remittance data used in Gottlieb et al. (2018), my data do not allow me to directly measure billing complexity (such as claim denials). Therefore, I use the number of paid claims per visit as an indirect test of this alternative explanation. To measure this, I count the number of claims per outpatient day, since outpatient visits typically do not extend beyond a single day. If the number of claims per outpatient day increased after the transition to MMC, it would suggest that MMC was more likely to approve claims for a given visit. However, I find no such change (Appendix Table B.17), suggesting that this channel is unlikely to drive the findings of this paper.

I also consider an alternative explanation for the same-signed spillovers. The spillovers could indicate providers using the same practice style for patients with the same insurer, instead of treating all patients the same based on a common practice style. To test this mechanism, I identify the largest MMC plan in each county using the enrollment records from New York state and whether it has employer business. I then stratify the private insurance sample by whether the county's dominant MMC insurer has employer business. If the results are driven by providers treating the patients from the same insurer the same, the increase in routine care should be pronounced in counties with a dominant MMC insurer with employer business. Panel C of Appendix Table B.13 summarizes the results. Note that the dominant MMC insurer in most counties does not have employer business. For infants, I find that the same-signed spillover effect (i.e., the increase in routine care) is driven by counties where the dominant MMC insurer does not have employer business. For pregnant women, the point estimate is larger in counties where the dominant MMC insurer has employer business than counties without such insurer, but the estimates are not statistically distinguishable. These findings suggest that the same-signed spillovers are unlikely to reflect providers using a common practice style for patients with the same insurer. Rather, it is consistent with providers using a common practice style for all patients.

Table 7: Effects of the MMC mandate by the county share of high spending plans

	(1)	(2)	(3)	(4)
	Infants' PCP E&M office visits		Women's OB-GYN prenatal care visits	
	Below median	Above median	Below median	Above median
MMC mandate	0.564**	1.080***	0.192	0.789*
	(0.233)	(0.120)	(0.202)	(0.416)
Observations	49942	51580	39018	40434
Mean before mandate	0.486	0.945	1.018	1.320

Notes: Each cell shows an estimated β from equation (1). The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. Please see the main text for the definition of high-spending plans. * Significant at 10%, ** significant at 5%, *** significant at 1%.

6.4 Heterogeneity and generalizability of main findings

Unlike the single insurer under FFS, multiple managed care plans participate in MMC. In this section, I examine whether the main effects vary by plan spending levels, motivated by Geruso et al. (2023), who found substantial variation in healthcare spending across plans. While Geruso et al. (2023) identified plan-specific spending effects using auto-assignments to MMC plans in New York, I do not have information on auto-assignment to plans or transaction prices (i.e., how much Medicaid paid for each managed care service encounter). As a result, I am unable to causally identify high-spending plans as done in Geruso et al. (2023). Instead, I conduct the following exercise to examine heterogeneous effects across plans with observably different spending levels.

First, I impute the cost of each outpatient visit under MMC using the FFS price. Second, I calculate the total (imputed) outpatient spending for each beneficiary×plan×year×month observation. Third, using this beneficiary×plan×year×month panel, I compute the average monthly outpatient spending for each managed care plan. Fourth, I divide the plans into four groups based on the quartiles of average monthly outpatient spending and define the "high-spending plans" as those in the top quartile. I then compute the share of enrollees in these high-spending plans for each county. Finally, I divide the main sample into two groups based on whether the patient's county has below-median or above-median share of enrollees in high-spending plans.

Table 7 summarizes the heterogeneous effects based on whether the county has a below-median or above-median presence of high-spending plans. For infants, I find that routine visits increase significantly in both areas, but the absolute magnitude is larger in counties with an above-median share of high-spending plans. For pregnant women, I find that the increase in the number of prenatal visits with OB-GYNs is much larger in counties with an above-median share of high-spending plans. Overall, these findings suggest that the effects of MMC may vary across plans, and that my finding of the increased utilization following the transition to MMC may not generalize to all plans.

7 Conclusion

This paper examines both direct and indirect effects of Medicaid managed care on infants and pregnant women. Examining the direct effects on Medicaid beneficiaries, I find that the transition to MMC increased the quantity and the quality of routine care. These findings align with recent studies highlighting higher quality in MMC (Layton et al. 2019; Lee and Vabson 2024), but contrast with evidence from other settings where MMC adoption led to declines in quality and health outcomes (Aizer et al. 2007; Kuziemko et al. 2018).

Moreover, this paper finds that Medicaid's adoption of managed care had a broader impact on the health care market through spillovers on private insurance. When Medicaid transitioned to a system that emphasizes preventive medicine, routine office visits increased for patients with Medicaid as well as for patients with private insurance coverage. I find that such spillovers were concentrated in low-income areas where physicians were likely to see many Medicaid patients. These results suggest that physicians face a significant cost of adjusting their practice style specific to each insurer and use a general practice style based on the average characteristics of their patients (Einav et al. 2020; Barnett et al. 2020). Additional analyses further suggest that within-provider changes in practice style are the key mechanism driving these positive spillover effects. I provide several pieces of evidence that are inconsistent with alternative explanations, such as patient sorting and changes in provider prices.

One important caveat of interpreting my findings is that New York may be different from other states in various ways. First, New York is one of many states that operate a quality incentive program. New York provides private plans with bonuses and privileges based on their performance on quality metrics. Managed care plans in states without such an incentive program might not face as strong an incentive to invest in quality as those in New York. Second, while New York has a generous Medicaid program overall in terms of its Medicaid spending per enrollee,[15] it pays particularly low fees to Medicaid providers compared to Medicare fees. For example, in 2019, New York was ranked 46th in terms of

15. In 2019, New York was ranked 10th in overall Medicaid spending per enrollee (https://www.kff.org/medicaid/state-indicator/medicaid-spending-per-enrollee/ was accessed on May 31, 2022).

Medicaid-to-Medicare fee ratios according to Kaiser Family Foundation.[16] Low Medicaid fees in New York suggest that there was likely room for managed care plans to improve provider reimbursement as well as quality relative to the public program. In addition, the heterogeneity analysis by plan spending levels suggests that the effects may also vary across managed care plans, even within New York.

Moreover, an important limitation of this study is that the two datasets—Medicaid claims and private insurance claims—cannot be linked. As a result, I am unable to identify providers who treat both types of patients or to calculate the share of patients in different insurance arrangements for a given provider. Ideally, a dataset that contains all-payer claims as well as uncompensated care would allow one to examine the complete picture of how providers respond to the MMC mandate in treating all types of patients. Such dataset would also enable a further examination of the mechanism by comparing the size of the spillovers across providers with varying shares of Medicaid patients. While it is beyond the scope of this paper, a future examination of such data to study spillovers between public and private insurers would be useful.

