Essays on Health Insurance Markets with Regulated Competition

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Essays on Health Insurance Markets with Regulated Competition

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Essays on Health Insurance Markets with Regulated Competition

Abstract

This dissertation explores three issues relating to health insurance markets with regulated competition. First, I study how switching between health plans affects enrollees' utilization of health services and their health outcomes. To overcome possible self-selection into switching I exploit plan exits in the Medicaid Managed Care regulated markets. I find that switching between plans disrupts beneficiaries' care and leads to adverse health outcomes. Loss of access to familiar primary care physicians and changes in the network of providers, changes in drug formularies, and frictions in receiving prior authorizations for services - all may play a role in disrupting care after a switch.

The second dissertation chapter studies whether providing a publicly-financed insurance through a regulated market of private managed care plans costs less to the government than a direct public fee-for-service program. The chapter focuses on disabled beneficiaries in Medicaid, and exploits county-level managed care enrollment mandates to identify the fiscal effect of transitioning them to private plans. We find that these transitions eventually increase Medicaid's spending and we discuss how procurement rules may lead to this result.

Lastly, I study risk adjustment systems, that reallocate funds among competing health insurers in regulated markets. Within the setting of the U.S. Marketplaces, I demonstrate how explicitly setting the level of utilization that triggers a claims-based risk adjustor, can both increase the system's fit and decrease the incentive to game the system.

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To my wife Sarith

Acknowledgments

> Rabba bar bar Ḥana said: Why are matters of Torah compared to fire, as it is stated: "Is not My word like fire, says the Lord" (Jeremiah 23:29)? To tell you: Just as fire does not ignite in a lone stick of wood but in a pile of kindling, so too, matters of Torah are not retained and understood properly by a lone scholar who studies by himself, but by a group of Sages.
>
> — Babylonian Talmud, Tractate Ta'anit,7a, Steinsaltz Translation

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The Impact of Switching Health Insurance – Evidence from Medicaid Managed Care

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1.1 Introduction

In the U.S. health care system almost no one keeps their health insurance from cradle to grave. Switching between health plans or types of health coverage is as much a certainty in one's life as death and taxes. Every year, about one fifth of enrollees in employer-sponsored insurance experience a change in their insurance (Cebul et al. (2008)). This share is similar for enrollees in Medicare Advantage plans and for low-income adults, and is more than twice as high for enrollees with individual insurance (Sommers et al. (2016), Jacobson et al. (2016), Austic et al. (2016), Cebul et al. (2008)).

Switching health plans, with similar financial characteristics, may have had little non-monetary consequences a few decades ago, when the dominant insurance type was an open-network fee-for-service plan. Today, traditional fee-for-service plans are virtually extinct from the commercial market, and most plans use at least some managed care tools to guide their enrollees' medical care and limit their expenses.[1] Managed care plans have become ubiquitous also in public programs such as Medicaid and Medicare, that (Gruber (2017)). While all fee-for-service plans are essentially alike in their non-financial components, each limiting plan, limits its enrollees' care in its own way. These differences may create frictions when switching a plan and may directly lead to non-monetary switching costs. While a large literature estimates the effect of gaining or losing health insurance, this paper aims to add to the surprisingly small literature that explicitly studies the consequences of switching between insurance plans without a gap in coverage. Such switches may still the enrollee's care, at least for some initial period. For example, changes in the providers' network may interfere with continuity of care, breaking relationships with primary care physicians (PCPs) and with specialists (Barnett et al. (2017)) and lowering the probability of having a usual source of care

[1] In a 1980 survey of the Bureau of Labor Statistics, nearly all full time workers in large private establishments had a traditional fee-for-service plan. In 2013, 67 percent were in Preferred Provider Organization plans (PPO), 18 percent in Health Maintenance Organizations (HMO), and only 2 percent in fee-for-service plans. See: https://www.bls.gov/opub/btn/volume-4/understanding_health_plan_types.htm (Visited 8/11/21)

(Lavarreda et al. (2008)). Losing access to a familiar PCP often disrupts utilization patterns (Schwab (2018), Sabety (2021)), and discontinuities of care may be associated with adverse health outcomes (Saultz and Lochner (2005)). Furthermore, new drug formularies may force a change in the prescription drugs used, require new pre-authorizations, and reduce drug adherence.

Identifying the effect of switching on utilization and health outcomes is challenging when one examines enrollees that choose to switch, as this decision is not random. It may be motivated, for example, by dissatisfaction with their current plan, a new diagnosis, a required procedure, or a life event that changes eligibility and needs. Such self-selection into switching may bias estimates for the effect of the switch. To overcome this challenge, I exploit involuntary switches in Medicaid managed care (MMC), in which a whole group of enrollees *must* switch out of their current plan after the plan fails to win a contract in a state's bid. Since these plan exits are independent of the enrollees' preferences, selection into switching is much less a concern in this setting. Involuntary switches between insurers are very common: Ndumele et al. (2017) find that every year about 600,000 Medicaid beneficiaries are enrolled in MMC plans that exit the market; In Medicare Advantage, more than a quarter of all switchers (5% of MA beneficiaries) switch involuntarily every year after their plan exits (Jacobson et al. (2016)); In the employer-sponsored market, most switches are due to plan cancellations and job changes (Cebul et al. (2011), Cunningham and Kohn (2000)).

My main empirical analysis, explained in Section 1.3, examines non elderly beneficiaries that were continuously enrolled in their plan for at least a year and a half before a plan exit occurred in their state. While this selection criterion excludes many beneficiaries that churn in and out of Medicaid, it helps to overcome possible differential enrollment to soon-to-exit plans, and to focus on enrollees with established relationships with their providers. The main analytic sample excludes all observations from the last seven months of the pre-exit year, as in this period states begin to announce bid winners and award them new contracts. This information shock may affect services and their utilization even before the switch. I examine this period separately in the Appendix (section A.0.1).

I use a difference-in-differences framework to compare the treatment group - beneficiaries in exiting plans, forced to switch out of their insurer - to the control group of beneficiaries in plans that remain in the market after the bid's new contracts go into effect. I present event studies showing that treatment and control groups had no differential trends in outcomes before contracts are awarded in the states' bids. On top of this reduced-form estimation, I use plan exits as an instrumental variable for individuals' switches between plans at the time new contracts go into effect.

Section 1.2 presents the data I use. To identify plan exits after MMC bids, I collect public information on these bids - including information on bidders, winners and losers, and the bid's milestone dates (e.g. when contracts are awarded and when the the new contracts begin). I then use administrative data from the Medicaid Analytic eXtract (MAX) to examine enrollment and utilization around bid-induced plan exits in five states - Arizona, Minnesota, Missouri, Texas, and Washington. To verify that exiting and remaining plans don't have a differential level of data reporting that could bias post-exit estimates, I run placebo tests to verify that services that are presumably independent to insurers' influence (e.g. deliveries or admissions due to acute appendicitis) indeed show no change immediately after a switch (Section 1.7).

Section 1.4 presents the results. I find that beneficiaries in exiting plans ("switchers") experience a significant disruption to their utilization patterns of health services and prescription drugs, and suffer adverse health outcomes. Throughout the year after the switch, the share of switchers with any filled prescription is lower by about 15% relative to the baseline mean (3.7 percentage points lower). The use of prescription drugs decreases even among patients with chronic conditions. The number of days' supply in filled prescriptions for anti-diabetic drugs, for patients that use them before the switch, is lower by almost 10% during the year after the switch. The number of days' supply in prescriptions for anti-depressants and anti-psychotics is lower by 7%.

Switchers have 6% to 8% fewer visits to primary care physicians throughout most of the post-exit year, and they use emergency departments (ED) up to 5% more in the beginning of that year.

Towards the end of the switching year, switchers are admitted more often to hospitals (they have 11% more inpatient admissions) and spend more time hospitalized (14% to 21% more inpatient days).

Section 1.5 explores heterogeneity by age and pre-switch utilization levels. The results suggest that children are more sensitive to disruptions in their care after a switch, and suffer more adverse health outcomes than adults. Children's hospital admissions due to Ambulatory Care Sensitive Conditions (ACSC), deemed preventable with appropriate community care, are higher by 17% during the second quarter after the exit. For adults, the number of ACSC-related admissions decreases after the exit. This heterogeneity corresponds with Lavarreda et al. (2008). Using survey data from California, they 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.

Section 1.6 explores whether differences between MMC plans may serve as mechanisms behind the effect of a switch on utilization and health. First, I examine the change in switchers' network of out-patient providers. Before new contracts are awarded, two out of every three visits to out-patient providers are made to known providers, i.e. providers that the patient met during the year before. After a switch, less than half of switchers' out-patient visits are made to familiar providers. Focusing on primary care physicians (PCPs), I find that 23% of switchers that visited PCPs in the year before the switch no longer have access to them in their new plan's network. Less than 3% of beneficiaries in remaining plans suffer such a loss. Losing a PCP after the switch is correlated with worse disruptions, including higher use of emergency departments, lower use of prescription drugs, and a larger increase in the number of preventable hospital admissions.

Second, I explore two mechanisms that may lower the use of prescription drugs - The share of switchers' familiar pharmacies decreases by up to 12% after the switch, suggesting a change in the network of pharmacies. In addition to that, the share of familiar drugs in switchers' prescriptions decreases by 7%. This may imply that new drug formularies and new providers are leading switchers

to change their medications.

Third, I find that immediately after the switch, switchers use fewer out-patient services that are likely subject to pre-authorization, suggesting that the pre-authorization process at the new plan may also serve as a mechanism for post-switch disruptions.

Lastly, differences in the extent to which plans affect their enrollees' utilization may serve as another mechanism. Geruso et al. (2020) exploit random assignment of enrollees to show that MMC plans differ in their causal effect on utilization. To explore this potential mechanism, I estimate *observational* risk-adjusted plan effects on utilization for all pre-exit plans. After the exit, 59% of switchers "downgrade" to a less generous plan (i.e. with a lower effect on utilization), and only 14% are "up-graders". For both up-graders and down-graders, a switch is associated with an increase in the use of emergency departments. However, up-graders' utilization of prescription drugs barely changes, in contrast to the significant decrease for down-graders. This suggests that the decrease in utilization of prescription drugs is strongly related to switching to less generous plans.

Several policies may reduce switching disruptions. First, a classic recipe to address switching costs is to increase compatibility (Farrell and Klemperer (2007)). Plans can be made more similar, for example, by setting a uniform drug formulary. Second, policy makers may decrease the frequency of plan exits, e.g. by limiting free entry and increasing contract lengths. Third, policy makers may allow beneficiaries to visit prior providers and fill prior prescriptions in a longer transition period after a switch. Lastly, policy makers can improve the matching of beneficiaries to plans by adopting auto-enrollment algorithms that take into account beneficiaries' relationships with current providers. I discuss such policies in Section 1.8.

1.2 Data

1.2.1 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 - 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 Medicaid Analytic eXtract (MAX) - an administrative dataset managed by the Centers for Medicare and Medicaid Services (CMS). I use MAX data for the years 2007 to 2015 on all Medicaid enrollees, and for the years 2004 to 2006 on disabled beneficiaries only. Enrollment information on Medicaid beneficiaries is taken from the MAX Personal Summary files (PS), that contains 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. This file provides monthly information on the enrollment in managed care plans in each state and county - allowing to identify the month in which a plan exits.[2] 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 covers verified bid-induced exits in five states: Arizona, Minnesota, Missouri, Texas, and Washington. See Table 1.1 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

[2] 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.

between counties and states are excluded from the sample. The analytic sample focuses on beneficiaries that were enrolled in a HMO at the month before the exit in the state, and were enrolled in the same HMO during the 18 months before the exit. The treatment group includes beneficiaries in HMOs that exit the market. Beneficiaries in other HMOs are included in the control group. Table 1.2 presents the sample selection criteria and their effect on the sample size. The sample restrictions significantly decrease the sample size, as many Medicaid beneficiaries that are enrolled in an HMO a month before the exit weren't enrolled continuously in Medicaid or in the same HMO during the year and a half before the exit. However, the analytic sample eliminates concerns of differential enrollment in exiting vs. non-exiting HMOs at the months before the exit (when future exit is known). It also 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 many switchers in other insurance markets.

Table 1.1: Medicaid managed care bids included in the sample

	Milestones Dates				
State	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

Note: For bids included in the sample, the table presents information on the 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. 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 HMOs at the month before the exit, and appear in the MAX PS files.

Figure 1.1 presents the share of switching beneficiaries among the treatment and control groups.

Table 1.2: Sample selection

Sample restrictions	Number of Beneficiaries Treatment	Control
1) In HMO 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 HMO 18 months pre-exit	164,843	1,001,587

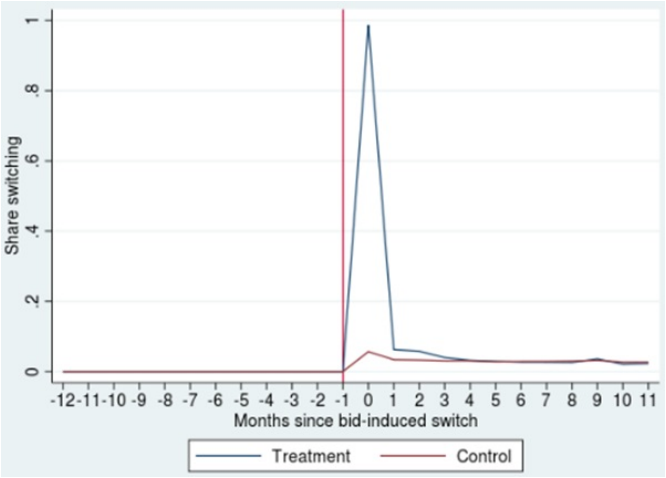
Note: Table presents the effects of the sample selection criteria on the sample size. The treatment group includes beneficiaries enrolled in exiting plans at the month before the exit. The control group includes enrollees of non-exiting plans during this month.

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 HMO, 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 1.2 presents the share of beneficiaries in the treatment and control groups that leave Medicaid every month. While beneficiaries in exiting HMOs 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.

1.2.2 Data on utilization of services

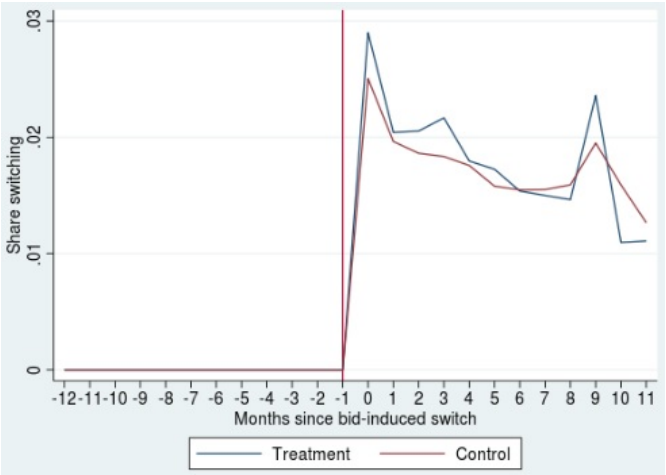
Data on beneficiaries' utilization is included in the MAX Inpatient (IP), Other Services (OT), and Prescription Drug (RX) files. These files track claims for services provided by the fee-for-service system and also include encounter data on services provided by private managed-care plans. Drugs that

Figure 1.1: The monthly share of enrollees switching out of their plan in the treatment and control groups



Note: Figure shows the share of enrollees switching out of their HMO each month, around a bid-induced exit, in the treatment and control groups. Switchers either switch to another HMO, or to the fee-for-service system, or leave Medicaid.

Figure 1.2: The monthly share of enrollees leaving Medicaid in the treatment and control groups



Note: Figure shows the share of enrollees leaving Medicaid each month, around a bid-induced exit, in the treatment and control groups.

appear in RX claims are classified to therapeutic classes using the RxNorm database.[3] To study the

[3] 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)).

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).[4]

Table 1.3 presents summary statistics for the treatment and control groups, one year before the pre-exit month (in which assignment to treatment is determined). Exiting plans have fewer non-white beneficiaries and fewer disabled beneficiaries than other 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 overall similar in both groups.

1.2.3 Measuring the disruption 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 (CoC). To measure such changes I 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). This measure builds on the Known Provider measure for continuity of care, which equals 1 if the current provider was seen at least one time in the previous year (Smedby et al. (1986)).[5] This measure focuses on network changes at the beneficiary-level (and not at the plan level) to examine the de facto networks, as experienced by enrollees. A provider could be listed in a plan's network, but offer only very limited availability for

[4] 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.

[5] 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)).

Table 1.3: Descriptive statistics for enrollees in exiting and non-exiting plans, 1 year before the pre-exit month

	Control	Treatment
Number of beneficiaries	1,001,587	164,843
Number of HMOs	70	27
Share of females (%)	52.8	54.0
Share of whites (%)	27.5	48.1
Share disabled (%)	6.9	2.9
Age structure (share, %):		
Under 5	32.0	28.2
5 to 15	39.6	38.3
15 to 20	11.9	12.8
20 to 45	10.8	14.7
45 to 65	5.7	6.1
Monthly Utilization (#):		
PCP Visits (per 1,000)	224.7	195.4
ED Visits (per 1,000)	57.2	59.3
Hospitalizations (per 1,000)	4.9	4.3
Inpatient days (per 1,000)	20.1	12.9
ACSC-Related Hospitalizations (per 1,000)	1.8	1.2
Days' supply in prescriptions filled	16.8	17.7
Share filling any prescription (%)	25.4	24.7
Share using any out patient service (%)	19.5	18.6
Share of women giving birth (ages 15 to 44) (%)	7.7	7.7

Note: Table presents summary statistics for the Medicaid beneficiaries included in the sample: non-elderly, non-dual beneficiaries that remain in their HMO 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 HMOs at the month before the exit. The control group includes beneficiaries in other, non-exiting, HMOs. The presented statistics are for a single month - one year before the pre-exit month, i.e. 13 month before the exit. The number of HMOs is the number of different HMO IDs in the MAX database.

Medicaid beneficiaries. In addition to that, I study beneficiaries' access to their current primary care physicians (PCPs) after a switch, by examining whether PCPs that the beneficiary visited before the exit are included in the post-exit plan's network.

1.3 Empirical Framework

Examining the impacts of enrollees' switches between health plans is challenging when enrollees choose whether to switch. Such a decision may depend on their preferences over the characteristics of plans in the market, including the costs and the adequacy of the network of providers to their needs. As changing needs may simultaneously trigger a switch to another plan and affect utilization, selection into voluntary switching may bias estimates for the effects of switching on utilization and health outcomes. To avoid such a selection bias, this paper exploits *involuntary* plan switches after plans exit the market in Medicaid managed care. To estimate the effects of switching between health plans I use a difference-in-differences (DID) framework. I compare, before and after an exit, beneficiaries in exiting plans, that are forced to switch out of their HMO, to beneficiaries in non-exiting plans, that mostly remain in the same HMO. I explore the dynamics of the outcomes in monthly event studies around exits, and using stacked DID regressions with quarterly- or half-year-pooled data.

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). The dataset is used to estimate event studies around plan exits. The estimated equation is:

$$\begin{aligned} Y_{ist} =& \alpha_0 + \sum_{l=-12}^{11} \theta_l 1\{t - Exit_s = l\} + \sum_{l=-12}^{11} \beta_l 1\{t - Exit_s = l\} * Treat_i \\ &+ \gamma_i + \delta_t + month_t + \varepsilon_{it} \end{aligned} \tag{1.1}$$

where Y_{ist} is the outcome for individual i, residing in state s, at time t. $1\{t - Exit_s = l\}$ is an indicator for being l months relative to the exit in state s. $Treat_i$ equals 1 if the enrollee is in an exiting plan, i.e. is forced to switch out of her HMO, and equals 0 otherwise. γ_i is an enrollee fixed-effect, δ_t is a time fixed effect, and the equation also includes a month of year fixed effect to account for possible sea-

sonality in some services. The equation is estimated on a sample from 13 months before the exit up to 12 months after it, setting the (omitted) base period to be 13 months before the exit - a time well before contracts are awarded in the states' bids. After contracts are awarded, plans, providers, and beneficiaries may behave differently due to the imminent exit. The future exit, thus, may affect utilization and its reporting during this pre-switch period. In the main analysis, I drop all periods after contracts are awarded and before the actual exit (I drop all observations between 7 months before the exit and a month before the exit to keep the number of states constant in all time periods). I examine this award-to-exit period separately in Appendix A.0.1.

In addition to event studies, I run Difference-in-Differences regressions that pool the months after the exit into quarters (or halves in some specifications):

$$\begin{aligned} Y_{ist} =& \alpha_0 + \sum_{l=0}^{3} \theta_l 1\{Q_t - QExit_s = l\} + \sum_{l=0}^{3} \beta_l 1\{Q_t - QExit_s = l\} * Treat_i \\ &+ \gamma_i + \delta_t + month_t + \varepsilon_{it} \end{aligned} \tag{1.2}$$

where $QExit_s$ is the quarter of exit in the state, and the rest is similar to the event-study regression above. The DID regression is run on the same sample as the event study, thus it estimates the effect of being in the treatment group of involuntary switchers, comparing to the control group of beneficiaries in remaining plans, relative to the whole period starting at 13 months before the exit and ending before contracts are awarded in any state, 8 months before the exit.

In addition to the reduced-form DID, I also examine a specification in which enrollment in an exiting plan serves as an instrumental variable to beneficiaries' actual 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 at this time, the differences between the reduced-form and the IV estimation are small. I present the estimated equations and the results of this additional estimation in Appendix A.0.2.

1.4 Results

1.4.1 Utilization of health care services

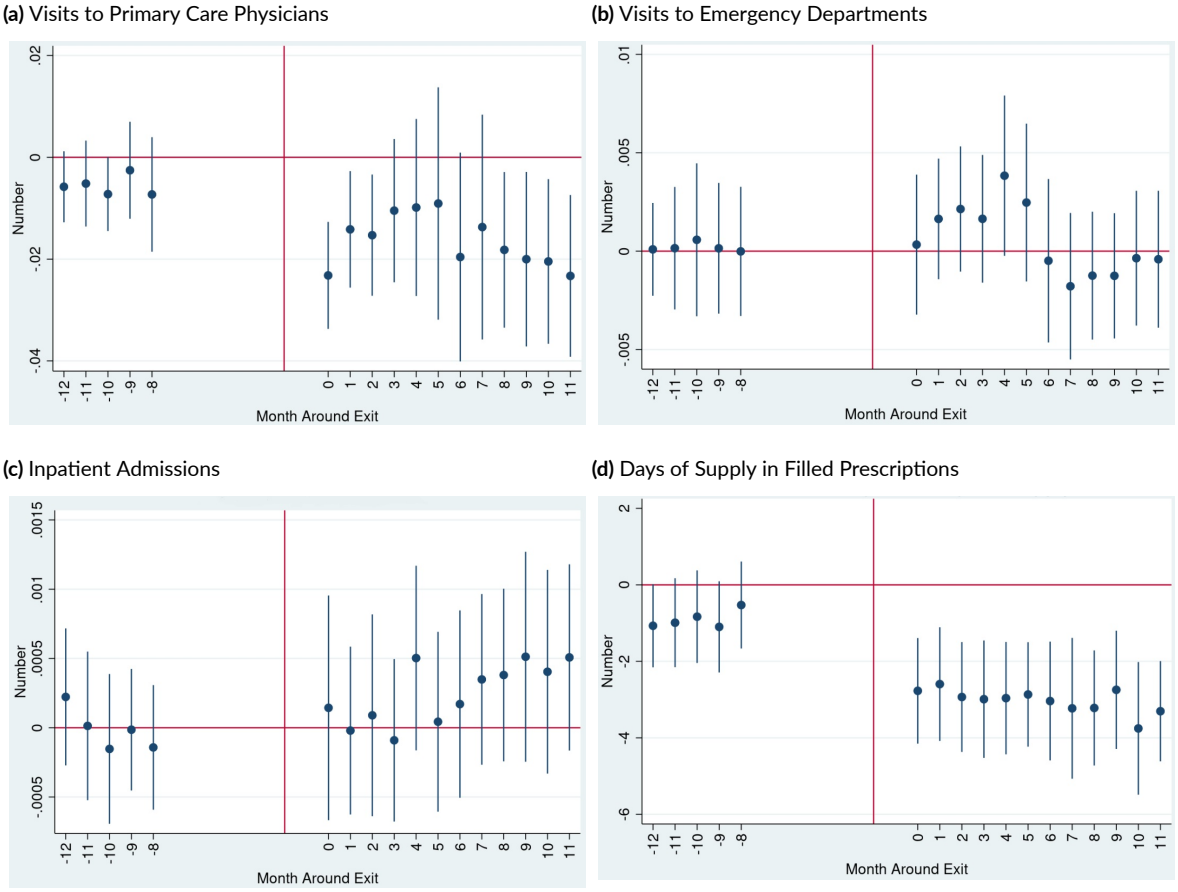
I examine the dynamic effects of a switch on the utilization of health care services. Event studies around a plan exit are presented in Figure 1.3. I find that beneficiaries in exiting and non-exiting plans mostly share similar trends in utilization at the beginning of the pre-exit year, before contracts are awarded in their state's MMC bid. After the exit, there are evidence for disruptions in the care provided to switchers from exiting plans.

Table 1.4 presents estimates of the quarterly pooled DID regressions. The number of monthly visits to primary care physicians (PCP) is lower for switchers by 6% during the first quarter after switching (column 1) - 12.5 fewer visits per 1,000 switchers.[6] The number of PCP visits increases back in the quarter after that, but is lower again throughout the rest of the post-switch year by up to 8% relative to the baseline. Switchers increase their use of hospitals' emergency departments (ED) in the second quarter after the switch (column 2).[7] The number of ED visits is higher during this quarter by 5% relative to the baseline - 2.8 additional ED visit per 1,000 switchers. Switchers' are admitted more often to hospitals, and have longer stays, especially at the second half of the post-exit year. At the fourth quarter after a switch, the number switchers' inpatient admissions (column 3) is higher by 11% relative to the baseline (0.5 additional admission per 1,000). During the last two quarters of the post-exit year, the number of switchers' inpatient days (column 4) rises by 14% to 21%. The number of admissions related to Ambulatory Care Sensitive Conditions (ACSC) does not change in a statistically significant way throughout the post-switching year.

[6] 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

[7] I follow the method described in Hennessy et al. (2010) to identify visits to ED using revenue codes and CPT codes.

Figure 1.3: Utilization of services and prescription drugs around plan switches



Note: Figure shows event studies around the time plans exit the market when new MMC contracts go into effect (marked by a red vertical line). Data points are the coefficients β_l from Equation 1.1 for each month around the switch. They show the effect on beneficiaries in exiting plans ("switchers"), relative to beneficiaries in remaining plans, with the month 13 months before the exit as the base period. All the periods between 8 months before the exit and a month before the exit are dropped from the sample used for estimation, as states already awarded contracts in their bids during this period.

