Measuring Restrictiveness of Medicare Advantage Networks: A Claims-Based Approach

Yevgeniy Feyman,[1,2] Steven D. Pizer, [1,2] Paul R. Shafer, [1,2] Austin B. Frakt,[1,2,3] Melissa M. Garrido [1,2]

1. Department of Health Law, Policy and Management, Boston University School of Public Health, Boston, Massachusetts
2. Partnered Evidence-Based Policy Resource Center, Boston VA Healthcare System, Boston, Massachusetts
3. Department of Health Policy & Management, Harvard T.H. Chan School of Public Health, Cambridge, Massachusetts.

Abstract

As is common in the private insurance market, insurers offering Medicare Advantage (MA) plans typically restrict the network of providers from which patients can seek care. Existing work evaluating the breadth of these provider networks relies on provider directories and typically measures breadth as the fraction of providers in-network within some geography. However, well-documented errors prevalent in provider directories and data identifying provider locations can lead to errors with this approach. We use Medicare prescription drug event data from 2011 to 2017 to identify interactions between MA beneficiaries and providers, creating a predictive model that estimates counterfactual utilization to measure the degree to which networks restrict the number of providers seen by patients.

We detail the construction, tuning, and estimation of this model for one high-prescribing specialty (primary care). A random forest model outperformed other choices. Our estimates indicate that, as theory predicts, preferred provider organizations (PPOs) are less restrictive than health maintenance organizations (HMOs), that issuers with higher market share impose more restrictive networks, and that increased provider supply is associated with less restrictive networks. These results help validate our methodological approach, providing justification for application more generally.

We also provide guidance on how our approach can be replicated for other markets and/or specialties.

Introduction

The use of provider networks to concentrate utilization among certain providers has become a common component of modern-day insurance design. Across all insurance markets (including employer, Affordable Care Act marketplace, and Medicaid Managed Care), there is substantial variation in the extent to which insurance networks include coverage of nearby clinicians and hospitals.[1] Clinicians may be excluded from insurer networks to reduce costs, improve quality, or to increase profit for the insurer. While some plan types might offer "out-of-network" coverage, this typically imposes additional out-of-pocket costs on the enrollee—usually a separate deductible and out-of-pocket maximum at double the in-network values as well as a higher coinsurance rate.

Networks are also prevalent in the Medicare Advantage (MA) market, which is large and growing. Over 3,000 MA plans are offered by private insurers as alternatives to Traditional Medicare (TM), with the average beneficiary having access to 39 plans in 2022.[2] Nearly half of all Medicare beneficiaries are enrolled in such plans.[3] MA issuers can enhance plan design relative to TM by modifying cost-sharing (e.g., adding an out-of-pocket maximum not present in TM) and benefits (e.g., dental, vision) that they offer, so long as they at least cover the services TM does. Nearly all MA plans are required to establish provider networks.[4] These networks can affect both cost and quality. For instance, if providers are selected into networks on the basis of more appropriate use of screenings, beneficiaries might benefit through lower utilization and better outcomes. By contrast, if insurers construct networks such that accessing providers becomes difficult for beneficiaries, then patients may face reduced access to care, leading to worse outcomes in the future. Because of this, understanding how networks influence enrollee access to care and how that is associated with market factors is crucial for assessing the performance and value of the MA program.[5] Understanding whether particular geographies – for instance, those with more concentrated MA markets or rural areas – are disproportionately affected by provider networks can help guide regulatory change to minimize harms.

A routinized means of assessing the influence of MA networks on access could help focus policy development and regulatory scrutiny. A data-based measure of network restrictiveness would allow for important outcomes research that could inform regulatory thresholds and enforcement.

Note that while defining access to care may be challenging, for our purposes, 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,[6] it is not clear whether the requirements are binding on plans or how they may affect access to care. Indeed, regulatory review of networks is rare with only one percent of contracts having networks reviewed in early years of network adequacy guidelines, and the vast majority of exceptions to requirements being approved.[7] One way to increase the scope of regulatory review without being overly burdensome is to measure the effects of MA plan network extent from readily available data, following up with regulatory effort where it appears most warranted (e.g., where access to care is most constrained).

One approach to assessing networks and their impacts is to use provider directories published by MA plans. These have recently become machine-readable and available to researchers. With them, researchers have examined the scope and breadth of provider networks in MA,[1,8] as well as their relationship with plan quality.[9] But, a key limitation is that provider directories are error prone.[10–12] 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 are likely to be more restrictive. 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.[13] In short, access to care as conveyed by directories may not reflect the reality that enrollees experience.

One might take a different approach to assessing networks by relying on utilization data. Such an approach is used by New Hampshire for provider network regulation on ACA Marketplace plans. The state's insurance regulator identified the share of available providers (based on the state's all payer claims database) that are considered in network (listed in the directory) for a given plan.[14] Note that this addresses some potential issues with the directory (e.g., listing providers as in network who no longer show up in claims) and the denominator (more accurately identifying available providers), but does not address other directory inaccuracies (such as listing providers who are available, but no longer contract with the plan).