Nevertheless, this paper provides insights into how MMC operates and how changes in one insurer can affect others within a healthcare system where providers serve patients covered by multiple insurers. I find that a payment reform aimed at improving care for publicly insured individuals also generated positive externalities for privately insured patients. If an insurer does not fully internalize the benefits of an intervention for patients covered by other insurers, such interventions are likely to be underprovided. Conversely, if an insurer does not fully internalize the costs imposed on third parties, such interventions may be overprovided. These results underscore the importance of considering non-targeted patients when designing or evaluating public program reforms, both in terms of social benefits and social costs.

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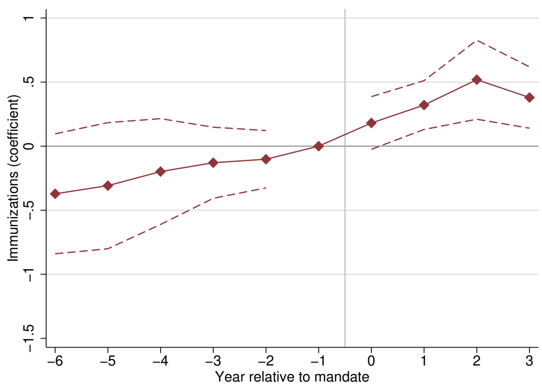
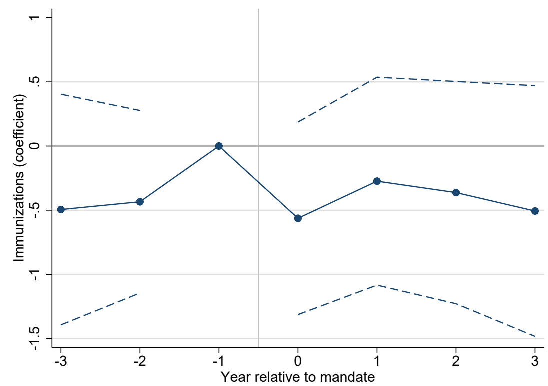
Appendix A. Appendix Figures

Figure A.1: Event study estimates, infants, other outcomes

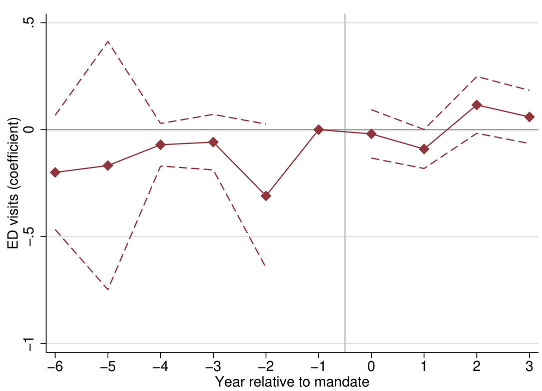
(a) Immunizations, Medicaid



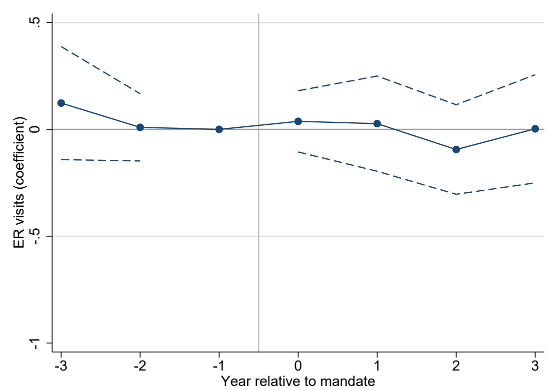
(b) Immunizations, private



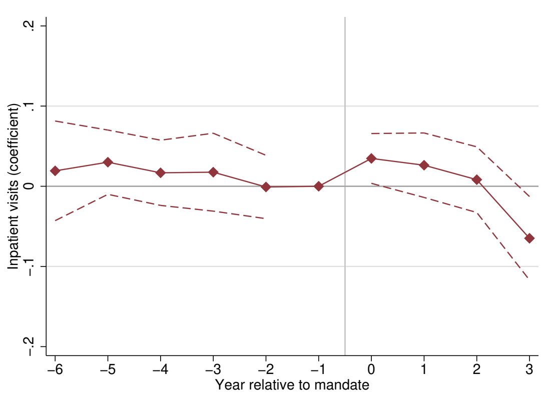
(c) ED visits



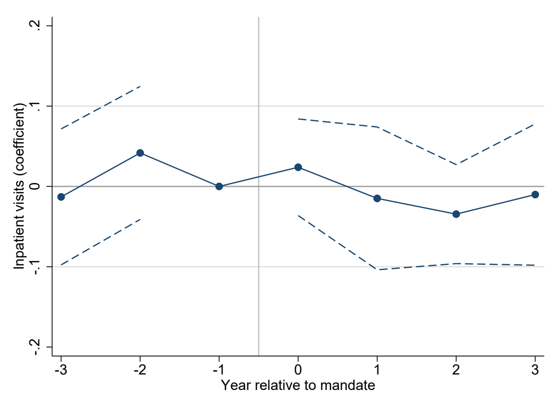
(d) ED visits



(e) Inpatient visits



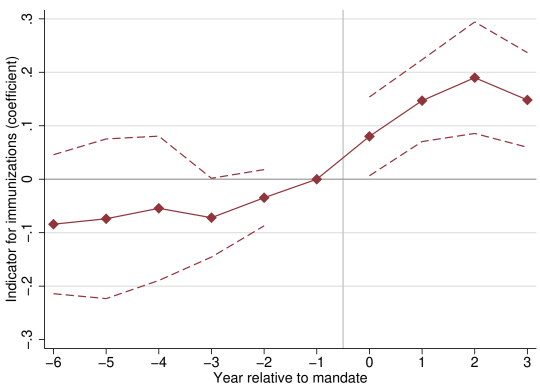
(f) Inpatient visits



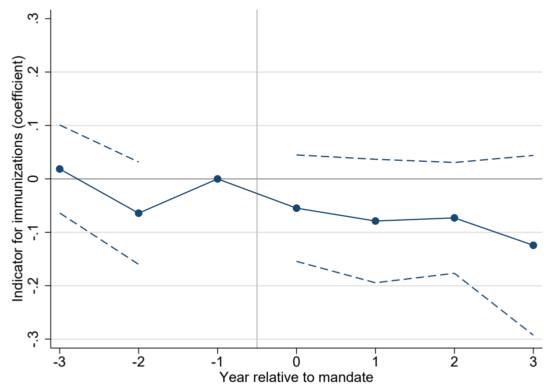
Notes: Each panel plots β_js from equation (2) for each dependent variable.