1.4.2 Prescription drugs

Switchers' consumption of prescription drugs drops after the exit-induced switch. The event studies presented in Figure 1.3 show the share of beneficiaries filling any prescription and the number of days of supply in filled prescriptions. For both measures, the treatment and control groups share

Table 1.4: The effects of an exit-induced switch on monthly utilization of services

Periods interacted w. Exit-Switcher Indicator	(1) PCP Visits per 1,000	(2) ED Visits per 1,000	(3) Inpatient Admissions per 1,000	(4) Inpatient Days per 1,000	(5) ACSC-Related Admissions per 1,000
Post-switch Q1	-12.52***	1.08	0.06	0.40	0.06
	(4.10)	(1.02)	(0.25)	(1.26)	(0.14)
Post-switch Q2	-0.28	2.82**	0.13	-0.05	0.08
	(6.91)	(1.24)	(0.20)	(1.11)	(0.10)
Post-switch Q3	-13.82*	-1.77	0.25	2.76**	-0.09
	(7.23)	(1.33)	(0.19)	(1.27)	(0.09)
Post-switch Q4	-15.37***	-0.88	0.46**	1.77*	0.06
	(5.57)	(1.11)	(0.23)	(0.93)	(0.11)
Baseline Mean	195.4	59.3	4.3	12.9	1.2
# of observations			20,507,366		
# of beneficiaries			1,166,430		
# of counties			354		

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

Note: Table presents the DID estimates (reduced-form) of the impact of a plan exit on utilization of services among its beneficiaries, that are forced to switch out of their plan. All specifications include also the non-interacted period variables, a constant, individual fixed effects, month fixed effects, and month-of-year fixed effects. Standard errors are clustered at the county level. The coefficients are from estimating Equation (1.2).

similar trends at the beginning of the year before the exits. Table 1.5 presents the difference-in-differences estimates for these two measures.

Relative to the period before contracts are awarded, and comparing to beneficiaries in non-exiting plans, the share of switchers filling any prescription (column 1) is lower by about 3.5 percentage points throughout the first year after the switch - a 14% decrease relative to the baseline mean. The number of days of supply in filled prescriptions (column 2) is lower by 12% (2 days) at the first quarter after the switch, and keeps decreasing in the quarters after that. The number of days' sup-

Table 1.5: The effects of an exit-induced switch on monthly utilization of prescription drugs

	(1)	(2)	(3)	(4)
			Among pre-exit users	
Periods interacted w. Exit-Switcher Indicator	Any Filled Prescription (%)	Days Supply All Drugs	Days Supply Diabetes	Days Supply Mental Health
Post-switch H1	-3.42***	-2.17***	-2.62**	-1.81***
	(1.15)	(0.56)	(1.12)	(0.47)
Post-switch H2	-3.93***	-2.63***	-2.94***	-1.55***
	(1.03)	(0.53)	(1.02)	(0.53)
Baseline Mean	24.7	17.6	29.2	24.3
# of observations	20,507,366		234,151	587,128
# of beneficiaries	1,166,430		13,640	33,996
# of counties	354		219	256

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

Note: Table presents the DID estimates (reduced-form) of the impact of a plan exit on utilization of prescription drugs among its beneficiaries, that are forced to switch out of the plan. All specifications include also the non-interacted period variables, a constant, individual fixed effects, month fixed effects, and month-of-year fixed effects. Standard errors are clustered at the county level. The coefficients are from estimating Equation (1.2).

ply is lower by 14% by the fourth quarter. Columns 3 and 4 in the table present the estimates for the consumption of prescription drugs to treat some chronic diseases, for patients that were using these drugs at the year before the exit. Among such patients that are forced to switch out of their HMOs, utilization of these chronic medications decreases. The number of days' supply decreases by up to 10% (3 days) for anti-diabetic drugs, and by up to 8% (2 days) for anti-depressants and anti-psychotics.

1.5 Heterogeneity

1.5.1 Heterogeneity by age

Almost 80% of switchers in my 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. Relative to the baseline levels, the estimates in Table 1.6 demonstrate that the effect on utilization of hospital services is different for the two groups. While children switchers have 3% to 7% more visits to emergency departments (ED) during the first two quarters after the switch, the number of adults' visits to ED is lower, especially during the third and fourth quarter after a switch (by up to 9% relative to the baseline). The number of total hospital admissions is higher by up to 15% for both groups, but the increase in inpatient admissions happens earlier in the year for children. Most of the early increase in children's hospital admissions is due to admissions related to ambulatory-care-sensitive conditions (ACSC), that temporarily rise by 19% in the second quarter after switching. In contrast to that, adults' ACSC-related admissions decrease, especially during the third quarter after the switch, when they are lower by 30% relative to the baseline mean. The effects of switching on the number of PCP visits and on the utilization of prescription drugs is similar for children and adults (Table A.4 in the Appendix).

The results suggests that children are more sensitive to disruptions in their care after switching, and have more adverse health outcomes than adults after changing their insurance plan.

1.5.2 Heterogeneity by pre-exit utilization

Sick beneficiaries with intense utilization of health care services may be affected differently than healthier beneficiaries after switching to another plan. Such sicker beneficiaries may be more sensitive to disruptions to care, but may also try harder and receive more assistance to navigate their care

Table 1.6: The effects of switching on the use of hospital services - children and adults

Periods X Exit-Switcher Indicator	ED Visits per 1,000		IP Admissions per 1,000		ACSC-Related Admissions per 1,000	
	Children	Adults	Children	Adults	Children	Adults
Post Q1	1.65*	-1.78	0.02	0.40	0.06	-0.14
	(0.87)	(2.45)	(0.13)	(0.88)	(0.11)	(0.44)
Post Q2	3.38**	-0.41	0.21*	0.08	0.15**	-0.37
	(1.41)	(2.52)	(0.12)	(0.78)	(0.08)	(0.35)
Post Q3	-0.56	-8.14**	0.28**	0.42	0.05	-0.86**
	(1.21)	(3.36)	(0.12)	(0.69)	(0.07)	(0.35)
Post Q4	-0.01	-6.73***	0.18	1.97**	0.06	-0.11
	(1.16)	(2.46)	(0.12)	(0.89)	(0.08)	(0.39)
Baseline Mean	50.8	90.9	1.9	13.3	0.8	2.8

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

Note: Table presents estimates of the effect of an exit-induced switch on utilization of hospital services for children (under age 20), and adults. Ages are measured at the month before the exit. Specifications include also a the non-interacted period variables, constant, and fixed effects for individuals, months, and month-of-year. Standard errors are clustered at the county level.

in the new plan. To examine this issue I identify pre-exit "heavy-users" - 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 (24% of the sample) and for the rest of the beneficiaries. The results (presented in Table A.5 in the Appendix) show that both groups suffer disruptions to their care after switching. However, relative to the baseline mean, the effect on heavy-users seems smaller - their number of PCP visits decreases by at most 2% (vs. 13% for non heavy-users); their utilization of prescription drugs decreases by 10% (about half of the decrease for non heavy-users); they have 5% more visits to emergency departments in the second quarter after the switch (7% for non heavy-users); and they are admitted to hospitals 15% more by the end of the switching year (vs. 20% for non heavy-users).

1.6 Mechanisms

1.6.1 Changes in the network of providers after a switch

Disruptions to the continuity of care and the breaking of patients' relationships with their familiar providers may lead to a change in enrollees' utilization patterns after switching a Medicaid managed care plan. To examine this potential mechanism, I first measure the change in the provider network, as experienced by individual patients. For each enrollee using a service during the month, I find the share of known providers - the share of providers that the patient has already seen during the previous year (relative to the exit[8]). Table 1.7 presents the reduced form DID estimates for the effects of plan exits on their beneficiaries' network of visited providers.

In the baseline period - one year before plans exit - about two out of every three visits to outpatient providers (column 1) are made to providers that were already seen at the previous year. After the exit, comparing to beneficiaries in non-exiting plans, the share of known providers among switchers is lower by around 20 percentage points throughout the year after the switch (26% to 32% lower than the baseline mean). This means that only about half of all switchers' visits to outpatient providers are now made to providers that were seen at the year before. This result is consistent with Chernew et al. (2004), that find that switchers to a new HMO in the employer-sponsored market have a 50% likelihood of keeping their physicians.

The share of known providers of prescription drugs (i.e. mostly pharmacies) decreases by 10 to 11 percentage points at the year after the switch (12-13% lower than the baseline mean). Among switchers that are admitted to a hospital after the involuntary switch, the share of known hospitals (i.e. hospitals to which they were admitted in the previous year) does not change much initially, but increases by 9 percentage points in the second half of the post-switch year (a 53% increase relative to

[8] This means that the set of known providers is updated with the providers seen over the last year at the following points in time: 12 months before the switch and at the time of the switch.

Table 1.7: The effects of an exit-induced switch on utilizers' share of known providers and share of known prescription drugs

Periods interacted w. Exit-Switcher Indicator	(1) Outpatient Providers	(2) RX Providers (Pharmacies)	(3) Hospitals (Inpatient)	(4) Drugs (by NDC)
Post-switch H1	-21.38***	-10.99***	4.04	-3.66***
	(7.64)	(3.21)	(3.70)	(0.79)
Post-switch H2	-17.56**	-9.64***	9.03**	-3.57***
	(7.34)	(3.26)	(3.76)	(0.59)
Baseline Mean	67.2	83.7	17.0	53.5
# of beneficiaries	763,859	690,967	11,584	690,967
# of counties	347	347	207	347

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

Note: Table presents DID estimates (reduced-form) for the effect of an exit-induced switch on the share of known providers and known 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). The outpatient providers specification (column 1) excludes primary care physicians. All specifications include also the non-interacted period variables, a constant, and fixed effects for individuals, months, and month-of-year. Standard errors are clustered at the county level.

the baseline mean). This suggests that while the networks of out-patient providers and pharmacies change significantly for switchers in the sample due to a plan exit, the access to familiar hospitals remains mostly unchanged for beneficiaries switching their plan.

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[9].

[9]I define the post-exit plan as the plan in which the beneficiary is enrolled 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

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 "PCP losers" and "PCP keepers" after they switch to a new plan. Table 1.8 presents the estimation for these two groups. Since beneficiaries may choose the plan into which they switch after their previous plan exits, and thus may choose whether they lose or keep their PCPs, these estimates may no longer be considered causal.[10]

Table 1.8: The correlation between switching and utilization of services - switchers that lost or kept access to their Primary Care Physicians

Periods X Exit-Switcher Indicator	PCP Visits per 1,000		ED Visits per 1,000		IP Admissions per 1,000		Any Filled Prescription (%)	
	Lost	Kept	Lost	Kept	Lost	Kept	Lost	Kept
Post H1	16.99*	-10.57	10.51***	-0.28	0.58	0.11	-11.33***	-3.87***
	(9.55)	(6.99)	(1.95)	(1.81)	(0.44)	(0.36)	(4.00)	(0.71)
Post H2	-35.96*	-20.29***	3.95*	-5.59***	0.90*	0.11	-10.65***	-4.81***
	(20.71)	(5.85)	(2.13)	(2.04)	(0.53)	(0.32)	(3.50)	(0.71)
Baseline Mean	350.4	406.0	63.6	92.5	2.9	7.6	30.8	40.4

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

Note: Table presents estimates for the correlation between exit-induced plan switching and utilization of services, among beneficiaries that visited a PCP at least once during the pre-exit year, before contracts were awarded. The control group includes 1 million beneficiaries in non-exiting plans in 353 counties. Two treatment groups are examined: First, 19,133 switchers that lost access to their Primary Care Providers (PCPs) after the exit (i.e. All the PCPs they visited at the pre-exit year are missing from their new plan's network), and 62,799 switchers who kept access to (at least one of) their PCPs. All specifications include also the non-interacted period variables, a constant, and fixed effects for individuals, months, and month-of-year. Standard errors are clustered at the county level.

Losing access to one's PCP is surprisingly associated with an increase in the number of PCP visits

reconnect with their PCP then.

[10] Notably, PCP keepers have higher utilization of most services in the baseline period - a year before the exit. This suggests that a higher utilization may be associated with a more active choice of plan after one's current plan exits the market.

at the first half of the post-switch year. This is partly due to an increase in the number of *new* PCP visits among PCP losers (not shown). However, during the rest of the year, PCP losers visit their (newly assigned) PCPs much less, and their number of visits is lower by 10% relative to their baseline mean (vs. 5% decrease among PCP keepers). Losing a PCP is correlated with worse disruptions to care: higher use of emergency departments throughout the post-switch year (by up to 17% initially), while PCP keepers' use of ED mostly decreases; 31% more hospital admissions, partly due to preventable causes (not shown) - in contrast to no significant change in admissions among PCP keepers; and lower utilization of prescription drugs (37% decrease in the share filling any prescription immediately after the switch vs. 10% decrease for PCP keepers). Sabety (2021) finds a larger (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.

1.6.2 Changes in prescribed drugs after a switch

After switching to a new health plan, beneficiaries may receive prescriptions from new providers and may face a new drug formulary. 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 (4) of Table 1.7. Comparing to beneficiaries in non-exiting plans, the share of known drugs used by exit-induced switchers is lower in the post-switch year by about 7% 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 patient to change their medication.

1.6.3 Prior authorization requirements for out patient services

Alongside formularies and selective contracting in networks, requiring a prior authorization (PA) before a service is provided is another tool in the managed-care toolbox, allowing insurers to assess the medical necessity of planned medical services. Schwartz et al. (2021) study services that are potentially subject to prior authorization in Medicare. To identify such services, they use a list of services that require PA from a large Medicare Advantage insurer - Aetna. Aetna uses the same list also in non-Medicare plans.

I use the Aetna list from Schwartz et al. (2021) to count the number of PA services provided to MMC beneficiaries in the sample.[11] Table 1.9 presents the DID estimates for the effect of an exit-induced switch on the number of prior-authorization services provided to switchers, compared to beneficiaries in remaining plans. In the first quarter after the switch, the number of PA services decreases by 22% relative to the baseline mean (0.8 fewer service per 1,000 population). It is again lower, by 11%, at the last quarter of the post-switch year. The results suggest that delays and frictions in receiving prior authorizations after a switch may play a role in the impact of switching on utilization and health outcomes. Though PA services constitute only about 0.5% of all outpatient services in the baseline period, they may be important for patients that require them and delays may lead to adverse health outcomes.

[11] I count all PA services provided in an outpatient setting, excluding services provided at the same day as a visit to an emergency department. Such services may meet criteria for emergency exemption of prior authorization. PA services are identified by their HCPCS code.

Table 1.9: The effects of switching on prior-authorization services in out patient setting

Periods interacted w. Exit-Switcher Indicator	PA Services per 1,000
Post-switch Q1	-0.80***
	(0.30)
Post-switch Q2	0.45
	(0.39)
Post-switch Q3	-0.25
	(0.40)
Post-switch Q4	-0.41*
	(0.22)
Baseline Mean	3.71
# of observations	20,507,366
# of beneficiaries	1,166,430
# of counties	354

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

Note: Table presents the DID estimates for the impact of an exit-induced switch on the number of out-patient services that may require prior authorization (PA). PA services are determined by their inclusion in a list of a large insurer (Aetna), and include tests, visits, and drugs administered in outpatient settings. The specification includes also the non-interacted period variables, a constant, and fixed effects for individuals, months, and month-of-year. Standard errors are clustered at the county level.

1.6.4 Plans' effect on utilization - switching to less generous plans

The plans that participate in the Medicaid Managed Care (MMC) markets in each state often differ in their average utilization 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 to another plan could be the result of the plan's effect on utilization. Such effects could be more per-

manent in nature, rather then temporary disruptions to utilization patterns.

To examine this issue, I first estimate state-level plan effects for all the pre-exit MMC plans. The effects estimate the correlation between each plan and the probability that its enrollees have any utilization over the first five months of the pre-exit year, controlling for enrollees' gender, race, and age group, and for month-county fixed effects. My estimates are risk-adjusted *observational* measures of plans' effect on utilization, and are not causal effects. However, for the NYC market, Geruso et al. (2020) show that risk-adjusted observational measures are correlated with causal differences between plans' utilization.[12]

When the new contract period begins, 59% of the Medicaid beneficiaries that were forced to switch out of their exiting plan switch to a less generous plan (i.e. a plan with a lower pre-exit plan effect on utilization). 13.5% of switchers switch to a more generous plan. 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 (4.5%). To examine how plans' generosity is related to post-switching disruptions, I repeat my estimation for two sub-samples of switchers from the exiting plans: "Up-graders", that switch to a more generous plan relative to their pre-exit plan, and "down-graders", that switch to a less generous plan. Main results for these two subsamples are presented in Table 1.10. 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 1.10 should not be interpreted as causal.

Switching to a less generous plan is correlated with a decrease in the number of PCP visits, especially in the second half of the post-switch year. In contrast, switching to a more generous plan is not correlated with such a decrease, and PCP visits seem to increase right after the switch. Another stark difference emerges for the utilization of prescription drugs - while the number of days' supply

[12] 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.

Table 1.10: The effects of switching on the monthly utilization of services - switchers to a plan with a higher vs. lower (observational) effect on utilization

Periods X Exit-Switcher Indicator	PCP Visits per 1,000		ED Visits per 1,000		Inpatient Days per 1,000		Days Supply All Drugs	
	Up	Down	Up	Down	Up	Down	Up	Down
Post-switch H1	13.17	-3.33	1.79	2.96**	-0.25	1.17	0.01	-2.25***
	(8.57)	(5.29)	(1.56)	(1.19)	(1.42)	(1.60)	(0.48)	(0.82)
Post-switch H2	-4.46	-20.65**	-3.90**	-1.39	3.97*	0.04	-0.25	-2.83***
	(7.01)	(9.28)	(1.86)	(1.36)	(2.29)	(1.07)	(0.38)	(0.82)
Baseline Mean	205.0	202.2	61.4	58.6	8.1	15.8	17.9	17.9

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

Note: Table presents estimates for the correlation between exit-induced plan switching and utilization of services. The control group includes 1 million beneficiaries in non-exiting plans in 353 counties. Two treatment groups are examined: First, 22,772 switchers that switch to a plan with a higher (observational) effect on any utilization ("Up" columns); second, 95,863 switchers that switch to a plan with a lower effect ("Down" columns). These plan effects are measured for all pre-exit plans, using utilization in the first five months of the pre-exit year (i.e. before contracts are awarded in the bid). All specifications include also the non-interacted period variables, a constant, and fixed effects for individuals, months, and month-of-year. Standard errors are clustered at the county level.

in filled prescriptions is lower by up to 16% for down-graders, throughout the year after they switch, up-graders experience no significant change in the utilization of prescription drugs. These results may suggest that the negative effects of switching on the number of PCP visits and the utilization of drugs is mostly due to switching to less generous plans.

Both up-graders and down-graders use emergency departments (ED) more often in the first half-year after switching, and less often in the second half-year. However, the initial increase in ED use is higher for down-graders and the later decrease is larger for up-graders. As plan effects would imply an opposite result, this correlation may suggest that generous plans succeed in lowering ED use, while increasing other services, such as PCP visits and adherence to drugs. In any case, both up-graders and down-graders initially increase their use of emergency departments, suggesting that both

groups suffer disruption to their care after a switch. Up-graders are also admitted for longer stays in hospitals during the second half-year after a switch, while switching to a less generous plan is not correlated with a significant change in the number of inpatient days.

Examining network changes for the two subsamples (Table A.6 in the Appendix), I find that the experienced change in the network of out-patient providers and pharmacies is similar for both up-graders and down-graders.[13] The share of known drugs decreases initially by about 6% for both groups. While down-graders keep using more unfamiliar drugs during the whole year after the switch, up-graders return to their pre-exit level of familiar drugs by the end of the first year. This may suggest a more lenient drug formulary in more generous plans, and may partly explain why drug utilization barely changes for up-graders.

1.7 Robustness - Differential Reporting of Encounter Data

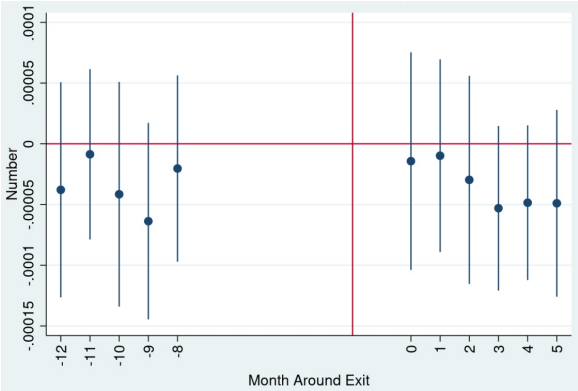
The data that I use to measure utilization around plan switches comes from the Medicaid Analytic eXtract (MAX) files and is based mainly on encounter data from Medicaid managed care plans. This data suffers from reliability issues in some states, and can be partial (Leonard et al. (2017), Li et al. (2018)). 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 1.4 presents placebo-tests event studies for two such services - deliveries

[13] Specifically for PCPs, the shares of switchers who visit PCPs before the exit and lose access to them after the switch are 21% for down-graders 23% for up-graders.

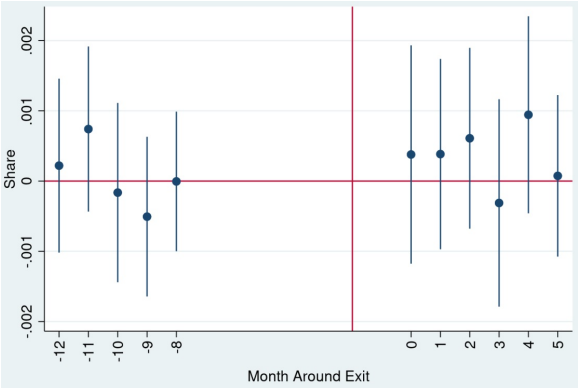
and hospital admissions for acute appendicitis. In both cases, no level shift in the number of services can be detected after the exit. This supports the assumption of no differential level of reporting between exiting and remaining plans.

Figure 1.4: Services presumably independent of plans' influence around plan exits

(a) Acute Appendicitis Admissions



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



Note: Figure shows event studies around plans' exit for the share of women at the ages of 15 to 44 having a hospital delivery, and for the number of hospital admissions due to acute appendicitis. The lack of a level shift immediately after beneficiaries switch out of exiting plans supports the assumption of no differential level of reporting between exiting and remaining plans.

1.8 Discussion

1.8.1 Switching costs and inertia

The disruptions to the utilization of services and prescription drugs after plan switching suggest that switching costs can be significant when changing health insurance. The expected disruptions (or the efforts required to avoid them when switching is voluntary) may provide some rational source for inertia in the current health plan. Handel (2013) finds substantial inertia in employer-sponsored health insurance. However, in his settings, all plans have the same network of providers, while relative premiums change over time. In Medicaid managed care, monetary differences between plans

are mostly irrelevant as all plans have zero premiums and at most nominal co-payments, but the network of providers and formularies may differ between plans. Marton et al. (2014) present evidence for inertia in such a setting - Medicaid managed care plans in Kentucky, where most auto-assigned beneficiaries don't switch even out of the lowest quality plans. As switching costs increase inertia, they may also reduce adverse selection by sicker beneficiaries. For some of these beneficiaries switching costs may be especially significant, as their utilization of services is higher, and their relationships with existing providers may be stronger.

1.8.2 Policies to reduce aggregate switching costs

Policy makers have several ways to lower the total costs due to switching: First, they may reduce the frequency of plan exits from MMC. This can be done, for example, by limiting free entry of unviable plans - contracting only with insurers that can serve beneficiaries throughout a defined contract period, and by lengthening the effective contract period in MMC bids. Second, policy makers may 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, and new beneficiaries complete an initial health risk assessment within 90 days of enrollment. Treatment

plans should be developed for enrollees with special health care needs.[14] 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. (2018a)), the state-defined auto-assignment rules may have a large impact on switching disruptions and total switching costs. Lastly, 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.

1.9 Conclusion

I find substantial disruptions to the utilization of health services and prescription drugs after MMC beneficiaries are forced to switch between plans. More and longer admissions to hospitals suggest that switches also lead to adverse health outcomes, especially for children. I present evidence for a significant change in beneficiaries' networks of out-patient providers and pharmacies after a switch, as well as evidence for changes in their drug formularies. While some of the effects I find may stem from switching to less generous plans, I show that even switchers to more generous plans suffer disruptions to their care.

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

[14] "Enrollment process for Medicaid managed care" web page on the MACPAC website, visited 2/8/21. https://www.macpac.gov/subtopic/enrollment-process-for-medicaid-managed-care/

to decrease disruptions after switching. 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. Lastly, my empirical strategy exploits involuntary switches due to plan exits. Such involuntary switches between insurers are very common in the U.S. but many enrollees still switch voluntarily between plans. Understanding the selection into voluntary switches and examining the effect of such switches on enrollees' utilization and health outcomes also requires further research.

2

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

°This chapter is joint work with Prof. Timothy Layton, Harvard University. We thank participants in the Harvard PhD in Health Policy Research Seminar for their helpful comments and suggestions.

2.1 Introduction

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

Using national data on Medicaid enrollees between 2004 and 2015, we exploit county-level enrollment mandates that swiftly shift large shares of disabled beneficiaries into private managed care plans. Within a difference-in-differences framework (DID), we compare counties that roll out a mandate to counties that remain in the public FFS system. In using enrollment mandates we follow previous papers that examine transitions to managed care in Medicaid. Duggan and Hayford (2013) conduct their analysis at the state-level, using the share of population living in a county with a mandate as an instrument for the state's penetration rate of Medicaid managed care plans. They examine mandates rolled out mostly in the 90's for the general Medicaid population and find that private plans don't reduce Medicaid's fiscal spending on average. Our empirical approach is similar to Lay-

ton et al. (2019), that examine transitions of disabled beneficiaries from FFS to Medicaid managed care plans in mid-2000 Texas. They exploit staggered introduction of county-level managed care enrollment mandates, and find that such transitions increase Medicaid fiscal spending by 11.7% relative to control counties that remain in the public system. However, while Layton et al. (2019) focus on a single state, we examine national data on all the transitions of disabled beneficiaries to managed care during our twelve-years sample period.

Our analysis uses monthly individual-level data on Medicaid enrollment and costs from an administrative database - the Medicaid Analytic eXtract (MAX). The data is described in section 2.2. Section 2.3 presents our empirical approach. We use the MAX data to calculate the penetration rate of comprehensive managed care plans among disabled beneficiaries in each county over time. We identify enrollment mandates, in the data, when the penetration rate increases sharply and swiftly - at least by 20 percentage points over at most 3 months. Our baseline analytic sample includes a treatment group of beneficiaries in 936 counties with an enrollment mandates - counties that had no significant MC penetration before the mandate. The control group includes beneficiaries in 723 counties that remain in FFS throughout our sample period. We control for individual and quarter fixed effects in all our specifications and use event studies to show that our treatment and control groups share the same spending trends during the three years before a mandate is rolled out.

