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Medicare advantage: provider networks, payment, and value

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MEDICARE ADVANTAGE:

PROVIDER NETWORKS, PAYMENT, AND VALUE

by

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DEDICATION

To my parents, Inna and Mikhail Feyman. They uprooted their lives and came to the U.S. so that I could have a better life. They instilled in me a love of learning and knowledge, and sacrificed tirelessly so I could make it to where I am.

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ABSTRACT

Medicare Advantage (MA), a private alternative to Traditional Medicare (TM), covers over 50 percent of Medicare beneficiaries and accounts for a similar share of spending (in 2023). The government pays private insurers a monthly amount to offer coverage to beneficiaries. The plans covering most MA enrollees – preferred provider organizations (PPOs), health maintenance organizations (HMOs), and point of service (POS) plans – are also required to maintain provider networks that restrict access to certain providers and meet government adequacy requirements. In paper one, we develop a method for measuring the restrictiveness of provider networks in MA without relying on provider directories. This approach relies on prescription drug event (PDE) data for MA enrollees to identify providers seen by enrollees. Focusing on primary care providers (PCPs) as a high-prescribing specialty, we use a prediction model trained on stand-alone prescription drug plans (PDPs) to estimate the number of providers that would have been seen absent network restrictions, allowing estimation of a measure of network restrictiveness for MA plans. Our findings suggest that MA plans reduced access to PCPs to 60.6% of what we would expect it to be absent network restrictions. HMOs tended to have the most restrictive networks, and rural areas were most affected by network

restrictions.

When developing provider networks, MA insurers seek to maximize profit while meeting regulatory standards. To make networks attractive to patients, insurers might have to include providers that are differentiated by quality, brand-name, or other characteristics. These so-called “star providers” are those that are difficult to exclude from networks due to market power, potentially driven by product differentiation or other behavior. In the second paper, we build on prior work identifying star providers in other markets, and using claims data, we develop a measure of demand for provider groups among TM beneficiaries. Using this measure, we identify star provider groups, of which 81.04% are in-network for at least one MA plan, compared to 26.3% for others (SMD: 1.31). While these groups had a larger share of beneficiaries than others (5.69% vs 1.14%, SMD: 0.57) (indicating market power), they tended to have a similar number of providers. These findings suggest that there exist provider groups that limit the ability of MA insurers to flexibly modify networks, which may affect how regulators view proposed mergers.

Insurers participating in MA must offer benefits at least as valuable as TM, but typically expand benefits beyond what TM offers, and they are required to have an out-of-pocket limit on beneficiary costs. Payment changes might affect the value of these benefits. Reductions in payment might lead to narrower networks or less expansive benefits, for instance. In the third and final paper, we use a one-time reduction in government payments in 2015 to identify the extent to which payments change network breadth, benefits, and/or advertising effort. We find that less than 100% of the reduction is passed through to beneficiaries. 40.6% of the reductions are passed through as less generous benefits while 27.6% are passed through as higher premiums. We find a reduction in zero-premium plans but no effect on advertising effort or network restrictiveness.

A major contribution of our analyses is the development of a novel method for measuring provider network restrictiveness, allowing regulators and researchers to evaluate the role of provider networks in affecting access without relying on provider director data. Our results are consistent with prior work suggesting that the MA market is generally non-competitive and that a less than competitive provider market may make it difficult for insurers to modify provide networks.

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LIST OF ABBREVIATIONS

ACA .... Affordable Care Act

AHRF .... Area Health Resources File

CCP .... Coordinated care plan

CI...Confidence Interval

CMS ....Centers for Medicare and Medicaid Services

DID ....Difference in differences

FFS .... Fee-for-service

HCC .... Hierarchical condition category

HHI .... Herfindahl-Hirschman Index

HMO .... Health maintenance organization

HMO-POS.. Health maintenance organization-point of service

MA .... Medicare Advantage

MA-PD.. Medicare Advantage Prescription Drug Plan

MBSF .... Medicare beneficiary summary file

MD-PPAS .... Medicare Data on Provider Practice and Specialty

NPI .... National provider identifier

O/E .... Observed-expected

OLS .... Ordinary least squares

OOPC.. Out of pocket cost

PBT .... Plan bid tool

PCP .... Primary care physician

PDE .... Prescription drug event

PDP .... Prescription Drug Plan

PPO .... Preferred provider organization

QBP .... Quality bonus payment

RMAE .... Root mean absolute error

RMSE .... Root mean squared error

SD .... Standard deviation

SE .... Standard error

SMD .... Standardized mean difference

SNP .... Special needs plan

TIN .... Tax ID number

TM .... Traditional Medicare

TWFE .... Two-way fixed effects

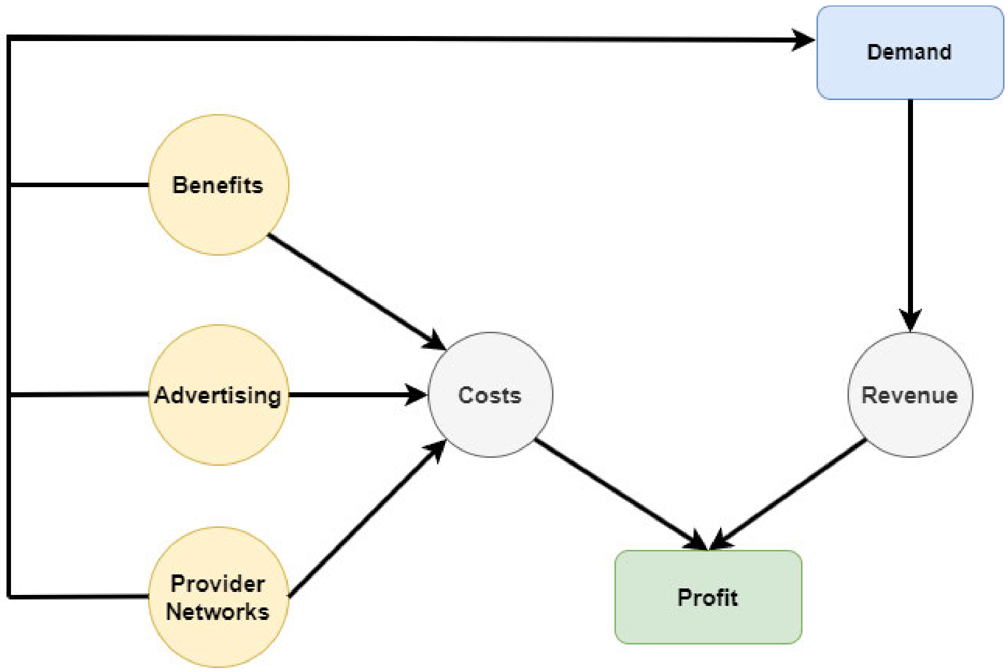
US .... United States

WMP .... Wesleyan Media Project

CHAPTER ONE: INTRODUCTION

The Medicare Advantage (MA) program is a privately offered insurance alternative to the public Traditional Medicare (TM). These private plans are paid by the federal government to offer benefits that are at least as generous as TM, though insurers typically offer more generous benefits and use a network to restrict access to providers. Since its inception, the MA program has grown and changed substantially, with mixed evidence on costs and quality.[1–4] Networks are prevalent in MA market. Over 3,000 MA plans are offered by private insurers, with the average beneficiary having access to 39 plans in 2022.[27] In 2023, over fifty percent[5] of Medicare beneficiaries were enrolled in privately-offered plans, while annual spending has reached over $360 billion.[6] Concerns have been raised with respect to the efficiency of this program. These have included potential overpayments to plans[7] and favorable selection of healthier beneficiaries,[3] particularly in the context of imperfect risk-adjustment. Others have noted potentially untoward behavior including the use of deceptive advertising[8] and promoting inaccurate provider network directories.[9]

Figure 1.1 Conceptual Model of Insurer Profit Maximization



While there has been much work estimating overpayments to MA plans and favorable selection in the program,[3,10–15] there has been less evidence examining provider networks and their construction. A framework of profit-maximization (see Figure 1.1), drawing from economic theory, provides a useful way of thinking about what might be expected. The conceptual model suggests that insurers should be expected to design provider networks (as well as advertising and benefits) in a way that maximizes profit. This can include both reducing utilization through network restrictiveness, but also by including high-demand providers to make plans attractive. Existing work has primarily sought to measure the breadth of provider networks, relying on provider directory data collected by Ideon (formerly Vericred). Meyers et al. (2022),[16] for instance, used this directory data to measure at the national level, variation in inclusion of providers in any MA network across several specialties. Mental and behavioral health providers had the lowest inclusion rates, while for some specialties inclusion rates were over 70%. Perhaps surprisingly, narrow networks were found to have the highest quality ratings. This is consistent with other work by Sen et al. (2021).[17] Notably, with one prior exception,[18] all work on MA networks has relied on these provider directories. While they are increasingly accessible to researchers, the data often have inaccuracies and may lead to measurement error in network breadth.[19,20] Methods that allow one to avoid relying solely on directory-based approaches – such as those based on measured utilization – can serve as complementary tools to help regulators evaluate provider networks in minimally burdensome ways.

When developing provider networks, insurers must decide which providers and provider groups will be included in-networks. This represents a constrained optimization problem for insurers. A profit-maximizing insurer would want to include low-cost providers, potentially those who deliver higher-quality care and minimize utilization, while ensuring that providers in high-demand by beneficiaries (perhaps due to brand name recognition) are included in order to make the insurance product attractive. When providers (so-called “star providers”) have market power – due to noncompetitive behavior, brand name recognition, and/or quality of care – this limits the flexibility of insurers to adjust provider networks and may lead to higher costs. Prior investigations have focused primarily on hospitals. Katherine Ho identified so-called “star hospitals” – defined as those with a predicted market share above the 90th percentile in a counterfactual where all insurers contract with all hospitals – generate higher revenues and profits than others.[21] In other work, Shepard (2022)[22] found that inclusion of “star hospitals" in provider networks led to adverse selection against plans. While these results may translate to individual providers and provider groups, this isn't guaranteed. Individual providers may focus less on profit-maximization than hospitals and operate under substantively different market structures.

Notably, the provider networks established by MA insurers do not exist in a vacuum. MA insurers must simultaneously optimize provider networks, plan design components such as co-pays and out-of-pocket maximums, premiums, and advertising effort. Changes to provider networks or other plan features might occur because of broader market-level dynamics, or because of changes in payment rates set by the federal government. With over $360 billion in payments to plans in 2023,[6] a key policy concern is understanding how insurers might respond when payments change. Some existing work has sought to estimate this using a variety of quasi-experimental methods. Marika Cabral and co-authors (2018)[23] used a policy-driven change in plan payments in 2000 to estimate that 45 percent of payment increases are passed through as lower premiums, while 9 percent are passed through as more generous benefits. Meanwhile, relying on a geographic discontinuity, Duggan et al. (2016)[24] estimate that only about 12 percent of increased payments lead to more generous benefits or lower premiums, with a large increase in advertising spending as well. A notable limitation of this and other work is that it covers periods of time during which the MA program looked very different than it does today (e.g., far fewer enrollees and different government subsidization design). While more recent work has expanded analyses to 2015,[25] these analyses are not able to examine potential pass-through to other outcomes including provider networks and advertising effort. This underscores the need for up-to-date estimates of payment pass-through in the MA program that can identify changes across multiple dimensions.

Overview of Dissertation Chapters

In chapter 2, we develop a machine learning prediction model trained on Medicare Part D PDE data to estimate the number of PCPs that would have been seen my MA beneficiaries absent network restrictions. We find that MA provider networks reduce the number of PCPs seen by MA beneficiaries to 60.6% of what it would otherwise have been absent network restrictions. HMO plans restricted access the most, and rural areas in general had the most restrictive networks, reducing the number of PCPs seen to 31.6% of what it would have been absent network restrictions. Evidence suggested that a higher provider supply in a county was associated with less restrictiveness, while the parent company's market share in an area was associated with more restrictive networks, possibly suggesting a role of market power in affecting access to providers.

In chapter 3, we use the inferential method based on PDEs from chapter 2 to identify so-called "star provider groups" that are more likely to be in-network than other groups. 81% of all provider groups defined as star providers were in-network for at least one MA plan nationally, compared to 26% of all other providers. Even though star provider groups were smaller, on average, than other provider groups, they maintained a higher share of beneficiaries in the counties in which they operated. Multivariable models indicated that star provider groups were 53% to 58% more likely to be included in at least one MA network than other provider groups operating in the same markets. These results underscore the importance of market power and/or product differentiation by provider groups in affecting the flexibility of MA plans to modify provider networks.

In chapter 4, we investigate how MA insurers respond to changes in payments by relying on a one-time national payment reduction in 2015 that affected some plans but not others. We find that the change in payments led to a roughly $41 reduction in payments per member per month. Our estimates suggest that 40.6% of this reduction was passed-through to beneficiaries as less generous benefits, and 27.6% of the reduction was passed-through as higher premiums. Additionally, the probability of offering a zero-premium plan fell by 16.7%. We detected no statistically significant effect of payment reductions on advertising effort by insurers or on the network restrictiveness of their PCP provider networks. Consistent with prior work, these results suggest that MA insurers are unlikely to fully pass-through changes to payments, suggesting that the remainder may flow to higher profits or other residual claimants (such as providers).

Lastly, chapter 5 provides concluding thoughts and further discussion of policy implications.

CHAPTER TWO: MEASURING RESTRICTIVENESS OF MEDICARE ADVANTAGE PROVIDER NETWORKS: A CLAIMS-BASED APPROACH

Introduction

Provider networks concentrate utilization among certain providers and are common in health insurance, with substantial variation in insurance networks' coverage of nearby clinicians and hospitals.[26] Clinicians may be excluded from insurer networks to reduce costs, improve quality, or to increase profit for the insurer.

Nearly all MA plans are required to establish provider networks,[28] which can affect cost and quality. For instance, if providers are selected into networks based on more appropriate use of screenings, beneficiaries might benefit through lower utilization and better outcomes. Alternatively, if insurers construct networks such that accessing providers becomes difficult, outcomes may suffer. Thus, understanding how networks influence access to care and how that is associated with market factors is crucial for assessing the performance and value of the MA program.[29] Understanding whether particular geographies – for instance, rural areas – are disproportionately affected by provider networks can help guide regulatory change to minimize harms.

We conceptualize access as the extent to which contracted provider networks affect the number of providers seen by beneficiaries relative to a counterfactual with no networks. While MA networks are regulated with annually updated requirements,[30] it is not clear whether the requirements are binding (e.g., whether they limit plans' ability to modify networks or not) on plans or how they affect access to care. Indeed, while regulatory review of networks is now more common than before, and most exceptions to requirements have generally being approved.[31] One way to make regulatory review more efficient is to measure the effects of MA plan network extent from readily available data, following up with regulatory effort where access to care is most constrained.

One approach to assessing networks and their impacts is to use recently available machine-readable provider directories published by MA-PD plans. With them, researchers have examined the scope and breadth of provider networks in MA,[16,26] as well as their relationship with plan quality.[17] However, provider directories are error prone.[19,20,32] Providers may have specialties misclassified, office addresses may be incorrect, or the provider may simply not be seeing new patients. Thus, while provider directories may offer an indication of the “official” network being offered by a plan, the actual networks available to beneficiaries may look different. Indeed, in other work, researchers found a large prevalence of so-called “ghost providers” in Medicaid Managed Care provider networks who have few or zero interactions with patients. Roughly one-third of physicians who were officially contracted with a plan saw fewer than ten beneficiaries in a given year.[33] In short, access to care as conveyed by directories may not reflect the reality that enrollees experience.

Another approach to assessing networks relies on utilization data. New Hampshire, for instance, uses the state’s all payer claims database to identify the share of available providers that are listed in the provider directory for plans on the ACA marketplace.[34] In prior work taking a similar approach, we used prescription drug utilization for Medicare beneficiaries in MA-Prescription Drug (MA-PD) plans to determine which primary care providers are used by MA-PD beneficiaries instead of relying on network directories.[18] We assessed MA-PD primary care physician (PCP) network extent relative to the total number of PCPs in the market as measured by data from IQVIA (formerly SK&A). The enabling insight of this work is that a Medicare drug claim indicates the plan and the prescriber. Thus, we were able to observe the extent to which networks restrict access to available prescribers. (One could use MA encounter data to make similar inferences, but the data is often incomplete.[35]) This approach to inferring the effects of provider networks from claims circumvents the challenges of collecting accurate provider data and ensuring network directory accuracy but also answers a slightly different question. Because it is based on utilization, it reflects which providers are demonstrably accessible to beneficiaries — the ones they actually see. A key assumption is that for high-prescribing specialties, encounters occurring with a prescription constitute a representative sample.

In this analysis, we build on our prior work with additional years of data and an improved approach that that avoids the need to directly measure the number of available providers. We use prescription drug utilization data and a prediction model to develop a measure of the effects of MA-PD provider networks on access (hereafter called network "restrictiveness"). We use the relationship between utilization and plan-level factors and demographics for TM beneficiaries to estimate counterfactual utilization for MA-PD beneficiaries had they not been subject to the influence of networks. Results are presented for a high-prescribing specialty (PCPs) with details that would allow other researchers and regulators to replicate our approach.

Medicare Advantage Plan Background

The configuration of MA offerings follows a structure that informed our study design. We focused on Local Coordinated Care Plans (CCPs), which draw the majority of enrollment in MA, are open to public enrollment, and are required to maintain provider networks. Their payment rates are calculated at the county level. We excluded regional plans, employer plans, special needs plans (SNPs), private fee-for-service plans, and other non-standard plan types.

Insurers participating in the MA program offer products in a hierarchy. Contracts are umbrellas for individual plans offered by insurers and determine the county-level service areas from which insurers may enroll beneficiaries. While the underlying benefit and cost design (e.g., cost-sharing, covered services) can vary within a contract, networks are typically regulated at the contract level. Nonetheless, because there may be different plan types (Health Maintenance Organization [HMO], Preferred Provider Organization [PPO], Health Maintenance Organization-Point of Service [HMO-POS]) within a contract, observed networks may still vary between plan types within a contract. Stand-alone prescription drug plans (PDPs), which compete with MA-PD plans for enrollees, offer products in a similar way, except that they may only be combined with plans that do not establish provider networks, such as TM.

Data and Methods

Conceptual Framework

Consider an MA-PD contract i offering plans (stratified by plan type, h) in county c, and year t. Y represents the total number of unique providers seen by enrollees in this contract and D^z represents whether enrollees face network restrictions, where $z = 1$ indicates the presence of restrictions and $z = 0$ indicates the absence. An estimate of the restrictiveness of this contract is the ratio of unique providers seen by enrollees in the contract (observed) and an estimate the total number of unique providers that would have been seen by enrollees in a plan *absent* network restrictions, conditional on various plan-level factors (estimated), an (O/E) ratio:

Eq. 2.1
$$\frac{O}{E} = \frac{(Y_{icht}|D^1)}{(Y_{icht}|D^0)}$$

This ratio must be non-negative, and for specialties for which there is an expectation that MA-PD plans restrict utilization (e.g., specialists), it must be less than or equal to 1. However, it is ambiguous whether networks would reduce the number of unique PCPs seen by beneficiaries. Indeed, there is evidence MA-PD beneficiaries may actually see *more* unique PCPs than those in PDPs.[36] Thus, for PCPs, the O/E ratio may be greater than 1. (Appendix A.2).

A key challenge is that $Y_{icht}|D^0$ is unobserved among MA-PD beneficiaries and is a *potential* rather than realized outcome. Typically, with observational data, one estimates a counterfactual with either an exogenous change that affects treatment assignment or with a comparable population. We focus on the latter, directly estimating the counterfactual $Y_{icht}|D^0$ with a predictive model using PDP beneficiaries as a comparable population facing no network restrictions. That is, we used our PDP sample to identify relationships between the observed variables and the unique number of providers and then used these estimated relationships to predict counterfactuals for the MA-PD sample.

Two key assumptions are (1) that unobserved variables are uncorrelated utilization and selection into TM vs MA-PD, and (2) that the covariates used to predict counterfactual utilization are correlated with utilization in a similar way for both MA-PD and TM beneficiaries. While we attempt to adjust for difference in risk and health status by including contract-plan type-county level mortality rates separately for MA-PD and PDP samples, there may be remaining unobserved variables (related to selection on health status) correlated with utilization or the included covariates.[3] For example, if MA-PD enrollees are systematically healthier than TM enrollees in ways we don't observe, we expect that our counterfactual based on PDP utilization will be overestimated. In that case, our estimates would overestimate network restrictiveness.