Figure A.2: Event study estimates, infants, other outcomes, extensive margin measures

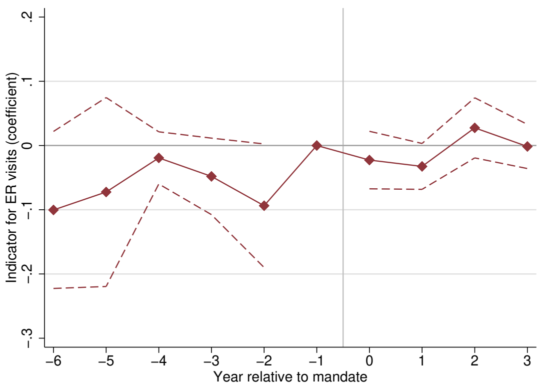
(a) Probability of an immunization, Medicaid



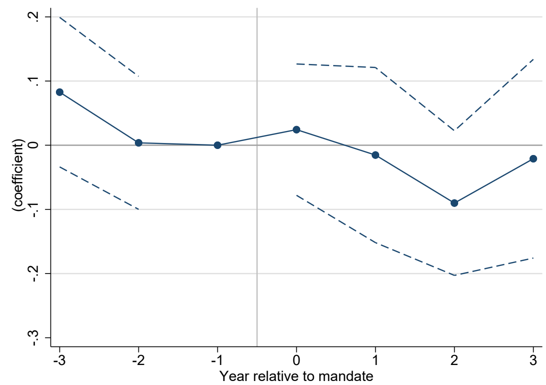
(b) Probability of an immunization, private



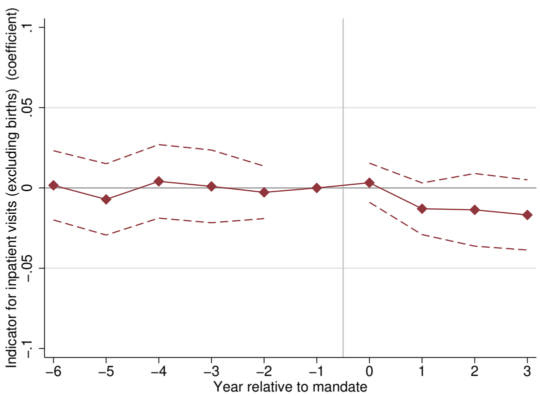
(c) Probability of an ED visit, Medicaid



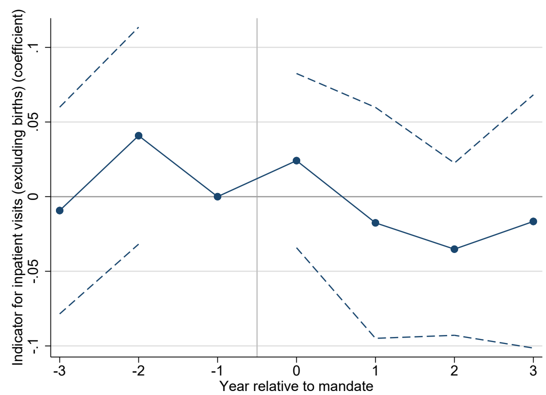
(d) Probability of an ED visit, private



(e) Probability of an inpatient visit, Medicaid



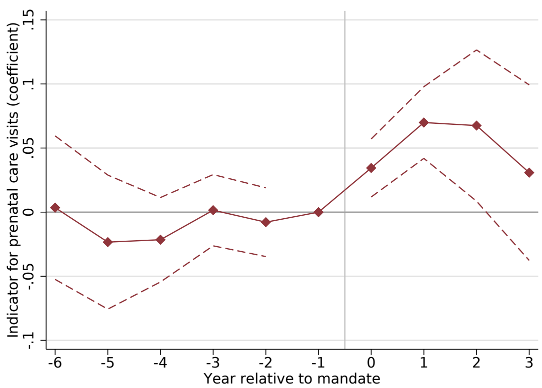
(f) Probability of an inpatient visit, private



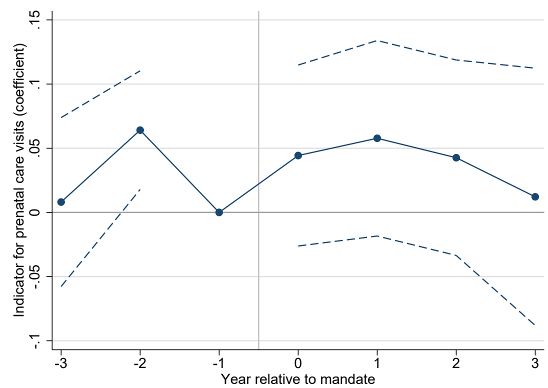
Notes: Each panel plots β_js from equation (2) for each dependent variable.

Figure A.3: Event study estimates on prenatal care visits, pregnant women, extensive margin measures

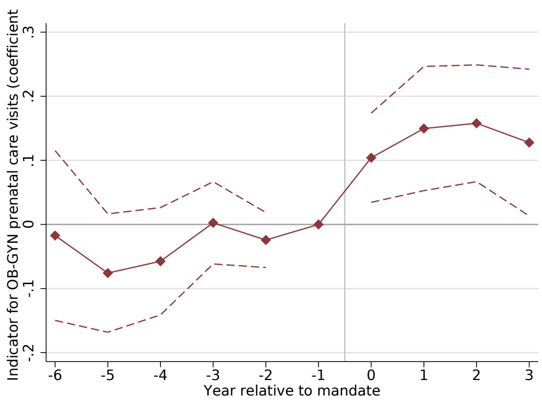
(a) Probability of a prenatal care visit, Medicaid



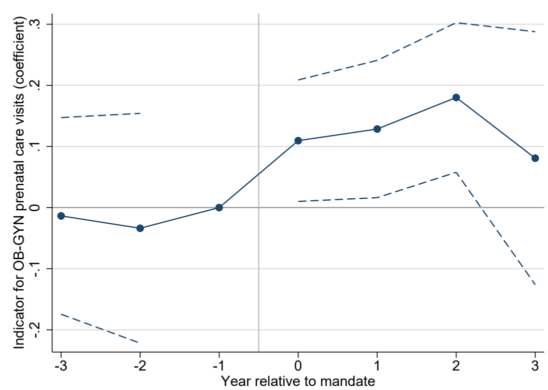
(b) Probability of a prenatal care visit, private