Our results are presented in section 2.4. We find that the average county-level mandate eventually increases the share of disabled beneficiaries that are enrolled in managed care plans by 63 percentage points. At the first year after a mandate, the $387 increase in per-member-per-month (PMPM) capitated payments to the private plans is offset by a decrease in direct FFS payments to providers. In total, there is little change in Medicaid fiscal spending. After the first year, the total spending in treatment counties increases by $98 (PMPM) relative to control counties - a 8% increase over the baseline mean. This increase is driven solely by higher capitated payments to the managed care plans,

while spending on FFS payments to providers continues to decrease.[1] Using the mandates as an instrument for individuals' enrollment in managed care plans we find a similar spending dynamics - there is little change in total Medicaid spending at the first year after a transition from FFS to managed care, but spending increases after that by $194 - 16% of the baseline mean.

We test the robustness of our results to alternative analytic samples. First, we examine the reliability of the MAX data in each state by comparing the aggregate spending in the MAX database to the verified spending amounts that states report annually to the federal government in CMS-64 forms. Our results are robust to using only a subsample of state-years with reliable data. Second, we repeat the estimation on a sample that includes a balanced panel of treatment counties, and find similar results. In addition to that, we test a different specification, comparing contiguous treatment and control counties. Lastly, we use a Stacked DID approach. First, we create a stacked dataset in which only counties that are never treated serve as controls for each treated county, avoiding a possible bias when treatment effect changes over time and already-treated counties implicitly serve as controls (Goodman-Bacon (2021)). Second, we examine a specification in which only later-treated counties serve as controls (following Deshpande and Li (2019), Fadlon and Nielsen (2021) and others). Our main results remain unchanged in all these specifications and they strongly suggest that transition from the public FFS system to private managed care plans does not save money to the Medicaid program - spending eventually increases within a range of 0.5% to 30% of the baseline mean. In addition to these robustness tests, we run a placebo test in which we examine the effect of Medicaid enrollment mandates on *Medicare's* spending on its non-elderly disabled beneficiaries. Finding no effect supports our empirical approach and the assumption that no other concurrent shocks in our treatment counties led to the increase of medical spending for the disabled after enrollment mandates.

Since Medicaid programs are different in each state, we examine possible heterogeneity in our

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

results in section 2.5. We find that after the first year of an enrollment mandate, Medicaid spending weakly increases in all our treatment states except Louisiana. We show that states with lower pre-mandate FFS payment rates, tend to have a higher spending increase when beneficiaries are shifted to managed care.[2] This suggests that private plans find it harder to decrease payments to providers when the FFS rates are already very low to begin with. Duggan and Hayford (2013) find a similar result - states with higher FFS prices (closer to the rates of commercial insurers), decrease their Medicaid fiscal spending after a transition to managed care. While Duggan and Hayford (2013) rely on the price gap for a single service - newborn delivery - our price index uses claims data on *all* outpatient services to Medicaid's disabled beneficiaries.

In section 2.6 we discuss possible mechanisms behind our results. Beyond higher payment rates for managed care plans (especially in states with already low rates), costs could also be higher for MMC plans if disabled beneficiaries are under-served in the FFS system. Drug caps, that limit the number of prescriptions a beneficiary can fill each month, are an example for an explicit limit on utilization within the public FFS system - a limit relaxed under managed care plans (Layton et al. (2019)). Higher costs for private plans affect Medicaid fiscal spending due to the way their capitation payments is adjusted. CMS' rate development guidelines require states to have actuarially fair capitation rates, updated annually and based on the experience of the Medicaid population in the recent three years. Thus, past increases in plans' costs would lead to higher payments from the Medicaid program. This may reduce the saving incentive for the plans.

Our paper contributes to the literature that assesses the impacts of managed care, especially in Medicaid, and adds to the few papers that focus on the disabled population within this program. Our results are in line with the economic literature in finding no fiscal savings from Medicaid's transition to private managed care plans, and is in contrast with policy makers' believes that such

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

savings will occur. Private provision of publicly-financed insurance is becoming more and more popular in both Medicaid and Medicare (Gruber (2017)), and is common in many developed countries (McGuire and van Kleef (2018)). Our paper highlights that the impact of private provision on spending depends on both the public system it replaces and the procurement rules of the private plans.

2.2 Data

Our main data source is the Medicaid Analytic eXtract (MAX) - an administrative dataset managed by the Centers for Medicare and Medicaid Services (CMS). We use data for the years 2004 to 2015. Enrollment information on Medicaid beneficiaries is taken from the MAX Personal Summary files (PS), that contain person-month enrollment status. For individuals enrolled in Medicaid, these files hold data on demographic characteristics, the basis for Medicaid eligibility, and whether the individual is enrolled in a comprehensive managed care plan. Data on Medicaid's fiscal spending for each beneficiary is included in the MAX Inpatient (IP), Other Therapy (OT), and Prescription Drug (RX) files. These files track claims for services provided by the public FFS system. They also include information on the capitated premium payments to managed care plans. Our full sample includes all non-elderly beneficiaries that are eligible for full benefits from Medicaid due to disability, and are not enrolled in Medicare. We exclude all beneficiaries that ever moved between states or counties[3]

To assess the reliability of the MAX data in each state, we compare the state's aggregate Medicaid spending in MAX to the Medicaid spending that the state reports to CMS in CMS-64 forms. These forms report actual quarterly expenditures for which all supporting documentation has been compiled, and are used to determine the federal reimbursement to states.[4] As robustness tests, we repeat

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

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

our estimates for subsamples that only include data from reliable state-years, using a both a liberal and a more conservative definition of reliability.

In addition to Medicaid data, we use CMS' data on Medicare enrollment and the claims of non-elderly disabled beneficiaries. The base segment of the Master Beneficiary Summary File (MBSF) includes information on Medicare enrollment, enrollment in Medicare Advantage and in a Prescription Drug Program plan, and information on the basis of eligibility. Spending information is gathered from the MedPAR file, that contains information on inpatient hospital and skilled nursing facility stays, and the Carrier file, that holds claims submitted by professional providers. We use data on disabled beneficiaries that joined traditional (FFS) Medicare before 2004 - the first sample year of our Medicaid data.

2.3 Empirical Framework

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

[5] As explained in section 2.2, all movers are dropped from our sample. This means that individual fixed effects account also for time invariant characteristics of their county and state of residence.

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

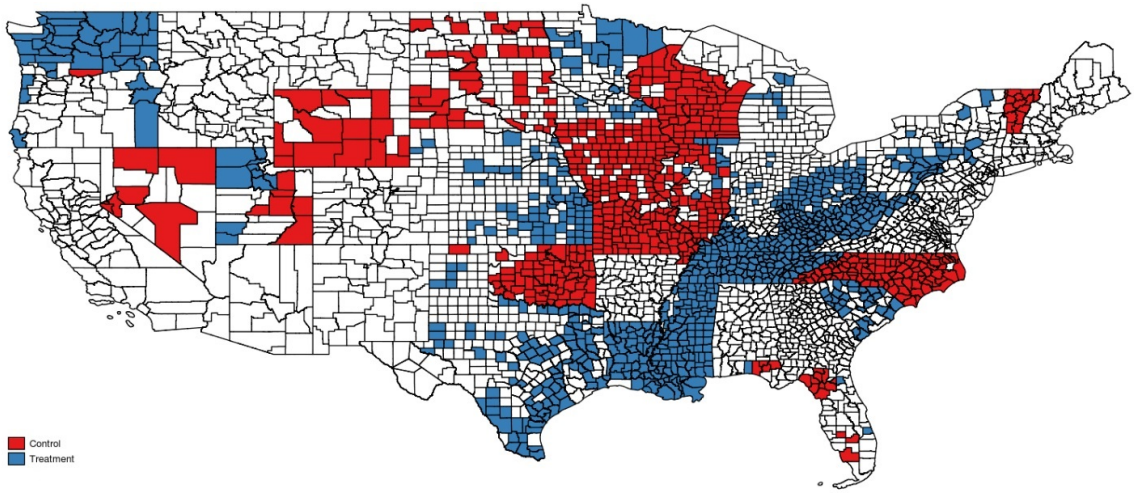
Including individual fixed-effects helps us also to address a possible threat to identification if private managed care plans affect who becomes or remains a Medicaid beneficiary. Currie and Fahr (2005) present some evidence that Medicaid managed care plans change the composition of children enrolled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

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

2.3.1 Identifying managed care enrollment mandates

To identify county-level managed care enrollment mandates, we use the MAX data to calculate the penetration rate of managed care plans among the disabled beneficiaries in each county over time. We define a county-level mandate as a sharp and swift increase in the penetration rate - an increase of at least 20 percentage points in the rate over at most 3 months. We use counties with a mandate as treatment counties, excluding mandates that occur at the first or last six months of our sample. Counties in which the managed care penetration rate never exceeds 10 percents, i.e. counties that rely on the FFS public system throughout our sample period, are used as control counties. Our baseline sample includes only treatment counties in which managed care penetration was below 10 percents before the mandate. Figure 2.1 presents the map of the identified treatment (blue) and control (red) counties in our baseline sample. Most of the mandates in our sample occur in 2011 to 2013 (Figure B.1 in the appendix presents the histogram of mandates by quarter throughout our sample period).

Figure 2.1: Treatment and Control Counties in the Baseline Analytic Sample



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

Table 2.1 presents summary statistics for the treatment and the control counties in our baseline sample.

Table 2.1: Descriptive statistics for treatment and control counties in the baseline analytic sample in 2004 (pre-mandate)

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

Note: This table presents summary statistics for the baseline analytic sample of counties in the treatment and control groups in 2004. The treatment counties are counties in which we identify a managed care enrollment mandate between 2004 and 2015, and the managed care penetration rate before the mandate doesn't exceed 10%. Control counties are counties in which managed care penetration *never* exceeds 10%. The statistics shown use data on treatment counties before any mandate occurred.

The sample includes 936 treatment counties in 20 states, and 723 control counties. The gender and age mix of disabled beneficiaries is almost identical in the average treatment and control county. However, treatment counties have more beneficiaries in them - there are 947 beneficiaries in the average treatment county vs. 518 in the average control county. Treatment counties also have a larger share of disabled beneficiaries eligible for Supplemental Security Income (SSI). Moreover, the total monthly Medicaid spending per beneficiary is higher in control counties, especially due to higher

FFS spending on "other" services (mainly outpatient services). Some of these differences are most likely related to the higher prevalence of managed care mandates in urban areas.

2.3.2 Event studies around enrollment mandates

To study the spending trends in our treatment and control groups we run event studies that examine three years before a mandate and four years after it occurs. The reduced form analysis is performed at the individual level and at a quarterly duration. We control for individual fixed effects in all our specifications. As all movers are dropped from the sample, these fixed effects control for all individual, county and state time-invariant characteristics. As mandates occur at different times throughout our sample period, and all control counties serve as control for all treatment counties, we include in the specification quarter fixed effects, that control for quarter-specific common shocks that affect similarly both treatment and control counties. As mandates are rolled out at the county-level, we cluster our standard errors at this level. The regression specification is the following:

$$
\begin{aligned}
Y_{ict} &= \alpha_0 + \sum_{j=-12}^{15} \beta_j I_{jct} + \gamma_i + \delta_t + \varepsilon_{it} \\
s.t.\ I_{jct} &= \begin{cases} 1(Quarter_t - MandateQuarter_c = j) & \text{if } Treat_c = 1 \\ 0 & \text{if } Treat_c = 0 \end{cases}
\end{aligned}
\tag{2.1}
$$

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

2.3.3 Mandates as IV for enrollment in a managed care plan

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

$$Y_{ict} = \beta_0 + \beta_1 PostY1_{ct} + \beta_2 PostY2On_{ct} + \gamma_i + \delta_t + \varepsilon_{it} \tag{2.2}$$

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

The IV specification uses the county-level mandates as an instrument for individuals' enrollment in a managed care plan. The IV estimates are local average treatment effects (LATE) for the population of disabled beneficiaries that transition from FFS to managed care due to the enrollment mandate in the county (i.e. "compliers"). The first stage regression is:

$$InMMC_{ict} = \beta_0 + \beta_1 Post_{ct} + \gamma_i + \delta_t + \varepsilon_{it} \tag{2.3}$$

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

effect. ε_{it} represents a random error term. The IV regression specification is:

$$Y_{ict} = \theta_0 + \theta_1 \widehat{InMMC_{ct}} + \gamma_i + \delta_t + \psi_{it} \tag{2.4}$$

where $\widehat{InMMC_{ct}}$ is the predicted value from equation 2.3 and ψ_{it} is a random error. θ_1 is the LATE - for beneficiaries that were shifted from the public FFS to managed care plans due to our identified mandates (i.e. "compliers"), it represents their average difference in the outcome Y_{ict} between managed care plans and the public FFS. To examine possible dynamic responses when using the IV, we repeat the IV estimation with two subsamples - one in which we drop all observations in treatment counties that are from quarters later than the first year after the mandate, and another subsample in which we drop all observations from treatment counties during the first year after a mandate.

2.3.4 Contiguous treatment and control counties

To make the treatment and control groups more comparable, we also analyze contiguous treatment and control counties. This restricted sample includes treatment counties (i.e. counties with an identified mandate), only if they have contiguous control counties (i.e. with managed care penetration that never exceeds 10 percents). Each treatment county and its contiguous control counties form a cohort. The analytic sample is constructed by stacking all the different cohorts together. In all specifications, we cluster the errors at the cohort level. Figure B.2 in the appendix presents a map of the (blue) treatment counties and (red) control counties. The stacked dataset is used to examine event studies around the mandates. For event studies, we use a specification similar to the one described in equation 2.1, but change the value of the indicator function I_{jct} in control counties to be equal to the value of the function in the cohort's treatment county at each quarter, so timing relative to the mandate is defined and uniform for all counties in the cohort. We also add, in all specification, a fixed effect for each interaction of cohort and quarter, allowing for cohort-specific time trends. The

reduced form specification is now:

$$Y_{icbt} = \beta_0 + \beta_1 PostY1_{bt} \times Treat_c + \beta_2 PostY2On_{bt} \times Treat_c + \gamma_i + \delta_t + \theta_b \times \delta_t + \varepsilon_{it} \quad (2.5)$$

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

$$InMMC_{ict} = \beta_0 + \beta_1 Post_{ct} \times Treat_c + \gamma_i + \delta_t + \theta_b \times \delta_t + \varepsilon_{it} \quad (2.6)$$

and the IV regression specification remains unchanged from equation 2.4.

2.3.5 Stacked DID with limited control groups

As additional robustness tests we estimate the fiscal effect of Medicaid Managed Care in a stacked difference-in-differences framework. Following this approach, used by Deshpande and Li (2019), Fadlon and Nielsen (2021), and others, we create a separate dataset for each cohort of treated counties, that includes the treated counties that have a mandate at a certain quarter, and all the control counties. All the counties in the dataset are assigned the same timing variables relative to the quarter of the mandate in the cohort's treatment counties. These cohort-by-cohort data sets are then stacked together to create the analytic sample. We examine two groups of control counties. First, we use counties that have a MMC mandate in the future, at least three years after the treated cohort's mandate. This option may make the group of control counties more similar to the treatment group,

further supporting the assumption of parallel trends required for the identification. With later-treated controls, results are identified off the timing of mandates, rather than their occurrence. Second, we use a control group that only includes counties that are never treated, i.e. have no mandate over the whole sample period. This specification shuts down a possible bias when using two-way fixed effects DID methods. As Goodman-Bacon (2021) demonstrates, these methods implicitly use already-treated counties as controls, leading to a biased estimate when treatment effects change over time. The estimated equations are identical to the specification in the contiguous counties case, in which we also create a stacked dataset of cohorts (Equations 2.5 and 2.6). All estimations include individual and quarter fixed effects, as well as a fixed effect for each interaction of cohort and quarter, allowing for separate time trends for each cohort of treated counties.

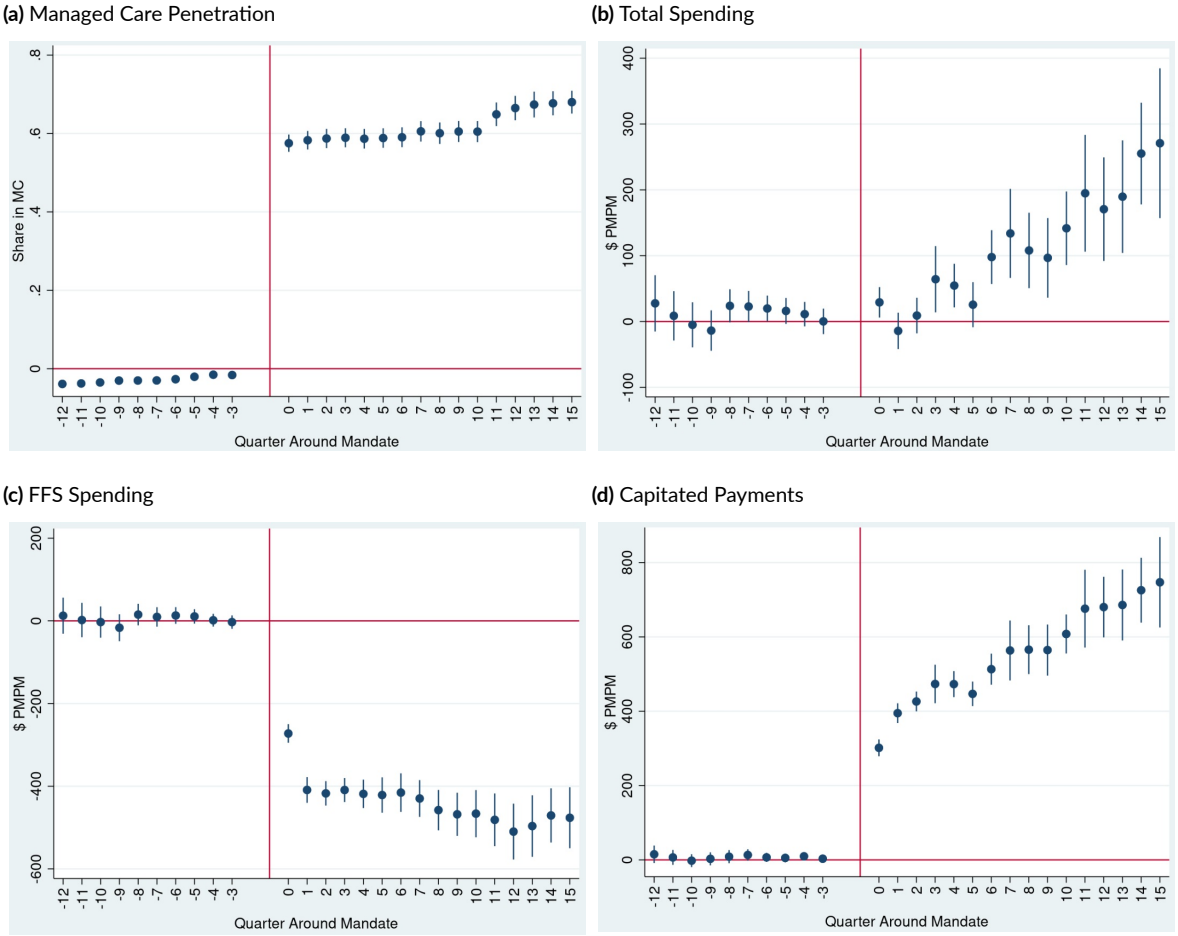
2.4 Results

2.4.1 Event studies around mandates

Figure 2.2 presents event studies examining the difference in outcomes between our treatment and control groups around a managed care enrollment mandate. For all the examined outcomes, the event studies show no significant differential trends between treatment and control counties in the three years before a mandate is rolled out.

After a mandate, panel A shows an increase of 50 to 60 percentage points in the penetration rate of managed care plans in treatment counties relative to control counties. Panel B examines the changes in total Medicaid spending around a mandate. The total spending in treatment counties, after a mandate, shows little consistent change during most of the first year after a mandate, but then starts to increase. Spending rises modestly at first, but increases more and more as time goes by. This result shows no support to the claim that managed care mandates save money to the Medicaid program, and it indicates that mandates lead to dynamics of increasing spending. The differences in

Figure 2.2: Event studies around managed care enrollment mandates



Note: figures show event studies around managed care enrollment mandates, i.e. the difference in the examined outcome between treatment counties and control counties, relative to the two quarters before the mandate (quarters -1 and -2). Quarter zero is the first quarter in which the mandate is in place. The sample includes only treatment counties with low pre-mandate MC penetration. Panel A presents the managed care penetration in the county, i.e. the share of disabled beneficiaries enrolled in a managed care plan. Panel B shows the dollar differences between treatment and control in the total Medicaid spending per beneficiary per month (PMPM). Panels C and D break the total into differences in FFS spending (panel C), and in capitated payments to managed care plans (panel D).

total spending are broken down to differences in Medicaid FFS spending (panel C) and differences

in capitated payments (panel D). As expected, a mandate that shifts a large share of enrollees from the public FFS system to managed care plans decreases the FFS spending and increases the amount of capitated payments.

2.4.2 Reduced form and IV estimates

Table 2.2 presents the reduced form estimates of the effects of a managed care enrollment mandate. After a mandate, the share of disabled beneficiaries enrolled in managed care increases by 60 percentage points in treatment counties (column 4). At the first year after a mandate there is no significant change in the total fiscal spending of the Medicaid program. However, in the period after the first year, the monthly spending per beneficiary increases in treatment counties by $98 relative to control counties (Column 1). This is a 8 percent increase over the pre-mandate mean spending in the treatment counties. At the first mandate year, the monthly FFS spending per beneficiary decreases in these counties by $375 (column 2), while spending on capitated payment rises by a similar amount of $387 (column 3). FFS spending continues to decrease in later years, by additional $66, but this decrease in spending is overwhelmed by a $151 increase in capitated payments to the managed care plans.

Table 2.3 presents the IV estimates of the effect of individuals' enrollment in a managed care plan on Medicaid's monthly spending for them. Enrollment is instrumented using county-level managed care enrollment mandates. Columns (2) and (3) present the estimates for the first year after a mandate and for the years after that, accordingly. The IV estimates confirm the dynamics observed in the reduced form estimation - change in Medicaid spending is insignificant at the first year after an enrollment in a managed care plan (due to a mandate). In later years, the monthly Medicaid spending per beneficiary increases by $194 relative to beneficiaries that remain in FFS (in control counties) - a 16% increase over the baseline spending in treatment counties.

Table 2.2: The effects of a managed care enrollment mandate (reduced form)

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

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

Note: Table shows estimates of the impact of a managed care enrollment mandate on the examined outcomes in treatment counties, relative to control counties. All specification include also a constant, individual fixed effects and quarter fixed effects. Standard errors are clustered at the county level. Column (1)-(3) show the effect on total/FFS/Capitated spending, accordingly, all measured in dollars per beneficiary per month (PMPM). Column 4 presents the effect of a mandate on the share of disabled beneficiaries enrolled in a managed care plan. Baseline mean values are calculated for the quarter before a mandate. All coefficients are from estimating Equation (2.2). For more details see Section 2.3

2.4.3 Placebo test: Spending on Medicare's disabled

For our IV estimates to be valid, the mandate instrument should satisfy the exclusion restriction, i.e. Medicaid managed care enrollment mandates should affect Medicaid spending only through their effect on enrollment in managed care plans. To support the assumption that this requirement is satisfied, we run a placebo test in which we examine the effect of the Medicaid mandates on *Medicare*'s spending on non-elderly disabled beneficiaries.[6] The event study graph in figure 2.3 shows no difference in Medicare's spending on the disabled between treatment and control counties after Medicaid enrollment mandates. This result reduces the concern that other concurrent shocks in our treat-

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

Table 2.3: The effect of enrollment in a managed care plan on total spending (IV)

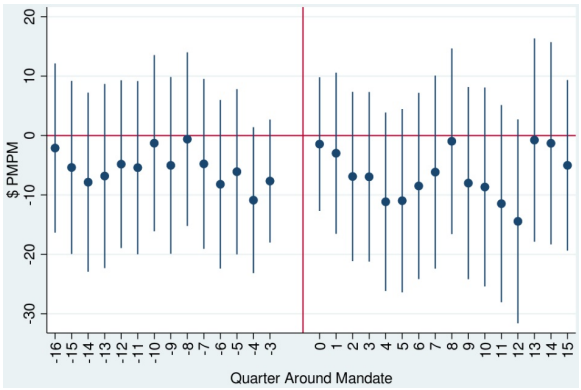
	(1) All Post-Mandate Years	(2) First Year	(3) After the First Year
In a MMC Plan	86.28***	-2.18	194.40***
	(26.25)	(21.49)	(37.60)
Baseline Mean	1,227	1,227	1,227
# of beneficiary-quarter obs.	38,887,578	31,013,833	35,308,346
# of beneficiaries	3,034,342	2,661,490	2,977,712
# of counties	1,663	1,663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

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

Note: Table shows instrumental variable estimates of the impact of individuals' managed care enrollment on the Medicaid spending on them. Individual's MC enrollment is instrumented by MC enrollment mandates in the individual's county. All specification include also a constant, individual fixed effects and quarter fixed effects. Standard errors are clustered at the county level. Column (1) is estimated on the baseline sample. Column (2) is estimated on a sample that drops all post-mandate observations after the first year. Column (3) show estimates for a sample that drops all observations from the first year after a mandate. These coefficients are from estimating Equation (2.4). For more details see Section 2.3

ment counties affect medical spending for the disabled after enrollment mandates, and it provides support for the assumption that the exclusion restriction holds for the instrument.

Figure 2.3: Placebo test: effect of Medicaid mandates on Medicare spending on disabled



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

2.4.4 Robustness I: Alternative samples

In this section we explore the robustness of our estimates to restricting the analytic sample to state-years with more reliable MAX data. We measure reliability by the difference between aggregate Medicaid spending in the MAX database and the aggregate spending reported in the state's CMS-64 reports. We use a liberal criteria of reliability and a conservative (more restrictive) criteria and repeat our estimation for the two resulting subsamples. Columns (1) and (2) in Table 2.4 present the IV estimates when these county selection criteria are applied. Column (3) presents the results of an estimation on a third sample that includes a balanced panel of treatment counties, for which reliable data (under the "liberal" definition) is available for the three years before and after the enrollment mandate. In all alternative estimations, like in our baseline results, enrolling in a MMC plan (due to a mandate) has no significant effect on Medicaid's spending during the first year, but leads to a 13% to 21% increase in spending over the years after that.