In prior work, 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 beneficiaries instead of relying on network directories.[15] (Note that going forward, we refer to MA-PD plans as MA plans for simplicity.) We assessed MA 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 are able to observe the extent to which networks restrict access to available prescribers. (One could alternatively use MA encounter data and make the same kind of inferences, but the number of years for which such data are known to be of adequate quality is small.)[16] 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 accessible to beneficiaries, and which 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 approach that can be applied to all MA contracts, avoiding 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 provider networks on access (hereafter called network "restrictiveness"). We use the relationship between utilization and plan-level factors as well as demographics for TM beneficiaries to estimate counterfactual utilization for MA beneficiaries had they not been subject to the influence of networks. Results are presented for a high-prescribing specialty (primary care) 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, which we review here. We focused on Local Coordinated Care Plans (CCPs). These are the plans drawing the majority of enrollment in MA, those that are open to public enrollment, and those that are required to maintain provider networks. Additionally, 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 (HMO, PPO, HMO-POS) within a contract, observed networks may still vary between plan types within a contract.

Stand-alone prescription drug plans (PDPs) offer products in a similar way, except that they do not establish provider networks.

Study Data and Methods

Conceptual Framework

Consider an MA-PD contract i offering plans (stratified by plan type, h) in county c, and year t. Let Y represent the total number of unique providers seen by enrollees in this contract and let D^z represent whether enrollees are restricted to some provider network (typically only among providers who participate in TM), where $z = 1$ indicates the presence of restrictions and $z = 0$ indicates the absence. To estimate the restrictiveness of this contract, we would want to 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 (such as number of enrollees). Restrictiveness for a given observation can be expressed as the following ratio. Going forward, we interpret this as an observed to expected (O/E) ratio.

Eq. 1	$$\frac{(Y_{icht}\mid D^1)}{(Y_{icht}\mid D^0)}$$

Note that in theory, this ratio should be non-negative, and for specialties where MA restricts utilization, it must be less than or equal to 1. While we assumed that for most specialties, restrictions reduce the number of providers seen (as that is their intent), it is ambiguous as to whether networks would reduce the number of unique PCPs seen by beneficiaries. Indeed, there is evidence to suggest that MA beneficiaries may actually see *more* unique PCPs than those in PDPs.[17] We discuss how we incorporated this constraint further below.

A key challenge is that $Y_{icht}|D^0$ is unobserved among MA-PD beneficiaries and is a *potential* rather than realized outcome. This is the same concern present in all analyses that seek to estimate treatment effects.

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 TM beneficiaries in stand-alone prescription drug plans (PDPs) as a comparable population facing no network restrictions.

An important assumption here is that unobserved variables are not correlated with both utilization and selection into TM vs MA. We also assume that the covariates used to predict counterfactual utilization are correlated with utilization in a similar way for both MA and TM beneficiaries. To the extent that there are remaining unobserved variables correlated with either utilization or the covariates we condition on, they are likely to be related to selection on health status.[18] If MA enrollees are systematically healthier than TM enrollees in ways we don't observe, we expect that our counterfactual based on PDP utilization will be overestimate. In that case, our estimates would overestimate network restrictiveness. Sample construction and analysis are detailed below.

Data Sources and Study Sample

We focus analysis on two groups: first, the TM beneficiaries enrolled in a PDP between 2011 and 2017; second, the beneficiaries enrolled in an MA-PD plan over the same time period. 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 given 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). (Note that June is selected because it is the middle of the year.) We linked enrollment data to the 20% random sample of prescription drug events (PDEs) among Medicare beneficiaries on the unique, encrypted beneficiary ID.

Providers who prescribe to Medicare beneficiaries 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 CMS. We focus on primary care providers who can be listed as internal medicine, family practice, general practice, and geriatric medicine. Hospitalist primary care providers were excluded.

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. Thus, our unit of analysis was the year-contract-plan type-county. This is because while provider networks are officially regulated at the contract level, plan structure (e.g., gatekeeper HMO-style plans vs open-network PPOs) within a contract could affect utilization.

As noted previously, there must be a bound to the ratio of unique providers seen by MA-PD enrollees facing network constraints, to the number of unique providers we would expect them to see absent network constraints. We refer to this as the maximum tolerable O/E ratio. In order to identify this ratio, we relied on the 2018 Fee-For-Service (FFS) Carrier File and the 2018 MA Encounter Carrier File. These data include data on claims and encounters with professional providers, and were restricted to beneficiaries in stand-alone PDP plans in the FFS file and following prior work,[16] to contracts that have relatively complete data in the MA Encounter file. The methods for doing so are described in the "Estimating Maximum Tolerable O/E Ratios" section.

County-level characteristics were obtained from the Area Health Resource File (AHRF) produced by the Health Resources and Services Administration.

Variables and Definitions

Our key outcome of interest was the contract-plan type-county-year level estimate of network restrictiveness (Equation 1). The numerator was the observed number of providers seen by beneficiaries in an MA 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 plan's network constraints.

We considered a provider potentially in-network for plans in a county if there were beneficiaries in that county receiving prescriptions from that provider, even if that provider practiced in a different county. This is consistent with CMS' network adequacy requirements, that allow a provider to be part of a contracted network regardless of whether their practice is located within a particular service area.[6] 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 Table 1.

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.