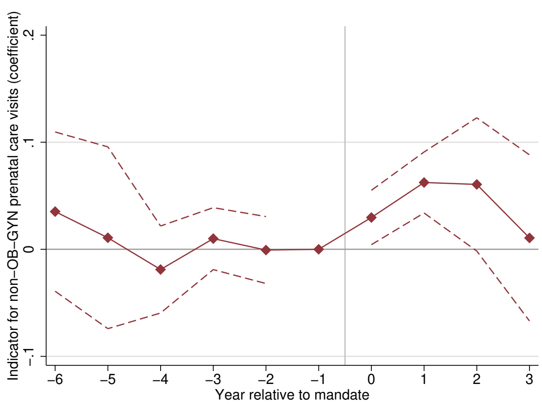
(c) Probability of a prenatal care visit with an OB-GYN, Medicaid



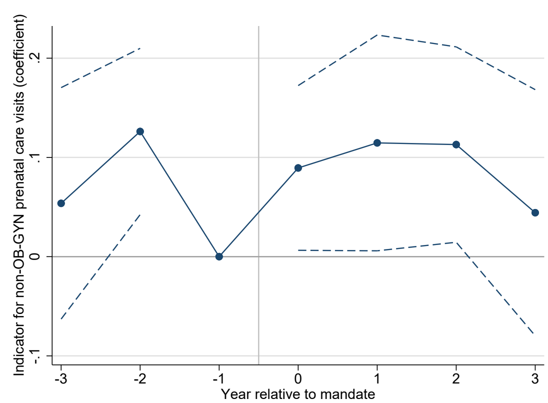
(d) Probability of a prenatal care visit with an OB-GYN, private



(e) Probability of a prenatal care visit with a non-OB-GYN, Medicaid



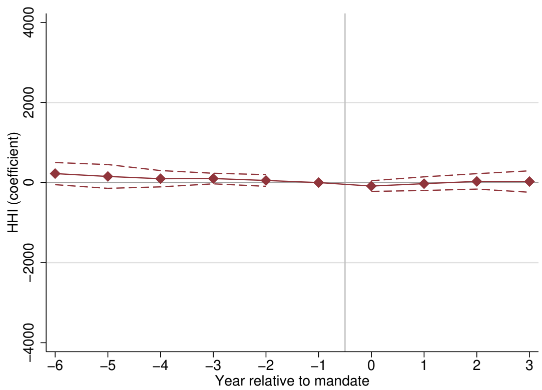
(f) Probability of a prenatal care visit with a non-OB-GYN, private



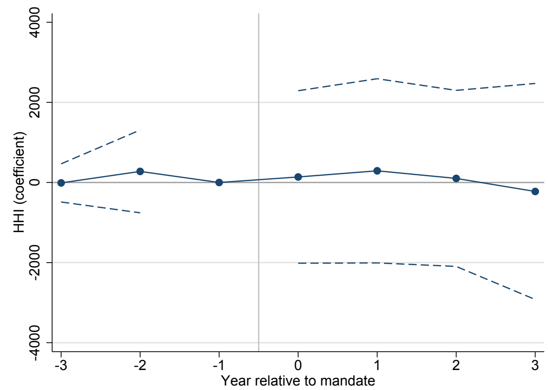
Notes: Each panel plots β_js from equation (2) for each dependent variable.

Figure A.4: Event study estimates for HHI and provider counts, pregnant women

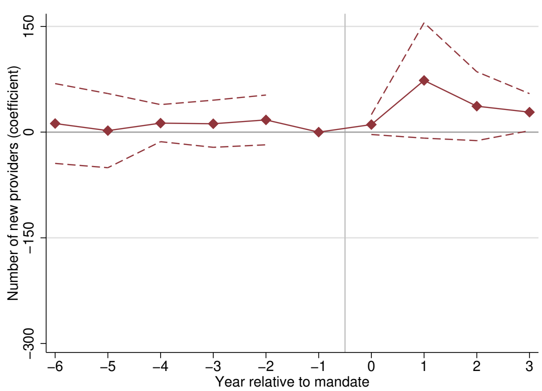
(a) HHI for prenatal care providers, Medicaid



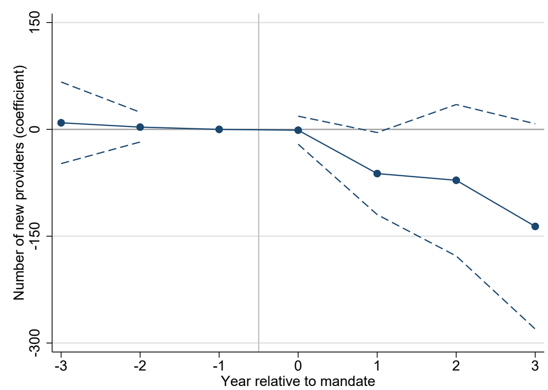
(b) HHI for prenatal care providers, private



(c) Number of prenatal care providers, Medicaid



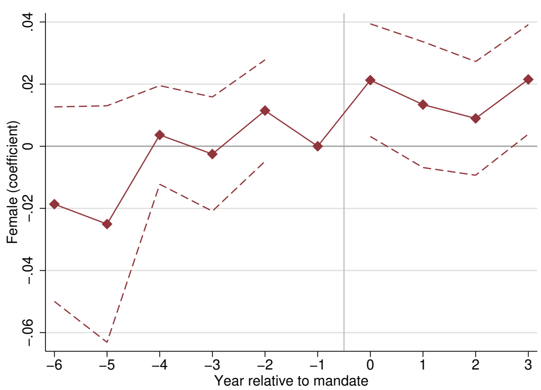
(d) Number of prenatal care providers, private



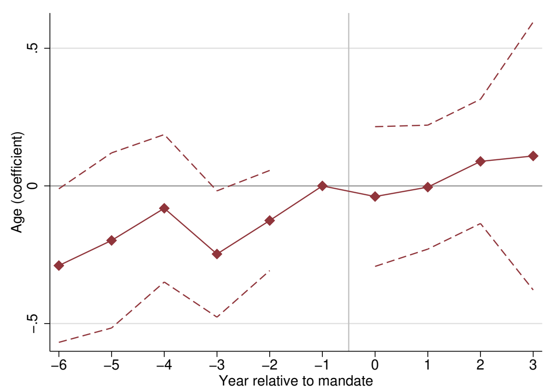
Notes: Each panel plots β_js from equation (2) for each dependent variable.

Figure A.5: Changes in characteristics of enrollees

(a) Share of girls, Medicaid



(b) Maternal age, Medicaid



Notes: Each panel plots β_js from equation (2) for each dependent variable.

