A Change of Plans: Switching Costs in the Procurement of Health Insurance

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Abstract

The provision of public health insurance through regulated markets requires a dynamic procurement of insurers over time. I study switching costs between insurers, exploiting non-renewed contracts with incumbent insurers after a state bid in Medicaid managed care. Using a difference-in-differences framework, I find that beneficiaries that are forced to switch health plans after these bids have fewer visits to primary care physicians and lower utilization of prescription drugs, including for chronic conditions. Children, non-whites, and sicker switchers have more preventable hospital admissions. In the year following the exit, insurers' spending on switchers is 4% lower than the pre-exit baseline. Changes in the network of providers and in drug formularies may serve as mechanisms.

Keywords: Health Insurance, Medicaid, Procurement, Switching Costs
JEL: I13, I18, H75

1. Introduction

Public health insurance programs in the U.S. are increasingly being provided through regulated markets of private insurers (Gruber (2017)). In the dynamic procurement of insurers that participate in these markets, contracts with incumbent insurers are not always renewed, forcing enrollees to switch to another health plan. These transitions can disrupt patients' utilization patterns, cause discontinuities of care, and result in adverse health outcomes, thereby giving rise to non-pecuniary switching costs. Switching may also affect insurers' costs and the government's spending. For regulators, switching costs present a tradeoff between the potential to reduce costs and/or increase quality by replacing an insurer, and the disruptions that such a change may cause.

I examine switching costs in the procurement of health insurance within the setting of Medicaid managed care (MMC) — the regulated markets of private managed care plans that provide publicly-financed health insurance to approximately 70% of Medicaid beneficiaries (see Layton et al. (2018) for a review). I focus on incumbent plans that do not win a new contract in a state bid to serve a county or a service area, forcing all their enrollees in these areas to switch out. To identify plan exits, I collect public information on MMC bids, including the bidders, winners and losers, and the bid's milestone dates. I then use administrative data from the Medicaid Analytic eXtract (MAX) for the years 2006 to 2014 to examine enrollment and utilization patterns around bid-induced plan exits in five states where such exits are identified in the data: Arizona, Minnesota, Missouri, Texas, and Washington. I focus on non-elderly beneficiaries who were continuously enrolled in their plan for at least a year and a half before the exit — a group with ample time to establish relationships with their providers.

Within a stacked difference-in-differences framework, I compare about 165,000 (treated) enrollees, in 27 plans that exit 122 counties, to a control group of 1 million (never-treated) enrollees in plans that remain in the market. The stacked framework avoids possible biases due to heterogeneous treatment effects (Sun and Abraham (2021); Goodman-Bacon (2021)). To mitigate biases due to anticipatory effects, the baseline pre-period includes only the time before contracts are awarded in the bid.[1] I conduct event studies, controlling for individual and state-specific time fixed-effects, to verify that no differential trends are apparent between the enrollees of exiting and remaining plans before contracts are awarded, as well as no differential level of data reporting by these plans.

I find that beneficiaries in exiting plans ("switchers") experience significant disruptions to their care and some suffer adverse health outcomes. Throughout the year after a plan exit, switchers use fewer prescription drugs, including drugs that treat chronic conditions such as diabetes or depression. Comparing to the control group, the number of days' supply in switchers' filled prescriptions is lower by 16% (2.4 days) relative to the baseline period. Switchers also have 6% to 9% fewer visits to primary care physicians throughout the post-exit year, and by the end of the year they are admitted to hospitals 10% more often.[2] Using

[1] I study separately the period between the awards of the contracts and the actual exit, by examining event studies around the contracts' awards bid milestone.

[2] Switchers' utilization patterns begin to change already after contracts are awarded in the bid, but these pre-exit changes are mostly smaller — the number of PCP visits decreases by 4%, and there is no change in the number of hospital admissions. However, possibly lower data reporting

prices from Medicaid's fee-for-service (FFS) program, I estimate that insurers in the market save \$151 (4%) on each switcher during the post-exit year.

Children are more sensitive to disruptions after a plan exit and have up to 4% more visits to emergency departments (EDs) after the switch, while adults do not show a significant change. By the end of the switching year children have 15% more hospital admissions, with a third of them attributed to ambulatory-care-sensitive conditions (ACSC), which are deemed preventable with appropriate community care. Sicker switchers and non-white beneficiaries also have more preventable hospital admissions.

Changes in the networks of providers and in drug formularies may serve as mechanisms for the post-exit disruptions. First, I find that a significant share (23%) of switchers no longer have access to their pre-exit primary care physicians (PCPs) in the network of their new plan, compared to only 3% of beneficiaries in remaining plans. Losing a PCP is correlated with more severe switching disruptions. Second, the share of visits to other outpatient providers that were seen in the previous year decreases for switchers from 70% of all visits to only half after the exit. Lastly, I explore two mechanisms that may contribute to the lower use of prescription drugs. I find that switchers fill prescriptions more often in unfamiliar pharmacies after the exit, suggesting that some pharmacies used in the pre-exit period are excluded from the new plan's network. Additionally, the share of familiar drugs in switchers' prescriptions is lower by 7% in the first half-year after the exit, indicating that new drug formularies (and new providers) prompt switchers to change their medication.

Finally, although health plans may vary in their causal affect on their enrollees' utilization and health (Geruso et al. (2020); Abaluck et al. (2021)), I find that switches to plans with higher or lower observational effect on spending are both correlated with fewer visits to PCPs, lower utilization of prescription drugs, and more hospital admissions. This suggests that plans' effects on costs are not a major mechanism for the observed transactional disruptions.

This paper contributes to the literature in two main areas. First, it adds to the empirical literature on switching costs (see Farrell and Klemperer (2007) for a review of the issue). Recent papers on switching in health insurance mostly used a structural choice model to estimate the cost of individuals' switching frictions, that may include also inattention, information frictions, hassle costs, etc. (e.g. Heiss et al. (2021); Polyakova (2016) in Medicare Part D, and Handel (2013);

by exiting plans during this awards-to-exit period may bias these estimates down.

Handel and Kolstad (2015) in employer-sponsored insurance). This paper does not estimate the implied negative value that individuals attach to switching plans, but the actual implications of switching. The disruptions and adverse health outcomes due to a switch may support a rational explanation for the observed inertia of enrollees in their MMC plan (Marton et al. (2017)). These results are consistent with Dahl and Forbes (2023), who separately estimate doctor switching costs and find that they account for most of enrollees' inertia, with older and sicker individuals willing to pay higher premiums to maintain access to their primary care physicians. Switching costs are present in the procurement of other services, such as computer and IT systems (Greenstein (1993)), and regulators may take them into account when contracting with insurers in regulated markets.

Second, this paper extends the literature on the effects of disruptions in health care. Recent studies have primarily focused on disruptions to the relationships between patients and their primary care providers, mostly due to retirement or relocation (Schwab (2018); Sabety (2021); Zhang (2022); Staiger (2022)). Only few studies examined disruptions at the insurer-level, providing observational evidence that changes in provider networks after a plan switch can harm the relationships between patients and their physicians (Barnett et al. (2017); Lavarreda et al. (2008)). This paper is the first to causally identify the impact of involuntary plan switching within a regulated market, demonstrating that such switches can disrupt relationships not only with familiar primary care physicians, but also with specialists and pharmacies. Furthermore, after switching to a new insurer patients may encounter different drug formularies and rules for prior authorization of drugs and procedures, leading to additional frictions and care discontinuities. Assessing the effects of plan switching is particularly important in the fragmented U.S. health care system, where no plan offers health insurance from cradle to grave and switching between health plans or types of health coverage is inevitable. A significant share of these switches are involuntary, both in employer-sponsored insurance (Cebul et al. (2011); Cunningham and Kohn (2000)), and in Medicaid's and Medicare's regulated markets (Ndumele et al. (2017); Jacobson et al. (2016)).[3]

The rest of the paper proceeds as follows. Section 2 presents the data. Section 3 outlines the empirical framework, and Section 4 presents the results. Heterogeneity in the results is explored in Section 5 and Section 6 examines possible mechanisms. Section 7 discusses the findings, and Section 8 concludes.

[3] Insurance switching rates are high in general for low-income adults, and are very high for enrollees with individual insurance (Sommers et al. (2016); Austic et al. (2016)).

2. Data

2.1. MMC bids and plan exits

To identify plan exits due to Medicaid managed care bids, I first collect publicly-available information. This includes states' documents, such as request for proposals (RFPs) or contracts with insurers, and reports in the general and professional media. I extract information on bidders, winners, losers, and the bid's milestones, i.e., the dates in which the bid closes to offers, contracts are awarded to winners, and service starts. I verify the bid-induced plan exits using the 2007 to 2014 Medicaid Analytic eXtract (MAX) — an administrative dataset managed by the Centers for Medicare and Medicaid Services (CMS). Enrollment information on Medicaid beneficiaries is taken from the MAX Personal Summary files (PS), that contain person-month enrollment status. For individuals enrolled in Medicaid, these files hold data on demographic characteristics, the basis for eligibility, whether the individual is enrolled in a comprehensive managed care plan, and the characteristics of this plan. I can not observe in the data whether beneficiaries actively choose their plan or are automatically assigned by the Medicaid program. The PS files provide monthly information on the enrollment in managed care plans in each state and county, allowing to identify the month in which a plan exits.[4] An exit of a plan in the MAX data may also occur due to mergers and acquisitions. In this case ownership changes but enrollees may not experience any immediate change. Using only exits that are verified by both MAX data and public bid information eliminates the concern of misidentified exits. The analytic sample includes five states where bid-induced exits are verified: Arizona, Minnesota, Missouri, Texas, and Washington. See Table 1 for more details about these bids.

My sample includes non-elderly beneficiaries from the sample states, that are eligible for full benefits and are not enrolled also in Medicare at any time during the year. Beneficiaries that move between counties and states are excluded from the sample. The analytic sample focuses on beneficiaries that were enrolled in a MMC plan at the month before the exit in the state, and were enrolled in the same plan during the 18 months before the exit. The treatment group includes beneficiaries in plans that exit the market. Beneficiaries in other plans are included in the control group. The sample restrictions significantly decrease the sample size (see

[4] I consider a plan exit month as the month in which enrollment in it drops to zero, or drops by at least a half — partial exit that may apply to a certain subgroup of enrollees.