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 (BETOS) codes classified as evaluation and management (E&M) by CMS. In the MA dataset, we restricted visits to those with E&M HCPCS 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 ($\frac{158{,}080}{4{,}378{,}181} \div \frac{133{,}570}{2{,}131{,}989} = 1.74$).

Prediction Model: Overview

To predict the expected number of in-network providers for an MA 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 listed in Table 1. We used the estimated coefficients from the PDP sample to predict the number of providers we would expect to see in the MA-PD sample if there were no network constraints. This provides a reasonable counterfactual as long as the relationship between the model inputs and the outcome are similar for TM and MA-PD contracts.

Our prediction approach modeled the number of unique providers seen by beneficiaries as a function of several variables. These variables included: the total number of unique providers in a given specialty seen by any beneficiary in the county, the total number of all MA enrollees in the county, the number of enrollees in the observation, the number of prescription drug events among enrollees, binary indicators for the state, the average age of enrollees, the percent of enrollees in several age groups (less than 65, 65 to 74, 75 to 84, and 85+), and the share of beneficiaries that died in a given year.

To select an appropriate model, we compared the performance of a quasi-Poisson model with state fixed effects and a random forest model. The quasi-Poisson model is well-suited for count outcomes and previous work found that the random forest model outperforms other algorithms for predicting count outcomes in a health care setting.[19] We considered a negative binomial, Poisson, and a zero inflated Poisson model as well, but these failed to converge. While some variations converged with a smaller subset of covariates, these were likely to be less robust for prediction, which is why we focused on quasi-Poisson and random forest instead. Additionally, the quasi-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).[20] All models were estimated using five-fold cross validation.[21] 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 quasi-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. We tested every possible value of the number of variables with every value of the maximum number of trees from 10 to 500 in intervals of 10, giving over 1,000 combinations of the two hyperparameters. We selected hyperparameter values that minimized RMSE.

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.

Thus, 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. Our other model selection metrics, RMSE and RMAE, are sensitive to the number of observations, and thus are mechanically larger as the sample size shrinks (or the number of PDEs required for inclusion grows).

O/E ratios were calculated by taking the observed value for the observation and dividing by the predicted value from the model. O/E ratios greater than 1.74, the maximum tolerable O/E ratio, were topcoded and set to 1.74 (this occurs less than one percent of the time). To examine performance across these metrics, we standardized each metric (to account for differences in scale) and averaged across them.

Relationship With Market and Plan-Level Factors

After developing a measure of network restrictiveness for each contract-plan type-county-year observation among MA-PD plans, we sought to understand whether this measure is associated with various plan and market-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 Herfindahl-Hirschman Index (HHI) of the county,[22] 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.

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.[15,23,24]

The general form of our specification is in equation 2.

Eq. 2	$restrictiveness_{ipct} = \beta_0 + \theta X_{ct} + \gamma\, Z_{ipct} + C + T + \varepsilon_{ipct}$

Where i indexes a contract, p indexes a plan type, c indexes a county, and t indexes a year. $restrictiveness$ 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 also 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 OLS with standard errors clustered at the county level.

Robustness Checks

One potential concern was that our results might be sensitive to the amount of enrollment and prescription drug activity in a contract. In a contract with many PDEs relative to enrollment, or simply with high enrollment, we may identify more providers than in smaller contracts. This would lead to measurement error if we observed differentially sized contracts among MA-PDs relative to PDPs.

While we control for the number of PDEs and enrollment in our predictive models, there may be residual differences between MA-PDs and PDPs. To better understand the risk of measurement error from differentially sized contracts, we examined the relationship between overall PDE volume and our estimated measure of network restrictiveness.

Separately, we conducted two additional validation exercises. First, if our estimates of network restrictiveness are directionally accurate, we would expect that vertically-integrated insurers are 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 assure ourselves that our overall estimates are not driven by an inability to observe enough 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.

Additional details can be found in the Appendix.

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 observations and 86.5% were PDP observations (N=414,204). In nearly every comparison, the random forest algorithm outperformed the quasi-Poisson. Across 100 cutoffs, 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. Only the percent of observations with O/E>1.74 (lower indicates better performance) was negligibly higher in the quasi-Poisson. (Table 2)

Within the same dataset, we evaluated the average performance of the random forest algorithm for each of 100 cutoffs. Of these, a cutoff of greater than or equal to 961 prescription drug events had the best performance. (Figure 1) 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 final hyperparameters selected, we used the random forest algorithm to generate O/E ratios as discussed in the methods. Our final analytic dataset for our market factors analysis, restricted to MA contracts, included 64,253 observations (99.8% of observations with non-missing county-level variables).

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 contracts was 41.1% of what we would expect it to be absent network restrictions. After weighting for beneficiary enrollment, the estimated network restrictiveness was 60.6%. Going forward, all results are presented after weighting for beneficiary enrollment unless otherwise noted.

With respect to market and plan-level factors, we found several unadjusted relationships consistent with prior literature and some that were not. 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) 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%). Lastly, we found that rural areas had the most restrictive networks (31.6%; 95% CI 29.0% to 34.2%) while metropolitan areas had the least restrictive networks (61.5%; 95% CI 61.3% to 61.7%).

Multivariable Results

After accounting for various potential sources of confounding, we observed similar results as in our naïve analyses. (Table 3 presents standardized coefficients). Results were similar 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 3), 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).