Appendix B. Appendix Tables

Table B.1: Summary statistics before the MMC mandate, 2007-2010, race/ethnicity for Medicaid enrollees

	(1)	(2)
	Observations	Mean [SD]
Panel A. Infants		
White	25219	0.915 [0.279]
Black	25219	0.025 [0.156]
Hispanic	25219	0.008 [0.088]
Other race	25219	0.053 [0.223]
Panel B. Pregnant women		
White	16941	0.928 [0.255]
Black	16941	0.025 [0.152]
Hispanic	16941	0.009 [0.094]
Other race	16941	0.038 [0.187]

Notes: Standard deviations are in brackets. Patient race/ethnicity is not available for the privately insured.

Table B.2: Summary statistics before the MMC mandate, 2007-2010, by trimester for pregnant women

	(1)	(2)	(3)	(4)
	Medicaid		Private insurance	
	Observations	Mean [SD]	Observations	Mean [SD]
Panel A. All pregnant women				
Number of visits				
Prenatal care† visits	16967	5.038 [4.287]	3078	6.630 [5.261]
In the first trimester	16967	0.515 [1.074]	3078	1.201 [1.589]
In the second trimester	16967	2.359 [2.047]	3078	2.235 [1.992]
In the third trimester	16967	2.164 [2.551]	3078	3.194 [3.455]
Panel B. Restricting pregnant women who are continuously enrolled				
Number of visits				
Prenatal care visits	8027	6.647 [4.637]	1919	7.884 [5.261]
In the first trimester	8027	0.969 [1.361]	1919	1.817 [1.662]
In the second trimester	8027	3.113 [2.098]	1919	2.815 [1.826]
In the third trimester	8027	2.565 [2.772]	1919	3.252 [3.563]

Notes: † The following CPT codes are used to identify prenatal care: 59025, 59400, 59425, 59426, 59430, 59510, 59610, 59618, 76801-76817, 76818, 82105, 82106, 88271-88275, 88291.

Table B.3: Provider characteristics before the MMC mandate, 2007-2010

	(1)	(2)	(3)	(4)
	Medicaid		Private insurance	
	Observations	Mean [SD]	Observations	Mean [SD]
Panel A. Infants				
Family practice	19817	0.067 [0.278]	1183	0.065 [0.233]
Pediatrics	19817	0.067 [0.316]	1183	0.178 [0.367]
Internal medicine	19817	0.018 [0.148]	1183	0.019 [0.128]
OB-GYN	19817	0.044 [0.203]	1183	0.008 [0.086]
Panel B. Pregnant women				
Family practice	30142	0.066 [0.264]	2777	0.058 [0.223]
Pediatrics	30142	0.005 [0.076]	2777	0.008 [0.085]
Internal medicine	30142	0.051 [0.252]	2777	0.028 [0.159]
OB-GYN	30142	0.107 [0.344]	2777	0.139 [0.333]

Notes: Standard deviations are in brackets.

Table B.4: Effects of the MMC mandate on the average share of Medicaid patients in MMC, using provider panels

Sample:	Infants		Pregnant women	
	(1)	(2)	(3)	(4)
MMC mandate	0.300***	0.276***	0.291***	0.252***
	(0.034)	(0.034)	(0.031)	(0.026)
Observations	78485	78247	157463	157225
Mean before mandate	0.162	0.162	0.266	0.266
Provider fixed effects	N	Y	N	Y

Notes: I construct a provider panel by collapsing the raw outpatient records at the provider $\times$ beneficiary $\times$ county $\times$ year level. Each cell shows an estimated β from equation (1) for each dependent variable. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.5: Additional outcomes: other types of visits and services for infants

	(1)	(2)	(3)	(4)
	Medicaid		Private insurance	
	PCP office visits	Specialist office visits	PCP office visits	Specialist office visits
Panel A. Number of visits				
MMC mandate	2.116***	-0.495*	1.880*	-1.559
	(0.201)	(0.262)	(0.941)	(1.416)
Observations	101522	101522	2477	2477
Mean before mandate	1.016	3.363	7.707	2.297
Panel B. Probability of any visit				
MMC mandate	0.390***	-0.075**	0.106	-0.083
	(0.047)	(0.032)	(0.112)	(0.096)
Observations	101522	101522	2477	2477
Mean before mandate	0.281	0.601	0.813	0.381

Notes: Each cell shows an estimated β from equation (1) for each dependent variable. A 'specialist' is defined as any provider whose speciality is not family practice, pediatrics, internal medicine, or obstetrics and gynecology. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.6: Effects of the MMC mandate on other outcomes

	(1)	(2)	(3)	(4)
	Infants		Pregnant women	
	Medicaid	Private	Medicaid	Private
Panel A. Minutes per visit				
MMC mandate	-0.274	0.363	-0.483	1.504
	(0.498)	(0.710)	(0.921)	(1.337)
Observations	66629	2229	43210	2288
Mean before mandate	17.567	16.090	19.102	17.618
Panel B. Number of prescriptions				
MMC mandate	0.104	0.282	0.212	0.659
	(0.094)	(0.517)	(0.156)	(0.802)
Observations	101522	2477	79452	3200
Mean before mandate	2.902	2.404	4.556	3.414

Notes: Each cell shows an estimated β from equation (1) for each dependent variable. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.7: Effects of the MMC mandate on adequacy of prenatal care, Medicaid

	(1) More than 9 visits	(2) More than 11 visits	(3) More than 14 visits
MMC mandate	0.116***	0.090***	0.051***
	(0.038)	(0.031)	(0.016)
Observations	79452	79452	79452
Mean before mandate	0.130	0.078	0.036

Notes: Each cell shows an estimated β from equation (1) for different dependent variables. The unit of observation used in the analysis is at the enrollee level, with data aggregated over 40 weeks of pregnancy for pregnant women. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.8: Effects of the MMC mandate on pregnant women's health care utilization, extensive margin measures

Probability of any ...	(1) Prenatal care visit	(2) OB-GYN prenatal care visit	(3) Non-OB-GYN prenatal care visit	(4) ED visit	(5) Inpatient visit
Panel A. Medicaid					
MMC mandate	0.054***	0.132***	0.046***	0.022	-0.006
	(0.010)	(0.044)	(0.011)	(0.021)	(0.005)
Observations	79452	79452	79452	79452	79452
Mean before mandate	0.823	0.368	0.793	0.394	0.065
Panel B. Private insurance					
MMC mandate	0.020	0.125	0.029	-0.027	0.015
	(0.031)	(0.073)	(0.045)	(0.051)	(0.017)
Observations	3200	3200	3200	3200	3263
Mean before mandate	0.949	0.629	0.830	0.281	0.033

Notes: Each cell shows an estimated β from equation (1) for each dependent variable. The unit of observation used in the analysis is at the enrollee level, with data aggregated over 40 weeks of pregnancy for pregnant women. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.9: Heterogeneous effects of the MMC mandate on the privately insured by the number of Medicaid enrollees