Table 1. Medicaid Managed Care bids included in the sample

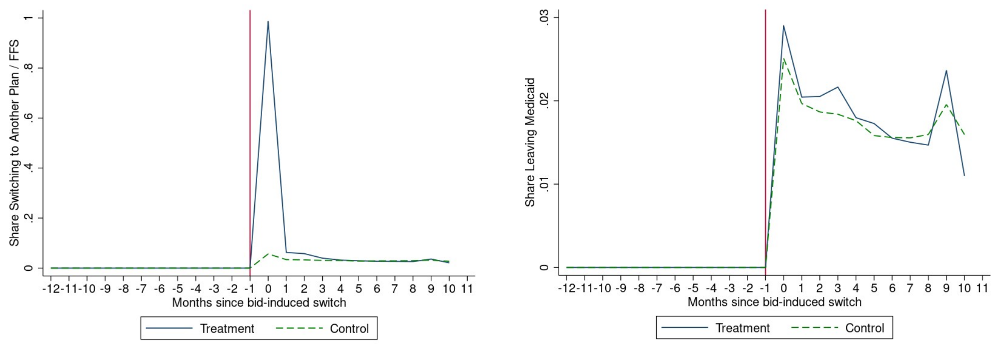
State	Milestones Dates Bid Close	Awards	Service Start	Plan Exits	# in Exiting Plans (MAX)
Arizona	3/2008	5/2008	10/2008	Pima Health Systems, Arizona Physicians IPA, Mercy Care	109,702
Minnesota	06/2011	08/2011	01/2012	MHP, Blue Plus, Medica	58,070
Missouri	12/2011	02/2012	07/2012	Molina, Missouri Care, WellCare, Blue-Advantage Plus, Children's Mercy	77,693
Texas	05/2011	08/2011	03/2012	Amerigroup, BCBS, Sendero, Superior	29,599
Washington	12/2011	01/2012	07/2012	CUP, CHPW	102,070

Notes: The table presents information on the bids included in the sample. It shows bids' milestones dates: Bid close (last date to submit proposals), Awards (when winners and losers are announced), and Service Start (when plans that were awarded contracts start serving beneficiaries). The table lists the known plan exits due to the bid, as gathered from public information. Bids are often published for specific counties or service areas, and some plans may exit only some counties in the state, while keeping operations in others. The table also shows the number of beneficiaries in each state that are enrolled in exiting plans at the month before the exit (using the administrative data in MAX PS files).

Table A1 in the Appendix), as many Medicaid beneficiaries that are enrolled in a plan a month before the exit weren't enrolled continuously in Medicaid or in the same plan during the year and a half before the exit. However, the analytic sample allows to examine the effects of switching on beneficiaries with enough tenure in their plans to form relationships with providers — a situation more similar to the experience of switchers in other insurance markets.

Figure 1a presents the share of switching beneficiaries among the treatment and control groups. By construction of the sample, there are no switches in the year and a half before the exit. At the month of the exit, almost all beneficiaries in the treatment group (98.6%) switch out of their plan, while only 5.7% of the control group switches out. The share of switches among beneficiaries in exiting plans continues to be a bit higher than the control group in the two months after that, as switchers have 90 days to switch again to another plan without cause. Later, switching rates are similar for both groups (switches are allowed with cause for both groups after the first 90 days).

To examine possible differential churning out of Medicaid after plans exit the market, Figure 1b presents the share of beneficiaries in the treatment and control groups that leave Medicaid every month. While beneficiaries in exiting plans tend to churn out of Medicaid more after their plan exits, the difference in the churn rate between the treatment group (2.9% at the first month after exit) and the control group (2.5%) is very small.



(a) Share of enrollees switching out of their plan (b) Share of enrollees leaving Medicaid

Figure 1. Switching around plans' exit

Notes: Figure (a) shows the share of enrollees switching out of their plan each month, around a bid-induced exit, in the treatment and control groups (my first stage). Switchers either switch to another plan, or to the fee-for-service system, or leave Medicaid. While practically all of the beneficiaries in exiting plans (treatment) switch out of their plans, almost all beneficiaries in remaining plans (control) don't switch. Figure (b) shows the share of enrollees leaving Medicaid (and thus the sample) each month, around a bid-induced exit, in the treatment and control groups. These attrition rates are similar for both the treatment and control groups.

2.2. Data on utilization of services

I use data on beneficiaries' utilization included in the MAX Inpatient (IP), Other Services (OT), and Prescription Drug (RX) files. These files track claims for services provided by the fee-for-service system and also include encounter data on services provided by the private plans in the MMC program. My main outcome variables include the number of visits to primary care physicians (PCPs)[5] and to other outpatient physicians (i.e. specialists), the number of visits to emergency

[5]To identify primary care services, I follow the ACA definition, that includes CPT codes for evaluation and management (E/M) visits in an outpatient setting (99201 through 99215), in a nursing facility (99304 through 99340) and at home (99341 through 99350). See https://www.cms.gov/Regulations-and-Guidance/Guidance/Transmittals/downloads/R2161CP.pdf

departments (ED) in hospitals,[6] and the number of hospital admissions. To study the effect of switching on avoidable inpatient admissions, I examine the number of hospital admissions due to ambulatory care sensitive conditions (ACSC). These are admissions with acute conditions that are deemed preventable with appropriate and early community care. For example, these conditions include complications of diabetes or asthma, nutritional deficiency anemia, and vaccine-preventable diseases (see Brown et al. (2001) and Eggli et al. (2014) for more details).[7] Lastly, I examine the number of days' supply in filled prescriptions. Prescription drugs are classified to therapeutic classes using the RxNorm database.[8]

Table 2 presents summary statistics for the treatment and control groups, one year before the exit. Exiting plans have a higher share of white beneficiaries and a lower share of disabled beneficiaries than control plans. Most beneficiaries are children in both groups, but there are fewer babies and toddlers in exiting plans and more adults. Despite these differences, the levels of utilization of services are only slightly lower in the treatment group.

2.3. Prices and spending

To assess plans' spending I predict prices for the services they purchase, based on prices in fee-for-services (FFS) claims in the sample states (the prices plans actually pay are not observed in the MAX data).[9] Separately for out-patient and in-patient claims, I estimate regressions of the payment for each FFS claim on the claim's characteristics.[10] I use the results to predict plans' payment for each

[6] I follow Hennessy et al. (2010) in identifying visits to ED using revenue codes and CPT codes.

[7] The full list of conditions used is: Angina, asthma, cellulitis, congestive heart failure, convulsions and epilepsy, chronic obstructive pulmonary disease, dehydration and gastroenteritis, dental conditions, diabetes complications, ear nose and throat infections, gangrene, hypertension, influenza and pneumonia, iron or other nutritional deficiency anemia, nutritional deficiency, other vaccine preventable diseases, pelvic inflammatory disease, perforated/bleeding ulcer, pyelonephritis.

[8] These are publicly available data courtesy of the U.S. National Library of Medicine (NLM), National Institutes of Health, and the Department of Health and Human Services (Nelson et al. (2011)).

[9] Plans' purchase prices are replaced in MAX by their assessment of the dollar value of the service. This value is often missing, it depends on plans' interpretation, and is generally unreliable. CMS advises "extreme caution" when using the plans' value variable.

[10] For out-patient claims, the explanatory variables are fixed effects for state, place of treatment, and procedure code. For in-patient claims, the explanatory variable are fixed effects for state, procedure code, diagnosis, and number of inpatient days.

Table 2. Descriptive statistics, 12 months before the exit

	Control	Treatment
Number of beneficiaries	1,001,587	164,843
Number of MMC Plans	70	27
Number of Counties	353	122
Share of females (%)	52.8	54.0
Share of whites (%)	27.5	48.1
Share disabled (%)	7.0	2.9
Age structure (share, %):		
Under 5	31.8	27.9
5 to 20	51.7	51.4
20 to 45	10.8	14.7
45 to 65	5.7	6.2
Monthly Utilization:		
Share using any out patient service (%)	20.7	18.9
Share filling any prescription (%)	26.4	24.4
Hospitalizations (# per 1,000)	5.2	4.8
Share of women at age 15-44 giving birth (%)	0.8	0.8
Estimated total spending: ($ PMPM)	336.8	339.8
Estimated plans' spending	288.7	321.7
Fee-For-Service spending	48.1	18.1

Notes: This table presents summary statistics for the Medicaid beneficiaries included in the sample: non-elderly, non-dual beneficiaries that remain in their plan for at least 18 months before an exit occurred in their state. All movers are dropped from the sample. The treatment group includes beneficiaries enrolled in exiting plans at the month before the exit. The control group includes beneficiaries in other, non-exiting plans. The presented statistics are for a single month one year before the exit. The number of plans is the number of different HMO IDs in the administrative MAX database. Plans' estimated spending is based on predicted prices, derived from FFS claims in the sample states. Despite some demographic differences between treatment and control, utilization and total costs are similar for both groups.

encounter data record. For prescription drugs, I use the average payment per drug (by its National Drug Code) to predict plans' costs for each prescription filled. Table 2 presents the estimated spending on beneficiaries in the treatment and control groups. Estimated total spending is similar in both groups, at $337 to $340 per member per month (PMPM). Excluding spending through the public FFS system, plans' spending is higher for beneficiaries in exiting plans.

2.4. Measuring disruptions to the network of providers

Changes in the network of providers after a switch to a new plan may disrupt enrollees' relationships with providers and harm continuity of care. To measure such changes I first calculate, for each beneficiary and month, the share of the beneficiary's providers during the month that were already visited in the previous year (relative to the exit date).[11] These "share of known providers" measures focus on network changes at the beneficiary-level.[12] In addition to that, I study whether beneficiaries keep their access to primary care physicians (PCPs) after a switch, by examining whether PCPs that the beneficiary visited before the exit are included in the network of the post-exit plan.

3. Empirical Framework

3.1. Disruptions after a plan exit

To study health plan switching costs I examine involuntary switching out of plans that exit their county in the Medicaid Managed Care program. Within a stacked difference-in-differences (DID) framework, I compare, before and after an exit, beneficiaries in exiting plans that are all forced to switch out of their plan, to same-state beneficiaries in remaining plans. As I exploit involuntary switches, biases due to self-selection into switching are less of a concern in this setting.

In the analytical dataset, beneficiaries in each state are assigned time variables relative to the month of exit in their state, i.e. the month when service starts as part of the new contracts. Each state serves as a different experiment cohort with a single exit event, where treated beneficiaries (involuntary switchers) are compared to never-treated beneficiaries in the control group. Thus the empirical

[11]This measure builds on the Known Provider measure for continuity of care (CoC) (Smedby et al. (1986)). Other CoC measures consider the duration of time the patient used a particular provider, the density of her visits to this provider, and the dispersion of visits among multiple providers (Jee and Cabana (2006)).