Table 2.4: IV estimates for alternative samples

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

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

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

2.4.5 Robustness II: Contiguous treatment and control counties

Table 2.5 presents IV estimations for contiguous treatment and control counties (Table B.1 in the Appendix presents the reduced form estimates). Similar to the baseline estimation, monthly spending for beneficiaries that enroll in MMC (due to a mandate) is higher after the first year of the enrollment mandate - by $457 (30% of the baseline mean). During the first year, spending seems to decrease, by $90, but this decrease is not statistically different than zero.

Table 2.5: IV estimates for the contiguous counties sample

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

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

Note: Table shows IV estimates of the impact of individuals' managed care enrollment on Medicaid spending. Individual's MC enrollment is instrumented by MC enrollment mandates in the individual's county. All specification include also a constant, individual fixed effects, quarter fixed effects, and fixed effects for cohort-quarter interactions. Standard errors are clustered at the county level.

2.4.6 Robustness III: Stacked Difference-In-Differences

Table 2.6 presents the results of the IV estimations when using the stacked DID approach. All counties in the sample come from states with reliable MAX data (using the "Liberal" definition for reliability), and all treatment counties have low pre-mandate penetration rate of managed care. In column (1) we estimate the effect of an MMC enrollment mandate when the control group is restricted to counties that during our sample period never experience an MMC enrollment mandate. Similarly to our main results, there is little change in Medicaid's spending in the first year after

the mandate, but spending increases significantly in later years - here, by $172 PMPM (14% increase relative to the mean in the baseline period). Column (2) presents the estimation's result when the control group for each treated county comprises counties that have a mandate later on, at least three years in the future. The estimates show a decrease of $109 in spending at the first year after a mandate - 9% relative to the baseline mean. However, this decrease doesn't hold in later years. After the first year, there is a $6 increase in spending (0.5%), though this increase is statistically insignificant. This is the only specification we use in which the increase in spending after the first year is so small and not different than zero in a statistically significant way. Even in this specification there is no evidence that a transition to MMC reduces Medicaid's spending beyond the first year.

Table 2.6: IV estimates for stacked DID estimations

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

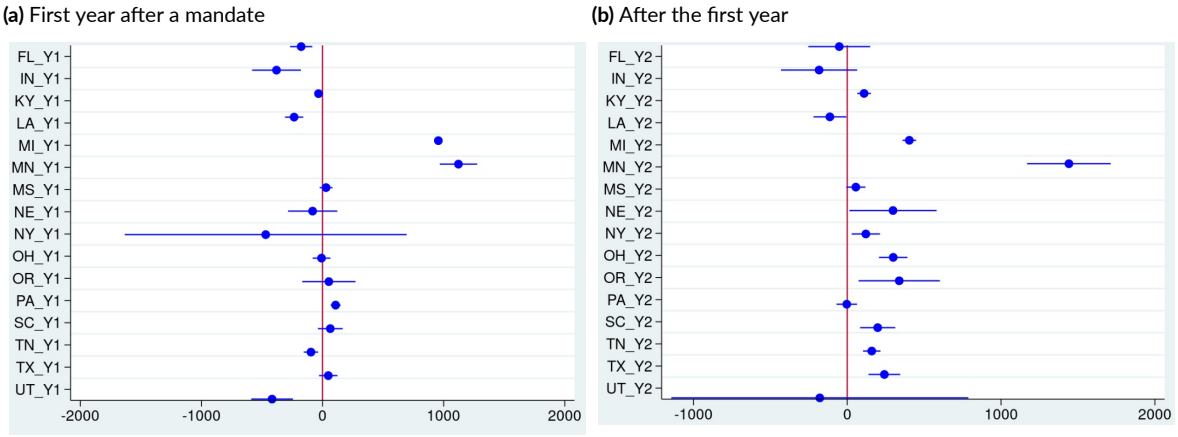
Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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

2.5 Heterogeneity Between States

All previous results have estimated the *average* effect of managed care mandates and managed care enrollment on Medicaid spending. However, since the Medicaid program differs from state to state, we turn now to examine heterogeneity in the effect of managed care on spending. Figure 2.4 presents the IV estimates of the effect of enrollment in a managed care plan for each state with treatment counties. The estimates use data from treatment counties in which the penetration rate of managed care is below 10% before the mandate is rolled out.

Figure 2.4: State by state IV estimates of the effect of MMC on Medicaid spending



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

Panel A presents the IV estimates for the first year after the mandate. Out of 16 states with treat-

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

2.5.1 Heterogeneity by pre-mandate FFS prices

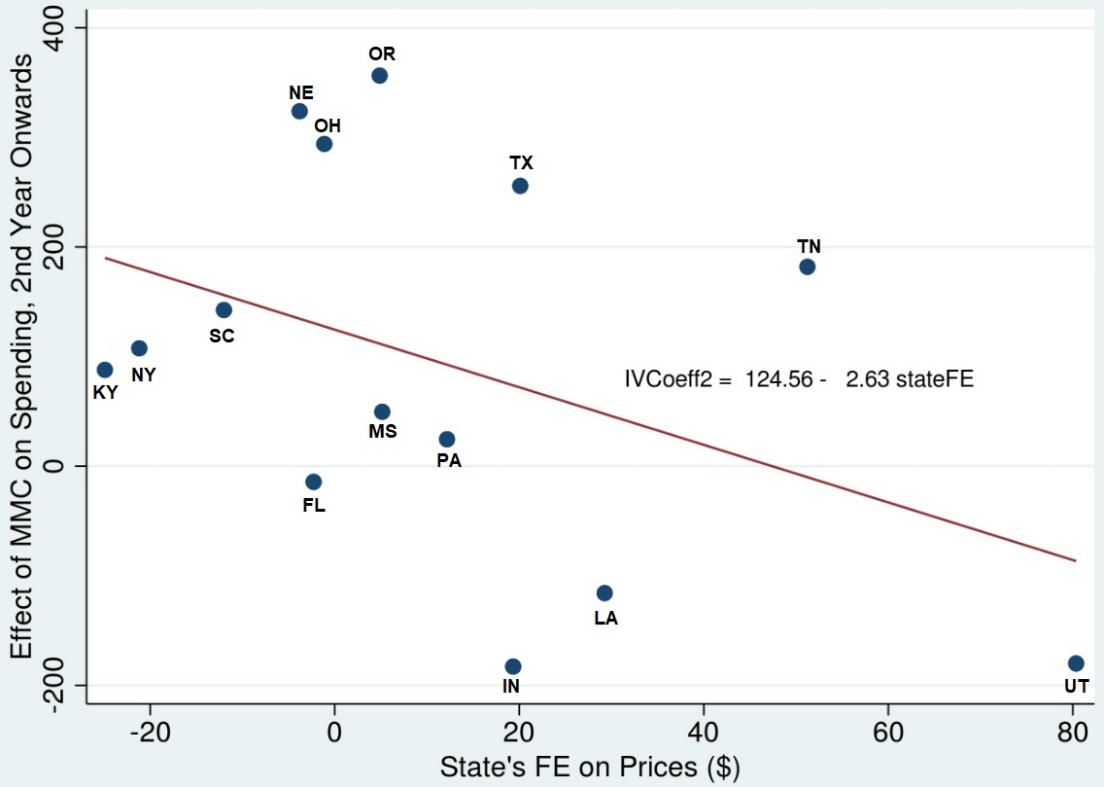
The ability of managed care plans to reduce costs by reducing payment rates to providers may depend on the baseline payment rates in the public FFS system they replace. If rates are very low to begin with, managed care plans may face higher rates than the public FFS system, increasing their costs. Duggan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

$$Price_{ips} = \beta_0 + \gamma_p + \delta_s + \varepsilon_{ips} \tag{2.7}$$

where $Price_{ips}$ is the payment for procedure p in claim i in state s. γ_p is the procedure fixed-effects, δ_s is the state fixed-effects, and ε_{ips} is a random error. Figure 2.5 presents, for each state, the IV es-

timate for the effect of managed care enrollment on Medicaid spending after the first year, relative to the state's fixed effect on FFS prices in 2004. The scatter plot suggests that states with lower pre-mandate FFS prices tend to experience higher increases in spending after their beneficiaries transition from FFS to private managed care plans.

Figure 2.5: The effect of managed care on Medicaid spending in a state and the state's fixed effect on Medicaid FFS prices



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

2.6 Discussion

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

2.6.1 Mechanism I: Higher costs for managed care plans

Managed care plans may see higher costs for treating disabled Medicaid beneficiaries if, first, utilization is higher in managed care comparing to the public FFS system. This may be the case if indeed disabled beneficiaries are under-served in Medicaid's public FFS system (KFF (2012)), and have needs that are met when they transition to private managed care plans. Layton et al. (2019) provide an example for such unmet needs in Texas, where the FFS system limited the number of prescriptions that beneficiaries could fill to three per month. This drug cap was eliminated after a transition to managed care, increasing the utilization of prescription drugs and the spending on them. Second, cost in managed care plans may be higher than the FFS system if the plans pay higher prices to providers. This may happen if the state's FFS payment rates are already very low before the transition to managed care, and plans need to pay higher prices to accommodate the higher utilization. For example, Layton et al. (2019) find an increase in outpatient prices in Texas after a transition from FFS to managed care, alongside an increase in the utilization of these services. The association we find between lower pre-mandate prices in a state and higher Medicaid spending post-mandate provides another support for this price mechanism. Third, costs could be higher under managed

care if plans' administrative costs are higher than in the FFS system. Lastly, disruptions in care during the initial period after a mandated transition to managed care may hurt individuals' health and lead to higher costs later on (see chapter 1 of this dissertation).

2.6.2 Mechanism II: Dynamic increase in Medicaid spending

Higher costs for managed care plans don't necessarily lead to higher costs for the Medicaid program. However, the procurement process of managed care plans in many states could lead to such a connection. The contracts signed with MC plans include the capitation rates paid for each beneficiary and the rules for updating the rates. These rules are affected by the CMS guidelines for the development of capitation payment rates to managed care plans. The guidelines (CMS (2019)) direct states to set actuarially sound rates based on the experience of the Medicaid's population in the recent three years. These rates should be updated annually. Such rules make sure that past increases in plans' costs will lead to higher payments by the Medicaid program. These procurement rules may decrease plans' incentives to save costs, leading to dynamics of continuous increases in spending. Many state follow CMS guidelines, requiring plans to comply with a Medical Loss Ratio (MLR) of 85% or higher. This requirement may further decrease plans' incentives to save, as higher spending is required to achieve higher (absolute) profits.

2.7 Conclusion

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

3

The Impact of Utilization Thresholds in Risk Adjustment Systems on Fit and Incentives for Gaming

°I am grateful to Tom McGuire for his guidance. For their helpful comments and explanations, I thank Konstantin Beck, John Bertko, Randall Ellis, Kobi Glazer, Lukas Kauer, Richard van Kleef, Tim Layton,

3.1 Introduction

Risk adjustment schemes are a cornerstone of a functioning managed competition market for health insurance. They reallocate funds among competing health plans based on the risk of their enrollees, and by that decrease plans' incentives to select profitable (typically healthier) enrollees and deter unprofitable (sicker) ones (see Ellis et al. (2018), Layton et al. (2018c) for an in-depth review of risk adjustment). To improve their predictive accuracy, risk adjustment systems have long ago advanced from relying only on age and gender adjustors that are exogenous to plans' influence, and now often use adjustors established from medical claims.[1] All these adjustors depend on enrollees' utilization of services either directly, e.g. adjustors based on the utilization of prescription drugs, or indirectly, e.g. diagnoses-based adjustors that are established during provider-patient interactions (Geruso and McGuire (2016)). Any adjustor based on utilization requires a decision regarding the minimum level of utilization that will trigger the adjustor - the utilization threshold. This decision is often made implicitly - in most cases diagnoses-based adjustors require a single appearance of a diagnosis over a year. However, other thresholds are possible. Germany, for example, explicitly requires that out-patient diagnoses appear twice over the year, in two separate quarters, to trigger a morbidity adjustor. Explicit thresholds are also common for adjustors based on the use of prescription drugs. To limit the incentive for gaming these adjustors by manipulating the prescription behavior, most countries that use them require a minimum level of utilization before a prescription affects a patient's risk score - often 90 or 180 days of supply.

This paper studies utilization thresholds and examines how the choice of their level affects the

Michael McWilliams, Joe Newhouse, Sonja Schillo, Erik Schokkaert, Wynand van de Ven, and participants of the Harvard Health Care Policy Department's Health Economics seminar, and the Risk-Adjustment Network 2020 meeting.

[1] Better predictive accuracy, i.e. higher fit of the actual costs, may decrease plans' incentive for cost saving - a tradeoff long acknowledged in the literature. Moreover, using adjustors that are endogenous to plans' influence may create opportunities for manipulation.

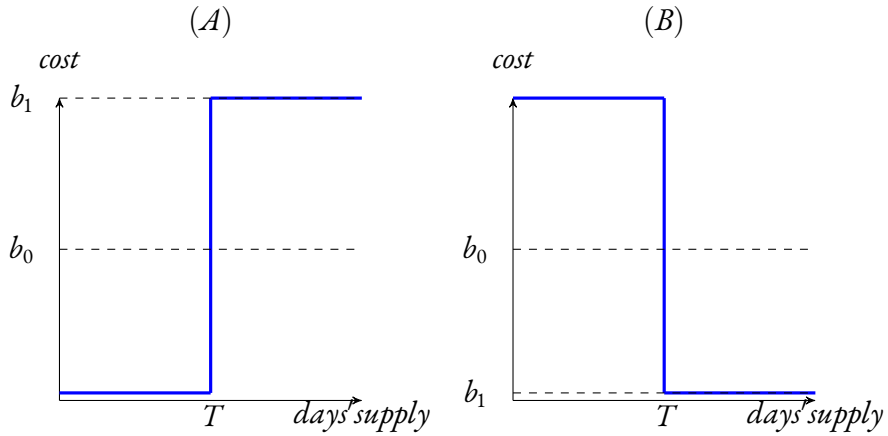
performance of the risk adjustment model. Explicit thresholds would be desirable when a certain level of utilization is, first, more predictive of spending, and second, less prone to manipulation comparing to the baseline. I show that the optimal level of a threshold is an empirical question, and may be unique for each adjustor. As a case study, I examine thresholds for new prescription-drug adjustors, added in 2018 to the risk adjustment model in the U.S. Marketplaces established by the Affordable Care Act (CMS (2016a,b)). Based on simulations of multiple thresholds, I find that choosing the right threshold for each drug adjustor may increase the fit of the model's predictions. It may also decrease the incentives for gaming. For some adjustors, there is no tradeoff between fit and incentives for gaming, as both are improved when setting the optimal threshold. While the main analysis in the paper focuses on drug-adjustors, the results are similar for morbidity-based adjustors, when choosing an explicit threshold for the number of times a diagnosis appears in claims over the year (Section C.0.1 in the Appendix).

Intuition may suggest that a higher utilization threshold always harms the fit, as information about some utilizers seems to be ignored. However, this intuition is wrong as a rule. To see this, consider a simple risk adjustment system with only one adjustor that indicates the use of drug X. For each patient using the drug, a plan receives a risk-adjustment payment that equals the average of the additional costs for all drug-X users.[2] If there is a utilization threshold, then the payment is the average additional cost of only patients with utilization above the threshold. Suppose that the cost of drug X itself is negligible and the number of users is small relative to the number of non-users. Figure 3.1 presents two possible distributions of the additional cost of patients using the drug, ordered by the number of days' supply in prescriptions they fill during the year. Costs may be higher for high utilizers of the drug (panel A) if a higher use indicates a severe chronic condition with additional co-morbidities. Alternatively, costs may be lower for high utilizers (panel B) if higher and

[2] This will be the payment if the coefficient for the drug-X adjustor comes from an OLS estimation of enrollees' costs on a constant and the single adjustor. This kind of estimation is the typical way to set coefficients for adjustors in risk adjustment formulas.

continuous use indicates a patient with good drug adherence and a controlled disease.

Figure 3.1: Possible distributions of enrollees' costs, by days' supply of drug X



The figure presents two possible distributions of the additional costs for users of a certain drug ("drug X") by the days' supply of the filled prescriptions for each patient during the year. In panel A, high-cost patients have high utilization of the drug, while in panel B low-cost patients have higher utilization. b_0 is the average additional cost over all users of drug X. Hence, b_0 equals also the payment to the plan for each such patient. Setting a utilization threshold of T days' supply changes the payment due to patients below the threshold to zero. Patients above the T-days threshold have an average cost of b_1, and hence the plans receive a payment of b_1 for them. The threshold increases the individual fit in panel A as each type of patient receives the correct payment. However, the threshold decreases the fit in panel B, as all patients receive zero payment.

Without a threshold, the average additional cost for all patients with the drug-adjustor turned on is b_0, and hence the payment to the plan is also b_0. Setting a utilization threshold that requires prescriptions of at least T days' supply has a very different impact on the individual fit in these two cases. In panel A, setting a threshold T would improve the fit, as the plan would be payed zero for low-cost patients and the correct cost b_1 for high cost patients. In panel B, setting an identical threshold of T days' supply, would decrease the fit as both high-cost patients and low-cost patients would receive zero payment. This is equivalent to eliminating the adjustor altogether and fit must be better with the adjustor than without it.

The simple example demonstrates that utilization thresholds may sometime increase and not decrease the fit of a risk adjustment model. I study this empirically in the setting of the ACA Market-

places. Section 3.2 describes the risk adjustment model in these markets and elaborates on the new drug adjustors added in 2018. To simulate thresholds, I use the IBM Truven Marketscan database - a large database of claims from employers and commercial health plans. The dataset, described in section 3.3, is the same one used to develop the Marketplaces' risk adjustment model (Kautter et al. (2014)). I use the data from 2015 and 2016 for calibration of the model, and apply the risk adjustment model to enrollees in 2017.

Section 3.4 presents the empirical evaluation of the impact of thresholds on model fit. For each of the ten current drug adjustors (RXCs), I simulate multiple days' supply thresholds between zero and 360 days. For each threshold, I first remove the RXC indication from patients with utilization below the required days' supply amount. Then, the risk adjustment model is re-estimated on data from 2015 and 2016. The coefficients from this revised model are used to predict the risk scores of the 2017 enrollees, and the payments to the plan. Lastly, fit is calculated, comparing the payment under the threshold to the baseline 2017 costs.[3] I calculate both the overall individual fit (for all enrollees) and the individual fit in each RXC-HCC disease group of patients with either an RXC prescription (regardless of thresholds) or a diagnosis related to the examined drug adjustor.[4] The results show a unique pattern of the fit for each drug. For five out of the ten current drug adjustors, a non-zero utilization threshold would improve both the overall fit (by 0.07% to 0.17% of the baseline fit) and the fit for the disease group related to the drug (by 2.1% to 9.6% of the baseline fit). The fit-maximizing threshold is 60 days' supply for anti Hepatitis-C agents, 120 days for Multiple Sclerosis agents and for Immune Suppressants, and 180 days for Anti-HIV agents and for Cystic Fibrosis agents. For five other drug adjustors - Antiarrhythmics, Phosphate Binders, Inflammatory Bowel

[3] I calculate the "individual fit" - the R-square of a model that predicts individuals' costs using the adjustors included in the risk-adjustment formula. I also present the calculation of the Cumming's Prediction Measure.

[4] For example, RXC1 is the drug-adjustor of Anti-HIV Agents. When simulating thresholds for this adjustor, I calculate the individual fit for the group including all patients with a prescription for these drugs or with an HIV diagnosis (HCC1).

Disease, Insulin, and Other Anti-Diabetic Agents - fit is maximized with no threshold.

The results confirm the intuition from the simple example in Figure 3.1: In the drug groups where a threshold improves the fit, patients' costs are an increasing function of the number of days' supply in their prescriptions. In these groups, patients with prescriptions covering fewer days are generally over-compensated (i.e. the plans' revenue for them is higher than their cost), while patients with long prescriptions are under-compensated. The optimal threshold decreases the payment to the over-compensated, as the drug-adjustor is turned off for them, and increases the payment to the under-compensated as the payment is re-estimated to fit their average cost.

Utilization thresholds may impact not only the fit of the risk adjustment model, but also the incentives for gaming it. A higher utilization threshold does not necessarily reduce the incentives for gaming. One reason for that is that a higher threshold affects not only the costs of gaming but also the revenues from it. First, because the opportunity for gaming depends on the existing prescription behavior and the resulting distribution of patients by days' supply. A higher threshold may increase the number of patients susceptible for gaming if a larger group is left below it. For example, if all patients in the group are prescribed with 28 days' supply, then setting a 30-days threshold will lead to higher gaming incentives than with no threshold at all. Another reason that higher thresholds may increase the potential revenues from gaming is that the payment per patient above the threshold may rise as the threshold increases. That may happen when patients' costs increase in the number of days' supply in their prescriptions. For all these reasons, finding whether a threshold decreases or increases the incentives for gaming requires an empirical examination.

In section 3.5, I introduce three measures of the plans' incentives to prescribe more - incentives measured by the potential net revenue plans can gain from this gaming. It should be noted that the actual response of the plans to these incentives are not studied in this paper. The first measure examines gaming incentives in relation to patients with either an existing prescription for a drug in the drug category (the RXC) or a related diagnosis. It examines incentives to prescribe an *unlimited*

number of additional days to push *all* these patients across the utilization threshold, thus serving as an upper bound for the scope of gaming. The other two measures examine a more limited gaming activity of prescribing no more than 30 additional days of supply to push patients across the threshold. They separately examine such gaming activity for patients with an existing prescription, and for patients with a related diagnosis but no prescription.

The effect of thresholds on the incentives for gaming varies by drug group. Using simulations, I find that days' supply thresholds may reduce the potential net revenue from unlimited gaming for seven out of the ten drug-adjustors. For three adjustors (Anti-HIV agents, Antiarrhytmics, and Phosphate binders) the incentive for unlimited gaming is minimized without a threshold. In an important finding, for four drug-adjustors, setting a utilization threshold may pose no trade-off between fit and gaming incentives as both are improved. Relative to the no-threshold scenario, setting a 60-days threshold for anti Hepatitis-C agents (RXC2), improves the individual fit for the disease group by 3.1%, while reducing the potential net revenue from unlimited gaming by 54%. A 120-days threshold improves the fit by 9.6% for RXC8 (Multiple Sclerosis agents) and by 2.1% for RXC9 (Immune Suppressants and Immunomodulators), decreasing the incentive for unlimited gaming by about 40%. Lastly, setting a 180-days threshold for Cystic Fibrosis agents (RXC10) improves the fit by 8.6%, while the incentive for gaming decreases by 5%.

I discuss the results in section 3.6. Section 3.7 concludes.

3.2 Prescription Drugs in the Risk Adjustment Scheme in the Marketplaces

Risk equalization among the plans in the U.S. Marketplaces includes two components: the Department of Health and Human Services (HHS) risk adjustment model and a transfer formula (Layton et al. (2018b)). The basic model predicts this year's plan liability for enrollees based on their age,

sex and the diagnoses drawn from their claims, producing a risk score for each person. The transfer formula redistributes plans' premium revenues by the average risk score in each plan and other factors. The prediction model produces 15 sets of risk adjustment coefficients: three age-specific models (adult, child and infant), and five models specific for each coverage level in the Marketplaces (platinum, gold, silver, bronze, catastrophic).

Beginning in the 2018 benefit year, CMS started using a "hybrid drug-diagnosis" risk adjustment model in the Marketplaces, adding adjustors indicating a filled prescription for the included drugs (CMS (2016a,b)). For example, a patient who filled a prescription for insulin will have a higher risk score, potentially increasing the risk adjustment transfer to her plan, whether she has a diabetes diagnosis in one of her claims or not. The drug adjustors are meant to indicate health risk when a diagnosis is missing. This can happen due to a mistake, to avoid stigma, or because the patient did not visit a physician. However, the drug adjustors appear independently in the risk adjustment model, and are not used only to "turn on" a related diagnosis-adjustor. The drug adjustors may also provide information on the severity of a diagnosed illness. To do this, the model includes interactions of drug-adjustors and their related diagnosis-adjustors. In the model, no minimum utilization is required for a prescription to increase a patient's risk score, e.g. a prescription of insulin for a single day will suffice to increase the score, and will have the same effect as a prescription for a year's supply. The Centers for Medicare & Medicaid Services (CMS), the federal agency that administers this risk adjustment system, considered setting days' supply thresholds for some potential drug adjustors, but eventually set no threshold.

The baseline risk adjustment model in this paper is the CMS 2019 model (*HHS-HCC V0519*), that includes ten drug-adjustors (RXCs). Each RXC is a prescription drug category that may include several drugs, identified by their National Drug Code (NDC). CMS chose RXCs that are closely related to diagnoses that were already included in the model within Hierarchical Condition Categories adjustors (HCC), that group diagnoses. Each RXC appears in the model as both

an independent adjustor and within an interaction with its paired HCCs. Table 3.1 describes the RXC-HCC disease groups in the 2019 model.

Table 3.1: Drug-Diagnosis Pairs in the 2019 Marketplaces Risk Adjustment Model

RXC	RXC Label	Related Diagnoses (HCCs)
1	Anti-HIV Agents	HIV/AIDS
2	Anti-Hepatitis C (HCV) Agents	Chronic Hepatitis C, Cirrhosis of Liver, End-Stage Liver Disease, and Liver Transplant
3	Antiarrhythmics	Specified Heart Arrhythmias
4	Phosphate Binders	End Stage Renal Disease, Kidney Transplant, Chronic Kidney Disease - Stage 5, Chronic Kidney Disease - Severe (Stage 4)
5	Inflammatory Bowel Disease Agents	Inflammatory Bowel Disease, Intestine Transplant
6	Insulin	Diabetes, Pancreas Transplant
7	Anti-Diabetic Agents, Except Insulin and Metformin Only	Diabetes, Pancreas Transplant
8	Multiple Sclerosis Agents	Multiple Sclerosis
9	Immune Suppressants and Immunomodulators	Rheumatoid Arthritis and Specified Autoimmune Disorders, Systemic Lupus Erythematosus and Other Autoimmune Disorders, Inflammatory Bowel Disease, Intestine Transplant
10	Cystic Fibrosis Agents	Cystic Fibrosis, Lung Transplant

The coefficients in the 2019 version of the model are based on an average of the coefficients separately estimated for the years 2014, 2015 and 2016 (CMS (2018)). CMS estimated the 2014 and 2015 coefficients using the Truven MarketScan database, that includes claims from large employers and health insurers in the commercial market (this is the database used in this paper and is described in section 3.3). The 2016 coefficients were estimated using claims data from the insurers that operate in the Marketplaces (data from the External Data Gathering Environment servers - EDGE). This was the first time CMS used such marketplace-based data in the risk adjustment estimation. After blending the coefficients from three years, CMS adjusts them post-estimation for clinical reason-

ableness and to decrease gaming.

