What Does a Provider Network Do? Evidence from Random Assignment in Medicaid Managed Care

By Jacob Wallace*

Leveraging the random assignment of over 50,000 Medicaid enrollees in New York, I present causal evidence that narrower networks are a blunt instrument for reducing health care spending. While narrower networks constrain spending, they do so by generating hassle costs that reduce quantity, with modest effects on prices paid to providers. Enrollees assigned to narrower networks use fewer of both needed and unneeded services, and are less satisfied in their plans. Using my causal estimates to construct counterfactuals, I identify an alternative assignment policy that reduces spending without harming satisfaction by matching consumers with narrower networks that include their providers.

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I. Introduction

Health care spending is projected to outpace growth in the rest of the economy over the next decade, leading to considerable interest in strategies that control health care costs.[1] One approach has been to use demand-side cost sharing, such as deductibles and copays, which require that consumers pay a greater share of the price of their care.[2] However, demand-side incentives are increasingly viewed as a blunt tool for reducing health care spending because they are not well-understood by consumers (e.g., Handel and Kolstad, 2015*b*; Bhargava, Loewenstein and Sydnor, 2017) and lead to reductions in the use of needed (and unneeded) health services (Baicker, Mullainathan and Schwartzstein, 2015). Due to these limitations, there is renewed interest in whether supply-side policies like narrow networks—that restrict patients to a small set of physicians and hospitals—may be a more effective way to control health care costs.

Compared to the substantial academic literature on the impact of demand-side cost sharing, relatively little is known about the impact of narrow networks. Network restrictions aim to reduce health care spending by steering patients away from high-cost providers, but they do so by limiting consumer choice and potentially disrupting ongoing provider relationships.[3] They also increase the costs (or reduce the utility) of using care by requiring patients to travel further, wait longer for appointments, or see less desirable providers. Whether these hassle costs lead to broad-based reductions in the quantity demanded of both needed and unneeded care—akin to demand-side cost sharing—or to more targeted reductions in wasteful services—remains an open question. Recent evidence based on individuals with employer-sponsored insurance suggests narrow networks may reduce health care spending without harming quality (Gruber and

[1]National Health Expenditure Projections 2017-2026, Centers for Medicare and Medicaid Services.

[2]For evidence on the impact of demand-side incentives, see, for example, Manning et al. (1987), Finkelstein et al. (2012), or Brot-Goldberg et al. (2017).

[3]There is growing evidence that consumers prefer broader networks and networks that contain their preferred providers (Ericson and Starc, 2015; Shepard, 2016; Drake, 2018). The welfare effects of disrupting ongoing provider relationships are ambiguous. If narrow networks exclude less efficient providers, disruptions will tend to shift patients away from low-quality or high-cost providers, but recent evidence suggests that the gains from steering patients to more efficient providers may be modest relative to the costs associated with the disruptions (Kwok, 2019).

McKnight, 2016), but, due to limitations in study design, the researchers were unable to distinguish the impact of narrow networks from other potential differences in how narrow and broad network plans ration care.

In this paper, I use administrative data for over 50,000 randomly assigned Medicaid managed care enrollees in New York to estimate the causal impact of provider network breadth on health care spending, quality, and consumer satisfaction. Medicaid managed care—where states contract with managed care organizations ("plans") to deliver covered services—is an ideal setting to study provider networks because network restrictions are common (Ndumele et al., 2018), covered benefits and cost sharing are identical across plans, and a subset of enrollees are randomly assigned to plans.[4] To permit a comparison of my results to those of prior studies that examine the effects of demand-side and supply-side incentives in health care (e.g., Gruber and McKnight, 2016; Brot-Goldberg et al., 2017), I focus on the outcomes examined in those studies—health care utilization, spending, and the use of potentially high-value and low-value care—and supplement them with a utility-based measure of plan satisfaction. To construct the satisfaction measure I use ex-post demand, specifically whether randomly assigned enrollees remain in their assigned plans. While it differs from a traditional willingness-to-pay measure, in a world of consumer choice frictions (e.g., Handel and Kolstad, 2015*b*; Handel, Kolstad and Spinnewijn, 2019) an advantage of this measure is that it reflects the utility an enrollee experiences in their assigned plan (Israel, 2005).

My identification relies on the fact that enrollees are assigned to different physician and hospital networks based on the zip code ("zip") they live in—zip is the level at which I measure provider network breadth—and the plan they are randomly assigned to. This helps address the key threat to internal validity—unobservable selection on network breadth (Shepard, 2016). However, a second threat to internal validity exists in this setting. In addition to affecting an enrollees's provider network breadth,

[4]In addition, I observe enrollees' health care utilization and provider choices prior to assignment. I observe these baseline characteristics while enrollees are temporarily covered by the publicly-operated, Medicaid fee-for-service program, allowing me to test for balance using a rich set of baseline outcomes (e.g., health care spending) and identify enrollees' usual sources of care prior to assignment.

the plan an enrollee is assigned to may directly impact their outcomes through other plan-level, supply-side tools such as prior authorization, care management, or physician gatekeeping (e.g., Cutler, McClellan and Newhouse, 2000), potentially confounding the true relationship between network breadth and enrollee outcomes.

The ex ante direction of this bias is unclear. While it is tempting to think that narrow network plans would employ more draconian utilization management tools, that need not be the case. In my setting, the narrowest network plan is wholly-owned by a public hospital system and—perhaps as a result of this ownership structure—takes a less aggressive approach to using managed care tools to ration services.[5] To distinguish the effect of network breadth from that of each plan, in some specifications I control for the assigned plan (in addition to zip) and estimate the causal effect of network breadth using the rich variation that remains at the plan-by-zip level. My primary results are qualitatively similar whether or not I include plan controls.

My estimates should not be thought of as measuring the effects of enrollment in a *narrow network* versus *broad network* plan (e.g., Gruber and McKnight, 2016), as narrow and broad network plans differ along multiple dimensions.[6] Rather, my approach isolates the effects of enrollment in a health plan with a *narrower* provider network, holding other plan characteristics fixed. This is a supply-side analog to estimates of the consumer response to demand-side cost sharing (e.g., Manning et al., 1987), where here we are varying provider network breadth rather than a cost sharing parameter (e.g., the coinsurance rate).

To quantify network breadth, I use techniques adapted from the literature. First, I estimate hospital and physician demand systems using micro-data on health care use and provider choice for the Medicaid managed care population.[7] The models recover a

[5]Estimates of causal plan effects reveal that enrollees assigned to this plan used more care and generated more spending than the average auto-assignee, despite the plan's narrow network. An in-depth discussion of the full set of plan effects, which are economically and statistically significant, is beyond the scope of this paper and covered in Geruso, Layton and Wallace (2020). In my specifications without plan controls, I remove the enrollees in this "provider-owned plan."

[6]For example, a broad network PPO and a narrow network formed by an integrated system like Kaiser Permanente may differ both in terms of non-network characteristics (e.g., vertical integration, physician incentives, etc.) as well as the number of physicians and hospitals that are in-network.

[7]My specification is based on the pioneering work of Town and Vistnes (2001), Capps, Dranove and

significant hassle cost for going out-of-network, an important finding in Medicaid where, unlike private insurance, cost sharing is not higher for out-of-network care.[8] These estimates are then used to simulate provider choices in an "unconstrained" counterfactual in which all providers are in-network for all plans. I measure network breadth as the share of these simulated visits covered by each plan's provider network at the plan-by-zip-by-year level. I then examine whether post-assignment outcomes differ for enrollees who live in the same zip code but are randomly assigned to broader or narrower provider networks based on the breadth of their assigned plan's network in that zip code.[9]

I find evidence that narrower networks constrain health care spending by lowering quantity, with modest effects on the prices paid to providers. A one standard deviation reduction in assigned network breadth was associated with a decrease of 6.7 log points (7%) in spending.[10] Health care prices were a small factor in the observed spending reductions, a difference from prior evidence on the impact of narrow networks in employer-sponsored insurance (Gruber and McKnight, 2016; Atwood and LoSasso, 2016). Given that quantity reductions drive my spending results, I examine what services enrollees in narrower networks forgo. Following Brot-Goldberg et al. (2017), I assess enrollees' use of potentially high-value (i.e., "needed") services believed to be effective at improving population health and reducing the incidence of costly disease (Chernew, Schwartz and Fendrick, 2015), and potentially low-value (i.e., "uneeded") services cited for potential overuse (e.g., imaging and lab). The service-level results suggest that restrictive provider networks are a blunt tool for reducing health care spending—enrollees in narrower networks use fewer needed and unneeded services—and, in addition, enrollees in narrower networks are less satisfied with their plans (i.e., they have lower experienced utility).

I next examine which characteristics of narrower networks are most important for

Satterthwaite (2003), and Ho (2006, 2009).

[8]In place of higher cost sharing, Medicaid plans often require that enrollees have a justification for requesting out-of-network referrals and, even when approved, out-of-network providers are often unwilling to treat Medicaid enrollees.

[9]My primary results are robust to using two other measures of network breadth in the literature: "network utility" and the "covered share of visits." They are highly correlated with my preferred measure ($\rho \approx 0.90$).

[10]At the plan-level, a one standard deviation reduction in network breadth was associated with a plan's network including 4,000 fewer physicians and 7.5 fewer hospitals in New York City.

explaining my results. I measure the breadth of each plan's physician and hospital networks, finding evidence that physician network restrictions drive the reductions in health care spending (and effects on quantities) while reductions in consumer satisfaction are driven by both physician and hospital network restrictions. I then examine whether the effect of a narrower network is mediated by whether that network contains an enrollee's usual source of care (often a hospital for Medicaid enrollees).[11] To do this I construct a sample of approximately 25,000 Medicaid enrollees whose usual source of care (i.e., "provider") I observe prior to assignment. I then modify my specification by adding an indicator for whether an enrollee's assigned plan included that provider. When I include this variable, I no longer find an association between provider network breadth and consumer satisfaction, suggesting that restrictive networks reduced consumers experienced utility primarily by disrupting ongoing relationships with providers (similar to findings in Higuera, Carlin and Dowd (2018)). Enrollees who were assigned to a network that did not include their provider were 5.2 percentage points (90%) more likely to switch plans—a very large effect—and 81% of switchers moved to plans covering their provider.

Finally, I consider whether an alternative auto-assignment policy could reduce health care spending without harming consumer satisfaction.[12] Using my causal estimates to predict each enrollee's counterfactual spending and satisfaction, I reassign enrollees across plans—using only information available to the state at the time of assignment—to minimize spending for any given level of satisfaction. The results are striking. Relative to the current policy, the state could reduce the likelihood enrollees switch plans by approximately 10% without increasing cost or, alternatively, reduce spending by nearly 2.5% without lowering satisfaction. Furthermore, the exercise identifies a set of alternative assignments that increase predicted satisfaction *and* reduce predicted spending.

[11] Since enrollees are covered by a public, Medicaid fee-for-service plan prior to assignment, I identify their usual source of care at baseline before they are constrained by the network of their assigned plan. I am only able to identify an enrollee's usual source of care prior to assignment if I observe a physician or hospital claim in their first three months in Medicaid fee-for-service. This restriction reduces my sample to 25,256 unique enrollees.

[12] As motivation for this exercise, I note that out of the 25,256 enrolles whose provider I observed prior to assignment, only 67% were assigned to a plan including their provider but 97% had a provider participating in at least one plan.

Intuitively, this is achieved by matching enrollees with narrower networks (to reduce spending) that nevertheless include their usual source of care (to increase satisfaction). These simulations have clear policy implications for New York but offer a broader lesson to the more than 30 states that operate mandatory Medicaid managed care programs: auto-assignment can be a powerful tool for achieving program goals (e.g., reducing cost and increasing satisfaction) without unnecessarily restricting enrollee choice of plans.

This paper contributes to three strands of the literature on health care provider networks. My research is most closely related to recent work on the impact of narrow networks in employer-sponsored insurance markets (Gruber and McKnight, 2016; Atwood and LoSasso, 2016). I extend this literature by examining the impact of narrower networks in Medicaid managed care, where network restrictions are more common and random assignment allows me to recover causal effects. By examining a wider set of outcomes, including a utility-based measure of plan satisfaction, I document that narrower networks, like high deductibles (Brot-Goldberg et al., 2017), are a blunt instrument for reducing health care spending. The results suggest that—at least in this context—there are real tradeoffs to narrowing networks—a finding that diverges from prior work (e.g., Gruber and McKnight, 2016) and represents a key contribution of this study. My paper is also linked to a related literature on how to measure network breadth, where the methods I use were originally developed to study insurer-hospital bargaining (e.g., Gaynor and Vogt, 2003; Shepard, 2016; Ho and Lee, 2019). To the best of my knowledge, these methods have not been used to measure physician network breadth nor have they been applied in the Medicaid context, two additional contributions of this paper. Lastly, my research connects to a literature on pricing and consumer valuation of provider network breadth (Dafny, Hendel and Wilson, 2015; Ericson and Starc, 2015; Polsky, Cidav and Swanson, 2016). Using a novel, utility-based measure of consumer satisfaction—whether randomly assigned enrollees remain in their assigned plans—I find evidence that narrower networks reduce satisfaction, with sicker consumers placing the highest value on broader networks (Drake, 2018).

This paper is also related to the broader literature in health economics on the design of optimal health insurance in the presence of moral hazard. A large body of research explores the role of demand-side incentives in constraining health care spending.[13] A much smaller literature explores how supply-side policies—that restrict access to health care providers, services, or technologies—impact health care spending and consumer satisfaction (e.g., Cutler, McClellan and Newhouse, 2000; Marton, Yelowitz and Talbert, 2014; Clemens, Gottlieb and Molnár, 2017; Van Parys, 2017; Cooper et al., 2018; Geruso, Layton and Wallace, 2020). A related set of papers examine how health care spending and outcomes change when public programs contract with private managed care plans to provide covered services, as with Medicaid managed care (e.g., Aizer, Currie and Moretti, 2007; Duggan and Hayford, 2013; Dranove, Ody and Starc, 2017; Duggan, Gruber and Vabson, 2018; Layton et al., 2018; Curto et al., 2019; Agafiev Macambira et al., 2021). These studies rarely distinguish which managed care mechanisms drive their results, a contribution of my work. My results suggest that supply-side tools that constrain cost, such as narrower networks, offer similar tradeoffs to demand-side tools like patient copays and deductibles in that they do not solely target "wasteful" services for removal, but rather reduce the quantity demanded for both needed and unneeded care and lower consumer utility.

Finally, my work is related to a literature in economics on how to allocate resources in markets without prices (e.g., Roth and Peranson, 1999; Abdulkadiroğlu and Sönmez, 2003; Roth, Sönmez and Ünver, 2004; Bansak et al., 2018). Medicaid bears some resemblance to these settings in that there are no prices to consumers (i.e., premiums are zero) but enrollees often have a choice among a set of differentiated plans. In addition, states operating mandatory Medicaid managed care programs have to develop mechanisms to assign passive enrollees (i.e., those that don't make choices). My results suggest that "smart defaults" could be an effective mechanism for achieving programmatic goals in Medicaid without restricting consumer choice among plans. Smart default policies are common in other contexts—such as retirement investment decisions (Carroll

[13]Papers in this literature include Manning et al. (1987), Newhouse (1993) , Chandra, Gruber and McKnight (2010), Brot-Goldberg et al. (2017), Abaluck, Gruber and Swanson (2018), and Chandra, Flack and Obermeyer (2021).

et al., 2009)—and are gaining traction in health insurance markets (see for e.g., Handel and Kolstad, 2015*a*). With nearly half of Medicaid managed care enrollees being auto-assigned to a health plan, rather than making a choice (Smith et al., 2015), smarter defaults could be a powerful tool for improving the efficiency of the Medicaid program.

The rest of the paper is organized as follows. Section II describes my data and setting. Section III describes how I measure network breadth. Section IV describes the research design. Section V presents my main results. Section VI examines which network characteristics (e.g., physician vs. hospital breadth) are most important for explaining my effects. Section VII explores counterfactual auto-assignment policies. Finally, Section VIII discusses the implications of the study and concludes.

II. Data and Setting

To estimate the impact of provider network breadth, I analyze administrative health records obtained from the New York State Department of Health for the time period 2008 to 2012. I restrict the analyses to New York City—the second largest Medicaid managed care market in the United States—where two-thirds of the Medicaid enrollees in New York State reside. The data set includes three key components. First, I obtained demographic data, monthly enrollment data, and the universe of medical claims covered by Medicaid for each enrollee. The medical claims include detailed patient diagnoses, procedures, provider identifiers, and the amount paid by the insurer (which may be the state or a Medicaid managed care plan). These data elements are similar to those used in other recent studies of how demand-side and supply-side incentives affect health care utilization (e.g., Gruber and McKnight, 2016; Brot-Goldberg et al., 2017). I use these data to construct enrollee-month level outcomes related to health care use and spending, which I discuss in more detail below. Second, I obtained the list of physicians and hospitals covered by each Medicaid managed care plan during the study period. The provider network directories allow me to observe the *de jure* network for each plan, rather than having to use patient flows to infer which physicians and hospitals each plan covers. Third, and key to my identification strategy, the enrollment data identifies the subset of enrollees that are randomly-assigned to their Medicaid managed care plan by the state's "auto assignment" algorithm. Appendix Section A provides additional details on the data used to estimate the effects of provider networks.

A. *Medicaid Managed Care in New York*

New York Medicaid offers an ideal setting to study the effects of provider networks. The state relies on private, managed care plans that contract with different sets of physicians and hospitals but offer identical benefits with no cost sharing.[14] In the absence of cost sharing, differences in the outcomes of enrollees assigned to different plans can be attributed to the supply-side tools plans use, including provider networks. To construct their networks, the plans bargain directly with physicians and hospitals, contracting on both negotiated prices (which are observed in the data) and the terms of payment (i.e., fee-for-service or risk-based contracting). While plans must comply with network adequacy standards—rules for how many and what types of providers they must contract with—I observe large differences in the networks across plans. For example, plans ranged from covering 39.6% to 84.9% of the hospitals in New York City during my study period, a more than two-fold difference.

Little is known about the ways in which network restrictions affect how enrollees access care in Medicaid. For patients with private insurance, the use of an out-of-network provider often results in higher out-of-pocket costs than what they would pay for the same service at an in-network provider. However, there is no cost sharing in Medicaid (including for out-of-network care), yet only a small share of physician and hospital visits are to out-of-network providers. Further research is needed to understand why network restrictions bind in Medicaid, but conversations with health plan officials suggest at least one channel—that enrollees must request an "out-of-network referral" before plans approve payment for out-of-network services.[15] The data bears this out

[14]New York, like many other states, has been expanding its reliance on Medicaid managed care—both increasing the number of enrollees in managed care and the number of services managed care plans cover—in an effort to achieve a stated "triple aim" that encompasses quality improvement, improved population health, and reduced cost (Roby et al., 2018).

[15]Because Medicaid reimbursement rates are lower than Medicare and private payers, providers have

in my setting—discrete choice models of physician and hospital demand estimate large "hassle costs" for going out-of-network, suggesting that network restrictions bind in Medicaid and are an important area of study.

B. *Auto-Assignment to Plans in New York State's Medicaid Program*

This section discusses the auto-assignment policy in New York State that I leverage to identify the causal effects of network breadth. New York State encourages Medicaid enrollees to actively choose their health plans. However, enrollees that fail to choose a plan within a designated choice period are automatically enrolled in a plan, a policy known as "auto assignment."[16] enrollees that qualify for auto-assignment each month are randomly allocated across eligible plans with equal probability. However, plans may not be eligible to receive auto-assignees in all periods and counties,[17] hence randomization probabilities differ by county $\times$ year $\times$ month (the unit of randomization). There are two exceptions to the auto assignment policies described above. First, New York takes into account the plan in which family members are enrolled. If family members are enrolled in a managed care plan at the time of auto assignment, the enrollee defaults to the family member's plan. For enrollees without family members in managed care, there is a second exception to auto assignment. This exception applies to enrollees that were enrolled in a managed care plan in the year prior to assignment. These enrollees are reassigned to their previous plan. I remove these enrollees from my primary sample or auto assignees, as discussed below.

little incentive to accept Medicaid patients unless they are contracted to do so.In addition to lower payment rates, the administrative costs of dealing with Medicaid managed care are high relative to other payers (Gottlieb, Shapiro and Dunn, 2018), and out-of-network providers face additional barriers to receiving payment from plans when enrollees are not authorized to go out-of-network (see Appendix A). More broadly, access is a perennial concern in Medicaid, where provider participation rates have historically been low leading to concerns about how far enrollees have to travel and how long they have to wait to receive needed care (Sommers and Kronick, 2016), though recent work suggests access to providers in Medicaid has been increasing over time (Ndumele et al., 2018; Wallace, Lollo and Ndumele, 2020).

[16] The auto-assignment rate is low in New York State (5-10%), possibly due to state activities aimed at reducing auto assignment (e.g. investments in facilitated enrollers who help enrollees select plans).

[17] The authority for auto-assignment is provided by N.Y. Soc. Servs. L. § 364-j(4)(f)(i). Plans qualify for performance-based assignment based on a yearly composite measure that incorporates state-specific quality measures, Consumer Assessment of Healthcare Providers and Systems (CAHPS) responses, Prevention Quality Indicators (PQIs), and regulatory compliance measures.

C. Primary sample

I construct an estimation sample using data on adult Medicaid enrollees in New York City that were randomly-assigned to their managed care plans during the period April 2008 to July 2012. I restrict this sample in five ways. First, I drop enrollees that live outside the five boroughs of New York City. Approximately one-third (35%) of Medicaid enrollees reside outside New York City. The focus on New York City allows me to identify the impact of provider networks while controlling flexibly for geography with zip code fixed effects. Second, I restrict the sample to enrollees aged 18 to 65. I exclude individuals aged 65 and older because they become eligible for Medicare (often referred to as "dual eligibles") and are excluded from Medicaid managed care. I remove enrollees below age 18 because I study the impact of provider networks on plan choice and it is difficult to interpret plan choice behavior for children. Third, I exclude Medicaid enrollees who were enrolled in managed care plans within the year prior to assignment or whose family members are enrolled in a Medicaid managed care plan at the time of auto assignment since they are preferentially placed into their prior plan or a family member's plan, respectively. Fourth, I remove individuals who qualify for Medicaid because they receive Supplemental Security income (SSI) due to differences in their auto-assignment policy. Fifth, to keep the sample balanced during the primary follow-up period, I restrict my primary sample to enrollees that are in Medicaid for at least three months prior, and six months after, auto-assignment.[18] These sample restrictions leave me with 58,172 enrollees in five counties and ten plans (see Appendix Table 1).

D. Outcome Measures

To estimate the impact of limited provider networks, I use detailed administrative data obtained from the New York State Department of Health to construct enrollee-

[18]There is considerable churn of enrollees on and off the Medicaid program. Six months after assignment there is a large reduction in the share of enrollees in Medicaid (Panel A of Appendix Figure 1) due to loss of eligibility for Medicaid. Pursuant to a Federal Medicaid rule from the 1997 Balanced Budget Act, New York guarantees eligibility for Medicaid enrollees in Medicaid managed care (MMC) for 6 months following the start of MMC enrollment, hence there's no evidence of differential attrition during that period (Panel B of Appendix Figure 1).

month level outcomes. To permit a comparison of my results to those of prior studies examining the effects of demand-side and supply-side incentives in health care (e.g., Manning et al., 1987; Gruber and McKnight, 2016; Brot-Goldberg et al., 2017), I focus on the outcomes relied on in those studies. Hence, my primary outcomes relate to health care utilization and spending, including whether enrollees use a set of potentially high-value and low-value services. However, it is arguably as important to understand how provider networks affect consumers' experience utility in their plans. Hence, the final outcome I study is enrollee utility as measured by whether or not an enrollee stays in their randomly assigned plan (i.e., "willingness-to-stay"). While this differs from a traditional willingness-to-pay measure, in a world of consumer choice frictions (e.g., Handel and Kolstad, 2015*b*; Handel, Kolstad and Spinnewijn, 2019), an advantage of this measure is that it reflects the utility an enrollee experiences in their assigned plan (Israel, 2005). See Appendix A for detailed descriptions of the outcome measures.

III. Network Measure Construction

The setting for my study is New York City ("NYC"), where Medicaid managed care plans differed markedly in the share of hospitals and physicians they cover (see Figure 1). While many of the networks shared overlapping providers, none were identical.[19] This is because managed care plans narrowed their networks by restricting access to different sets of providers (i.e., narrow networks were not all narrower in the same way). The one provider-owned plan in the market, for example, operated the narrowest hospital network (and overall network), but its vector of covered hospitals was negatively-correlated with the networks of other plans that also had narrower hospital networks, indicating that the plans excluded different sets of hospitals (Appendix Figures 3-4).

[19]To examine the degree of overlap in physician and hospital networks I constructed a vector for each plan indicating which physicians and hospitals were in-network in 2010 (a year in the middle of my study period). A comparison of these vectors across plans revealed generally positive, but modest, correlations in the set of physicians that were in-network. There was greater variability in the correlations across hospital networks, with some plans that tended to contract with very different sets of hospitals ($\rho < 0$) and others with very similar hospital networks ($\rho > 0.8$) (Appendix Figures 2 and 3). For hospitals—where I had access to facility-level characteristics—participation in Medicaid managed care networks was associated with non-profit status (relative to being a public hospital), being located in a lower income zip code, and having a lower overall hospital rating (Appendix Table 2).

To measure the breadth of a plan's provider network in each zip code, one must take into account the number of in-network providers for the plan, where each provider is located, and what the distribution of patient preferences over those (and other) providers looks like. To do this, I build on the pioneering work of Town and Vistnes (2001), Capps, Dranove and Satterthwaite (2003), Ho (2006, 2009), and Ericson and Starc (2015). A key insight in these papers is that enrollee preferences over providers lead to patient flows which, when observed in the data, allow researchers to recover enrollee preferences and use them to model provider demand or measure network utility. Intuitively, plans that cover the providers that enrollees in a particular zip code value highly (measured via revealed preference) will have broader effective networks (in that zip code) than plans that cover fewer providers or providers valued less highly. Differences in how enrollees value providers may arise, for example, due to the distance an enrollee would have to travel to see a provider or unobserved enrollee preferences over providers. Once I recover these preferences, I simulate provider choices in an "unconstrained" counterfactual in which there are no provider network restrictions and calculate the share of simulated visits covered by each plan's network in each year for each zip code in NYC.[20]

A. Models of Physician and Hospital Demand

This section describes how I use micro-data on patient flows to physicians and hospitals to estimate models of physician and hospital demand. The goal of this exercise is to recover parameters from demand models that can be used to simulate provider choices under the (unobserved) counterfactual in which all Medicaid managed care providers are in-network for each plan.

I begin with a discussion of the hospital demand model, which is more straightforward than the physician demand model due to the tractable size of the hospital choice set in NYC. Following Ho (2006), I estimate a model of hospital choice using micro-data on inpatient hospitalizations for Medicaid managed care enrollees.[21] The main covariates

[20]Because this measure uses administrative claims it does not account for any differences between plan networks in provider wait times or appointment availability, which are not observed in the data.

[21]While I restrict the data to hospitalizations for enrollees that reside in New York City, I include

are distance and hospital characteristics, which are allowed to vary with patient observables in some specifications to capture preference heterogeneity.[22] Unlike past work, I do not include coinsurance (or hospital prices) as covariates since Medicaid enrollees in New York are not charged cost sharing for hospital admissions.[23] Following Shepard (2016), I include out-of-network hospitals in the choice set. This is appropriate in New York Medicaid where enrollees can seek a referral to see an out-of-network provider and approximately nine percent of hospital admissions are out-of-network. To capture any potential hassle costs associated with seeking care from out-of-network providers, I include an out-of-network hassle cost term in the hospital choice model.

Consider consumer i in plan j who is hospitalized with diagnosis d. Following the discussion above, we can write their utility from visiting hospital h at time t as:

$$u_{i,j,d,t,h} = \underbrace{\delta(\text{Dist}_{i,h} \times Z_{i,d,t})}_{\text{Distance}} + \underbrace{\lambda(X_h \times Z_{i,d,t}) + \xi_h}_{\text{Hospital Characteristics}} + \underbrace{\psi_j \cdot 1\{h \notin N_{j,t}\}}_{\text{Out-of-Network Cost}} + \epsilon_{i,d,t,j,h} \tag{1}$$

where $\text{Dist}_{i,h}$ is patient travel distance and distance-squared (in minutes), X_h are observed hospital characteristics, ξ_h are unobserved hospital characteristics (represented by hospital fixed effects), $1\{h \notin N_{j,t}\}$ is an indicator that hospital h is out-of-network for plan j in time t (with ψ_j the hassle cost), and $\epsilon_{i,d,t,j,h}$ is an i.i.d. Type 1 extreme value error. Patient observables $Z_{i,d,t}$ are interacted with distance and hospital characteristics in some specifications to allow for preference heterogeneity.

The model is estimated using maximum likelihood.[24] Appendix Table 3 shows the

their hospitalizations at facilities outside of New York City to avoid introducing differences in how network breadth is measured at city boundaries.

[22]Unfortunately the data do not permit us to observe whether the source of admission was the emergency department or a scheduled inpatient stay. As a result, our data include both emergent and nonemergent inpatient hospitalizations. This may not be a big limitation as Ho (2006) estimates a similar model and finds that removing emergency admissions had little effect on the coefficients.

[23]Ho and Pakes (2014) find that hospital prices impact referral patterns if doctors are paid by capitation. Although the data on this in New York Medicaid is imperfect, capitation claims account for a small share of paid physician claims.