[12]A provider may be listed in a plan's network, but offer only very limited availability to Medicaid beneficiaries. Thus, beneficiary-level measures allow to examine the de-facto networks as experienced by enrollees.

approach avoids possible biases due to estimating two-way fixed effects (TWFE) regressions with staggered events (Goodman-Bacon (2021); Sun and Abraham (2021)). The dataset is used to estimate reduced-form event studies around plan exits. The estimated equation is:

$$Y_{ist} = \sum_{l} \beta_l 1\{t - Exit_s = l\} * Treated_i + \gamma_i + \delta_{st} + month_t + \varepsilon_{it} \tag{1}$$

where Y_{ist} is the outcome for individual i, residing in state s, at month t. $1\{t - Exit_s = l\}$ is an indicator for being l months relative to the exit in state s. $Treated_i$ equals 1 if the enrollee is in an exiting plan, i.e. is forced to switch out of her plan, and equals 0 otherwise. γ_i is an enrollee fixed-effect, which controls also for enrollees' county and state of residence as all movers are dropped from the sample. δ_{st} is a state-specific time fixed effect, and the equation also includes a month of year fixed effect to account for possible seasonality in some services. The coefficient of interest is β_l which is the average of the monthly treatment effects across all states. The empirical identification of causal treatment effects rely on the assumption that absent the exit of a plan, its beneficiaries would have shared similar trends in utilization with beneficiaries in the remaining plans. This parallel trends assumption is supported by the estimated event studies.

My main analysis estimates the equation on a sample that includes two years around the exit, excluding observations between the contracts award milestone and the actual exit (7 months to 1 month before the exit).[13] After contracts are awarded, plans, providers, and beneficiaries may behave differently due to the imminent exit, leading to anticipatory effects on utilization and, moreover, on plans' data reporting. Excluding this pre-exit period allows to focus on the effect of the involuntary switch itself. The (omitted) base period is set to 8 months before the exit — the month before any state begins to award contracts. An additional analysis of monthly event studies includes all the observations in the two years around the exit, without excluding the awards-to-exit period. The results of this analysis are presented in Figures A1 and A2 in the Appendix. In all versions, I estimate equation 1 using ordinary least squares (OLS) and cluster standard errors at the county level, as contracts are mostly signed with insurers for a specific county or for a service area that includes several counties.

In addition to the graphic presentation of monthly event studies, I present in tables results from difference-in-differences event studies that pool the months

[13] As cumulative churn rates out of Medicaid are significant, the setting and the empirical approach are not suitable for examining long-term outcomes.

after the exit into half-years:

$$(2) \qquad Y_{ist} = \sum_l \beta_l 1\{H_t - HExit_s = l\} * Treated_i + \gamma_i + \delta_{st} + month_t + \varepsilon_{it}$$

where $HExit_s$ is the quarter of exit in the state, $l = \{0, 1\}$, and the rest is similar to the event-study regression above. In the main analysis, the pooled event study regressions are run on a sample that excludes the awards-to-exit period. Thus they estimate the effect of being in the treatment group of involuntary switchers, comparing to the control group of beneficiaries in remaining plans, relative to the period starting at 12 months before the exit and ending before contracts are awarded in any state, 8 months before the exit.

In addition to the main reduced-form DID analysis of plan exits, I also examine a specification in which enrollment in an exiting plan serves as an instrumental variable to beneficiaries' switch from one plan to another. As almost all beneficiaries in the treatment group switch out of their plan immediately at the time of the exit, and only a small share of the control group switches voluntarily at this time, the differences between the reduced-form and the IV estimation are small (see Appendix A.3).

3.2. Anticipatory effects: disruptions after contracts are awarded

The exit of a plan from a county does not come as a surprise. Plans know they lost in a bid well before their service is due to end, and their providers and enrollees are notified some time after that. Because of this information shock, the effects of a (future) exit on utilization may manifest even before the exit occurs: First, plans may have "horizon effects", as their incentives to invest in their enrollees' health is weaker due to their short horizon in the plan (Fang and Gavazza (2011) find evidence for such effects in the employer-sponsored market, for employees with high turnover); Second, providers may stop seeing new patients from an exiting plan, or leave the plan's network to form contracts with plans that remain in the market; Third, enrollees may either avoid some care until switching to the new plan, or alternatively, may hoard prescription drugs and rush to receive care from their familiar providers before the exit; Lastly, apparent changes in utilization may be the result of a weaker incentive for exiting plans to report accurate encounter data, as their effect on next-year's risk-adjusted income and quality measures are less of a concern. Such differential data reporting between exiting and remaining plans could have biased estimates of the effect of switching, had the awards-to-exit-period been included in the pre-exit baseline period.

To study possible anticipatory effects, I estimate pooled event studies regressions that examine utilization around the contract awards date — the bid milestone in which the state reveals the winners and losers in the bid. Timing variables are re-assigned in this analysis, relative to the month of awards in each state. The estimated regression is similar to the one described in Equation 2, but the event is different, and the sample period ends before the actual exit (the exit occurs 5 to 7 months after the awards in the sample). The pooled half-year event-study equation is:

$$Y_{ist} = \beta AwardToExit_{st} * Treated_i + \gamma_i + \delta_{st} + month_t + \varepsilon_{it} \quad (3)$$

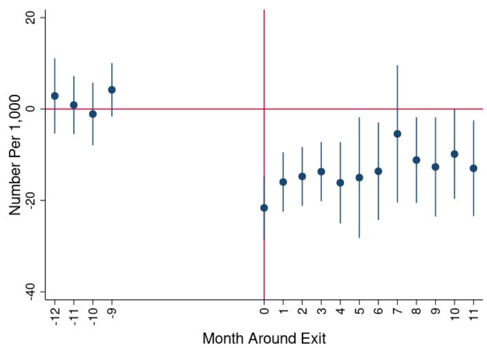
where $AwardToExit_{st}$ equals 1 if month t occurs after contracts are awarded in the state's bid, and before the plan's actual exit. The rest is the same as in Equation 1. The base period is the pre-award period.

An extreme case of differential data reporting after the contracts-award milestone is apparent in plans from Washington — data on utilization of prescription drugs is almost completely missing for exiting plans after contracts are awarded. Claims for other services in Washington's exiting plans and for all services in other states do not show such a sharp differential drop in the awards-to-exit period. Due to this missing data, all regressions that estimate the effects of contracts award on the utilization of prescription drugs or on spending, exclude observations from Washington. An additional regression focuses on two states, Texas and Missouri, where Medicaid pharmacy benefits were carved out in the examined period. As the states' fee-for-service programs payed directly for drugs, data reporting on drugs utilization is stably reliable around the contracts award milestone for all plans.

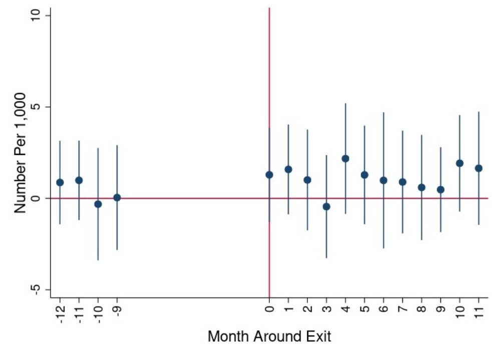
4. Results

4.1. Utilization of services and prescription drugs after an exit

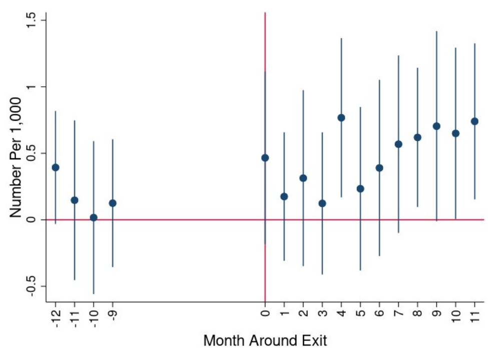
I examine the dynamic effects of involuntary switches due to plan exits on utilization. The reduced-form event studies around a plan exit are presented in Figure 2 and they show that beneficiaries in exiting and non-exiting plans mostly share similar trends in utilization at the beginning of the pre-exit year, before contracts are awarded in their state's MMC bid. After the exit, disruptions in the care provided to switchers are evident.



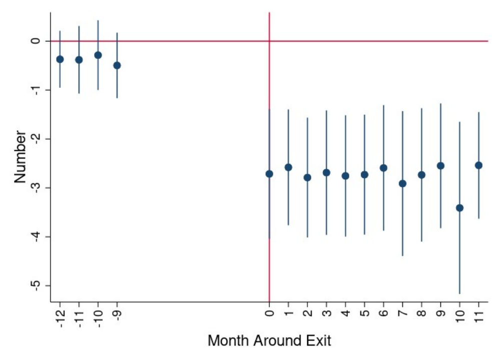
(a) Visits to Primary Care Physicians



(b) Visits to Emergency Departments



(c) Inpatient Admissions



(d) Days of Supply in Filled Prescriptions

Figure 2. Utilization of services and drugs around an exit-induced switch

Notes: Figure shows (reduced-form) event studies two years around the time plans exit the market and new contracts go into effect (marked by a red vertical line). Data points are the coefficients β_l from Equation 1 for each month around the exit. They show the utilization of beneficiaries in exiting plans (”switchers”), relative to beneficiaries in remaining plans, with the month 8 months before the exit as the base period (controlling for individual and state-specific time fixed effects, as well as month of year fixed effects). Since seven months before the exits states begin to award contracts to winners in the bids, all the period after this point and up to the exit is washed out in this analysis (shown as a blank area in the figures). Event studies for the whole two years around exit, that include this wash-out period, are presented in Figure A1 in the Appendix.

Table 3 presents estimates of the half-year pooled DID regressions. The number of monthly visits to primary care physicians (PCPs) is lower for switchers by 9.2% during the first half-year after switching — 17.6 fewer visits per 1,000 switchers. A low level of PCP visits persists during the second half of the post-exit year (6.4% lower than the baseline). Switchers increase their use of hospitals' emergency departments (ED) in the year after the switch, but the increase for the whole group is small (1.4% of the baseline) and I am unable to reject the null of no change in ED utilization. Lastly, switchers are admitted more often to hospitals at the second half of the post-exit year — the number of hospital admissions increases then by 9.8% relative to the baseline (0.47 additional admissions per 1,000 switchers). Table A4 in the appendix presents the effects of a switch on the utilization of additional services, including the number of visits to specialists, that also decreases after a switch.

Switchers' consumption of prescription drugs drops after the exit-induced switch. Relative to the period before contracts are awarded, and comparing to beneficiaries in remaining plans, the number of days of supply in switchers' filled prescriptions is lower by 16% (2.4 days). This lower level of utilization persists later in the year. Columns (5) and (6) in Table 3 examine the utilization of prescription drugs to treat some chronic diseases, focusing on patients that were using these drugs at the year before the exit. Among such patients that are forced to switch out of their plan, utilization of chronic medications decreases. The number of days' supply decreases by up to 11.3% (2.7 days) for anti-diabetic drugs, and by up to 6.9% (1.4 days) for anti-depressants and anti-psychotics. This lower utilization of prescription drugs may contribute to the increase in hospital use by the end of the switching year. Chandra et al. (2010) present such an "hospitalization offset" for Medicare beneficiaries whose utilization of prescription drugs and physicians declines after an increase in cost sharing.