Similarly, networks in rural areas were more 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 HHI of the MA market 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 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 little evidence for an association with area-level income, the number of Veterans in a county, or the HCC risk score of the TM population in a county.

Robustness Checks

We present results from several robustness checks. First, Appendix A1 illustrates that our average restrictiveness measure is robust to inclusion of contracts with varying amounts of PDE counts. Second, Appendix A2 confirms the theoretical prediction that Kaiser (an integrated contract) has a network substantially more restrictive than other contracts in California. Lastly, Appendix A3 shows three coefficients (PPOs, providers per 1,000 population, and market share) and that they vary relatively little by changing the sample based on PDE count.

Discussion

In this analysis, using prescription drug event data, we developed a novel approach to measuring the effects of provider network restrictions on utilization. Focusing on primary-care physicians as a high-prescribing specialty, we estimated that provider networks tended to be most restrictive among 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 restrictive) but not for others (i.e., our findings on rurality). The differences in our work likely stem from how we estimate the denominator. In particular, our approach measures observed utilization relative to counterfactual utilization and allows providers to be considered in-network for a given county whether they have an office in that county or not. Moreover, because we estimate restrictiveness rather than network breadth, our estimates may not be directly comparable to estimates of network breadth based on directories.

Our approach relied on off-the-shelf machine learning techniques that could be applied by researchers and regulators interested in assessing the *observed effects* of provider network configurations rather than reported network construction. This approach overcomes concerns about using network data from insurers, which may include providers who don't have room on their panels or who no longer practice, or may incorrectly list providers as being in-network. Additionally, this approach obviates the need for correctly identifying provider locations (since beneficiary county of residence is used), further addressing issues with measuring the number of providers available in an area.

While this approach cannot be applied proactively, it provides regulators a tool with which to assess networks retroactively. If networks are marketed as relatively broad and accessible, but in fact restrict access substantially, such an approach allows regulators to more effectively target potential audits when oversight resources are limited.

There are several important limitations to our work. First, because our measure of provider networks uses an inferred counterfactual network extent (e.g., how many providers would beneficiaries see without network constraints), it is not directly comparable to existing measures of network breadth that are assessed relative to a fixed number of providers in a market. This means that for regulatory purposes, it would be useful retrospectively, but cannot be used to examine networks contemporaneously. Indeed, we view our measure as complementary to traditional approaches to examining network breadth.

Second, if there are remaining unobserved differences between MA-PD and TM plans that both affect the number of providers seen by enrollees and interact differently with market factors, our prediction model may be inaccurate. The relatively high calibration slope suggests that our chosen model is accurate in predicting the number of unique providers seen by beneficiaries, which helps alleviate some concerns on this front. Nonetheless, unobserved variables are a common concern in research relying on observational data, and thus can rarely be fully overcome with complete certainty. In particular, if there are remaining unobserved variables correlated with utilization and selection into MA vs TM, then our measure might overestimate network restrictiveness in MA.

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. It is possible that other models or an ensemble model would outperform our prediction approaches. This is a necessary limitation in nearly all prediction tasks.

Lastly, our analysis relating various factors to network restrictiveness is descriptive and correlational. One should not infer causality from any of our analysis, as it is designed to describe the landscape of network restrictiveness rather than measure the effect of any one variable on another.

In conclusion, relying on widely-accessible claims data, we applied a machine learning algorithm to estimate the effective network restrictiveness of primary care provider networks in MA. In future work, we will expand this analysis to other high-prescribing specialties and compare our results more directly with those obtained using network directories. Additional validation may also include comparisons to survey responses with respect to provider access and measures of receiving preventive services.

Table 1. Variable Definitions

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+.