Data Sources and Study Sample

We included two groups: TM beneficiaries enrolled in a PDP and beneficiaries enrolled in an MA-PD plan from 2011 to 2017. The former was used to develop an estimate of counterfactual utilization without the influence of plan network constraints. The latter was used to assess the degree to which utilization is restricted by provider networks.

Beneficiaries were assigned to an MA-PD plan or a PDP plan based on enrollment in June of a calendar year according to the Medicare Beneficiary Summary File (MBSF). We linked enrollment data to the prescription drug events (PDEs) of a 20% random sample of beneficiaries on the unique, encrypted beneficiary ID.

Providers were identified based on the National Provider Identifier (NPI), which is present in the PDE data. Provider specialty was obtained from the Medicare Data on Provider Practice and Specialty (MD-PPAS) database maintained by the Centers for Medicare and Medicaid Services (CMS). We excluded hospitalists, and focused on internal medicine, family practice, general practice, and geriatric medicine specialties. We obtained data on the service area and plan type of each MA-PD and PDP plan from CMS' Service Area Files and Plan Characteristics files, respectively. PDEs were aggregated to the plan-type level within each contract-year-county observation. Because plan structure (e.g., HMO vs. PPO) within a contract could affect utilization, our unit of analysis was the year-contract-plan type-county.

Appendix A.2 described the creation of a bound on the counterfactual we estimate, which we refer to as the "maximum tolerable O/E ratio." County-level characteristics were obtained from the Area Health Resource File (AHRF) produced by the Health Resources and Services Administration.

Variables and Definitions

Our outcome of interest was the contract-plan type-county-year level estimate of network restrictiveness (Equation 2.1). The numerator was the observed number of providers seen by beneficiaries in an MA-PD plan calculated by identifying the total number of unique NPIs prescribing to beneficiaries in a contract-plan type-county-year observation. The denominator was the predicted number of unique providers that would be seen absent the MA-PD plan's network constraints.

A provider was considered in-network for plans in a county if there were beneficiaries in that county receiving prescriptions from that provider, regardless of provider location. This is consistent with CMS' network adequacy requirements, allowing a provider to be part of a contracted network regardless of their physical location.[30] When we observed no prescription drug events in a given county of a plan's service area, we set the count of prescription drug events and prescribers to zero. Complete variable definitions are listed in Appendix A.1.

Prediction Model: Overview

To predict the expected number of in-network providers for an MA-PD contract (our denominator), we modeled the relationship between the observed number of unique providers prescribing to enrollees in a contract-plan type-year-county from PDE data in the PDP sample and the variables in the prediction model section of Appendix A.1. The model included: the number of unique providers in a given specialty seen by any beneficiary in the county, the number of all MA-PD enrollees in the county, the number of enrollees in the observation, the number of prescription drug events among enrollees (these three affect the number of observed prescribers for a given observation); binary indicators for the state (because state-level insurance regulation could affect selection into coverage correlated with utilization); the percent of enrollees in several age groups (<65, 65-74, 75-84, 85+) and the average age of enrollees (because age is correlated with prescription drug utilization), and the share of beneficiaries that died in a given year (to at least partially account for differences in health risk between PDP and MA-PD enrollees). (Appendix A.1) While a more complete model might include additional information, such as rates of illness for both the MA-PD and PDP segments, these data are generally not easily accessible to researchers.

We used the estimated relationships to predict the number of providers we would expect to see in the MA-PD sample if there were no network constraints. In this way and in contrast to most previous network estimation work, we do not assume that all providers in a market are at risk for being in-network in an MA-PD contract.

We compared the performance of a pseudo-Poisson model[37] with state fixed effects and a random forest model. The pseudo-Poisson model is well-suited for count outcomes and the random forest model can outperform other algorithms for predicting count outcomes in a health care setting.[38] Other models (negative binomial, Poisson, and a zero-inflated Poisson) only converged with a smaller subset of covariates, making them less robust for prediction. The pseudo-Poisson model also failed to converge with an offset or exposure, so we estimated it without one. To select the best-performing prediction algorithm, we considered three measures of model performance (lower is better for all measures): root mean squared error (RMSE), root mean absolute error (RMAE), and the maximum difference between the observed and predicted values (Emax).[39] All models were estimated using five-fold cross validation.[40] For each excluded fold, predictions were obtained from the four remaining folds. Overall performance was obtained by averaging predictions across each excluded fold. We estimated algorithms at each cutoff of the number of PDEs per observation (described further below) and used the average performance across all cut-offs to compare performance.

The pseudo-Poisson model was estimated with nonlinear terms (up to cubic terms) of each variable, as well as base level effects to allow for additional non-linear relationships.

For the random forest model, we tuned two hyperparameters: the maximum number of trees and the number of variables to randomly include in each split. To select the optimal combination of the two hyperparameters we used a grid search approach, testing every possible value from 10 to 500 in intervals of 10, giving over 1,000 combinations of the two hyperparameters. We selected hyperparameter values that minimized RMSE.

We estimated the best performing algorithm on data with cutoffs ranging from 1 to 1000 PDEs, in intervals of 10 PDEs (e.g., excluding less than 10 PDEs, less than 20 etc.). We used three metrics to select the best performing subset within an algorithm: Emax, the share of O/E ratios greater than the maximum tolerable O/E ratio, and the calibration slope of the model. RMSE and RMAE, are sensitive to the number of observations, and thus were not used.

After selecting the best-performing prediction algorithm, we evaluated its performance in samples with varying numbers of PDEs per observation in the PDP data. We did this because observations with few PDEs may be noisy and lead to unstable predictions. For instance, a PDP plan with only five observed PDEs might provide less information for estimating the counterfactual than a PDP with 100 observed PDEs.

O/E ratios greater than 1.74, the maximum tolerable O/E ratio, were top-coded and set to 1.74 (this occurs less than one percent of the time) (Appendix A.2). To examine performance across these metrics, we standardized each metric by subtracting each data point from the sample mean and dividing by the standard deviation (to account for differences in scale) and averaged across them.

Relationship With Market and Plan-Level Factors

We evaluated relationships between network restrictiveness and the following county-level variables: the average TM HCC risk score for all Medicare beneficiaries in the county, the total number of doctors per 1,000 population in a county, an indicator for rurality of the county, the MA-PD Herfindahl-Hirschman Index (HHI) of the county,[41] the natural log of per capita income in the county, and the number of Veterans per 1,000 population in the county. These variables measure different dimensions of the market in which MA-PD operate, including underlying risks of the population, the degree of market concentration, and income among residents in the market. (Details in Appendix A.1)
In addition to county-level variables, we included several contract-plan type level variables: the plan type (HMO, HMO-POS, PPO), the parent company's market share at the county-year level, and the year that the contract became active in MA. All are factors that have been found to be associated with MA enrollment, penetration, market entry, and network breadth.[18,42,43]

The general form of our specification is in Equation 2.1.

Eq. 2.2 $$\widehat{O/E}_{ipct} = \beta_0 + \theta X_{ct} + \gamma\, Z_{ipct} + C + T + \varepsilon_{ipct}$$

Where i indexes a contract, p indexes a plan type, c indexes a county, and t indexes a year. $\widehat{O/E}$ refers to the O/E ratios described above, C is a vector of state fixed effects, T is a vector of year fixed effects, ε is an error term we cluster within county, X is the vector of county-level characteristics described above, and Z is the vector of contract-plan type characteristics described above. θ and γ are vectors of coefficients on these characteristics, respectively. Regressions were weighted with analytic weights by the number of enrollees in the contract-plan-county-year.

In separate specifications, we included additional fixed effects: parent company, parent company interacted with state, and parent company interacted with year. These help to capture variation in strategic decisions made by the parent company of the contract. We estimated all regressions using ordinary least squares with standard errors clustered at the county level.

Robustness Checks

If our results were sensitive to the amount of enrollment and prescription drug activity in a contract, we might identify more providers in large contracts than in smaller ones. This would lead to measurement error if we observed differentially sized contracts among MA-PDs relative to PDPs. While we controlled for the number of PDEs and enrollment in our predictive models, residual differences between MA-PDs and PDPs may remain. Thus, we examined the relationship between overall PDE volume and our estimated measure of network restrictiveness.

Separately, we conducted three additional validation exercises. First, if our estimates of network restrictiveness are directionally accurate, we would expect vertically integrated insurers to be more restrictive than others. To test this hypothesis, we compared our estimates for Kaiser Permanente in California with all other MA-PD plans in California. Second, to further ensure our estimates are not driven by unobserved PDEs in small plans, we estimated the regression models described above for plans with an increasing number of PDEs. We did so for PDE counts from greater than 1 to greater than

100. Third, we used an approach that matches enrollees in MA-PD and PDP contracts that does not rely on a predictive model (see Appendix A.8).

Fourth, we tested how well drug events proxy for encounters. Using 2016 Fee-for-Service carrier, inpatient, and home health claims, we identified all unique NPI-contract observations in PDE data (for PDP enrollees) and across all other settings. We examined the share of contract-NPI PDE observations that matched to a contract-NPI observation from other settings.

Fifth, because low levels of enrollment in MA-PD or PDP plans may affect our ability to observe networks, we estimated our primary specification restricted to areas with higher levels of enrollment (see Appendix A.9).

This study was considered non-human subjects research by the institutional review board of Harvard T.H. Chan School of Public Health.

Results

Algorithm Optimization

To identify the appropriate prediction algorithm, we used a dataset with 478,561 contract-plan type-year-county observations. Of these, 13.5% (N=64,357) were MA-PD observations and 86.5% were PDP observations (N=414,204). Across 100 cutoffs (in intervals of 10), the average Emax, RMSE, and MAE were smaller (which indicates better performance) for the random forest. The calibration slope (higher indicates better performance) was similarly higher in the random forest. The percent of observations with O/E>1.74 (lower indicates better performance) was negligibly higher in the pseudo-Poisson (Table 2.1).

Table 2.1. Random Forest Algorithm Outperforms Pseudo-Poisson For Network Prediction

	Emax	Calibration Slope	% O/E>1.74	RMSE	MAE
Pseudo-Poisson	2668.9	0.87	0.18%	61.3	23.1
Random Forest	878.5	0.97	0.26%	29.4	18.4

Notes: Results indicate the performance of a pseudo-Poisson and random forest model for predicting the unique number of PCPs seen by beneficiaries in a given contract-plan type-county-year. Emax indicates the maximum absolute difference between predictions and observed values. Calibration slope indicates the r-squared from a regression of the observed value on the expected value among PDPs. % O/E>1.74 indicates the percent of observations with O/E>1.74 in the MA sample. RMSE and MAE are the root mean squared prediction error and mean absolute error, respectively.

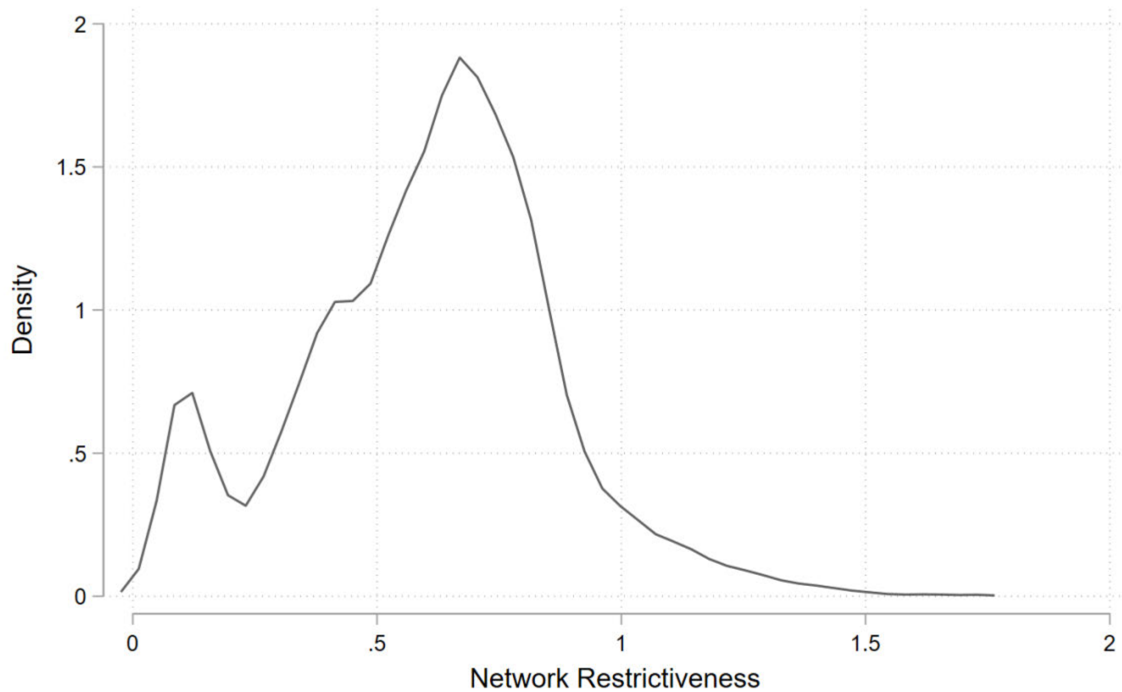
A cutoff of greater than or equal to 961 prescription drug events had the best performance (Appendix A.3). Once this cutoff was selected, we tuned the random forest, finding that the optimal number of subtrees was 60 and the optimal number of variables to randomly include in a split was 35. With the hyperparameters selected, we generated O/E ratios as discussed in the Methods section.

Our final analytic dataset for our market factors analysis, restricted to MA-PD contracts, included 64,253 observations.

Summary Results

On average, the network restrictiveness of PCP networks was 41.1%. Said differently, the number of unique providers seen by beneficiaries in MA-PD contracts was 41.1% of what we would expect it to be absent network restrictions. After weighting by enrollment, the estimated network restrictiveness was 60.6%. All results presented below are enrollment weighted unless otherwise noted. Distribution of network restrictiveness is presented in Figure 2.1.

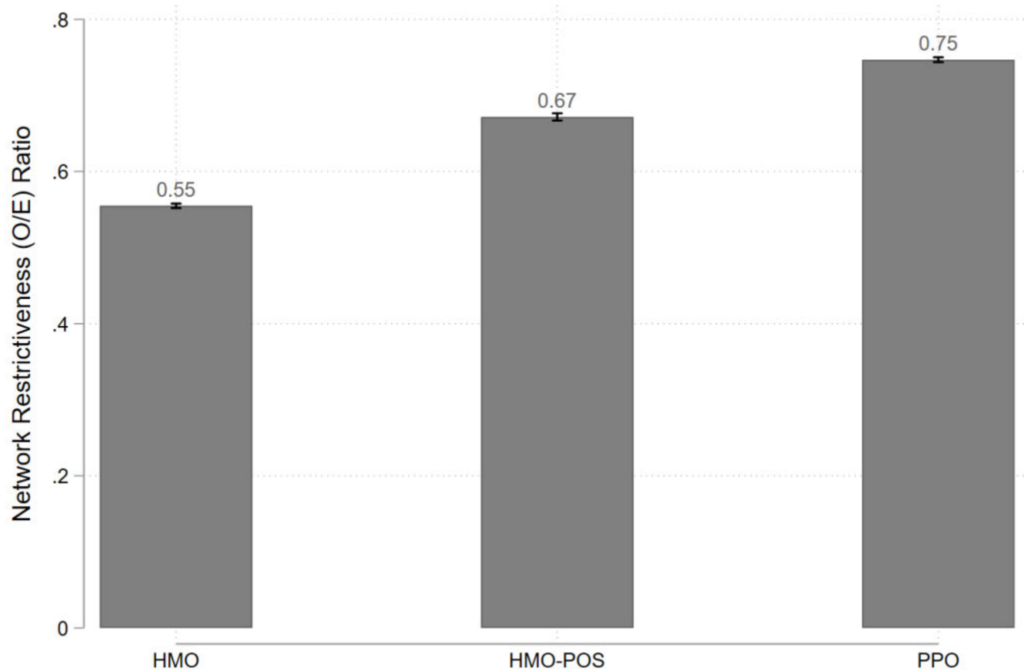
Figure 2.1. Kernel Density of Network Restrictiveness



Notes: This kernel density plot presents the distribution of network restrictiveness across all observations (N=64,253). Estimates are weighted by number of beneficiaries. Epanechnikov kernel was used with a bandwidth of 0.0242.

We found several unadjusted relationships with plan and market factors. Consistent with theory and prior work, HMO plans tended to be more restrictive (55.5%; 95% CI 55.3% to 55.7%) than HMO-POS plans (67.2%; 95% CI 66.7% to 67.8%) or PPO plans (74.7%; 95% CI 74.3% to 75.1%) (Figure 2.2).

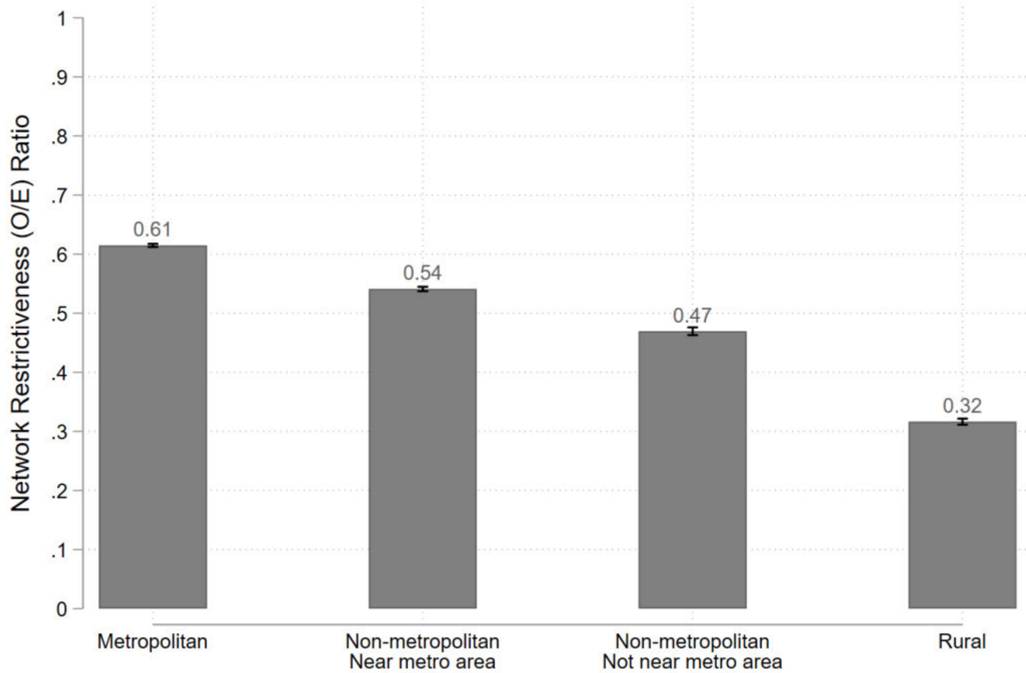
Figure 2.2. HMO Plans Had The Most Restrictive Networks



Notes: Values indicate the average network restrictiveness for each plan type. Plan type data is obtained from CMS plan characteristics file. Observations are weighted first by the number of beneficiaries in contract-plan type. Network restrictiveness is measured by creating an observed-to-expected (O/E) ratio of the observed unique number of providers seen by beneficiaries in an MA plan type, divided by the predicted number of unique number of providers that would have been seen absent network restrictions. Data is aggregated across all years of data. Complete methods are described in the text. Error bars indicate 95% confidence intervals. HMO: Health Maintenance Organization; HMO-POS: Health Maintenance Organization-Point of Service; PPO: Preferred Provider Organization

Similarly, areas that had low market concentration as measured by the HHI had more restrictive networks (44.3%; 95% CI 41.2% to 47.4%) than those that were highly concentrated (62.5%; 95% CI 62.3 % to 62.8%) (Appendix A.4). Lastly, we found that rural areas had more restrictive networks (31.6%; 95% CI 29.0% to 34.2%) compared to metropolitan areas (61.5%; 95% CI 61.3% to 61.7%) (Figure 2.3).