4.2. Spending after a switch

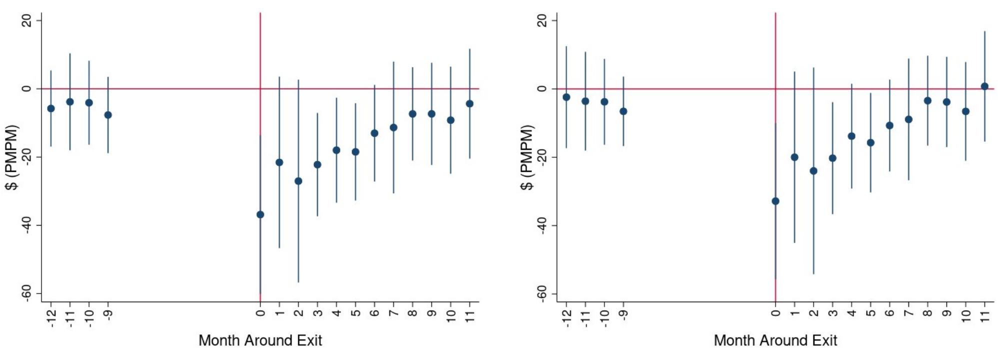
Before contracts are awarded in the MMC bid, beneficiaries in exiting and remaining plans share similar trends in spending (Figure 3). After the exit, insurers' spending on involuntary switchers is lower, compared to control beneficiaries. The monthly spending per switcher (PMPM) is lower by 6.2% ($19.8) in the first half-year, but the gap almost disappears towards the end of the switching year (Table 4). Annual plans' spending on switchers is lower than the baseline by 3.9% in the

Table 3. Effects of switch on utilization

	(1)	(2)	(3)	(4)	(5)	(6)
					Among pre-exit users	
Periods Interacted w. Treated Indicator	PCP Visits per 1,000	ED Visits per 1,000	Inpatient Admissions per 1,000	Days Supply All Drugs	Days Supply Diabetes	Days Supply Mental Health
Post-switch H1	-17.59	0.83	0.21	-2.40	-2.62	-1.36
	(3.38)	(0.89)	(0.19)	(0.54)	(0.85)	(0.36)
Post-switch H2	-12.35	0.77	0.47	-2.48	-2.72	-1.27
	(6.43)	(0.96)	(0.19)	(0.48)	(0.75)	(0.43)
Baseline Mean	191.8	59.3	4.8	15.0	24.1	19.8
# of observations	19,340,936				220,511	555,132
# of beneficiaries	1,166,430				13,640	33,996
# of counties	354				219	256

Notes: The table presents the estimated effects of exit-induced involuntary switching on the monthly utilization of services and prescription drugs. It shows the (reduced-form) estimates of the pooled DID event studies described in Equation (2), that control for individual and state-specific time fixed effects, as well as month of year fixed effects. Columns (1) to (4) examine the entire analytic sample. In columns (5) and (6), changes in the utilization of prescription drugs that treat chronic conditions are estimated for beneficiaries that used such drugs pre-exit. Mental Health prescription drugs (column 5) include anti-depressants and anti-psychotics. Baseline means are calculated 12 months before the exit. Standard errors are clustered at the county level and shown in parentheses.

switching year,[14] saving \$151 on each new enrollee coming from an exiting plan. As fee-for-service (FFS) spending doesn't change much for switchers, relative to other beneficiaries, the decrease in total spending for switchers is similar to the decrease in plans' spending — 5.3% in the first half-year after the switch. This decrease disappears almost completely by the end of the year.



(a) Managed Care Plans' Spending

(b) Total Medicaid Spending

Figure 3. Estimated spending per beneficiary around plan exits

Notes: Figures show (reduced-form) event studies that examine spending two years around the time plans exit the market when new MMC contracts go into effect (marked by a red vertical line). Figure (a) presents the spending of the health plans, estimated using prices from the fee-for-service (FFS) Medicaid program, and figure (b) presents the total Medicaid spending, that includes both health plans' spending and spending through the public FFS program. Data points (estimated from Equation 1) show the monthly spending on beneficiaries in exiting plans, relative to beneficiaries in remaining plans, with the month 8 months before the exit as the base period. Since seven months before the exits states begin to award contracts to winners in the bids, all the period after this point and up to the exit is washed out in this analysis (shown as a blank area in the figures). Event studies for the whole two years around exit, that include this wash-out period, are presented in Figure A2 in the Appendix.

4.3. Anticipatory effects after contracts are awarded

Table 5 presents the pooled DID estimates for the effect of the contract awards milestone on utilization among beneficiaries in plans that are about to exit the market. The results suggest that switchers' utilization of some services decreases

[14] Annual spending changes were estimated using additional regressions (not shown), in which a single post-exit period was used, instead of two half-years.

Table 4. Effects of switch on spending

Periods interacted w. Exit-Switcher Indicator	(1) Estimated Plans' Spending $ (PMPM)	(2) Fee-For-Service Spending $ (PMPM)	(3)=(1)+(2) Estimated Total Spending $ (PMPM)
Post-switch H1	-19.84	1.92	-17.93
	(8.93)	(2.77)	(8.55)
Post-switch H2	-4.59	2.30	-2.28
	(8.16)	(2.67)	(6.92)
Baseline Mean	321.7	18.1	339.8
# of observations		19,340,936	
# of beneficiaries		1,166,430	
# of counties		354	

Notes: The table presents the estimated effects of exit-induced involuntary switching on monthly spending. It shows the (reduced-form) estimates of the pooled DID event studies described in Equation (2), that control for individual and state-specific time fixed effects, as well as month of year fixed effects. Plans' spending (column 1) is estimated using prices from the Medicaid fee-for-service system (FFS). Total spending (column 3) includes both health plans' spending and spending through the public FFS program. Baseline means are calculated 12 months before the exit. Standard errors are clustered at the county level and shown in parentheses.

already in the pre-exit period after contracts are awarded, but the decreases are small and other services show no change in this period. The number of visits to PCPs is lower for (future) switchers by 3.9% relative to the baseline (six months before the milestone). For ED visits and inpatient admissions, there is no significant change in utilization. For the sample that excludes Washington, the number of days' supply in switchers' filled prescription is lower in the awards-to-exit period by 3.1%. However, focusing only on states where the drug benefit is carved out during this time, and hence data may be more stably reliable around the awards milestone, there is no significant change in the utilization of prescription drugs at this period. Lastly, there is no significant change in plans' spending or in total Medicaid spending in the awards-to-exit period (Table A3 in the Appendix).

Table 5. Effects of the contract awards milestone on utilization

	(1)	(2)	(3)	(4)	(5)
	PCP Visits per 1,000	ED Visits per 1,000	Inpatient Admissions per 1,000	Days Supply All Drugs	Carved Out Days Supply All Drugs
Award to Exit Period X Exiting Plan Indicator	-7.46	-1.64	-0.17	-0.57	-0.10
	(3.81)	(1.27)	(0.24)	(0.30)	(0.12)
Baseline Mean	191.8	60.7	4.5	18.6	12.3
# of observations		12,830,730		10,262,010	6,399,024
# of beneficiaries		1,166,430		932,910	533,252
# of counties		354		315	213

Notes: Table shows estimates of the impact of contract awards milestones in states' MMC bids, on beneficiaries in plans that didn't win a new contract (exiting plans), comparing to beneficiaries in remaining plans, and relative to the pre-awards period. Columns (1) to (3) examine the entire analytic sample. Column (4) is estimated on a sample that excludes Washington, where drug claims are mostly missing for exiting plans after the contracts award milestone. The results in column (5) are estimated on a sample that includes only Missouri and Texas, where drug benefit is carved out of plans' coverage and drugs are paid by the public fee-for-service program during this period. This makes the utilization data more stably reliable around the contracts award milestone. The table's results come from estimating Equation 3, that controls for individual and state-specific time fixed effects, as well as month of year fixed effects. Baseline means are calculated 6 months before the awards milestone. Standard errors are clustered at the county level and shown in parentheses.

5. Heterogeneity

5.1. Heterogeneity by age

Almost 80% of switchers in the sample are children and young adults under the age of 20. To examine whether the effects of switching for this group is different than the effect for adults, I repeat the DID estimation for these two groups separately. The results suggest that children are more sensitive to disruptions in their care after switching, and have more adverse health outcomes than adults. The estimates in Table 6 demonstrate that the effect on utilization of hospital services is different for the two groups. While children switchers have 3.2% to 3.6% more visits to emergency departments (ED) during the year after the switch, the number of adults' visits to ED shows little change after they switch. The number of hospital admissions of child switchers increases by 15.2% in the second half-year after the switch, higher than a 10.2% increase for adults. More than third of the increase in hospital admissions for children seem to be due to ambulatory-care-sensitive conditions (an estimate on the border of significance). The number of such avoidable admissions doesn't change much for adults. Lastly, the number of visits to PCPs and the utilization of prescription drugs decreases in a similar way for children and adults, relative to their baselines (Table A5 in the Appendix). This results are inline with Lavarreda et al. (2008) who find that children in fair or poor health, that switch to another health insurance, have much higher odds of reporting a delay in care than adults.

Table 6. Effects of switch on use of hospital services, children vs. adults

	(1)	(2)	(3)	(4)	(5)	(6)
Periods X Exit-Switcher Indicator	ED Visits per 1,000		Inpatient Admissions per 1,000		ACSC-Related Admissions per 1,000	
	Children	Adults	Children	Adults	Children	Adults
Post H1	1.54	-0.32	0.18	0.75	0.14	0.16
	(0.85)	(0.22)	(0.10)	(0.65)	(0.09)	(0.26)
[Relative to baseline (%)]	[3.2]	[-0.3]	[8.6]	[5.1]	[15.6]	[4.9]
Post H2	1.76	-0.48	0.32	1.51	0.13	-0.10
	(0.87)	(0.28)	(0.11)	(0.65)	(0.08)	(0.22)
[Relative to baseline (%)]	[3.6]	[-0.5]	[15.2]	[10.2]	[14.4]	[-3.0]
Baseline Mean	48.9	98.0	2.1	14.8	0.9	3.3
# of observations	16.1m	3.2m	16.1m	3.2m	16.1m	3.2m
# of beneficiaries	962,083	204,347	962,083	204,347	962,083	204,347
# of counties	354	244	354	244	354	244

Notes: This table presents estimates of the effect of an exit-induced involuntary switch on the monthly utilization of hospital services for children (under age 20), and for adults. Ages are measured at the month before the exit. The examined services are the number of visits to emergency departments (columns 1 and 2), the number of inpatient hospital admissions (3,4), and the number of hospital admissions due to ambulatory-care-sensitive conditions (ACSC), deemed preventable with appropriate community care (5,6). The table shows the (reduced-form) estimates of the pooled DID event studies described in Equation (2). Baseline means are calculated 12 months before the exit. Standard errors are clustered at the county level and shown in parentheses.