[24]The identification of this model is based on the assumption that the covariates (e.g., distance to hospitals) are exogenous. One concern raised by Shepard (2016) is that if enrollees select their plans on the basis of unobservable hospital preferences, estimates of the network hassle costs will be biased upwards. I examine this by re-estimating the model in Equation 1 using a sample of enrollees randomly assigned to their plans. The results of this analysis are available upon request. When I restrict my analysis to randomly assigned enrollees, the estimated hassle costs are lower, suggesting

results for two models based on the specification. Columns (1) and (2) report results for comparison from a basic model that includes distance (and distance squared), an indicator to capture out-of-network hassle costs, and hospital fixed effects. Columns (3) and (4) report the results of a more complex model, which includes distance (and distance squared) interacted with diagnoses and enrollee observables, plan-specific out-of-network hassle costs and hospital fixed effects. In both cases, the model fit is good.[25] The full model does not improve the fit much relative to the simple model so I use the coefficients from the basic model to construct network measures. Similar to prior work, I find a disutility associated with travel distance. The estimates imply that an extra 10 minutes travel time reduces a hospital's share by 84% (on average) and that being out-of-network reduces a hospital's share by 72% (on average).[26]

I now turn to modeling the demand for physicians, using micro-data on physician office visits for Medicaid managed care enrollees to estimate a model of how patients choose doctors. The method and specification for estimating physician demand differ from the hospital model in two ways. First, due to the large physician choice set (n=22,983), and the small volume of Medicaid claims for many physicians, it is not possible to estimate a fixed effect for each physician (as was done for each hospital). Instead, I estimate separate physician demand models in each of the forty-two neighborhoods (defined by zip) in NYC, including fixed effects for the largest practices. The large choice set also makes it infeasible to estimate the conditional logit model using the full set of alternatives for each observation. Instead, I follow McFadden (1978) and for each choice instance select four random alternatives (in addition to the chosen physician) and proceed with the estimation using these subsets (see Appendix B for additional details).

Appendix Table 4 shows the results of estimating the physician demand model. Similar to the hospital setting, there is disutility associated with travel distance and out-of-

that the original estimates were biased by selection on unobservable hospital preferences. However, the model still estimates a negative and precisely estimated hassle cost that reduces out-of-network hospital's shares by 38% on average.

[25]McFadden's R^2 for the basic and full models are 0.401 and 0.408, respectively. Both indicate good fit (McFadden, 1977).

[26]A reduction that is similar in magnitude to what was reported for commercially-insured, low-income residents in Massachusetts (Shepard, 2016).

network hassle costs. The estimates imply that an extra 10 minutes travel time reduces a physician's share by 48% (on average) and being out-of-network reduces a physician's share by 92% (on average), a reduction that is significantly larger than in the hospital setting. The table also presents the largest coefficients for physician characteristics x service interactions; the remaining coefficients are mostly significantly positive.[27]

B. Simulating "Unconstrained" Demand and Measuring Network Breadth

I use the coefficients from the physician and hospital choice models to simulate physician and hospital visits under a counterfactual in which all providers are in-network for each Medicaid managed care plan.[28] I then construct my "covered share of simulated visits" measure at the plan-by-year-by-zip level as the fraction of simulated visits in a given zip that are covered by each managed care network. Table 1 provides summary statistics for this measure. Plans covered an average of 62.5% of simulated hospital visits and 56.8% of simulated physician visits. When measuring overall network breadth I take a weighted average of physician and hospital network breadth.[29] This measure has a mean of 59.7% and a standard deviation of 15.1%. A one standard deviation increase in the share of simulated visits covered by a plan was equivalent to covering an additional 4,000 physicians and 7.5 hospitals in NYC (see Appendix Figure 4).

To conduct robustness tests, I construct two additional measures of network breadth: a "covered share of visits" measure as in Ericson and Starc (2015) and an expected network utility measure as in Ho (2009).[30]

[27]As in the hospital analysis, I test for bias in the hassle cost by re-estimating a citywide version of the model in Equation B1 using a sample of enrollees that made active choices and a sample of enrollees randomly assigned to their plans. These results are available upon request. Unlike the hospital setting, the hassle cost is similar for the active choice and random assignment samples. The likeliest explanation for this is that there is less selection on unobservable preferences in the physician setting.

[28]Due to the imprecise estimates of several of the physician fixed effects, I use empirical bayes methods to shrink the estimated physician fixed effects prior to simulating counterfactual physician shares (Chandra et al., 2016).

[29]I weight by physician and hospital service quantity with shares of 52.5% and 47.5%, respectively.

[30]See Appendix B for additional details. Consistent with prior work, the different methods of measuring network breadth are highly-correlated (Appendix Figure 5).

IV. Research Design

A. Econometric Model

The main empirical goal of this paper is to estimate the causal effect of provider network breadth on outcomes (e.g., health care spending) at the enrollee level. I posit a data generating process for health care spending where log spending (Y_{izjct}) for enrollee i living in zip code z enrolled in plan j is determined by a location component (ω_z), plan component (γ_j), provider network component (Γ_{zj}), enrollee-level fixed effect (ζ_i), time-varying observables (X_{it}), and a mean zero shock (ϵ_{ijzct}):

$$Y_{izjct} = \omega_z + \gamma_j + \beta\Gamma_{zj} + \zeta_i + \delta X_{it} + \epsilon_{ijzct} \tag{2}$$

To recover the effect of a broader network on health care spending, I estimate Equation 2 at the enrollee-level, combining γ_j, ζ_i, and ϵ_{ijzct} into a compound error term η_{ijzct}:

$$Y_{izjct} = \alpha + \omega_z + \beta\Gamma_{zj} + \phi_{ct} + \delta X_{it} + \eta_{ijzct} \tag{3}$$

where β is the coefficient of interest, α is a constant, ω_z are zip code fixed effects, ϕ_{ct} are county $c \times$ month t of assignment fixed effects (the unit of randomization), and X_{it} is a vector of individual controls. I discuss the unique identification challenges posed by the plan component (γ_j) and how I address them below.

The primary threat to internal validity is unobservable selection on networks (e.g., see Shepard, 2016; Kreider et al., 2020). One possibility is that enrollees who expect to consume a lot of health care choose plans with broader networks, biasing cross-sectional comparisons. Another possibility is that enrollees switch to broader network plans when their health care needs change, biasing within-person comparisons. To address the endogeneity of enrollees sorting into plans on the basis of network breadth, I restrict to auto-assigned enrollees and instrument for an enrollee's network breadth (Γ_{zj}) with the breadth of their randomly-assigned plan. Intuitively, my identification comes from comparing the outcomes of enrollees that reside in the same zip code but are randomly assigned to different plans and, hence, exposed to different provider network breadths.[31] Because I rely on random assignment, the vector of individual controls adds precision but isn't needed for identification. I present results with and without individual controls for transparency.

Since auto assignment is not binding, I estimate the causal impact of network breadth with two-stage least squares using enrollee's assigned provider network breadth to instrument for their actual provider network breadth. The first-stage estimating equation takes the form:

$$\Gamma_{ijzct} = \alpha + \omega_z + \pi\widetilde{\Gamma_{zj}} + \phi_{ct} + \nu X_{it} + \mu_{ijzct} \tag{4}$$

where $\widetilde{\Gamma_{zj}}$ is the breadth of the provider network of plan j that enrollee i residing in zip code z was assigned to. An enrollee's actual network breadth, Γ_{zj}, may differ from the enrollee's assigned network breadth, $\widetilde{\Gamma_{zj}}$, if they switch plans. Here, the parameter of interest is π, which captures the first-stage effect of the instrument on actual network breadth. To account for any serial correlation within randomization cohorts, I cluster standard errors at the county $\times$ month of assignment level.

To test the strength of the first stage, Figure 2 plots actual network breadth against assigned network breadth. The binned scatterplot is constructed by first regressing assigned network breadth and actual network breadth on the baseline set of control variables (i.e. county $\times$ month of assignment and zip code), calculating residuals, and grouping the residualized network breadth measures into plan-by-zip bins. The mean for each variable is added back in to ease interpretation. The solid line and corresponding coefficient are based on an OLS regression of the residualized outcome on the residual network breadth measure, with standard errors clustered at the county $\times$ month of assignment level (Chetty, Friedman and Rockoff, 2014). Assigned network breadth is highly predictive of actual network breadth due to inertia in plan assignment.[32] As

[31]For example, although on average, the ten plans covered 54% of the visits for enrollees residing in zip 10471, the narrowest plan covered only 27% of such visits and the broadest plan 75%, nearly a three-fold difference.

[32]Appendix Table 5 contains additional details on the first stage regression for the primary specifica-

a byproduct of the strength of the instrument, the estimated local average treatment effects (LATEs) will be similar to average treatment effects (ATEs) in this setting.

The second stage estimating equation uses the predicted provider network breadth ($\widehat{\Gamma_{zj}}$) from the first-stage regression to estimate the effect of provider network breadth on the outcomes:

$$Y_{izjct} = \alpha + \omega_z + \beta\widehat{\Gamma_{zj}} + \phi_{ct} + \delta X_{it} + \eta_{ijzct} \tag{5}$$

This instrumental variables strategy results in an estimate of the effect of provider network breadth, β, that uses only variation in Γ_{zj} due to auto-assignment. Intuitively, β is an estimate of the consumer response to provider network breadth, a supply-side analog to to the consumer response to demand-side cost sharing (e.g., Manning et al., 1987), where here we are varying provider network breadth (i.e., the share of simulated visits covered) rather than a cost sharing parameter (e.g., the coinsurance rate).

An additional empirical challenge in this setting is that the outcomes of enrollees in plans with narrower (or broader) networks may be impacted by other unmeasured plan characteristics, such as plans' use of managed care tools (e.g., prior authorization). While I do not directly observe the set of managed care tools used by each plan, Geruso, Layton and Wallace (2020) find large *causal* differences in health care spending and utilization across the plans in this market. If these "plan effects" (the γ_j from Equation 2) are correlated with enrollees' assigned network breadth, $\widetilde{\Gamma_{zj}}$, they may bias estimates of β in Equation 5. The ex ante direction of this bias is not clear. While it is tempting to think that narrower network plans would employ more draconian utilization management tools, that need not be the case.

In my setting, the narrowest network plan (hereinafter referred to as the "provider-owned plan") is a wholly-owned subsidiary of a public hospital system. Appendix Figure 6 demonstrates that the enrollees assigned to this plan generated a lot of health care

tion as well as alternative specifications that include enrollee- and plan-level controls. The Cragg-Donald F-statistic on the first stage is over 3,000,000, consistent with assigned network breadth being a very strong instrument for actual network breadth.

spending and had high levels of experience utility, despite its narrow network, potentially biasing naive comparisons between network breadth and enrollee outcomes at the plan level.[33] One possible explanation is that a provider-owned plan has weaker incentives to control cost, and hence may take a less aggressive approach to rationing services.[34] The challenge this poses for identification is highlighted by the plot lines, which are based on estimating a reduced form version of Equation 5 on the underlying, enrollee-level data. The solid line and corresponding coefficients omit the provider-owned plan. When the provider-owned plan is included, the resulting relationships between network breadth and the outcomes (the dashed line) are severely attenuated. While this plan is a clear outlier, its existence highlights the challenge of inferring the effects of network breadth from comparisons of *narrow network* vs. *broad network* plans.

To address this empirical challenge, I remove enrollees in the "provider-owned plan" when estimating Equation 5, and use the variation in provider network breadth that arises due to quasi-random auto-assignment among the other nine, more homogenous plans.[35] This approach estimates the effect of network breadth by comparing the outcomes of enrollees that reside in the same zip code but are randomly assigned different provider network breadths (by virtue of their plan assignments).[36] To account for potential bias due to the correlation between network breadth and plan effects, I also estimate specifications with plan controls and the full sample of enrollees (see Appendix Section C for details), using the variation in network breadth that remains after including plan and zip code fixed effects (see Panel D in Appendix Figure 7). My primary results are qualitatively similar whether or not I include plan controls. Because I remove the provider-owned plan, and include plan controls in some specifications, my results do not generalize to the effects of enrolling in a *narrow network* versus *broad network* plan, as

[33]The binned scatterplot is constructed by first regressing assigned network breadth and our outcomes on the baseline set of control variables, calculating residuals, and grouping the residualized network breadth measures into plan bins.

[34]A more in-depth discussion of the causal differences in health care use and spending across plans is beyond the scope of this paper and covered in Geruso, Layton and Wallace (2020).

[35]My results are qualitatively similar if, instead of dropping enrollees in the provider-owned plan, I include all enrollees and add a fixed effect for enrollees assigned to the provider-owned plan.

[36]Panel B in Appendix Figure 7 plots a histogram of the variation in network breadth that remains after residualizing on zip code.

plans may differ along several dimensions (e.g., a comparison of a broad network PPO to an integrated system like Kaiser Permanente). Rather, my estimates measure the effects of a *narrower* provider network, holding other plan characteristics fixed.

B. *Specification Checks*

Two additional conditions must hold to interpret the two-stage least squares estimate of β from Equation 5 as the local average treatment effect of network breadth for enrollees that comply with assignment. First, assignment may only impact enrollee outcomes through its impact on network breadth. Second, the impact of network breadth must be monotonic across enrollees.

The first assumption is that the assigned network breadth only impacts enrollee outcomes through its impact on actual network breadth. If assigned network breadth is correlated with unobservables that affect the outcomes I study, my estimates of β will be biased. Table 2 contains several balance tests. Columns 2, 3, and 4 present the estimates from multivariate regressions of enrollees' baseline characteristics on their assigned network breadth. For each of these regressions, a test of the joint significance of all the coefficients is presented at the bottom of the column. None of the three sets of coefficients are jointly significant. Column 5 presents results from an additional balance test. Here, I estimate bivariate OLS regressions of baseline enrollee characteristics on assigned network breadth. None of the coefficients are statistically significant at the 5 percent level; the auto-assignment sample appears *well-balanced.* Across numerous tests, I find little relationship between enrollees' baseline characteristics and their assigned network breadth. By comparison, analogous estimates for an equal-sized group of enrollees that made active plan choices suggest that sample is *highly imbalanced* (See Appendix Table 6). The imbalance among enrollees that made active plan choices indicates that the balance in the auto-assignee sample is not an artifact of statistical noise or based on the set of baseline characteristics assessed. Moreover, it suggests that nonrandom selection could be an important confounder in this setting, underscoring the importance of relying on quasi-random auto-assignment to estimate causal effects.

The second assumption is monotonicity.[37] Though I cannot test this assumption, Angrist, Imbens and Rubin (1996) show that the bias introduced by violations of monotonocity is a decreasing function of the strength of the first stage. Hence, violations of monotonicity would introduce minimal bias in this setting where the first stage is strong.[38]

C. External Validity

My primary causal estimates in this paper are based on Medicaid enrollees auto-assigned to Medicaid managed care plans in New York. Since auto-assignees are not a random sample of Medicaid enrollees (i.e., auto-assignment only occurs if a plan choice is not made during a designated period), it is important to consider the *external validity* of my results, first to New York State's Medicaid population and then more broadly.

I begin with a comparison of the baseline characteristics and health care utilization patterns of the auto-assignee sample to those of New York Medicaid enrollees that made active choices.[39] Since the auto-assignment rate is low in New York (5-10%), one concern related to external validity is that the small share of enrollees that fail to make a plan choice may be less engaged in the health care system than active choosers. Appendix Table 7 presents baseline characteristics (based on the 3 months prior to a plan assignment or plan choice) separately for auto assignees and active choosers.[40] Health care spending and utilization in the two groups were similar at baseline, allaying concerns that the sample of auto assigned enrollees is less engaged in the health care system.

[37]For monotonicity to hold, assignment to a broader (narrower) network must not result in enrollees experiencing a narrower (broader) network. One concern, for example, would be that enrollees assigned to narrow networks might be "shocked" to search and end up switching to a broader network than enrollees assigned less narrow networks (Ho, Hogan and Scott Morton, 2017).

[38]Appendix Figures 8 and 9 plot actual network breadth against assigned network breadth separately by gender, age, and predicted spending. The first stage is strong across each of the subgroups.

[39]Appendix Section A describes how I construct this alternative sample of active choosers.

[40]I compare the characteristics of the two groups at baseline (prior to assignment or plan choice), because at that time all enrollees are enrolled in fee-for-service Medicaid. After assignment, active choosers sort into different plans than auto assignees (who are randomly assigned) and comparisons may become contaminated by plan and provider network effects.

Because the groups differ along some observable dimensions,[41] I conduct two additional analyses to assess the external validity of my results. First, I estimate OLS regressions of the effects of network breadth on health care use and spending in the active choice sample, relying on rich baseline characteristics (including an enrollee's health care use and spending prior to their plan choice) to control for potential, enrollee-level confounders. The OLS results based on active choosers are broadly consistent with the IV estimates that rely on the auto assignees though, as expected, more sensitive to controls. Second, I re-estimate my primary specification after reweighting the auto-assignees to match the broader Medicaid population on demographics and baseline characteristics. I present the results of these analyses in Section VIII.

V. Results

This section presents my main results on the impact of broader provider networks on health care spending, quality, and consumer satisfaction. For context, Table 1 presents summary statistics on the primary sample of enrollees randomly assigned to plans.

A. Health Care Use and Spending

Panel A of Figure 3 reveals a precisely estimated relationship between assignment to a broader provider network and post-assignment health care spending. A one standard deviation increase in assigned network breadth (akin to a network covering an additional 13.1% of the visits in a zip code) increases monthly spending by 6.7 log points. Panel B of Figure 3 presents this estimate by month relative to auto assignment. Although these estimates are noisier due to splitting the data, consistent with the binned scatterplots, they demonstrate that a positive relationship between assignment to a broader network and health care spending emerges after assignment.[42]

Table 3 presents my primary instrumental variable estimates. There is an economically and statistically significant impact of network breadth on post-assignment health care

[41]While the groups were similar in age, the auto assignees were more likely to be male and Black.

[42]The results are similar if, instead of a two-way fixed effects model as in Panel B of Figure 3, I subset the data and estimate reduced form versions of Equation 5 separately by month relative to assignment (Appendix Figure 10).

use and spending. Pooling outcomes across the six months after assignment, Column 1 reveals that a one standard deviation increase in network breadth was associated with an increase in health care spending of 7.1 log points (std. err. = 1.5), or roughly 7%, and a 1.1 percentage points (std. err. = 0.2) increase in the probability of using any care in a month, or roughly 3.5%. In Column 2, I demonstrate that these estimates are robust to the inclusion of enrollee-level controls for age, gender, race, and baseline outcomes. Column 3 illustrates that the results are qualitatively similar when I control for plan of assignment, and estimate the relationship between network breadth and spending using within-plan variation.[43]

Panels B and C of Table 3 present results separately by enrollee age, gender, and predicted health care spending. Each row is a different subsample of the data, with Column 1 reporting the share of the auto-assignee sample each subsample represents. In nearly every subsample, assignment to a broader provider network was associated with increased health care spending, revealing that the overall spending estimate in Panel A is not driven by a particular subset of enrollees.[44]

The above findings also mask heterogeneity in the impact of provider network breadth by component of care. Panel A of Appendix Table 11 presents IV estimates of the impact of a one standard deviation increase in network breadth on log spending by components of care. I find no effect of provider network breadth on the use of inpatient care. However, a one standard deviation increase in network breadth was associated with increases of 5.7 log points (std. err. = 0.9) for outpatient care spending, 2.7 log points (std. err. = 1.0) for pharmacy spending, and 2.5 log points (std. err. = 1.3) for other types of care.

[43]Appendix Tables 8-10 show that these estimates are robust to using alternative measures of network breadth, specifications, or alternative transformations to address skewness in the spending data. Panel A of Appendix Figure 11 demonstrates that the results are also robust to dropping any one of the plans and, if controls for plan of assignment are present, to including enrollees from the provider-owned plan.

[44]Despite this, there is some evidence of heterogeneity in treatment effects for different types of enrollees. The effects of a broader network on health care use and spending in Column 2 were larger for females and younger enrollees, whereas there is little evidence of heterogeneity by health status. I formally test the equality of coefficients across sub-samples. I find suggestive evidence that the effects of provider network breadth on spending are larger for females (p=0.067) and older enrollees (p=0.062). I cannot reject the null of no difference between the coefficients for each quartile of predicted spending (p=0.85).

Spending Differences Largely Driven by Utilization, Not Prices

One potential explanation for my primary spending result is differences in negotiated health care prices. There is a growing literature on price variation between health insurers and health care providers. Cooper et al. (2018) find that variation in health care provider prices in the commercial sector explains a majority of the variation in health care spending, but it is unclear whether these findings generalize to Medicaid managed care. More recent work by Craig, Ericson and Starc (2018) also finds significant price variation between health insurers. However, studies of the Medicare Advantage market find less evidence of price variation, with insurer prices closely tracking publicly-administered, Medicare fee-for-service prices (Berenson et al., 2015; Pelech and Hayford, 2019). To assess the role of prices in my setting, I conduct two additional analyses. First, I construct a measure of health care spending that varies only based on quantity (Gruber and McKnight, 2016). Second, I directly examine the relationship between network breadth and the prices paid to providers. Both analyses reveal that spending differences are driven by utilization, not prices (see Appendix Tables 11 and 12).[45]

Short vs. Long-Run Spending Effects

Because my primary estimates measure the short run effects of provider network breadth (i.e., in the 6 months after assignment), it is possible that the reductions in health care use and spending associated with narrower networks reflect short-term disruptions in care that are mitigated over time as enrollees learn to navigate a new provider network or establish relationships with new providers. To examine long run effects, Ap-

[45]Panel B of Appendix Table 11 examines the impact of provider network breadth on quantity. Following Brot-Goldberg et al. (2017), I reprice health care services at their sample means, removing any price variation at the insurer-, provider-, or insurer-by-provider-level. A one standard deviation increase in network breadth was associated with an increase of 6.2 log points in price-standardized spending, a modestly smaller effect than our estimated effects on overall spending. A one standard deviation increase in provider network breadth was also associated with increases in two additional quantity measures: a simple count of services ("quantity of services") and the likelihood that enrollees used any care in a month ("any spending in a month"). Appendix Table 12 directly examined the effect of assignment to a broader network on prices, finding modest, if any, effects. However, It is still possible that the savings achieved by narrower networks are mediated by bargaining between insurers and providers. For example, plans may bargain with providers on quantity—rather than price—threatening to exclude providers unless they manage care efficiently.

pendix Figure 13 presents event study plots with the follow-up period extended to 12, 18, and 24 months post-assignment.[46] For each follow-up period, I present event study results for two samples: a balanced sample of enrollees that remain in Medicaid through the extended post-assignment period, and an imbalanced sample of enrollees in Medicaid for at least the 6 months post-assignment.[47] The figures show that my results do not merely reflect short-term disruptions in care, but rather that the relationship between network breadth and spending persists for up to 24 months post assignment.

B. Health Care Quality: Use of Potentially High-Value and Low-Value Services

Having established that narrower provider networks reduce health care spending primarily via lower quantity demanded, I now examine whether quantity reductions are concentrated in services where overuse is a concern, or if they are broad-based, spanning high-value and low-value care (e.g., Brot-Goldberg et al., 2017; Curto et al., 2019).

Panels A and B of Figure 4 reveal precisely estimated relationships between assignment to a broader provider network and enrollees' use of potentially high-value and low-value care. A one standard deviation increase in assigned network breadth increased the probability of enrollees utilizing potentially high-value care in a month by 0.65 percentage points (4%), and potentially low-value care in a month by 0.63 percentage points (4%), nearly identical estimates. Panels C and D present these estimates by month relative to auto assignment. A positive relationship between assignment to a broader network and the use high-value and low-value care emerges after assignment.[48]

[46]Though there is a lot of churn in my primary sample (motivating the focus on short run effects), there does not appear to be evidence of differential attrition out of the Medicaid program (see Appendix Figure 1). While there is no evidence of differential attrition, the strength of my instrument (i.e., assigned network breadth) weakens over time as more enrollees switch out of their assigned plans. Hence, I present IV event study estimates for comparison as the reduced form estimates fade (relative to the IV estimates) over time as the first stage weakens.

[47]The enrollees I include in the extended event studies differ slightly from my primary specification, even for the imbalanced sample, due to imposing additional restrictions on enrollees (e.g., they had to remain in New York City for at least 12 months following assignment (rather than 6 as in my primary sample). For additional details on the construction of these samples, see Appendix Section C.

[48]The results are similar if I subset the data and estimate reduced form versions of Equation 5 separately by month rather than estimate a two-way fixed effects model (Appendix Figure 10). Extended event study estimates suggest that effects persist beyond the first 6 months post assignment. However, the effects of network breadth on potentially high-value and low-value care begin to fade after 18 and 12 months, respectively (Appendix Figures 14 and 15).

Table 4 presents my primary instrumental variable estimates of the effect of network breadth by service.[49] Assignment to a broader provider network was associated with using more high-value professional services (e.g., primary care), recommended preventive services (e.g., HbA1C testing), potentially high-value drugs (e.g., anti-depressants), but also potentially low-value imaging and laboratory services.[50] Appendix Table 15 presents results separately by enrollee sex, age, predicted health care spending, and health condition. The use of high-value services by enrollees with diabetes or cardiovascular disease was particularly sensitive to network breadth, with suggestive evidence that assignment to a broader network may have also reduced the use of the emergency department (for diabetics). This pattern didn't hold for enrollees with behavioral health conditions—for these enrollees the use of low-value services (i.e., lab and imaging) was more sensitive to network breadth than the use of high-value services, suggesting that as networks broaden the mix of services tilts towards less needed care.

The service-level results suggest that restrictive provider networks, like demand-side cost sharing, are a blunt tool for reducing health care spending; narrower provider networks reduce health care spending, but they lead enrollees to use fewer of both needed and unneeded services (Manning et al., 1987; Brot-Goldberg et al., 2017).

C. Consumer Satisfaction

Prior work on the effects of demand-side and supply-side health care incentives has focused primarily on outcomes related to health care utilization and health outcomes, but it is arguably as important to understand how these incentives affect consumers' experienced utility in their plans. To do this, I examine whether randomly assigned enrollees remain in their assigned plans, and assume that enrollees' experienced utility in a health plan is revealed by subsequent plan switches (Israel, 2005).[51]

[49] Instrumental variable estimates for the specific services that comprise potentially high-value and low-value care are presented in Appendix Tables 13 and 14.

[50] Appendix Table 8 demonstrates that the estimates are robust to the use of alternative measures of network breadth. Panels B and C of Appendix Figure 11 demonstrate the estimates are not sensitive to which plans are in the sample.

[51] Medicaid managed care enrollees have three months from the time of assignment to switch plans before they face a nine-month "lock-in" period during which they may only switch for "good cause."

Panel A of Figure 5 depicts a strong (and positive) relationship between the breadth of the assigned network and the likelihood enrollees remain in their assigned plan (i.e., "willingness-to-stay"). A one standard deviation increase in assigned network breadth reduces the probability a enrollee switches out of their assigned plan by 1.11 percentage points (std. err. = 0.09), a 19 percent reduction in the probability of a plan switch (relative to the sample mean of 5.8 percentage points). Panel B examines heterogeneity by enrollee health status (as proxied for by predicted health care spending). The point estimates increase monotonically with quartiles of predicted spending, consistent with sicker enrollees placing higher value on provider network breadth (Appendix Table 16).

Reduced form estimates of the impact of network breadth on satisfaction over time are presented in Panel D of Appendix Figure 10. There is no baseline measure of willingness-to-stay since enrollees are in Medicaid fee-for-service prior to assignment. Hence, instead of a two-way fixed effects event study model, I present the results of estimating reduced form versions of Equation 5 separately by month. Enrollees assigned to narrower networks become more likely (relative to those assigned to broader networks) to switch plans over time, presumably as they learn about the limitations of their network. Two-thirds of the switchers move to a plan with a broader network in their zip code (Appendix Figure 16).

VI. Heterogeneity by Network Characteristics

What characteristics of narrower networks are most important for explaining the observed reductions in health care spending, high-value and low-value service use, and consumer satisfaction? This section examines the differential effects of restricting access to physicians and restricting access to hospitals, and also explores to what extent these effects are mediated by whether a network covers an enrollee's usual source of care.

Even during the lock-in period I observe plan switching, suggesting a low threshold for "good cause."

A. *Disentangling the Effects of Physician and Hospital Networks*

Up to this point, my measure of network breadth has been a weighted average of physician and hospital network breadth, which are positively correlated. To separate the effects of physician and hospital network breadth, I now include separate terms for physician and hospital network breadth in my estimating equation:

$$Y_{izjct} = \alpha + \omega_z + \gamma_j + \beta_1 \widehat{Phys_{zj}} + \beta_2 \widehat{Hosp_{zj}} + \phi_{ct} + \delta X_{it} + \eta_{ijzct} \tag{6}$$

where $\widehat{Phys_{zj}}$ and $\widehat{Hosp_{zj}}$ are enrollees' predicted physician and hospital network breadth from first stage regressions that use assigned physician and hospital network breadth as instruments.[52] Unlike my primary estimates, the estimated effects of physician and hospital network breadth are sensitive to the inclusion of plan controls (see Appendix Figure 19). Hence, my preferred specification includes controls for plan of assignment. I present results without plan controls in Appendix Table 17.

Columns 2 and 3 of Table 5 present my preferred estimates of the impact of physician and hospital network breadth on health care spending, quality, and satisfaction.[53] For reference, Column 1 replicates my primary results on the effects of overall network breadth. I find that physician network breadth has a large estimated impact on post-assignment health care use and spending. A one standard deviation increase in physician network breadth was associated with increased health care spending of 5.9 log points (std. err. = 2.1), or roughly 6%, and increased utilization of both potentially high-value and low-value services. By comparison, having a broader hospital network had a smaller impact on health care spending, and no detectable effect on the use of high-value or low-value care. Panel D presents evidence that broader physician and hospital networks both increase satisfaction as measured by willingness-to-stay (i.e., the probability an enrollee remains in his or her assigned plan).

[52]One concern, given the correlation of physician and hospital network breadth (Appendix Figure 17), is whether sufficient variation remains in this model to estimate the effect of the different network breadth measures. Appendix Figure 18 shows that substantial variation in network breadth remains.

[53]Appendix Table 18 verifies balance by demonstrating that enrollee's baseline characteristics do not predict the breadth of their assigned physician or hospital network.

B. The Role of Network Coverage for Enrollees' Usual Sources of Care

A second dimension of heterogeneity I consider is whether a provider network covers an enrollee's usual source of care. There is a growing body of evidence suggesting that disrupting a patient's usual source of care may impact their health care use (e.g. Barnett et al., 2017; Kwok, 2019) and that consumers will switch plans to retain access to their providers (Shepard, 2016; Higuera, Carlin and Dowd, 2018).[54] Since the breadth of an enrollee's assigned network is likely to be correlated with whether or not that plan covers their usual source of care, this is an important channel to consider.