Figure 2.3. Rural Areas Had Most Restrictive Networks



Notes: Values indicate the average network restrictiveness for each level of rurality. Rurality is defined at the county-level from the AHRF. Observations are weighted by the number of beneficiaries in contract-plan type. Network restrictiveness is measured by creating an observed-to-expected (O/E) ratio of the observed unique number of providers seen by beneficiaries in an MA plan, divided by the predicted number of unique number of providers that would have been seen absent network restrictions. Error bars indicate 95% confidence intervals. Data is aggregated across all years of data. Complete methods are described in the text.

Multivariable Results

Multivariable results were similar to unadjusted analyses (Table 2.2). Results were largely unchanged whether we included parent company fixed effects and/or interactions between parent company and year. In a model including parent company fixed effects but no interaction terms (Model 2 in Table 2.2), HMO-POS plans were 0.136 standard deviations less restrictive than HMOs (95% CI: 0.084 to 0.189) and PPOs were 0.197 standard deviations less restrictive than HMOs (95% CI: 0.141 to 0.254).

Networks in rural areas were 1.151 standard deviations more restrictive than those in urban areas (95% CI: -1.251 to -1.052). A one-standard deviation increase in market share of a given contract-plan type was associated with a 0.104 standard deviation increase in restrictiveness (95% CI: -0.137 to -0.0712) while a one standard deviation increase in MA-PD HHI was associated with a 0.051 standard deviation decrease in restrictiveness (95% CI: 0.016 to 0.087).

A one-standard deviation increase in the year in which a contract became active in the MA-PD program (e.g., a newer contract) was associated with a 0.008 standard deviation reduction in restrictiveness (95% CI: 0.004 to 0.012). Lastly, a one standard deviation increase in a county's number of doctors per 1,000 population was also associated with 0.075 standard deviations reduction in restrictiveness (95% CI: 0.04 to 0.109). We found imprecise associations with area-level income, the number of Veterans in a county, or the HCC risk score of the TM population in a county.

Table 2.2. Multivariable Association Between Restrictiveness and Market and Plan Level Factors

	Model 1	Model 2	Model 3
Medicare County Average HCC Score	0.0171 [-0.0423 – 0.0765]	0.0305 [-0.0151 – 0.0760]	0.0309 [-0.0144 – 0.0762]
Plan Type (Ref: HMO)			
HMO-POS	0.153*** [0.0930 – 0.213]	0.136*** [0.0840 – 0.189]	0.152*** [0.0988 – 0.206]
PPO	0.269*** [0.204 – 0.334]	0.197*** [0.141 – 0.254]	0.205*** [0.148 – 0.263]
MDs Per 1,000 Population	0.0867*** [0.0480 – 0.125]	0.0745*** [0.0400 – 0.109]	0.0743*** [0.0397 – 0.109]
Rurality			
Non-metropolitan, near urban area	-0.495*** [-0.573 – -0.416]	-0.474*** [-0.545 – -0.402]	-0.473*** [-0.546 – -0.401]
Non-metropolitan, not near urban area	-0.708*** [-0.838 – -0.579]	-0.682*** [-0.809 – -0.555]	-0.682*** [-0.810 – -0.555]
Rural	-1.201*** [-1.309 – -1.092]	-1.151*** [-1.251 – -1.052]	-1.151*** [-1.251 – -1.051]
Ln(Income)	-0.0230 [-0.0665 – 0.0205]	0.0176 [-0.0223 – 0.0575]	0.0179 [-0.0220 – 0.0578]
Market Share	-0.166*** [-0.226 – -0.107]	-0.104*** [-0.137 – -0.0712]	-0.103*** [-0.136 – -0.0693]
MA HHI	0.0733** [0.0244 – 0.122]	0.0513** [0.0160 – 0.0867]	0.0501** [0.0141 – 0.0862]
Veterans per 1,000	0.0340* [2.18e-05 – 0.0679]	0.0124 [-0.0159 – 0.0407]	0.0117 [-0.0167 – 0.0402]
Effective Year of Contract	0.0113*** [0.00635 – 0.0163]	0.00804*** [0.00387 – 0.0122]	0.00789*** [0.00365 – 0.0121]
N (excluding singletons)	63,909	63,901	63,869
Parent Company FE	No	Yes	Yes
Parent Company x Year FE	No	No	Yes

Notes: Results of three models with network restrictiveness as the outcome and the listed variables as predictors are presented. Coefficients are standardized and are thus continuous variables are interpreted as a β standard deviation change in network restrictiveness for a one standard deviation change in the covariate. All models include state fixed effects. 95% confidence intervals calculated from heteroscedasticity-robust standard errors clustered at the county level in brackets. Observations vary due to singletons. Effective year of contract indicates when the contract became active in MA. HHI: Herfindahl-Hirschman Index. MD: Medical Doctor. HMO: Health Maintenance Organization; HMO-POS: Health Maintenance Organization-Point of Service; PPO: Preferred Provider Organization

Robustness Checks

We examined whether prescription volume and enrollment appeared to explain the results in our analysis. Appendix A.5 illustrates that our average restrictiveness measure is robust to inclusion of contracts with varying amounts of PDE counts. Appendix A.7 shows that three important coefficients (PPOs, providers per 1,000 population, and market share) vary little by changing the sample based on PDE count. Appendix A.8 illustrates that a matching approach which equalized enrollment between TM and MA, some of our key (counterintuitive) results on rurality continue to hold. In appendix A.9, we show that differences in county-level enrollment in MA-PD and PDP plans have little effect on our results. In Appendix A.6, we provide evidence confirming the theoretical prediction that Kaiser (an integrated contract) is more restrictive than other contracts in California.

Lastly, we found that 94.4% of PCPs who prescribe to beneficiaries in a PDP contract also see beneficiaries enrolled in that contract in other care settings, suggesting that we are likely identifying providers who see patients in MA-PD contracts even though our inferences rely on prescription drug data.

Discussion

We developed a novel approach to measuring the effects of provider network restrictions on utilization. Focusing on PCPs as a high-prescribing specialty, we estimated that provider networks were most restrictive in HMO plans and in rural areas. Increased provider supply and smaller contracts (those with less market share) were both associated with less restrictiveness.

Our estimates correspond with prior work on some measures (i.e., HMO plans being relatively more restrictive) but not others (i.e., our findings on rurality). These differences likely stem from the fact that our novel measure is different in an important way from those used in other studies of insurance plan networks. Our approach measures observed utilization relative to counterfactual utilization that is designed to strip away only the effects of plan design on utilization, while retaining constraints common to all plans in an area (e.g., provider appointment availability, drive time, etc.). In addition, our approach allows providers to be considered in-network for a given county regardless of their physical location. In this way, our approach circumvents concerns about inaccurate provider directories and obviates the need to restrict providers to serving an arbitrary geographic region (like a county). Prior approaches that measure network breath relative to a count of providers in a geographic area do not have these properties.

Additionally, our use of claims data captures actual utilization (hence incorporates what providers enrollees have genuine access to) while capture the providers that plans claim are covered (but which may not actually be accessible due to directory errors, for example). While neither are perfect for network-related inference, neither is a gold standard. They are simply different and complementary.

For example, regulators can use our approach to assess networks relative to those plans claim to offer. If networks are marketed as relatively broad and accessible, but are in fact far less so in practice (as revealed by claims data), that may warrant regulatory scrutiny and action.

We demonstrated that our model performed well after training and calibration.

Additionally, we have shown that one potential source of measurement error (PDE volume) does not affect our results (Appendix A.7). Future work should consider potential sources of uncertainty. In particular, where modeled network restrictiveness is on the right-hand side of an equation, traditional methods like bootstrapping are likely to work well. Alternatively, regression forests can generate prediction intervals from random forest specifications, allowing for better quantification of error due to prediction. However, when these measures are used on the left-hand-side of an equation (as an outcome), additional robustness testing with different model specifications (such as different algorithms or hyperparameters) may be necessary.

Limitations

There are several limitations to our work. First, as mentioned, our approach is not directly comparable to existing measures of network breadth relative to a fixed number of providers in a market. Relatedly, because our measure relies on prescription interactions it is unsuitable to low-prescribing specialties. However, many important specialties do prescribe frequently, including psychiatrists and cardiologists.

Second, remaining unobserved factors could bias our estimates. However, we include a rich set of observable variables and use fixed effects to minimize the risk of bias. Additionally, the high calibration slope of our prediction model suggests that our chosen model is accurate in predicting the number of unique providers seen by beneficiaries based on observable factors.

Third, there are many variations of prediction models available. We chose to evaluate a subset that are well-studied and perform well with the type of outcome that we examine. Nevertheless, others we have not examined may be superior.

Fourth, differences in enrollment rates among MA-PD and PDP contracts could affect our ability to observe provider interactions. While our modeling efforts account for volume of providers in the county as well as enrollment, there may be remaining differences. Alternative approaches (such as a matched sample of MA-PD and PDP enrollees) could help to address these issues. While such approaches are both computationally infeasible and likely impossible to implement because of enrollment differences, we illustrate in Appendix A.8 that using a simplified matching approach leads to directionally similar results with respect to rurality in particular (which one might expect is most prone to problems stemming from low enrollment). Nevertheless, generalizability of the approach for all applications to communities with lower enrollment may be reduced. To assess whether this biases results for our application, we estimated our primary specification restricted to areas with enrollment in MA-PD and PDPs above the first quartile. Appendix A.9 illustrates that our model results remain largely unchanged.

Fifth, our measure of network restrictiveness may capture plan-design components that include networks, but also include differences in care management strategies by plans, effects of supplemental benefits that affect utilization, and many other factors. Our prediction model attempts to capture some of these differences by accounting for plan types and some beneficiary characteristics, but ultimately, our results reflect the effect of *all* utilization management strategies.

Sixth, there may be measurement error in the measure of network restrictiveness as it is a modeled variable. If measurement error is random, it leads to inefficiency. But non-random measurement error could bias our results. While we cannot be certain that measurement error is random, we attempted to account for nonrandom error in several ways. Our primary specification (Equation 2.2) relied on variation in restrictiveness within state and year, with other specifications restricting variation to being within parent company-year and state, potentially limiting the scope of measurement error. The most restrictive specification is consistent if this measurement error is random within parent company-year and state, and if measurement error is smaller for contracts with more enrollment (due to the weighting used in the analysis). Nonetheless, it is impossible to fully address all potential measurement error.

Lastly, our analysis relating various factors to network restrictiveness is descriptive and correlational. It is designed to describe the landscape of network restrictiveness rather than measure the causal effect of any one variable on another.

CHAPTER THREE: NETWORK FORMATION IN MEDICARE ADVANTAGE: DO STAR PROVIDERS MATTER?

Introduction

Existing work on provider networks in MA has generally focused on measuring the breadth of networks[16,18,26,44,45] and their association with quality.[17] Other work has suggested that dissatisfaction with a plan and/or its providers, might explain relatively higher rates of switching among rural enrollees.[46]

Economic theory generally views insurers as profit-maximizing. Insurers might maximize profits through various mechanisms including setting premiums to cover the expected marginal cost of care and disincentivizing service use (e.g., through co-insurance). Additionally, as part of negotiations, insurers may seek to pay providers lower rates via the threat of potential network exclusion. In some cases, this threat may be constrained because providers that have market power may be non-excludable by insurers from their networks without making their product unattractive to patients.[47] Thus, network formation is nested under a broader theory of profit-maximization under imperfect competition.

Some existing work has sought to examine network formation empirically, focusing on its role in adverse selection, pricing, and hospital-insurer bargaining. In a seminal paper focusing on the commercial market, Katherine Ho identified so-called "star hospitals" that play a crucial role in network formation.[21] These hospitals — defined as those with a predicted market share above the 90th percentile in a counterfactual where all insurers contract with all hospitals — are able to exert significant power in bargaining, generating higher revenues and profits than others. In this work, these hospitals are shown to have a higher likelihood of being teaching hospitals and have higher-quality services across a number of service lines. In related work, Ho and Lee (2019)[48] considered the social costs and benefits of hospital networks that exclude some facilities finding that insurer incentives to exclude hospitals from networks often exceed what would be socially optimal. Lastly, analyzing the pre-Affordable Care Act (ACA) nongroup market in Massachusetts, Shepard (2022)[22] found that inclusion of "star hospitals" in provider networks led to adverse selection against plans.

Notably, most of the work on network formation focuses exclusively on hospital networks and on the commercial insurance market. Though it is possible that some of these findings translate to individual providers and provider groups, because of differences in incentives (individual providers may be more altruistic)[49] and different market structures, the provider and provider group markets may experience different dynamics. While a strong base of literature has examined the breadth and scope of provider networks in MA, the underlying mechanisms and reasons for network formation are unexplored. One qualitative analysis suggests that MA star ratings – measures of quality that increase payment – are a major motivator for maintaining narrow networks that allow closer management of clinician performance.[50]

Whether there are so-called "star providers" that are differentiated enough (e.g., because of quality) that they *must* be included in MA networks is unknown. If providers are able to differentiate themselves (and by doing so, accrue market power), this likely affects the ability of insurers to make network changes. This would mean that plans are limited in their ability to engage in cost containment in the presence of providers who are able to differentiate themselves. Understanding whether some provider groups command market power is critical, particularly in developing more appropriate regulation of MA insurers and the scope and breadth of their provider networks.

We sought to address this gap in the literature by answering two questions: 1) are there star providers with disproportionate market share (and thus market power) among Medicare beneficiaries? 2) if they exist, are star providers more likely to be included in an MA provider network? Our approach to these two questions provides a first order, conservative measure of the existence of market power in the provider group market.

Data and Methods

Data

We focused our analysis on local CCPs. These are MA contracts that are required to maintain a provider network and offer prescription drug benefits. We further restricted our sample to contracts that are open for general enrollment and those that were either PPOs, HMOs, or HMO-POS plans. Under the MA program, insurers can also offer plans with restricted enrollment. These include special needs plans, demonstration plans, and employer plans, all of which restrict enrollment to specific populations. We excluded these plans from our analysis because they are not open for general enrollment. We identified plan characteristics and the counties within their service areas from the CMS plan characteristics files and CMS service area files.

A major challenge with identifying physicians who participate in MA provider networks is the quality of provider directories.[19] These directories often include inaccurate information, and are not always up-to-date.[31,51] While machine-readable directories have been made available and used in research,[16,17,26] these directories' quality has generally not been assessed, and they are not available for many years of data, limiting the opportunity to conduct longitudinal analysis. In this analysis, we build on prior work[18,52] that relied on interactions between prescribing physicians and beneficiaries to identify physicians who are likely to be in-network. Because MA encounter data is similarly unavailable for many years and is of uncertain quality,[35,53] we relied on prescription drug claims to identify providers who are potentially in-network for MA plans. These data are available for both TM enrollees in standalone PDPs and MA enrollees who are in MA plans that offer prescription drug coverage (MA-PD plans). The key insight is that interactions between a prescriber and a beneficiary may indicate that the provider is in-network for the beneficiary's plan (some observed interactions may be out-of-network, which is generally difficult to verify).

We focused on PCPs who prescribed to Medicare beneficiaries in the years of our analysis (2011-2017) and in any of the 50 states or the District of Columbia. PCPs were identified in the MD-PPAS data if their primary specialty was general practice, family practice, internal medicine, or geriatric medicine. We excluded physicians who were either flagged as primarily hospital providers in MD-PPAS or those who had more than 90% of claims in an inpatient hospital-based setting. Prescribing activity was identified based on the Medicare Part D PDE 20% sample.

The 20% Fee-for-Service (FFS) Carrier File was used to identify outpatient TM interactions with NPIs, and the MBSF was used to identify patient county of residence.

Methods: NPI to Provider Group Assignment

Because we hypothesized that network participation would likely be a practice group characteristic, we sought to aggregate national provider identifiers (NPIs) for individual providers under their respective provider groups. We identified provider groups using tax ID numbers (TINs), that generally represent the business entity under which the NPI is billing for services. We assigned prescriber NPIs to TINs using the FFS 20% Carrier File.

Each NPI was assigned a single TIN but was allowed to vary by county (e.g., an NPI could be assigned to a TIN in one county, and another in a different county). Following prior work,[54,55] where a prescribing NPI could be assigned to multiple TINs within a county, we used the NPI-TIN combination with the majority of beneficiaries to make the TIN assignment. (83% of NPIs only operated under a single TIN, while an additional 7 percent had more than one TIN, but one had more than 50% of beneficiaries. The remaining NPIs didn't have a single TIN accounting for more than half of their beneficiaries.) After performing the TIN assignment, we identified the total number of unique TM beneficiaries that were seen by each TIN in each county from the 20% Carrier File. The county used was the beneficiary's county of residence. Note that this allows for a TIN's service area to extend beyond a single county, as it is based on where beneficiaries travel from. This allows for us to be indifferent as to where provider offices are actually located.

Methods: Demand Measure Creation

Our goal was to estimate the degree to which provider groups (TINs) were disproportionately used by TM beneficiaries. While one might simply examine market shares of provider groups, this would be misleading, as it could simply be a function of the size of the provider group. An alternative counterfactual, assuming no product or provider differentiation, might be that a TIN's share of beneficiaries in a county is similar to its size in the county. Thus, we wanted to compare whether a provider group's market share of beneficiaries was *relatively larger* than the provider group's size, where provider group size is their share of NPIs in a given market. A relatively larger market share of beneficiaries would suggest that there are some features – quality, marketing, reputation, or prices for instance – that led beneficiaries to disproportionately use that provider group.

We focused on TM beneficiaries to establish a measure of demand for providers for two reasons. First, this allowed us to approximate what demand for providers looks like assuming no network restrictions (analogous to the modeled market shares in prior work).[21] Second, by calculating demand for a population unaffected by network restrictions (the TM population), we avoided endogeneity issues with estimating demand based on MA utilization.

To develop this measure, we first identified the number of unique NPIs assigned to each TIN within each county, and the total number of unique NPIs seeing TM beneficiaries from each county from the 20% Carrier File. For a given TIN-county observation, we divided the total number of unique NPIs assigned to each TIN by the total number of NPIs serving beneficiaries in that county. Equation 3.1 shows this calculation, where p is a provider group, t is a year, and c is a county.

Eq. 3.1 $$Denominator_{ptc} = \frac{NPI\ Count_{ptc}}{\sum_{\forall p \in tc} NPI\ Count_{ptc}}$$

Then, to develop the numerator, we calculated the number of unique beneficiaries receiving outpatient care from the TIN (based on outpatient interactions with the assigned NPIs) as a share of all beneficiaries residing in the county. Equation 3.2 shows this calculation, where p is a provider group, t is a year, and c is a county.

Eq. 3.2 $$Numerator_{ptc} = \frac{Beneficiary\ Count_{ptc}}{\sum_{\forall p \in tc} Beneficiary\ Count_{ptc}}$$

Our measure of demand was then generated as a ratio of $Numerator$ to $Denominator$. All calculations above were based on the 20% Carrier File and the MBSF. NPI counts and beneficiary counts above were restricted to NPIs who prescribe and provide outpatient services, as well as beneficiaries who receive outpatient services. A higher value of this measure indicates disproportionate demand. Additionally, based on prior work,[21] we considered a definition of star provider to be when the TIN was in the 90th percentile of our measure of disproportionate demand.

Following Feyman et al. (2019),[18] TINs were considered to be contracting with MA if we identified at least one NPI associated with the TIN that prescribed to any beneficiary enrolled in one of the MA contracts included in our sample. This was based on prior work measuring MA provider networks in a similar way.[18] To probe the sensitivity of our results to prescribing volume, we calculated additional indicators of network inclusion that required more than one and more than two prescriptions. We excluded any TINs for which we could not identify any prescriptions to either TM or MA beneficiaries but allowed for TINs that only prescribed to TM beneficiaries.

Methods: Comparing Star Providers and Other Providers

To examine the difference in several important market characteristics and measures of volume between star and non-star providers, we first calculated standardized mean differences (SMDs) and conducted two-sample t-tests between star and non-star provider groups.

Next, we sought to examine more thoroughly the relationship between disproportionate demand for a provider group and the likelihood of that provider group being in-network for at least one MA contract. To do so, we relied on a linear probability model (Equation 3.3) estimated via ordinary least squares, predicting a TIN being in-network for any MA plan in a given county-year, with the measure of disproportionate demand on the right-hand side.