5.2. Heterogeneity by race

To examine whether switching health plans leads to different effects on white beneficiaries and non-whites, I repeat the estimation separately for these two groups. Non-white beneficiaries are admitted more often to hospitals than whites after involuntarily switching to a new health plan (Table 7) — at the second half-year after a switch the number of hospital admissions is higher by 14.9% for non-whites, while the increase for whites is lower (5.9%) and the null hypothesis of no change in whites' admissions can not be rejected. A large share of the additional admissions for non-whites comes from preventable admissions, related to ambulatory care sensitive conditions (ACSC). While the number of ACSC-related admissions increases by 25.4% for non-white beneficiaries in the second half-year after the switch, it *decreases* for whites by 18% (an estimate on the border of significance).[15] The effects of a switch on non-inpatient services are similar for both groups (Table A6 in the appendix).

5.3. Heterogeneity by pre-exit utilization

Sick beneficiaries with high use of health care services may be affected differently than healthier beneficiaries after an involuntary switch. Such sicker beneficiaries may be more sensitive to disruptions, but may also try harder and receive more assistance to navigate their care in the new plan. To examine this issue I identify pre-exit "heavy-users", i.e., beneficiaries with some utilization of services during at least four out of the five months before contracts are awarded. I repeat the estimations separately for heavy-users from the treatment and control groups[16] (24% of the sample) and for the rest of the beneficiaries ("light users"). The results (presented in Table 8) show that relative to their baseline, heavy users suffer less disruptions in the utilization of primary care services and prescription drugs than light users — the number of PCP visits in the first half-year decreases by 4.8% for

[15] Since the share of children is higher among non-white beneficiaries than among white beneficiaries, I repeat the estimation for beneficiaries under the age of 20 (Table A7 in the Appendix). I find similar results — switching health plans leads to a significant increase in the number of preventable hospital admissions for non-white children, while this number does not change significantly for white children.

[16] Since the control group includes the same type of users as the treatment group, effects of possible regressions to mean among heavy or light users should be netted out in the estimates. This assumes that the patterns of regression to the mean are similar in both the treatment and control groups.

Table 7. Effects of switch on use of hospital services, by race

Periods X Exit-Switcher Indicator	(1)	(2)	(3)	(4)
	Inpatient Admissions per 1,000		ACSC-Related Admissions per 1,000	
	White	Non-white	White	Non-white
Post H1	0.07	0.36	-0.14	0.33
	(0.22)	(0.22)	(0.14)	(0.14)
[Relative to baseline (%)]	[1.3]	[8.4]	[-9.3]	[25.4]
Post H2	0.31	0.64	-0.27	0.33
	(0.21)	(0.25)	(0.16)	(0.11)
[Relative to baseline (%)]	[5.9]	[14.9]	[-18.0]	[25.4]
Baseline Mean	5.3	4.3	1.5	1.3
# of observations	5.9m	13.5m	5.9m	13.5m
# of beneficiaries	368,778	815,921	368,778	815,921
# of counties	317	324	317	324

Notes: The table presents estimates of the effects of involuntary switch on whites' and non-whites' utilization of hospital services — the number of inpatient hospital admissions (columns 1 and 2), and the number of hospital admissions due to ambulatory-care-sensitive conditions (ACSC), deemed preventable with appropriate community care (3,4). The table shows the (reduced-form) estimates of the pooled DID event studies described in Equation (2). Baseline means are calculated 12 months before the exit. Standard errors are clustered at the county level and shown in parentheses.

heavy-users vs. 15.5% for light users. However, the sick heavy users seem more sensitive to these disruptions — heavy users are admitted more often to hospitals throughout the whole year after the switch — by 7.8% more already in the first half-year, and by 2.4% afterwards. The increase in the number of admissions of heavy users is driven by a higher number of preventable admissions (ACSC-related), which increase by 13.8% in the first half-year after the switch. For light users, utilization of inpatient services doesn't change significantly.

Table 8. Effects of switch on utilization, by pre-exit utilization

Periods X Exit-Switcher Indicator	(1) PCP Visits per 1,000	(2)	(3) Days Supply All Drugs	(4)	(5) Inpatient Admissions per 1,000	(6)	(7) ACSC-Related Admissions per 1,000	(8)
	Heavy	Light	Heavy	Light	Heavy	Light	Heavy	Light
Post H1	-23.2	-14.5	-6.91	-0.92	1.09	0.04	0.58	-0.02
	(6.76)	(2.51)	(1.43)	(0.27)	(0.50)	(0.09)	(0.30)	(0.05)
[Relative to baseline (%)]	[-4.8]	[-15.5]	[-13.4]	[-35.4]	[7.8]	[2.4]	[13.8]	[-4.0]
Post H2	-8.54	-11.2	-7.43	-0.82	0.34	0.02	0.42	0.01
	(13.64)	(4.71)	(1.29)	(0.23)	(0.10)	(0.12)	(0.29)	(0.06)
[Relative to baseline (%)]	[-1.8]	[-12.0]	[-14.4]	[-31.5]	[2.4]	[1.2]	[10.0]	[2.0]
Baseline Mean	483.6	93.6	51.7	2.6	14.0	1.7	4.2	0.5
# of observations	4.7m	14.7m	4.7m	14.7m	4.7m	14.7m	4.7m	14.7m
# of beneficiaries	286K	881K	286K	881K	286K	881K	286K	881K
# of counties	327	322	327	322	327	322	327	322

Notes: The table presents the estimates of the effects of involuntary switching on utilization for two treatment groups: sicker ”heavy-users”, defined as beneficiaries that had some utilization of medical services during at least four out of the five months in the pre-exit year before contracts are awarded (24% of beneficiaries), and healthier ”light users” which are the rest of the beneficiaries. ACSC-related admissions are hospital admissions due to ambulatory-care-sensitive conditions, deemed preventable with appropriate community care. The table shows the (reduced-form) estimates of the pooled DID event studies described in Equation (2). Baseline means are calculated 12 months before the exit. Standard errors are clustered at the county level and shown in parentheses.

6. Mechanisms

6.1. Changes in the network of providers after an exit

The networks of Medicaid managed care plans never include all the providers in their service area. Using plan directories data, Graves et al. (2020) finds that the median MMC plan includes less than 30% of physicians or hospitals within a 60-minutes drive of potential enrollees, and that among in-network providers, close to 40% are not included in any other MMC network. Insurer switching, when networks are narrow and exclusive, may disrupt the continuity of care and break patients’ relationships with their familiar providers.

To examine this potential mechanism, I first measure the extent of changes in provider networks after a switch to another plan. Columns (1) to (4) in Table 9 present the pooled DID estimates for the effects of switching on the shares of known providers among providers visited each month. In the baseline period, one year before plans exit, about 70% of visits to outpatient providers (PCPs, specialists, and other providers) are made to providers that were already seen at the previous year (column 1 and 2). Throughout the year after the exit, comparing to beneficiaries in non-exiting plans, the share of switchers' known providers is lower by around 20 percentage points. This means that only about half of all switchers' visits to outpatient providers are now made to familiar providers. This result is consistent with Chernew et al. (2004), that find that switchers to a new plan in the employer-sponsored market have a 50% likelihood of keeping their physicians. The share of known pharmacies in which switchers fill their prescriptions decreases by 10 to 11 percentage points at the year after the exit (12-13% lower than the baseline mean). While the networks of out-patient providers and pharmacies change significantly for switchers due to a plan exit, the access to familiar hospitals remains mostly unchanged.

In addition to calculating the share of known providers, I study specifically the role of losing access to one's primary care physicians (PCPs). The analysis is focused on a subsample of beneficiaries that had at least one PCP visit in the pre-exit year, before contracts are awarded. This subgroup constitutes 53% of the full sample. I identify beneficiaries that lose access to their PCP by examining whether their PCPs from the pre-exit year are part of the network in their post-exit MMC plan[17]. If all the pre-exit PCPs are missing from the network during the whole post-exit year, I classify the beneficiary as a "PCP loser". I find that 23% of switchers lose access to their PCPs after switching, while the share of PCP losers is only 3% among enrollees in remaining plans.

I examine separately the utilization and health outcomes of switchers that are PCP losers and "PCP keepers" after the exit. The control group in both cases includes all the beneficiaries from remaining plans who had a pre-exit PCP visit. Since beneficiaries may choose the plan into which they switch after their plan exits, and thus may choose whether they lose or keep their PCPs, these estimates may no longer be considered causal.[18] Table 10 presents the results of the two

[17] I define the post-exit plan as the beneficiary's plan at the first month after the exit. A small number of switchers in the treatment group switch again after the first month, voluntarily, and may reconnect with their PCP then.

[18] Notably, PCP keepers have higher utilization of most services in the baseline period, suggesting that higher utilization may be associated with a more active choice of plan post-exit.

Table 9. Effects of switch on shares of known providers and known prescription drugs

Periods interacted w. Exit-Switcher Indicator	(1) Primary Care Physicians	(2) Other Outpatient Providers	(3) RX Providers (Pharmacies)	(4) Hospitals (Inpatient)	(5) Drugs (by NDC)
Post-switch H1	-24.08	-22.72	-11.17	1.42	-3.96
	(8.00)	(7.26)	(3.10)	(3.94)	(0.79)
Post-switch H2	-23.05	-19.50	-9.76	6.26	-2.98
	(7.49)	(6.74)	(3.10)	(4.07)	(0.56)
Baseline Mean	71.0	67.2	83.7	17.0	53.5
# of observations	2,701,469	3,371,646	4,322,457	32,440	4,322,457
# of beneficiaries	622,149	763,859	690,967	11,584	690,967
# of counties	313	347	347	207	347

Notes: This table presents the estimates for the effect of an involuntary switch on the share of known providers and known prescription drugs used by switchers. The shares are estimated among utilizers of the service in each month. A known provider is a provider that the beneficiary has seen during the previous (pre-exit) year. A known drug is a drug for which a prescription was filled during the previous (pre-exit) year, identified by its National Drug Code (NDC). Column (2) presents the estimates for outpatient providers, excluding primary care physicians (PCPs) that are examined in column (1). The table shows the (reduced-form) estimates of the pooled DID event studies described in Equation (2). Baseline means are calculated 12 months before the exit. Standard errors are clustered at the county level and shown in parentheses.

estimations.

Losing access to one's PCP is associated with an 11% decrease in the number of PCP visits during the first half-year after a switch, twice as high as the rate of decrease for switchers whose PCPs remain accessible in their new plan. While the decrease in the number of PCP visits subsides for PCP keepers later in the year (only 2.2% lower than the baseline), the decrease worsens for PCP losers, that visit their PCPs 12.6% less often than the baseline during this period.