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

Table 2. Quasi-Poisson vs Random Forest Algorithm Prediction Performance

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

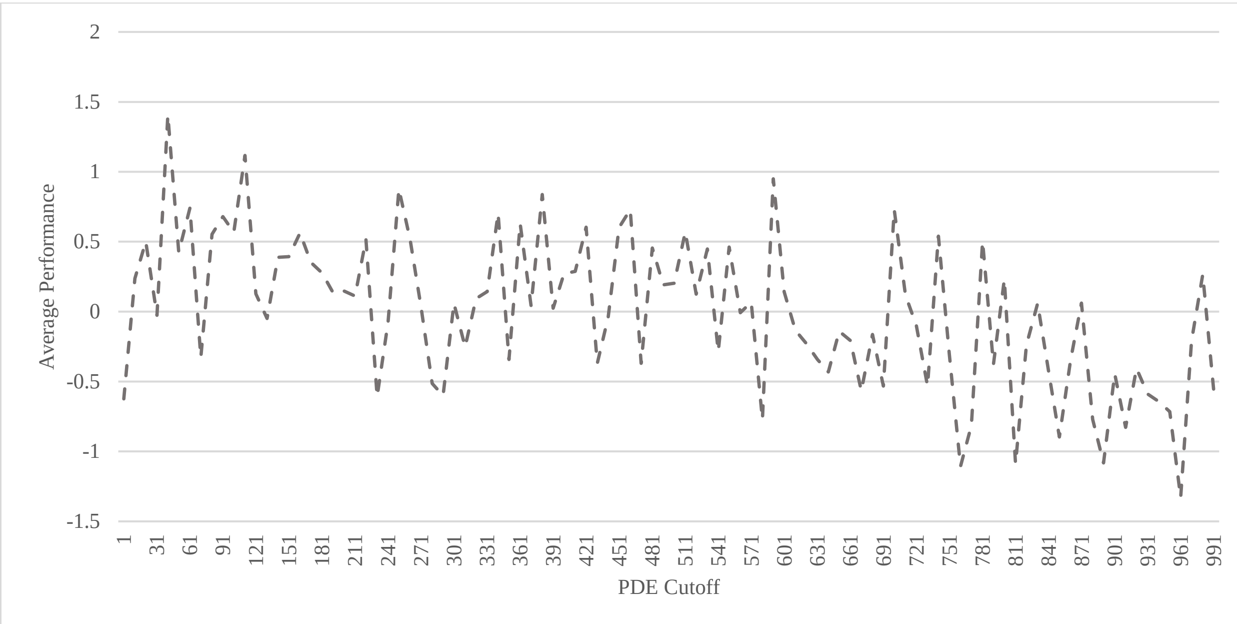
Notes: Emax indicates the maximum absolute difference between predictions and observed values. Calibration slope indicates the r-squared from a regression of the observed value on the expected value among PDPs. % O/E>1.74 indicates the percent of observations with O/E>1.74 in the MA sample. RMSE and MAE are the root mean squared prediction error and mean absolute error, respectively.

Table 3. Multivariable Results: Association Between Network 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

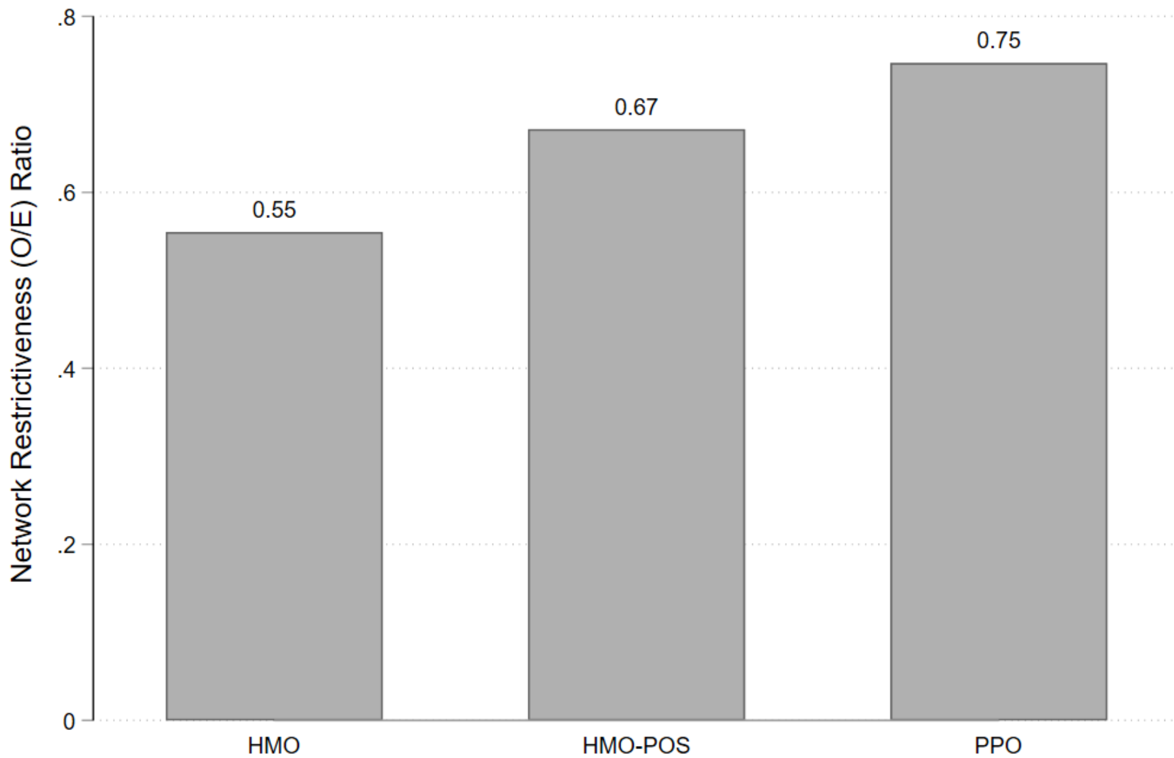
Notes: All models include state fixed effects. 95% confidence intervals calculated from heteroscedasticity-robust standard errors clustered at the county level in brackets. Observations vary due to singletons. Effective year of contract indicates when the contract became active in MA. Coefficients are 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.