To examine this channel, I construct a sample of 25,256 randomly-assigned enrollees with sufficient baseline spending to infer their usual source of care. For each of these enrollees, I use their Medicaid fee-for-service claims in the period prior to assignment. I define their usual source of care as the hospital or physician with whom they accrued the most spending prior to assignment.[55] Thirty percent of these enrollees were assigned to plans that did not include their usual sources of care, leading to a disruption.[56]

I then adapt Equation 6 to include an indicator variable for whether an enrollee's usual source of care is in-network:

$$(7)\ \ Y_{ipzjct} = \alpha + \omega_z + \gamma_j + \beta_1 \widehat{Phys}_{zj} + \beta_2 \widehat{Hosp}_{zj} + \beta_3 \widehat{USOC}_{pj} + \phi_{ct} + \lambda_p + \delta X_{it} + \eta_{ipjzct}$$

where p indexes an individual's pre-assignment provider, $\widehat{USOC}_{pj}$ is an indicator that

[54]The welfare effects of these disruptions are unclear. If narrow networks exclude less efficient providers, disruptions will tend to shift patients away from low-quality or high-cost providers, but recent evidence suggests the gains from steering patients to more efficient providers may be modest relative to the costs of disrupting their care (Kwok, 2019).

[55]For enrollees with more than three months of fee-for-service claims prior to assignment, I used data from up to twelve months prior to assignment but placed a higher (double) weight on claims in the three months prior to assignment. Appendix Table 19 demonstrates that this subset of auto-assignees has higher monthly spending, and are more likely to use care in a given month, than the broader auto-assignee sample.

[56]The hospital network breadth of an enrollee's assigned plan was a strong predictor of whether the enrollee's usual source of care was covered. A one standard deviation increase in assigned hospital network breadth is associated with an 18 percentage point (27%) increase in the probability that an enrollee's usual source of care is in-network for their assigned plan. There is no association between assigned physician network breadth and whether an enrollee's usual source of care is in-network for their assigned plan because this is a sample of Medicaid enrollees that tended to seek care in the hospital setting prior to assignment.

an individual's usual source of care (i.e., their pre-assignment provider) is covered by their plan's network, instrumented for by whether their assigned plan covered their usual source of care, and λ_p is a vector of fixed effects for the pre-assignment providers. This approach estimates the impact of a consumer having their usual source of care in-network, β_3, by comparing enrollees with the same pre-assignment provider whose plan assignments led to differences in whether that provider was covered.

Columns 4-6 of Table 5 present the results of estimating Equation 7 on my primary outcomes.[57] A one standard deviation increase in physician network breadth was associated with a 6.6 log point (std. err. = 3.8) increase in health care spending. Enrollees assigned to plans covering their pre-assignment providers (i.e., those not disrupted) also had higher health care use and spending, a result that differs from prior work which finds that disruptions (rather than continuity) increase spending (Kwok, 2019).

Panels B and C of Table 5 present evidence that assignment to a broader physician network increases the use of both potentially high-value and low-value care. By comparison, the breadth of enrollees' assigned hospital network was not associated with the use of any of the services designated as potentially high-value or low-value care. Whether an enrollee's usual source of care was included in their network, on the other hand, had large estimated impacts on their use of potentially high-value and low-value services. Enrollees whose networks covered their usual source of care were 1.8 percentage points (std. err. = 0.5), or 11%, more likely to use high-value professional services, 1.2 percentage points (std. err. = 0.4), or 9%, more likely to use high-value prescription drugs, and 1.8 percentage points (std. err. = 0.5), or 8%, more likely to use any potentially high-value care in a month. There is also suggestive evidence that enrollees whose networks covered their usual source of care were 0.3 percentage points (std. err. = 0.1), or 10%, more likely to comply with recommended HEDIS preventive care measures. Enrollees whose usual sources of care were in-network were also more likely to use low-value care

[57]Appendix Table 20 verifies balance by demonstrating that an enrollee's baseline characteristics do not predict whether their assigned plan will cover their usual source of care. As before, the physician and hospital network breadth measures are z-score normalized. The "key provider in assigned" column, however, is a dummy variable (i.e., 0 or 1), so estimates should be interpreted as the effect of an enrollee having their usual source of care in-network.

services, driven by imaging and lab utilization.

Panel D of Table 5 presents the reduced form results of estimating Equation 7 on enrollees experience utility (i.e., satisfaction) in their health plans. Columns 4 and 5 demonstrate that adjusting for whether an enrollee's usual source of care is in-network mediates the effect of physician and hospital network breadth on satisfaction. Neither point estimate is significant in this specification, though the estimate for physician network breadth was only partly attenuated but is now less precisely estimated. However, it is clear from the results that enrollees' access to their pre-assignment providers had the largest effect on their satisfaction. Enrollees who were assigned to networks that did not include their usual source of care were 5.2 percentage points (std. err. = 0.4), or 90%, more likely to switch plans and 81% of the switchers moved to plans covering their usual source of care.[58]

The primary network heterogeneity estimates I present in this section measure the short run effects of network breadth (within 6 months of assignment). Hence, it is possible that the effects reflect short-term disruptions in care—particularly when examining enrollees' access to a usual source of care—that are mitigated over time as enrollees adjust to a new provider network, find a new provider, or switch to a plan that better meets their needs. Appendix Tables 22–24 report reduced form estimates over different post-assignment time periods, presenting evidence that the main effects presented in Table 5 persist for up to two years post-assignment.

VII. Counterfactual Auto-Assignment Policies

The evidence in Sections V-VI suggests that narrower networks are a blunt tool for reducing health care use and spending. While they constrain cost, narrower networks reduce the use of both needed services (e.g., primary care) and unneeded services (e.g.,

[58]Because the standard errors are large, and multicollinearity may be a problem, Appendix Table 21 presents the results of estimating a version of Equation 7 that restricts to enrollees whose assigned plans contain their usual source of care, instead of including the $\widehat{USOC}_{pj}$ term. The results are qualitatively similar to my primary findings, with broader physician networks associated with an 8.3 log point (std. err. = 4.7) increase in spending, an 0.85 percentage point (std. err. = 0.45) increase in the likelihood of using of high-value medical services in a month, and a 1.00 percentage point (std. err. = 0.56) increase in the likelihood of using low-value medical services in a month.

imaging), and lower enrollees' satisfaction with their plans. Given the mixed effects of provider networks on health care quality, I focus on the tradeoff between health care spending and consumer satisfaction in this section.

Could counterfactual auto-assignment policies reduce health care spending without harming consumer satisfaction? To assess this, I use my estimates to reassign enrollees across plans—using information available to the State at the time of assignment—to minimize cost for any given level of predicted satisfaction while maintaining plan shares.[59] This entails solving linear optimization problems of the form:

$$\begin{aligned} \min_{P_ij} \quad & \sum_{i=1}^{N}\sum_{j=1}^{J} P_{ij} \cdot \widehat{Cost}_{ij} \\ \text{subject to} \quad & \forall_i \sum_{j=1}^{J} P_{ij} = 1, \\ & \forall_j \sum_{i=1}^{N} P_{ij} = share^j, \\ & \sum_{i=1}^{N}\sum_{j=1}^{J} P_{ij} \cdot \widehat{Satisfaction}_{ij} \geq \underline{\text{s}} \end{aligned}$$

where i indexes individuals, j indexes plans, P_{ij} is an indicator that individual i is assigned to plan j, $\widehat{Cost}_{ij}$ is predicted log cost for individual i if assigned to plan j, $share^j$ is the share of Medicaid enrollees assigned to plan j under the state's random assignment policy, $\widehat{Satisfaction}_{ij}$ is the predicted satisfaction for individual i if assigned to plan j and $\underline{\text{s}}$ sets a floor for satisfaction (the satisfaction constraint).[60] I use reduced form (rather than IV) estimates from Equation 7 when predicting cost and satisfaction because they take into account the attenuation of the effects of a counterfactual allocation policy that would be expected due to subsequent plan switches. For each enrollee,

[59]The restriction that plan shares be maintained is for consistency with a New York law that requires enrollees are assigned in equal share to qualifying plans. See N.Y. Soc. Servs. L. § 364-j(4)(f). I relax this constraint in subsequent analyses.

[60]Since individuals can only be assigned to exactly one plan, integer programming methods (e.g. Hungarian Method) would provide a more exact solution. For tractability, however, linear programming techniques were used to solved this on a random sample of 4,000 auto-assigned enrollees. Fortunately, the results generally assigned enrollees to only one plan, allaying this concern.

the prediction (e.g., for cost) for each plan accounts for differences in the breadth of the physician and hospital networks between a given alternative plan and their default assigned plan (based on their zip code of residence and the networks of both plans), as well as whether the alternative (and default) plan included their usual source of care.

Figure 6 plots predicted spending and satisfaction for a set of counterfactual auto-assignment policies. Each point depicts the mean differences in predicted cost (on the x-axis) and satisfaction (on the y-axis) for a counterfactual set of plan assignments. Relative to the default policy, there exist alternative policies that both increase predicted satisfaction *and* reduce predicted spending (the upper-left quadrant). In Appendix Figure 20, I demonstrate that the potential cost savings or increases in satisfaction are even greater if the restriction that plan shares be maintained is relaxed. However, the optima often entail very small (or no) allocations to some plans and large allocations to others, with the potential to reshape the market in unintended ways (Shepard, 2016).

Appendix Table 25 contains my primary estimates of the impact of counterfactual auto-assignment policies on health care spending and satisfaction (with plan shares fixed) as well as details on how enrollees' network characteristics vary in each counterfactual. In the six months post-assignment, the state can reduce the likelihood enrollees switch plans by approximately 10% without increasing expenditures, or reduce spending by 2.2 log points ($\approx$ 2%) without lowering satisfaction.[61] Intuitively, this is achieved by matching enrollees with narrower networks (to reduce spending) that nevertheless include their usual source of care (to increase satisfaction). This intuition is borne out in columns 3-5, where, for most counterfactuals, enrollees are being shifted into plans with narrower physician networks containing their usual sources of care.

[61]The baseline plan switching rate for this sample is 8%. In the fourth counterfactual presented here there is an increase of 0.71 percentage points in satisfaction with no increase in cost, equating to a reduction in plan switching of 0.71/8, or 8.875%.

VIII. Discussion

A. Comparison to Other Estimates

I compare my causal estimates with observational estimates based on New York State Medicaid enrollees that made active plan choices.[62] I estimate OLS regressions of the effects of network breadth on health care use, spending, and quality for this sample of active choosers (Appendix Tables 26 and 27). As expected, OLS estimates are sensitive to the inclusion of enrollee-level controls. When I include enrollee-level controls, however, the OLS and IV estimates are broadly consistent. Because the auto-assignee and active chooser groups differ on observables, I also estimate my primary specification after reweighting the auto-assignees to match the broader Medicaid population on demographics and predicted spending. The reweighting did not affect my qualitative results (Appendix Tables 28–29). Appendix Figure 21 plots IV, reweighted IV, and OLS estimates to facilitate an easy comparison between models.

I also compare my estimates to those from the literature. Prior studies on the effects of provider networks have compared outcomes between narrow and broad network plans (Gruber and McKnight, 2016; Atwood and LoSasso, 2016); however, narrow network plans may differ from broad network plans on non-network dimensions (e.g., prior authorization, vertical integration), making it difficult to ascribe differences in outcomes to differences in network breadth. Since my estimates are based on a direct measurement of network breadth—that varies both between and within plans—I can control for non-network dimensions (i.e., supply-side tools) that may vary across plans. Despite the differences in approach, I find that narrower networks reduce health care spending and utilization, a result consistent with the literature (Gruber and McKnight, 2016; Atwood and LoSasso, 2016). However, the reductions in spending in my setting come primarily via reductions in quantity, rather than lower prices paid to providers. In addition, my results suggest that narrower networks—like high deductibles (Brot-Goldberg et al., 2017)—are a blunt instrument for reducing health care spending—a finding that

[62] I provide additional details on the construction of this sample in Appendix Section A and compare the baseline characteristics of the auto-assignee and active chooser samples in Appendix Table 7.

diverges from prior evidence on the effects of narrow networks.

B. Policy Implications

My results have implications for how state and federal network adequacy requirements—rules for how many and what types of providers plans must contract with—should be designed. Ho and Lee (2019) argue that these kind of regulations can weaken plan bargaining leverage with providers, leading to higher negotiated prices and, ultimately, lower consumer welfare. However, network adequacy requirements may also affect health care spending, quality, and consumer satisfaction if these outcomes are responsive to network breadth (holding other plan characteristics fixed). My work suggests this is an important channel; broader networks increase health care spending, but they also raise consumer utility and increase the use of needed (and unneeded) care. I also present evidence that the plan choices of sicker enrollees are more responsive to network breadth, suggesting that—absent regulation—plans may construct networks that are narrower than socially optimal in an effort to select healthier patients.

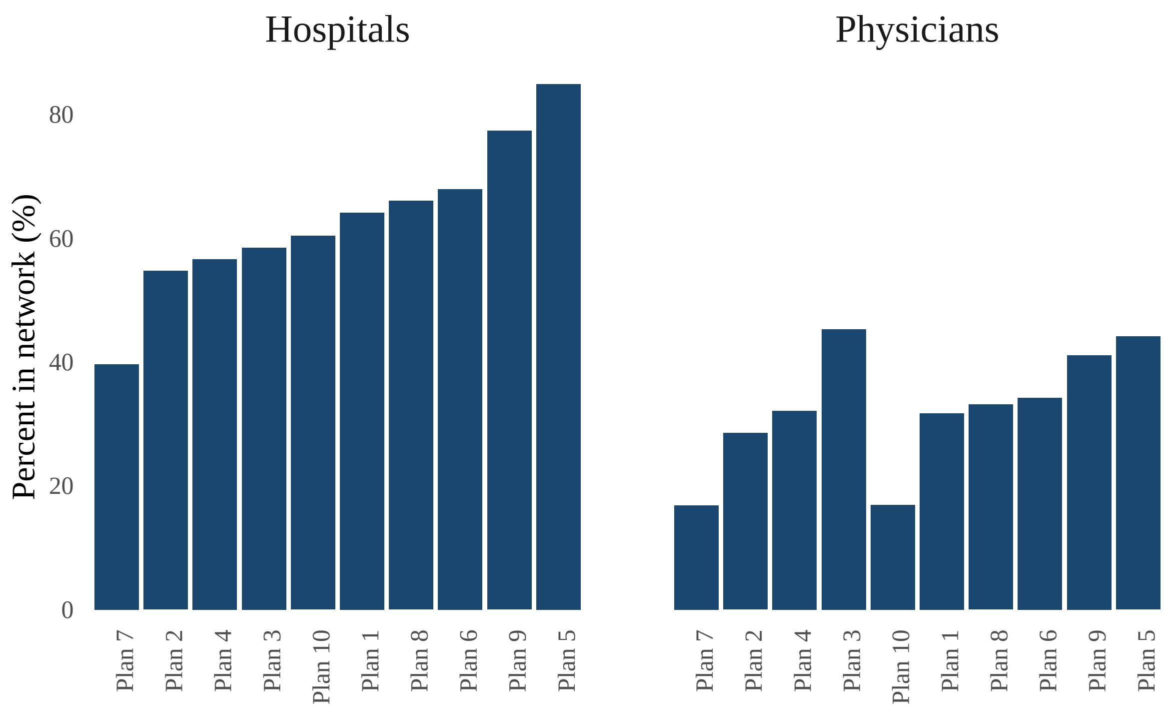
Lastly, my results suggest that "smart defaults" may be an effective way to achieve programmatic goals in Medicaid. Smart default policies in retirement investment decisions, such as defaulting people into retirement savings levels that take advantage of the full match rate of an employer, are now common (Madrian and Shea, 2001; Carroll et al., 2009) and the idea of smart defaults is gaining traction in health insurance markets (see for e.g., Handel and Kolstad, 2015*a*). In New York, I identify alternative approaches to allocating enrollees across Medicaid plans that both *increase* satisfaction and *reduce* spending. These simulations have clear policy implications for New York but offer a broader lesson to the more than 30 states that operate mandatory Medicaid managed care programs: auto-assignment can be a powerful tool to achieve program goals without unnecessarily restricting enrollee choice among plans.

The generalizability of these results should be considered in light of the characteristics of my study population and the features of the New York Medicaid program. While auto assignment is an important source of random variation, enrollees who actively choose their plans may differ from those who don't in unobservable ways. It is also important to note that the results are based on enrollees in New York City—the most densely populated metropolitan area in the country—and, additionally, that there is substantial cross-state variation in Medicaid eligibility rules, procurement policies, and health systems. Lastly, my results measure the partial equilibrium effects of changes in provider network breadth. The effects of large policy changes may differ as stakeholders respond to changes in the market (Finkelstein, 2007).

C. Conclusion

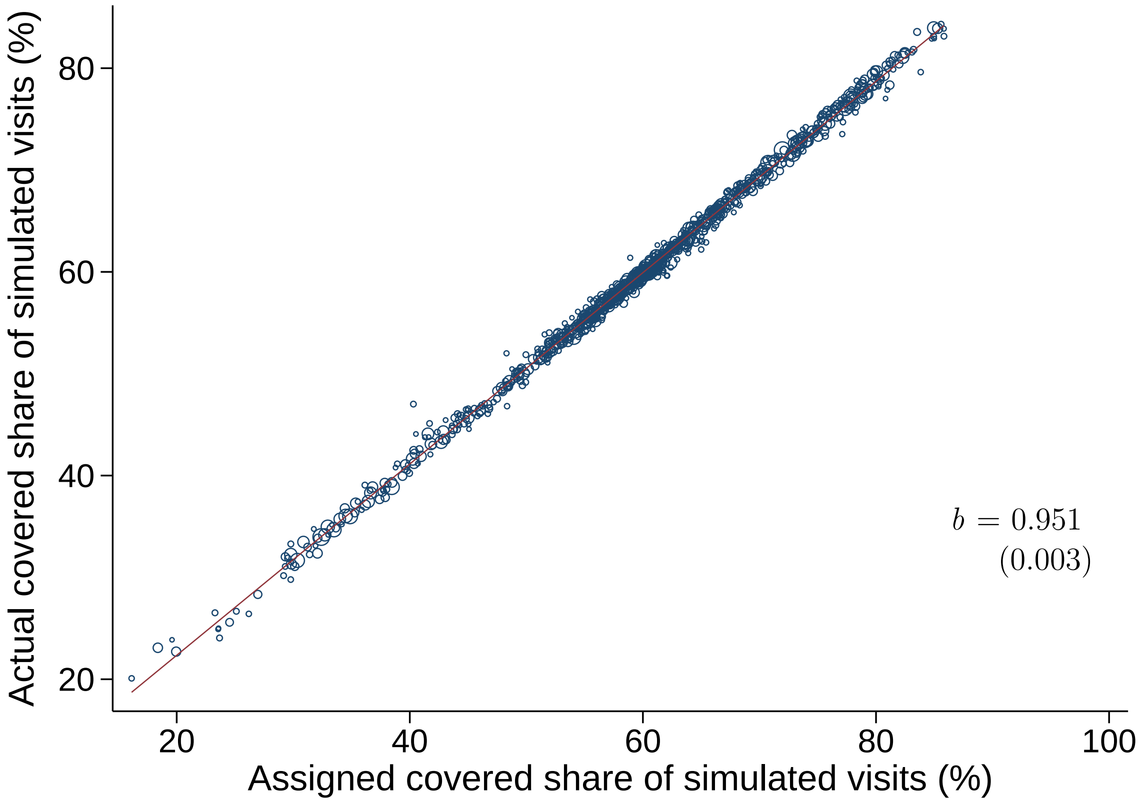
Leveraging the random assignment of over 50,000 Medicaid enrollees in New York, I present causal evidence that narrow networks are a blunt instrument for reducing health care spending. While narrower networks constrain spending, they do so by generating hassle costs that lead to broad-based reductions in quantity demanded, including roughly equal reductions in high-value and low-value services. They also reduce enrollees' experienced utility in their plans. The results suggest that—at least in this context—there are real tradeoffs to narrower networks. Reducing health care spending by narrowing networks comes at a utility cost—both because narrower networks disrupt ongoing provider relationships and because they reduce spending by reducing quantity (rather than price). Based on my causal estimates, I identify counterfactual auto-assignment policies that reduce health care spending without harming consumer satisfaction. These findings inform state auto-assignment policy and the broader debate about the role of government in regulating network adequacy.

Figure 1. : Percent of New York City Providers In Network by Plan



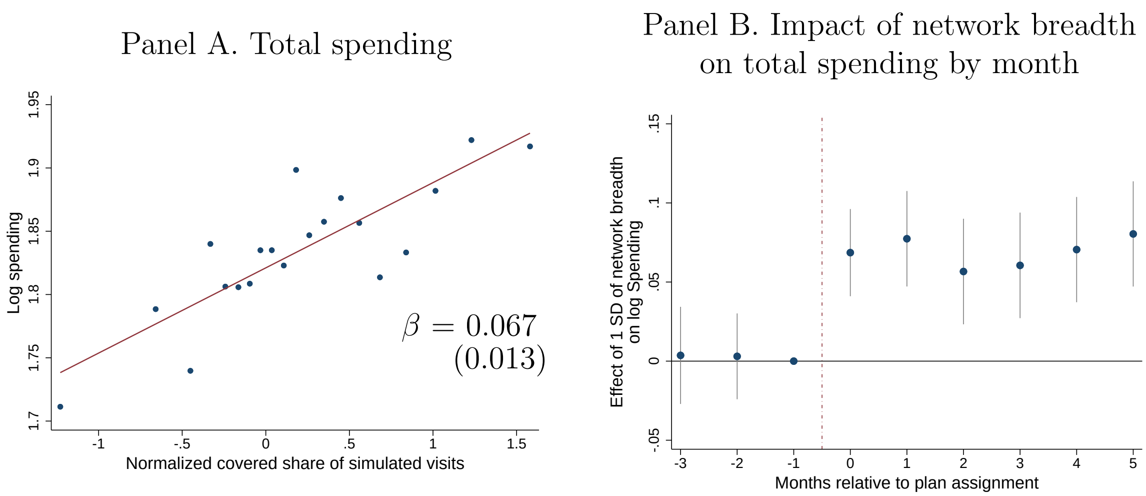
Notes: These figures plot the fraction of physicians and hospitals in the five boroughs of New York City covered by each of the Medicaid managed care plans in my sample. The data on physician and hospital network participation with each plan is drawn from the Provider Network Data System (PNDS) plan directories for 2010. Hospital and physician counts for each New York City county are drawn from the Area Health Resources File for 2010. The plan names are masked at the request of the New York State Department of Health.

Figure 2. : First Stage: Assigned and Actual Provider Network Breadth



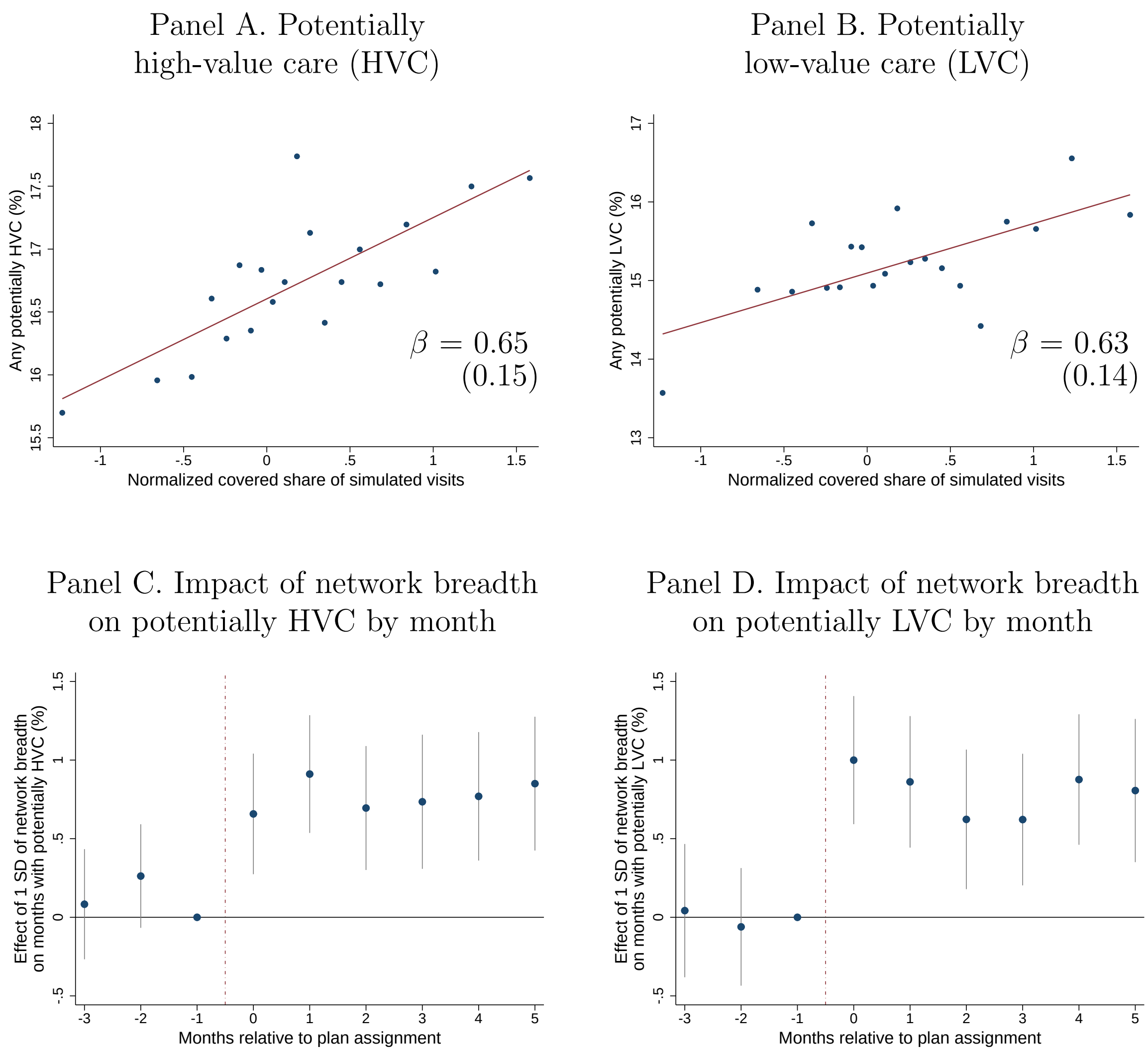
Notes: This figure plots enrollees' actual simulated visited shares against their assigned simulated visit shares (my primary measure of network breadth). Results are based on my primary sample (see Section II for details on primary sample construction). The binned scatterplot is constructed by first regressing assigned network breadth and actual network breadth on the set of control variables (i.e. age, gender, race, tenure, baseline outcomes, county × month of assignment), calculating residuals, and grouping the residualized network breadth measure into bins at the plan × zip level. The mean is added back in to ease interpretation. The solid line and corresponding coefficient are based on an OLS regression of the residualized outcome on the residual network breadth measure, with standard errors clustered at the county × month of assignment level (Chetty, Friedman and Rockoff, 2014).

Figure 3. : Assigned Network Breadth and Health Care Spending



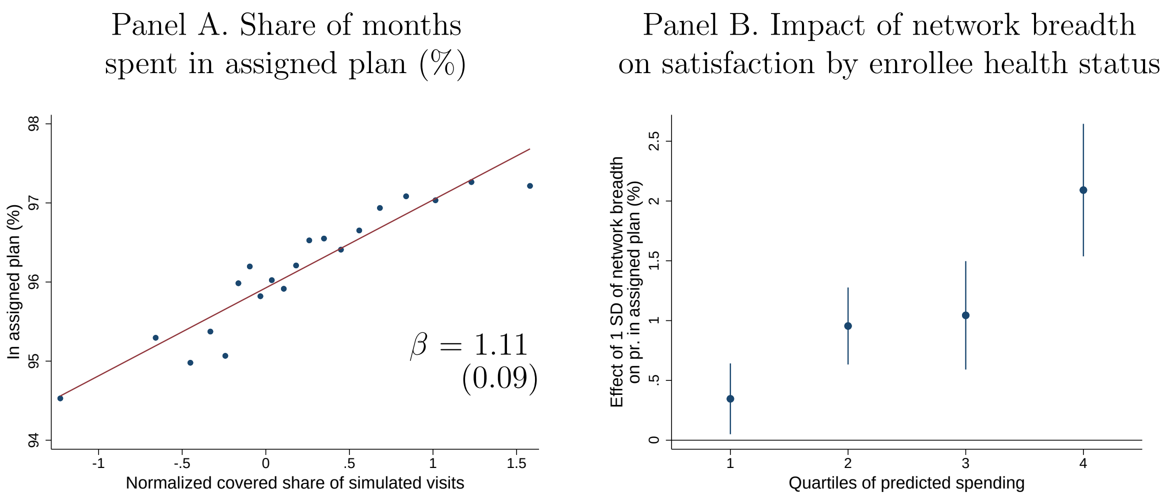
Notes: Results are based on my primary sample (see Section II for details on primary sample construction). Panel A plots a residualized binned scatterplot of the reduced form impact of the normalized covered share of simulated visits (network breadth) on total health care spending. The binned scatterplot is constructed by first regressing assigned network breadth and health care spending on the set of control variables (i.e. age, gender, race, tenure, baseline outcomes, county $\times$ month of assignment), calculating residuals, and grouping the residualized network breadth measure into 20 equal-sized bins. The mean is added back in to ease interpretation. The solid line and corresponding coefficient are based on an OLS regression of the residualized outcome on the residual network breadth measure, with standard errors clustered at the county $\times$ month of assignment level (Chetty, Friedman and Rockoff, 2014). Panel B presents an event study model. Point estimates are displayed along with 95% confidence intervals as described in Appendix C. The baseline (omitted) period is 1 month prior to auto assignment, indicated by the dashed vertical red line in the plot. The y-axis presents the effect of a one standard deviation increase in assigned network breadth on log spending.