Eq. 3.3 $$y_{ict} = \beta_0 + \beta_1 Demand_{ict} + Year_t + \varepsilon_{ict}$$

Here, y is a binary indicator for whether the TIN (i) is considered in-network for at least one MA contract in county c and year t. The coefficient β_1 indicates the relationship between a change in the $Demand$ measure (as defined in the data section) and the probability of being in-network for at least one MA contract. $Year$ indicates a vector of year fixed effects, and ε is an error term that is clustered at the TIN level. Separately, we estimated Equation 3.1 with $Demand$ replaced with a binary measure of an observation being a star provider. We considered versions of Equation 3.1 with county and county + TIN fixed effects as well.

Sensitivity Analysis

A key assumption in our analysis was that an interaction between an NPI and a beneficiary represented an in-network interaction. Additionally, if star providers are simply more likely to prescribe to beneficiaries than other providers, then we might simply be more likely to identify these providers in prescription drug data.

To assess the degree to which these assumptions and limitations affected our results, we considered alternative measures of a TIN being in-network that require more than one, two, and three prescription events respectively. Results being consistent across these different specifications would suggest that these limitations are not strong enough to affect our overall results.

Results

Our analytic dataset included 78,800 unique TINs, and 1,538,397 TIN-year-county combinations. On an unweighted basis, 31.8% of TINs were considered to be in-network for at least one MA contract in a given county. Nationally, this rose to 81% of TINs being in-network for at least one MA contract across the country, which was larger than prior work estimating that 58% of PCPs were in-network for at least one MA contract.[16] On average, the measure of disproportionate demand was 1.41 (SD: 2.78), indicating that the included TINs tended to interact with 41% more beneficiaries than one would expect given the number of NPIs billing under the TIN in a given county-year.

There were large differences between TINs considered to be a star provider in a given year as compared to those that were not. There were 153,924 TIN-county-year observations that were classified as star providers, accounting for 39,536 unique TINs. These TINs were much more likely to be in-network than those that were not (81.04% vs 26.3%, SMD: 1.31). Moreover, while these TINs accounted for a somewhat smaller share of NPIs in a given county-year (0.79% vs 1.2%, SMD: 0.13) they accounted for a substantially larger share of beneficiaries in a given county-year (5.69% vs 1.14%, SMD: 0.57). While these providers prescribed more (26.36 PDEs vs 9.16 PDEs, SMD: 0.37), the number of PDEs per beneficiary was lower (0.31 vs 0.95, SMD: 1.5). All differences were statistically significant. Surprisingly, there was a small difference in local MA penetration in areas with star providers (27.38% vs 26.39%, SMD: 0.07), suggesting that baseline MA enrollment doesn't explain differences between providers. (Table 3.1)

Table 3.1. Summary Statistics

	Overall (N=1,538,937)		Star (N= 153,924)		All Others (N=1,385,013)		SMD
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	
Demand	1.41	2.78	8.00	4.81	0.68	0.82	2.12
In Network (%)	0.32	0.47	0.81	0.39	0.26	0.44	1.31
TIN NPI Share (%)	0.01	0.04	0.01	0.02	0.01	0.04	0.13
TIN Beneficiary Share (%)	0.02	0.06	0.06	0.10	0.01	0.05	0.57
MA-PD PDEs	2.22	14.37	5.24	11.51	1.88	14.61	0.26
PDP PDEs	8.67	35.88	21.13	34.58	7.28	35.75	0.39
Total PDEs	10.88	48.57	26.36	44.15	9.16	48.74	0.37
Total PDEs per Beneficiary	0.89	0.57	0.31	0.20	0.95	0.57	1.5
65+ Population	93284.99	175283.03	121630.50	215652.20	90129.61	169905.795	0.16
TIN NPI Count	2.21	6.58	1.85	3.19	2.25	6.86	0.08
County NPI Count	892.37	1164.23	1087.45	1348.46	870.69	1139.87	0.17
Total County No. Contracts	6.13	5.22	6.80	5.61	6.06	5.17	0.14
TM County Average HCC	1.00	0.10	1.02	0.11	1.00	0.10	0.20
MA Penetration (%)	26.49	13.94	27.38	14.10	26.39	13.92	0.07

Notes: Analysis of TIN-county-year level data. Beneficiary share indicates the share of beneficiaries residing in a county who received services from an NPI in the given TIN. NPI share indicates the share of NPIs assigned to a given TIN out of all NPIs providing care to beneficiaries in a given county. Star-provider indicates the ratio of beneficiary share to NPI share. A two-sample t-test allowing for unequal variances indicated that all differences were statistically significant at $p<0.001$. SMD: Standardized Mean Difference. TIN NPI count refers to the number of unique NPIs assigned to a TIN. County NPI count refers to the total number of unique NPIs in a county. Total County No. of Contracts refers to the total number of MA contracts operating in a county.

The relationship between disproportionate demand and probability of being in-network was consistent across variations of Equation 3.1. Regardless of what fixed effects were included, the results suggest that a one-unit increase in disproportionate demand was associated with a 7-percentage point (SE: 0.06) increase in the probability of a TIN being in-network. Similarly, being a star provider was associated with between a 53.2 percentage point (SE: 0.2) and a 58.3 percentage point (SE: 0.2) increase in the probability of being in-network. (Table 3.2) Allowing for in-network status to be defined based on a greater number of PDEs did not materially change these results. (Appendix B.1).

Table 3.2. Relationship Between In-Network Probability and Star Provider Status

Demand Model	0.07*** (0.0006)	0.07*** (0.0006)	0.07***(0.0006)
Star Indicator	0.547*** (0.002)	0.532*** (0.002)	0.583*** (0.002)
Fixed Effects	Year	Year, County	Year, County, TIN
N	1,538,937	1,538,854	1,533,786

Notes: Outcome is the probability of an observation being in-network. "Demand Model" indicates that the predictor is the measure of disproportionate demand. "Star Indicator" indicates that the predictor is an indicator for being a star provider group. Standard errors clustered at the TIN level in parentheses.
*** $p<0.001$, ** $p<0.01$, * $p<0.05$

Discussion

In this novel analysis of MA network dynamics, we identified the presence of star provider groups – those that attract a disproportionate share of beneficiaries relative to the number of clinicians – that are more likely to be part of MA provider networks than other provider groups. Specifically, a star provider group had a 50% higher probability of being in-network as compared to other provider groups.

Notably, the inclusion of these provider groups in-network does not appear to be solely due to volume of prescriptions, the presence of MA beneficiaries in the county, or due to the size of the provider groups. Instead, it appears to be strongly linked to beneficiary demand. This might suggest that these providers offer higher-quality care, and thus are able to attract more beneficiaries. Alternatively, such providers might simply be "brand-name" providers, such as those affiliated with well-known academic medical centers, or they might simply be easier to access (via bus or metro, or with convenient parking).

Regardless of the reasons for this "star" status, our findings suggest that MA provider networks might have limited flexibility in some areas. While there are necessarily limits to provider network construction due to network adequacy requirements, the existence of star provider groups implies that beneficiary demand for particular providers also plays a moderating role in how these networks are established. It is likely that these star providers are able to differentiate themselves and the services they offer, leading to disproportionate demand among Medicare beneficiaries.

Our work complements previous analyses of star hospitals [21,48]and their role in network formation. This prior work found that star hospitals appear to differentiate themselves from other hospitals (via higher-quality care, and more high-tech services), commanded higher market shares, but also led to adverse selection against plans including them in their networks. While we were unable to examine the quality or types of services offered by the star providers that we identified, it is possible that similar dynamics play out in the provider market.

Future work should investigate whether there are providers that participate in *all* networks in an area. This might suggest substantial market power or other approaches to differentiation. Additionally, more work to identify potential harms or benefits accruing from star provider groups could be useful. For instance, if such groups are higher cost and perform more procedures on beneficiaries without higher quality, then that would suggest a need for better regulatory oversight, and potentially more stringent antitrust enforcement. Alternatively, if these provider groups are indeed higher quality, understanding whether insurers including them in-network face adverse selection would be critical.

Limitations

This analysis faces several limitations. First, because we do not observe actual contracted networks, nor do we observe non-prescription interactions between providers and MA beneficiaries, our measure of a provider being in-network may fail to capture all providers, and it may capture providers that are potentially out-of-network. However, sensitivity analyses suggested this was unlikely to significantly affect our results.

Second, we use measured utilization to identify a provider group being both in-network and to measure disproportionate demand for the provider group. This might raise reverse causality concerns. However, as noted in the methods section, because our measure of disproportionate demand is restricted to TM beneficiaries, while our measure of being in-network is based on MA beneficiaries, we do not believe this remains a concern.

Third, because we require interactions with TM beneficiaries to identify star provider groups, our sample of provider groups likely already represents providers that are already more attractive to beneficiaries. This limits generalizability to all other provider groups.

Lastly, this is an observational study. We don’t rely on exogenous variation in our measure of disproportionate demand. Thus, our results don’t imply a causal relationship. Instead, our results are descriptive and should be seen as a method by which one could measure disproportionate demand (and potentially identify star providers) without relying on a discrete choice model.

CHAPTER FOUR: MEDICARE ADVANTAGE PASS-THROUGH: BENEFITS AND PROFIT-SEEKING BEHAVIOR

Introduction

The MA program reflects a common decision faced by governments in how best to deliver public benefits. On the one hand, provision of health insurance directly by government actors can ensure standardization, potentially minimize inequities, and may leave less room for gaming and other untoward behavior. Moreover, it can minimize the costs associated with excessive choice, such as errors in plan choice.[58] On the other hand, allowing health insurance to be offered by private actors can help plans better reflect preference heterogeneity, and potentially avoid inefficiencies that might arise in the case of a single monopoly insurer.[59]

In today's MA program, the U.S. government pays insurers a monthly capitated amount to deliver Medicare coverage with some constraints. Economic theory predicts that these insurers will maximize profits. In a competitive market with a zero-profit equilibrium, insurers would bid the average cost of providing coverage. However, imperfect competition would lead to a variety of strategic behaviors. A strategic insurer might maximize benefits that attract more profitable enrollees and minimize benefits expected to attract less profitable enrollees, for instance.[60] This favorable selection has been well-documented,[3,61] though it appears to have declined over time (partly due to improved risk-adjustment).[62] Nonetheless, MA insurers still face unique incentives to strategize with respect to benefit and plan design.

Because nearly all plans are required to maintain provider networks (one-tenth of a percentage point of enrollees are in plans without networks),[7] and the most popular plans offer both medical and prescription drug coverage (91 percent of enrollees are in such plans),[56] an insurer might trade off maximizing one set of benefits (e.g., drug benefits) at the expense of others (e.g., medical benefits). Similarly, they may also strategically design or modify provider networks to achieve similar goals.

Relatedly, it is ambiguous how much of the government's payment flows to insurer profit and profit-seeking activities (such as advertising) versus benefits that improve beneficiary access to care or health outcomes. This underpins important concerns that have been raised about the program, particularly as enrollment has increased.

Initially, MA maintained a relatively small share of enrollment among all Medicare beneficiaries. This was partly because the federal government paid MA insurers less than the average cost of individuals covered by TM, leading to low enrollment and favorable selection. However, changes to MA payment policy over the past two decades have incorporated risk adjustment and have increased payments leading to increased enrollment in plans offered by private insurers.[63] The ACA further tied payments more directly to TM costs and provided bonuses for plans that achieve minimum levels of quality.

As popularity of MA has grown over the past decade, regulators, researchers, and patient advocates have criticized the program for leading to overpayments to MA plans,[64] often due to coding practices that make patients appear sicker for risk adjustment ("upcoding");[65] inaccurate provider directories,[19,20] leading to delays in care for patients;

favorable selection of healthier patients into the program;[66] and concerns about misleading and aggressive advertising by plan sponsors.[67]

The degree to which the program delivers benefits despite these concerns is frequently disputed. The evidence on health outcomes suggests some patients may receive more appropriate care under MA. For example, patients with coronary artery disease were more likely to receive evidence-based treatment in MA,[68] while other work found higher readmission rates for MA patients with three common conditions compared to TM patients.[69] Overall, however, a growing base of evidence suggests that MA enrollment is associated with better process measures, and somewhat better performance on health outcomes. A recent systematic review of the MA outcome literature found that MA appeared to be associated with lower spending, increased use of preventive care, and lower rates of preventable hospitalizations.[4] A limitation in much of the work, however, is the inability to account for favorable selection into MA or for upcoding by MA plan sponsors.

These concerns, combined with the lack of clarity on whether MA delivers an equal or better beneficiary experience and/or health outcomes compared to TM suggest an ongoing need to better understand the effectiveness of paying private insurers to deliver a public benefit. Some existing work has sought to understand how much of changes in MA payments lead to improved benefits for beneficiaries, yielding a wide range of estimates. In one reduced-form analysis, relying on changes to payment rates to plans under the ACA through 2015, Pelech and Song[25] estimated that plans pass-through some 60% of payment changes to enrollees in the form of benefit changes such as lower cost-sharing for certain services. Other work by Curto et al.[70] found that about two-thirds of surplus generated by the program is captured by insurers. Similar work by Colleen Carey has found that about 40% of government subsidies are passed through to enrollees as lower out-of-pocket costs,[71] work by Cabral et al. estimated that pass-through varies with the competitiveness of the market and on average is about 9% for benefit generosity,[23] and Duggan et al.[24] estimated a pass-through of around 12.5%.

There are several key limitations of existing work. First, with the exception of Pelech and Song, no other work has examined a period of time in which the ACA changes to MA payments have been fully implemented. Second, in much prior work, analyses have relied on a somewhat narrow scope of identification strategies, potentially limiting generalizability. Lastly, the existing work has generally not focused on other possibilities for pass-through – in particular, whether payments translate to increased advertising effort (a profit-generating activity) or provider networks (a less visible, but highly-salient element of plan configuration).

In this analysis, we combined publicly-available data on MA plan benefits and offerings from 2012 through 2017 with advertising data and novel measures of provider network restrictiveness.[52] Relying on a natural experiment occurring in 2015, we estimate a total payment pass-through of 69%, with the majority flowing through to benefit generosity and then premiums. We estimate an imprecise null for pass-through in the form of provider network restrictiveness and advertising.

Data and Methods

Medicare Advantage Payment System

Our empirical strategy, described further below, relies on exogenous variation driven by a change to the MA payment system in 2015. In this section, we describe the payment system and how it has evolved.

MA insurers receive monthly capitated payments to provide Part A and B services to beneficiaries, and an increasing number of plans also provide drug coverage through the Part D program (for which they receive a separate capitated payment). Products are structured as contracts nested under an insurer, which operate in multiple counties, and can offer unique plans (with varying benefits and premiums) within a contract. An insurer can have multiple contracts.

Initially, capitated payments were linked to a lagged, 5-year moving average of modeled TM spending. Over time, some risk adjustment accounting for demographics and health status (through the Hierarchical Condition Category [HCC] model) was implemented. Starting in 2006, insurers were required to bid to offer coverage to beneficiaries. These bids were intended to represent the cost of covering a beneficiary of "average health" (e.g., an HCC risk score of 1.0). Bids were compared to a statutorily calculated benchmark – if they were above the benchmark, insurers would receive the benchmark and would have to charge premiums over the standard Part B premium. Bids that were below the benchmark led to an additional rebate of varying size, with the intent of increasing benefit generosity and offering supplemental benefits.

The ACA modified how the benchmark was calculated beginning in 2012.

Counties would now be ranked on a lagged measure of average TM spending per beneficiary (which is updated [or "rebased"] at least every three years). Counties in the lowest quartile of lagged TM spending receive an applicable percentage of 115% of a modeled estimate of current TM spending, those in the second quartile receive 107.5%, counties in the third quartile receive 100%, and those in the fourth receive 95%. Counties changing quartiles over time receive the average of the two applicable percentages for 1 year as a transition. This applicable percentage is then multiplied by a modeled estimate of current TM spending to obtain the benchmark.

Additional components of the benchmark include: a county-specific phase-in period that weighs the pre-ACA calculation and the post-ACA calculation; quality bonuses for plans, with up to a five-percent bonus for highly-ranked plans; and lastly, a cap by what the benchmark would have been under pre-ACA calculations.[72]

Equation 4.1 illustrates this payment formula:

Eq. 4.1
$$Benchmark_{it} = \begin{cases} (App\%_{it} \times E[TMSpend_{it}] \times \sigma) + \left(OldRate_{it} \times (1-\sigma)\right) \; if \; OldRate_{it} > Benchmark_{it} \\ OldRate_{it} \; if \; OldRate_{it} \leq Benchmark_{it} \end{cases}$$

Where $Benchmark_{it}$ is the non-bonus benchmark for county i in year t, $App\%_{it}$ is the applicable percentage from ranking counties by quartiles, $E[TMSpend]_{it}$ is the modeled contemporaneous TM cost, σ is the phase-in factor, and $OldRate_{it}$ is the pre-ACA rate calculated for the current year. For benchmarks with bonuses, the payment formula is analogous to Equation 4.1, but as with the standard benchmark, if a bonus payment brings the benchmark higher than the pre-ACA rate, the pre-ACA rate still acts as a ceiling.

The bonus payments to plans mentioned above were enacted under the ACA, with the size of the bonus tied to a measure of contract quality, the contract's star rating. Bonuses were intended to be phased in, reaching five percent by 2014 for contracts with four or more stars out of five, and a 3.5 percent bonus for new plans. Instead, from 2012 to 2014, the quality bonus payment (QBP) program was modified to act as a temporary demonstration, increasing bonus payments to plans. Bonuses were set to five percent for contracts with four stars and up by 2014; 3.5 percent for contracts with 3.5 stars by 2014; three percent for contracts with three stars by 2014; and 3.5 percent for new contracts by 2014. Appendix C.1 illustrates the bonus payments under the QBP demo as well as under the ACA. Bonuses reverted to ACA levels after 2014. We relied on the end of the QBP demo, which shifted 3.5- and 3-star contracts back to receiving no bonuses as the source of exogenous variation for our analysis.

Dataset Construction

We obtained several data sources for our analysis. These included: MA contract service area files, monthly enrollment files, ratebook files, payment files, and plan bid tool (PBT) files, all of which are publicly available from CMS. Additionally, we obtained the Medicare Out of Pocket Cost (OOPC) data through a request from CMS. We obtained measures of network restrictiveness from Feyman et al. 2023[52] for primary care providers, and MA advertising data from the Wesleyan Media Project (WMP) under license from Kantar/Campaign Media Analysis Group. Lastly, we incorporated county-level data from the AHRF maintained by the Health Resources and Services Administration.

To compile our analytic dataset, we first identified the set of MA contracts that were of interest (those that are available for enrollment among all beneficiaries and offer drug coverage) through CMS monthly enrollment data by contract-plan-county. Because MA enrollment data is publicly available at the monthly level, but our analytic dataset varied at the year level, requiring us to choose a representative month of enrollment. We selected June following prior work.[18] While many observations are missing enrollment data, this is due to CMS censoring enrollment counts that are less than 10 and thus represents very small plans. Our focus were local CCPs that represent the majority of MA enrollment. We excluded employer plans, special needs plans, and those that did not offer drug coverage. Though our final sample of contracts represented 56.5 percent of enrollment years across all MA plans, it accounted for 81 percent of enrollment years among plans open to general enrollment (employer plans and special needs plans have restrictions). (Appendix C.1) We then merged this data at the contract-county-year level with information on each contract's service area and excluded enrollment in counties that were not in the contract's service area. Enrollment outside of a contract's official service area might occur due to data reporting errors or because individuals might move during the year.

To measure payments to plans and the statutory benchmarks that determine payments to plans, we used the CMS ratebook files and plan payment data. The ratebook files provided the statutory benchmark and its components at the county-year level. The plan payment data provide the plan membership's average risk score as well as payments and rebates accruing to the plan, across all service areas. This data is at the contract-plan-year level and does not vary by county. We merged this data with the service area and enrollment files at the contract-plan-year level.

We obtained quality measures of MA contracts from the PBT files. These data provide plan-submitted bids to offer coverage for each contract-year. The star rating included in these bid files are the lagged measures that are used to determine payment for a given year rather than contemporaneous measures. These data vary at the contract-year level.