Figure 1. Random Forest Average Performance For Different PDE Cutoffs



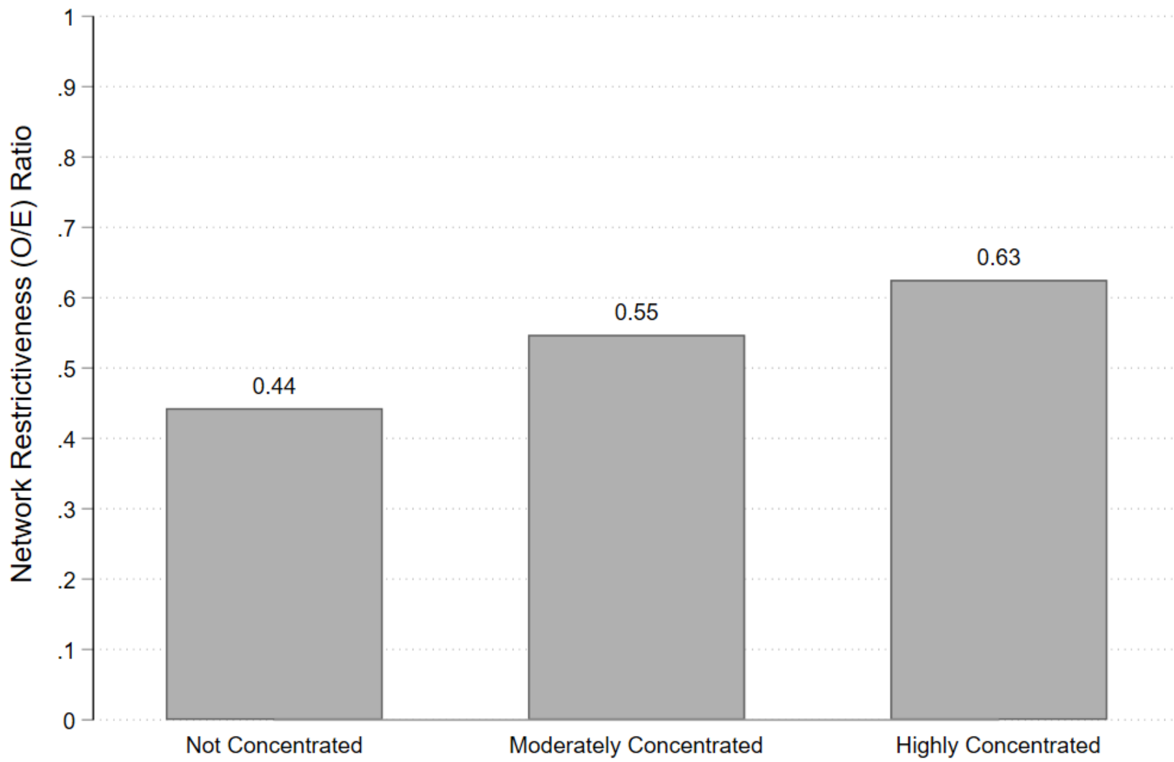
Notes: Performance indicates the average across standardized measures of Emax and the percent of observations with an O/E greater than 1.74. A lower value indicates better performance. These results indicate that a PDE cutoff of 961 was optimal for maximizing the performance of the prediction model.

Figure 2. Network Restrictiveness by Plan Type



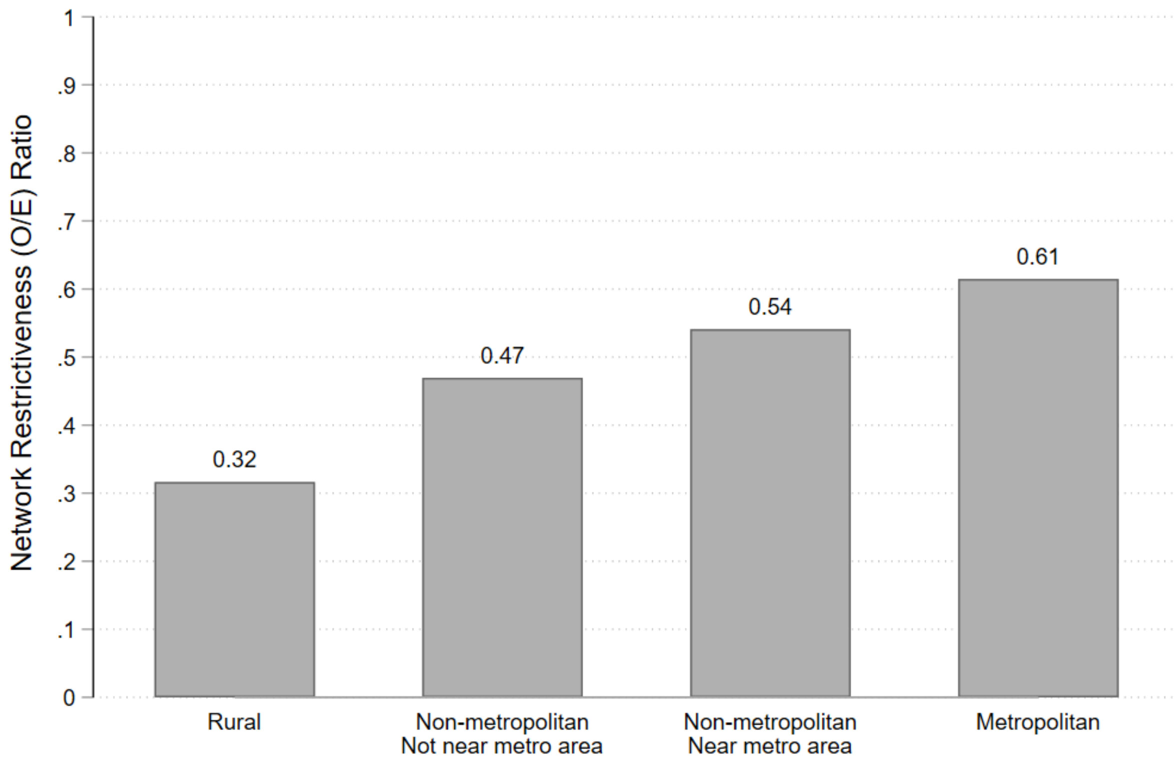
Notes: Plan type data is obtained from CMS plan characteristics file. Observations are weighted first by the number of beneficiaries in contract-plan type. Network restrictiveness is measured by creating an observed-to-expected (O/E) ratio of the observed unique number of providers seen by beneficiaries 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. HMO: Health Maintenance Organization; HMO-POS: Health Maintenance Organization-Point of Service; PPO: Preferred Provider Organization