Figure 4. : Assigned Network Breadth and the Use of Potentially High-Value and Low-Value Care



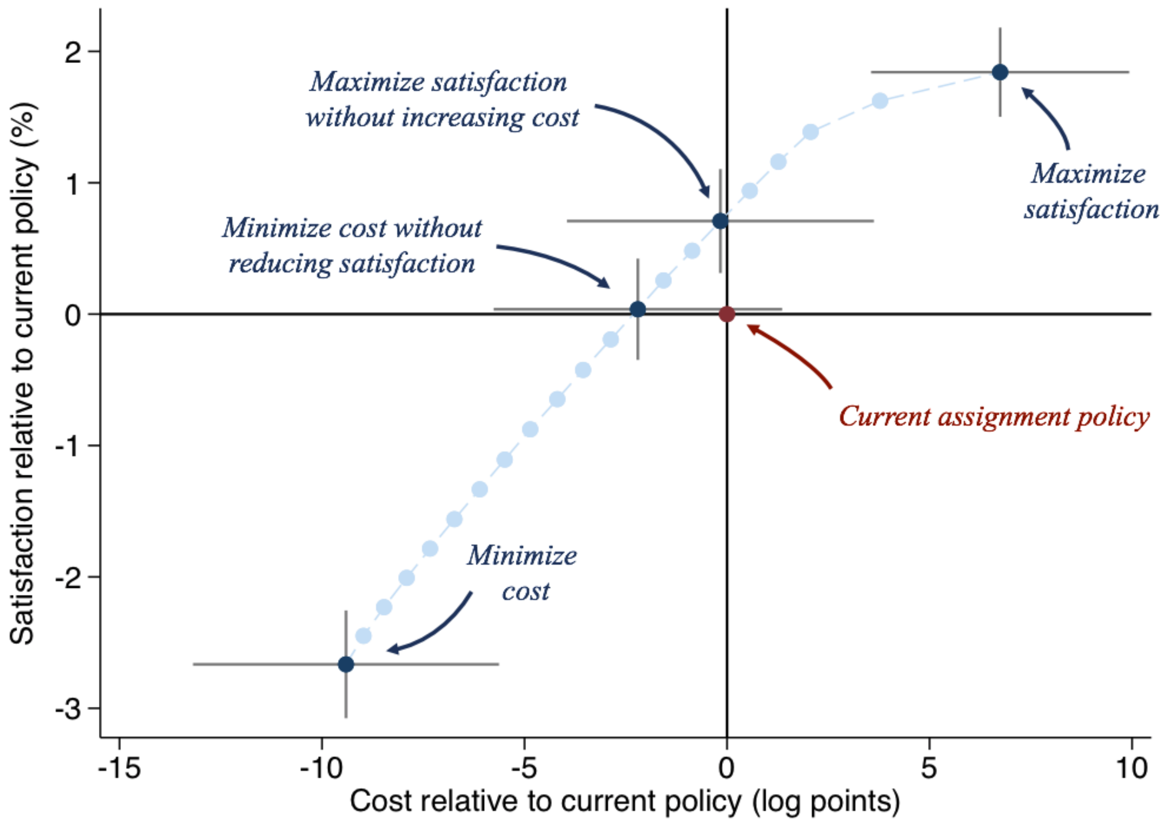
Notes: Results are based on my primary sample (see Section II for details on primary sample construction). Panels A and B plot residualized binned scatterplots of the reduced form impact of the normalized covered share of simulated visits (network breadth) on potentialy high-value and low-value care. The binned scatterplots are constructed by first regressing assigned network breadth and the outcomes on the set of control variables (i.e. age, gender, race, tenure, baseline outcomes, county × month of assignment), calculating residuals, and grouping the residualized network breadth measures into 20 equal-sized bins. The mean of each outcome is added back in to ease interpretation. The solid lines and corresponding coefficients are based on OLS regressions of the residualized outcome on the residual network breadth measure, with standard errors clustered at the county × month of assignment level (Chetty, Friedman and Rockoff, 2014). Panels C and D present event study models. Point estimates are displayed along with 95% confidence intervals as described in Appendix C. The baseline (omitted) period is 1 month prior to auto assignment, indicated by the dashed vertical red line in the plot. The y-axis presents the effect of a one standard deviation increase in assigned network breadth on the outcomes.

Figure 5. : Assigned Network Breadth and Consumer Satisfaction



Notes: Results are based on my primary sample (see Section II for details on primary sample construction). Panel A plots a residualized binned scatterplot of the reduced form impact of the normalized covered share of simulated visits (network breadth) on the likelihood an enrollee remains in their assigned plan (i.e., experience utility). The binned scatterplot is constructed by first regressing assigned network breadth and willingness-to-stay on the set of control variables (i.e. age, gender, race, tenure, baseline outcomes, county $\times$ month of assignment), calculating residuals, and grouping the residualized network breadth measure into 20 equal-sized bins. The mean is added back in to ease interpretation. The solid line and corresponding coefficient are based on an OLS regression of the residualized outcome on the residual network breadth measure, with standard errors clustered at the county $\times$ month of assignment level (Chetty, Friedman and Rockoff, 2014). Panel B presents evidence of treatment effect heterogeneity in the effects of assigned network breadth on willingness-to-stay by predicted enrollee spending (a proxy for health status). Point estimates are displayed along with 95% confidence intervals based on separate regressions for each quartile of enrollee predicted spending. The point estimates increase monotonically, indicating that sicker enrollees place higher value on provider network breadth. Appendix Table 16 contains the results in tabular form.

Figure 6. : Impact of Alternative Assignment Policies on Health Care Spending and Consumer Satisfaction



Notes: This figures plots the mean difference between predicted spending and satisfaction for 21 counterfactual auto-assignment policies relative to the state's current (random) assignment policy. The x-axis measures the mean difference between log spending for each counterfactual policy and the current auto-assignment policy. The y-axis measures the mean difference between enrollee satisfaction (i.e., willingness-to-stay) for each counterfactual and the current auto-assignment policy. The counterfactual auto-assignment policies are identified by randomly sampling 4,000 enrollees from the 28,010 unique enrollees in our sample with data available on their primary provider and using linear programming to reassign enrollees across the plans in a way that minimizes cost subject to a satisfaction constraint and the requirement that plan shares remain unchanged. The mean differences in cost and satisfaction for each counterfactual are predicted after estimating reduced form versions of Equation 7. Section VII provides additional details.

Table 1—: Summary Statistics

	Mean	Std. Dev.	Observations
	(1)	(2)	(3)
Demographics			
Age (years)	35.655	12.281	349,044
Male (%)	59.4	49.1	349,044
Black (%)	51.8	50.0	349,044
Assigned network breadth, %			
Covered share of simulated visits	59.618	15.134	349,044
Covered share of simulated physician visits	56.933	14.179	349,044
Covered share of simulated hospital visits	62.466	20.577	349,044
Network covers primary provider	67.365	46.888	157,536
Healthcare spending, $			
Total spending	397.365	2,351.631	349,044
Outpatient spending	69.962	335.901	349,044
Inpatient spending	168.972	2,107.052	349,044
Pharmacy spending	60.632	355.266	349,044
Other spending	97.799	458.270	349,044
Healthcare use, %			
Any spending	32.778	46.941	349,044
Any outpatient spending	17.701	38.168	349,044
Any inpatient spending	1.877	13.573	349,044
Any pharmacy spending	18.909	39.158	349,044
Any other spending	20.919	40.673	349,044
Potentially high-value care, %			
Any high-value medical care	11.729	32.176	349,044
Any recommended preventive care	2.137	14.462	349,044
Any high-value prescription drugs	9.343	29.103	349,044
Any potentially high-value care	17.697	38.165	349,044
Potentially low-value care, %			
Any lab or imaging	13.462	34.132	349,044
Any emergency department use	5.362	22.526	349,044
Any avoidable hospitalizations	0.407	6.365	349,044
Any designated low-value care	0.207	4.550	349,044
Any potentially low-value care	16.007	36.668	349,044
Satisfaction, %			
In assigned plan	94.211	23.354	349,044

Notes: This table reports summary statistics. Summary statistics are based on my primary sample (see Section II for details on primary sample construction), including enrollees in the provider-owned plan. Observations are at the enrollee-month level and restricted to the six months post-assignment (my primary sample). Details on the construction of the measures of network breadth are included in Section III. Additional details on the broad service categories or specific services identified as potentially high-value or low-value care are included in Appendix A.

Table 2—: Balance Test

	Mean (SD)	Multivariate OLS			Bivariate OLS
	(1)	(2)	(3)	(4)	(5)
Age	35.447	0.0020	0.0092	0.0084	0.0034
	(12.280)	(0.0038)	(0.0144)	(0.0071)	(0.0049)
Male	0.594	0.0062	−0.0001	−0.0033	0.0081
	(0.491)	(0.0042)	(0.0114)	(0.0056)	(0.0053)
Black	0.518	−0.0016	−0.0029	0.0003	−0.0017
	(0.500)	(0.0038)	(0.0061)	(0.0031)	(0.0044)
Outpatient spending	94.832	0.0039	0.0114	0.0065	0.0012
	(249.646)	(0.0040)	(0.0155)	(0.0080)	(0.0047)
Inpatient spending	249.180	0.0007	0.0010	−0.0006	0.0001
	(1920.226)	(0.0034)	(0.0041)	(0.0021)	(0.0044)
Pharmacy spending	65.266	0.0024	0.0087	0.0075	0.0030
	(359.975)	(0.0033)	(0.0102)	(0.0053)	(0.0045)
Other spending	126.654	−0.0016	0.0042	0.0045	−0.0023
	(429.309)	(0.0040)	(0.0092)	(0.0051)	(0.0053)
Any high-value medical care (%)	17.193	−0.0008	0.0077	0.0120	−0.0035
	(37.732)	(0.0039)	(0.0168)	(0.0084)	(0.0047)
Any recommended preventive care (%)	4.550	0.0002	0.0028	0.0047	−0.0020
	(20.840)	(0.0033)	(0.0075)	(0.0039)	(0.0044)
Any high-value prescription drugs (%)	14.837	0.0000	0.0266	0.0273	−0.0008
	(35.546)	(0.0039)	(0.0426)	(0.0209)	(0.0047)
Any lab or imaging (%)	23.021	−0.0049	−0.0001	−0.0006	−0.0062
	(42.097)	(0.0038)	(0.0186)	(0.0102)	(0.0047)
Any emergency department use (%)	14.727	−0.0061	−0.0034	0.0009	−0.0085
	(35.437)	(0.0035)	(0.0067)	(0.0037)	(0.0043)
Any avoidable hospitalizations (%)	1.208	−0.0005	0.0003	0.0012	−0.0011
	(10.926)	(0.0037)	(0.0052)	(0.0027)	(0.0048)
Any designated low-value care (%)	0.562	0.0036	0.0036	0.0004	0.0038
	(7.476)	(0.0039)	(0.0040)	(0.0021)	(0.0051)
Predicted spending	392.133		−0.0012	−0.0021	−0.0005
	(664.401)		(0.0064)	(0.0031)	(0.0053)
Predicted any potentially HVC (%)	17.697		−0.0373	−0.0339	−0.0026
	(16.384)		(0.0675)	(0.0323)	(0.0049)
Predicted any potentially LVC (%)	16.007		−0.0056	0.0022	−0.0055
	(10.686)		(0.0429)	(0.0232)	(0.0050)
Predicted share of months in assigned plan (%)	94.211		0.0018	0.0114	0.0017
	(4.187)		(0.0353)	(0.0180)	(0.0050)
P-value on joint F-test		0.64	0.82	0.88	
Observations	58,172	58,172	58,172	58,172	58,172
Baseline Controls		X	X	X	X
Plan Controls				X	

Notes: This table reports reduced form results testing the conditional random assignment of enrollees to provider networks and health plans. Results are based on my primary sample (see Section II for details on primary sample construction). Baseline outcomes are the average for each enrollee in the three months prior to assignment. Predicted spending, high-value care (HVC), and low-value care (LVC) are formed using the other baseline variables. Detailed descriptions of the outcome measures are included in Appendix A. Columns 2-4 present the results of multivariate OLS models with enrollee characteristics as the independent variables and the assigned network breadth as the dependent variable. Column 5 presents bivariate OLS regressions with enrollee characteristics as the independent variable and assigned network breadth as the dependent variable. Standard errors are clustered at the county × month of assignment level.

Table 3—: Estimates of the Impact of Network Breadth on Health Care Use and Spending

	Share of sample (1)	Sample Mean (2)	2SLS (3)	2SLS (4)	2SLS (5)
Panel A. Total healthcare use and spending					
Any spending (%)	1.00	31.451	1.122	1.002	1.006
			(0.235)	(0.201)	(0.258)
Log spending	1.00	371.916	0.071	0.071	0.070
			(0.015)	(0.014)	(0.019)
Observations		295,728	295,728	295,728	349,044
Panel B. Spending by enrollee characteristics					
Male	0.60	406.086	0.053	0.059	0.077
			(0.022)	(0.018)	(0.022)
Female	0.40	321.607	0.097	0.088	0.054
			(0.022)	(0.022)	(0.028)
18-39	0.65	263.953	0.056	0.050	0.051
			(0.016)	(0.015)	(0.021)
40-64	0.35	569.574	0.104	0.119	0.104
			(0.031)	(0.028)	(0.037)
Panel C. Spending by predicted enrollee health status					
1st quartile predicted spending	0.25	91.099	0.065	0.063	0.039
			(0.018)	(0.017)	(0.023)
2nd quartile predicted spending	0.25	137.084	0.070	0.073	0.057
			(0.018)	(0.018)	(0.026)
3rd quartile predicted spending	0.25	268.800	0.057	0.057	0.068
			(0.030)	(0.029)	(0.040)
4th quartile predicted spending	0.25	990.682	0.063	0.087	0.085
			(0.041)	(0.039)	(0.051)
Baseline Controls			X	X	X
Enrollee Controls				X	X
Plan Controls					X

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II for details on primary sample construction). The independent variable is overall network breadth as measured by the normalized covered share of simulated visits. The dependent variable is log spending for Panels B and C. Columns 3 and 4 report the main two-stage least squares (2SLS) results from estimating Equation 5 for overall networks breadth with and without enrollee-level controls. Column 5 reports 2SLS results based on a model with plan fixed effects (see Appendix C) estimated on a broader sample that includes enrollees in the provider-owned plan. All standard errors are clustered at the county $\times$ month of assignment level.

Table 4—: Estimates of the Impact of Overall Network Breadth on Potentially High-Value and Low-Value Care and Consumer Satisfaction

	Sample Mean (1)	2SLS (2)	2SLS (3)	2SLS (4)
Panel A. Potentially high-value care				
Any high-value medical care (%)	11.729	0.757	0.742	0.485
		(0.147)	(0.135)	(0.175)
Any recommended preventive care (%)	2.137	0.109	0.118	0.112
		(0.049)	(0.047)	(0.070)
Any high-value prescription drugs (%)	9.343	0.306	0.213	0.065
		(0.148)	(0.115)	(0.178)
Any potentially high-value care (%)	17.697	0.788	0.680	0.479
		(0.186)	(0.161)	(0.224)
Panel B. Potentially low-value care				
Any imaging and lab (%)	13.462	0.291	0.294	0.731
		(0.159)	(0.145)	(0.193)
Any emergency department use (%)	5.362	−0.138	−0.110	−0.076
		(0.085)	(0.083)	(0.117)
Any avoidable hospitalizations (%)	0.407	−0.013	−0.014	−0.015
		(0.027)	(0.025)	(0.031)
Any designated low-value care (%)	0.207	0.004	0.003	−0.017
		(0.016)	(0.015)	(0.023)
Any potentially low-value care (%)	16.007	0.651	0.664	0.645
		(0.164)	(0.151)	(0.194)
Panel C. Satisfaction				
In assigned plan (%)	94.211	1.004	1.112	1.078
		(0.097)	(0.095)	(0.177)
Observations	295,728	295,728	295,728	349,044
Baseline Controls	—	X	X	X
Enrollee Controls	—		X	X
Plan Controls	—			X

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II for details on primary sample construction). The independent variable is overall network breadth as measured by the normalized covered share of simulated visits. The dependent variables include specific high-value and low-value services, and an ex-post demand measure of enrollee satisfaction. Panel C presents reduced form (rather than 2SLS) estimates as the outcome measures the likelihood that enrollees remain in their assigned plans. Columns 2 and 3 report the main two-stage least squares (2SLS) results from estimating Equation 5 for overall networks breadth with and without enrollee-level controls. Column 4 reports 2SLS results based on a model with plan fixed effects (see Appendix C) estimated on a broader sample that includes enrollees in the provider-owned plan. All standard errors are clustered at the county × month of assignment level.

Table 5—: Heterogeneity in Impact of Provider Network Breadth by Network Characteristics

	Main sample			Usual source of care sample		
	Primary model	Alternative model w/ physician and hospital		Alternative model w/ physician and hospital and usual source of care		
	Overall Network (1)	Physician Network (2)	Hospital Network (3)	Physician Network (4)	Hospital Network (5)	USOC in assigned plan (6)
Panel A. Healthcare use and spending						
Log spending	0.070	0.059	0.031	0.066	−0.007	0.176
	(0.019)	(0.021)	(0.017)	(0.038)	(0.030)	(0.041)
Any spending (%)	1.006	0.522	0.642	0.673	0.188	2.211
	(0.258)	(0.280)	(0.231)	(0.493)	(0.394)	(0.593)
Panel B. Potentially high-value care						
Any high-value medical care (%)	0.485	0.579	0.112	1.107	−0.463	1.848
	(0.175)	(0.196)	(0.166)	(0.363)	(0.314)	(0.459)
Any recommended preventive care (%)	0.112	0.027	0.090	0.044	−0.020	0.273
	(0.070)	(0.077)	(0.059)	(0.139)	(0.103)	(0.142)
Any high-value prescription drugs (%)	0.065	−0.014	0.071	−0.235	−0.248	1.244
	(0.178)	(0.201)	(0.167)	(0.376)	(0.308)	(0.404)
Any potentially high-value care (%)	0.479	0.486	0.162	0.450	−0.512	1.875
	(0.224)	(0.257)	(0.202)	(0.458)	(0.369)	(0.549)
Panel C. Potentially low-value care						
Any imaging and lab (%)	0.731	0.678	0.287	1.059	−0.094	1.363
	(0.193)	(0.239)	(0.179)	(0.420)	(0.306)	(0.411)
Any emergency department use (%)	−0.076	0.015	−0.081	−0.087	−0.175	−0.339
	(0.117)	(0.135)	(0.101)	(0.255)	(0.206)	(0.258)
Any avoidable hospitalization (%)	−0.015	−0.010	−0.008	−0.030	−0.067	0.123
	(0.031)	(0.041)	(0.030)	(0.085)	(0.067)	(0.081)
Any designated low-value care (%)	−0.017	0.025	−0.031	0.068	−0.057	0.055
	(0.023)	(0.022)	(0.021)	(0.042)	(0.041)	(0.046)
Any potentially low-value care (%)	0.645	0.600	0.252	0.867	−0.174	1.382
	(0.194)	(0.237)	(0.185)	(0.414)	(0.336)	(0.445)
Panel D. Consumer satisfaction						
In assigned plan (%)	1.078	0.543	0.698	0.454	0.187	5.173
	(0.177)	(0.192)	(0.148)	(0.365)	(0.261)	(0.367)
Observations	349,044	349,044	349,044	157,536	157,536	157,536
Baseline controls	X	X	X	X	X	X
Enrollee Controls	X	X	X	X	X	X
Plan controls	X	X	X	X	X	X
Usual source of care sample				X	X	X

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II for details on primary sample construction), including enrollees in the provider-owned plan. The dependent variables include measures of healthcare use and spending, specific high-value and low-value services, and an ex-post demand measure of enrollee satisfaction. Panel D presents reduced form, rather than 2SLS, estimates of the likelihood that enrollees remain in their assigned plans. In Column 1, the independent variable is overall network breadth (normalized covered share of simulated visits). Columns 2 and 3 report the main two-stage least squares (2SLS) results from estimating Equation 6 using physician and hospital network breadth in the same model. Columns 4-6 restrict the sample to enrollees who could be attributed to a physician or hospital based on care they sought prior to assignment (the "usual source of care sample"). The column reports the results of estimating Equation 7 on this restricted sample. All standard errors are clustered at the county × month of assignment level.

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Appendix for:

What does a provider network do? Evidence from random assignment in Medicaid managed care

Appendix A. Data and Outcomes

To estimate the impact of limited provider networks on Medicaid enrollees, I merge administrative health records from the New York State Department of Health (NYSDOH), managed care provider network directory information, and hospital characteristics from the American Hospital Association. I briefly describe each data source and my outcomes here.

A1. Administrative enrollment and claims data

I obtained de-identified administrative data on enrollment, plan choice, and insurance claims for the entire New York Medicaid population from 2008 to 2012.[1] The state requires all full risk managed care plans which enroll Medicaid beneficiaries to collect and submit standardized encounter data for all contracted services through the Medicaid Encounter Data System (MEDS). Data submissions are validated by a system of electronic edits and reviewed by Medicaid staff.

There are, and continue to be, concerns about the completeness of plan encounter data which includes both paid claims by plans and "encounters" reported to plans by capitated providers. The data provided by the state includes an indicator that separately identifies claims paid directly by the plan from encounters reported by providers. A recent evaluation of encounter data completeness by the Lewin Group identified New York encounter data as usable for research (The Lewin Group, 2012).

For each enrollee, I observe limited demographic data, monthly enrollment data, and claims for medical services covered by Medicaid. The data cover the months in which enrollees are in Medicaid fee-for-service (FFS) and managed care. The enrollment data include an indicator that I use to identify enrollees that are randomly-assigned to their health plans by the "auto assignment" algorithm.

The medical claims include detailed patient diagnoses, procedures, provider identifiers, and the amount paid by the insurer (MMC or FFS). New York State Department of Health staff have standardized the fee-for-service and managed care data. For the outpatient data, I use the Berenson-Eggers Type of Service (BETOS) codes to assign each HCPCS code to one of seven categories: evaluation and management, procedures, imaging, tests, durable medical equipment, other, or unclassified. For the inpatient data, I use the Clinical Classifications Software (CCS) developed by the Healthcare Cost and Utilization Project (HCUP) to assign each inpatient admission to a clinically meaningful category based on the primary diagnosis.

[1] The data was obtained pursuant to a Data Exchange Application & Agreement (DEAA) with New York Medicaid. The data was de-identified to protect the privacy of Medicaid enrollees.

A2. Outcome measures

Healthcare use and spending outcomes. When measuring healthcare use and spending, I include services paid for by the Medicaid managed care plans as well as any additional "carved out" services paid for by fee-for-service Medicaid. I use service categories provided by the NYSDOH to measure spending separately by broad category of service. Prior to assignment or plan choice, enrollees are covered by the publicly-operated, Medicaid fee-for-service program which allows me to observe their baseline healthcare use and spending. This enables powerful balance tests and allows me to construct a measure of enrollee health status (uncontaminated by provider network or plan effects) using a cross-validated, LASSO regression that takes as inputs enrollee demographics, diagnoses, and baseline spending to predict spending post-assignment. Appendix Section A7 describes this model in more detail.

Potentially high- and low-value services. Following Brot-Goldberg et al. (2017), I use my administrative health records to examine enrollees' use of a wide range of medical services, including those that are potentially wasteful and those considered to be of high value.

I examined three sets of potentially high-value services that policymakers worry are underused: (1) high-value medical care (e.g., primary care); (2) recommended preventive care; and (3) high-value prescription drugs (e.g., statins). Each set of services is intended to improve population health and reduce the incidence of costly disease (Chernew, Schwartz and Fendrick, 2015) and it is common for policymakers to provide financial rewards to managed care plans if the utilization of these services is high. The high-value medical care category includes primary care, mental health services, physical therapy, and prenatal/postpartum care. To measure the receipt of recommended preventive care, I use a set of measures developed by the Secretary of Health and Human Services (HHS) for adult Medicaid enrolles. These include the frequency of flu vaccination for adults ages 18 to 64, breast cancer screening, cervical cancer screening, smoking cessation counseling, HbA1c testing, and chlamydia screening in women. I also examined a set of low-value services either cited for potential overuse or believed to reflect underuse of primary or preventive care: (1) imaging and lab, services often cited as wasteful (e.g., Sorenson et al., 2020); (2) emergency department use; (3) avoidable hospitalizations; and (4) services designated as low-value care by clinicians (see Schwartz et al., 2014). The avoidable hospitalization measure I uses includes hospitalizations for: diabetes short-term complications, chronic obstructive pulmonary disease or asthma in adults, heart failure, and asthma in younger adults. For each service, I construct an indicator for whether an enrollee received that service in a month. I also construct indicators for "any potentially high-value care" and "any potentially low-value care" that measure whether enrollees received any of the potentially high- or low-value services, respectively, in a month.

Consumer satisfaction. The final outcome I study is enrollee utility or satisfaction as measured by whether or not an enrollee stays in their randomly assigned plan. I assume that enrollees' preferences are revealed through their subsequent plan choices since auto-assigned enrollees may switch plans after assignment. Specifically, for the first three months after assignment enrollees may switch for any reason, after which a nine-month lock-in period begins during which they may only switch for "good cause." While this differs from a traditional willingness-to-pay measure, in a world of consumer choice frictions (e.g., Handel and Kolstad, 2015*b*; Handel, Kolstad and Spinnewijn, 2019), an advantage of this measure is that it reflects the utility an enrollee experiences in their assigned plan (Israel, 2005), which is revealed in their subsequent plan switches.

A3. Alternative sample: extended sample of auto-assignees

The construction of my primary sample of auto-assignees is described in Section II. I construct two alternative samples of auto-assignees. First, I construct balanced samples of enrollees that remain in Medicaid for at least 12, 18, and 24 months post-assignment. Second, I construct an imbalanced sample of enrollees that are in Medicaid for at least the 6 months post-assignment, but begin to attrit from the sample after 6 months. I impose the sample restrictions used to construct my primary sample (Section II), with the exception that I require additional months of enrollment in Medicaid for the extended balanced samples. In addition, the enrollees differ slightly from my primary specification, even for the imbalanced sample, due to the imposition of small additional restrictions—for example, enrollees had to remain in New York City for at least 12 months following assignment (rather than 6 as in my primary sample).

A4. Alternative sample: enrollees that made active plan choices

The construction of my primary sample of auto-assignees is described in Section II. I construct an alternative sample using data on adult Medicaid enrollees in New York City that made active plan choices during the period April 2008 to July 2012. I restrict this sample in four ways to ensure comparability to my primary estimation sample. First, I drop enrollees that live outside the five boroughs of New York City. Second, I restrict the sample to enrollees aged 18 to 65. Third, I remove individuals who qualify for Medicaid because they receive Supplemental Security income (SSI) due to differences in their auto-assignment policy. Fourth, to keep the sample balanced, I restrict the primary sample to enrollees that are in Medicaid for at least three months prior, and six months after, their active plan choice. I make these restrictions because I'm interested in identifying a set of enrollees in the same market, age band, and eligibility category as the auto-assignee population. These sample restrictions leave me with 95,888 enrollees in five counties and ten plans. Appendix Table 7 for how the baseline characteristics of this sample compare to the auto-assignees—the two sets of enrollees have similar characteristics and healthcare utilization patterns at baseline.

A5. Provider Network Data

I assemble a unique dataset on the physician and hospital managed care networks using New York's Provider Network Data System (PNDS). Recent research has highlighted inaccuracies in managed care provider networks (Resneck Jr et al., 2014). Reassuringly, New York has a long history of collecting and verifying managed care network data. New York began collecting data on managed care networks in 1996 to determine compliance with network adequacy requirements and create provider directories for consumers. HHS (2014) examined state standards for access to care in Medicaid and reported that New York, unlike most states, had several policies in place to ensure timely and accurate submission of provider network data. Federal law requires that states contract with external quality review organizations (EQRO) to evaluate access to care for Medicaid managed care enrollees.[2] In New York, the state's EQRO uses secret-shopper calls to determine the accuracy of managed care provider directories.

The PNDS is standardized, allowing us to construct comparable network measures for each plan. The managed care plans all report several provider identifiers, including the state license number and the national provider identifier (NPI) for both physicians and hospitals. The plans also report Medicaid provider identification numbers which allow us to merge the network data with fee-for-service claims and managed care encounter data. While the PNDS data is reported quarterly, I construct an indicator for whether a provider is in-network at the annual level. The indicator is set to one if the provider is in network in any quarter. The PNDS also includes an indicator for each provider-insurer pair that identifies which insurance products the provider is in network for. Since many of the managed care plans serve both the Medicaid and commercial markets this indicator allows me to isolate providers in their Medicaid network.

The PNDS also includes basic data on provider characteristics, including gender, type, specialty, and address. With provider and patient zip code data, I construct travel time for each patient-provider pairing in New York City using the ArcGIS Network Analyst.[3] For hospitals I follow Ericson and Starc (2015) and use the 2007 to 2012 American Hospital Association (AHA) data to identify the set of general medical and surgical hospitals, excluding long-term care, rehabilitation and Veterans Affairs hospitals. This data was hand-merged to the New York Medicaid operating certificates for hospitals to identify the set of hospitals serving New York Medicaid enrollees. The AHA data was then used to construct variables (such as services provided or location) for each Medicaid hospital in New York City. As in Ho (2006), I fill in missing data using surrounding years wherever possible. The final dataset comprises 63 hospitals.

A6. Restrictions on payment to Medicaid providers for out-of-network services

Only a small share of physician and hospital visits are to out-of-network providers in New York Medicaid. This section discusses the rules related to out-of-network service use and billing for out-of-network services by Medicaid providers in New York State.

Guidance[4] from New York Medicaid states that unless a provider and Medicaid enrollee agree in advance of the provision of services that the enrollee is being seen as a private pay patient, the provider is prohibited from billing the enrollee for services, or otherwise requesting compensation for services other than any applicable copayments. This applies whether the enrollee is enrolled in the Medicaid fee-for-service program or Medicaid managed care. The guidance suggests that wherever a provider and enrollee reach such an agreement, best practice is for the provider to obtain and keep a signed written consent memorializing the agreement. Although the guidance linked to above dates from 2014, the prohibition on billing Medicaid enrollees absent a private pay arrangement is a longstanding rule based in federal statute.

[2] 42 CFR §§ 438.310-370.

[3] I thank Fei Carnes at the Center for Geographic Analysis at Harvard University for assistance with this.

[4] See New York State Medicaid Update - February 2014, Volume 30 - Number 2: https://www.health.ny.gov/health_care/medicaid/program/update/2014/201402.htm#bill accessed on February 16, 2021.

New York's Medicaid managed care model contract ("model contract") also suggests that providers that furnish Medicaid-covered services to a Medicaid managed care enrollee are not entitled to payment from the enrollee's plan unless: (1) the provider is in-network with the plan; (2) the plan authorized the enrollee to receive the services before they were rendered (because, for example, there were no in-network providers available to render the service to the enrollee); or (3) the plan is legally required to grant a limited period of service continuity (ranging from 60 to 90 days, or up to 60 days after delivery for pregnant women) to preserve an ongoing treatment relationship. See Section 15.6 of the model contract.[5]

According to the Model Contract, a limited period of service continuity is required only in the case of:

1) New plan enrollees with a life-threatening or degenerative and disabling condition for up to 60 days following enrollment;

2) New plan enrollees who enroll in the second trimester of pregnancy, for up to 60 days after delivery; or

3) Existing plan enrollees whose provider leaves the network for reasons other than imminent harm to patients, fraud, or a final disciplinary action, for up to 90 days from the provider's departure from the network or 60 days after delivery.