Benefit generosity was measured as the expected out of pocket cost to enrollees in a given plan, similar to actuarial value measures used in prior work.[23] We operationalized this using the Medicare Out Of Pocket Cost (OOPC) data maintained by CMS. The OOPC measure is generated through CMS software designed to estimate the out-of-pocket spending that a hypothetical beneficiary of average risk would be expected to spend in the plan, with the Medicare Current Beneficiary Survey providing the data on beneficiary utilization.[73] A higher value indicates lower benefit generosity. These data are provided at the contract-plan-year level, and do not vary by county. These data also include premiums for each observation and allowed us to measure both the total premium charged by the plan (including the base Part B premium) as well as whether a given plan charges any additional premium above the Part B premium. We merged this with other data at the contract-plan-year level.

Measures of network restrictiveness obtained from Feyman et al. 2023[52] were calculated based on prescription drug utilization among MA and TM beneficiaries. The unique number of primary care physicians prescribing to MA beneficiaries and TM beneficiaries with standalone prescription drug coverage were obtained for each county-year-contract-plan type. A prediction model was trained on TM beneficiary utilization and predicted number of unique physicians were generated for MA observations as a counterfactual estimate assuming no plan design or network restrictions were applied. This generated an observed-to-expected ratio for MA observations where higher values indicate less restrictive networks. These data are available at the contract-year-plan type-county level and were merged with other data at the same level.

Health insurance advertising obtained from WMP was at the individual ad level. Each county is uniquely assigned to one of 210 U.S. media markets, by proportion of the county population in each media market if a county is not wholly contained within one. After cross-walking media markets to counties, we aggregated these data to the parent company-county-year level, restricting only to ads focused on Medicare based on content coding previously done by WMP.[74] Advertising volume was calculated relative to the number of individuals 65 years and older in the county. Where we identified no advertising, we set the number of ads to zero. Additionally, we calculated the total number of all Medicare-focused ads by other sponsors in a given county relative to the number of individuals 65 years and older in the county to include as a covariate.

Lastly, we obtained county-year level measures of socio-demographics from the AHRF. These variables included: the TM average risk score, the share of individuals over 65 residing in the county, median income, the number of physicians and the number of hospital inpatient beds per 1,000 population, the poverty rate, and the share of residents who are male. All variables have previously been associated with MA and managed care market entry and costs, as well as with health care utilization and costs.[42,72,75,76] A flowchart of dataset construction is presented in Appendix C.2. The level of variation of all key outcomes and predictors is presented in Appendix C.3.

Outcomes

There were six primary outcomes we were interested in: monthly contract-plan specific benchmarks ($) (e.g., those based on the contract's quality rating), contract-plan monthly premiums ($), the probability of a zero premium plan (%), monthly contract-plan generosity ($), advertising (measured as the number of parent company ads per 100 65+ population), and network restrictiveness (an observed-to-expected ratio indicating the degree to which MA networks restrict access to primary care providers).

Each of these outcomes represents a potential channel for MA insurers to either change benefits for enrollees and/or increase profits. We include these specific outcomes for several reasons: the effect of the policy change on benchmarks represents a "first stage," which would drive changes in other measures; increased premiums and reduced benefit generosity represent an increase in costs to beneficiaries (and therefore a reduction in benefits); zero premium plans are a common tool used by MA insurers to make plans appear more attractive, and account for a large share of enrollment (Table 4.1);[2] advertising effort may reflect profit-seeking activity, suggesting pass-through to other measures rather than benefits; lastly, increasing network restrictiveness represents an opportunity for insurers to reduce costs in a way that is less visible to beneficiaries.

Methods

Our primary research question was focused on understanding the extent to which payments to MA insurers affect benefit generosity, premiums, advertising effort, and provider networks. We hypothesized that given imperfectly competitive markets, there would be less than a dollar-for-dollar pass-through of payments to benefits, premiums, and provider networks. We expected a non-zero amount of pass-through to advertising effort, despite prior evidence that MA advertising appears to be somewhat ineffective.[77]

Eq. 4.2 $$Y_{ipct} = f\left(Pmt_{ipt}, {X'}_{ipct}, W'_{it}, {M'}_{ct}\right)$$

Equation 4.2 represents the general form of the relationship we sought to estimate between our outcomes, Y for contract i, plan p, county c, in year t. X' is a vector of contract-plan characteristics that vary by county and year, W' is a vector of contract characteristics that vary by year, and M' is a vector of county characteristics that vary by year. Our interest was in the relationship between payment (Pmt) and each of our outcomes.

A key challenge to estimating this relationship is the potential for endogeneity. While we can account for a rich set of covariates, there are likely to be unobserved confounders correlated with plan payments and each of our outcomes. For instance, benefits, premiums, and provider networks are likely to be configured both in response to expected payments, but also based on insurer effort, intensity of competition, and underlying health status in the market. The same is likely to be true for advertising. Prior work has relied on various strategies to identify exogenous variation in plan payment rates. These have included county-border discontinuities,[23,78] statutory benchmarks,[79] and simulated benchmarks.[72,79] As the structure of the MA market and the calculation of payments has changed over time, these approaches have become less applicable for several reasons. County-border discontinuities empirically identify less variation in payment rates than they previously did, particularly with the inclusion of fixed effects; statutory benchmarks after implementation of the ACA are less defensibly exogenous, and thus may lead to biased estimates; and as the ACA payment rates have been fully phased-in, simulated benchmarks used in previous work also identify little remaining exogenous variation.

Our approach relies on the abrupt end of the QBP demonstration and reversion to ACA-specified payment rates in 2015. While the QBP demonstration was intended to end after 2014, payers appeared to believe that CMS had authority to extend it and asked CMS to do so.[80] Thus, while some payers may have expected the program to end, it is likely that many also believed it would be extended.

We operationalize this with a two-way fixed effects (TWFE) difference-in-differences (DID) estimation strategy. Equation 4.3 provides our estimating equation:

Eq. 4.3 $$Y_{ipct} = Post_t \times Treat_p + Post_t + Treat_p + \varepsilon_{ipct}$$

Where $Post_t$ indicates whether the observation is before or in/after 2015 and $Treat_p$ indicates whether the contract faced a reduction in bonus payments. ε_{ipct} is an error term that we allow to be serially correlated within contract. Our coefficient of interest is $Post_t \times Treat_p$, which indicates the average treatment effect (ATT) of the QPB demonstration ending. Y_{ipct} takes the value of the outcomes we are interested in: monthly benchmarks ($), monthly premiums ($), probability of a zero premium plan (%), monthly plan generosity ($), advertising (parent company ads per 100 65+ population), and network restrictiveness (an observed-to-expected ratio indicating the degree to which MA networks restrict access to primary care providers). While benchmarks are county-year level, we construct a contract-county-year specific benchmark based on the contract-year star rating.

A DID strategy assumes that outcomes for treated units would follow the same path as those for control units, but for the treatment occurring at a particular point in time. To justify this conclusion, two primary assumptions are necessary. The first of these is often referred to as "parallel pre-trends." This simply requires that outcomes before treatment follow a parallel path that would have plausibly continued during the post-period in the absence of treatment.[81] Because testing for parallel pre-trends is often conservative and might be biased towards the null, we estimate event study specifications to visually investigate pre-trends. The second assumption, which is generally untestable, is often called "common shocks."[82] This assumption requires that any other changes except the treatment under investigation affect both treated and control units similarly. While we cannot test this explicitly, there are good reasons to believe that it holds. There were no other relevant policy changes occurring in 2015, coincident with the end of the QBP demo that might have differentially affected treatment and control contracts. Additionally, we include a variety of fixed effects to further address potential violations of this assumption.

We rely on a simple TWFE specification because the treatment we study happens at a single point in time and is not staggered.[83]

Estimation Approach

To estimate Equation 4.3, we first identified treated and control contracts. Because star ratings are assigned at the contract-year level, we assigned contracts to be treated if, in 2015, they were being paid based on a 3.5- or 3-star quality rating. This indicates that their bonus payments fell in 2015. Control contracts were those that were 4, 4.5, and 5-star contracts in 2015. Because 5-star contracts have unique features (beneficiaries can switch to a 5-star contract once at any point during the year without penalty)[84], we examined the sensitivity of our results to exclusion of these contracts.

We varied each specification to include or not include controls for baseline covariates. All specifications included county, contract, year, and parent company fixed effects, as well as indicators for plan type (HMO, PPO, or HMO-POS). We used contemporaneous enrollment as an analytic weight in all specifications and clustered standard errors at the contract level. Note that because we estimate Equation 4.3 with contract and year fixed effects, only the interaction remains in our estimate. All analyses were restricted to a balanced panel of contracts. Event-study estimates were conducted with and without baseline covariates, and without contract fixed effects (as they would be collinear with the treatment indicator).

Results

There were 70,524 contract-county-year-plan type observations in our data with non-missing enrollment data, spanning from 2012 to 2017. 18,457 observations were dropped to ensure a balanced panel, 13,307 observations were excluded that either entered or exited treatment after 2015. Our final analytic sample included 38,760 observations — 6,900 were treated and 31,860 were control observations — accounting for 34,126,147 enrollment years. Key summary statistics of variables used in our modeling are presented in Table 4.1.

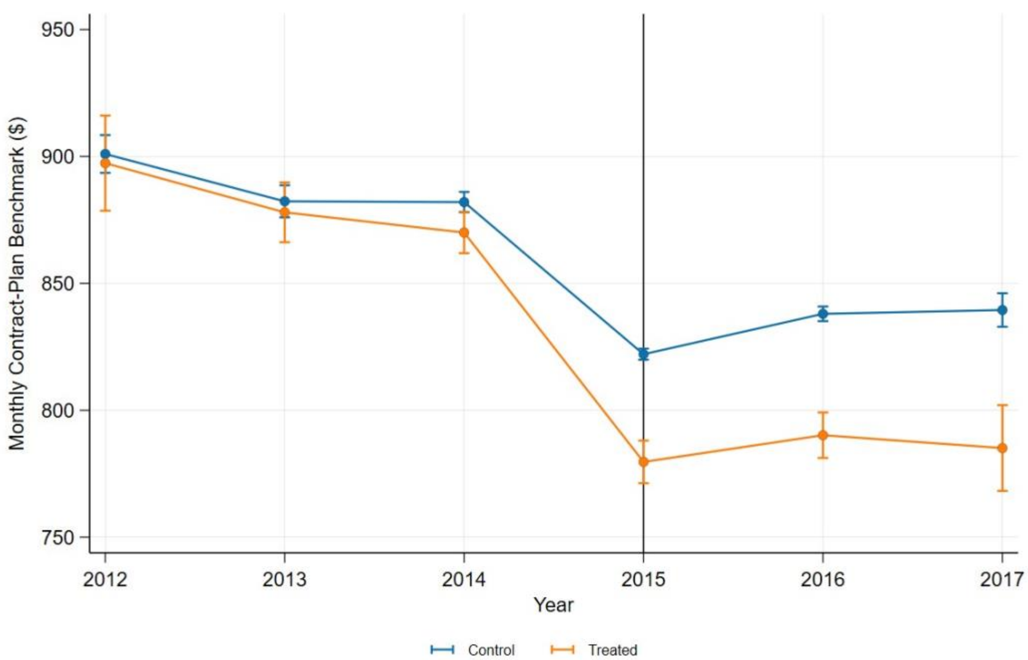
Table 4.1. Summary Statistics

Variable Name	Mean	Standard Deviation
Outcomes		
Contract-Plan-Specific Benchmark	851.04	77.46
Total Premium	142.19	55.80
Zero Premium (%)	0.50	0.50
OOPC	207.03	46.81
Advertising Per 100 Population Over 65	0.95	6.04
Network Restrictiveness	58.09	26.83
County-Level Factors: Covariates		
MA Benchmark	811.82	76.24
MA Benchmark With 5% Bonus	859.16	79.23
Average TM HCC Score	1.03	0.10
% Population >65	15.20	3.72
Pre-ACA Benchmark	888.88	103.62
Bonus County (%)	0.30	0.46
Ln(Median Income)	10.91	0.29
Physicians Per 1k Population	2.87	1.84
Beds Per 1k Population	2.09	1.50
Poverty Rate	14.02	4.30
% Male	49.17	0.96
Modeled TM Per Capita Cost	775.49	102.09
Rebased TM Per Capita Cost	765.12	107.15
Capped MA Payment Rate (%)	0.04	0.02
Contract-Level Factors: Covariates		
No Advertising (%)	0.45	0.50
Other Ads Per 100 Population Over 65	4.29	15.55
Average Contract Risk Score	1.02	0.17
Contract Star Rating	4.09	0.57
Contract-Plan-County Enrollment	11,550.44	20,218.30

Notes: Data are weighted by enrollment and averaged across all years of data. Pre-ACA benchmark indicates the benchmark as it would be calculated under rules pre-ACA. N=38,760

Figure 4.1 presents results of an event-study-like plot generated from a regression of contract-county specific benchmarks on the interaction of year and treatment group. These results indicate that the effective monthly benchmark from $882.02 (95% CI: $ 878.03 to $886.01) in 2014 to $822.08 (95% CI: $819.92 to $824.24) in 2015 among contracts unaffected by the bonus payment reduction versus $870.03 (95% CI: $861.90 to $878.16) in 2014 to $779.81 (95% CI: $771.61 to $788.01) in 2015 among contracts that saw bonuses fall. The pre-trends in Figure 4.1 suggest that the requirement of parallel pre-trends is likely satisfied in this analysis. Appendix C.4–C.8 present similar plots for all other outcomes, also indicating that pre-trends are visually consistent with the parallel trends assumption. Trends were largely similar when including baseline covariates in the model (Appendix C.11–C.15).

Figure 4.1. Monthly Benchmark Change Over Time



Notes: Estimates are from a regression interacting year and treatment group indicator. Regression includes county fixed effects, plan type fixed effects, and parent company fixed effects, with standard errors clustered at the contract level. Regression is weighted by enrollment. Treated: contracts with 3 or 3.5 stars in 2015 (N=6,865); Control: contracts with 4, 4.5, or 5 stars in 2015 (N=31,766). 129 singleton observations were excluded.

Table 4.2 presents results of the TWFE estimate for each outcome both with and without baseline covariates. We focus on results without baseline covariates. Estimation results indicate that the end of the QBP demonstration led to a $40.98 (95% CI: $61.04 to $20.92) decline in benchmarks, a $16.65 (95% CI: $27.36 to $5.94) decline in plan generosity, and an $11.30 (95% CI: $2.91 to $19.69) increase in premiums. Additionally, we observe a large and statistically significant reduction in the probability of offering a zero-premium plan (-16.7%; 95% CI: -30.7% to -2.6%). Results were largely unchanged when excluding five-star contracts. (Appendix C.9)

While estimates suggest an increase in network restrictiveness and advertising effort, these coefficients are imprecise. Taken together, our results imply that 40.6 percent of the payment reduction was passed through to enrollees as reductions in plan generosity, and 27.6 percent of the payment reduction was passed through as increases in premiums. Inclusion of baseline covariates did not meaningfully affect the passthrough estimates.

Table 4.2. Two-Way Fixed Effects Estimates

	Benchmarks ($)	Plan Generosity ($)	Premiums ($)	Network Restrictiveness (O/E Ratio, from 0 to 100)	Advertising (Ads per 100 individuals)	Pr(Zero Premium) (Proportion)
Without Covariates	-40.98*** (-61.04 – -20.92)	-16.65*** (-27.36 – -5.94)	11.30*** (2.91 – 19.69)	-1.88 (-4.23 – 0.462)	0.48 (-0.11 – 1.076)	-0.167*** (-0.307 – -0.026)
W/ Baseline Covariates	-40.90*** (-60.94 – -20.86)	-16.53*** (-27.23 – -5.83)	11.33*** (2.99 – 19.67)	-1.95 (-4.27 – 0.366)	0.38 (-0.14 – 0.89)	-0.168*** (-0.308 – -0.028)

Notes: Estimates from two-way fixed effects model based on equation 4.3. Estimates indicate the coefficient from the interaction of post and treat. 95% confidence intervals in parentheses, standard errors clustered at the contract level. Models with baseline covariates include all covariates in Table 1 at their baseline levels. All models are weighted by enrollment and include county fixed effects, plan type fixed effects, and parent company fixed effects. N without covariates: 38,631; N with covariates: 38,598. 129 singleton observations were excluded. *** p<0.05, ** p<0.01, * p<0.00

Discussion

In this analysis of payment pass-through in the MA market, we investigated six dimensions of plan characteristics, and found that there was a 68.2 percent pass-through of payment reductions across several plan characteristics, a novel addition to the literature that has previously only examined premiums and measures of plan generosity. Our findings suggest that nearly two-thirds of this pass-through (40.6 percent) came in the form of reduced benefit generosity with another 27.6 percent through increased premiums. Notably, we found little evidence of changes in profit-seeking behavior (as proxied by advertising) or in network restrictiveness.

Our estimated effect of the benchmark reduction on the prevalence of zero premium plans is particularly substantial. Before 2015, treated observations on average (weighted by enrollment) offered zero-premium plans 71.4 percent of the time, while control observations offered zero-premium plans 47.4 percent of the time. Our estimated reduction (16.7 percentage points) represents a relative decline of 23.4 percent in the probability of offering a zero-premium plan.

Our results broadly align with prior work finding that pass-through of payments to MA plans is less than 100%.[23–25,85] This suggests that the MA market continues to function in an imperfectly competitive manner. Additionally, our analysis is one of only two that examines payment *reductions*, and we estimate a pass-through of similar magnitude.[25]

Our results also underscore the idea that payers are likely to pass through payment changes along different margins depending on their saliency. Given that beneficiaries are more likely to enroll in zero premium plans,[2] it is likely that premiums are a more salient feature of plan design for beneficiaries than plan generosity. Our findings are consistent with this notion, indicating a larger share of payment changes flowing through to plan generosity rather than premiums.

A novel component of our analysis is the ability to examine the effects of payment changes on advertising effort and network restrictiveness. Neither are visible to the beneficiary, but a less restrictive network imposes a cost on payers – through administration burden and fixed costs, increased utilization among beneficiaries, and likely becoming more attractive to sicker beneficiaries – while expanded advertising efforts might be profit-generating. Our analysis finds no evidence of an effect of payment changes on either margin. This might be true for a number of reasons. First, advertising may simply not be a very profitable activity. Recent work suggests that TV advertising is a relatively expensive way for insurers to accrue new customers.[77] Similarly, both advertising and network structure may be relatively constant over time. If there is a high fixed cost to modifying advertising efforts and/or network structure (particularly when networks are generally not visible to beneficiaries), insurers may forego otherwise optimal changes.

Our results indicate that changes to payment rates may get passed-through to beneficiaries, through a combination of highly salient and visible channels (premiums and the probability of a zero-premium plan) as well as less visible channels (benefit generosity). This means that optimizing MA payment policy through payment reductions might have negative effects on beneficiaries. This may suggest a need to use savings generated through payment reductions to minimize harms to beneficiaries, for instance by expanding benefits in TM, to make the fallback option more attractive for beneficiaries.

Limitations

There are several important limitations to this analysis. First, as with all analyses relying on natural experiment-driven variation, our results are sensitive to whether the assumptions necessary for DID hold. While we provide evidence that this is the case, it is never possible to prove that a control group in a natural experiment is completely appropriate.

Second, while we use data from CMS to measure plan generosity, there could be remaining measurement error. The OOPC data we rely on is a modeled estimate of plan generosity based on the utilization of a hypothetical average beneficiary. To the extent that these models are inaccurate, they may lead to bias in our estimates.

Third, while we categorize observations into treated and control groups, this is an approximation. Because plans receiving five percent bonuses are also competing with plans receiving no bonus after 2015, they may still respond to changes in strategy and benefit structure among the treated group. Similarly, the way that treated plans respond to payment changes may also be affected by the fact that they compete with control plans, and thus may be biased downward. We include a range of fixed effects, including contract fixed effects (which allow us to examine changes *within* contract) to try to ameliorate these concerns. Nonetheless, it is impossible to fully account for this measurement error.

Fourth, there are likely other outcomes to which payment changes may flow. We are unable to measure insurer profit or provider payments, for instance. These and other measures may represent a remaining unmeasured component of pass-through.