Figure 3. 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.

Figure 4. Network Restrictiveness by Rurality



Notes: Rurality is defined at the county-level from the Area Health Resource File. 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.

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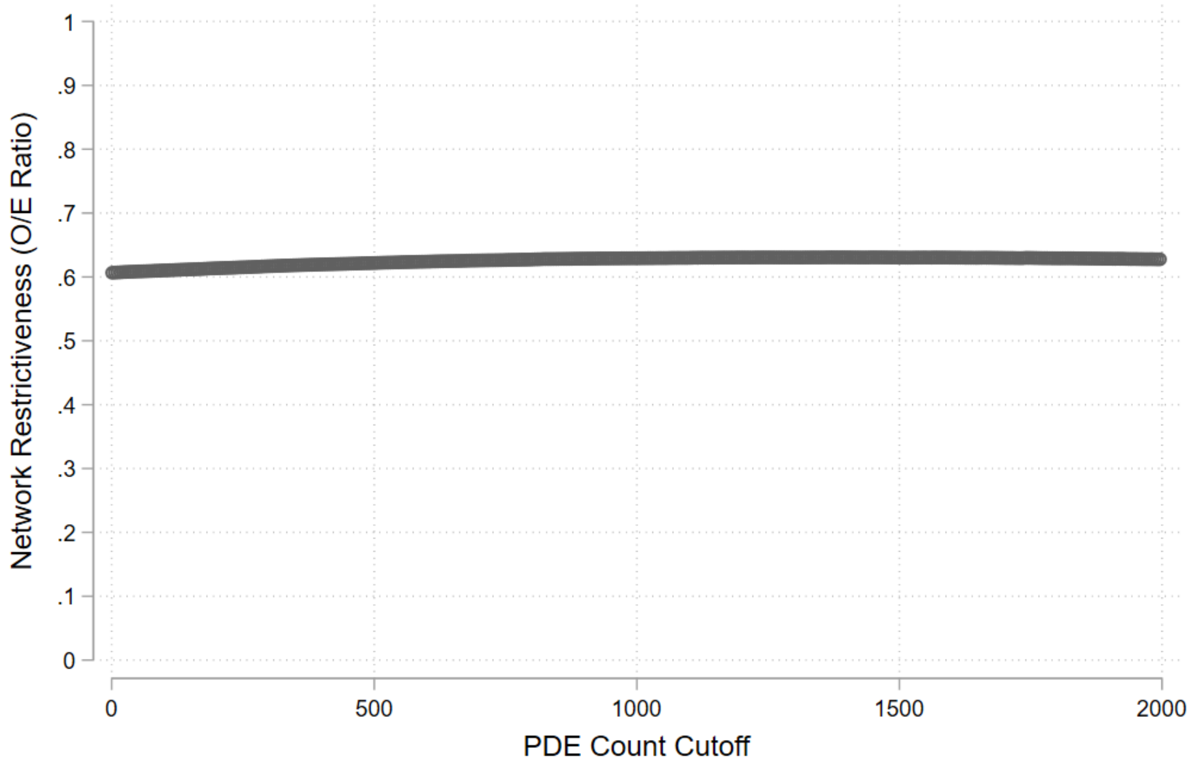
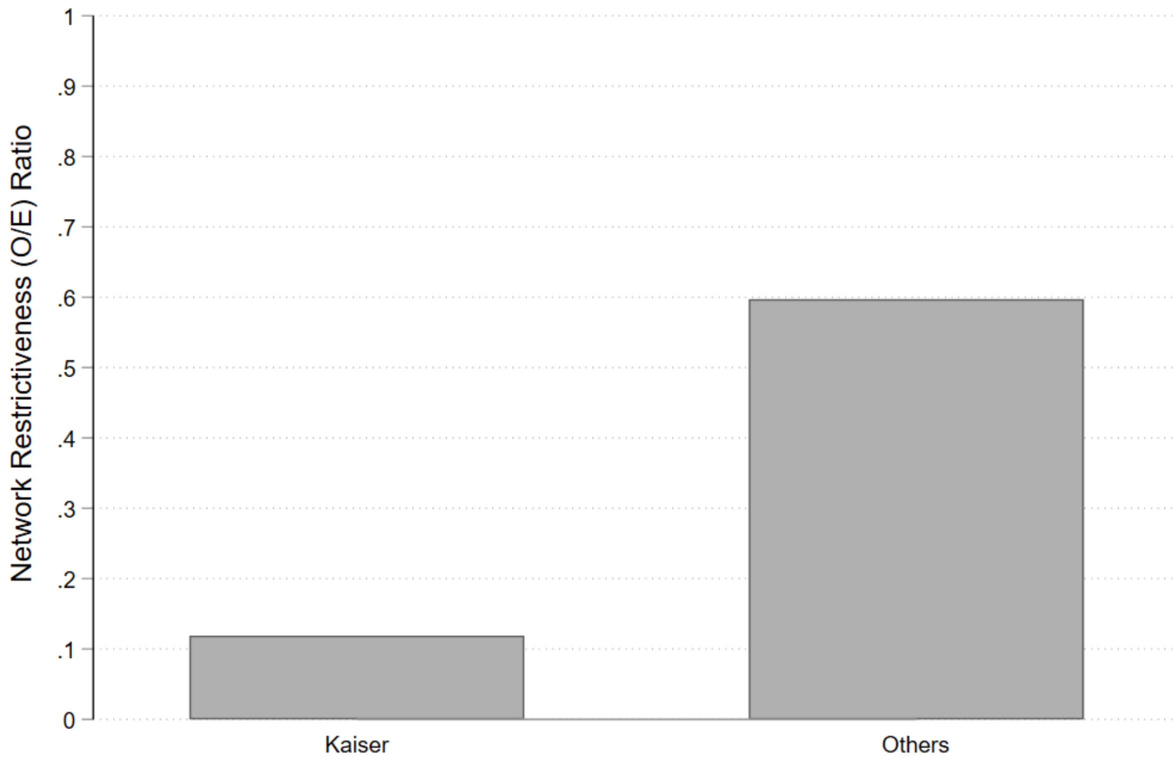
APPENDIX