CMS considered in a 2016 White Paper (CMS (2016a)) whether to require a utilization threshold to trigger a drug indication - either require multiple prescriptions for the same drug, or prescriptions totalling at least 30 or 60 days' supply. CMS' clinical consultants suggested that for some potential RXCs, a minimum days' supply utilization threshold would be useful to distinguish severely ill patients from those with milder conditions. However, CMS decided to not include a days' supply restriction in the model, requesting feedback from the public.

Prescription drugs serve as adjustors in risk adjustment models in other countries as well. In most cases, some minimum utilization threshold is required to trigger an indication. In Germany, 183 days' supply are required for drug adjustors to validate most chronic diseases, 42 days are required for diseases with medication to be taken as needed, and 10 days are required for acute diseases. Switzerland, the Netherlands and the Czech Republic demand prescriptions of at least 180 days' supply for most drug groups. The Netherlands has a 90 days threshold for some specific groups, and no threshold at all for extremely high-cost drugs. See Table C.1 in the appendix for more details on the use of drug adjustors in these countries.

3.3 Data

This paper uses the IBM Truven MarketScan database of medical claims from the employer-sponsored insurance market to measure spending, record diagnoses, and examine the utilization of prescription drugs. Utilization of drugs is measured by the number of days' supply, i.e. - the number of days for which supply will last for the patient when using the maximum dose prescribed.[5] The Truven database was used to develop the original Marketplace payment system (Kautter et al. (2014)),

[5] This measure, appearing in U.S. pharmacy claims, is different than the number of Defined Daily Doses - a uniform standard dose defined for each drug by the World Health Organization.

and until recently was used exclusively in updating it.[6] I estimate the risk adjustment coefficients using the 2015 and 2016 versions of the database, and use the 2017 version to simulate payments under different utilization thresholds. The analytic sample is composed of adults, between ages 21 and 65. It includes individuals who had coverage for both prescription drugs and mental health, were continuously enrolled for twelve months, and had fee-for-service claims data for the whole period (i.e. no encounter data from managed care plans).[7] Table 3.2 reports summary statistics for the 12,227,124 individuals in the analytic sample. Their average annual spending is $5,741. 5.9% of them have a prescription for a drug included in one of the ten RXC drug-adjustors. The cost of treating these patients is 4.5 times higher than the cost of the average enrollee. Table 3.2 also presents the share of patients and the average cost for each RXC-HCC disease group, and for patients with a prescription for the RXC drugs.

For each RXC drug group, Figure 3.2 presents the distribution of patients with a prescription by their annual number of days supplied. The share of the prescribed patients among the RXC-HCC group is noted at the upper-left corner of each graph.

Not surprisingly, the number of days' supply are mostly bunched in multiples of 30-days or 28-days.[8] Patients with more than a year-worth of supply are top coded in the figure and included in the 365 days' supply category.[9] Figure 3.2 also shows the average cost of patients for each days' supply category. Costs increase as a function of the number of days' supply for RXCs 1,2,8 and 10, decrease with days' supply for RXC 3, and are mostly stable for RXCs 4, 5, 6 and 7. For RXC 9, costs are stable for patients with 30-days multiples of prescriptions, and are higher and increasing for

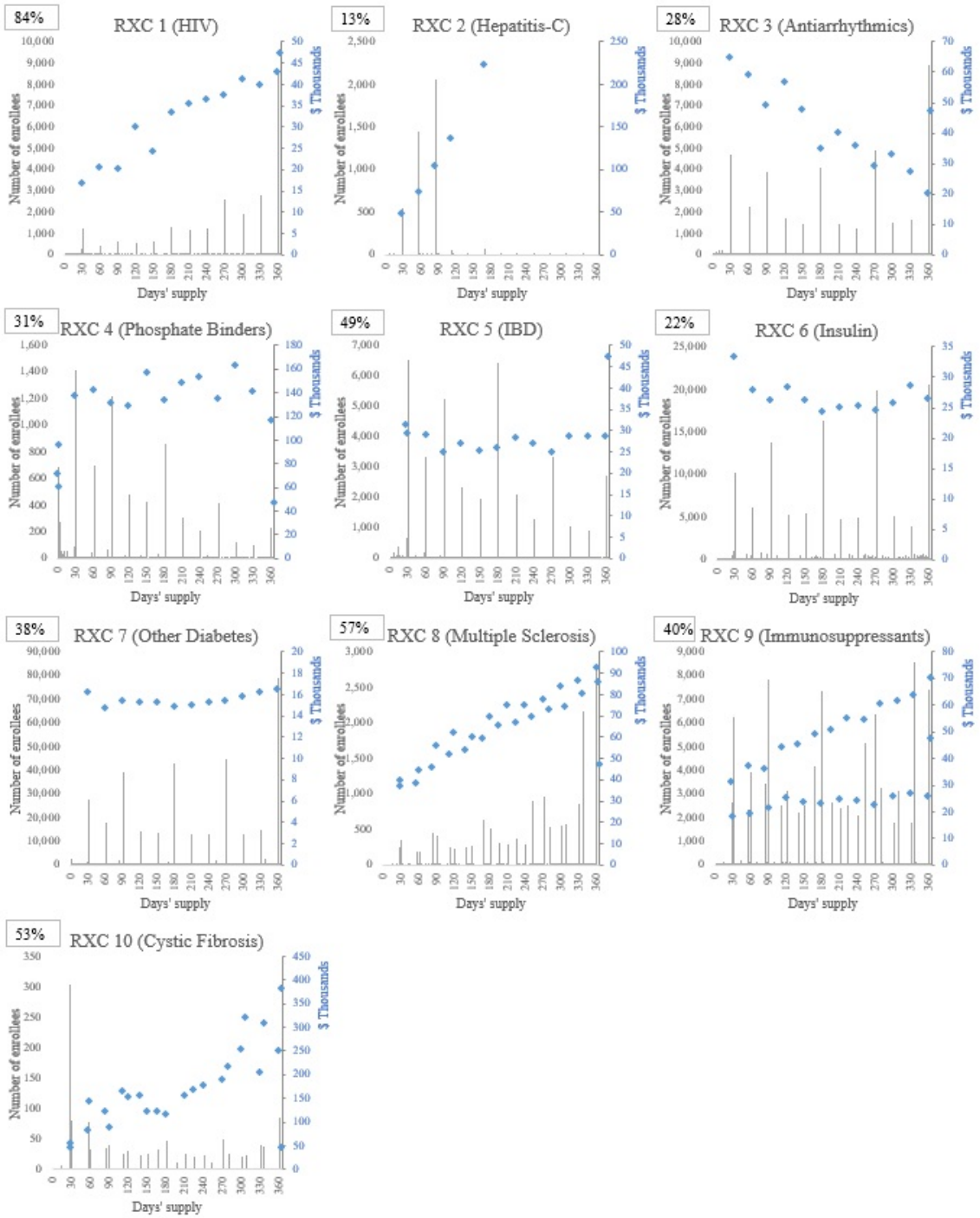
[6] Starting in the model for 2019, CMS is gradually shifting to using claims data from the plans in the Marketplaces themselves (EDGE data), instead of the Truven database.

[7] I allow for negative claims, but drop enrollees with a negative sum of their total spending for the year, as well as enrollees with a negative sum of spending for one of the categories: in-patient care, out-patient care, or drugs.

[8] This bunching presumably reflects the common packaging of the RXC drugs. It happens despite the growing popularity of 90-days prescriptions in both mail-orders and retail pharmacies.

[9] During a single year, some patients may fill prescriptions with more than a year's worth of supply if, for example, a long prescription is filled toward the end of the year.

Figure 3.2: The distribution of prescribed patients and their average annual cost, by annual number of days' supply



For each RXC, the graph shows: 1. The share of patients in the disease group prescribed with an RXC drug (box at the upper left corner); 2. The number of patients by the number of annual days' supply in their prescriptions (bars, left axis). Patients with no prescription are excluded. All patients with 365 days' supply or more are top-coded to the 365 days category; 3. The average costs of patients by the number of annual days' supply in their prescriptions (dots, right axis). Costs are shown only for days' supply categories with at least 1% of the prescribed patients in the RXC.

Table 3.2: Descriptive statistics for the 2017 sample (N=12,227,124)

Variable	Share of enrollees (%)	Mean spending ($)
All	100	5,741
Share of:		
Females	52.6	6,354
21-29	17.6	3,063
30-39	20.3	4,142
40-49	23.6	5,199
50-65	38.5	8,139
Any RXC	5.9	25,585
RXC-HCC 1	0.27	34,480
RXC1 Prescribed Patients	0.23	38,624
RXC-HCC 2	0.27	47,056
RXC2 Prescribed Patients	0.03	92,208
RXC-HCC 3	1.30	30,387
RXC3 Prescribed Patients	0.36	39,373
RXC-HCC 4	0.24	76,717
RXC4 Prescribed Patients	0.07	126,120
RXC-HCC 5	0.71	29,790
RXC5 Prescribed Patients	0.35	27,628
RXC-HCC 6	7.33	15,770
RXC6 Prescribed Patients	1.59	27,012
RXC-HCC 7	8.31	16,628
RXC7 Prescribed Patients	3.20	16,387
RXC-HCC 8	0.27	57,269
RXC8 Prescribed Patients	0.15	77,545
RXC-HCC 9	2.28	28,260
RXC9 Prescribed Patients	0.92	39,281
RXC-HCC 10	0.02	122,968
RXC10 Prescribed Patients	0.01	146,288

This table presents summary statistics of the analytic sample used in the paper.

patients with 28-days multiples.

I use the 2019 HHS-HCC risk adjustment methodology, implemented in CMS' *HHS-HCC*

V0519 software,[10] to calculate the risk scores, and thus the risk adjustment payment for each person. The V0519 methodology is used to decide which adjustors should be "turned on" for each enrollee. To calculate the risk scores for each enrollee the adjustors' vector is multiplied by their corresponding coefficients, which I re-estimate in each step (see details in the next section). While CMS adjusts the risk adjustment coefficients post-estimation for clinical reasonableness and to decrease gaming, I apply no restrictions on the estimated coefficients.

3.4 Fit Under Alternative Utilization Thresholds

3.4.1 Simulation of days' supply thresholds

To examine the effect of days' supply utilization thresholds on the model's fit I use multiple simulations. I first calculate the actual total cost of each enrollee, and find the fit for the baseline scenario, where there is no threshold (i.e. the implicit zero-days threshold applies). In addition to that, for each drug group (RXC) I simulate twelve thresholds - all the 30-days multiples between 30 and 360 days. Each simulation of a single threshold includes the following five steps:

1. **Drop claims below the threshold** - Drop all the claims for drugs whose annual number of days' supply for the enrollee is lower than the threshold. This step is done for all the data years.[11]

2. **Turn on risk adjustors** - Use the CMS software to turn on the risk adjustors for the calibration years (2015 and 2016), based on the revised data.[12]

[10] The 2019 HHS Risk adjustment software can be downloaded here: https://www.cms.gov/CCIIO/Resources/Regulations-and-Guidance/

[11] The number of days' supply is summed up at the single-drug level (NDC-by-NDC) and not at the RXC level (by drug group). e.g. two 90-days prescriptions for different drugs in the same RXC do not sum up to cross a 180-days threshold.

[12] The software is used as a "black box", that can be replaced, potentially, by software from different years or different risk adjustment systems. This has a computational efficiency cost as non-changing diagnosis-based adjustors are turned on in each and every simulation.

3. **Reestimate model coefficients** - Reestimate the risk adjustment model on the enrollees' revised risk adjustors (from the previous step) for each of the years 2015 and 2016. The dependent variable in these estimations is the *actual* total cost of the enrollee in the examined year.[13] Average the coefficients from the two estimations to get the payment coefficients for the modified 2017 risk adjustment model.

4. **Recalculate risk scores** - Use the CMS software to get the risk scores for the 2017 enrollees, using the revised data for this year (from step 1), and the coefficients calculated in step 3. Calculate the payment (i.e. the predicted cost) for each enrollee.

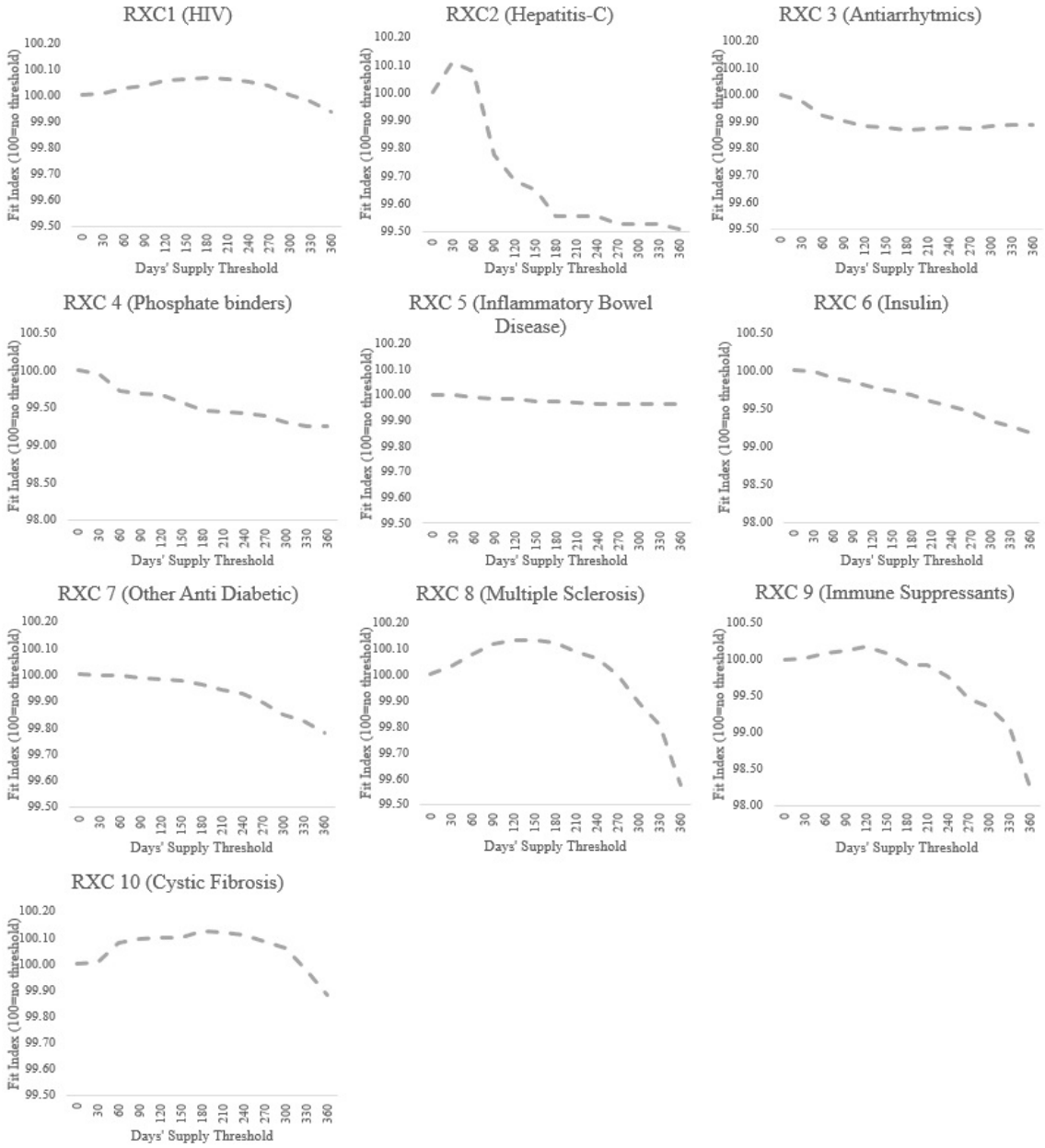
5. **Calculate fit** - Calculate the fit measures, comparing the *actual* costs from the baseline 2017 data with the payments from the previous step.

Risk adjustment models are essentially prediction models, and a fit statistic measures the accuracy of their prediction. The most common fit statistic is the R^2 individual-level fit. This is the R^2 of a regression of enrollees' cost on the adjustors included in the risk model. For each threshold, two individual R^2 fit measures are calculated: First, the individual fit for the entire population of enrollees. This measure is presented in Figure 3.3. Second, the individual fit for the patients in each RXC-HCC disease group. This group includes the patients with any prescription for a drug in the RXC and the patients that are diagnosed with one of the related diseases (described in Table 3.2). Figure 3.4 presents this fit measure. Both figures present the results by days' supply thresholds of each RXC. They normalize the baseline no-threshold fit to 100, and hence show the percent change in the fit, relative to the baseline.[14]

[13] I use the cost not covered by the Marketplace risk sharing scheme, that pays 60% of costs above $1 million. This cost is also used for fit calculations. "Payment system fit" is a more general fit statistic to calculate the fit including these omitted costs. See Layton et al. (2018c) for details on this measure.

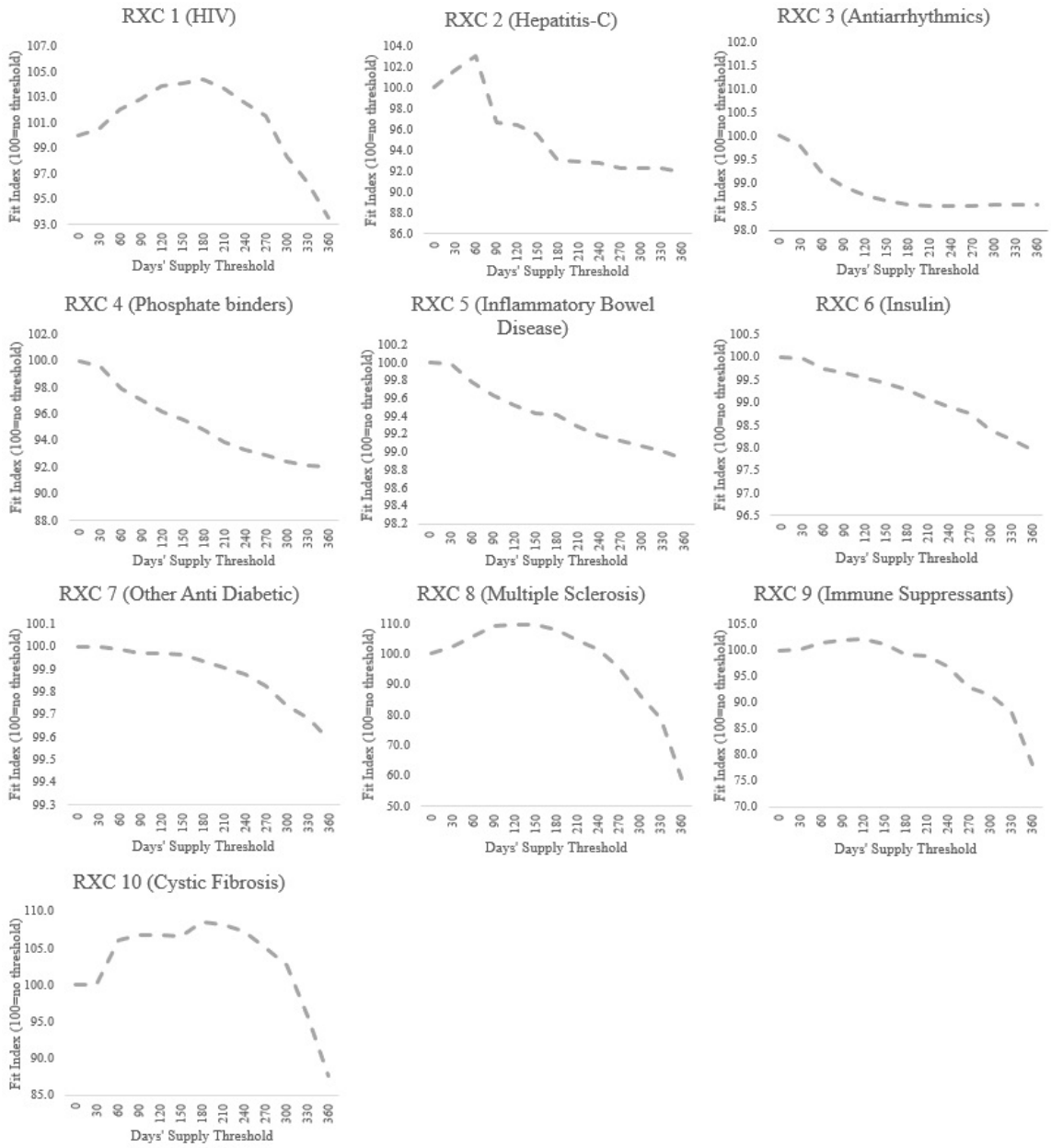
[14] The actual individual fit results for each RXC appear in Table C.3 in the Appendix. The appendix also presents an alternative fit measure - the Cumming's Prediction Measure (CPM), that does not square the prediction-cost deviations as the R^2 measure. Table C.2 in the appendix presents the model coefficients for the RXC and HCC adjustors and their interactions in each simulation.

Figure 3.3: Individual fit for all the enrollees, by days' supply threshold (0-days=100)



For each RXC drug-adjustor, the figure presents the individual R^2 fit statistic for all enrollees in the the simulated days' supply thresholds that are multiples of 30 days, between 0 (i.e. no-threshold) and 360.

Figure 3.4: Individual fit for enrollees in the RXC-HCC disease Group, by days' supply threshold (0-days=100)



For each RXC drug-adjustor, the figure presents the individual R^2 fit statistic, for enrollees in the RXC-HCC disease group, in all the simulated days' supply thresholds that are multiples of 30 days, between 0 (i.e. no-threshold) and 360. The RXC-HCC group includes patients prescribed with a drug included in the RXC or diagnosed with a related diseases (HCC).

The results show a unique impact of thresholds on the fit for each RXC. While the overall fit in the population doesn't vary a lot since the number of patients in most groups is quite small (Figure 3.3), the fit for patients within each RXC-HCC disease group may be affected in a significant way by the choice of the threshold (Figure 3.4). For five drug groups - RXC3 (Antiarrhytmics), RXC4 (Phosphate Binders), RXC5 (Inflammatory Bowel Disease), RXC6 (Insulin), and RXC7 (Other Anti Diabetic) - both fit measures are maximized at the baseline, without any explicit utilization threshold. For five other RXCs, days' supply thresholds may improve both the overall fit and the fit for the disease group related to the drug: For RXC1 (Anti HIV Agents), a utilization threshold of 180 days is optimal. It increases the overall R^2 fit by 0.07% relative to the baseline fit (of 34%) and increases the individual R^2 fit in the disease group by 4.4%; RXC2 (Anti Hepatitis-C Agents) benefits from a 60 days' supply threshold, that improves overall fit by 0.07%, and increases the fit in the disease group by 3.1%; For RXC8 (MS Agents), a 120 days' supply threshold maximizes both fit measures: overall fit increases by 0.13% and the fit for the disease group increases by 9.6%; A 120-days threshold maximizes fit also for RXC9 (Immune Suppressants) - overall fit increases by 0.17% and the fit in the disease group rises by 2.1%; For RXC10 (CF Agents), both measures of fit are maximized with a 180 days' supply threshold. With such a threshold, the overall fit increases by 0.12% and the fit for the disease group increases by 8.6%.

3.4.2 Determinants of the impact of thresholds on fit

Relative to a scenario without a threshold, setting a utilization threshold for a certain RXC may influence the predicted costs (and thus payments) for patients in the RXC-HCC disease group in several ways: First, a threshold decreases the predicted costs of patients below the threshold, as the drug adjustor (RXC), and possibly its interactions with morbidity adjustors, are no longer turned on; Second, the predicted costs of patients above the threshold change to better match their average additional cost, as the drug-adjustor coefficient is re-estimated on this smaller group of patients.

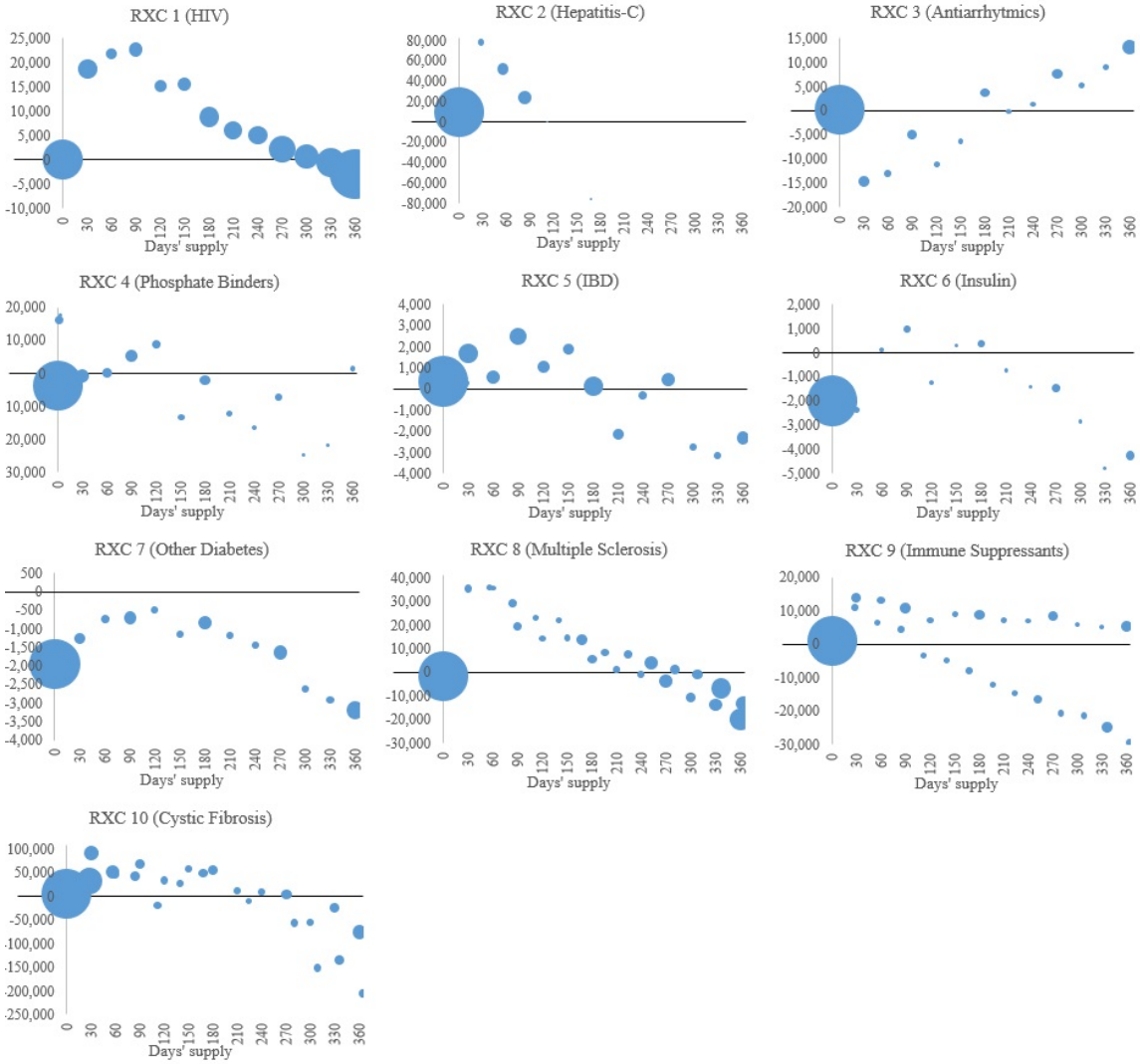
This could mean either an increase or a decrease of the payment to patients above the threshold; Third, a threshold may change the predicted costs of all diagnosed patients, with or without a prescription, as the related diagnosis-based adjustors (HCCs) are re-estimated, pooling the patients without a prescription with the diagnosed patients that have a prescription below the threshold. Theses changes in the predicted costs affect the individual fit statistic, as it is based on the difference between each individual's actual costs and the costs predicted by the risk adjustment model. The R^2 statistic applies a quadratic loss function on these differences.