	(1)	(2)	(3)	(4)
	Infants' PCP E&M office visits		Women's OB-GYN prenatal care visits	
	Below median	Above median	Below median	Above median
MMC mandate	0.931	1.934**	0.095	1.506**
	(0.697)	(0.686)	(0.451)	(0.591)
Observations	887	1590	1131	2062
Mean before mandate	3.671	3.906	3.194	2.976

Notes: Each cell shows an estimated β from equation (1) for each dependent variable. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. For this analysis, I calculate the number of Medicaid enrollees at the county level separately for infants and pregnant women using the Medicaid records. I then merge this data with the private insurance claims data to examine the heterogeneity based on the size of the Medicaid population. However, since many of the "never-treated" counties (i.e., counties that transitioned to MMC after the end of the study period) are small counties with relatively few Medicaid enrollees, simply dividing the entire sample based on the size of the Medicaid population results in no control group for the larger Medicaid counties, making my main model inestimable. To address this issue, I divide both the treatment and control groups separately into two subgroups based on Medicaid population size. I then compare the smaller treatment group to the smaller control group and the larger treatment group to the larger control group. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.10: Effects of the MMC mandate on HHI and the number of providers

	(1)	(2)	(3)	(4)
	Medicaid		Private insurance	
	HHI	Number of new providers	HHI	Number of new providers
Panel A. E&M office providers for infants				
MMC mandate	-189.238	22.750	853.252	-1.987
	(114.858)	(16.131)	(913.600)	(4.463)
Observations	238	238	73	73
Mean before mandate	1010.034	60.657	3447.569	9.532
Panel B. Prenatal care providers for pregnant women				
MMC mandate	-71.164	18.188*	49.622	-5.304
	(69.727)	(10.050)	(951.456)	(11.085)
Observations	238	238	78	78
Mean before mandate	593.527	86.469	984.692	39.371

Notes: Each cell shows an estimated β from equation (1) for each dependent variable. The unit of observation is county by year. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.11: Number of new Medicaid providers who participate in both FFS and MMC

	(1) Infants	(2) Pregnant women
MMC mandate	19.694	14.842
	(14.372)	(9.074)
Observations	238	238
Mean before mandate	60.297	85.120

Notes: Each cell shows an estimated β from equation (1) for each dependent variable. The unit of observation is county by year. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.12: Effects of the MMC mandate on imaging services

	(1)	(2)	(3)	(4)
	Infants		Pregnant women	
	Medicaid	Private	Medicaid	Private
MMC mandate	0.001	0.154	0.166*	0.292
	(0.036)	(0.257)	(0.089)	(0.465)
Observations	101522	2471	79446	3200
Mean before mandate	0.574	0.975	2.310	3.433

Notes: Each cell shows an estimated β from equation (1) for each dependent variable. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.13: Heterogeneous effects of the MMC mandate on the privately insured

	(1)	(2)	(3)	(4)
	Infants' PCP E&M office visits		Women's OB-GYN prenatal care visits	
	Fewer Medicaid visits	More Medicaid visits	Fewer Medicaid visits	More Medicaid visits
Panel A. By the number of Medicaid visits per county-month				
MMC mandate	0.782**	1.781**	0.853	0.683
	(0.353)	(0.622)	(0.904)	(0.665)
Observations	1414	1502	1955	1956
Mean before mandate	2.567	2.828	2.079	2.249
Panel B. By the number of Medicaid visits per county-month between March and August				
MMC mandate	0.744**	1.190**	1.054*	0.430
	(0.290)	(0.463)	(0.564)	(0.524)
Observations	1136	1122	1748	1755
Mean before mandate	1.480	1.840	1.170	1.382
	Infants' PCP E&M office visits		Women's OB-GYN prenatal care visits	
	No	Yes	No	Yes
Panel C. By whether the county has a dominant MMC plan with employer business				
MMC mandate	1.820***	0.098	0.995	0.353
	(0.572)	(0.385)	(0.609)	(0.366)
Observations	2147	330	2741	459
Mean before mandate	3.920	2.943	3.049	3.164

Notes: Each cell shows an estimated β from equation (1) for each dependent variable. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.14: Insurer payment per procedure for private insurance

	(1)	(2)
	Average price per procedure at PCP E&M office visits for infants	Average price per procedure at OB-GYN prenatal care visits during pregnancy
MMC mandate	5.468	21.042
	(7.738)	(26.545)
Observations	391	1173
Mean before mandate	80.814	177.576

Notes: The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.15: County-year number of enrollees

	Infants		Pregnant women	
	Medicaid	Private insurance	Medicaid	Private insurance
MMC mandate	15.495	1.701	7.079	-0.094
	(15.072)	(6.980)	(26.236)	(4.759)
Observations	238	77	238	80
Mean before mandate	385.269	40.851	326.211	24.175

Notes: Each cell shows an estimated β from equation (1) for each dependent variable. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.16: Changes in characteristics of enrollees

	(1)	(2)	(3)	(4)	(5)	(6)
			Previous maternal utilization		Zip code level income†	
	Share of girls	Maternal age	Inpatient visits	ED visits	Below-median	Above-median
Panel A. Medicaid						
MMC mandate	0.011**	0.140*	-0.003	0.006	-0.008	0.008
	(0.005)	(0.076)	(0.010)	(0.074)	(0.006)	(0.006)
Observations	101516	79446	41091	41091	100661	100661
Mean before mandate	0.486	25.051	0.062	0.853	0.574	0.426
Panel B. Private insurance						
MMC mandate	-0.058	0.880	-0.009	-1.437	-0.009	0.009
	(0.074)	(0.612)	(0.016)	(1.413)	(0.006)	(0.006)
Observations	2477	3186	1838	1838	3162	3162
Mean before mandate	0.466	29.184	0.016	1.679	0.995	0.005

Notes: Each cell shows an estimated β from equation (1) for each dependent variable. † I obtain median income at the five-zip code level from the 2006-2010 American Community Survey. Because the most granular level of geographic information available in MarketScan is the three-digit zip code, I calculate the average median income at the three-digit zip code level for the analysis with MarketScan. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Table B.17: Number of claims per outpatient day, Medicaid

	(1) Infants	(2) Pregnant women
MMC mandate	0.052	0.090
	(0.078)	(0.073)
Observations	101522	79452
Mean before mandate	1.903	1.694

Notes: Each cell shows an estimated β from equation (1) for each dependent variable. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Appendix C. TWFE and robustness to alternative methods

In this section, I discuss the issues associated with the TWFE model and assess the robustness of my findings using alternative estimators. Given that some methods are data-intensive, I focus on a relatively large sample from Medicaid claims for this analysis.