Losing a PCP is correlated with worse disruptions to care: higher use of emergency departments throughout the post-switch year (by up to 9.7%), while PCP keepers' use of ED barely changes; up to 33% more hospital admissions, partly due to preventable causes (not shown), in contrast to 4.3% increase among PCP keepers (and for keepers, the null of no change in admissions can not be rejected); Lastly, PCP losers have tremendously lower utilization of prescription drugs (55% decrease in the number of days supply in filled prescriptions during the first half-

Table 10. Correlations of switching and utilization — losing vs. keeping access to primary care physicians

Periods X Exit-Switcher Indicator	(1) PCP Visits per 1,000	(2)	(3) ED Visits per 1,000	(4)	(5) IP Admissions per 1,000	(6)	(7) Days Supply All Drugs	(8)
	Lost	Kept	Lost	Kept	Lost	Kept	Lost	Kept
Post H1	-33.95	-22.46	4.30	0.29	0.74	0.37	-7.65	-3.07
	(5.04)	(4.07)	(1.95)	(1.79)	(0.46)	(0.35)	(1.99)	(0.53)
[Relative to baseline (%)]	[-11.0]	[-5.5]	[7.2]	[0.3]	[23.1]	[4.2]	[-55.0]	[-10.9]
Post H2	-39.04	-9.13	5.80	0.17	1.06	0.38	-6.13	-3.66
	(18.62)	(4.15)	(1.56)	(1.95)	(0.52)	(0.31)	(1.62)	(0.69)
[Relative to baseline (%)]	[-12.6]	[-2.2]	[9.7]	[0.2]	[33.1]	[4.3]	[-44.1]	[-13.0]
Baseline Mean	308.8	409.3	60.1	94.7	3.2	8.8	13.9	28.2
# of observations	9.3m	10.0m	9.3m	10.0m	9.3m	10.0m	9.3m	10.0m
# of beneficiaries	559K	604K	559K	604K	559K	604K	559K	604K
# of counties	310	309	310	309	310	309	310	309

Notes: The table presents estimates for the correlation between involuntary switching and utilization of services, among beneficiaries that visited a PCP at least once during the pre-exit year before contracts were awarded. Two treatment groups are examined: First, involuntary switchers that lost access to all their pre-exit primary care providers after the switch (i.e., the providers are missing from their new plan's network). Second, involuntary switchers who kept access to (at least one of) their PCPs. The table shows the (reduced-form) estimates of the pooled DID event studies described in Equation (2). Baseline means are calculated 12 months before the exit. Standard errors are clustered at the county level and shown in parentheses.

year vs. 10.9% decrease for PCP keepers).

Sabety (2021) finds a similar *causal* decrease (14%) in the number of PCP visits after Medicare beneficiaries lose access to their retiring or relocating PCP. In her setting, PCP exits lead to smaller increases in the number of ED visits (4%) and the number of hospital admissions (1.5%), and the utilization of prescription drugs remain mostly unchanged. However, PCP losers in my setting experience additional disruptions due to switching their entire health plan — disruptions that beneficiaries in traditional Medicare avoid, even when losing access to their familiar PCP.

6.2. Changes in prescribed drugs after a switch

After switching to a new health plan, beneficiaries may face a new drug formulary and receive prescriptions from new providers. Both changes could lead switchers to change their medication after the switch. To examine medication changes, I estimate the effect of a switch on the share of known drugs — the share of drugs prescribed during the month that were used in the pre-exit year (drugs are identified by their unique National Drug Code). The estimates are presented in Column (5) of Table 9. Comparing to beneficiaries in non-exiting plans, the share of known drugs used by switchers is lower in the first half-year post-switch by about 7.4% relative to the baseline mean (almost 4 percentage points lower). That means that after a switch, beneficiaries are being prescribed new drugs more often, suggesting that drug formularies may be changing or that new providers lead patients to change their medication. The decrease in the share of known drugs partially subsides later in the year —it is 5.6% lower than baseline in the second half-year after the switch.

6.3. Plans' effect on utilization — switching to higher vs. lower-spending plans

The plans that participate in the Medicaid Managed Care (MMC) markets in each state often differ in their average cost per beneficiary. Geruso et al. (2020) exploit random assignment to MMC plans in New York City to show that such differences can be the result of *causal* plan effects on the utilization of services. Since plans can reduce their enrollees' utilization, even when co-payments are low or zero and benefits are uniform, some of the ”disruptions” that I find after switching could be the result of the differences in plans' effect on costs. Such effects could be more permanent in nature, rather then transactional or temporary disruptions to utilization patterns.

To examine this issue, I first estimate state-level plan effects for all the pre-exit MMC plans — I estimate the correlation between each plan and the costs of its enrollees to the plan,[19] controlling for enrollees' gender, race, and age group, and for county-specific time fixed effects. My estimates are risk-adjusted *observational* measures of plans' effect on costs, and are not causal.[20] When new contract pe-

[19]The costs of these encounters are estimated using prices from the FFS system. See section 2.

[20]For the New-York City market, Geruso et al. (2020) show that risk-adjusted observational measures are correlated with causal differences between plans' utilization, although they overstate

riods begin post-exit, 39% of switchers switch to a lower-spending plan and 34% switch to a higher-spending plan.[21]

To study how plans' effects on spending is related to post-switch disruptions, I repeat my estimation for two sub-samples of switchers from the exiting plans: "up-graders", that switch to a higher-spending plan relative to their pre-exit plan, and "down-graders", that switch to a lower-spending plan. Main results for these two subsamples are presented in Table 11. While all enrollees must switch *out* of their exiting plan, some of them do actively choose the plan they switch *into* after the exit. As this post-exit choice could lead to selection bias, the estimates in Table 11 should not be interpreted as causal. Additional limitation of this analysis is that if plans' effects in the pre-exit year are impacted by temporary flactuations in utilization, regression to the mean in the post-exit period could bias the estimates. Estimating state-level effects, while exits occur at the county-level, may ameliorate this issue.

For both up-graders and down-graders, an involuntary switch is correlated with fewer visits to PCPs right after the switch, lower utilization of prescription drugs, and higher number of hospital admissions later in the switching year. This may indicate that switching disruptions are not mainly the result of a change in the composition of plans' effect after new contracts begin. In general, in higher-spending plans switchers' utilization of primary care services and prescription drugs decreases by a higher rate than in lower-spending plans (relative to baseline), but these switchers suffer more hospital admissions. This suggests that lower-spending plans are correlated with more intense use of outpatient care and lower use of (expensive) inpatient care.

the causal effect (The observational measures in Geruso et al. (2020) control also for enrollees' spending in the fee for service Medicaid system, prior to their MMC enrollment). Similarly for plans' effect on health, Abaluck et al. (2021) find that observed plan mortality rates unbiasedly predict causal mortality effects for Medicare Advantage plans.

[21]The rest of the beneficiaries either switch to a new plan that just entered the state's MMC program (23%), for which pre-exit plan effects could not be estimated, or switch to the Medicaid Fee-For-Service system.

Table 11. Correlations of utilization and switching to a higher vs. lower-spending plan

Periods X Exit-Switcher Indicator	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	PCP Visits per 1,000		ED Visits per 1,000		IP Admissions per 1,000		Days Supply All Drugs	
	Higher	Lower	Higher	Lower	Higher	Lower	Higher	Lower
Post-switch H1	-20.09	-11.55	1.77	0.03	0.35	0.25	-2.51	-1.91
	(5.43)	(3.74)	(1.28)	(1.33)	(0.25)	(0.26)	(1.25)	(0.31)
[Relative to baseline (%)]	[-11.2]	[-5.2]	[3.4]	[0.1]	[9.7]	[3.8]	[-21.5]	[-9.7]
Post-switch H2	-27.18	-1.56	1.53	0.42	0.49	0.46	-2.57	-2.41
	(11.64)	(4.33)	(1.40)	(1.82)	(0.24)	(0.30)	(0.87)	(0.60)
[Relative to baseline (%)]	[-15.1]	[-0.7]	[3.0]	[0.7]	[13.6]	[7.0]	[-22.0]	[-12.2]
Baseline Mean	179.9	222.4	51.9	64.8	3.6	6.6	11.7	19.7
# of observations	17.6m	17.7m	17.6m	17.7m	17.6m	17.7m	17.6m	17.7m
# of beneficiaries	1.06m	1.06m	1.06m	1.06m	1.06m	1.06m	1.06m	1.06m
# of counties	353	353	353	353	353	353	353	353

Notes: This table presents estimates of the correlation between involuntary switching and monthly utilization of services. Two treatment groups are examined: involuntary switchers that switch to a plan with a higher effect on cost, and those switching to a plan with a lower effect on cost. The plan effects are risk-adjusted observational measures, estimated for all pre-exit plans using costs in the first five months of the pre-exit year (before contracts are awarded in the bid). The table shows the (reduced-form) estimates of the pooled DID event studies described in Equation (2). Baseline means are calculated 12 months before the exit. Standard errors are clustered at the county level and shown in parentheses.

7. Discussion

7.1. Policies to reduce non-pecuniary switching costs

Policy makers have several ways to lower non-pecuniary costs due to switching: First, they may reduce the frequency of plan exits from MMC. For example, by limiting free entry of unviable plans (i.e., contracting only with insurers that can serve beneficiaries throughout a defined period), and by lengthening the effective contract period in MMC bids. Second, policy makers may follow a classic policy recipe for tackling switching costs (Farrell and Klemperer (2007)) and increase the compatibility of MMC plans, for example, by setting uniform drug formularies and uniform clinical protocols across all plans. Dolan and Tian (2019) report that states are increasingly adopting such measures, at least for some drug classes. Alternatively, states can increase compatibility, as experienced by beneficiaries, by carving services out of MMC (e.g. drug benefits, behavioral services etc.), so switching between plans have smaller effect on their utilization.

Third, some policies directly aim to reduce frictions in the immediate period after plan switching. For a limited time after the switch, such policies allow beneficiaries to continue filling prescriptions from their previous plans (usually for 90 days), continue visiting previous providers even if they are out of the new plan's network, and utilize previous pre-authorizations. Federal regulations require that plans coordinate to ensure that individuals are able to make smooth transitions between settings of care. The regulations also require that new beneficiaries complete an initial health risk assessment within 90 days of enrollment, and that treatment plans are developed for enrollees with special health care needs.[22]

Lastly, policy makers may try to improve the initial match of beneficiaries to plans. For actively-choosing switchers, this may include providing better information and choice counseling. For auto-enrolled beneficiaries, the assignment algorithms may use prior claims to minimize the disruption to beneficiaries' effective network of providers. Since a large share of Medicaid's enrollees are passive when choosing a health plan (Layton et al. (2018)), the state-defined auto-assignment rules may have a large impact on switching disruptions. It should be noted that some of the policies to reduce aggregate switching costs may come at a price of weakening competition between managed care plans, and limiting plans' ability to use managed care tools to control utilization and lower costs.