Appendix A1. Relationship Between PDE Count and Network Restrictiveness (Enrollment Weighted)



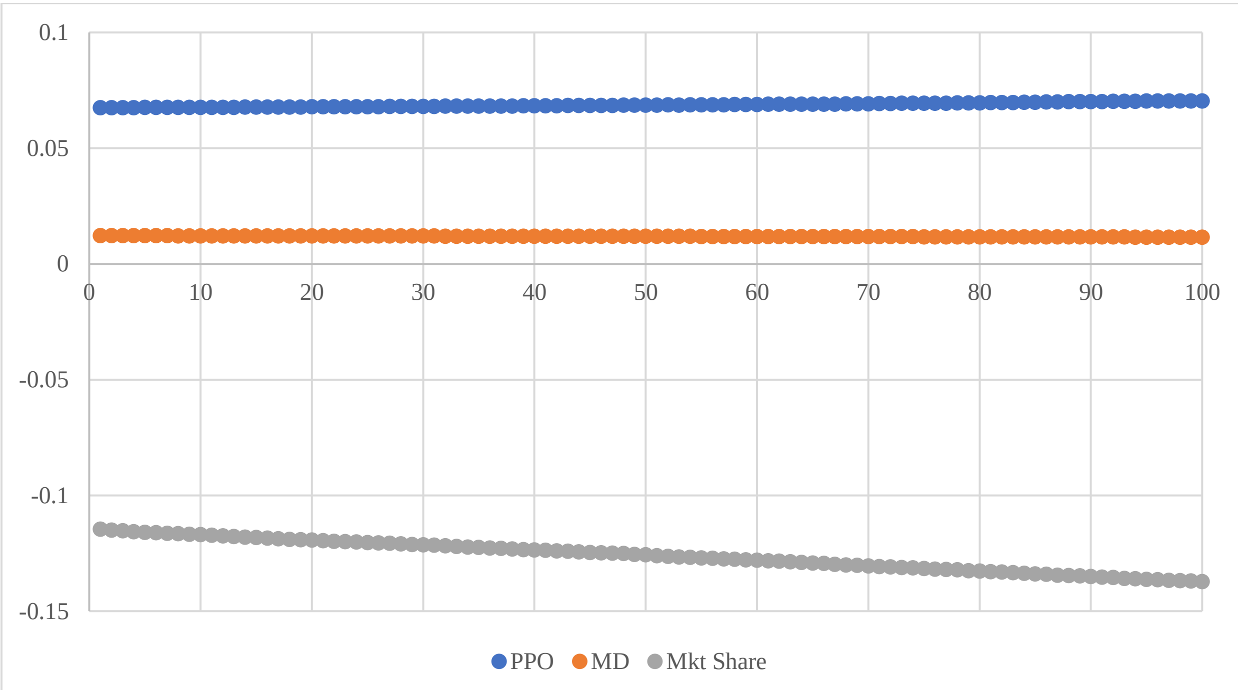
Notes: This illustrates the estimated network restrictiveness with observations limited to those with the number of prescription drug events at or above the indicated cutoff. This indicates that estimates of network restrictiveness are not sensitive to underlying volume. Observations are weighted by 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 A2. 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 is aggregated across all years of data. Complete methods are described in the text.

Appendix A3. 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.