It is useful to directly examine the prediction errors, i.e. the over- or under-compensation to patients. Figure 3.5 presents the differences between actual costs and predicted costs when there is no threshold. It groups patients by the annual number of days' supply in their prescriptions for drugs in the RXC. The size of the bubbles in the graph indicates the number of people in each days' supply group (normalized separately for each disease group).

In the groups related to RXCs 1,2,8,9, and 10, patients with shorter prescriptions are mostly over compensated while patients with longer prescriptions are under-compensated (or less over-compensated). This may explain why a non-zero threshold improves the fit in the disease group for these RXCs - it decreases payments for the over-compensated below the threshold, and increases payments to the under compensated above the threshold. This also confirms the basic intuition from the simple example presented in Figure 3.1 - for these RXCs, cost is an increasing function of the number of days' supply (see Figure 3.2), essentially similar to the distribution in panel A of the example. In such cases, a non-zero threshold may improve the fit.

RXC3 presents an opposite example. The cost for patients in the RXC-HCC disease group decreases as a function of the number of days' supply, basically similar to the distribution in panel B of Figure 3.1. As a result of the decreasing cost, patients with shorter prescriptions are under-compensated, while patients with longer prescriptions are over compensated. In such cases, a non-zero threshold will make things worse for the under-compensated below the threshold, further low-

Figure 3.5: Over/Under-compensation for patients in the RXC-HCC group when there is no threshold, by days' supply



For each disease group, the figure presents the average over- or under-compensation for patients, by the annual number of days' supply in their prescriptions for the RXC's drugs. It shows the compensation gap in the baseline scenario of no threshold, examining patients with 0 days' supply (i.e. diagnosed only) up to 365 days' supply. The dollar gap in compensation is calculated by the difference between the average payment to the patients in each days' supply subgroup, and the average baseline costs (which are the actual costs minus the costs covered by the reinsurance program). The size of the bubble around every point indicates the number of patients in each subgroup (normalized separately for each graph).

ering their predicted costs. For the patients above the threshold, the payment will be reduced to better match their actual costs. The total effect on the fit depends on the relative amounts of under and over compensation in each days' supply group, weighted by a quadratic loss function. In RXC3 any non-zero threshold leads to a worse fit.

3.5 Incentives for Gaming

A major concern when designing risk adjustment systems and choosing adjustors is that they should not allow plans and providers to readily manipulate them to increase plan payments (Ellis et al. (2018)). Unlike age or gender, adjustors based on the use of prescription drugs are susceptible to gaming. When adding drug adjustors to the Marketplace risk-adjustment model, CMS acknowledged that it "may provide an incentive to overprescribe medications" (CMS (2016b)). This concern has led CMS to exclude some drug groups from the model because medical professionals judged that they are "particularly subject to intentional or unintentional discretionary prescribing variation or inappropriate prescribing by health plans or providers." Fearing inappropriate prescribing when an inexpensive drug treats a medically expensive condition, CMS also restricted the payment for two of the RXCs included in the model - RXC3 (Antiarrhythmics) and RXC4 (Phosphate Binders) - to less than the average cost of supplying the drugs (CMS (2017)).[15] Gaming of drug-adjustors is a concern also in other risk adjustment systems. Lamers and van Vliet (2003) propose several measures to decrease the opportunities for gaming the drug adjustors in the Dutch risk-adjustment scheme - setting days' supply thresholds, preventing payments due to multiple chronic conditions by assigning only the most expensive drug-adjustor, reducing the compensation or restricting it, and avoiding drug adjustors that lead only to a small increase in predicted costs.

[15] Payment due to these RXC adjustors was a priori set to be equal to the average annual per capita cost of the drugs in the RXC (in the calibration dataset). In addition to that, the RXC-HCC interaction term was set to zero for both RXCs.

3.5.1 Measuring the incentives for gaming

There is no consensus about how to measure incentives for gaming in risk adjustment systems.[16] One indirect measure appears in Lamers et al. (1999) that examines the proposed Pharmacy-based Cost Group (PCG) model in the Netherlands. They calculate the ratio between the capitation payment for those assigned to one of the PCGs and their pharmacy costs. They find that revenues are on average about four times as high as the cost of drugs. While this ratio may suggest that there is some room for insurers to increase revenues by prescribing more drugs, it doesn't quantify the gaming incentives directly. My approach is similar to the way Behrend et al. (2007) examine the gaming opportunities of the then-proposed drug adjustors in the German risk adjustment system (IPHCC+RxGroups). The authors simulate three specific cases of gaming behaviors by health plans and calculate their net monetary returns per insured person: substitution to an alternative drug for hypertension that leads to a higher risk score[17]; increasing the prevalence of antidepressants use among patients already diagnosed with depression[18]; and increasing the use of diabetes drugs by diagnosing previously unidentified diabetic patients and supplying them with very short prescriptions.[19]

Before turning to the measures used here, I note that I quantify the *incentives* to prescribe more

[16] The literature is wider on the incentives for cost saving, that are related to incentives for gaming as both may depend on the effect of current plan's spending on its future revenue. To quantify incentives for cost-saving, Geruso and McGuire (2016) use the "power" concept to measure the share of costs borne by the plan at the margin. However, the power measure seems insufficient to measure incentives for gaming. Some gaming activities may entail no further utilization or cost at the enrollee level. Even when gaming requires additional utilization, e.g. prescribing additional days' supply, the resulting increase in revenue could be much higher than the additional cost, leading to a negative "power" measure - below the usual 0 to 1 range of this statistic.

[17] The simulation moves all the patients prescribed with ACE inhibitors to Angiotensin II receptor blockers - an alternative drug that leads to a higher risk score.

[18] To simulate the change, the authors randomly assign antidepressant drugs to patients diagnosed with depression, so the prevalence of the drug use increases by 30%.

[19] This simulation examines an increase of 4 percentage points in the prevalence of diabetes treatment among the relevant age groups - a 33% increase of the baseline prevalence.

and not the actual response of the plans.[20] The question of how plans respond to the incentives is not addressed in this paper. The reluctance of providers to prescribe more and the cost for patients to purchase more drugs may inhibit gaming. However, as plans can incentivize providers, change formularies and set non-linear price schedules for patients, strong incentives to prescribe more may still distort prescribing and purchasing behaviors in practice.

To define measures for the incentives for gaming one has to choose first the relevant population, for which gaming is examined. This choice creates a tradeoff – widening the population may enable a more comprehensive examination of incentives, but in most cases these incentives will be less and less actionable. For example, a plan may have a theoretical incentive to prescribe *everyone* with a drug if the resulting payment is higher than the drug's cost. However, it will be very hard for a plan to act on such an incentive - make providers prescribe unnecessary drugs to healthy individuals, and convince individuals to fill these prescriptions. In contrast to that, it will be most likely much easier for a plan to make providers and patients lengthen justified prescriptions for patients that already use a drug. A similar tradeoff exists when choosing the gaming activity that the measure examines - limiting the scope of the gaming activity will most likely make the incentives more actionable. For example, an incentive for a gaming activity that includes prescribing additional 180 days of supply is likely less actionable than an incentive to game the system by prescribing one more day of supply (regardless of the cost of the drug).

With these tradeoffs in mind, the measures I define focus either on the population of patients with an existing prescription for a drug in the RXC, or on the RXC-HCC disease groups that include patients with a prescription and also patients with a related diagnosis. Regarding the scope of the gaming activity, I examine the case with no limits on the number of additional days of supply when gaming a prescription. I also examine measures that limit the gaming activity to prescribing

[20] I avoid using the term "over-prescribing", which may hint that the baseline prescription behavior is optimal. Incentives to prescribe more may lead to optimal utilization if plans would have skimped on drugs without them.

no more than 30 days of additional supply. To quantify the extent of the gaming incentives I use the potential net revenue to the plan from the gaming activity. Table C.4 in the Appendix also presents the share of enrollees that are "gameable", i.e. the enrollees for whom the gaming activity yields a profit to the plan.

To assess the incentives under each utilization threshold, I calculate for each patient the additional revenue the plan would receive if the patient crosses the threshold.[21] I also calculate the minimum cost of the additional days of supply required for the crossing.[22] [23] The direct cost used in the calculation is most likely just a part of the total cost of gaming, that may include overcoming providers' reservations due to professional ethics and intrinsic concern for their patients. Such costs may become prohibitively high, especially when gaming is extensive or is outside of any gray area around the proper prescription behaviour. In that sense, the incentive measures defined here may serve as an upper bound for the true incentives for gaming.

Measure A: Net revenue from unlimited gaming of the disease group

The first measure examines the potential revenue from gaming the RXC-HCC disease group, i.e. the patients that are either prescribed with a drug in the RXC or diagnosed with a related disease. To make *all* these patients cross the utilization threshold plans may prescribe an unlimited number of additional days of supply. I measure the net revenue to the plan from this gaming activity, per

[21] This calculation uses the coefficients of the risk adjustors re-estimated in each simulation.

[22] I assume that an additional supply that allows the patient to *just* cross the threshold is possible. In practice, the cost of gaming could be higher if prescriptions must be rendered in multiples of certain days' supply due to availability of specific dosages. Costs could also be higher if adding days of supply requires an additional service from a provider (e.g. an office visit). The costs of gaming could be lower if plans shift patients from one drug, not included in the RXC, to another one that is included in the RXC. I ignore all such options in the calculations.

[23] For each patient with an existing RXC prescription, the cost of an additional day of supply is the average of the daily cost in her own prescriptions. For diagnosed patients with no prescription, the cost of an additional day is the average daily cost for all prescribed RXC patients.

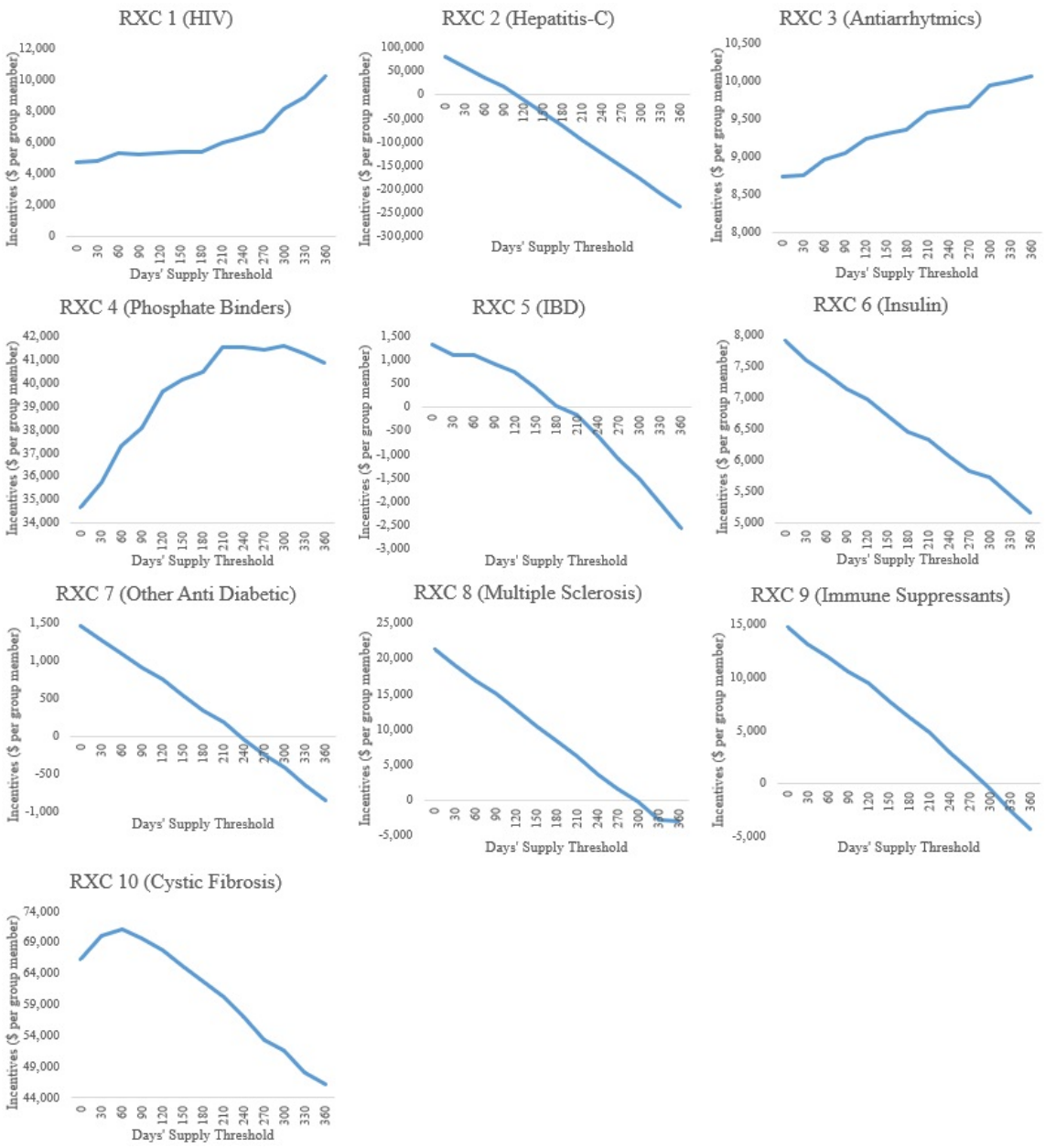
person in the disease group. The definition is:

$$MeasureA = \frac{1}{N}\sum_{i} \Delta R_i - \min_{j}(\Delta sup_{ij} * cost_j)$$
$$s.t.\ \forall i\ \exists j\ s.t.\ sup_{ij} + \Delta sup_{ij} > T \tag{3.1}$$

where N is the number of patients in the disease group, ΔR_i is the additional revenue to the plan due to pushing patient i over the threshold, sup_{ij} is the annual number of days' supply in patient i's prescriptions of drug j (included in the RXC), Δsup_{ij} is the number of additional days' supply prescribed as part of the gaming, and $cost_j$ is the daily cost of drug j. The incentive measure is calculated by using the cheapest way for patient i to cross the threshold of T days' supply.

Figure 3.6 presents measure A for each RXC, by the days' supply thresholds. The full results are in Table C.4 in the Appendix. At the baseline scenario of no-threshold, the measure essentially examines a gaming activity in which all the diagnosed patients in the disease group, without an existing RXC prescription, receive a new one-day prescription of a drug in the RXC. This gaming behaviour is always profitable, and yields the highest net revenue in RXC2 (Hepatitis-C) - $80,740 per patient in the group. In six disease groups - related to RXCs 2,5,6,7,8, and 9 - a higher threshold almost monotonously decreases the incentives for gaming. For most of these drug groups there is a threshold in the examined range that eliminates the incentive for gaming. For example, with a threshold just above 180 days' for RXC 5 (IBD), the cost of prescribing more exactly equals the revenue from this gaming. In contrast to that, in the disease groups related to RXCs 1,3, and 4 the incentive for gaming mostly *increases* with a higher threshold. This implies that the revenue from turning on the RXC adjustor to a growing number of patients increases faster than the additional cost of prescribing more. This may happen if the drug is inexpensive relative to the high cost of treating patients with long prescriptions. The result may justify CMS's choice to restrict the coefficients of RXCs 3 and 4.

Figure 3.6: Net revenue from gaming prescriptions so everyone in the disease group crosses the threshold, by days' supply threshold of each RXC



For each disease group, the figure shows the net revenue to a plan from a gaming behaviour that prescribes more of the RXCs drugs, so all the patients in the group cross a days' supply threshold. The net revenue, per member of the disease group, is shown for all thresholds that are multiples of 30-days, between 0 and 360. The net revenue of each patient is calculated by subtracting the minimal cost of the additional drugs, required to cross the threshold, from the additional revenue accrued to the plan from having the patient cross the threshold and have a higher risk score.

Net revenue from adding up to 30 additional days of supply

The next two measures limit the scope of the gaming activity examined, allowing plans to prescribe no more than 30 additional days of supply to push patients across the threshold. As these measures focus on patients at the margin of passing the threshold, a distortion of the prescription length for them is arguably easier and the incentives to do so are more actionable. The two measures separately examine such gaming for patients with an existing RXC prescription, and for diagnosed patients in the disease group with no prescription.

Measure B: Gaming patients with an existing prescription The second measure examines only patients with an existing prescription, excluding those with a related diagnosis alone.[24] The examined gaming activity allows to extend the existing prescription by no more than 30 additional days of supply. The measure calculates the average net revenue from the gaming activity, per patient in the disease group. The measure's definition:

$$\begin{aligned} &MeasureB = \frac{1}{N}\sum_i \Delta R_i - \min_j(\Delta sup_{ij} * cost_j) \\ &s.t.\ sup_{ij} + \Delta sup_{ij} > T \ \ \& \ \ \Delta sup_{ij} <= 30 \ \forall i \in \{i | sup_{ij} > 0\} \end{aligned} \tag{3.2}$$

The incentive is calculated as in equation (3.1), limiting the number of additional days' supply to 30 and changing prescriptions for a narrower group of patients. N is the number of patients in the RXC-HCC group, ΔR_i is the additional revenue to the plan due to pushing patient i over the threshold, sup_{ij} is the annual number of days' supply in patient i's prescriptions of drug j (included in the RXC), Δsup_{ij} is the number of additional days' supply prescribed as part of the gaming, and $cost_j$ is the daily cost of drug j. The incentive measure is calculated by using the cheapest way for

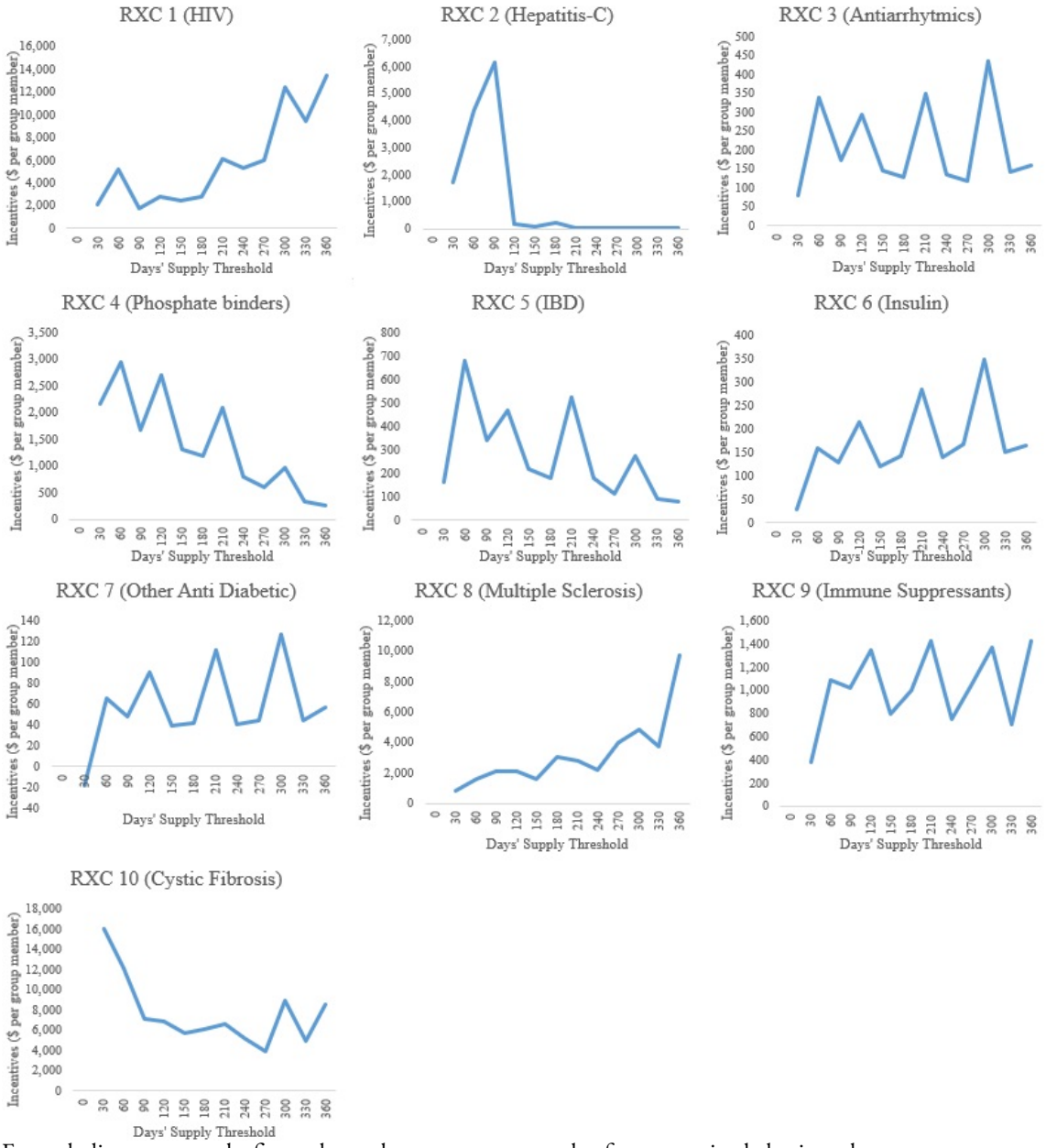
[24] This measure is applicable only when there is a utilization threshold. When there is no threshold, the RXC indicator is turned on for all the patients with a prescription, making gaming for this population futile.

patient i to cross the threshold of T days' supply.

Figure 3.7 presents the measure by days' supply threshold for each RXC. The full results are shown in table C.4 in the Appendix. The magnitude of this measure is naturally smaller than the first one, as it focuses on gaming the prescriptions for a smaller group of patients. The measure is affected by the distribution of patients by days' supply and their costs, shown in figure 3.2. For example, a large share of the patients treated for HIV (RXC1) fill prescriptions with 270 days' supply or more, making the gaming incentive higher when the threshold is above 270 days. For RXC1, a threshold of 90 days' supply creates the lowest incentive to prescribe up to 30 days' supply more to push marginal patients across the threshold. With such a threshold, the gaming yields a net revenue of $1,773 per patient in the HIV disease group. For none of the RXCs the incentives are monotonous, emphasizing the need for an empirical analysis to identify the effect of a threshold on these incentives.

Measure C: Gaming diagnosed patients with no prescription The third measure examines gaming of diagnosed patients in the disease group that have no prescription for one of the drugs in the RXC. Prescribing up to 30 additional days of supply for these patients is a possible gaming activity when there is no threshold, or the threshold is lower than 31 days of supply. The definition of this measure is similar to the definition of measure B (Equation 3.2), changing the group of patients susceptible to gaming to patients with no RXC prescription (i.e. $sup_{ij} = 0$). Table 3.3 presents the net revenue from the gaming activity when there is no threshold and when there is a 30-days threshold. In all RXCs, a threshold of 30 days' supply lowers the gaming incentive relative to the no-threshold scenario, by 0.5% to 30%.

Figure 3.7: Net revenue from prescribing up to 30 days more for patients with an existing prescription, by days' supply threshold



For each disease group, the figure shows the net revenue to a plan from a gaming behaviour that prescribes up to 30 additional days' supply of the RXC's drugs to push patients with an existing prescription across a days' supply threshold. The net revenue, per member of the disease group, is shown for all thresholds that are multiples of 30-days, between 0 and 360. The net revenue of each patient is calculated by subtracting the minimal cost of the additional drugs, required to cross the threshold, from the additional revenue accrued to the plan from having the patient cross the threshold and have a higher risk score.

Table 3.3: Net revenue from prescribing up to 30 days more for diagnosed patients with no RXC prescription, dollars per patient in the disease group

	RXC									
	1	2	3	4	5	6	7	8	9	10
# patients in disease group	33,623	33,386	158,635	114,753	87,017	896,307	916,779	32,879	278,230	2,332
Share w.o. prescription (%)	16	87	72	69	51	78	62	43	60	47
Net revenue ($):										
No threshold	29,048	92,531	12,085	50,338	2,578	10,089	2,385	48,888	24,646	142,163
30-days threshold	27,359	65,164	12,029	49,697	1,961	9,670	2,094	42,848	21,697	134,197
Change in net revenue with a 30-days threshold (%)	-5.8	-29.6	-0.5	-1.3	-23.9	-4.2	-12.2	-12.4	-12.0	-5.6

The table presents plans' potential net revenue from giving new RXC prescriptions to patients that are diagnosed with a disease related to the RXC but have no RXC prescription. The net revenue is the revenue to the plan from turning on patients' RXC adjustor, minus the cost of the additional drugs supplied. The measure presented is the average net revenue per member of the disease group (that includes both diagnosed and prescribed patients).

3.6 Discussion

3.6.1 The lack of a tradeoff between fit and the incentives for gaming

When implementing the new drugs-diagnoses risk adjustment system, CMS declared that it is seeking to "strike a reasonable balance between increasing predictive accuracy and reducing incentives for overprescription" (CMS (2016b)). This reflects a common belief in the existence of a tradeoff between fit and the incentives for gaming. However, this paper shows that such a tradeoff doesn't always exist. For four out of the ten RXCs, non-zero thresholds can both improve the fit and reduce the incentives for gaming the prescription behavior: A 60-days threshold for RXC2 (Anti Hepatitis-C agents) improves fit in the disease group by 3.1%. It also reduces by 54% the net revenue to the plan from gaming the prescription behavior so all patients cross the threshold (measure A) - from $80,740 to $37,015 per patient in the disease group; For RXC8 (MS Agents), a 120 days' supply threshold increases the disease group's individual fit by 9.6%. The net revenue from gaming under such threshold is $12,808 per member of the group, 40% lower than the incentive with no threshold; In RXC9 (Immune suppressants), a 120-days threshold increases the individual fit in the disease

group by 2.1%, and decreases the gaming incentive by 36%; Lastly, a 180-days threshold for RXC10 (Cystic Fibrosis) allows to improve the fit in the disease group (by 8.6%), while decreasing the incentive for gaming by 5%. The lack of tradeoff between fit and incentives for gaming is apparent also in cases where both measures become worse with a threshold. For example, a 180-days threshold for RXC3 (Antiarrhytmics) decreases the overall fit by 0.13% and hurts the fit in the disease group by 1.4%, while increasing the incentive for gaming by 15%.