For a provider to receive payments under circumstances 1, 2, or 3, the provider must agree to accept the plan's rates as payment in full (which may not exceed those provided to in-network providers), adhere to the plan's quality assurance requirements and provide the plan with all necessary medical information related to the care, and otherwise adhere to the plan's policies and procedures. *See* Section 15.6 of the Model Contract.[6]

Hence, out-of-network providers that furnish services to Medicaid enrollees without those enrollees receiving prior approval have limited means to collect payment from either the plan (who is not required to pay) or the enrollee (who cannot be charged). As a result, prior authorization for out-of-network care is a powerful tool to steer patients to in-network providers in Medicaid managed care in New York.

A7. Predicting enrollee health status using baseline characteristics

To predict enrollee health status I estimate a cross-validated Lasso regression with post-assignment healthcare spending (in the 6 months after assignment) as the outcome and use a set of demographic and baseline utilization measures as predictors. For demographics, I use enrollees' Medicaid eligibility category, zip code, race, five year age by gender bins, and an indicator for whether they were an "auto assignee" or "active chooser." In addition to these predictors, I use indicators for the 700 most common baseline diagnosis codes (those obtained

[5] The latest version of the MMC model contract can be accessed here: https://www.health.ny.gov/health_care/managed_care/docs/medicaid_managed_care_fhp_hiv-snp_model_contract.pdf

[6] There are two caveats to the answer provided above. First, in the case of family planning services, enrollees are entitled to see any Medicaid-enrolled provider, whether in-network or not, and the plan must pay for the services provided. Second, in practice an out-of-network provider could bill a plan for services rendered to a plan enrollee, no matter the circumstances. However, Section 22.3 of the Model Contract states that all covered services, with limited exceptions such as emergency services and family planning services, must be provided through provider agreements with network providers.

by enrollees at anytime in the 12 months prior to assignment), baseline medical spending, and baseline pharmacy spending. The baseline spending variables are z-score normalized because they are continuous and on a different scale than the binary indicators which can lead to problems in Lasso estimation.

A8. Approach to defining high-prevalence chronic conditions

To document chronic conditions among the enrollees, I assigned Hierarchical Condition Codes (HCCs) using up to 12 months of pre-assignment data for each enrollee. To avoid post-treatment bias, I do not use diagnoses or procedures obtained post-assignment. I categorized enrollees into three chronic conditions based on the following lists of HCCs:

Chronic condition	Hierarchical Condition Codes (HCCs)
Behavioral health	54, 55, 56, 58
Diabetes	17, 18, 19
Cardiovascular disease	84, 85, 86, 87, 88, 96, 99, 100, 103, 104, 106, 107, 108

I examine heterogeneous treatment effects using these categories in Appendix Table 15.

Appendix B. Network measure construction

In this section, I provide some additional details on the estimation of the physician demand model and discuss how I construct two alternative measures of network breadth—the "covered share of visits" measure and the "network utility" measures.

B1. Model of Physician Demand

I begin by providing additional details on the physician demand model. The method and specification for estimating physician demand differ from the hospital model in two ways. First, due to the large physician choice set (n=22,983), and the small volume of Medicaid claims for many physicians, it is not possible to estimate a fixed effect for each physician (as was done for each hospital). Instead, I estimate separate physician demand models in each of the forty-two neighborhoods (defined by zip) in NYC. For each neighborhood, I estimate fixed effects for the largest five percent of practices serving the enrollees of that neighborhood. Including neighborhood-specific fixed effects for these physicians is critical to fit since the distribution of claims across physicians is highly-skewed.[7] The remaining physicians are undifferentiated in the model beyond their observed characteristics. To minimize scaling differences across the models for each neighborhood, I normalize the fixed effect for the "small practices" to equal zero in each neighborhood.

The large choice set also makes it infeasible to estimate the conditional logit model using the full set of alternatives for each observation. Instead, I follow McFadden (1978) and for each choice instance select four random alternatives (in addition to the chosen physician) and proceed with the estimation using these subsets. McFadden (1978) demonstrates that the likelihood function for multinomial logit with a subset of alternatives reduces to the

[7] One limitation of this approach is that the designation of large practice is based on the data. Unfortunately, the available data on physicians do not include exogenous measures of practice size.

standard likelihood if the choice of the subset satisfies a "uniform conditioning property," a requirement that each alternative has an equal probability of being selected. The use of random subsets satisfies this property.

To estimate the physician demand model I assume that with some probability consumer i in neighborhood n enrolled in plan j seeks out a physician for services s. Their utility from visiting physician p at time t is given by:

$$u_{i,j,s,t,p,n} = \underbrace{\delta_n(\text{Dist}_{i,p} \times Z_{i,s,t})}_{\text{Distance}} + \underbrace{\lambda_n(X_p \times Z_{i,s,t}) + \xi_{p,n}}_{\text{Physician Characteristics}} + \underbrace{\psi_n \cdot 1\{p \notin N_{j,t}\}}_{\text{Out-of-Network Cost}} + \epsilon_{i,s,t,j,p,n} \tag{B1}$$

where $\text{Dist}_{i,p}$ is patient travel distance and distance-squared (in minutes), X_p are observed physician characteristics, $\xi_{p,n}$ are unobserved physician characteristics (represented by physician fixed effects for large practices),[8] and $1\{p \notin N_{j,t}\}$ is an indicator that physician p is out-of-network for plan j in time t (with ψ_n the hassle cost), and $\epsilon_{i,s,t,j,p,n}$ is an i.i.d. Type 1 extreme value error. Patient observables $Z_{i,s,t}$ are interacted with distance and physician characteristics to allow for preference heterogeneity. Since patients often receive multiple services in a single physician visit, s is a vector of indicator variables that identifies whether a visit contained the following services classified by BETOS codes: evaluation and management, procedures, imaging, tests, durable medical equipment, other, or unclassified. The physician demand estimates are presented in Appendix Table 4 and discussed in Section III.

B2. Construction of "covered share of visits" measure

To assess the robustness of my results to alternative measures of network breadth, I use methods from Ericson and Starc (2015) to a construct a "visit shares" measure at the plan-by-year-by-zip code level as the fraction of visits (hospital admissions or physician visits) for enrollees living in a given zip code covered by each managed care network. I pool healthcare claims for the sample period (April 2008 to December 2012) to construct this measure. Intuitively, the measure varies across plans and zip codes based on systematic differences in where enrollees in different zip codes seek physician and hospital care and which providers are in network for each plan. One limitation of this approach is that the provider choices of managed care enrollees are shaped by their networks, which is not accounted for in the visit share measure. This could be a problem, for example, in a zip code where one plan has a dominant market share. In that case the measure of network breadth may be artificially inflated for that plan because enrollees in that plan disproportionately seek care from in-network providers and these comprise a large share of the visits for all enrollees residing in that zip code.

B3. Construction of "network utility" measure

In addition to my primary covered shared of simulated visits measure, and the covered share of visits measure described in the prior section, I also calculate the expected utility provided by each plan's network at the plan-by-year-by-zip code level. In the hospital case, for example, I follow Ho (2006, 2009) and Shepard (2016) and define the expected utility of

[8] Physicians may be identified as a large practice in some neighborhoods and not in others.

the network for an individual i in plan j in year t:

$$\text{HospitalEU}_{i,t,j} \equiv E[\max_h (V_{i,j,t,h}(N_{j,t}) + \epsilon_{i,t,j,h})] = \log\left(\sum_h \exp(V_{i,j,t,h}(N_{j,t}))\right) \quad \text{(B2)}$$

where representative utility $V_{i,j,t,h}(N_{j,t})$ is defined as $u_{i,j,t,h} - \epsilon_{i,t,j,h}$. In constructing this measure, I use the coefficients from column 2 in Appendix Table 3. The measure accounts for unobservable hospital quality, distance (and distance squared) between patient zip code and each hospital, and whether or not the hospital is in network for each plan. Because the scale of network utility is arbitrary I normalize the measure to have mean zero and standard deviation one. The physician network utility measure is constructed in a similar fashion, with the major difference being that the coefficients in the physician model vary by neighborhood (due to the infeasibility of estimating a single physician choice model). Following Ericson and Starc (2015), the network utility measures are z-score normalized within each zip code. Consistent with prior work, the three different methods of measuring network breadth are highly-correlated (Appendix Figure 5).

Appendix C. Additional Details on Research Design

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C1. *Alternative specification with plan fixed effects*

This section describes the alternative specification introduced in Section IV. In this specification I include plan fixed effects (in addition to zip code fixed effects) to address the potential correlation between the γ_j (i.e., the plan effects) and enrollee's network breadth, Γ_{zj}, in Equation 2. In other words, there is a concern that the outcomes of enrollees assigned to narrower (or broader) networks may be impacted by the unobservable non-network characteristics of the plans they plans they are assigned to, such as how aggressively those plans use supply-side tools to ration care. Each plan may adopt a different bundle of managed care (i.e., supply-side) tools to manage their enrollees and this decision is made jointly with the formation and management of their provider networks. For example, one of the largest Medicaid Managed Care plans in New York City is owned by the local safety net hospital chain and operates a narrow hospital network, including only a handful of additional facilities. Appendix Figure 6 demonstrates that the enrollees assigned to this plan generated a lot of health care spending and utility, despite its narrow network, potentially biasing naive comparisons between plans.

To motivate the alternative specification, we return to our model of the data generating process for health care spending where log spending (Y_{izjct}) for enrollee i living in zip code z enrolled in plan j is determined by a location component (ω_z), plan component (γ_j), provider network component (Γ_{zj}), enrollee-level fixed effect (ζ_i), time-varying observables (X_{it}), and a mean zero shock (ϵ_{ijzct}):

$$Y_{izjct} = \omega_z + \gamma_j + \beta\Gamma_{zj} + \zeta_i + \delta X_{it} + \epsilon_{ijzct} \quad \text{(C1)}$$

In my primary specification, I recover the effect of network breadth on healthcare spending by estimating Equation C1 at the enrollee-level, combining γ_j, ζ_i, and ϵ_{ijzct} into a compound error term η_{ijzct} and remove enrollees assigned to the outlier plan. However, our estimates of β may be biased if plans have independent effects on enrollee outcomes (i.e., the differences in the γ_js are economically significant) and those "plan effects" are correlated with enrollees' assigned network breadths. To address this, the alternative specification recovers the effect of network breadth on healthcare spending by estimating Equation C1 at the enrollee-level with controls for both zip code and plan of assignment, combining ζ_i, and ϵ_{ijzct} into a compound error term υ_{ijzct}:

$$Y_{izjct} = \alpha + \gamma_j + \beta\Gamma_{zj} + \phi_{ct} + \omega_z + \delta X_{it} + \upsilon_{ijzct} \tag{C2}$$

where β is the coefficient of interest, α is a constant, γ_j are plan of assignment fixed effects, ϕ_{ct} are county $c \times$ month t of assignment fixed effects (the unit of randomization), ω_z are zip code fixed effects, and X_{it} is a vector of individual controls.

To address the endogeneity of enrollees sorting into plans, I restrict to auto-assigned enrollees and instrument for an enrollee's plan (γ_j) and provider network breadth (Γ_{zj}) with their assigned plan and the breadth of their assigned network. The resulting second stage estimating equation is:

$$Y_{izjct} = \alpha + \widehat{\gamma_j} + \beta\widehat{\Gamma_{zj}} + \phi_{ct} + \omega_z + \delta X_{it} + \epsilon_{ijzct} \tag{C3}$$

where $\widehat{\gamma_j}$ and $\widehat{\Gamma_{zj}}$ are predicted plan of enrollment and provider network breadth, respectively, based on first stage regressions that use assigned plan and assigned provider network breadth to instrument for actual plan and network.

The key source of identification in this model is the variation in network breadth that remains at the plan-by-zip level after controlling for enrollees' assigned plan and, separately, zip code. By virtue of including zip code fixed effects, our identification relies on within-zip code variation (as in our primary specification) and, hence, removes any potential bias due to a correlation between provider network breadth and location effects (i.e., provider networks may be broader in zip codes where enrollees tend to use more care for other reasons). However, within-zip code differences in provider network breadth may also be correlated with the plan effects (γ_j) if some plans are broader or narrower, on average. I address this, by also including controls for assigned plan (or instrumenting for plan with assigned plan). In this specification, we are comparing the outcomes for enrollees who are assigned broader provider networks because they are assigned to a plan in a zip code where that plan's network is relatively broad (both relative to its network elsewhere and to the networks of other plans in that zip code). To estimate β in Equation C3 requires that there exists variation at the plan $\times$ zip-level after residualizing on plan and, separately, zip code. Fortunately, in Panel D of Appendix Figure 7, we see that considerable variation in provider network breadth exists to estimate Equation C3.

Since auto assignment is not binding, I estimate the causal impact of network breadth with two-stage least squares using enrollee's assigned plan and provider network breadth to instrument for their actual plan and provider network breadth. Given 10 plans, there are 9 first stage estimating equations to predict actual plan enrollment (with 1 plan omitted) and an additional first stage equation that uses assigned provider network breadth to predict actual provider network breadth C3. The regressors in each of the first-stage equations are identical. To account for any serial correlation within randomization cohorts, I cluster standard errors at the county × month of assignment level in both the first and second stage regressions.

C2. Event study specification

This section describes the regression specification for our event study. Let i index enrollees. Let t indicate event-time, defined as months relative to auto assignment. The data is at the enrollee-month level.

For a given outcome, Y_{it}, our event study regression specification takes the form:

$$Y_{it} = \alpha_i + \alpha_t + \left[\sum_{t \neq -1} \beta_t \times \widetilde{\Gamma}_i \right] + \epsilon_{it}, \tag{C4}$$

where α_i are enrollee fixed effects, α_t are event-time fixed effects, $\widetilde{\Gamma}$ is the enrollee's assigned network breadth, and β_t are coefficients on network breadth that vary by event time. I omit the month prior to assignment, $\beta_{t=-1}$, so that the point estimates for the other event times can be interpreted relative to the pre-assignment baseline period. Because the strength of the instrument (i.e., assigned network breadth) weakens over time as enrollees switch out of their assigned plans, I also estimate an IV version of the event study where assigned network breadth, $\widetilde{\Gamma}$, is used to instrument for actual network breadth Γ, which may differ by period.

C3. Specification Checks Related to Analyses of Heterogeneity by Network Characteristics

This section presents specification checks for the models used to explore heterogeneity in the effects of network breadth by network characteristics, and discusses the differences in estimates with and without plan fixed effects.

Column 1 of Appendix Table 18 reproduces the results from the randomization test presented in Column 3 of Table 2, a regression of the full set of baseline and predicted outcomes on the simulated visit shares measure of network breadth. Columns 2 and 3 report results for the same regression, but with measures of physician and hospital network breadth. None of the regressors were significant at the five percent level in either regression. In Columns 4 and 5 I include hospital network breadth as a control in the regression with physician network breadth as the outcome and physician network breadth as a control in the regression with hospital network breadth as the outcome. A similar story emerges, with none of the regressors significant at the five percent level. Reassuringly, none of the F-tests of the joint significance of the coefficients for each of the five regressions are significant at the five (or ten) percent level.

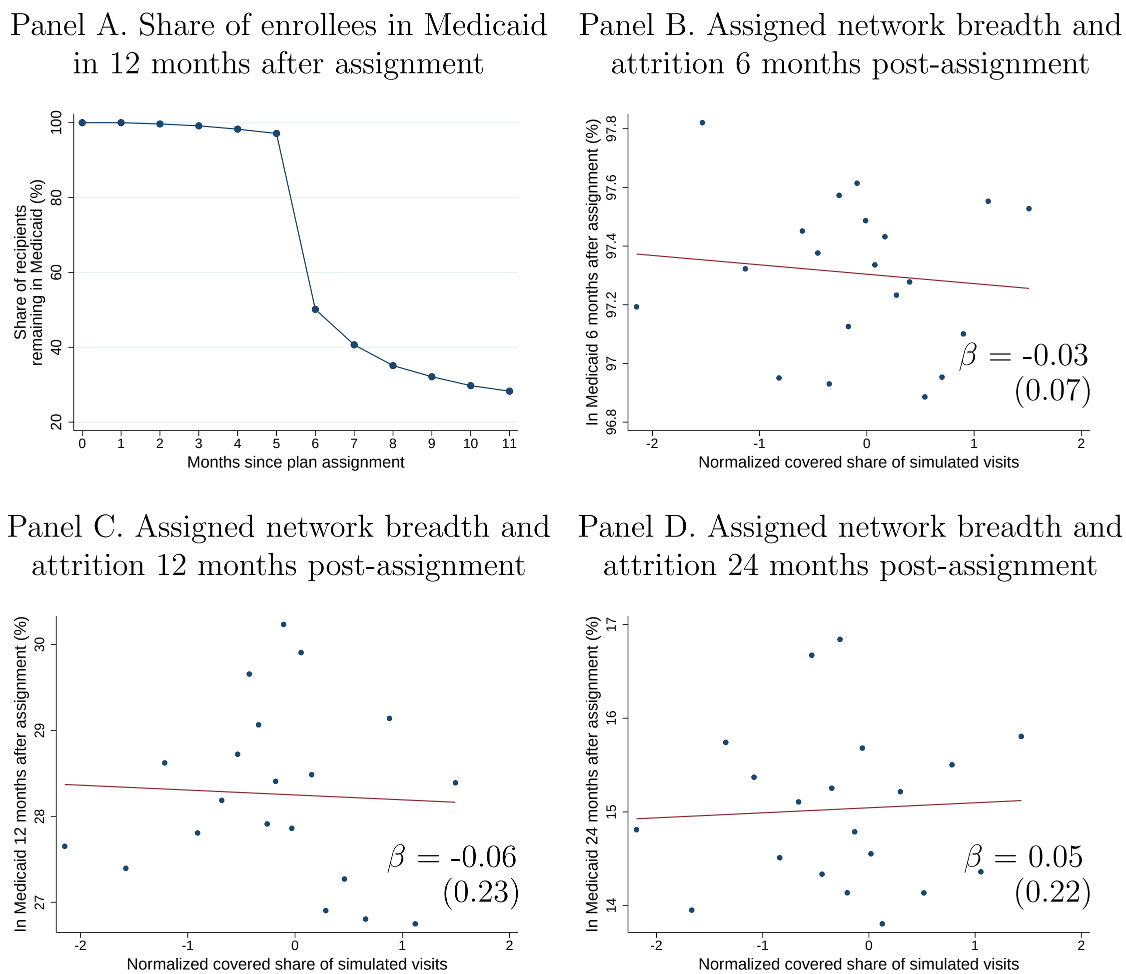
As in my primary specification, I examine the sensitive of the estimated effects of physician and hospital network breadth to the inclusion of plan controls. To do so, I add controls for plan of assignment to my specification as follows:

$$Y_{izjct} = \alpha + \omega_z + \widehat{\gamma}_j + \beta_1 \widehat{Phys_{zj}} + \beta_2 \widehat{Hosp_{zj}} + \phi_{ct} + \delta X_{it} + \eta_{ijzct} \tag{C5}$$

where $\widehat{\gamma}_j$ is the predicted plan of enrollment for each enrollee. In this specification, I control for plan and zip and use the rich variation that remains at the plan-by-zip level.

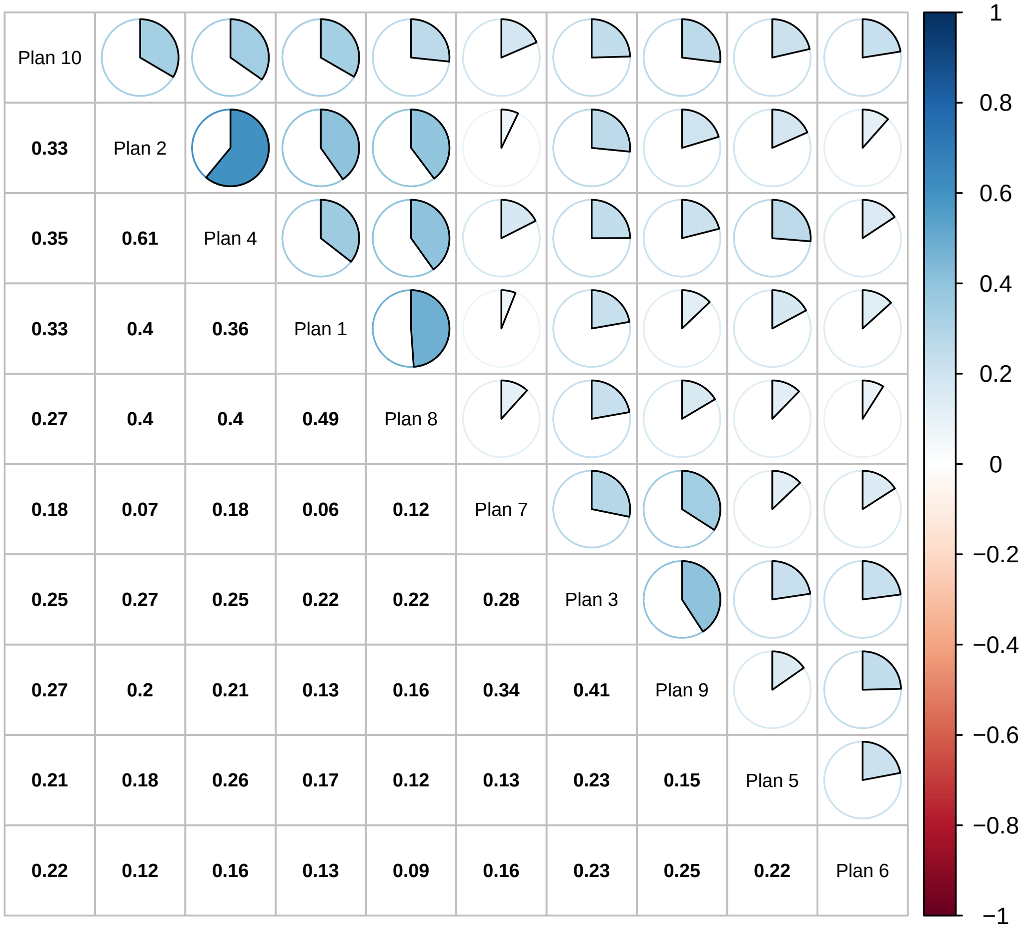
Unlike the primary results I present in Section V, my estimates of the effects of physician and hospital network breadth are somewhat sensitive to the inclusion of plan controls, particularly estimates of the effect of network breadth on the use of potentially high-value and low-value services. Panels A and B of Appendix Figure 19 document that two of the Medicaid managed care plans with the broadest physician networks (once I residualize on hospital network breadth and my controls) generate low rates of high-value and low-value service use among randomly assigned enrollees. However, when I condition on plan (i.e., include plan fixed effects) in Panels E and F, I find strong associations between assigned physician network breadth and utilization. Because non-network dimensions of these plans (e.g., utilization management, prior authorization, etc.) may be correlated with physician network breadth—in this case the plans with the broadest physician networks appear to ration care more aggressively—my preferred specification in this section includes plan fixed effects. Comparisons of Table 5 (with plan controls) and Appendix Table 17 (without plan controls) reveal that the main differences relate to estimates of the effects of physician and hospital network breadth on the use of potentially high-value and low-value services. Results related to health care spending and consumer satisfaction are qualitatively similar between the two models.

Appendix Figure 1. : Testing for Differential Attrition



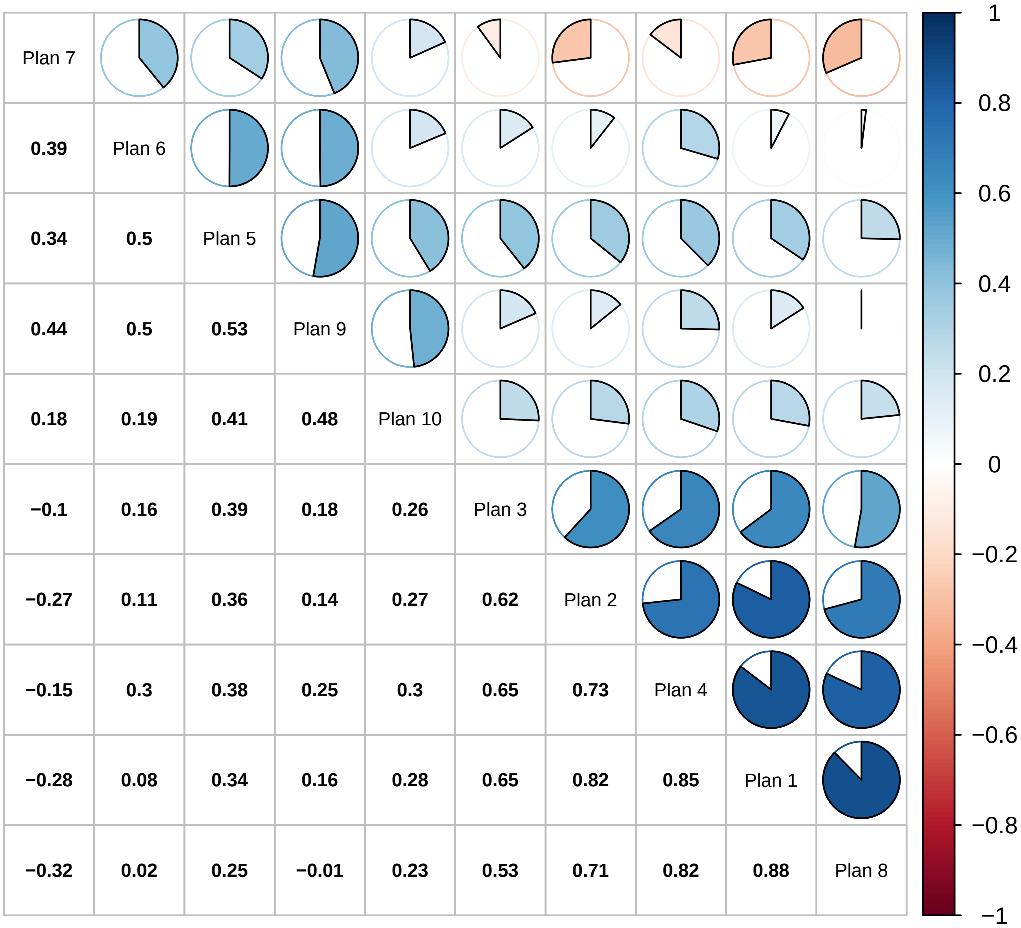
Notes: These figures examine the prevalence of differential attrition in my primary sample. Panel A plots the raw share of enrollees in Medicaid managed care separately for each month following auto assignment. I restrict the sample to enrollees that were auto assigned with at least 12 months remaining in the sample (an auto assignment date of January 2012 or earlier). The large drop in enrollment at six months is due to loss of eligibility that occurs for enrollees following a guaranteed six months of eligibility that starts at the beginning of their MCO enrollment (see New York State Socal Services Law 364-j (11)). Panels B-D contain residualized binned scatterplots of the reduced form impact of normalized covered share of simulated visits (network breadth) on enrollment in Medicaid in the six, twelve, and twenty-four months post-assignment. The binned scatterplots are constructed by first regressing assigned network breadth and the outcome variable on the set of control variables (i.e. age, gender, race, tenure, baseline outcomes, county $\times$ month of assignment), calculating residuals, and grouping the residualized network breadth measure into 20 equal-sized bins. The mean for each outcome is added back in to ease interpretation. The solid line and corresponding coefficient are based on an OLS regression of the residualized outcome on the residual network breadth measure, with standard errors clustered at the county $\times$ month of assignment level (Chetty, Friedman and Rockoff, 2014).

Appendix Figure 2. : Correlation of Physician Provider Networks Across Medicaid Managed Care Plans, 2010



Notes: This figure reports the correlation of physician network participation (at the individual physician level) across the ten Medicaid managed care plans in my sample. For the year 2010, I construct a vector of all physicians practicing in New York City and, based on the provider network data, an indicator for whether they are "in-network" for each plan. The figure reports the pairwise Pearson correlation coefficients between the vectors that measure physician network participation in each plan. Plan 7 is the provider-owned plan.

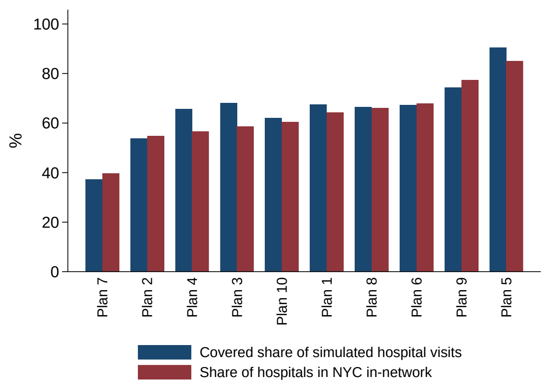
Appendix Figure 3. : Correlation of Hospital Provider Networks Across Medicaid Managed Care Plans, 2010



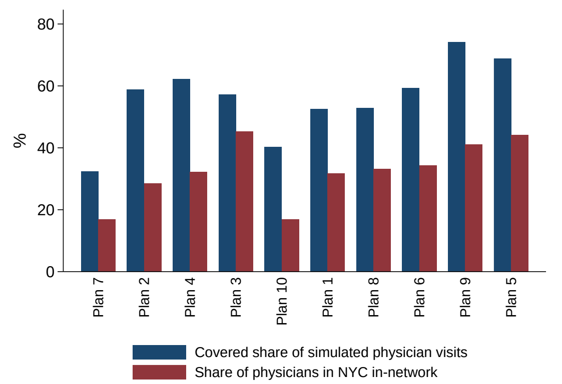
Notes: Notes: This figure reports the correlation of hospital network participation (at the hospital level) across the ten Medicaid managed care plans in my sample. For the year 2010, I construct a vector of all hospitals in New York City and, based on the provider network data, an indicator for whether they are "in-network" for each plan. The figure reports the pairwise Pearson correlation coefficients between the vectors that measure hospital network participation in each plan. Plan 7 is the provider-owned plan.