7.2. Gains from rebidding vs. costs of switching

The existence of significant non-pecuniary switching costs when changing a health insurer creates a tradeoff in government procurement (and potentially also in employers' contracting with insurers) — replacing a current contractor to reduce spending and/or improve quality comes with the transactional cost of disruptions to enrollees' care and health. If regulators and employers act as agents for their beneficiaries or employees and internalize these non-monetary costs, the switching costs may be a strong incentive to keep renewing a contract with a current

[22]"Enrollment process for Medicaid managed care" web page on the MACPAC website. https://www.macpac.gov/subtopic/enrollment-process-for-medicaid-managed-care/

insurer, creating a lock-in situation. This is a familiar result in the literature about switching costs (Farrell and Klemperer (2007)). This paper does not estimate the full benefits for Medicaid programs from rebidding an MMC contract, which requires information on the unfulfilled bids that exiting plans submitted in the bidding process. However, the post-exit reduced spending on switchers creates in itself some savings to the Medicaid program, as it may help decrease the capitated payments to insurers during the next few years.[23] Medicaid programs may put more weight on the potential savings from rebidding and less weight on beneficiaries' disruptions, especially if they are not perfect agents for their beneficiaries.

As involuntary switches save public funds, one may also think of them as ordeals — access hurdles that may improve the targeting efficiency of a public program (Nichols and Zeckhauser (1982)). However, I find that switches increase the number of avoidable hospital admissions, especially for children. As Hendren and Sprung-Keyser (2020) calculations show, direct investments in low-income children's health and education have historically had the highest Marginal Value of Public Funds. Thus, a policy that achieves cost savings by harming the health of low-income children is most likely not well-targeted.

8. Conclusion

I find substantial disruptions to the utilization of health services and prescription drugs after incumbent MMC plans don't win a new contract in a state bid, forcing all their enrollees to switch to another plan. Higher rates of hospital admissions suggest that these plan exits also lead to adverse health outcomes, especially for children, non-whites, and sicker beneficiaries. I find evidence that significant changes in beneficiaries' networks of out-patient providers and pharmacies after an exit serve as mechanisms, as well as changes in drug formularies.

As public programs such as Medicare and Medicaid rely more and more on regulated competition between private plans to provide insurance to beneficiaries (Gruber (2017)), switches *between* health plans become ever more prevalent even for the elderly, for people with disabilities, and for people with low income. This encourages policymakers to adapt a host of measures to decrease disruptions due

[23] Capitated payments must be "actuarially sound" and are based on recent plans' spending. See CMS's Medicaid Managed Care Rate Development Guides for details (https://www.medicaid.gov/medicaid/managed-care/guidance/rate-review-and-rate-guides/index.html).

to exit-induced plan switches. These measures include policies that provide a longer transition period after a switch and policies that improve the initial match between beneficiaries and plans. Future research may explicitly examine these policies, their effectiveness in reducing switching costs, and their impacts on competition and costs.

Appendix A. Appendix

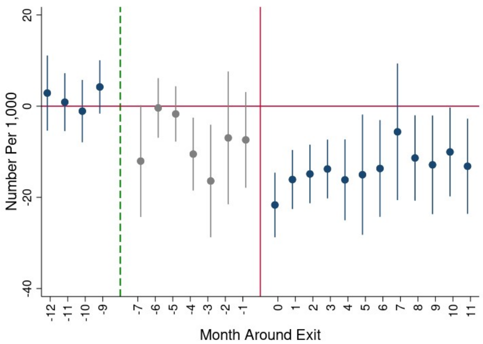
Appendix A.1. Sample Selection

Appendix Table A1. Sample selection

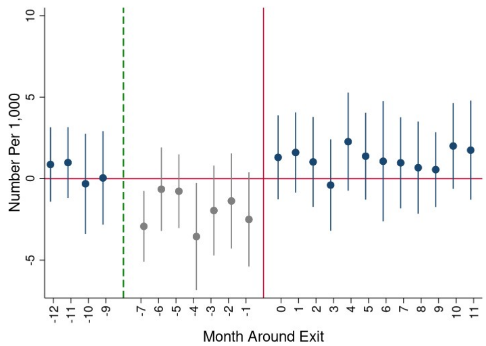
	Number of Beneficiaries	
Sample restrictions	Treatment	Control
1) In a MMC plan 1 month pre-exit	377,134	3,001,113
2) And: In sample 18 months pre-exit	327,354	2,452,406
3) And: Continuously in the same plan 18 months pre-exit	164,843	1,001,587

Notes: The table shows how the sample selection criteria affect the sample size. The treatment group includes beneficiaries enrolled in exiting plans at the month before the exit. The control group includes enrollees of non-exiting plans during this month. The selection criteria restrict the sample to beneficiaries that appeared in the MAX data throughout the 18 months pre-exit (excluding beneficiaries churning in and out of Medicaid), and were continuously in the same health plan during this period.

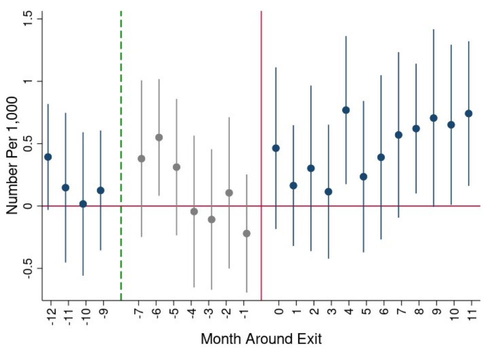
Appendix A.2. Event Studies of Whole Two Years Around Exit



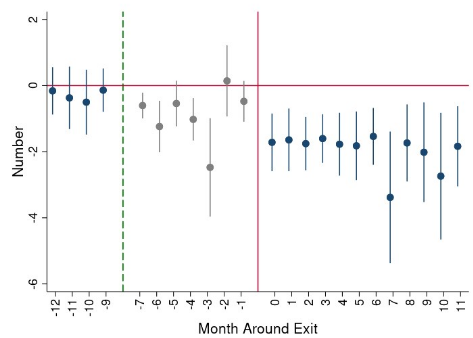
(a) Visits to Primary Care Physicians



(b) Visits to Emergency Departments



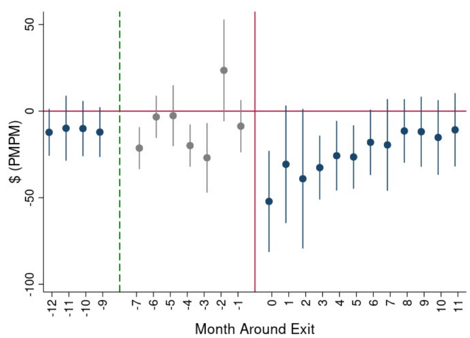
(c) Inpatient Admissions



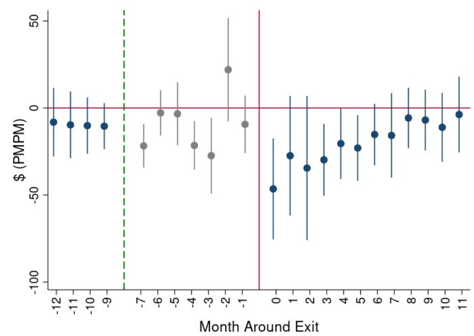
(d) Days of Supply in Filled Prescriptions

Appendix Figure A1. Utilization of services and drugs around involuntary switch

Notes: Figure shows (reduced-form) event studies two years around the time plans exit the market and new contracts go into effect (marked by a red vertical line). Seven months before the exits states begin to award contracts to winners in the bid and the beginning of this period is marked by a green dashed line. As reporting on prescription drugs is mostly missing for exiting plans in Washington during the awards-to-exit period, the event study in figure (d) is estimated on a sample that excludes beneficiaries from Washington. Data points in the graphs are the coefficients β_l from Equation 1 for each month around the exit. They show the utilization of beneficiaries in exiting plans (involuntary switchers), relative to beneficiaries in remaining plans, with the month 8 months before the exit as the base period (controlling for individual and state-specific time fixed effects, as well as month of year fixed effects). Event studies in which the awards-to-exit period is washed-out, are presented in Figure 2 in the main text.



(a) Managed Care Plans' Spending



(b) Total Medicaid Spending

Appendix Figure A2. Estimated spending per beneficiary around involuntary switches

Notes: Figure shows (reduced-form) event studies two years around the time plans exit the market and new contracts go into effect (marked by a red vertical line). Seven months before the exits states begin to award contracts to winners in the bid and the beginning of this period is marked by a green dashed line. As reporting on prescription drugs is mostly missing for exiting plans in Washington during the awards-to-exit period, the event studies are estimated using a sample that excludes all beneficiaries from this state. Data points in the graphs are the coefficients β_l from Equation 1 for each month around the exit. They show the utilization of beneficiaries in exiting plans ("switchers"), relative to beneficiaries in remaining plans, with the month 8 months before the exit as the base period (controlling for individual and state-specific time fixed effects, as well as month of year fixed effects). Event studies in which the awards-to-exit period is washed out, are presented in Figure 3 in the main text.

Appendix A.3. IV Estimates

The main analysis in the paper examines the reduced form effect of plan exits on their beneficiaries' utilization and health outcomes. In this section I use a plan's exit as an instrumental variable (IV) for beneficiaries switching to another health plan. As almost all beneficiaries in exiting plans switch to another plan, and only a small share of beneficiaries in non-exiting plans switch, the IV estimates should be very similar to the reduced form estimates.

The IV estimates are local average treatment effects (LATE) for the population of beneficiaries that switch to another plan due to their plan's exit (i.e. "compliers"). The first stage regression is:

$$isSwitcher_i = \beta_1 Treat_i + \gamma_i + \varepsilon_i \tag{A.1}$$

where $isSwitcher_i$ indicates whether beneficiary i switched from one plan to another at the time new contracts came into effect in his state. $Treat_i$ indicates whether beneficiary i is enrolled in an exiting plan. γ_i is the individual fixed effect and ε_i represents a random error term. The IV regression specification is:

$$Y_{ist} = \sum_l \beta_l 1\{H_t - HExit_s = l\} * \widehat{Treat_i} + \gamma_i + \delta_{st} + month_t + \psi_{it} \tag{A.2}$$

where $\widehat{Treat_i}$ is the predicted value from equation A.1, γ_i is the individual fixed effect, δ_{st} are the state-specific time fixed effects, and ψ_{it} is a random error. β_l is the LATE for beneficiaries that switch plans when the new MMC contracts come into effect due to their plan exiting the market.