First, I evaluate the severity of the issues related to the TWFE model in my setting using the tool proposed by De Chaisemartin and d'Haultfoeuille (2020), which computes the weights associated with the treatment effect for each treated group and time combination. I find that, in my analysis sample, all TWFE estimator weights are positive. Additionally, the decomposition suggested by Goodman-Bacon (2021) indicates that my TWFE estimates are primarily driven by comparisons between counties that were treated and those that were never treated during the study period. TWFE estimators are known to perform well when never-treated counties serve as controls (Callaway and Sant'Anna 2021).

To further assess the validity of the TWFE estimates, I compare them with results from three alternative methods: the CS approach (Callaway and Sant'Anna 2021), an imputation method (Borusyak et al. 2024), and a stacked DD design. The estimates are summarized in Appendix Table C.1. The main TWFE estimates are presented in the first column. Across all three alternative methods, the point estimates vary slightly but remain similar. These results indicate that the TWFE model produces estimates that closely align with those from alternative approaches, supporting its use as a reasonable choice in my setting.

Table C.1: Robustness to different specifications, Medicaid data only

	(1)	(2)	(3)	(4)
	TWFE	Callaway and Sant'Anna (2021)	Borusyak et al. (2024)	Stacked DD
Panel A. Infants, PCP E&M office visits				
MMC mandate	0.820***	0.739***	0.711***	0.793***
	(0.124)	(0.137)	(0.170)	(0.132)
Panel B. Pregnant women, OB-GYN prenatal care visits				
MMC mandate	1.040**	0.973**	0.705***	1.055**
	(0.422)	(0.305).	(0.234)	(0.426)

Notes: Column (1) shows the TWFE estimate, an estimated β from equation (1). Column (2) reports the DD estimate based on Callaway and Sant'Anna (2021). Column (3) reports the DD estimate based on Borusyak et al. (2024). Column (4) reports the TWFE estimate from a stacked dataset, which is constructed by appending event-specific datasets (i.e., each treatment cohort and its clean control group). For this estimation, county and year fixed effects are saturated with indicators for the dataset identifiers. * Significant at 10%, ** significant at 5%, *** significant at 1%.

It is important to note that the literature has not yet settled on a standard solution (Baker et al. 2022), and each alternative estimator comes with its own set of assumptions, methodological approaches, and limitations.

For example, the CS approach provides a valid estimate under the no-anticipation and unconditional parallel trends assumptions. The estimation process involves computing the ATTs for each treatment cohort, taking a weighted average across cohorts, and bootstrapping standard errors—making it computationally intensive.

Imputation methods (Wooldridge 2021; Borusyak et al. 2024) estimate a model for non-treated potential outcomes using non-treated observations and then extrapolate this model to treated observations to estimate the treatment effect. Since imputation is data-intensive, it may not perform well with small samples. This is particularly problematic in my analysis of private insurance claims, where the sample size is much smaller than that for Medicaid claims.

Another alternative is the stacked DD design (Deshpande and Li 2019), which constructs a stacked dataset of each treatment cohort with "clean" controls. This allows researchers to apply the TWFE model to clearly defined natural experiments. However, this approach offers less flexibility for aggregation compared to other alternative methods.

Overall, given the multiple alternative choices—each with different computational demands and limitations—as well as the extensive analysis and robustness checks already included in this paper, I use the TWFE model for the main results due to its computational efficiency and flexibility.

Appendix D. Alternative strategies for ED visits

In this section, I consider alternative strategies to estimate the effect of the MMC mandate on ED visits. Specifically, I consider using contiguous counties as controls in a stacked DD design, a synthetic control design (matching counties based on lagged dependent variables), as well as the Callaway and Sant'Anna 2021 method.

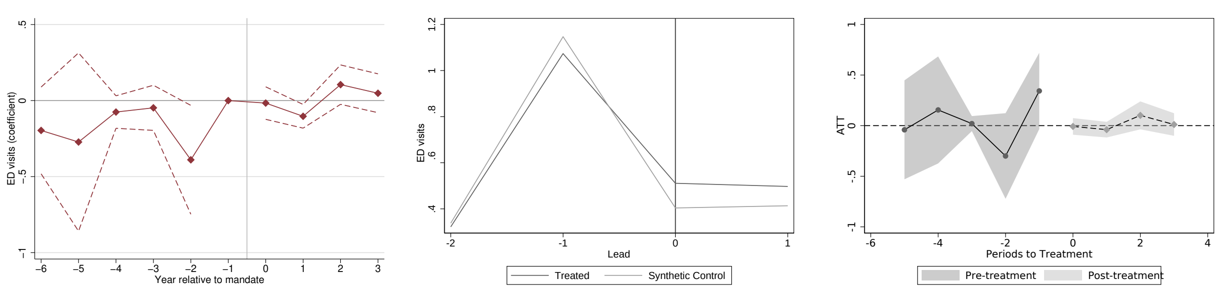
Table D.1: Effects of the MMC mandate on ED visits for Medicaid infants, alternative strategies

	(1)	(2)	(3)
	Contiguous counties as controls in a stacked DD design	Synthetic control	Callayway & Sant'Anna
MMC mandate	0.144**	0.085**	0.015
	(0.060)	[0.035]	(0.034)

Notes: Standard errors are in parentheses, and p-values are in brackets. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Figure D.1: Effects of the MMC mandate on ED visits for Medicaid infants, alternative strategies

(a) Contiguous counties as controls in a stacked DD design (b) Synthetic control (c) Callaway and Sant'Anna



Notes: Panel (a) uses on contiguous counties as controls in a stacked DD design. Panel (b) uses a synthetic control design, using lagged dependent variables to create a synthetic control. Panel (c) uses the Callaway and Sant'Anna 2021 to estimate dynamic effects.

Column (1) of Table D.1 summarizes the effect of the MMC mandate using continuous counties as controls in a stacked DD design. The estimate is very similar to the main estimate based on the TWFE model. Figure D.1(a) also shows large and negative estimates before the mandate. Column (2) and Figure D.1(b) show the estimate from the synthetic control design where I use lagged dependent variables to create a synthetic control group. The estimate becomes about half the size but significant. Finally, column (3) summarizes the estimate based on the Callaway and Sant'Anna method. The estimate on ED visits becomes very small and insignificant. The event study (Figure D.1(c)) also shows insignificant pre-trends and precise zeros after the mandate.