Appendix Figure 4. : Plan-Level Network Breadth and Contracted Physician and Hospitals

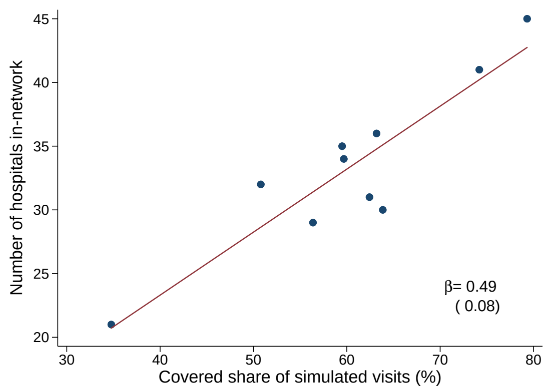
Panel A. Hospital network breadth and share of NYC hospitals in-network



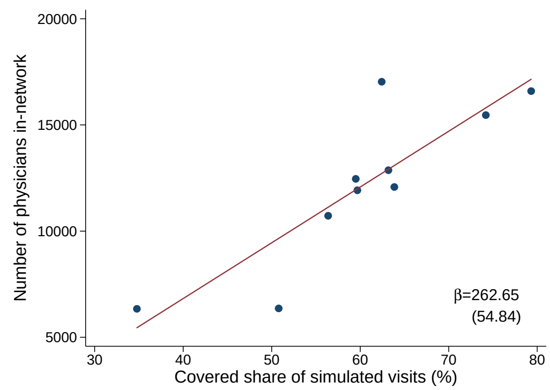
Panel B. Physician network breadth and share of NYC physicians in-network



Panel C. Overall breadth and number of contracted hospitals

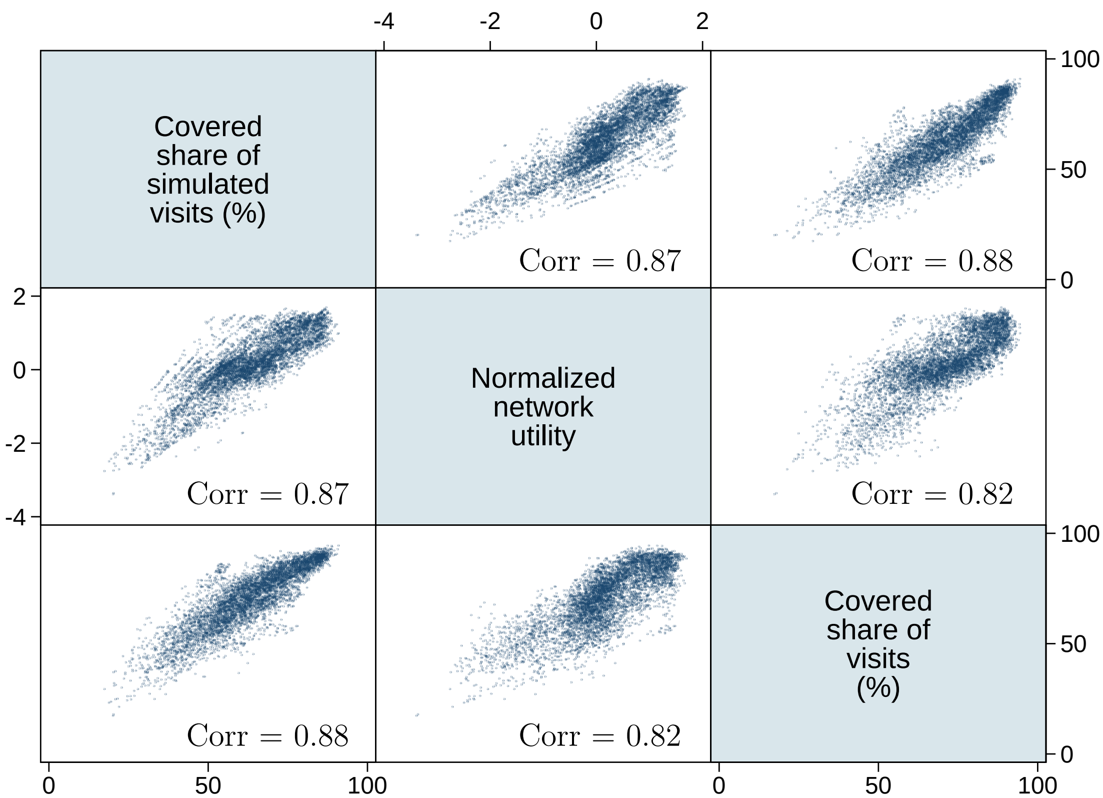


Panel D. Overall breadth and number of contracted physicians



Notes: Panels A and B of this figure plot the covered share of simulated hospital and physicians visits at the plan-level against the fraction of hospitals and physicians in New York City covered by each plan. Panels C and D plot the relationship between the overall share of simulated visits covered by each plan on the x-axis against the number of hospitals and physicians in New York City that participate in each plan on the y-axis. The data on physician and hospital network participation with each plan is drawn from the Provider Network Data System (PNDS) plan directories for 2010. The hospital and physician counts for each New York City county are drawn from the Area Health Resources File for 2010. Plan names are masked at the request of the New York State Department of Health.

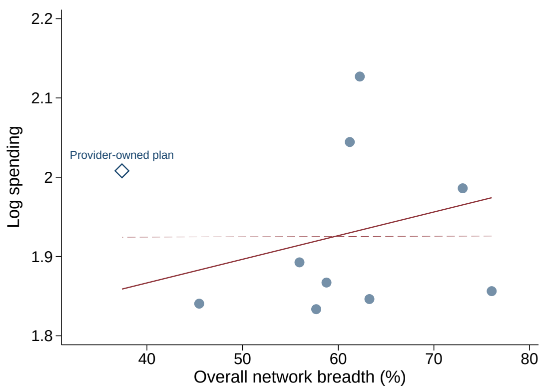
Appendix Figure 5. : Pairwise Correlations of Measures of Network Breadth Based on Different Measurement Methods



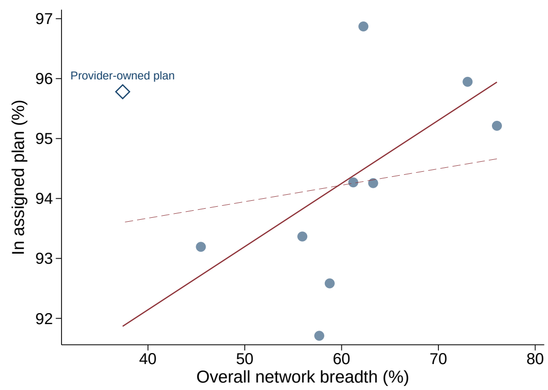
Notes: This figures plots pairwise correlations for different measures of overall network breadth. The covered share of simulated visits measure is my primary measure of network breadth. It is constructed at the plan-by-zip-by-year level (see Section III for a detailed description of how I construct this measure). The network utility measure captures the expected utility of each plan's physician and hospital network at the plan-by-zip-by-year level (e.g. Shepard, 2016). The covered share of visits measure is the share of observed visits for enrollees at the zip-by-year level that were covered by each of the managed care plan networks. Further details on the construction of each of the alternative measures is detailed in Appendix Section B.

Appendix Figure 6. : Assigned Network Breadth, Health Care Spending, and Consumer Satisfaction at the Plan Level

Panel A. Healthcare spending



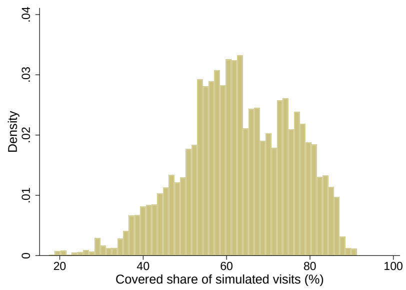
Panel B. Consumer satisfaction



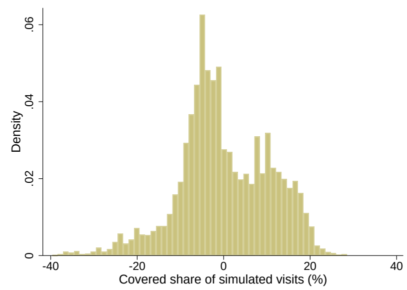
Notes: These figures plot residualized binned scatterplots of the reduced form impact of normalized covered share of simulated visits (network breadth) on healthcare spending and consumer satisfaction. Each binned scatterplot is constructed by first regressing assigned network breadth and the outcome variable on the set of control variables (i.e. age, gender, race, tenure, baseline outcomes, county × month of assignment), calculating residuals, and grouping the residualized network breadth measure into bins based on plan of assignment. The mean for each outcome is added back in to ease interpretation. The hollow diamond marks the provider-owned plan and the solid circles correspond to the other nine plans in the data. The provider-owned plan is a clear outlier. It has a narrower network than the other plans, but enrollees randomly-assigned to it generate higher levels of healthcare spending and consumer satisfaction as compared to enrollees randomly-assigned to other plans. The solid line and corresponding coefficients omit the provider-owned plan. For each panel, the inclusion of the provider-owned plan biases the effect of provider network breadth towards the null (the dashed line).

Appendix Figure 7. : Variation in Network Breadth Across and Within Zip and Plan

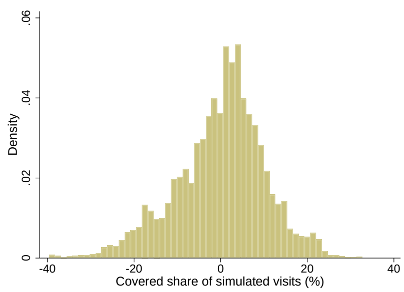
Panel A. Overall variation



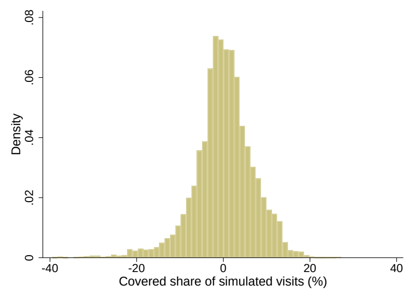
Panel B. Demeaned by zip



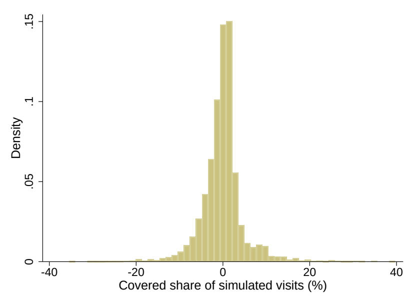
Panel C. Demeaned by plan



Panel D. Demeaned by plan and zip

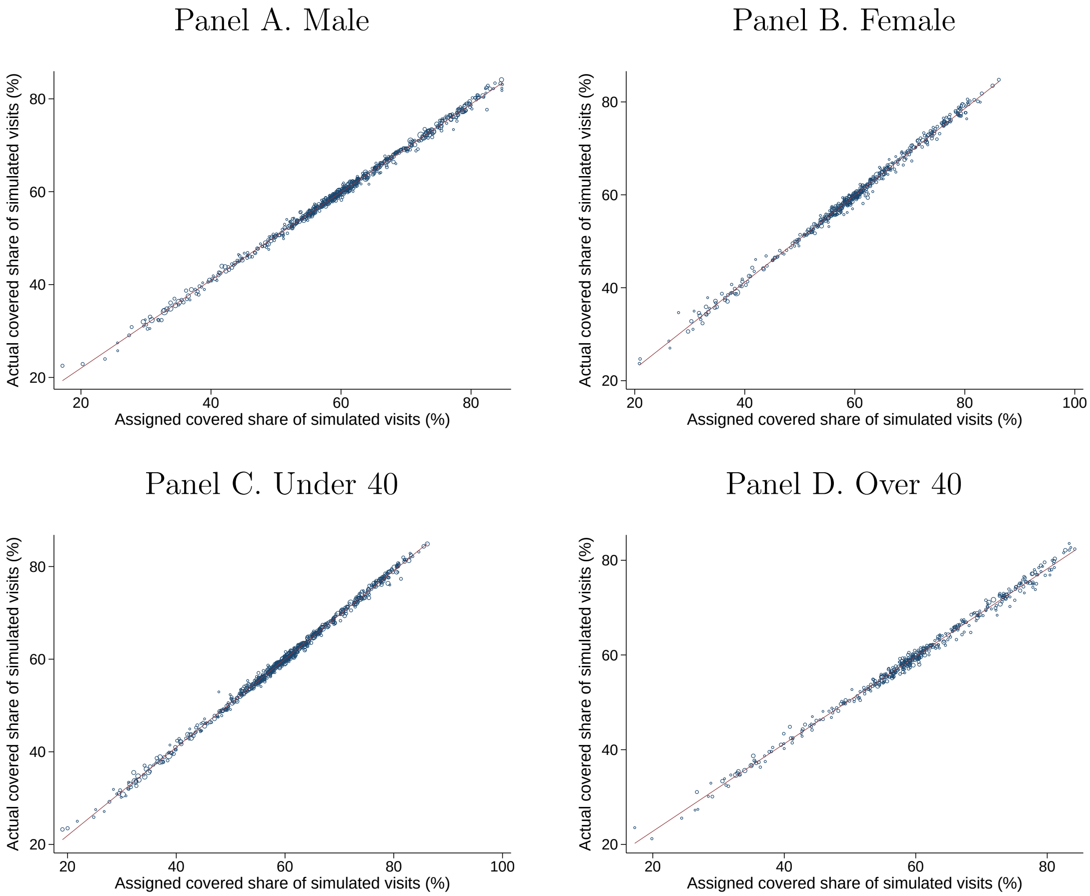


Panel E. Demeaned by plan-by-zip



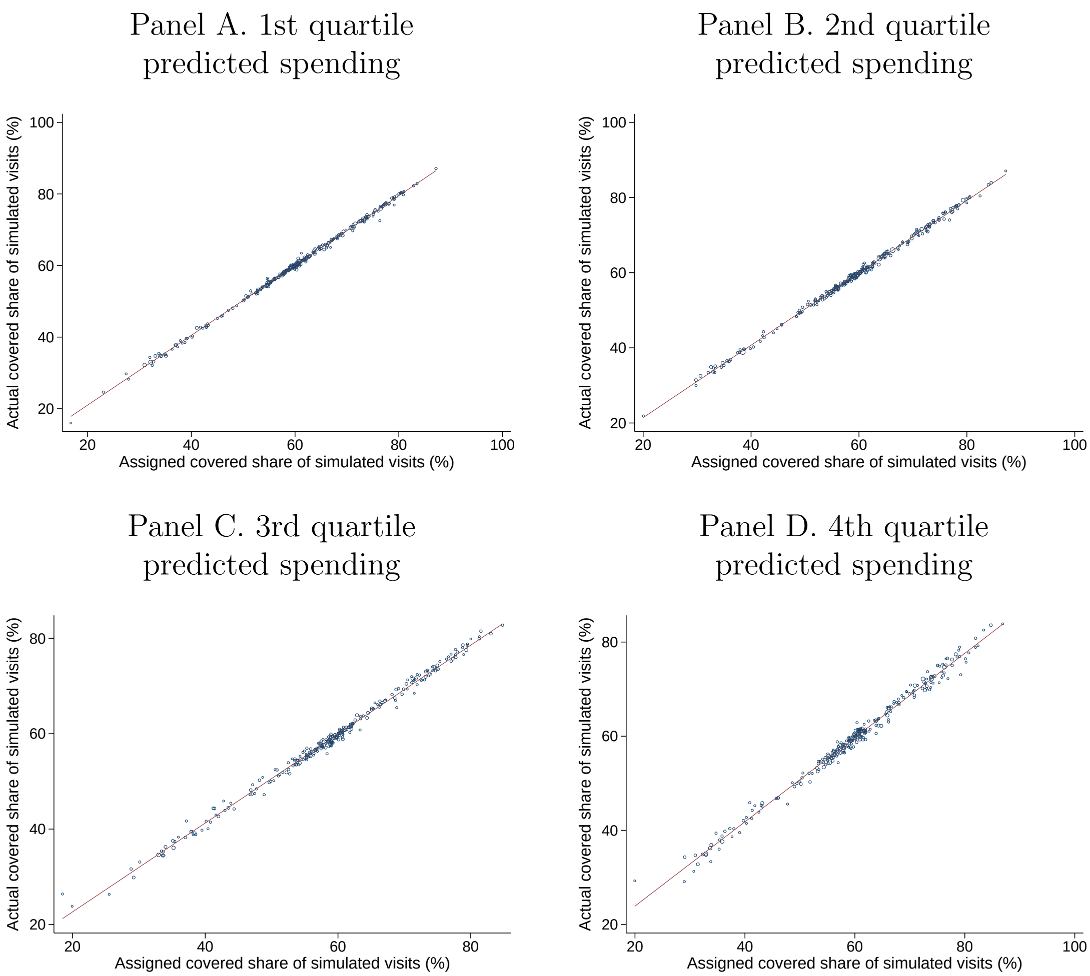
Notes: These figures plot the distribution of overall network breadth when residualizing on combinations of enrollee zip and randomly-assigned plan. Results are based on my primary sample (see Section II for details on primary sample construction). The network breadth measure is the z-score normalized covered share of simulated visits. Panel A plots the raw distribution of network breadth. Panel B presents the distribution of network breadth residualized on enrollee zip. Panel C presents the distribution of network breadth residualized on assigned plan. Panel D presents the distribution of network breadth when residualizing on assigned plan and, separately, enrollee zip. The remaining variation in this panel is what I exploit to estimate the affect of network breadth on consumers. Panel E presents the distribution of network breadth when residualizing on assigned plan-by-enrollee zip. Network breadth is relatively stable over time so little variation remains once I demean at the assigned plan-by-enrollee zip level.

Appendix Figure 8. : Variation by Subgroup in Relationship Between Assigned and Actual Network Breadth



Notes: Results are based on my primary sample (see Section II for details on primary sample construction). The panels plot residualized binned scatterplots of the reduced form impact of the normalized covered share of simulated visits (network breadth) on actual network breadth for different subgroups. The binned scatterplots are constructed by first regressing assigned network breadth and actual network breadth on the set of control variables (i.e. age, gender, race, tenure, baseline outcomes, county × month of assignment), calculating residuals, and grouping the residualized network breadth measure into bins at the plan-zip level. The mean is added back in to ease interpretation (Chetty, Friedman and Rockoff, 2014).

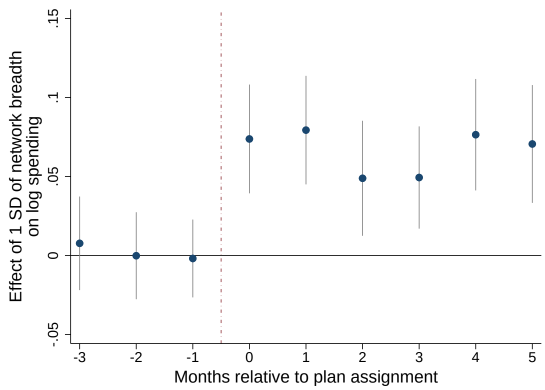
Appendix Figure 9. : Variation by Subgroup in Relationship Between Assigned and Actual Network Breadth



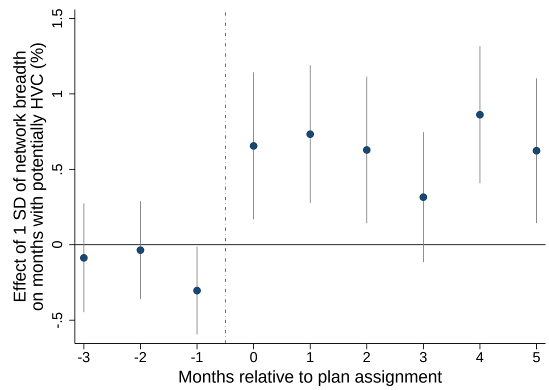
Notes: Results are based on my primary sample (see Section II for details on primary sample construction). The panels plot residualized binned scatterplots of the reduced form impact of the normalized covered share of simulated visits (network breadth) on actual network breadth for different subgroups. The binned scatterplots are constructed by first regressing assigned network breadth and actual network breadth on the set of control variables (i.e. age, gender, race, tenure, baseline outcomes, county × month of assignment), calculating residuals, and grouping the residualized network breadth measure into bins at the plan-zip level. The mean is added back in to ease interpretation (Chetty, Friedman and Rockoff, 2014).

Appendix Figure 10. : Reduced Form Estimates of the Impact of Assigned Network Breadth on Health Care Spending, Health Care Quality, and Consumer Satisfaction by Month

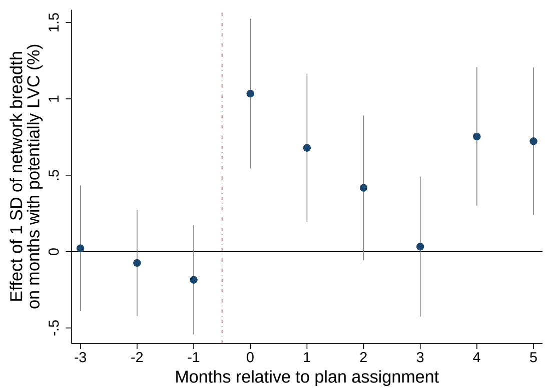
Panel A. Healthcare spending



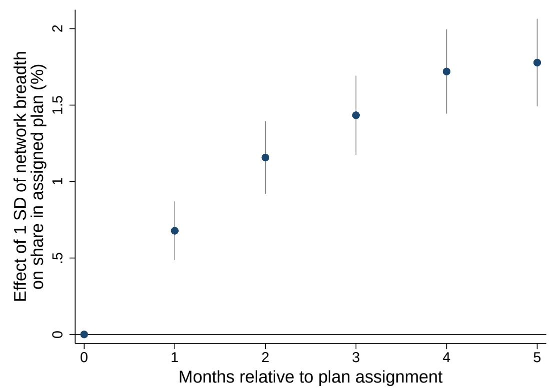
Panel B. Potentially high-value care



Panel C. Potentially low-value care

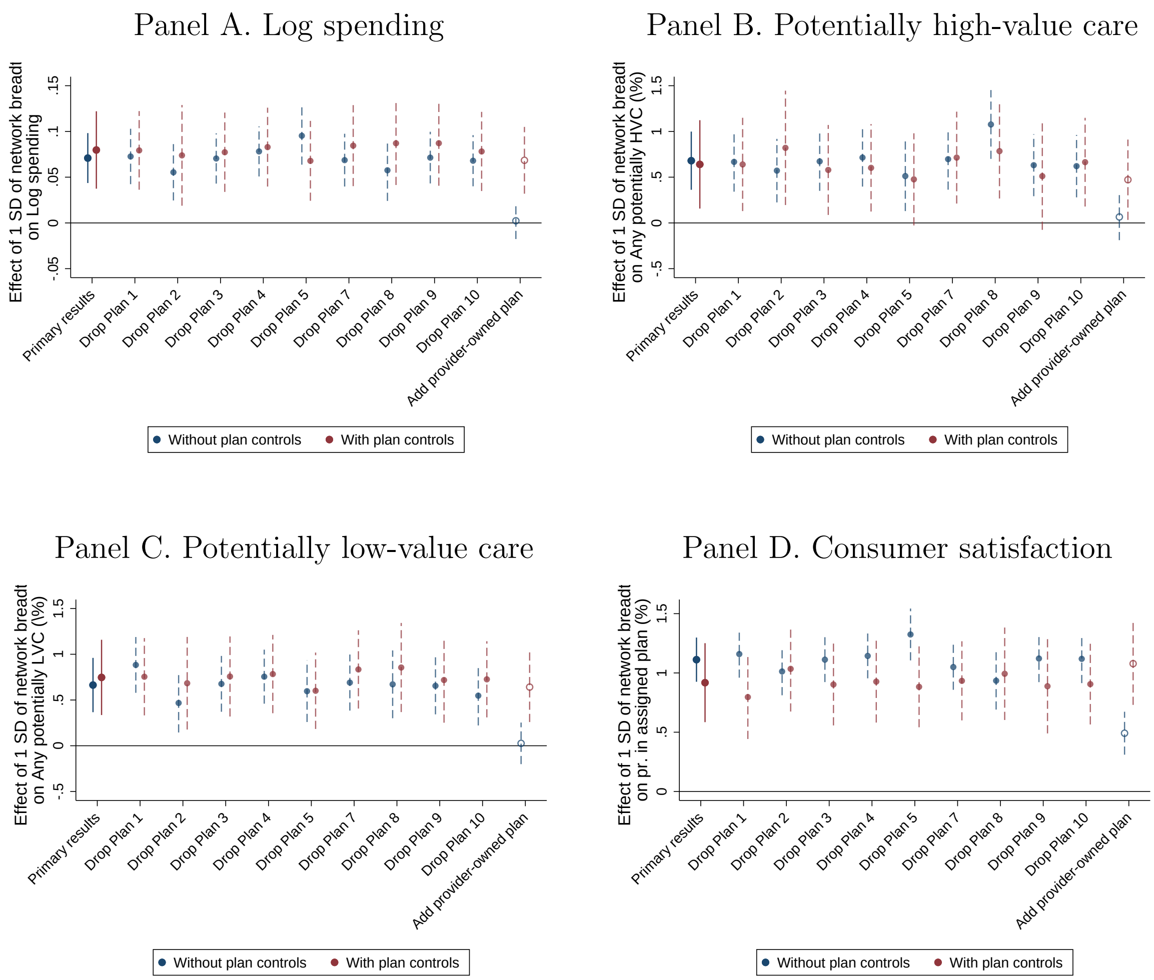


Panel D. Consumer satisfaction



Notes: These figures plot event study estimates of the reduced form impact of normalized covered share of simulated visits (network breadth) on healthcare spending, specific service use, and consumer satisfaction. Results are based on my primary sample (see Section II for details on primary sample construction). The point estimates and 95% confidence intervals are the result of estimating a reduced form version of Equation 5 separately for each month relative to auto assignment, with standard errors clustered at the county × month of assignment level. The vertical dashed red line indicates when auto assignment occurs. There is no baseline measure of satisfaction (in Panel D) since enrollees are in Medicaid fee-for-service prior to assignment. In addition, all enrollees are in their assigned plan for at least the first month following assignment, hence the null point estimate in the first period in Panel D.

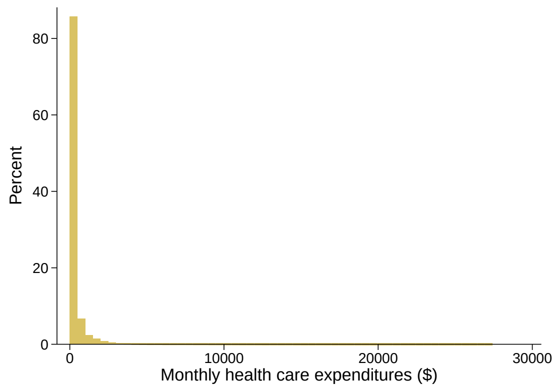
Appendix Figure 11. : Robustness of Primary Estimates to Changing the Compositions of Medicaid Managed Care Plans Included in Analyses



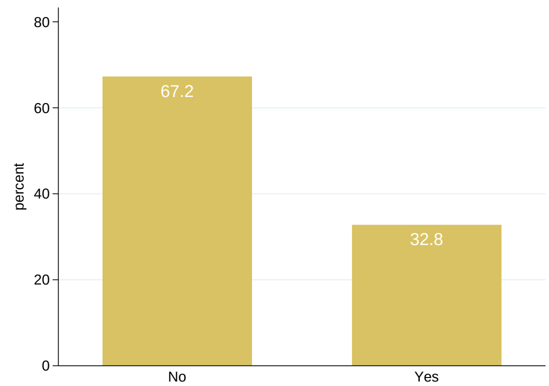
Notes: This figure displays the sensitivity of the results presented in Tables 3 and 4 to the sample of plans included in the estimation. Results are based on my primary sample (see Section II for details on primary sample construction). For reference, I include my "primary results" (from estimating Equation 5 on my primary sample). I then present the sensitivity of my results to sequentially dropping the enrollees in each of the plans in my primary sample. I also assess the sensitivity of my results to adding in the enrollees in the provider-owned plan. For each sensitivity, I estimate a specification with and without plan controls (i.e., fixed effects).

Appendix Figure 12. : Distribution of Monthly Medicaid Managed Care Health Care Spending

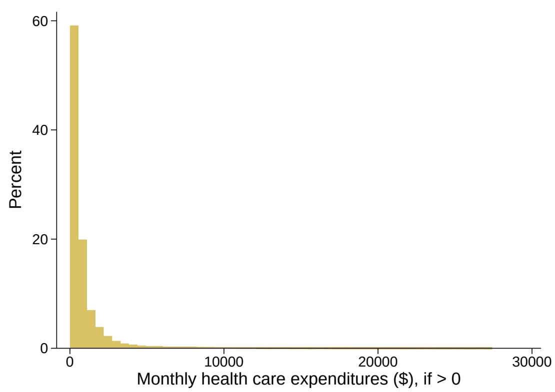
Panel A. Total spending



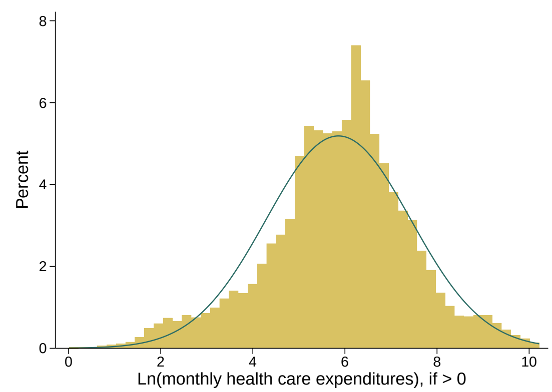
Panel B. Fraction of months with positive spending



Panel C. Total spending | spending > 0



Panel D. Ln(spending) | spending > 0

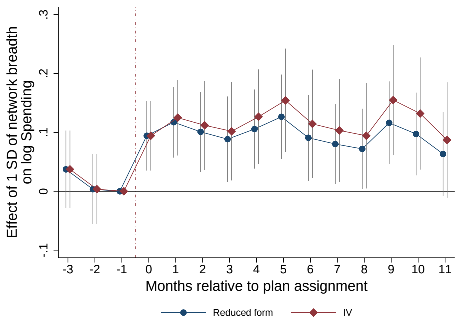


Notes: This figure displays the distribution of healthcare spending by enrollee month in my primary estimation sample. Results are based on my primary sample (see Section II for details on primary sample construction). For all four panels, I exclude observations above the 99.9 percentile (>$19,404).