Lastly, our results may not generalize to other instances of payment changes. We focus on a specific instance of policy-driven payment reductions, which may not necessarily indicate how insurers would respond to changes in payments on other margins. Nonetheless, despite our focus on an instance of payment reduction, existing work suggests symmetric responses to increases and reductions in payments.[25]

CHAPTER FIVE: CONCLUSION

Relying on widely-accessible claims data, in chapter 2, we applied a machine learning algorithm to estimate the effective network restrictiveness of primary care provider networks in MA. We made two novel contributions. First, we measured the restrictiveness of PCP networks in MA, finding that they reduced access to providers to 60.6% of what it would have been absent network restrictions imposed by insurers. While some of our results were consistent with prior work, a major difference was our finding that rural areas tended to have more restrictive PCP networks. These results suggest that existing challenges accessing care in rural areas might be exacerbated by restrictive provider networks. Second, we demonstrated how off-the-shelf predictive models can be applied to utilization data to retrospectively examine the performance of provider networks, independent of what officially-reported networks look like. Our results indicate that these networks do, in fact, reduce the number of PCPs seen by beneficiaries, disproportionately affecting those in rural areas. If this leads to use of higher-quality PCPs, it might be a positive for beneficiaries, but if leads to worse access then it can potentially harm quality. This provides a proof-of-concept for regulators interested in efficiently assessing the performance of provider networks in MA.

In chapter 3, we applied the method from chapter 2 and identified provider groups that attract a disproportionate share of beneficiaries, relative to their number of providers. We identified the presence of so-called “star provider” groups that have accrued substantial market shares of beneficiaries, and are more than 50% more likely to be included in MA provider networks than other provider groups. Notably, these groups were not larger than others, suggesting the results were not driven solely by size. Our analysis was the first to examine this phenomenon among provider groups, with all prior literature focusing on hospitals. These provider groups likely differentiate themselves either through quality or brand-name recognition, and thus are able to exert some degree of market power vis-à-vis MA insurers. This likely limits the ability of MA insurers to flexibly adjust provider networks and may lead to higher costs facing these plans and beneficiaries.

Lastly, in chapter 4, we applied a quasi-experimental research method to measure the effect of payment changes on MA plans. We estimated the extent to which reductions in payments from the government flow through to beneficiaries in the form of less generous benefits, higher premiums, and more restrictive provider networks. Additionally, we estimated the effect of these payment changes on an important profit-seeking activity, insurer advertising. Our results indicated that over 60% of payments were passed through in the form of less generous benefits and higher premiums. Surprisingly, we found no statistically significant effect of payments on advertising effort or network restrictiveness. While our results were directionally similar to prior work, our estimates of pass-through were higher. These findings suggest that current efforts to reduce payments to MA plans may indeed lead to less generous benefits and higher costs facing beneficiaries. However, because pass-through is less than 100%, the government could minimize harms to beneficiaries while still reducing spending on MA.

Taken together, our findings suggest that the MA market is generally not perfectly competitive, but that non-competitive provider markets may also pose problems for MA.

Furthermore, provider networks in MA do appear to reduce access to PCPs. This implies that attempts to reduce MA payments, a common proposal for legislative reform, may have negative implications for beneficiaries, but that the current status quo of the MA market is nonetheless sub-optimal.

APPENDIX A: Additional Materials for Chapter 2

Appendix A.1: Variable Definitions

Prediction Model	
No. of providers seen	Count of unique NPIs that beneficiaries in a given contract-plan type received prescriptions from in a given county-year combination.
Network restrictiveness	An observed-to-expected ratio of the number of actual providers seen divided by the predicted number of providers seen.
PDE count	Count of prescription drug events in a given contract-plan type-county-year combination.
No. of enrollees	Number of beneficiaries enrolled in a given contract-plan type-county-year combination in June of the given year.
State indicator	Binary variable indicating whether the observation is in a given state (equivalent to state fixed effects).
No. of providers seeing beneficiaries in a county	The count of all unique NPIs prescribing to *any* beneficiary residing in a given county.
Average age	The average age of beneficiaries enrolled in a given contract-plan type-county-year combination.
Mortality rate (%)	The share of beneficiaries who died in a given contract-plan type-county-year combination.
Age groups	Beneficiary ages bucketed into four groups: less than 65, 65-74, 75-84, and 85+.
Market Factors Association Model	
TM HCC risk score	The hierarchical condition category risk score of TM beneficiaries in the county
MDs per 1,000	The total number of medical doctors per 1,000 population in a county
MA-PD HHI	The Herfindahl-Hirschman Index of the MA-PD market in the county
Ln(Per Capita Income)	The natural log of per capita income in the county
Veterans per 1,000 population	The number of Veterans per 1,000 population in the county
Plan type	The plan type (HMO, HMO-POS, PPO) of the observation
Parent company market share	The market share of the parent company in the county
Effective year	The year that the contract began operating in MA

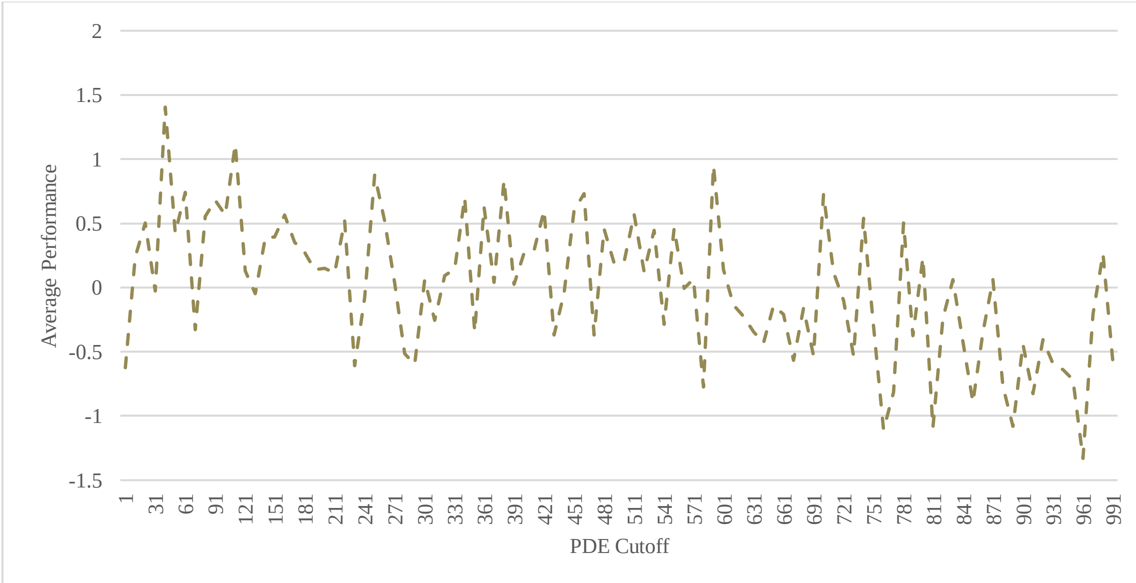
Notes: NPI: National provider identification number. PDE: prescription drug event. MA: Medicare Advantage.

Appendix A.2: Estimating Maximum Tolerable O/E Ratios

To identify the maximum extent to which MA-PD enrollees might see more PCPs than those in PDPs, we used the FFS Carrier File (restricted to beneficiaries with PDPs) and MA Encounter Carrier File (restricted to contracts with complete data) from 2018 to identify beneficiary encounters with PCPs. To identify beneficiaries, we further restricted our sample to beneficiaries who are part of the 20% sample in the MBSF and who were continuously enrolled in their respective coverage for the year. For MA-PD enrollees, only those in local CCPs were included, and following prior work,[35] those in contracts that have relatively complete data in the MA Encounter file. Additionally, we restricted to beneficiaries in stand-alone PDP plans in the FFS file.

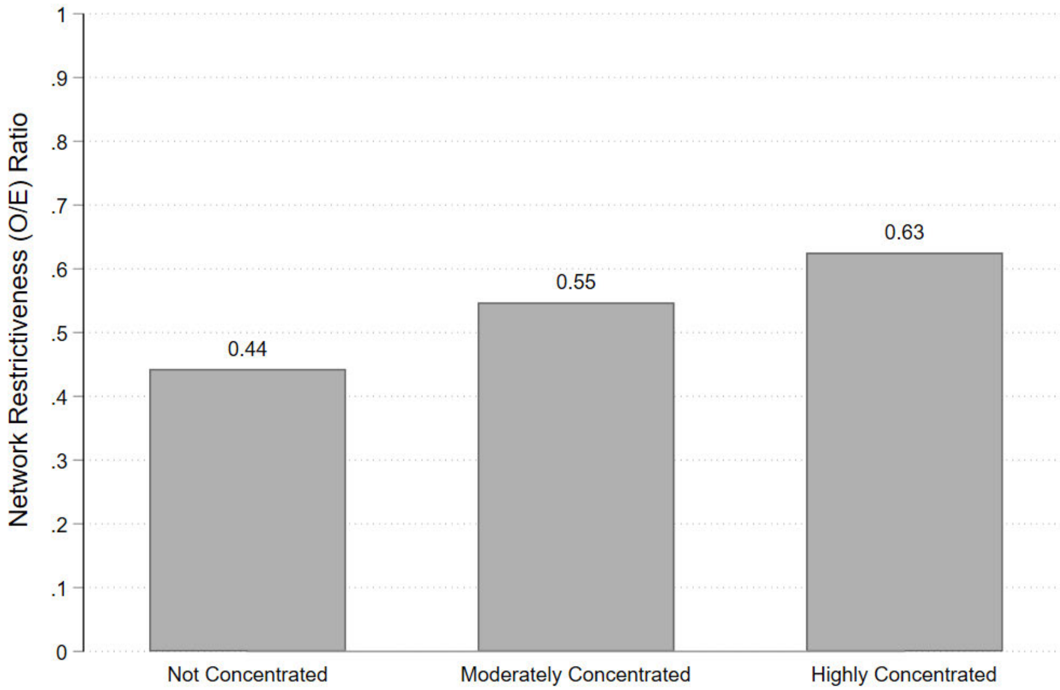
To identify PCPs, we relied on the FFS Carrier File. We identified every unique NPI and specialty classification in the 20% Carrier File and selected the most common specialty. As with the MD-PPAS, those with internal medicine, family practice, general practice, or geriatric medicine as the most common specialty were considered to be PCPs. In the FFS dataset, we restricted visits to those with Berenson-Eggers Type of Service codes classified as evaluation and management by CMS. In the MA dataset, we restricted visits to those with evaluation and management codes. After identifying all visits, we counted the total number of unique PCPs and included beneficiaries. We identified 4,378,181 million unique beneficiaries who saw 158,080 unique PCPs in the FFS data and 2,131,989 million unique beneficiaries who saw 133,570 unique PCPs in the MA data. Thus, the ratio of PCPs to beneficiaries among the PDP enrollees to MA enrollees is 1.74 ($\frac{158{,}080}{4{,}378{,}181} \div \frac{133{,}570}{2{,}131{,}989} = 1.74$).

Appendix A.3: Random Forest Average Performance for Different PDE Cutoffs



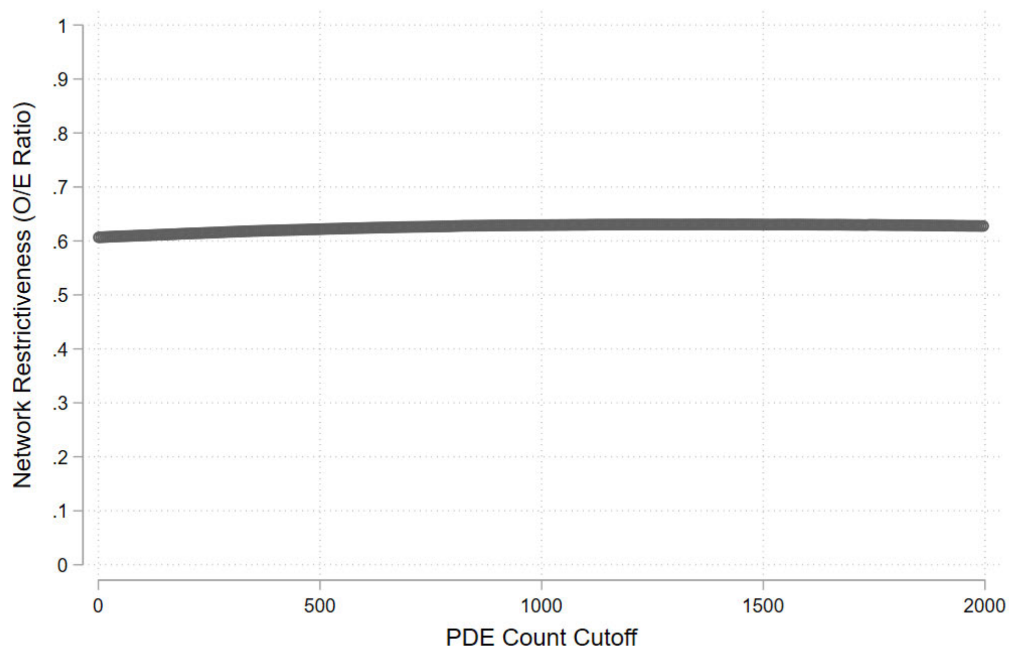
Notes: Performance indicates the average across standardized measures of Emax and the percent of observations with an O/E greater than 1.74. A lower value indicates better performance. These results indicate that a PDE cutoff of 961 was optimal for maximizing the performance of the prediction model.

Appendix A.4: Network Restrictiveness by HHI Category



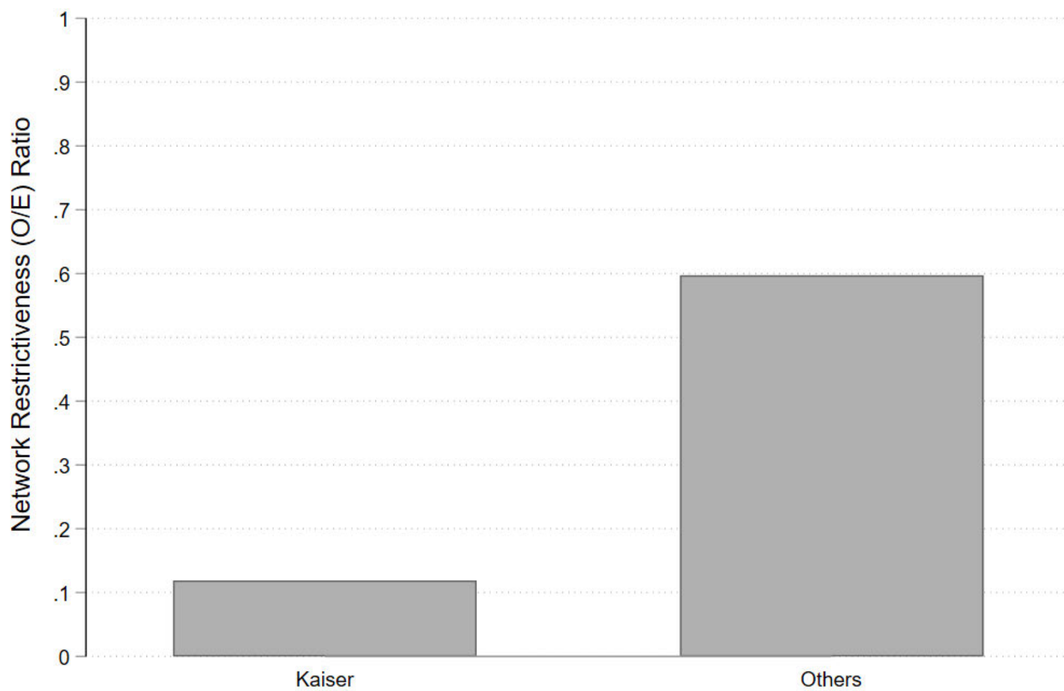
Notes: Market concentration is based on the Herfindahl-Hirschman Index within a county for the MA market. Market share is assigned to the parent company. Concentration categories are based on FTC classifications. Observations are weighted by the number of beneficiaries in contract-plan type. Network restrictiveness is measured by creating an observed-to-expected (O/E) ratio of the observed unique number of providers seen by beneficiaries in an MA plan, divided by the predicted number of unique number of providers that would have been seen absent network restrictions. Data is aggregated across all years of data. Complete methods are described in the text.

Appendix A.5: Relationship Between PDE Count and Network Restrictiveness (Enrollment Weighted)



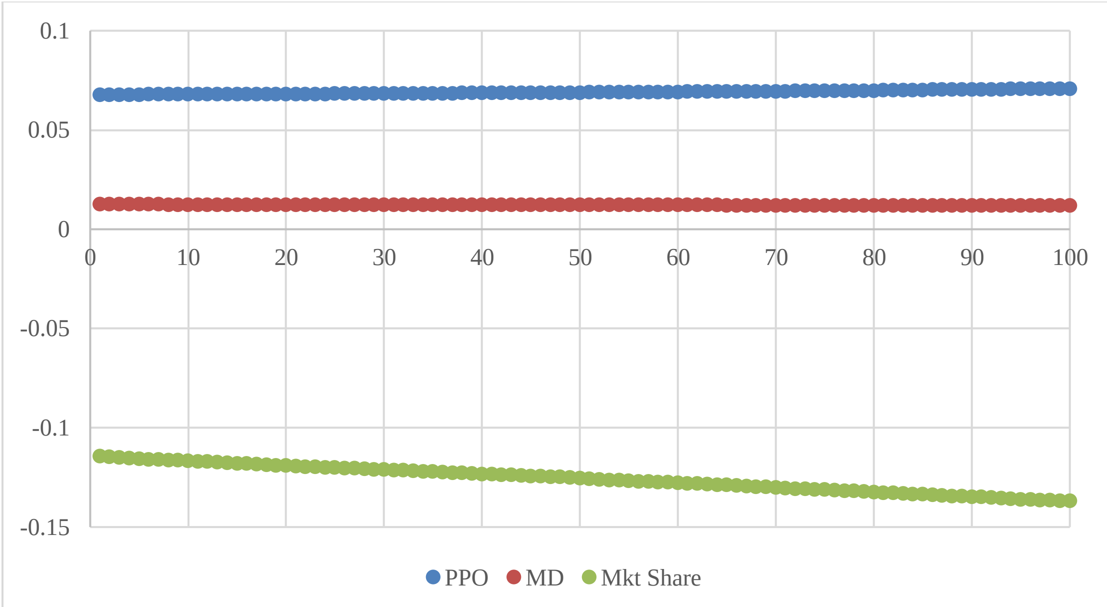
Notes: This illustrates the estimated network restrictiveness with observations limited to those with the number of prescription drug events at or above the indicated cutoff. This indicates that estimates of network restrictiveness are not sensitive to underlying volume. Observations are weighted by the number of beneficiaries in contract-plan type. Network restrictiveness is measured by creating an observed-to-expected (O/E) ratio of the observed unique number of providers seen by beneficiaries in an MA plan, divided by the predicted number of unique number of providers that would have been seen absent network restrictions. Data is aggregated across all years of data. Complete methods are described in the text.

Appendix A.6: Network Restrictiveness of Kaiser vs. Other Contracts



Notes: This illustrates the average network restrictiveness for all observations where Kaiser Permanente is the parent company compared to all other observations in the state of California. Observations are weighted by the number of beneficiaries in contract-plan type. Network restrictiveness is measured by creating an observed-to-expected (O/E) ratio of the observed unique number of providers seen by beneficiaries in an MA plan, divided by the predicted number of unique number of providers that would have been seen absent network restrictions. Data are aggregated across all years of data. Complete methods are described in the text.

Appendix A.7: Estimated Coefficients by PDE Count



Notes: This illustrates the estimated coefficient for three key variables — PPO plans, the number of doctors per 1,000 population, and the market share of the observation — and how they vary with samples restricted to those with a given number of prescription drug events or greater.

Appendix A.8: Matched Sample Validation

Because there may be differences in enrollment, propensity to receive prescriptions, underlying health status, and other factors between MA-PD and TM enrollees (and the areas in which they reside), we considered an alternative approach to measuring the effect of network restrictions on utilization. We focused on examining the relationship with rurality in our analysis — which indicated that networks are more restrictive in rural areas than urban — which differed when compared with prior work. If there were substantial differences in enrollment between MA and TM beneficiaries in rural areas, for instance, that could have led us to simply observe fewer PCP interactions in rural areas than urban areas for MA enrollees. In turn, we would estimate a more restrictive network in rural areas.