Moreover, I further examine the types of ED visits using the classification algorithm developed by New York University (Billings et al. 2000). Dividing the ED visits into four categories, I find that the increase in ED visits was most pronounced in emergent but primary care treatable conditions (panel A of Appendix Table D.2). This suggests that some families may have experienced worse access to care following the mandate. These results are inconsistent with other findings that suggest potential improvements in healthcare, such as improved access to routine care and increased immunizations.

However, similar to the estimate on the overall ED visits, the event studies (Appendix Figure D.2) suggest that the large and negative estimates before the mandate drive the positive DD estimates. Panel B of Appendix Table D.2 shows that the estimates become very small and insignificant when using an alternative model based on the Callaway and Sant'Anna 2021 method. The sensitivity to the model specification further suggests that the main DD estimate on ED visits should be interpreted with caution.

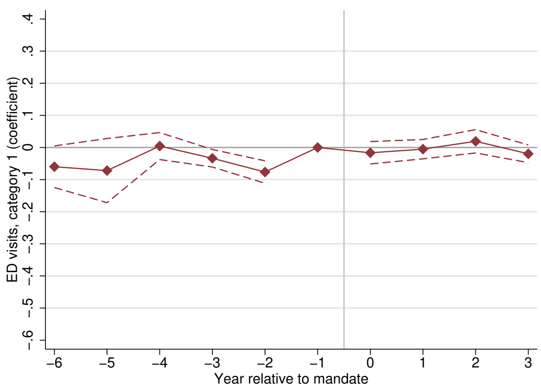
Table D.2: Effects of the MMC mandate on Medicaid infants' ED utilization

	(1)	(2)	(3)	(4)
		Emergent		
	ED care needed, not preventable	ED care needed, preventable	Primary care treatable	Non-emergent
Panel A. TWFE				
MMC mandate	0.031**	0.012**	0.102**	0.080**
	(0.012)	(0.005)	(0.038)	(0.032)
Observations	101522	101522	101522	101522
Mean before mandate	0.103	0.062	0.365	0.283
Panel B. Using the Callaway and Sant'Anna method				
ATT	-0.002	-0.002	0.042	0.016
	(0.013)	(0.009)	(0.027)	(0.027)

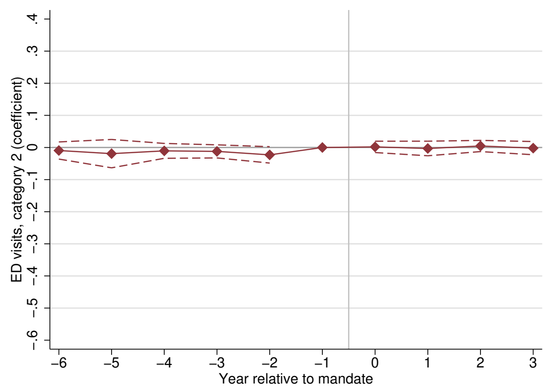
Notes: In panel A, each cell shows an estimated β from equation (1) for each dependent variable. Panel B shows an estimated average treatment effect using the Callaway and Sant'Anna 2021 method. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants. * Significant at 10%, ** significant at 5%, *** significant at 1%.

Figure D.2: Event study estimates on different types of ED visits, Medicaid infants

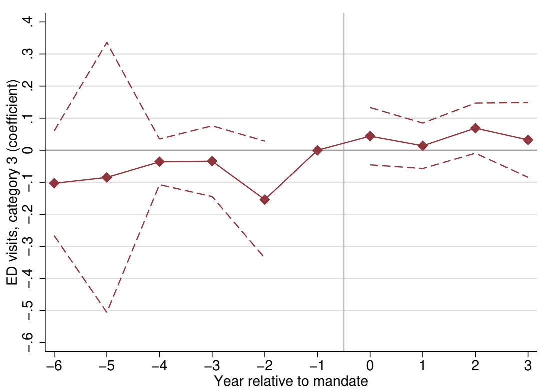
(a) Emergent, ED care needed, not preventable



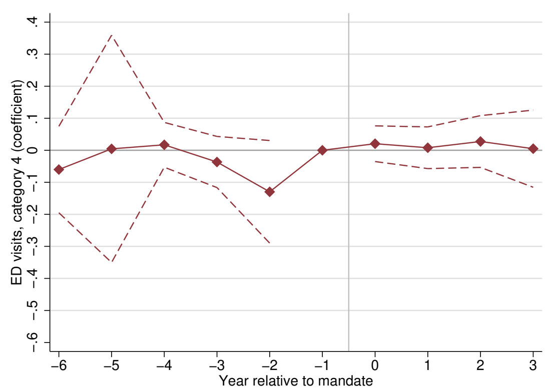
(b) Emergent, ED care needed, preventable



(c) Emergent, Primary care, treatable



(d) Non-emergent



Notes: Each panel plots β_js from equation (2) for each dependent variable.

Appendix E. Heterogeneity across race/ethnicity

Following the analysis in Kuziemko et al. (2018), I consider heterogeneity by race/ethnicity in this section. Table E.1 summarizes how the main estimates vary by race/ethnicity. For both infants and pregnant women, the increase in routine care is larger for white and Hispanic mothers compared to that for Black mothers. While I do not find evidence of worsening care for Black enrollees, the fact that the "improvement" in care is larger for Hispanic enrollees aligns with the findings of Kuziemko et al. (2018). As Kuziemko et al. (2018) alluded, it is possible that health plans invested more in relatively lower-cost groups, leading to larger increases in routine care. However, I am unable to directly test the hypothesis and am therefore hesitant to make any definitive claims based on this heterogeneity analysis.

Table E.1: Effects of the MMC mandate on health care utilization, heterogeneity by race

	(1)	(2)	(3)	(4)
	White	Black	Hispanic	Other
Panel A. Infants, outcome: PCP E&M office visits				
MMC mandate	0.829***	0.482***	0.950***	1.043***
	(0.131)	(0.109)	(0.207)	(0.183)
Observations	76485	6826	6436	10156
Mean before mandate	0.584	0.812	1.091	0.852
Panel B. Pregnant women, outcome: OB-GYN prenatal care visits				
MMC mandate	1.115***	0.272	1.588**	0.914*
	(0.362)	(0.493)	(0.730)	(0.503)
Observations	60732	4955	5543	6084
Mean before mandate	1.139	1.241	0.721	1.207

Notes: In panel A, each cell shows an estimated β from equation (1) for the dependent variable, the number of PCP E&M office visits for infants. In panel B, each cell shows an estimated β from equation (1) for the dependent variable, the number of OB-GYN prenatal care visits for pregnant women. The unit of observation used in the analysis is at the enrollee level, with data aggregated over the first year of life for infants and over 40 weeks of pregnancy for pregnant women. * Significant at 10%, ** significant at 5%, *** significant at 1%.