Table A2 presents the IV estimates for the main utilization variables. As expected, the IV estimates are very similar to the reduced form estimates.

Appendix Table A2. Effects of switch on utilization — IV estimates

	(1)	(2)	(3)	(4)	(5)
Periods interacted w. Exit-Switcher Indicator	PCP Visits per 1,000	ED Visits per 1,000	Inpatient Admissions per 1,000	Days Supply All Drugs	Estimated Plans' Spending $ (PMPM)
Post-switch H1	-18.81	0.89	0.22	-2.57	-21.21
	(3.56)	(0.96)	(0.20)	(0.57)	(9.54)
Post-switch H2	-13.17	0.82	0.50	-2.64	-4.98
	(6.78)	(1.02)	(0.20)	(0.50)	(8.66)
Baseline Mean	200.8	61.6	5.0	16.2	307.4
# of observations			19,340,936		
# of beneficiaries			1,166,430		
# of counties			354		

Notes: Table presents the IV estimates of the impact of involuntary switching from one plan to another at the time new contracts begin in a state after MMC bids. Plan exits serve as instrumental variable for involuntary switching. The IV equations are estimated using 2SLS. All specifications include also individual and state-specific time fixed effects and month-of-year fixed effects. Baseline means are calculated 12 months before the exit. Standard errors are clustered at the county level and shown in parentheses.

Appendix A.4. Anticipatory effects after contracts are awarded

Appendix Table A3. Effects of the contract awards milestone on spending on switchers

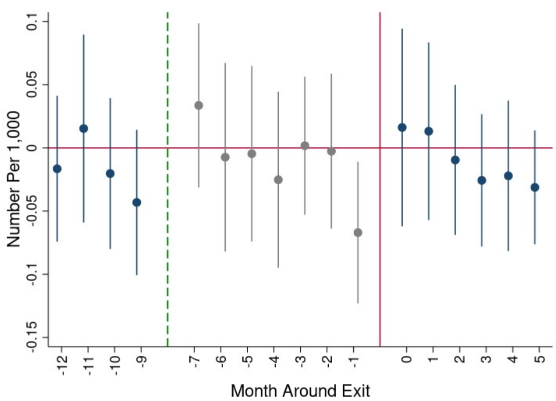
	(1) Estimated Plans' Spending $ (PMPM)	(2) Fee-For-Service Spending $ (PMPM)	(3)=(1)+(2) Estimated Total Spending $ (PMPM)
Award to Exit Period X Exiting Plan Indicator	3.61	-2.00	1.61
	(6.58)	(1.60)	(6.62)
Baseline Mean	424.3	19.8	444.1
# of observations		10,262,010	
# of beneficiaries		932,910	
# of counties		315	

Notes: Table shows estimates of the impact of contract awards in states' MMC bids, on spending on beneficiaries in plans that didn't win a new contract (exiting plans), comparing to beneficiaries in remaining plans, and relative to the pre-awards period. The sample excludes Washington, for which drug claims are mostly missing during the awards-to-exit period.The table's results come from estimating Equation 3, that controls for individual and state-specific time fixed effects, as well as month of year fixed effects. Baseline means are calculated 6 months before the awards milestone. Standard errors are clustered at the county level and shown in parentheses.

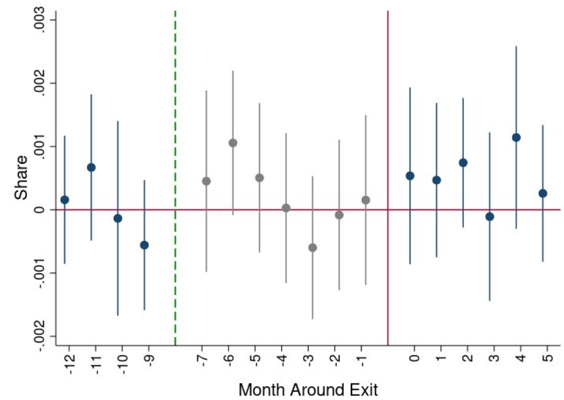
Appendix A.5. Robustness — differential reporting of encounter data

The data that I use to measure utilization around plan switches comes from the Medicaid Analytic eXtract (MAX) files and is based mainly on encounter data from Medicaid managed care plans. This data suffers from reliability issues in some states, and can be partial (Leonard et al. (2017); Li et al. (2018)). Partial reporting of encounter data is a threat to my empirical strategy only if there is a differential reporting level between exiting plans and remaining plans in the period before contracts are awarded. If this is the case, then some of the apparent changes in utilization after beneficiaries switch out of their exiting plans may be the result of the difference in reporting and are not real. To support the assumption that this is not case, I examine services that are presumably independent of plans' influence before and after the exit. If the levels of data reporting are different in exiting and remaining plans, examining such services should show a level shift in utilization immediately after beneficiaries switch. Figure A3 presents placebo-tests event studies for two such services: deliveries, and hospital admissions for acute appendicitis. In both cases, no sharp level shift in the number of services can be detected neither after the exit nor in the pre-exit period.[24] This supports the assumption of no differential level of reporting between exiting and remaining plans in the period before contracts are awarded.

[24]The only exception is the lower number of acute appendicitis admissions reported by exiting plans in the month right before the exit. This may suggest that some utilization is under-reported just before the exit occurs, but such under-reporting is not apparent when examining hospital deliveries.



(a) Acute Appendicitis Admissions



(b) Deliveries (Share Among Women, Age 15-44)

Appendix Figure A3. Services presumably independent of plans' influence around plan exits

Notes: Figure shows (reduced-form) event studies, two years around plan exits, for the number of hospital admissions due to acute appendicitis (a), and for the share of women at the ages of 15 to 44 having a hospital delivery (b). These are two acute services that are presumably independent of plans' influence. The lack of a level shift in the estimates immediately after beneficiaries switch out of exiting plans (marked by vertical red line) supports the assumption of no differential level of reporting between exiting and remaining plans in the period before contracts are awarded (left to the green dashed line) and generally also in the awards-to exit period (between the dashed green line and the red line).

Appendix A.6. Other appendix tables

Appendix Table A4. Effects of switch on utilization of services (2)

Periods interacted w. Exit-Switcher Indicator	(1) New PCP Visits per 1,000	(2) Specialist Visits per 1,000	(3) Lab Tests per 1,000	(4) Inpatient Days per 1,000	(5) ACSC-Related Admissions per 1,000
Post-switch H1	1.29	-21.53	-7.59	0.33	0.15
	(0.61)	(8.18)	(11.72)	(0.89)	(0.10)
Post-switch H2	-0.17	-18.72	-8.95	2.25	0.10
	(0.73)	(8.03)	(12.08)	(0.84)	(0.10)
Baseline Mean	13.5	315.2	422.6	15.0	1.4
# of observations			19,340,936		
# of beneficiaries			1,166,430		
# of counties			354		

Notes: The table presents the estimated effects of exit-induced involuntary switching on the monthly utilization of additional services (in addition to those presented in Table 3). It shows the (reduced-form) estimates of the pooled DID event studies described in Equation (2), that control for individual and state-specific time fixed effects, as well as month of year fixed effects. Specialist visits (column 2) are defined as outpatient visits to non-PCP physicians. ACSC-related admissions (column 5) are inpatient admissions due to ambulatory-care-sensitive conditions, deemed preventable with appropriate community care. Baseline means are calculated 12 months before the exit. Standard errors are clustered at the county level and shown in parentheses.

Appendix Table A5. Effects of switch on out-patient utilization, by age group

Periods X Exit-Switcher Indicator	(1)	(2)	(3)	(4)
	PCP Visits per 1,000		Days Supply All Drugs	
	Children	Adults	Children	Adults
Post H1	-15.82	-31.04	-1.27	-7.69
	(3.47)	(5.64)	(0.39)	(1.49)
Post H2	-12.78	-17.55	-1.08	-9.12
	(6.15)	(10.60)	(0.31)	(1.40)
Baseline Mean	156.8	321.2	6.8	45.1
# of observations	16.1m	3.2m	16.1m	3.2m
# of beneficiaries	962,083	204,347	962,083	204,347
# of counties	354	244	354	244

Notes: This table presents estimates of the effect of an exit-induced involuntary switch on the monthly utilization of primary care services and prescription drugs for children (under age 20), and for adults. Ages are measured at the month before the exit. The table shows the (reduced-form) estimates of the pooled DID event studies described in Equation (2). Baseline means are calculated 12 months before the exit. Standard errors are clustered at the county level and shown in parentheses.

Appendix Table A6. Effects of switch on utilization, by race

Periods X Exit-Switcher Indicator	(1) PCP Visits per 1,000	(2)	(3) ED Visits per 1,000	(4)	(5) Days Supply All Drugs	(6)
	White	Non-white	White	Non-white	White	Non-white
Post H1	-14.95	-19.06	0.41	1.12	-3.69	-1.51
	(3.58)	(4.16)	(1.14)	(1.13)	(0.84)	(0.33)
Post H2	-10.73	-11.79	0.23	1.44	-3.44	-1.76
	(8.74)	(5.46)	(1.21)	(1.31)	(0.65)	(0.42)
Baseline Mean	214.1	171.1	65.3	53.9	19.4	10.8
# of observations	5.9m	13.5m	5.9m	13.5m	5.9m	13.5m
# of beneficiaries	368,778	815,921	368,778	815,921	368,778	815,921
# of counties	317	324	317	324	317	324

Notes: The table presents estimates of the effects of involuntary switch on whites' and non-whites' utilization of outpatient services and prescription drugs. The table shows the (reduced-form) estimates of the pooled DID event studies described in Equation (2). Baseline means are calculated 12 months before the exit. Standard errors are clustered at the county level and shown in parentheses.

Appendix Table A7. Effects of switch on children' use of hospital services, by race

Periods X Exit-Switcher Indicator	(1)	(2)	(3)	(4)
	IP Admissions per 1,000		ACSC-Related Admissions per 1,000	
	White	Non-white	White	Non-white
Post H1	0.09	0.26	0.04	0.21
	(0.17)	(0.12)	(0.10)	(0.11)
Post H2	0.21	0.42	-0.07	0.24
	(0.17)	(0.12)	(0.11)	(0.09)
Baseline Mean	2.0	2.1	0.8	0.9
# of observations	4.5m	11.7m	4.5m	11.7m
# of beneficiaries	275,123	702,209	275,123	702,209
# of counties	317	323	317	323

Notes: The table presents estimates of the effects of involuntary switch on white children' and non-white children utilization of outpatient services and prescription drugs. Children are under 20 years old and ages are measured at the month before the exit. The table shows the (reduced-form) estimates of the pooled DID event studies described in Equation (2). Baseline means are calculated 12 months before the exit. Standard errors are clustered at the county level and shown in parentheses.

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