3.6.2 Dynamic vs. Static Incentives for Gaming

A caveat to the incentive measures defined in this paper is that the measures are static in nature, i.e. they measure the incentives for gaming within a single year (in the concurrent payment system used at the Marketplaces). The expected incentives may be lower for later years, especially when the risk adjustment model is re-estimated using claims data from the Marketplaces themselves (data that CMS began using in 2019), and when more plans, with a larger share of relevant patients, game the system. In such a case, even if the absolute return to gaming decreases, a plan that avoids gaming alone may suffer financially, as the predicted costs of gamed RXCs will decrease. Behrend et al. (2007) show that in a prospective payment system, gaming may be lucrative to health plans as long as the share of plans gaming and the share of patients gamed are not too high.

Another dynamic aspect is the potential effect of gaming on patients' selection into plans. Gaming activity that easily provides longer prescriptions to drugs that treat a certain disease, may attract patients with the disease to the plan. Such adverse selection may change the incentives in the following years. In this paper, I abstract away from such issues.

3.7 Conclusion

I examine the impact of utilization thresholds on the performance of risk adjustment systems, specifically the model fit and the incentives for gaming. The sign and size of this impact is an empirical question that, inter alia, depends on the cost distribution of enrollees by the threshold categories. I study this question in the setting of the U.S. Marketplaces, that added prescription-drug adjustors to their risk-adjustment model in 2018, but set no thresholds on the number of days' supply in these prescriptions. I show that for some drug adjustors, there is no tradeoff between fit and incentives for gaming - a non-zero days' supply threshold can both improve the model fit, and decrease the net revenue from gaming. The empirical analysis in this paper focuses on the risk adjustment system in the Marketplaces, but can be repeated in other systems, not only for adjustors based on utilization of prescription drugs, but also for other adjustors that are based indirectly on utilization, such as morbidity-based adjustors. I leave these analyses for future research.

A

Supplementary Material for Chapter 1

A.0.1 Changes in utilization around bids' contract awards

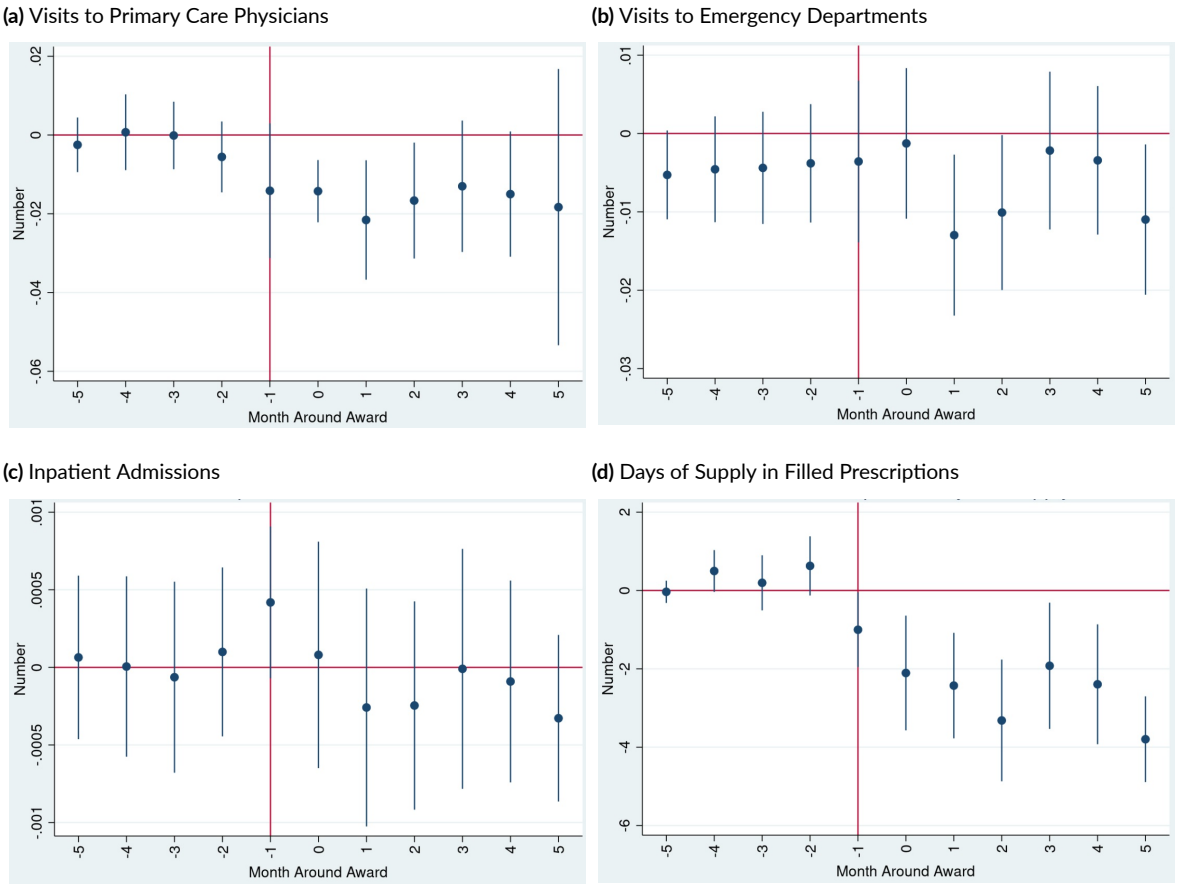
The exit of a plan from a county does not come as a surprise. Plans know they lost in a bid well before their service is due to end, and their providers and enrollees are formally notified some time after that (and might have heard about the exit in the news already). Because this information shock happens before the actual switch of enrollees to another plan, the effects of a (future) exit may man-

ifest even before the exit occurs. While the main analysis focused on the effects of the actual switch, this section studies the changes in utilization around the contracts award date - the bid milestone in which the state reveals the winners and losers in the bid. The information of a future exit may affect utilization in several ways: 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 such effects for employees with high turnover). Such sudden horizon effects may lead insurers to hurt beneficiaries' access to care, for example by employing a stricter pre-authorization process; Second, providers may leave the plan's network ahead of its exit, to form contracts with plans that will remain in the market; Third, enrollees may misunderstand the notices that inform them on their plan's future exit. In response, they may 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 effects on utilization may be the result of a decrease in plans' reporting of encounter data to the state, as reporting accurate information will no longer affect the plan's risk-adjusted income over the next year.

Figure A.1 presents event studies examining the utilization of beneficiaries in exiting plans around the contracts award milestone, comparing to beneficiaries in non-exiting plans, and relative to six months before the contracts are awarded. The included period ends before the actual exit (the exit occurs 4 to 7 months after the awards in the sample). DID estimates for the pooled effect during the award-to-exit period are presented in Table A.1. The results suggest that over the months after contracts are awarded, enrollees in plans that are about to exit the market experience a 9% decrease in the number of monthly primary care visits (20 fewer visits per 1,000 beneficiaries), and 7% decrease in the number of visits to emergency departments (8 fewer visits per 1,000 beneficiaries). The share of beneficiaries in exiting plans that fill any prescription drops by 5 percentage points (20% lower than the baseline mean), and the number of days' supply in filled prescriptions decreases by 2.8 days. The number of admissions to hospitals and the number of inpatient days decrease, but the change is not

significantly different than zero.

Figure A.1: Utilization of services around bids' contracts award month



Note: Figure shows event studies around the month in which states award contracts in a competitive MMC bid, i.e. when bids' losers and winners are announced (the month before the award is denoted by a vertical red line). Data points show the effect of contracts' award on beneficiaries in soon-to-exit plans, comparing to beneficiaries in remaining plans, and relative to six months before the awards milestone.

The results presumably suggest that the utilization of *all* services weakly decreases after contracts are awarded - there is no increase in the number of ED visits and hospital admissions, despite the lower use of primary care and prescription drugs. This result may support the claim that the estimated award-to-exit effects stem from a change in data reporting by exiting plans. To further

Table A.1: Monthly utilization of services in exiting plans after contracts are awarded

	(1) PCP Visits per 1,000	(2) ED Visits per 1,000	(3) Inpatient Admissions per 1,000	(4) Inpatient Days per 1,000	(5) Any Filled Prescription (%)	(6) Days Supply All Drugs
Award to Exit Period X Exiting Plan Indicator	-19.93***	-8.16**	-0.27	-1.02	-5.05***	-2.79***
	(6.51)	(3.83)	(0.21)	(1.25)	(1.43)	(0.65)
Baseline Mean	221.6	117.4	4.4	14.5	25.0	14.8
# of observations			15,169,050			
# of beneficiaries			1,166,850			
# of counties			354			

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

Note: Table shows estimates of the impact of contracts award in states' MMC bids, on beneficiaries in exiting plans, comparing to beneficiaries in remaining plans, and relative to the pre-award period. All specifications include also the non-interacted period variables, a constant, individual fixed effects, month fixed effects, and month-of-year fixed effects. Standard errors are clustered at the county level.

examine whether this is the case for the decrease in the utilization of prescription drugs, I repeat the estimation for two states in the sample that carved out their pharmacy benefits (before new contracts began) - Missouri and Texas. In these states, fee-for-service claims were generated by the state's Medicaid program when a prescription was filled. As a result, the data on utilization of prescriptions drugs is more reliable. Table A.2 presents the DID estimates for a sample that includes only these two states. After contracts are awarded, the share of beneficiaries filling any prescription in exiting plans decreases by 1.4% (0.4 percentage points lower), and the number of days' supply in filled prescriptions is lower by 2.8% (0.3 days) - much smaller decreases than in the full sample (columns 5 and 6 in Table A.1). The results suggest that data issues may explain a large part of the estimated effect in the full sample.[1] However, even when data is consistently reliable around the contracts award date, there is still evidence for a pre-exit decrease in the utilization of prescription drugs. If some

[1] An alternative reason is that carved out drug benefits in these states create much weaker incentives for plans to skimp on drugs, even when they are about to exit the market.

decrease in utilization of out-patient services and prescription drugs before the exit is indeed real, it could mean that unmet needs of switchers after the exit of their plan are even higher than indicated by the main analysis.

Table A.2: Monthly utilization of prescription drugs after contracts are awarded, in states with carved-out drug benefit (Texas, Missouri)

	(1) Any Filled Prescription (%)	(2) Days Supply All Drugs
Award to Exit Period X Exiting Plan Indicator	-0.35*	-0.33***
	(0.21)	(0.11)
Baseline Mean	24.5	11.7
# of observations	6,930,547	
# of beneficiaries	533,119	
# of counties	213	

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

Note: Table shows estimates of the impact of contracts award in Missouri's and Texas' MMC bids on the share of Medicaid beneficiaries in exiting plans that are filling any prescription, and the number of days' supply in filled prescriptions. All specification include also the non-interacted period variables, a constant, individual fixed effects, month fixed effects, and month-of-year fixed effects. Standard errors are clustered at the county level.

A.0.2 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:

$$isSwitcher_i = \beta_0 + \beta_1 Treat_i + \gamma_i + \varepsilon_i \tag{A.1}$$

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:

$$\begin{aligned} Y_{ist} =& \theta_0 + \sum_{l=0}^{3} \theta_l 1\{Q_t - QExit_s = l\} + \sum_{l=0}^{3} \beta_l 1\{Q_t - QExit_s = l\} * \widehat{Treat_i} \\ &+ \gamma_i + \delta_t + month_t + \psi_{it} \end{aligned} \tag{A.2}$$

where $\widehat{Treat_i}$ is the predicted value from equation A.1 and ψ_{it} is a random error. θ_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 A.3 presents the IV estimates for the utilization variables whose reduced form estimates appear in Tables 1.4 and 1.5: Numbers of PCP visits, ED visits, inpatient admissions, inpatient days per 1,000 beneficiaries, and the share of beneficiaries with any filled prescription. As expected, the IV estimates are very similar to the reduced form estimates.

Table A.3: The effects of switching on monthly utilization of services - IV

	(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	Inpatient Days per 1,000	Any Filled Prescription (%)
Post-switch Q1	-13.43***	1.17	0.06	0.43	-3.73***
	(4.38)	(1.10)	(0.27)	(1.35)	(1.19)
Post-switch Q2	-0.35	3.01**	0.14	-0.05	-3.62***
	(0.74)	(1.33)	(0.21)	(1.19)	(1.27)
Post-switch Q3	-14.74*	-1.87	0.27	2.94**	-3.83***
	(7.66)	(4.46)	(0.20)	(1.35)	(1.16)
Post-switch Q4	-16.33***	-0.92	0.49**	1.87*	-3.87***
	(5.89)	(1.19)	(0.24)	(0.98)	(0.98)
Baseline Mean	202.0	61.63	4.3	13.8	25.2
# of observations			20,507,366		
# of beneficiaries			1,166,430		
# of counties			354		

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

Note: Table presents the IV estimates of the impact of switching from one MMC plan to another at the time new contracts begin in a state after MMC bids. Plan exits serve as instrumental variable for switching. The equation are estimated using 2SLS. All specifications include also the non-interacted period variables, a constant, individual fixed effects, month fixed effects, and month-of-year fixed effects. Standard errors are clustered at the county level.

A.0.3 Other appendix tables

Table A.4: The effects of an exit-induced switch on utilization of out patient services for children and adults

Periods X Exit-Switcher Indicator	PCP Visits per 1,000		Any Filled Prescription (%)	
	Children	Adults	Children	Adults
Post Q1	-11.45***	-20.25***	-2.98***	-5.89***
	(4.16)	(6.94)	(1.06)	(1.55)
Post Q2	1.26	-11.22	-2.74**	-6.10***
	(6.78)	(12.35)	(1.19)	(1.55)
Post Q3	-15.00**	-14.55	-2.94***	-6.51***
	(6.68)	(11.31)	(1.07)	(1.50)
Post Q4	-14.31**	-27.17**	-2.84***	-7.30***
	(5.53)	(11.02)	(0.88)	(1.52)
Baseline Mean	165.7	305.0	18.6	47.2

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

Note: Table presents the estimates of the effect of an exit-induced plan switching on utilization. Two treatment groups are examined: children under 20 years old , and adults above 20 (ages measured at the month before the exit). All specifications include also a constant, individual fixed effects, month fixed effects, and month-of-year fixed effects. Standard errors are clustered at the county level.

Table A.5: The effects of an exit-induced switch on utilization of services for pre-exit heavy users and non-heavy users

Periods X Exit-Switcher Indicator	PCP Visits per 1,000		ED Visits per 1,000		IP Admissions per 1,000		Any Filled Prescription (%)	
	Heavy	Non-heavy	Heavy	Non-heavy	Heavy	Non-heavy	Heavy	Non-heavy
Post Q1	-8.61	-9.99***	1.88	1.59	0.53	0.05	-6.32***	-2.19***
	(7.61)	(3.11)	(2.68)	(0.75)	(0.69)	(0.13)	(2.39)	(0.80)
Post Q2	17.23	-2.87	5.88**	2.43*	0.92	0.02	-6.47***	-2.01**
	(13.41)	(5.35)	(2.51)	(1.31)	(0.61)	(0.12)	(2.45)	(0.91)
Post Q3	-4.36	-12.74**	-2.22	-0.91	0.28	0.39***	-6.90***	-2.12**
	(16.54)	(5.58)	(3.02)	(1.20)	(0.53)	(0.14)	(2.21)	(0.85)
Post Q4	-8.87	-13.21***	-0.69	-0.18	1.65***	0.23*	-6.87***	-2.12***
	(12.67)	(4.28)	(2.93)	(0.94)	(0.61)	(0.13)	(1.77)	(0.69)
Baseline Mean	473.6	101.9	126.5	36.8	11.4	1.92	67.4	10.3

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

Note: Table presents the estimates of the effect of an exit-induced plan switching on utilization. Two treatment groups are examined: "heavy-users", defined as beneficiaries that had some utilization of medical services in the pre-exit year before contracts are awarded (24% of beneficiaries), and "non-heavy users" which are the rest of beneficiaries. All specifications include also a constant, individual fixed effects, month fixed effects, and month-of-year fixed effects. Standard errors are clustered at the county level.

Table A.6: The effects of an exit-induced switch on the share of known providers and drugs for switchers to a plan with a higher vs. lower observational effect on utilization

Periods X Exit-Switcher Indicator	Outpatient Providers		RX Providers (Pharmacies)		Drugs by NDC	
	Up	Down	Up	Down	Up	Down
Post-switch H1	-22.50***	-18.27*	-9.17*	-8.35***	-2.39*	-3.45***
	(7.35)	(10.66)	(5.36)	(2.55)	(1.43)	(0.69)
Post-switch H2	-17.00**	-15.03	-8.27*	-6.74**	-0.74	-4.02***
	(6.90)	(10.23)	(5.26)	(2.88)	(0.93)	(0.64)
Baseline Mean (%)	73.2	63.4	80.8	84.4	49.7	55.1

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

Note: Table presents the estimates of the correlation between exit-induced plan switching and the shares of known providers and known prescription drugs, among users. Two treatment groups are examined: First, switchers that switch to a plan with a higher (observational) effect on any utilization ("Up" columns); second, switchers that switch to a plan with a lower effect ("Down" columns). These plan effects are measured for all pre-exit plans, using utilization in the first five months of the pre-exit year (i.e. before contracts are awarded in the bid). All specifications include also a constant, individual fixed effects, month fixed effects, and month-of-year fixed effects. Standard errors are clustered at the county level.

Table A.7: The correlations of an exit-induced switch with prior-authorization services for switchers to a plan with a higher vs. lower (observational) effect on utilization

Periods interacted w. Exit-Switcher Indicator	PA Services per 1,000	
	Up	Down
Post-switch Q1	-0.34	-0.70**
	(0.77)	(0.33)
Post-switch Q2	0.94*	0.08
	(0.50)	(0.47)
Post-switch Q3	0.02	-0.18
	(0.76	(0.49)
Post-switch Q4	-0.70	-0.44*
	(0.49)	(0.24)
Baseline Mean	3.3	4.0
# of observations	18,044,097	19,294,969
# of beneficiaries	1,024,331	1,095,311
# of counties	353	353

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

Note: Table presents the DID estimates (reduced-form) of the impact of a plan exit on the number of out-patient services that may require prior authorization provided to the beneficiaries that are forced to switch out of their plan. All specifications include also the non-interacted period variables, a constant, individual fixed effects, month fixed effects, and month-of-year fixed effects. Standard errors are clustered at the county level. The coefficients are from estimating Equation (1.2).

B

Supplementary Material for Chapter 2

B.1 Appendix Tables

Table B.1: Contiguous counties: The effect of a managed care enrollment mandate (reduced form)

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

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

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

Table B.2: Heterogeneity: Separately examining treatment counties with a 2007 mandate, and with a post-2007 mandate.

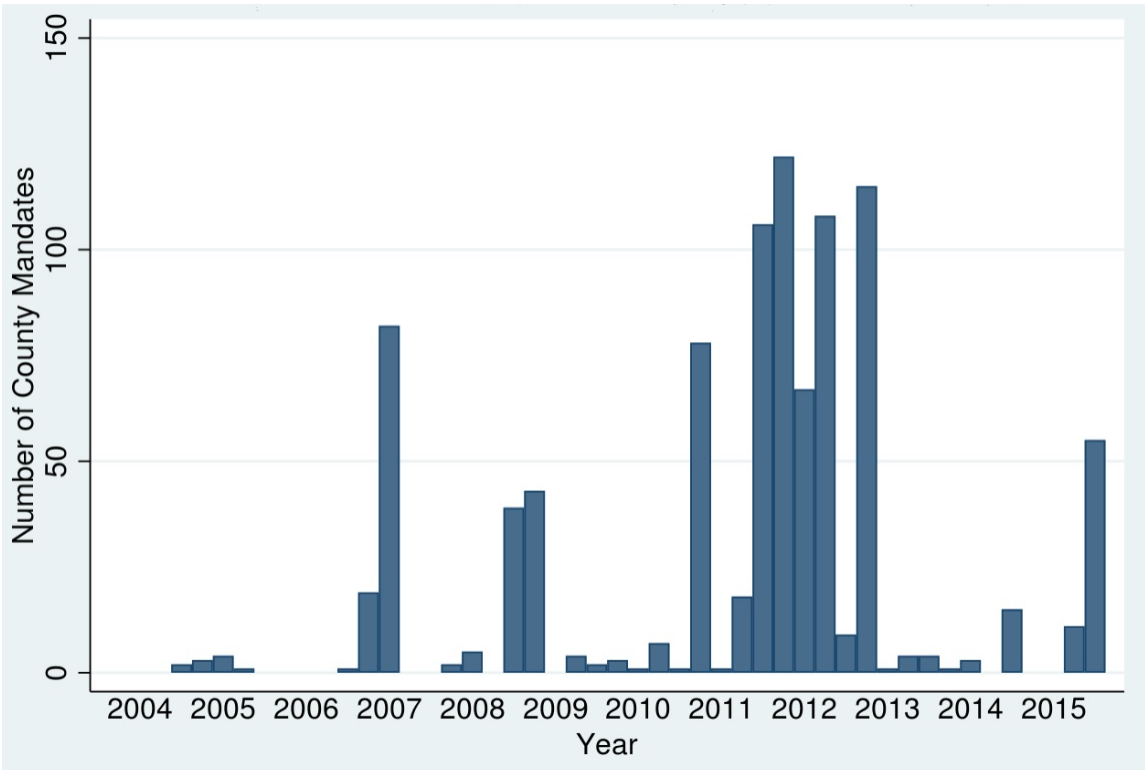
	(1) 2007 Mandates			(2) Post-2007 Mandates		
	Reduced Form	IV (First Year)	IV (After First Year)	Reduced Form	IV (First Year)	IV (After First Year)
First year	-17.63			15.30		
	(11.83)			(14.55)		
After the first year	122.54***			98.56***		
	(24.70)			(28.26)		
In a MMC Plan		-78.39***	256.87***		9.13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

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

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

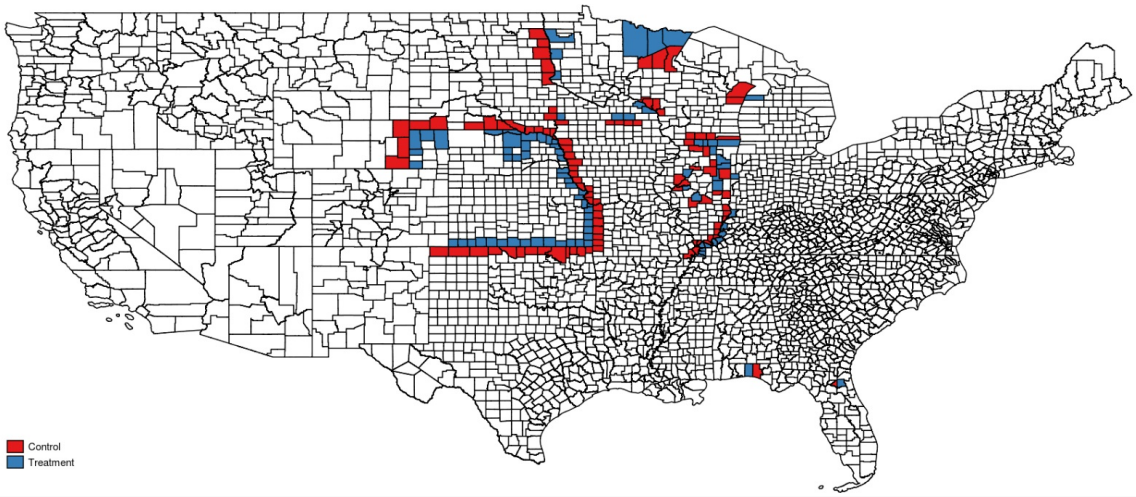
B.2 Appendix Figures

Figure B.1: The number of counties with a MMC mandate, by quarter of mandate



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

Figure B.2: Map of contiguous treatment and control counties



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

C

Supplementary Material for Chapter 3

C.0.1 Utilization thresholds for non-drug adjustors

Utilization thresholds may apply not only to adjustors based directly on utilization, like the consumption of prescription drugs, but also to any adjustor that is related to utilization indirectly. An example for the latter are morbidity-based adjustors. They are based on data that appears in claims, and thus depend on the utilization of services (Geruso and McGuire (2016)). In this section I ex-

amine two examples of utilization thresholds for diagnosis-based adjustors to demonstrate how the impact of these thresholds is again an empirical question - it may either decrease or increase the fit of the model. The examined thresholds are applied to the number of times a diagnosis appears in a patient's claims, regardless of the setting in which the diagnosis is recorded (in-patient or out-patient).[1] When counting the number of appearances, a diagnosis is counted at most once per hospital admission. Figure C.1 examines the effect of simulated thresholds for two adjustors in the marketplaces' risk adjustment model: To the left, CC19 that indicates "Diabetes with acute complications", and on the right, CC21, that indicates "Diabetes without complications".

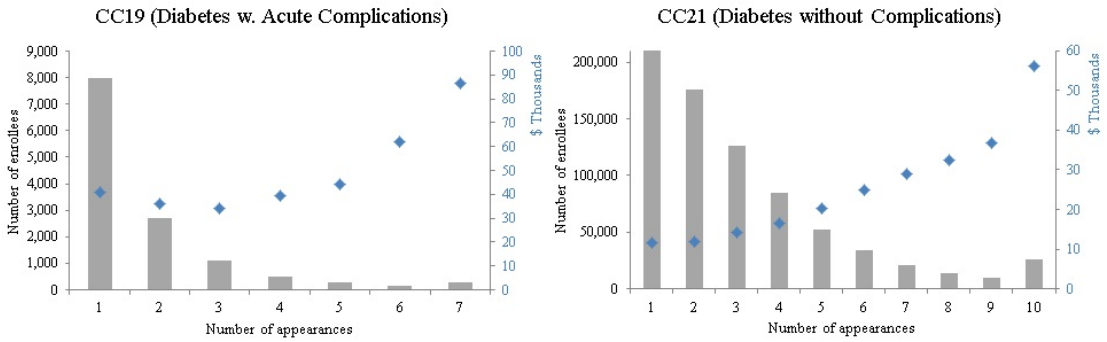
Panel A in the figure presents the distribution of patients for which the CC adjustor is turned on, by the number of times that CC diagnoses appear in their claims[2] (bars). It also presents the average annual cost of patients by the number of appearances (dots). Most patients with a CC19 diagnosis have only one claim that denotes it, and for 45% of them it is an in-patient claim from an hospital admission. Average cost are higher for patients with one appearance (40,828$) than for patients with two (35,891$) or three appearances (34,092$). In contrast to that, most patients with a CC21 diagnosis (a much larger group) have more than one appearance of these diagnoses, and almost all of these diagnoses are recorded in an out-patient setting. The annual cost of patients with one appearance is 11,740$ and cost monotonously increases as patients have a higher number of appearances. Panel B presents the individual fit for the group of patients with a diagnosis included in the CC group, by appearances thresholds. It present the R^2 measure (solid line) and the Cumming's Prediction Measure (dashed line), with the fit at the baseline scenario - a single diagnosis threshold - indexed to 100. The baseline scenario provides the best fit for CC19. For CC21 a 2-appearances threshold improves the R^2 fit (and a 4-appearances threshold maximize the CPM). Panel C presents

[1] Alternative thresholds may be applied only to out-patient diagnoses, examine whether the diagnoses were recorded in separate quarters of the years, require a minimum cost of a claim to allow the diagnosis included in it to be used for calculating risk scores, etc.