To examine whether our results held with an alternative approach, we first took a random sample of 1,000 MA-PD and 1,000 PDP enrollees for each county in the country for one year of our analysis (we focused on 2016 and limited our sample to the 50 states plus DC). In some cases, there were fewer than 1,000 enrollees for the category, and thus we took the full population. In total, our sample was 3,740,670 individuals.

For each sampled enrollee, we then identified the total number of unique PCPs seen by the enrollee. We then estimated a Poisson regression, with the total number of unique PCPs as the outcome, and predictors included MA enrollment, race, dual status, age category, and sex. We also included county fixed effects and used robust standard errors. Because we were interested in examining the sensitivity of our results on rurality to volume and enrollment, we estimated this regression separately for urban and rural counties, and limited to counties where there were 10 or more MA-PD and PDP enrollees, as well as those were the difference in total MA-PD and PDP enrollment was less than 100. We did this to restrict to observations that were more similar on enrollment (and thus likelihood of observing physician interactions).

The Poisson coefficient on MA enrollment for urban beneficiaries was -1.551 (95% CI: -1.557 to -1.544) while for rural beneficiaries it was -1.596 (95% CI: -1.630 to -1.562). This suggested that our results held directionally, and that MA-PD beneficiaries in rural areas saw differentially fewer PCPs than their urban counterparts, when compared with PDP enrollees.

Appendix A.9: Sensitivity of Multivariable Results to Low Levels of MA and PDP Enrollment

	Primary Model	**PDP > 1st Qt**	**MA > 1st Qt**	**MA+PDP > 1st Qt**
Medicare County Average HCC Score	0.0305 [-0.0151 – 0.0760]	0.0134 [-0.0163 – 0.0432]	0.0347 [-0.013 – 0.083]	0.0134 [-0.0163 – 0.0432]
Plan Type (Ref: HMO)				
HMO-POS	0.136*** [0.0840 – 0.189]	0.147*** [0.0774 – 0.217]	0.141*** [0.0865 – 0.195]	0.147*** [0.0774 – 0.217]
PPO	0.197*** [0.141 – 0.254]	0.157*** [0.108 – 0.206]	0.212*** [0.152 – 0.273]	0.157*** [0.108 – 0.206]
MDs Per 1,000 Population	0.0745*** [0.0400 – 0.109]	0.0789*** [0.0426 – 0.115]	0.0687*** [0.0312 – 0.106]	0.0789*** [0.0426 – 0.115]
Rurality				
Non-metropolitan, near urban area	-0.474*** [-0.545 – -0.402]	-0.464*** [-0.545 – -0.383]	-0.433* [-0.509 – -0.357]	-0.464*** [-0.545 – -0.383]
Non-metropolitan, not near urban area	-0.682*** [-0.809 – -0.555]	-0.666* [-0.789 – -0.544]	0.604*** [-0.762 – -0.446]	-0.666* [-0.789 – -0.544]
Rural	-1.151*** [-1.251 – -1.052]	-1.117*** [-1.214 – -1.02]	-1.093*** [-1.204 – -0.981]	-1.117*** [-1.214 – -1.02]
Ln(Income)	0.0176 [-0.0223 – 0.0575]	0.0133 [-0.0221 – 0.0487]	0.0202 [-0.022 – 0.063]	0.0133 [-0.0221 – 0.0487]
Market Share	-0.104*** [-0.137 – -0.0712]	-0.102*** [-0.141 – -0.064]	-0.118*** [-0.153 – -0.083]	-0.102*** [-0.141 – -0.064]
MA HHI	0.0513** [0.0160 – 0.0867]	0.0128 [-0.0228 – 0.0484]	0.0664*** [0.0273 – 0.106]	0.0128 [-0.0228 – 0.0484]
Veterans per 1,000	0.0124 [-0.0159 – 0.0407]	-0.0165 [-0.0514 – 0.0184]	0.00829 [-0.0231 – 0.0396]	-0.0165 [-0.0514 – 0.0184]
Effective Year of Contract	0.00804*** [0.00387 – 0.0122]	0.0091*** [0.0049 – 0.013]	0.00788*** [0.0036 – 0.0122]	0.00908*** [0.00489 – 0.0133]
N (excluding singletons)	63,901	47,906	48,089	47,906

Notes: All models include state fixed effects, parent company fixed effects, and year fixed effects. 95% confidence intervals calculated from heteroscedasticity-robust standard errors clustered at the county level in brackets. Effective year of contract indicates when the contract became active in MA. Coefficients are standardized and are thus continuous variables are interpreted as a β standard deviation change in network restrictiveness for a one standard deviation change in the covariate. PDP<Median indicates a restriction to areas with less than median PDP enrollment, MA<Median indicates a restriction to areas with less than median MA enrollment, and PDP+MA is the intersection of both (which is the same as sample as the MA<Median restriction).

APPENDIX B: Additional Materials for Chapter 3

Table B.1: Sensitivity Regression Results

	Demand Model	**Star Indicator**
Network Inclusion >1 PDE	0.06*** (0.0006)	0.52*** (0.002)
Network Inclusion >2 PDEs	0.05*** (0.0005)	0.42*** (0.002)
Network Inclusion >3 PDEs	0.04*** (0.0004)	0.33*** (0.002)
N	1,538,937	1,538,854

Notes: Outcome is the probability of an observation being in-network. "Demand Model" indicates that the predictor is the measure of disproportionate demand. "Star Indicator" indicates that the predictor is an indicator for being a star provider group. Standard errors clustered at the TIN level in parentheses.
*** p<0.001, ** p<0.01, * p<0.05

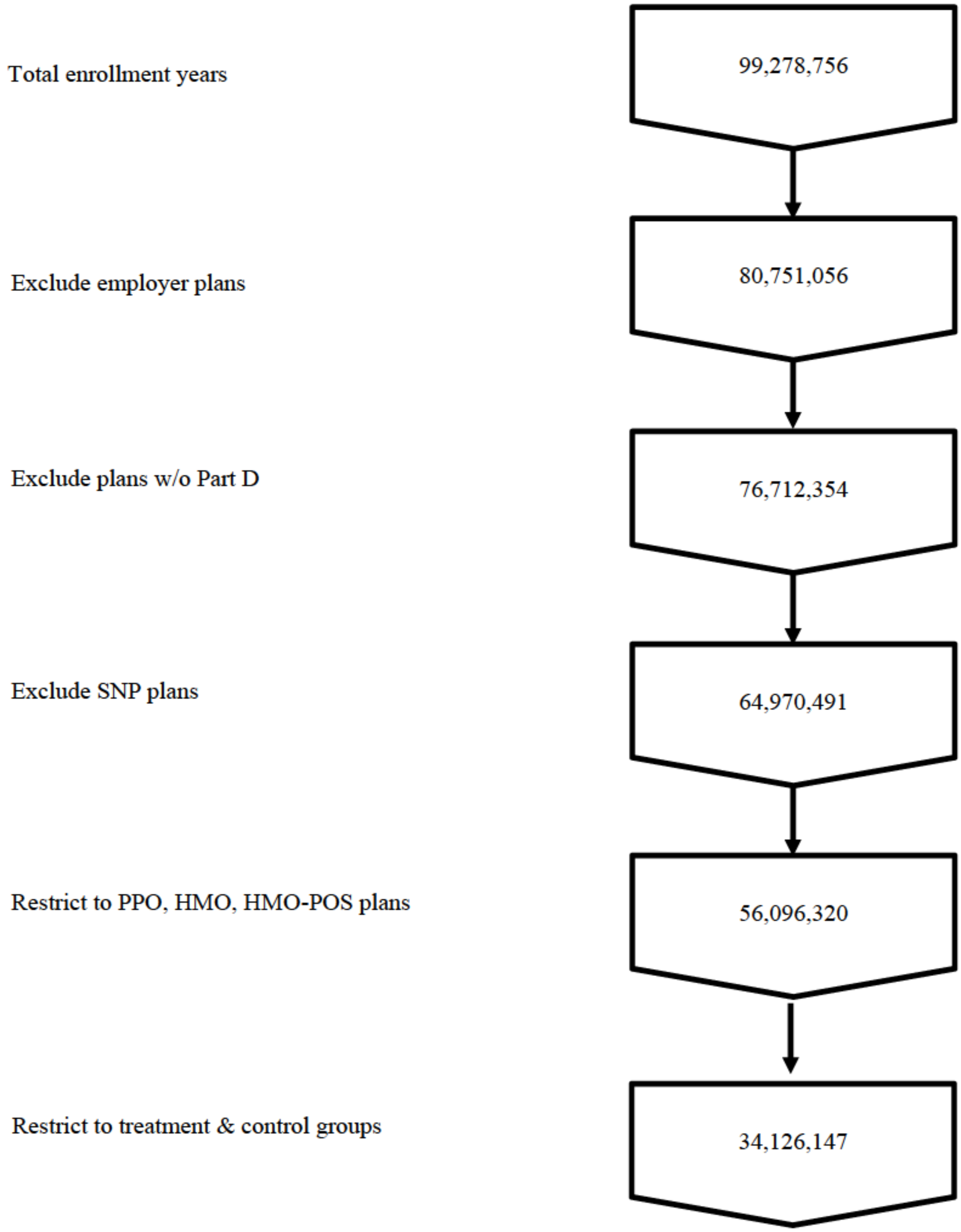
APPENDIX C: Additional Materials for Chapter 4

Appendix C.1: Bonus Payments as a Percentage of Benchmark

Star Rating	**ACA**			**QBP Demo**		
	2012	2013	2014	2012	2013	2014
5 stars	1.5	3	5	5	5	5
4 or 4.5 stars	1.5	3	5	4	4	5
3.5 stars	0	0	0	3.5	3.5	3.5
3 stars	0	0	0	3	3	3
< 3 stars	0	0	0	0	0	0
New plan	1.5	2.5	3.5	3	3	3.5

Source: GAO. Medicare Advantage: Quality Bonus Payment Demonstration Undermined by High Estimated Costs and Design Shortcomings. QBP: Quality bonus payment

Appendix C.2: Sample Selection Flowchart (2012-2017)

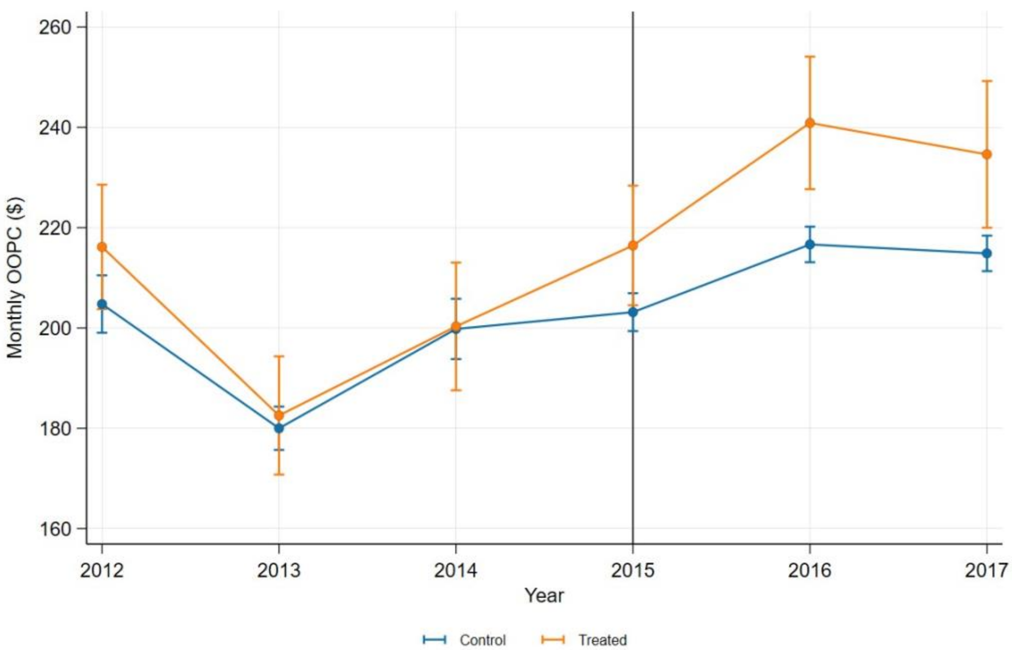


Appendix C.3: Level of Observation of Key Variables

Variable Name	Level of Observation
Expected Out-of-Pocket Cost (OOPC)	Contract-plan-year
Total Premium	Contract-plan-year
Star Ratings	Contract-year
Contract-Plan-Specific Benchmark	Contract-plan-year-county
Network Restrictiveness	Contract-plan type-year-county
Advertising per 100	Parent company-county-year

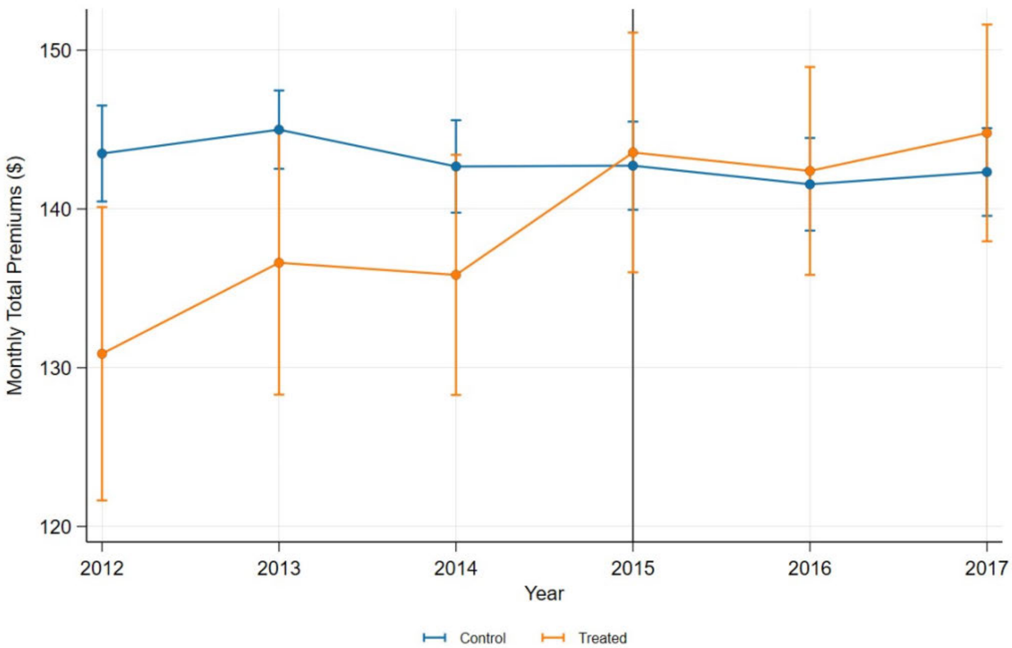
Notes: Level of observation indicates the level at which the variable varies within the analytic dataset.

Appendix C.4: Trend Plot: Plan Generosity



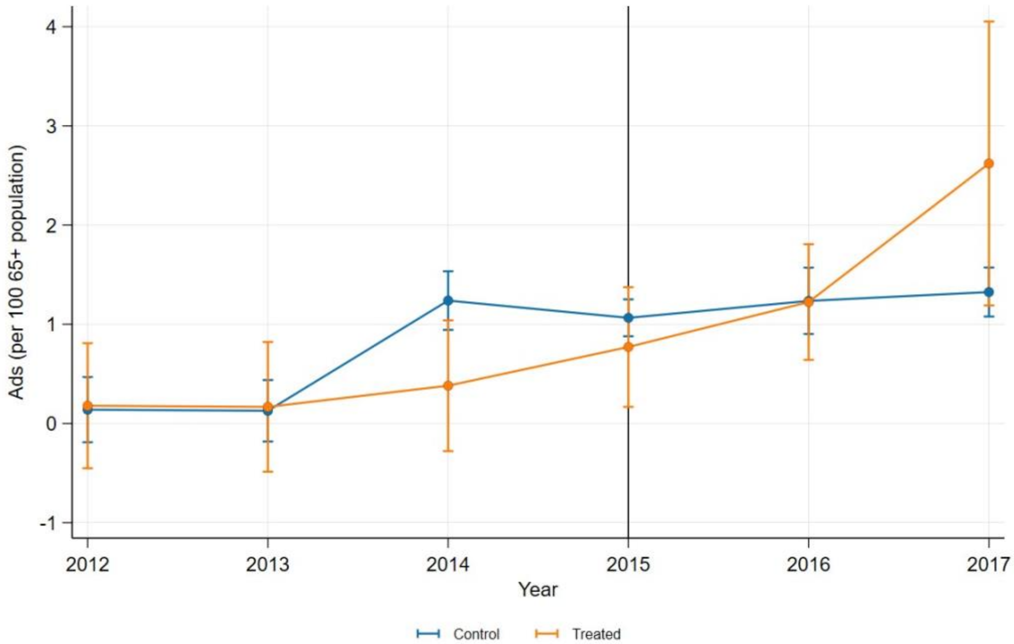
Notes: Estimates are from a regression interacting year and treatment group indicator. Regression includes county fixed effects, plan type fixed effects, and parent company fixed effects with standard errors clustered at the contract level. Regression is weighted by enrollment. Treated: contracts with 3 or 3.5 stars in 2015 (N=6,865); Control: contracts with 4, 4.5, or 5 stars in 2015 (N=31,766). 129 singleton observations were excluded.

Appendix C.5: Trend Plot: Premiums



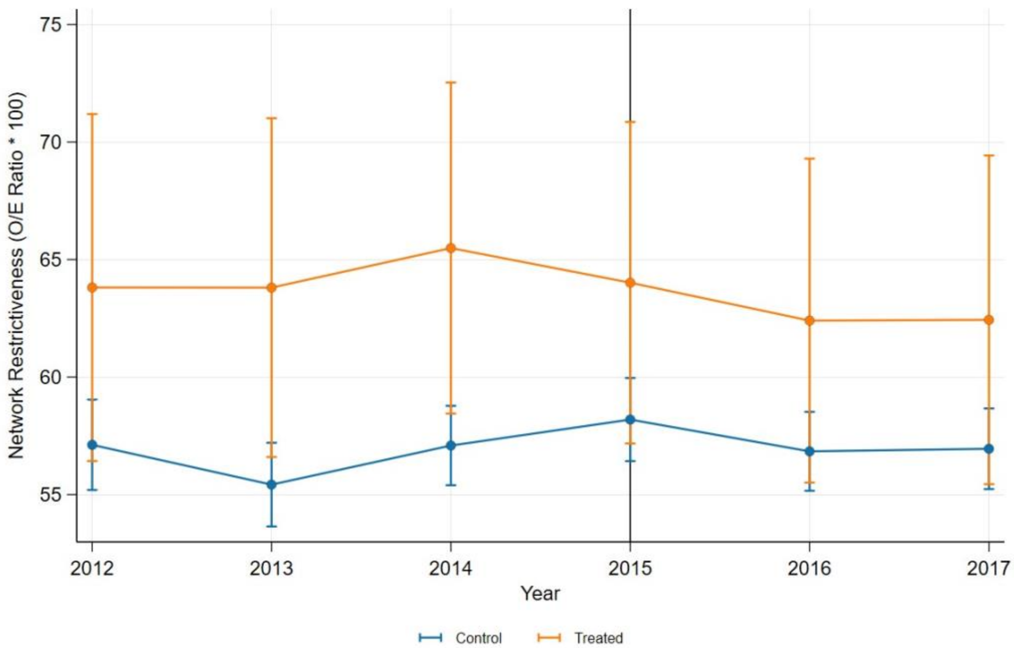
Notes: Estimates are from a regression interacting year and treatment group indicator. Regression includes county fixed effects, plan type fixed effects, and parent company fixed effects with standard errors clustered at the contract level. Regression is weighted by enrollment. Treated: contracts with 3 or 3.5 stars in 2015 (N=6,865); Control: contracts with 4, 4.5, or 5 stars in 2015 (N=31,766). 129 singleton observations were excluded.