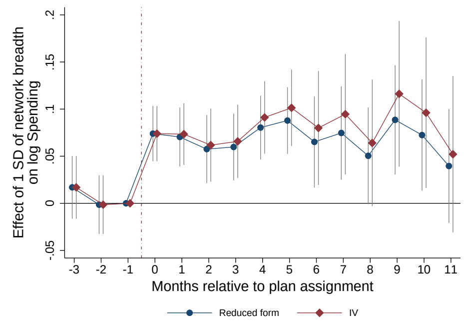
Appendix Figure 13. : Assigned Network Breadth and Health Care Spending

Panel A. Extended to 12 months post assignment

Balanced panel

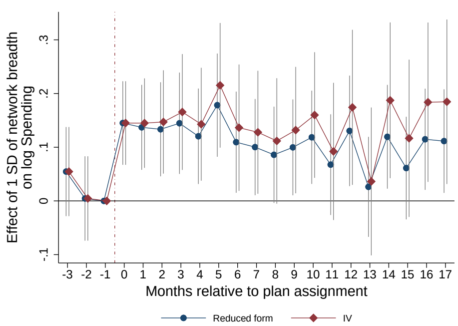


Imbalanced panel

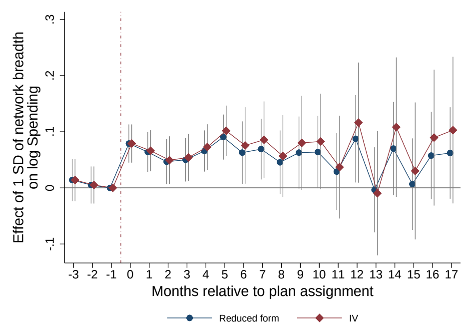


Panel B. Extended to 18 months post assignment

Balanced panel

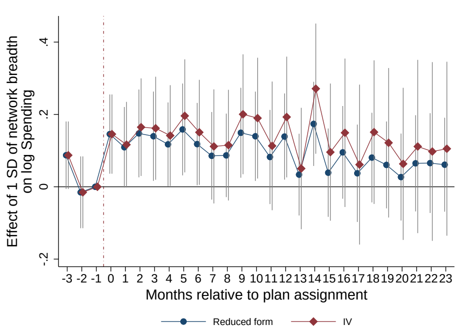


Imbalanced panel

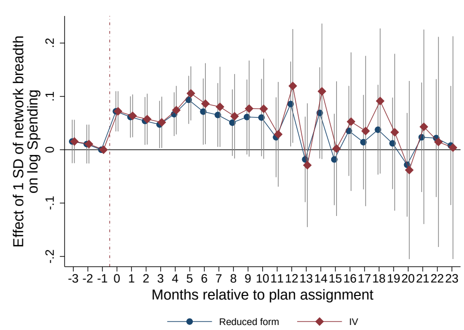


Panel C. Extended to 24 months post assignment

Balanced panel



Imbalanced panel

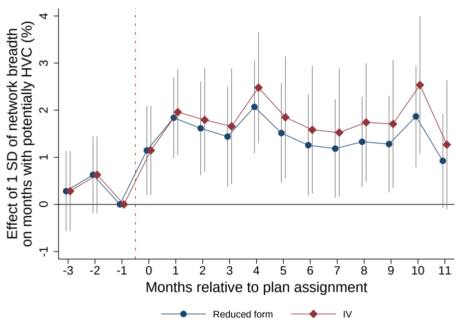


Notes: The panels plot event study estimates of the effect of network breadth on health care spending. Results are based on a secondary sample of enrollees (and enrollee-months) that allow for the estimation of effects beyond the first six months post-assignment. Appendix A describes the construction of these alternative samples. For each extended study period, I present results based on balanced and imbalanced samples of enrollees. I present point estimates along with 95% confidence intervals from estimating both reduced form (in blue) and IV (in red) versions of Equation C4, as described in Appendix C. The baseline (omitted) period is 1 month prior to auto assignment. The dashed vertical red line indicates when auto assignment took place. The y-axis presents the effect of a one standard deviation increase in network breadth on the outcome. All standard errors are clustered at the county × month of assignment level (Chetty, Friedman and Rockoff, 2014).

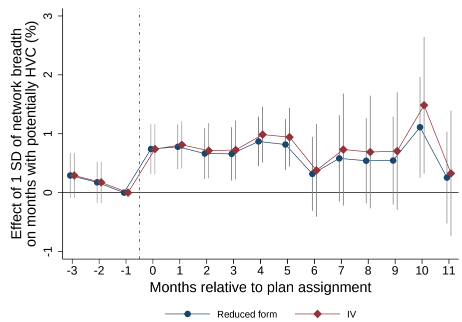
Appendix Figure 14. : Assigned Network Breadth and Potentially High-Value Care

Panel A. Extended to 12 months

Balanced panel

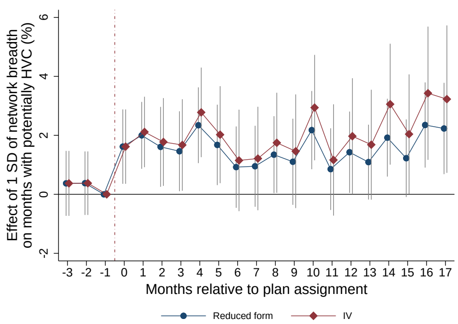


Imbalanced panel

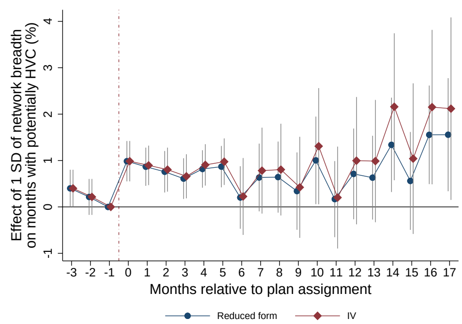


Panel B. Extended to 18 months

Balanced panel

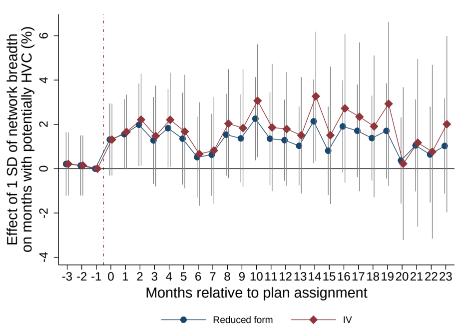


Imbalanced panel

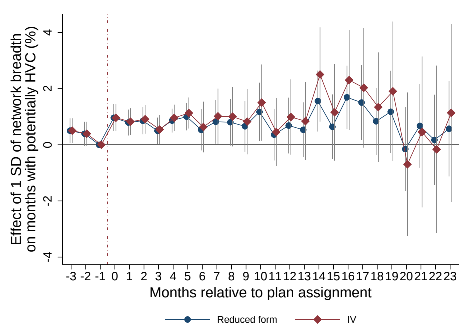


Panel C. Extended to 24 months

Balanced panel



Imbalanced panel

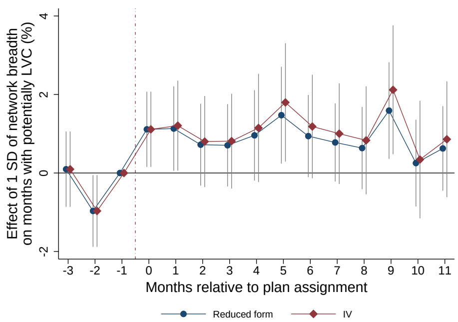


Notes: The panels plot event study estimates of the effect of network breadth on potentially high-value care. Results are based on a secondary sample of enrollees (and enrollee-months) that allow for the estimation of effects beyond the first six months post-assignment. Appendix A describes the construction of these alternative samples. For each extended study period, I present results based on balanced and imbalanced samples of enrollees. I present point estimates along with 95% confidence intervals from estimating both reduced form (in blue) and IV (in red) versions of Equation C4, as described in Appendix C. The baseline (omitted) period is 1 month prior to auto assignment. The dashed vertical red line indicates when auto assignment took place. The y-axis presents the effect of a one standard deviation increase in network breadth on the outcome. All standard errors are clustered at the county × month of assignment level (Chetty, Friedman and Rockoff, 2014).

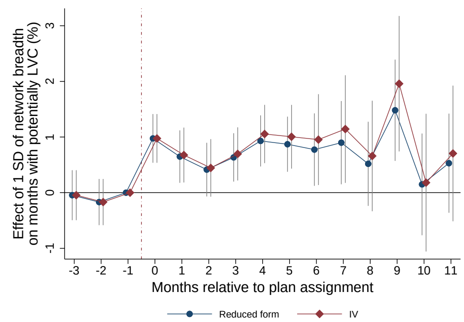
Appendix Figure 15. : Assigned Network Breadth and Potentially Low-Value Care

Panel A. Extended to 12 months

Balanced panel

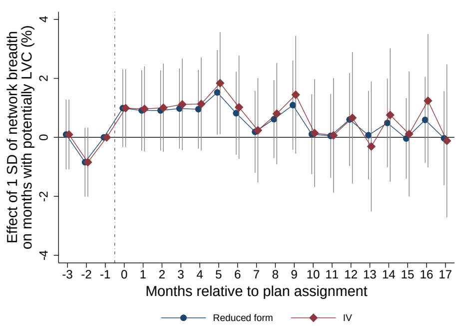


Imbalanced panel

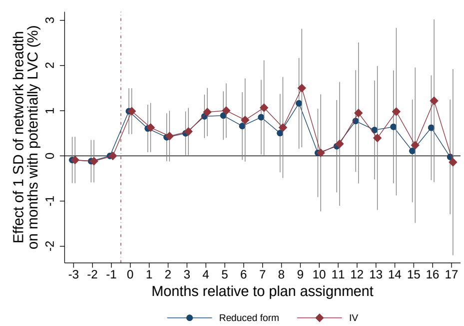


Panel B. Extended to 18 months

Balanced panel

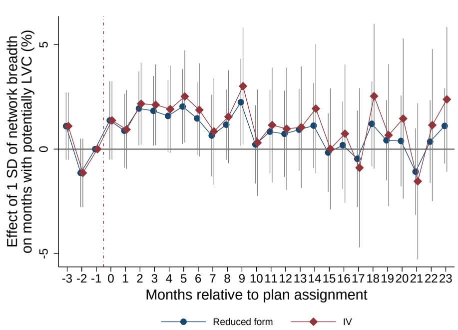


Imbalanced panel

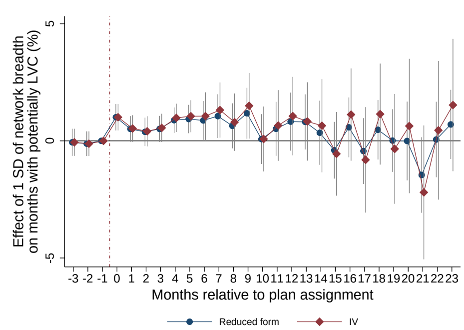


Panel C. Extended to 24 months

Balanced panel

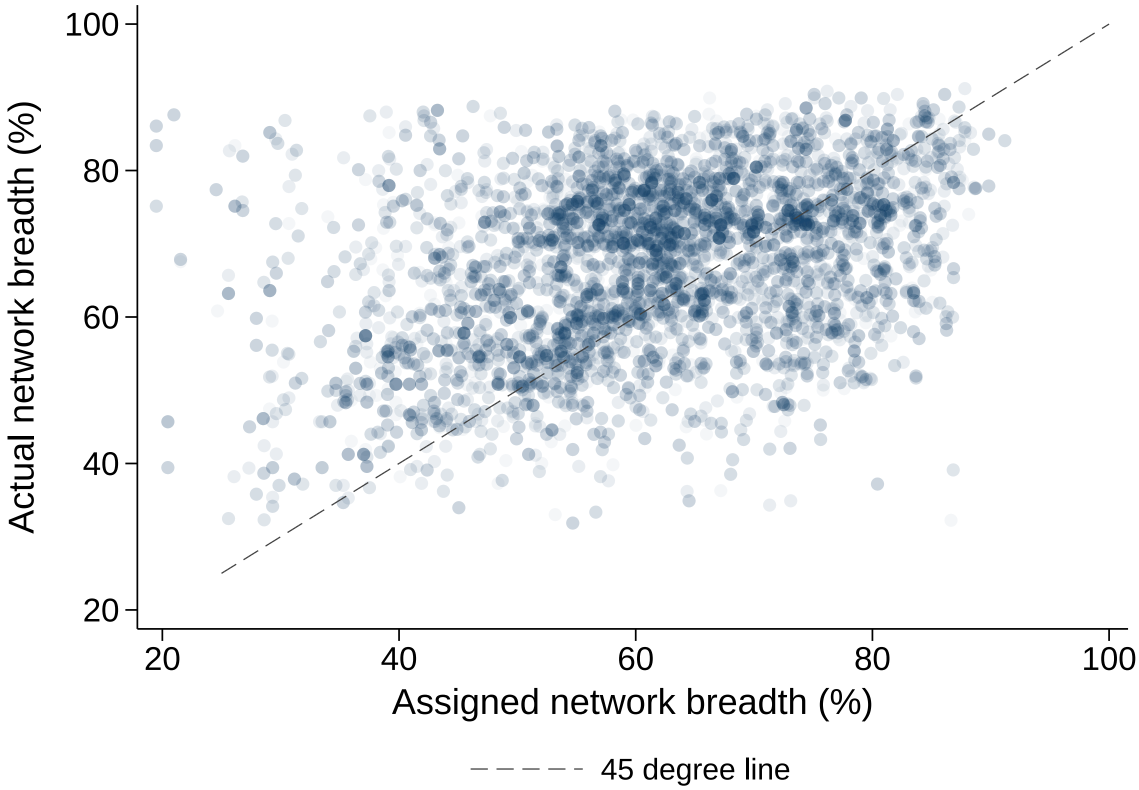


Imbalanced panel



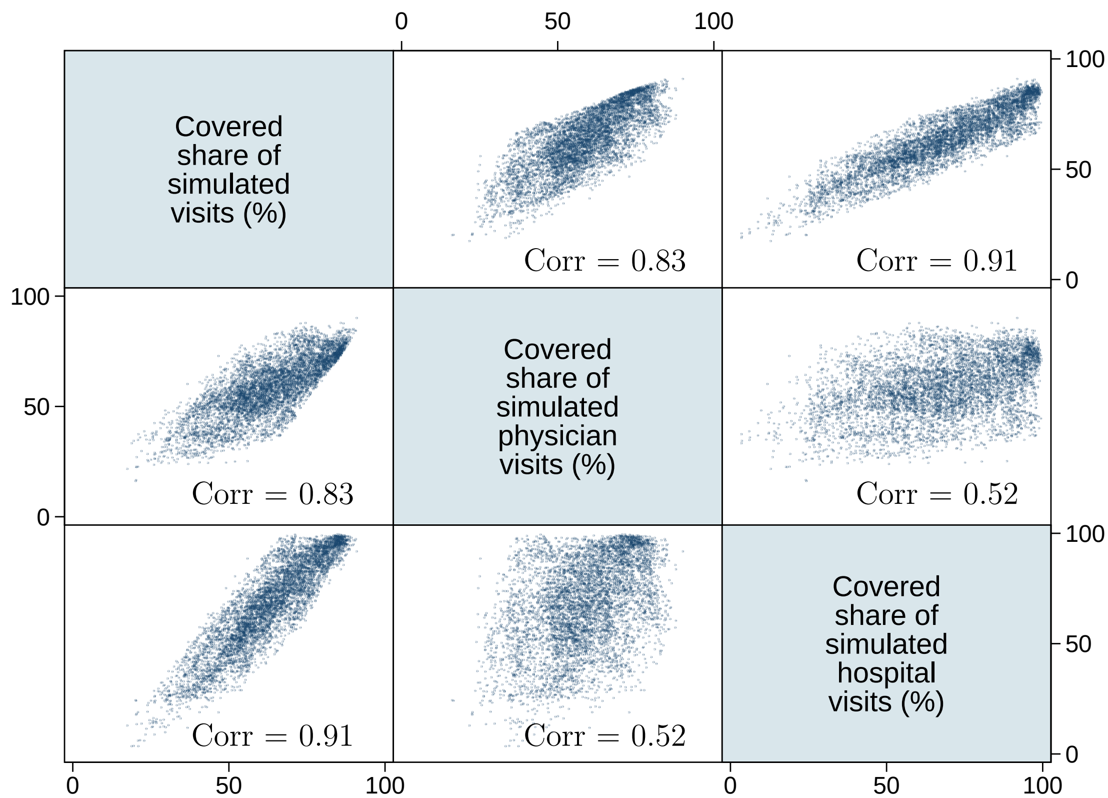
Notes: The panels plot event study estimates of the effect of network breadth on potentially low-value care. Results are based on a secondary sample of enrollees (and enrollee-months) that allow for the estimation of effects beyond the first six months post-assignment. Appendix A describes the construction of these alternative samples. For each extended study period, I present results based on balanced and imbalanced samples of enrollees. I present point estimates along with 95% confidence intervals from estimating both reduced form (in blue) and IV (in red) versions of Equation C4, as described in Appendix C. The baseline (omitted) period is 1 month prior to auto assignment. The dashed vertical red line indicates when auto assignment took place. The y-axis presents the effect of a one standard deviation increase in network breadth on the outcome. All standard errors are clustered at the county × month of assignment level (Chetty, Friedman and Rockoff, 2014).

Appendix Figure 16. : Assigned and Actual Provider Network Breadth for Enrollees that Switch Plans Post-Assignment



Notes: This figure plots enrollees' actual network breadth against their assigned network breadth for months in which the enrollees were *not* in their assigned plans (N=11,333 enrollee months). The dashed line is a 45 degree line. Points above the line indicate that an enrollee's actual network breadth is larger than their assigned network breadth. The cloud of points shifted above the 45 degree line indicates that enrollees who switch plans, tend to switch to plans with broader networks than the breadth of their assigned network.

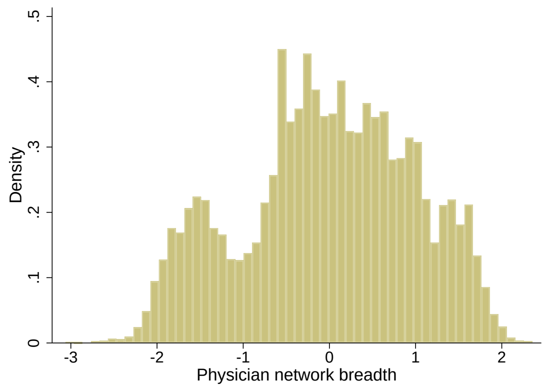
Appendix Figure 17. : Pairwise Correlations Between the Breadth of Overall Networks, Physician Networks, and Hospital Networks



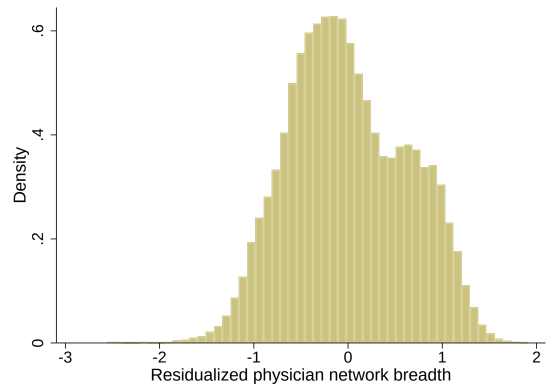
Notes: This figures plots pairwise correlations for measures of overall, physician, and hospital network breadth. Each measure is based on my primary method for constructing network breadth, the covered share of simulated visits (see Section III for a detailed description of how I construct this measure). Overall network breadth is a weighted average of the physician and hospital network breadth measures.

Appendix Figure 18. : Variation in Assigned Physician and Hospital Network Breadth

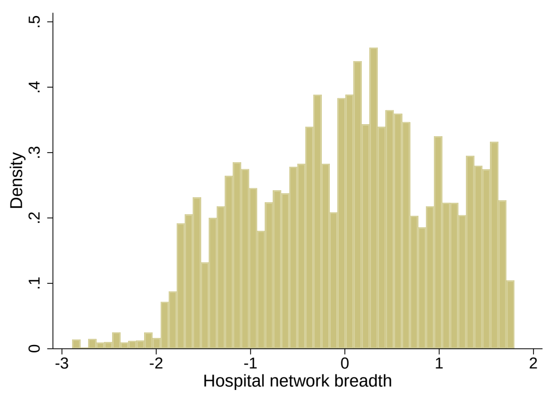
Panel A. Raw physician network



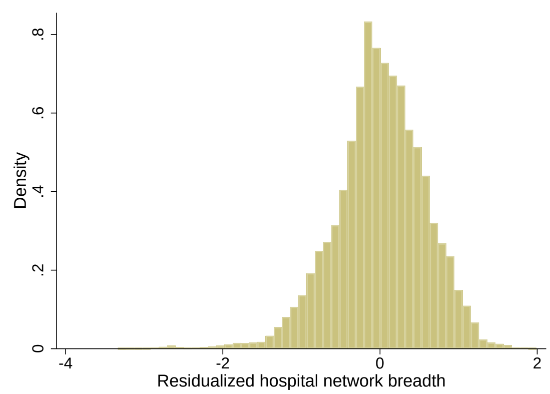
Panel B. Residualized physician network



Panel C. Raw hospital network



Panel D. Residualized hospital network

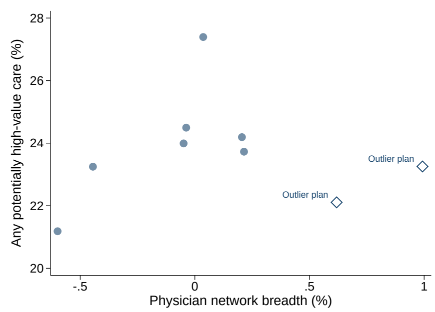


Notes: These figures plot the raw and residualized distributions of assigned physician and hospital network breadth. Results are based on the restricted, "usual source of care" sample (see Section VI for additional details). The physician and hospital network breadth measures are the z-score normalized covered share of simulated visits. Panel A plots the raw distribution of physician network breadth. Panel B presents the distribution of assigned physician network breadth residualized on my baseline controls (including enrollee zip) as well as assigned hospital network breadth and an indicator for whether an enrollee's usual source of care is in their assigned plan. Panel C plots the raw distribution of hospital network breadth. Panel D presents the distribution of assigned hospital network breadth residualized on my baseline controls (including enrollee zip) as well as assigned physician network breadth and an indicator for whether an enrollee's usual source of care is in their assigned plan.

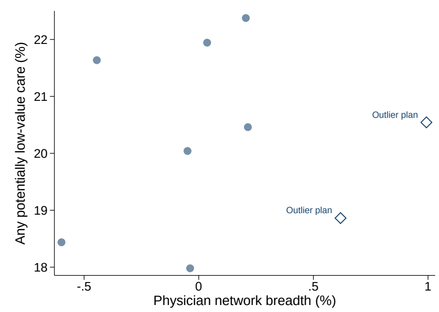
Appendix Figure 19. : Assigned Physician Network Breadth and Potentially High-Value and Low-Value Care

Panel A. Plan-level residualized binned scatterplot

Potentially high-value care

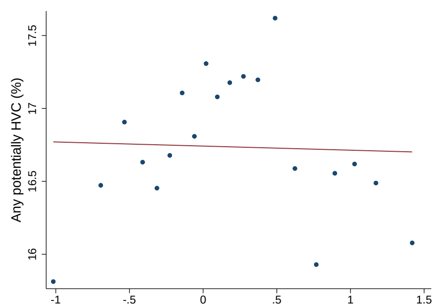


Potentially low-value care

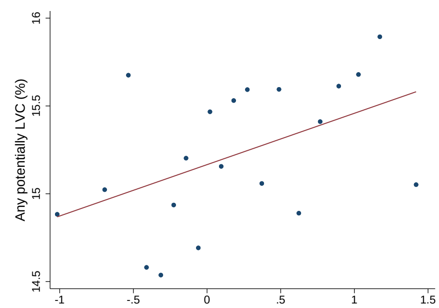


Panel B. Traditional residualized binned scatterplot, no plan controls

Potentially high-value care

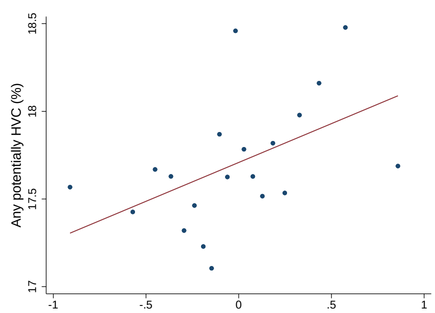


Potentially low-value care

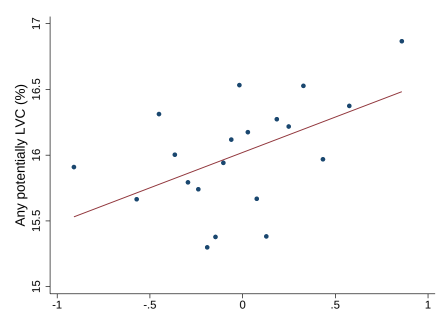


Panel B. Traditional residualized binned scatterplot, plan controls

Potentially high-value care



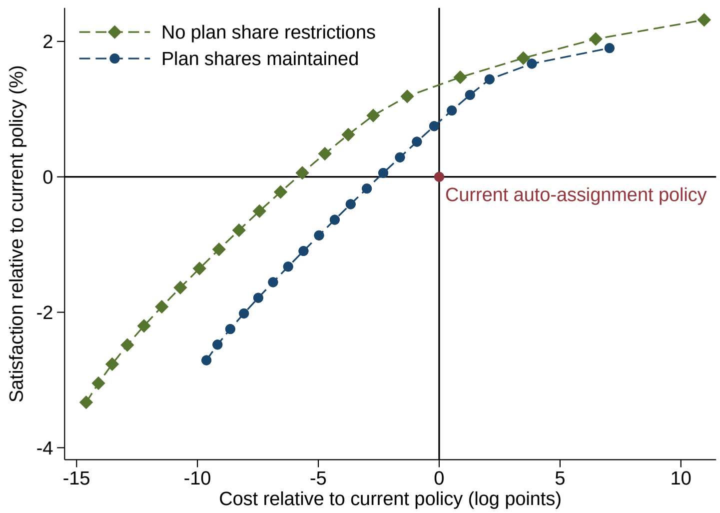
Potentially low-value care



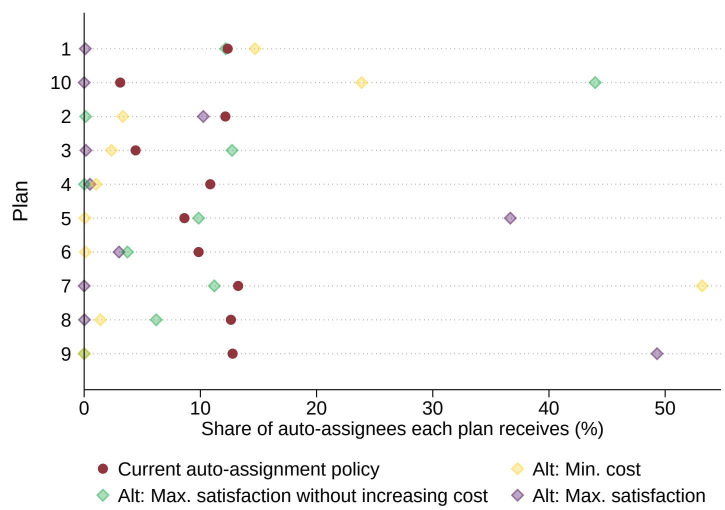
Notes: These figures plot residualized binned scatterplots of the reduced form impact of physician network breadth on the use potentially high-value and low-value care. In Panel A, the binned scatterplots are constructed by first regressing physician network breadth and the outcome variable on the set of control variables (i.e. age, gender, race, tenure, baseline outcomes, county × month of assignment) and hospital network breadth, calculating residuals, and grouping the residualized network breadth measure into bins based on plan of assignment. The mean for each outcome is added back in to ease interpretation. The hollow diamonds mark the two outlier plans and the solid circles correspond to the other seven plans in the data. Panels B and C plot residualized binned scatterplots in which the residualized physician network breadth measures are grouped into 20 equal-sized bins (instead of at the plan-level). Panel B does not include plan of assignment as a control variable. Panel C adds plan of assignment as an additional control variable (in addition to my baseline controls). Standard errors clustered at the county × month of assignment level (Chetty, Friedman and Rockoff, 2014).