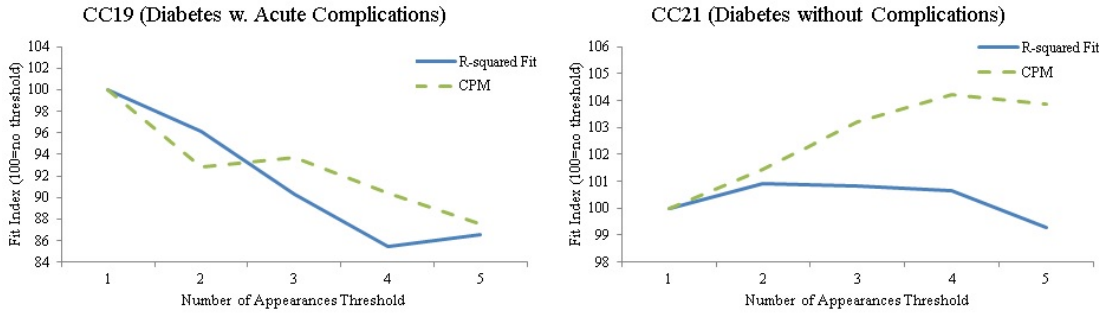
[2] If a patient has several diagnoses that are included in the same CC, the diagnosis with the highest number of appearances is used.

Figure C.1: Appearances thresholds for diagnoses-group adjustors: Distribution of enrollees, fit, and incentives for gaming

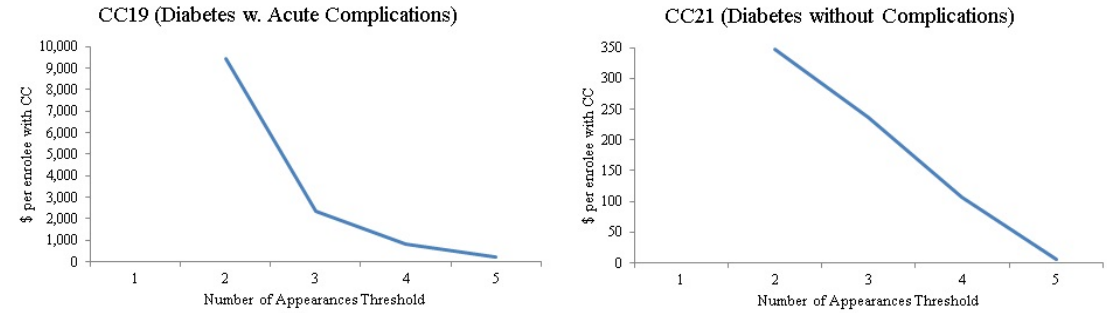
(a) The distribution of patients with a CC diagnosis and their average annual cost, by the number of annual appearances of the diagnosis



(b) Individual fit for patients with CC diagnosis, by appearances threshold



(c) Net revenue from gaming CC adjustors by adding at most a single appearance of a diagnosis to patients with an existing CC diagnosis, by appearances threshold



Panel A presents the distribution and costs of patients, by the number of times that a diagnosis included in the Condition Category (CC) appears in their annual claims. Bars show the number of patients in each number-of-appearances category (left axis). The dots show the average cost of patients in each category (right axis). Panel B show the calculated individual fit for patients with a diagnosis included in the CC, by the number-of-appearances threshold. It presents both the R^2 fit statistic (solid line) and the Cumming's Prediction Measure (dashed line). Panel C presents the potential net revenue to plans from gaming the number of times a diagnosis appears in a patient's claims. The target population for gaming includes patients with an existing CC diagnosis, and the gaming activity adds at most a single appearance, by inducing an additional out-patient claim that will record the diagnosis. Revenue is measured as dollars per enrollee with an existing CC diagnosis.

the net revenue from a gaming activity that adds at most a single additional appearance of a CC diagnosis to patients that are already diagnosed (a measure equivalent to Measure B for prescription drugs adjustors). The net revenue for each gamed patients is the revenue due to crossing the threshold and turning the CC on,[3] minus the cost of gaming, defined here as the cost of an additional out-patient visit in which the diagnosis is coded.[4] This kind of incentive for gaming decreases monotonously with higher thresholds for both CCs.

To conclude, a utilization threshold that requires more than one appearance of a diagnosis to turn on a CC-adjustor may increase fit for some adjustors, and decrease it for others. Higher thresholds may be more beneficial for CCs in which a large number of patients have more than one appearance of the diagnosis (e.g. chronic conditions rather than acute episodes), and when patients' costs increase in the number of appearances (then patients with a higher number of appearances tend to be under-compensated). Here again, simulations can serve as a tool to estimate the effect of utilization thresholds on the fit of the model and the incentives for gaming it.

[3] Turning the CC on may yield no additional revenue if a CC higher in the hierarchy of Condition Categories is already turned on.

[4] If the patient has out-patient visits in which a CC-diagnosis appears, than their average cost is used. If the patient has only in-patient claims, then the average cost of an out-patient visit for the whole group is used.

C.0.2 Appendix tables

Table C.1: The use of information on drug prescriptions in risk adjustment systems

Country	Insurance Market (year added)	Purpose	# of groups (year)	Utilization-based thresholds
U.S.A[5]	ACA Marketplaces	Independent adjustors; Severity indicators for related diagnoses	10 drug classes (RXC) (2019)	None
U.S.A[6]	Medicaid managed care (*Medicaid Rx*[7]: CA, DC; *CDPS+Rx*: DE, NJ, MO, PA, OH, FL)	Independent adjustors	Medicaid Rx: 45 therapeutic categories; CDPS+Rx: 15 drug categories (MRX)	None

[5] CMS (2016b)

[6] Gilmer (2013)

[7] Medicaid Rx and Chronic Illness & Disability Payment System (CDPS) are two risk adjustment models that U.S. states use to pay Medicaid managed care plans.

Country	Insurance Market (year added)	Purpose	# of groups (year)	Utilization-based thresholds
Germany[8]	Social Health Insurance (2009 onwards)	Validation of some outpatient and minor inpatient diagnoses; Severity interaction with some diagnoses; Independent Insulin use	21 (2019)	**10 days** for validation of acute-recurrent disease; **42 days** for medication to be taken as needed; **183 days** for validation of chronic diseases; For some diseases, different thresholds for children under age 12
Switzerland[9]	Social Health Insurance (2020)	Independent adjustors	35 Pharmaceutical Cost Groups (PCG) (2020)	**180 Days** (DDD); **3 days** for drugs for cancer; **15 days** for drugs for "complex cancer"

[8] Wasem et al. (2018), German Federal Office for Social Security (Bundesamt für soziale Sicherung) website (https://www.bundesamtsozialesicherung.de/de/themen/risikostrukturausgleich/festlegungen/)

[9] Schmid et al. (2018), Swiss Federal Office of Public Health Website (https://www.bag.admin.ch/bag/en/home/versicherungen/krar versicherer-aufsicht/risikoausgleich.html)

Country	Insurance Market (year added)	Purpose	# of groups (year)	Utilization-based thresholds
Netherlands[10]	Social Health Insurance (2002: Somatic Care, 2008: Mental Care)	Independent adjustors	Somatic care model: 33 PCGs; Mental care model: 7 PCGs	**181 days** (DDD) for most groups; **91 days** for 15 specific groups[11], and for four more groups for under 18 years old only[12]; **3 prescriptions** for Cancer and Immunoglobulins; **None** for extremely high-cost drugs
Belgium[13]	(2017)	Independent adjustors; Severity indicators for age categories	16	**90 days** (DDD)

[10] van Kleef et al. (2018), Lamers and van Vliet (2003)

[11] 91 days threshold applies to the groups: Glaucoma, Psychosis and addiction, Diabetes type I, Crohn's disease / ulcerative colitis, HIV / AIDS, Transplants, Parkinson's disease, cystic fibrosis / pancreatic enzymes, brain / spinal cord disorders: MS, hormone-sensitive tumors, pulmonary (arterial) hypertension, growth disorders, acromegaly, chronic anticoagulation, and hypertension.

[12] 91 days threshold for under 18 years old applies for the groups: Thyroid disorders, depression, asthma, and epilepsy.

[13] Schokkaert et al. (2018)

Country	Insurance Market (year added)	Purpose	# of groups (year)	Utilization-based thresholds
Israel[14]	National Health Insurance (1994)	Validation only for the "severe illnesses" adjustors	3 (2019)	Consumed in both halves of the year
Czech Republic[15]	(2018)	Independent adjustors	25 PCGs	**181 days** (DDD) for all PCG; MoH can set per-PCG thresholds between 121 and 365
Slovakia[16]	(2012)	Independent	24 PCGs (2012)	

[14] Israel Ministry of Health (2009)

[15] Bryndová et al. (2019)

[16] Health Policy Institute Website: http://www.hpi.sk/2012/08/poistovne-s-chorlavejsim-kmenom-budu-mat-viac-penazi/

Table C.2: Risk score coefficients of RXC adjustors and their related HCC adjustors, by days' supply threshold

	Days' Supply Thresholds												
Coefficient	0	30	60	90	120	150	180	210	240	270	300	330	360
RXC1	4.27	4.50	4.91	4.98	5.13	5.19	5.28	5.41	5.44	5.50	5.64	5.76	5.88
HCC1	0.25	0.26	0.28	0.35	0.47	0.63	0.82	1.27	1.62	1.90	2.54	2.81	3.12
RXC1 X HCC	0.80	0.57	0.22	0.13	-0.03	-0.19	-0.42	-0.87	-1.19	-1.46	-2.20	-2.47	-2.79
RXC2	15.29	16.84	18.94	31.77	34.92	36.35	54.73	58.62	61.58	64.42	67.97	78.24	87.15
HCC34	9.11	9.13	9.22	9.65	9.78	10.00	10.42	10.44	10.43	10.43	10.45	10.45	10.45
HCC35	4.06	4.10	4.17	4.44	4.55	4.66	5.12	5.14	5.15	5.15	5.15	5.16	5.16
HCC36	1.67	1.75	1.83	2.25	2.39	2.50	3.07	3.09	3.11	3.11	3.11	3.12	3.13
HCC37_1	0.27	0.53	1.62	4.36	4.50	4.61	5.03	5.06	5.07	5.08	5.10	5.12	5.12
RXC2 X HCC	0.99	0.54	-0.27	-3.08	-2.25	-1.80	-6.07	-0.77	1.58	-0.59	-1.93	-5.32	-21.40
RXC3	1.98	1.75	1.27	1.09	0.91	0.78	0.71	0.63	0.57	0.55	0.47	0.46	0.42
HCC142	1.48	1.51	1.67	1.77	1.87	1.94	1.99	2.04	2.07	2.09	2.09	2.10	2.10
RXC3 X HCC	0.13	0.30	0.30	0.16	0.00	-0.17	-0.37	-0.61	-0.73	-0.91	-1.02	-1.20	-1.32
RXC4	5.06	4.79	3.28	2.98	3.02	2.95	2.37	2.18	2.07	2.03	1.63	1.51	1.68
HCC183	3.03	3.14	3.34	3.50	3.69	3.81	3.89	3.98	4.01	4.04	4.10	4.11	4.12
HCC184	11.63	11.64	12.46	13.11	13.91	14.48	14.97	15.43	15.70	15.88	16.26	16.39	16.46
RXC4 X HCC	3.71	4.17	5.43	5.29	4.92	4.46	4.48	4.66	4.55	4.66	4.13	4.14	4.43
RXC5	1.67	1.69	1.71	1.69	1.75	1.74	1.76	1.84	1.86	1.90	2.00	2.06	1.96
HCC41	31.20	31.20	31.20	31.20	31.21	31.21	31.21	31.21	31.21	31.21	31.21	31.21	31.21
HCC48	2.04	2.04	2.09	2.09	2.09	2.10	2.11	2.11	2.12	2.13	2.15	2.17	2.19
RXC5 X HCC	-1.21	-1.23	-1.30	-1.26	-1.26	-1.24	-1.25	-1.24	-1.24	-1.29	-1.34	-1.39	-1.32
RXC 6	1.51	1.50	1.40	1.41	1.46	1.48	1.52	1.61	1.62	1.65	1.75	1.81	1.89
HCC18	4.83	4.84	4.87	4.88	4.92	4.94	4.96	4.99	5.00	5.01	5.00	5.00	5.01
HCC19	1.86	1.88	2.00	2.07	2.15	2.21	2.26	2.34	2.39	2.44	2.51	2.55	2.57
HCC20	0.61	0.62	0.65	0.67	0.71	0.74	0.77	0.82	0.85	0.88	0.94	0.97	0.99
HCC21	0.37	0.37	0.38	0.39	0.41	0.42	0.43	0.45	0.46	0.47	0.48	0.49	0.50
RXC6 X HCC	0.25	0.25	0.36	0.35	0.31	0.29	0.26	0.19	0.18	0.15	0.15	0.12	0.07
RXC 7	0.53	0.52	0.52	0.52	0.55	0.56	0.57	0.61	0.61	0.61	0.67	0.68	0.66
HCC18	4.83	4.83	4.84	4.84	4.84	4.84	4.84	4.85	4.85	4.85	4.86	4.86	4.87
HCC19	1.86	1.86	1.87	1.87	1.88	1.89	1.89	1.91	1.92	1.93	1.95	1.96	1.97
HCC20	0.61	0.61	0.62	0.62	0.63	0.64	0.65	0.66	0.67	0.68	0.71	0.72	0.73
HCC21	0.37	0.37	0.38	0.38	0.38	0.39	0.40	0.41	0.41	0.42	0.44	0.45	0.46
RXC7 X HCC	-0.11	-0.10	-0.09	-0.09	-0.09	-0.09	-0.09	-0.10	-0.10	-0.11	-0.13	-0.13	-0.12
RXC8	10.02	10.19	10.47	10.74	11.00	11.17	11.48	11.74	11.91	12.18	12.47	12.72	13.40
HCC118	2.83	2.86	2.94	3.09	3.29	3.46	3.87	4.31	4.65	5.15	5.61	5.89	6.61
RXC8 X HCC	-1.47	-1.57	-1.68	-1.85	-2.07	-2.26	-2.73	-3.18	-3.58	-4.11	-4.61	-4.95	-5.70
RXC9	4.31	4.38	4.76	5.01	5.42	5.59	5.73	6.26	6.44	6.54	7.28	7.41	6.95
HCC41	31.20	31.20	31.19	31.18	31.16	31.16	31.14	31.13	31.12	31.11	31.10	31.10	31.09
HCC48	2.04	2.07	2.11	2.16	2.19	2.23	2.29	2.34	2.38	2.47	2.53	2.61	2.81
HCC56	1.67	1.70	1.72	1.79	1.87	1.94	2.07	2.16	2.25	2.44	2.55	2.67	2.96
HCC57	0.77	0.78	0.79	0.80	0.83	0.84	0.86	0.89	0.91	0.92	0.95	0.97	0.99
RXC9 X HCC41_48	2.29	2.38	2.19	2.03	1.75	1.65	1.54	1.03	0.93	0.84	0.22	0.14	0.82
RXC9 X HCC56	-0.91	-0.96	-1.16	-1.31	-1.55	-1.67	-1.81	-2.03	-2.18	-2.41	-2.73	-2.92	-3.21
RXC9 X HCC57	-3.66	-3.73	-4.08	-4.31	-4.68	-4.84	-4.97	-5.44	-5.61	-5.74	-6.28	-6.53	-6.38
RXC9 X (41/48)&(56/57)	-0.79	-1.03	-1.04	-0.94	-0.77	-0.70	-0.58	0.02	0.14	0.29	0.70	0.71	0.00
RXC10	8.22	11.01	13.63	14.97	15.33	16.65	18.57	19.38	20.46	20.79	22.41	21.65	20.42
HCC158	19.39	19.83	20.37	20.74	20.87	20.98	21.06	21.19	21.20	21.29	21.43	21.43	21.49
HCC159	1.98	2.51	3.11	3.76	4.67	5.35	6.41	7.59	8.33	9.40	10.72	11.79	13.17
RXC10 X HCC	16.59	13.61	11.75	10.88	11.59	10.62	9.23	8.66	8.34	7.63	6.63	6.91	7.42

Table C.3: Individual fit for all enrollees and for each RXC-HCC disease group, by days' supply threshold

	Days' Supply Thresholds												
RXC	0	30	60	90	120	150	180	210	240	270	300	330	360
	R^2 Fit For All Enrollees (%)												
1	34.033	34.035	34.042	34.046	34.052	34.054	34.056	34.054	34.050	34.046	34.033	34.024	34.012
2	34.032	34.070	34.058	33.956	33.924	33.912	33.880	33.881	33.880	33.871	33.871	33.871	33.866
3	34.033	34.023	34.006	33.999	33.992	33.990	33.988	33.989	33.990	33.990	33.992	33.994	33.994
4	34.034	34.016	33.937	33.928	33.924	33.884	33.849	33.842	33.838	33.829	33.795	33.780	33.780
5	34.033	34.032	34.028	34.027	34.026	34.024	34.024	34.022	34.020	34.020	34.021	34.020	34.020
6	34.033	34.027	33.998	33.981	33.961	33.941	33.922	33.892	33.868	33.848	33.808	33.780	33.751
7	34.033	34.032	34.031	34.028	34.027	34.025	34.019	34.013	34.007	33.997	33.982	33.973	33.958
8	34.033	34.043	34.058	34.073	34.077	34.077	34.074	34.063	34.053	34.032	33.996	33.966	33.887
9	34.033	34.035	34.064	34.077	34.091	34.064	34.007	34.005	33.954	33.847	33.810	33.720	33.435
10	34.033	34.034	34.060	34.065	34.066	34.066	34.074	34.073	34.069	34.061	34.052	34.025	33.991
	R^2 Fit For the RXC-HCC Disease Group (%)												
1	38.89	39.08	39.68	39.99	40.42	40.50	40.59	40.33	39.90	39.47	38.25	37.46	36.38
2	38.35	38.99	39.54	37.07	36.99	36.66	35.69	35.61	35.60	35.39	35.38	35.37	35.26
3	38.15	38.07	37.84	37.74	37.66	37.62	37.59	37.58	37.58	37.58	37.58	37.59	37.59
4	32.10	31.97	31.42	31.14	30.86	30.68	30.41	30.13	29.96	29.83	29.67	29.58	29.53
5	39.22	39.21	39.14	39.08	39.03	39.00	38.99	38.94	38.90	38.88	38.85	38.83	38.80
6	39.16	39.14	39.06	39.02	38.98	38.93	38.88	38.80	38.73	38.67	38.53	38.44	38.35
7	38.99	38.99	38.99	38.98	38.98	38.98	38.97	38.96	38.94	38.92	38.89	38.87	38.83
8	26.59	27.24	28.13	28.98	29.15	29.10	28.75	27.76	26.93	25.36	22.97	20.96	15.70
9	29.72	29.79	30.12	30.25	30.36	30.08	29.47	29.37	28.81	27.64	27.18	26.21	23.24
10	34.60	34.61	36.71	36.93	36.93	36.88	37.59	37.44	37.11	36.32	35.54	33.14	30.30
	Cummings's Prediction Measure For the RXC-HCC Disease Group (%)												
1	21.65	22.66	25.16	25.73	26.34	26.41	26.01	24.12	22.20	20.43	17.10	14.82	11.58
2	20.65	21.32	22.11	18.42	17.88	17.28	14.87	14.76	14.71	14.65	14.63	14.60	14.53
3	26.38	26.44	26.22	26.06	25.90	25.82	25.76	25.74	25.74	25.72	25.78	25.78	25.80
4	28.03	28.77	28.67	28.46	28.20	27.93	27.63	27.45	27.30	27.16	26.92	26.82	26.78
5	28.66	28.74	29.01	29.15	29.34	29.42	29.45	29.64	29.65	29.62	29.60	29.54	29.46
6	30.92	30.96	31.10	31.20	31.35	31.39	31.40	31.34	31.27	31.16	30.84	30.68	30.46
7	30.76	30.77	30.86	30.92	31.04	31.09	31.12	31.21	31.22	31.21	31.20	31.16	31.05
8	28.38	29.04	30.11	31.06	31.24	31.11	30.14	28.46	27.00	24.49	21.35	18.72	11.39
9	22.55	22.67	23.39	23.57	23.92	23.62	22.67	22.71	21.93	19.95	19.44	17.97	13.71
10	27.13	27.56	29.51	29.43	29.17	28.84	28.76	28.69	27.63	26.15	24.43	21.93	18.70

Table C.4: Incentives for gaming, by days' supply threshold

	Days' Supply Thresholds												
RXC	0	30	60	90	120	150	180	210	240	270	300	330	360
	Measure A: Net revenue from unlimited gaming so all patients cross the threshold ($ per patient in the disease group)												
1	4,695	4,754	5,300	5,245	5,333	5,341	5,382	5,941	6,319	6,732	8,140	8,874	10,157
2	80,740	58,339	37,015	15,988	-12,030	-40,157	-68,183	-96,458	-124,758	-153,043	-181,350	-209,655	-237,963
3	8,743	8,760	8,966	9,055	9,233	9,300	9,353	9,573	9,629	9,669	9,943	9,998	10,065
4	34,650	35,693	37,291	38,080	39,621	40,115	40,484	41,519	41,549	41,427	41,588	41,243	40,835
5	1,321	1,087	1,107	903	734	400	26	-183	-616	-1,102	-1,513	-2,043	-2,584
6	7,898	7,592	7,377	7,135	6,960	6,702	6,457	6,322	6,060	5,816	5,711	5,436	5,167
7	1,468	1,277	1,102	913	747	543	338	176	-39	-254	-416	-641	-860
8	21,251	18,994	16,907	14,926	12,808	10,358	8,418	6,193	3,549	1,560	-271	-2,870	-3,059
9	14,691	13,163	11,923	10,587	9,374	7,753	6,185	4,787	2,917	1,173	-480	-2,593	-4,320
10	66,204	69,966	71,075	69,497	67,643	65,229	62,580	60,216	57,059	53,212	51,565	48,032	46,182
	Share of the disease group with potential profitable unlimited gaming (%))												
1	16.2	17.5	21.0	22.2	23.9	25.5	27.3	31.2	34.7	38.5	46.3	52.0	60.1
2	87.3	88.9	93.2	99.4	11.6	8.3	4.6	2.1	1.3	0.6	0.3	0.2	0.2
3	72.4	72.8	74.9	76.0	77.9	78.8	79.7	81.9	82.8	83.5	86.2	87.1	88.1
4	68.8	73.0	77.5	80.2	84.5	86.6	88.5	91.9	93.1	94.1	95.7	96.2	96.6
5	51.3	52.7	59.9	63.5	67.9	18.1	18.9	24.5	25.2	23.9	25.0	24.0	22.1
6	78.3	78.5	79.8	80.9	82.7	83.7	84.9	87.2	88.3	89.7	92.5	93.7	95.0
7	61.5	61.8	63.5	64.8	67.4	68.3	69.3	72.1	11.2	11.8	14.7	15.2	16.3
8	43.5	44.2	45.8	47.7	49.7	51.2	54.1	13.3	15.2	18.1	21.7	23.7	31.7
9	59.6	60.6	63.3	65.7	68.9	70.7	73.0	75.8	76.9	18.9	21.3	21.9	24.5
10	46.6	59.8	66.6	69.6	72.6	75.0	77.4	79.7	81.2	76.1	76.8	76.9	78.9
	Measure B: Net revenue from prescribing up to 30 days more to the prescribed ($ per patient in the disease group)												
1		2,059	5,214	1,773	2,770	2,424	2,816	6,074	5,297	5,954	12,405	9,411	13,450
2		1,693	4,375	6,146	160	86	226	22	3	21	3	5	5
3		79	341	174	294	146	127	350	135	118	434	141	158
4		2,155	2,940	1,681	2,712	1,317	1,193	2,089	797	595	974	342	269
5		159	678	341	466	219	178	526	178	111	273	87	78
6		29	159	127	216	120	142	284	139	168	350	150	166
7		-19	65	48	91	38	42	112	40	44	127	44	57
8		849	1,559	2,132	2,084	1,630	3,025	2,761	2,162	3,953	4,839	3,716	9,738
9		385	1,083	1,016	1,350	792	993	1,422	747	1,043	1,370	710	1,430
10		16,044	12,113	7,096	6,853	5,693	6,096	6,544	5,228	3,923	8,903	4,857	8,496
	Share of the disease group with potential profitable 30-days gaming to the prescribed (%)												
1		1.4	3.5	1.1	1.7	1.5	1.7	3.7	3.3	3.6	7.6	5.7	8.2
2		1.6	4.3	6.2	0.2	0.1	0.2	0.0	0.0	0.0	0.0	0.0	0.0
3		0.5	2.1	1.1	1.8	0.9	0.8	2.1	0.8	0.7	2.6	0.9	1.0
4		4.2	4.5	2.4	3.9	1.9	1.7	3.0	1.2	0.9	1.4	0.5	0.4
5		1.4	7.0	3.7	5.6	2.8	2.4	7.4	2.6	1.6	3.9	1.3	1.2
6		0.2	1.3	1.0	1.7	1.0	1.1	2.3	1.1	1.4	2.8	1.2	1.3
7		0.2	1.8	1.3	2.5	1.1	1.2	3.1	1.1	1.2	3.5	1.2	1.6
8		0.8	1.6	2.0	2.0	1.5	2.8	2.7	2.1	3.8	4.7	3.6	9.5
9		1.0	2.7	2.5	3.4	2.1	2.6	3.6	1.9	2.8	3.5	1.9	4.0
10		13.2	6.7	3.0	2.8	2.3	2.6	2.4	2.0	1.5	3.3	1.8	3.2

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