Appendix C.6: Trend Plot: Advertising



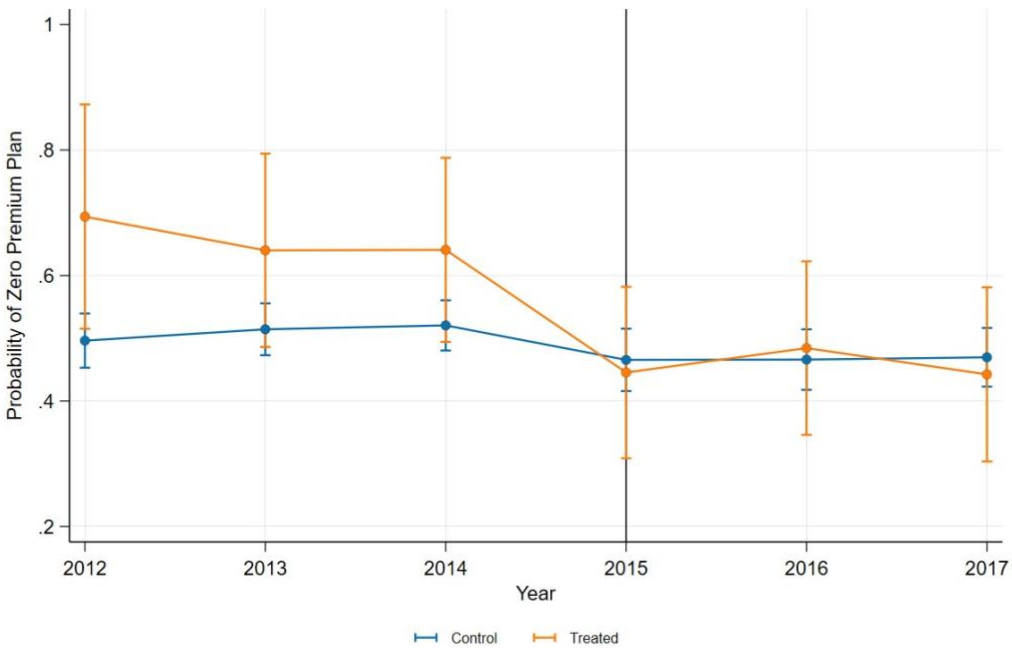
Notes: Estimates are from a regression interacting year and treatment group indicator. Regression includes county fixed effects, plan type fixed effects, and parent company fixed effects with standard errors clustered at the contract level. Regression is weighted by enrollment. Treated: contracts with 3 or 3.5 stars in 2015 (N=6,865); Control: contracts with 4, 4.5, or 5 stars in 2015 (N=31,766). 129 singleton observations were excluded.

Appendix C.7: Trend Plot: Network Restrictiveness



Notes: Estimates are from a regression interacting year and treatment group indicator. Regression includes county fixed effects, plan type fixed effects, and parent company fixed effects with standard errors clustered at the contract level. Regression is weighted by enrollment. Treated: contracts with 3 or 3.5 stars in 2015 (N=6,865); Control: contracts with 4, 4.5, or 5 stars in 2015 (N=31,766). 129 singleton observations were excluded.

Appendix C.8: Trend Plot: Probability, Zero Premium



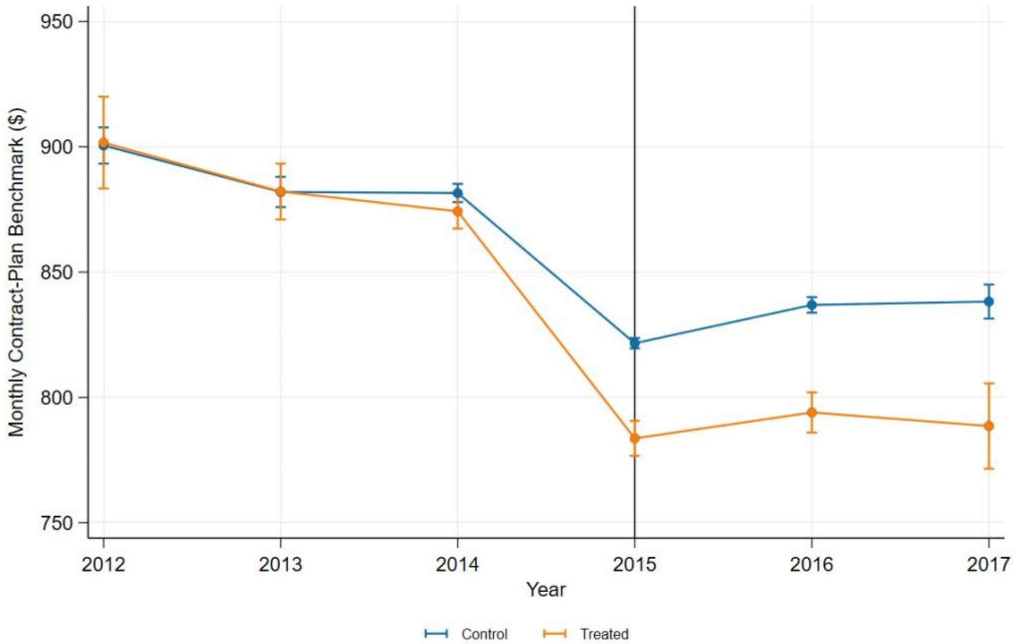
Notes: Estimates are from a regression interacting year and treatment group indicator. Regression includes county fixed effects, plan type fixed effects, and parent company fixed effects with standard errors clustered at the contract level. Regression is weighted by enrollment. Treated: contracts with 3 or 3.5 stars in 2015 (N=6,865); Control: contracts with 4, 4.5, or 5 stars in 2015 (N=31,766). 129 singleton observations were excluded.

Appendix C.9: Two-Way Fixed Effects Estimates, Without Five-Star Contracts

	Benchmarks ($)	Plan Generosity ($)	Premiums ($)	Network Restrictiveness (O/E Ratio, from 0 to 100)	Advertising (Ads per 100 individuals)	Pr(Zero Premium) (Proportion)
Without Covariates	-44.97*** (-66.22 – -23.71)	-14.38*** (-25.37 – -3.40)	11.26*** (2.42 – 20.11)	-1.50 (-3.95 – 0.95)	0.58 (-0.06 – 1.23)	-0.166*** (-0.32 – -0.018)
W/ Baseline Covariates	-44.91*** (-66.16 – -23.67)	-14.21*** (-25.18 – -3.23)	11.15*** (2.36 – 19.94)	-1.61 (-4.01 – 0.83)	0.46 (-0.10 – 1.03)	-0.166*** (-0.31 – -0.018)

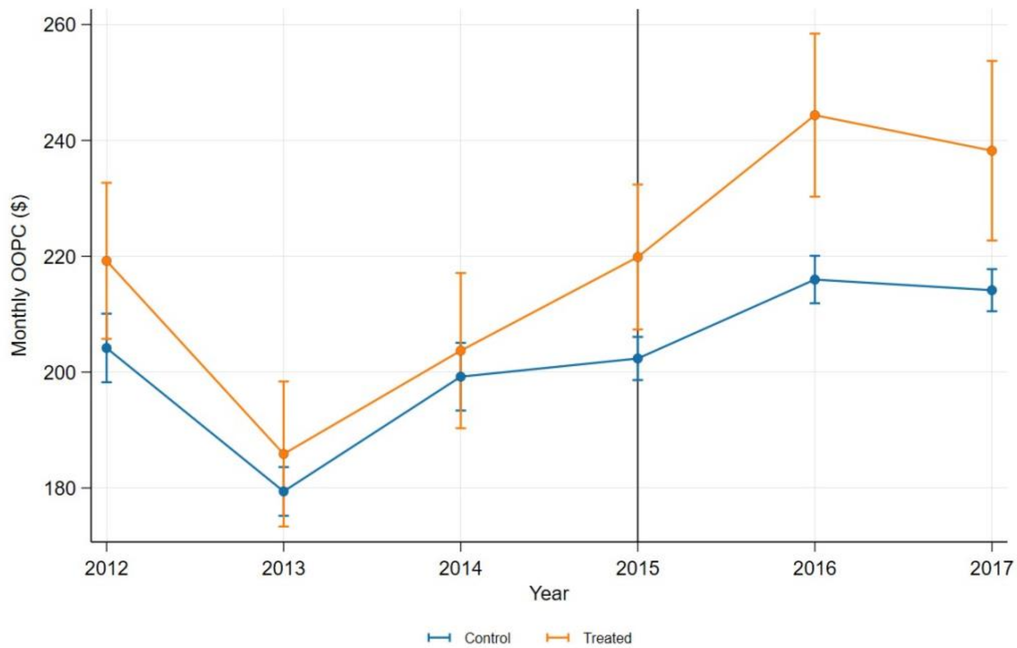
Notes: Estimates from two-way fixed effects model based on equation 4.3. Estimates indicate the coefficient from the interaction of post and treat. 95% confidence intervals in parentheses, standard errors clustered at the contract level. Models with baseline covariates include all covariates in Table 1 at their baseline levels. All models are weighted by enrollment and include county fixed effects, plan type fixed effects, and parent company fixed effects. N without covariates: 36,126; N with covariates: 36,114. 129 singleton observations were excluded.
*** p<0.05, ** p<0.01, * p<0.001

Appendix C.10: Trend Plot: Monthly Benchmarks, With Baseline Covariates



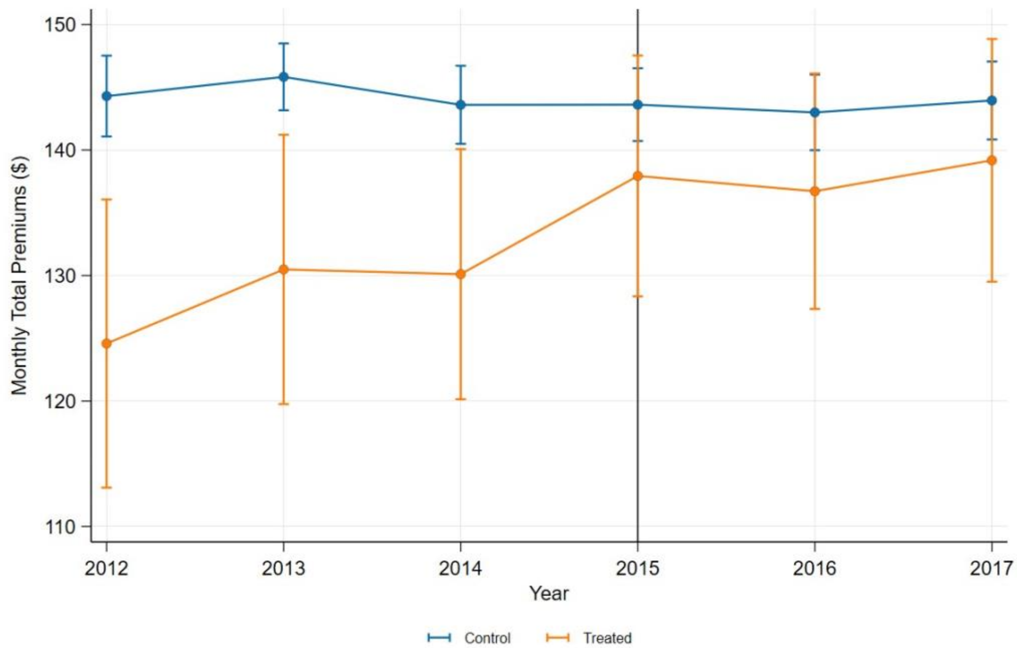
Notes: Estimates are from a regression interacting year and treatment group indicator. Regression is weighted by enrollment, includes county fixed effects, plan type fixed effects, and parent company fixed effects, and includes all baseline covariates listed in Table 1. Treated: contracts with 3 or 3.5 stars in 2015 (N=6,865); Control: contracts with 4, 4.5, or 5 stars in 2015 (N=31,729). 129 singleton observations were excluded.

Appendix C.11: Trend Plot: Plan Generosity, With Baseline Covariates



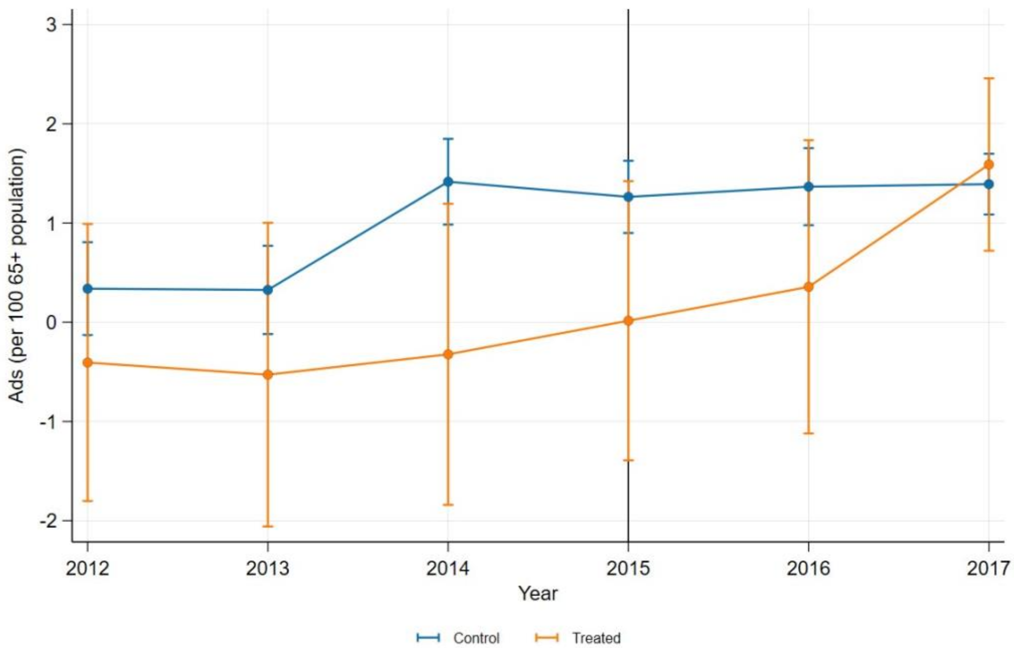
Notes: Estimates are from a regression interacting year and treatment group indicator. Regression includes county fixed effects, plan type fixed effects, and parent company fixed effects, with standard errors clustered at the contract level. Regression is weighted by enrollment and includes all baseline covariates listed in Table 1. Treated: contracts with 3 or 3.5 stars in 2015 (N=6,865); Control: contracts with 4, 4.5, or 5 stars in 2015 (N=31,729). 129 singleton observations were excluded.

Appendix C.12: Trend Plot: Premiums, With Baseline Covariates



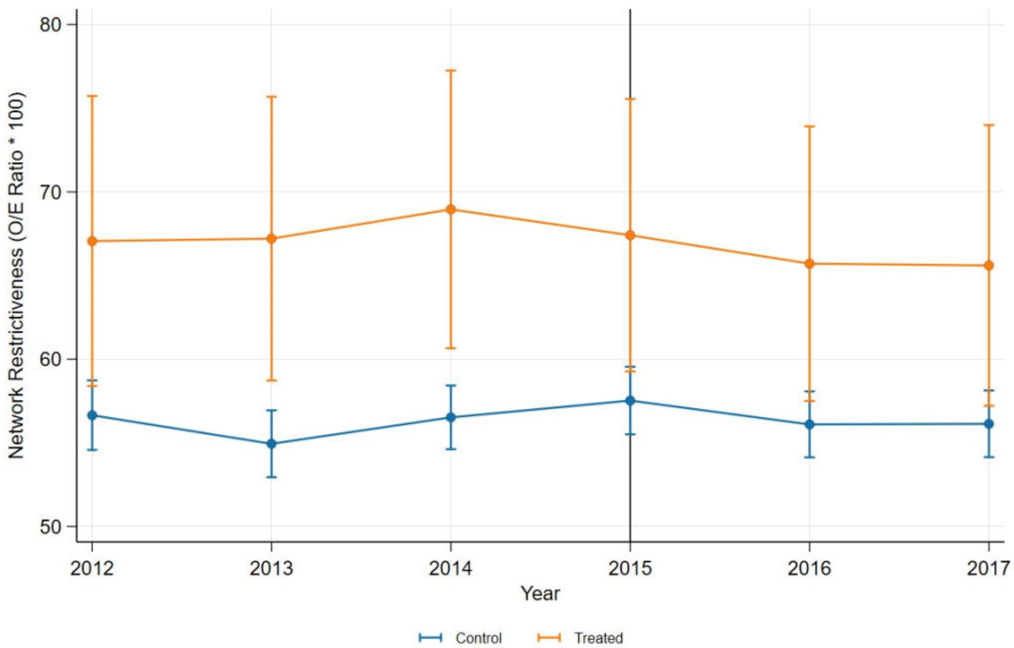
Notes: Estimates are from a regression interacting year and treatment group indicator. Regression includes county fixed effects, plan type fixed effects, and parent company fixed effects, with standard errors clustered at the contract level. Regression is weighted by enrollment and includes all baseline covariates listed in Table 1. Treated: contracts with 3 or 3.5 stars in 2015 (N=6,865); Control: contracts with 4, 4.5, or 5 stars in 2015 (N=31,729). 129 singleton observations were excluded.

Appendix C.13: Trend Plot: Advertising, With Baseline Covariates



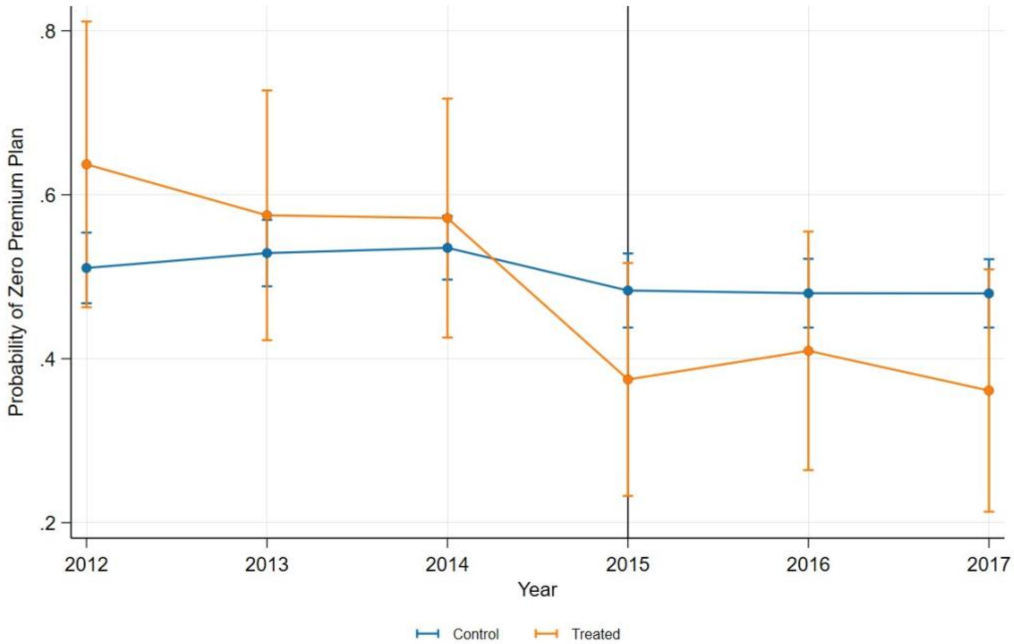
Notes: Estimates are from a regression interacting year and treatment group indicator. Regression includes county fixed effects, plan type fixed effects, and parent company fixed effects, with standard errors clustered at the contract level. Regression is weighted by enrollment and includes all baseline covariates listed in Table 1. Treated: contracts with 3 or 3.5 stars in 2015 (N=6,865); Control: contracts with 4, 4.5, or 5 stars in 2015 (N=31,729). 129 singleton observations were excluded.

Appendix C.14: Trend Plot: Network Restrictiveness, With Baseline Covariates



Notes: Estimates are from a regression interacting year and treatment group indicator. Regression includes county fixed effects, plan type fixed effects, and parent company fixed effects, with standard errors clustered at the contract level. Regression is weighted by enrollment and includes all baseline covariates listed in Table 1. Treated: contracts with 3 or 3.5 stars in 2015 (N=6,865); Control: contracts with 4, 4.5, or 5 stars in 2015 (N=31,729) 129 singleton observations were excluded.

Appendix C.15: Trend Plot: Probability, Zero Premium, With Baseline Covariates



Notes: Estimates are from a regression interacting year and treatment group indicator. Regression includes county fixed effects, plan type fixed effects, and parent company fixed effects, with standard errors clustered at the contract level. Regression is weighted by enrollment and includes all baseline covariates listed in Table 1. Treated: contracts with 3 or 3.5 stars in 2015 (N=6,865); Control: contracts with 4, 4.5, or 5 stars in 2015 (N=31,729) 129 singleton observations were excluded.

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