Appendix Figure 20. : Impact of Assignment Policies that Do Not Maintain Plan Shares

Panel A. Cost and satisfaction tradeoffs



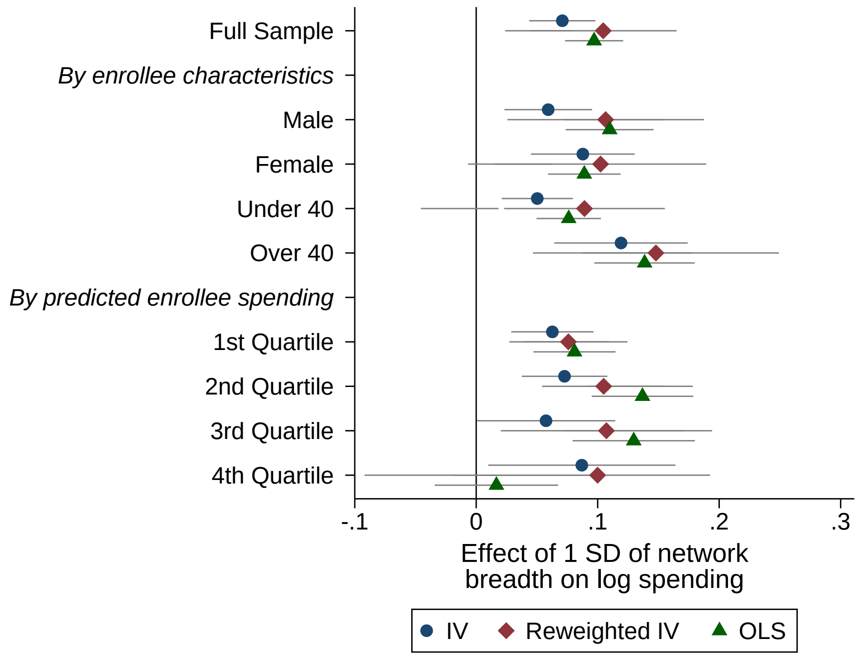
Panel B. Share of auto-assignees plans receives under alternative auto assignment policies



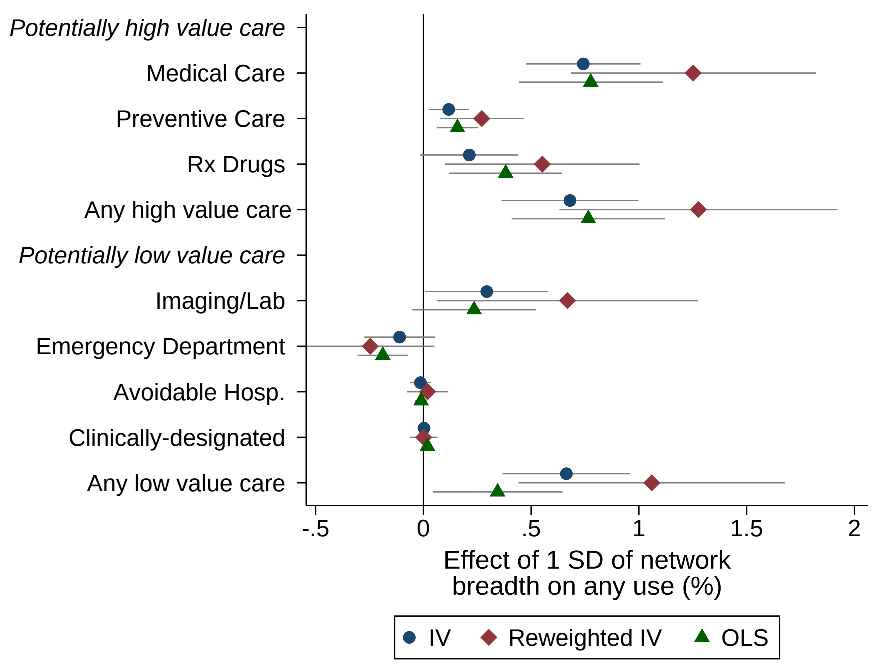
Notes: Panel A plots the mean difference between predicted spending and satisfaction for 21 counterfactual auto-assignment policies relative to the state's current (random) auto-assignment policy. We plot counterfactuals in which each plan's share of auto-assignees is constrained to be the same as under the current auto-assignment policy (in blue) and those in which plan shares are unconstrained (in green). The x-axis measures the mean difference between log spending for each counterfactual policy and the current auto-assignment policy. The y-axis measures the difference between mean enrollee satisfaction (i.e., the share of auto-assignees that remain in their assigned plan) for each counterfactual and the current auto-assignment policy. Panel B plots the share of auto-assignees each plan receives under the current auto-assignment policy and three alternatives in which there is no restriction that plan shares be maintained. Plans are arrayed on the y-axis. Each plan's share of the auto-assignee population under the state's current policy is indicated by a solid red circle. Some plans received fewer auto-assignees (even under the default policy) because they did not qualify to receive auto-assignees in all years of study. Plan shares under alternative assignment policies are indicated by diamonds. With no restriction that plan shares be maintained, the optimal counterfactual assignment often entail very small (or no) allocations to some plans and large allocations to others. Additional details are available in Section VII and the Figure 6 note.

Appendix Figure 21. : Comparison of IV Estimates of the Impact of Network Breadth on Health Care Spending and Health Care Quality to Other Estimates

Panel A. Health Care Spending



Panel B. Health Care Quality



Notes: These figures plot the main instrument variables (IV), Re-weighted IV, and ordinary least squares (OLS) estimates for health care spending and quality from Tables 3–4 and Appendix Tables 26–29. These estimates are based on a specification the includes enrollee controls but does not include plan controls. See the table notes of the respective tables for additional details on the data, samples, and specifications.

Appendix Table 1—: Sample Construction

Sample restrictions	Unique recipients	Fraction of original (%)
Recipients auto-assigned in New York City, 2005–2012	374,710	100.0
Removed children (under 18)	272,889	72.8
Removed Medicare eligibles (65 and over)	192,582	51.4
Removed recipients in MMC plan in spell pre-assignment	187,581	50.1
Removed recipients with a family member in MMC plan	145,169	38.7
Removed recipients in MMC in 12 months pre-assignment	127,424	34.0
Restricted sample to post-April 2008 (MMC policy change)	111,410	29.7
Required 3 months pre- and 6 months post-assignment in MMC	66,164	17.7
Removed recipients with Supplemental Security Income (SSI)	58,178	15.5
Removed recipients with missing data	58,172	15.5

Notes: This table reports the count of unique enrollees in the sample after a sequential set of sample restrictions. Enrollees in Medicaid managed care ("MMC") plans prior to assignment or those who had family members in MMC plans at the time (or prior to) assignment are removed from the sample because their auto-assignments are not random. A "Medicaid spell" refers to a period of continuous eligibility in Medicaid.

Appendix Table 2—: Correlates of Hospital Participation in Medicaid Managed Care Networks

	(1)	(2)	(3)	(4)
Public hospital	-0.0990	-0.112	-0.113	-0.115
	(0.0276)	(0.0262)	(0.0266)	(0.0264)
Hospital beds	0.0143	0.0286	0.0183	0.0262
	(0.0286)	(0.0273)	(0.0269)	(0.0273)
Teaching hospital	-0.0264	-0.0251	-0.0462	-0.0369
	(0.0504)	(0.0470)	(0.0482)	(0.0483)
Median zip code income		-0.0652		-0.0436
		(0.0272)		(0.0342)
Overall hospital rating			-0.0616	-0.0352
			(0.0272)	(0.0339)
Constant	0.772	0.767	0.778	0.772
	(0.0293)	(0.0274)	(0.0276)	(0.0278)
R^2	0.34	0.45	0.44	0.47
F	5.428	6.132	5.888	5.134

Notes: This table reports hospital-level correlates of participation in Medicaid managed care (MMC) networks. The outcome variable is the share of the ten MMC plan networks that a hospital participated in, in 2012. I limit the analysis to general, acute care hospitals that could be matched to the American Hospital Association (AHA) and Medicare Hospital Compare data. An indicator that the hospital is public, a count of hospital beds, and an indicator that the hospital is a teaching hospital are based on AHA survey data. Median zip code income is from the the 2006-2010 5-Year American Community Survey. The overall hospital rating is from the 2020 Medicare Hospital Compare data.

Appendix Table 3—: Hospital Choice Model

	Simple Model		Full Model	
	Coeff.	Std. Error	Coeff.	Std. Error
	(1)	(2)	(3)	(4)
Distance to Hospital				
Distance (Minutes)	−0.417	(0.004)	−0.391	(0.006)
Distance Squared	0.005	(0.000)	0.004	(0.000)
Distance x Pregnancy			−0.035	(0.008)
Distance x Respiratory			−0.131	(0.008)
Distance x Mental Illness			0.057	(0.009)
Distance x Circulatory			−0.023	(0.011)
Distance x Digestive			−0.050	(0.019)
Distance x Injury			−0.000	(0.010)
Out-of-Network Disutility				
Out-of-Network	−1.412	(0.007)		
Out-of-Network x Plan 1			−1.312	(0.021)
Out-of-Network x Plan 2			−0.919	(0.021)
Out-of-Network x Plan 3			−0.677	(0.012)
Out-of-Network x Plan 4			−1.124	(0.019)
Out-of-Network x Plan 5			−0.735	(0.037)
Out-of-Network x Plan 6			−1.184	(0.031)
Out-of-Network x Plan 7			−2.089	(0.012)
Out-of-Network x Plan 8			−1.556	(0.023)
Out-of-Network x Plan 9			−0.824	(0.019)
Out-of-Network x Plan 10			−0.641	(0.032)
Hospital Characteristics				
Hospital Fixed Effects	✓		✓	
Pregnancy x Obstetrics			2.323	(0.029)
Injury x Trauma Center			0.564	(0.018)
Mental Illness x Psych			0.331	(0.023)
Circulatory x Card Surg			0.285	(0.017)
Circulatory x Cath Lab			0.139	(0.016)
Model Statistics				
Pseudo R-Squared (McFadden)	0.401		0.408	
Choice Instances	697,803		697,803	

Notes: This table reports results from the multinomial logit hospital choice model described in Section III. The data used include all hospitalizations for Medicaid managed care enrollees during the period 2008 to 2012. Columns 1 and 2 report the coefficients and standard errors for a simple hospital choice model. Columns 3 and 4 report the coefficients and standard errors for a full hospital choice model which includes interactions of distance with diagnosis, network with plan and hospital characteristics with diagnosis. The full model also includes distance (and distance-squared) interacted with five-year age-by-gender bins (Shepard, 2016).

Appendix Table 4—: Physician Choice Model

	Simple Model		Full Model	
	Coeff.	# Sig.	Coeff.	# Sig.
	(1)	(2)	(3)	(4)
Distance to Hospital				
Distance (Minutes)	−0.207	42	−0.199	42
Distance Squared	0.002	42	0.002	42
Distance x DME			−0.003	20
Distance x Imaging			0.054	42
Distance x Evaluation and Management (E&M)			−0.025	37
Distance x Other			−0.020	31
Distance x Procedures			0.031	42
Distance x Test			0.003	32
Out-of-Network Disutility				
Out-of-Network	−2.788	42	−2.789	42
Physician Characteristics				
Optometry x DME			3.477	41
Radiology x Imaging			3.197	42
Phys. Med. x Procedures			2.576	42
Dermatology x Procedures			1.924	42
Cardiology x Tests			1.454	42
OB/GYN x Tests			1.428	42
Urology x Tests			1.322	42
Pathology x Tests			1.281	42
Allergy x E&M			0.919	42
Primary Care x E&M			0.733	42
Ophthalmology x E&M			0.732	42
Neighborhoods	42		42	
Choice Instances	Various		Various	
Average Pseudo R-Squared	0.766		0.792	

Notes: This table reports results from the multinomial logit physician choice model described in Appendix Section B. The data used include all physician office visits for Medicaid managed care enrollees during the period 2008 to 2012. The model is estimated separately for forty-two neighborhoods (defined by zip) in New York City. For each neighborhood, I estimate fixed effects for the largest five percent of practices serving the enrollees of that neighborhood. Since patients often receive multiple services in a single physician visit, s is a vector of indicator variables that identifies whether a visit contained the following services classified by BETOS codes: evaluation and management, procedures, imaging, tests, durable medical equipment, other, or unclassified. Columns 1 and 2 report the coefficients and standard errors for a simple physician choice model. Columns 3 and 4 report the coefficients and standard errors for a full physician choice model which includes interactions of distance with procedure type, and physician specialty with procedure type. The full model includes distance (and distance-squared) interacted with five-year age x gender bins (Shepard, 2016).

Appendix Table 5—: First Stage Estimates of the Impact of Assigned Network Breadth on Actual Network Breath

	Actual network breadth		
	(1)	(2)	(3)
Assigned network breadth	0.951	0.950	0.932
	(0.003)	(0.003)	(0.004)
Assigned to Plan 2			0.021
			(0.007)
Assigned to Plan 3			0.021
			(0.005)
Assigned to Plan 4			−0.000
			(0.004)
Assigned to Plan 5			0.035
			(0.007)
Assigned to Plan 6			0.001
			(0.006)
Assigned to Plan 7			0.005
			(0.004)
Assigned to Plan 8			0.028
			(0.005)
Assigned to Plan 9			0.016
			(0.005)
Assigned to Plan 10			0.007
			(0.009)
F-Statistic (Excluded Instruments)	1,955,384	1,955,441	80,686
Observations	295,728	295,728	349,044
Baseline Controls	X	X	X
Enrollee Controls		X	X
Plan Controls			X

Notes: This table reports first stage results. Results are based on my primary sample (see Section II for details on primary sample construction). The independent variables are assigned network breadth and indicators for assignment to each of the managed care plans in my sample (with one leave-out plan). The dependent variable is the enrollees actual network breadth, which is determined by the plan they are enrolleed in and the zip code they reside in for each month after assignment. Column 2 adds in enrollee-level controls. Standard errors are clustered on the county × month of assignment.

Appendix Table 6—: Balance Test for Enrollees that Made Active Plan Choices

	Mean (SD)	Multivariate OLS			Bivariate OLS
	(1)	(2)	(3)	(4)	(5)
Age	35.515	−0.0081	0.0031	0.0027	−0.0020
	(12.239)	(0.0041)	(0.0083)	(0.0039)	(0.0043)
Male	0.417	0.0231	−0.0331	−0.0136	0.0362
	(0.493)	(0.0042)	(0.0106)	(0.0051)	(0.0047)
Black	0.411	0.0015	−0.0056	−0.0060	0.0018
	(0.492)	(0.0051)	(0.0065)	(0.0029)	(0.0045)
Outpatient spending	127.555	−0.0573	0.0211	0.0057	−0.0717
	(260.232)	(0.0064)	(0.0097)	(0.0042)	(0.0057)
Inpatient spending	250.224	−0.0084	−0.0014	−0.0075	−0.0170
	(1458.834)	(0.0045)	(0.0056)	(0.0024)	(0.0047)
Pharmacy spending	59.054	−0.0086	0.0305	−0.0020	−0.0134
	(317.467)	(0.0052)	(0.0069)	(0.0025)	(0.0056)
Other spending	95.587	−0.0118	0.0439	0.0013	−0.0189
	(324.712)	(0.0049)	(0.0084)	(0.0039)	(0.0046)
Any high-value medical care (%)	24.004	−0.0327	−0.0280	0.0156	−0.0512
	(42.711)	(0.0054)	(0.0089)	(0.0042)	(0.0056)
Any recommended preventive care (%)	7.460	−0.0239	−0.0333	0.0010	−0.0401
	(26.274)	(0.0039)	(0.0049)	(0.0024)	(0.0040)
Any high-value prescription drugs (%)	13.468	0.0245	−0.0518	0.0110	0.0021
	(34.138)	(0.0050)	(0.0256)	(0.0123)	(0.0047)
Any lab or imaging (%)	26.645	0.0277	0.1457	0.0194	−0.0167
	(44.210)	(0.0052)	(0.0153)	(0.0069)	(0.0050)
Any emergency department use (%)	15.477	−0.0016	0.0276	0.0015	−0.0221
	(36.169)	(0.0044)	(0.0061)	(0.0028)	(0.0047)
Any avoidable hospitalizations (%)	0.930	0.0100	0.0268	0.0027	0.0053
	(9.600)	(0.0044)	(0.0048)	(0.0022)	(0.0047)
Any designated low-value care (%)	0.595	0.0110	0.0051	0.0025	0.0079
	(7.694)	(0.0038)	(0.0039)	(0.0016)	(0.0044)
Predicted spending	595.989		−0.0673	0.0043	−0.0697
	(604.939)		(0.0070)	(0.0028)	(0.0048)
Predicted any potentially HVC (%)	34.659		0.1709	−0.0158	−0.0451
	(18.302)		(0.0512)	(0.0242)	(0.0046)
Predicted any potentially LVC (%)	26.303		−0.2962	−0.0202	−0.0618
	(11.466)		(0.0459)	(0.0205)	(0.0050)
P-value on joint F-test		0.00	0.00	0.00	
Observations	58,170	58,170	58,170	58,170	58,170
Baseline Controls		X	X	X	X
Plan Controls				X	

Notes: This table reports reduced form results testing the conditional random assignment of enrollees to provider networks and health plans. Results are based on an alternative sample of enrollees that made active plan choices (see Appendix A for details on sample construction). Baseline outcomes are the average for each enrollee in the three months prior to a plan choice. Predicted spending, high-value care (HVC), and low-value care (LVC) are formed using the other baseline variables. Detailed descriptions of the outcome measures are included in Appendix A. Columns 2-4 present the results of multivariate OLS models with enrollee characteristics as the independent variables and the network breadth of the chosen plan as the dependent variable. Column 5 presents bivariate OLS regressions with enrollee characteristics as the independent variable and the network breadth of the chosen plan as the dependent variable. Standard errors are clustered at the county × month of assignment level.

Appendix Table 7—: Comparison of Auto-Assignee and Active Chooser Baseline Characteristics

	Auto assignees			Active choosers		
	Mean	Std. Dev.	Observations	Mean	Std. Dev.	Observations
	(1)	(2)	(3)	(4)	(5)	(6)
Demographics						
Age	35.280	12.281	174,522	35.348	12.239	287,664
Male	0.594	0.491	174,522	0.417	0.493	287,664
Black	0.518	0.500	174,522	0.411	0.492	287,664
Healthcare spending						
Total spending	535.932	3,241.627	174,522	532.419	2,533.002	287,664
Outpatient spending	94.832	311.314	174,522	127.555	328.070	287,664
Inpatient spending	249.180	3,044.629	174,522	250.224	2,332.355	287,664
Pharmacy spending	65.266	403.640	174,522	59.054	362.426	287,664
Other spending	126.654	481.871	174,522	95.587	420.441	287,664
Healthcare use						
Any spending (%)	37.446	48.399	174,522	42.781	49.476	287,664
Any outpatient spending (%)	22.045	41.455	174,522	30.801	46.167	287,664
Any inpatient spending (%)	2.423	15.377	174,522	2.593	15.893	287,664
Any pharmacy spending (%)	19.100	39.309	174,522	18.627	38.933	287,664
Any other spending (%)	25.813	43.761	174,522	25.013	43.309	287,664
Potentially high-value care						
Any high-value medical care (%)	8.743	28.247	174,522	13.144	33.788	287,664
Any recommended preventive care (%)	1.610	12.584	174,522	2.660	16.091	287,664
Any high-value prescription drugs (%)	9.193	28.893	174,522	7.919	27.004	287,664
Any potentially high-value care (%)	15.637	36.321	174,522	19.588	39.688	287,664
Potentially low-value care						
Any lab or imaging (%)	12.141	32.661	174,522	12.803	33.412	287,664
Any emergency department use (%)	5.964	23.681	174,522	6.147	24.019	287,664
Any avoidable hospitalization (%)	0.608	7.773	174,522	0.564	7.486	287,664
Any designated low-value care (%)	0.197	4.429	174,522	0.203	4.505	287,664
Any potentially low-value care (%)	15.785	36.460	174,522	16.794	37.381	287,664

Notes: This table reports summary statistics. The auto assignee results are based on my primary sample (see Section II) and the "active chooser" results are based on an alternative sample of enrollees that made active plan choices (see Appendix A). Observations are at the enrollee-month level and restricted to the six months post-assignment (or post-plan choice). The service categories used to stratify healthcare use and spending were provided by the New York State Department of Health (NYSDOH). Additional details on the specific services identified as potentially high-value or low-value care are described in Appendix A.

Appendix Table 8—: 2SLS Overall Network Breadth Results by Network Measure

	Mean of Dep. Var	Network breadth: Sim. visit shares	Network breadth: Visit shares	Network breadth: Network utility
	(1)	(2)	(3)	(4)
Panel A. Healthcare spending				
Log spending	1.835	0.071	0.071	0.067
		(0.014)	(0.012)	(0.013)
Any spending (%)	31.451	1.002	0.993	0.948
		(0.201)	(0.174)	(0.199)
Panel B. Potentially high-value care				
Any high-value medical care (%)	10.983	0.742	0.808	0.688
		(0.135)	(0.128)	(0.132)
Any recommended preventive care (%)	2.042	0.118	0.129	0.131
		(0.047)	(0.040)	(0.044)
Any high-value prescription drugs (%)	8.705	0.213	0.218	0.216
		(0.115)	(0.102)	(0.112)
Any potentially high-value care (%)	16.736	0.680	0.710	0.634
		(0.161)	(0.144)	(0.157)
Panel C. Potentially low-value care				
Any imaging and lab (%)	12.874	0.294	0.194	0.256
		(0.145)	(0.127)	(0.140)
Any emergency department use (%)	5.129	−0.110	−0.089	−0.124
		(0.083)	(0.072)	(0.080)
Any avoidable hospitalizations (%)	0.390	−0.014	−0.007	−0.009
		(0.025)	(0.020)	(0.024)
Any designated low-value care (%)	0.203	0.003	0.006	0.011
		(0.015)	(0.012)	(0.014)
Any potentially low-value care (%)	15.223	0.664	0.496	0.613
		(0.151)	(0.132)	(0.147)
Panel D. Consumer satisfaction				
In assigned plan (%)	96.153	1.112	1.182	1.096
		(0.095)	(0.094)	(0.094)
Baseline Controls		X	X	X
Enrollee Controls		X	X	X
Plan Controls				

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II for details on primary sample construction). The independent variable is overall network breadth as measured using three different methods (Columns 2-4). Section III and Appendix B describe the construction of the different network measures. The dependent variables include healthcare spending, specific high-value and low-value services, and an ex-post demand measure of enrollee satisfaction. Panel D presents reduced form, rather than 2SLS, estimates as they measure the likelihood that enrollees remain in their assigned plans. All standard errors are clustered at the county × month of assignment level.

Appendix Table 9—: 2SLS Overall Network Breadth Results with Control for Provider-Owned Plan

	Share of sample (1)	Sample Mean (2)	2SLS (3)	2SLS (4)	2SLS (4)
Panel A. Total healthcare use and spending					
Any spending (%)	1.00	32.778	0.745	0.833	1.006
			(0.225)	(0.187)	(0.258)
Log spending	1.00	397.365	0.045	0.054	0.070
Observations		349,044	349,044	349,044	349,044
Panel B. Spending by enrollee characteristics					
			(0.015)	(0.013)	(0.019)
Male	0.59	436.588	0.028	0.047	0.077
			(0.020)	(0.017)	(0.022)
Female	0.41	340.074	0.067	0.063	0.054
			(0.022)	(0.021)	(0.028)
18-39	0.64	279.446	0.035	0.038	0.051
			(0.015)	(0.015)	(0.021)
40-64	0.36	606.558	0.070	0.088	0.104
			(0.029)	(0.026)	(0.037)
Panel C. Spending by predicted enrollee health status					
1st quartile predicted spending	0.24	94.210	0.057	0.056	0.039
			(0.016)	(0.016)	(0.023)
2nd quartile predicted spending	0.25	138.769	0.059	0.061	0.057
			(0.018)	(0.017)	(0.026)
3rd quartile predicted spending	0.25	279.457	0.055	0.054	0.068
			(0.028)	(0.027)	(0.040)
4th quartile predicted spending	0.26	1,050.374	0.019	0.044	0.085
			(0.039)	(0.038)	(0.051)
Baseline Controls			X	X	X
Recipient Controls				X	X
Provider-Owned Plan Control			X	X	
Plan Controls					X

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II for details on primary sample construction). The independent variable is overall network breadth as measured by the normalized covered share of simulated visits. The dependent variable is log spending for Panels B and C. Columns 3 and 4 report the main two-stage least squares (2SLS) results from estimating Equation 5 for overall networks breadth with and without enrollee-level controls, with an dummy variable set to ones for enrollees assigned to the provider-owned plan. Column 5 reports 2SLS results based on a model with plan fixed effects (see Appendix C). Each model uses the broader sample that includes enrollees in the provider-owned plan. All standard errors are clustered at the county × month of assignment level.

Appendix Table 10—: Robustness of Estimates of the Impact of Overall Network Breadth on Health Care Spending to Alternative Specifications

	Mean Spending (1)	Log spending (2)	Inverse HS Spending (3)	Winsorized Spending (4)
Panel A. Spending by enrollee characteristics				
Full sample	371.916	0.081	0.089	9.594
		(0.022)	(0.024)	(3.695)
Male	406.086	0.097	0.107	14.003
		(0.026)	(0.029)	(5.049)
Female	321.607	0.051	0.058	2.113
		(0.032)	(0.036)	(5.068)
18-39	263.953	0.057	0.064	7.208
		(0.023)	(0.026)	(3.658)
40-64	569.574	0.129	0.142	15.361
		(0.044)	(0.048)	(8.390)
Panel B. Spending by enrollee health status				
1st quartile predicted spending	91.316	0.055	0.061	5.051
		(0.026)	(0.029)	(3.013)
2nd quartile predicted spending	140.561	0.059	0.066	5.438
		(0.029)	(0.032)	(3.963)
3rd quartile predicted spending	274.612	0.056	0.063	5.507
		(0.045)	(0.050)	(6.543)
4th quartile predicted spending	1,011.964	0.131	0.145	21.119
		(0.058)	(0.063)	(13.962)
Enrollee Controls	—	No	No	No

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II for details on primary sample construction). Because spending is a highly-skewed, limited dependent variable I also assess the robustness of my results to alternative transformations of the dependent variable, including inverse hyperbolic sine and winsorized levels. Appendix Figure 12 describes the distribution of monthly Medicaid managed care expenditures in my sample. The independent variable is overall network breadth (see Section III). The dependent variables include different transformations of healthcare spending: log spending (my preferred specification); the inverse hyperbolic sine of spending; and winsorized spending. All standard errors are clustered at the county $\times$ month of assignment level.

Appendix Table 11—: Estimates of the Impact of Overall Network Breadth on Health Care Use and Spending

	Sample Mean (1)	2SLS (2)	2SLS (3)	2SLS (4)
Panel A. Log spending by components of care				
Inpatient	168.972	0.002	0.001	−0.006
		(0.004)	(0.004)	(0.006)
Outpatient	69.962	0.057	0.050	0.030
		(0.009)	(0.009)	(0.012)
Prescription drugs	60.632	0.027	0.023	0.024
		(0.010)	(0.007)	(0.010)
Other	97.799	0.025	0.026	0.051
		(0.013)	(0.010)	(0.012)
Panel B. Healthcare spending, quantity, and prices				
Log spending	1.925	0.071	0.071	0.070
		(0.015)	(0.014)	(0.019)
Any spending (%)	32.778	1.122	1.002	1.006
		(0.235)	(0.201)	(0.258)
Quantity of services	2.176	0.070	0.048	0.069
		(0.027)	(0.019)	(0.026)
Price-standardized log spending	1.906	0.062	0.062	0.066
		(0.015)	(0.014)	(0.018)
Log spending conditional on any	1,212.287	0.018	0.025	0.018
		(0.015)	(0.013)	(0.016)
Baseline Controls		X	X	X
Enrollee Controls			X	X
Plan Controls				X

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II for details on primary sample construction). The independent variable is overall network breadth as measured by the normalized covered share of simulated visits. Panel A presents IV estimates of the effects of network breadth on log spending by components of care. Panel B presents IV estimates of the effects of network breadth on overall measures of healthcare spending and quantity. For the "log spending conditional on any" row, I first limit to months with positive spending and then estimate a regression with log spending in those months as the dependent variable. Columns 2 and 3 report the main two-stage least squares (2SLS) results from estimating equation (5) for overall networks breadth with and without enrollee-level controls. Column 4 reports 2SLS results based on a model with plan fixed effects (see Appendix C) estimated on a broader sample that includes enrollees in the provider-owned plan. All standard errors are clustered at the county $\times$ month of assignment level.

Appendix Table 12—: Estimates of the Impact of Overall Network Breadth on Prices Paid to Providers

	Sample Mean (1)	2SLS (2)	2SLS (3)	2SLS (4)
Unit Price	79.592	−0.577	−0.517	0.083
		(0.824)	(0.817)	(1.292)
Winsorized unit price	54.393	−0.589	−0.573	1.084
		(0.320)	(0.318)	(0.493)
Log unit price	3.164	0.013	0.013	0.031
		(0.004)	(0.004)	(0.007)
Observations		416,204	416,204	416,204
Baseline Controls		X	X	X
Enrollee Controls			X	X
Plan Controls				X

Notes: Standard errors in parentheses. Results are based on claims-level analyses restricted to services used by enrollees in my primary sample (see Section II for details on primary sample construction). Unit prices are the amounts paid by Medicaid managed care plans to providers. Columns 2 and 3 report the main two-stage least squares (2SLS) results from estimating equation (5) for overall networks breadth with and without enrollee-level controls. Column 4 reports 2SLS results based on a model with plan fixed effects (see Appendix C) estimated on a broader sample that includes enrollees in the provider-owned plan. All standard errors are clustered at the county × month of assignment level.

Appendix Table 13—: Estimates of the Impact of Network Breadth on Potentially High-Value Care

	Any use (%)			
	Mean	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
Any potentially high-value care (%)	17.697	0.788	0.680	0.479
		(0.186)	(0.161)	(0.224)
Panel A. Potentially high-value medical care				
Any primary care visits (%)	9.204	0.557	0.555	0.200
		(0.117)	(0.112)	(0.162)
Any mental health visits (%)	2.133	0.277	0.262	0.147
		(0.067)	(0.059)	(0.085)
Any physical therapy visits (%)	1.092	−0.048	−0.087	−0.004
		(0.040)	(0.038)	(0.066)
Any pre- or post-natal care visits (%)	0.771	0.064	0.073	0.072
		(0.046)	(0.044)	(0.054)
Any high-value medical care (%)	11.729	0.757	0.742	0.485
		(0.147)	(0.135)	(0.175)
Panel B. Recommended preventive care				
Any hbA1c test (%)	0.439	0.048	0.038	0.005
		(0.021)	(0.021)	(0.031)
Any chlamydia screening in women (%)	0.707	0.018	0.029	0.042
		(0.029)	(0.026)	(0.038)
Any breast cancer screening (%)	0.120	0.013	0.013	0.010
		(0.010)	(0.009)	(0.013)
Any cervical cancer screening (%)	0.740	0.070	0.075	0.056
		(0.025)	(0.024)	(0.036)
Any flu vaccinations (%)	0.434	−0.024	−0.023	−0.014
		(0.019)	(0.019)	(0.027)
Any preventive care (%)	2.137	0.109	0.118	0.112
		(0.049)	(0.047)	(0.070)
Panel C. Potentially high-value prescription drugs				
Any diabetes drugs (%)	1.777	0.067	0.028	−0.076
		(0.070)	(0.044)	(0.074)
Any statins (%)	1.917	0.106	0.040	−0.067
		(0.076)	(0.058)	(0.085)
Any anti-depressants (%)	2.702	0.094	0.114	0.116
		(0.080)	(0.064)	(0.093)
Any anti-psychotics (%)	2.615	0.025	−0.023	−0.067
		(0.080)	(0.065)	(0.090)
Any anti-hypertension drugs (%)	2.668	0.159	0.039	0.021
		(0.091)	(0.070)	(0.095)
Any anti-stroke drugs (%)	0.185	0.018	0.008	0.026
		(0.026)	(0.021)	(0.025)
Any asthma drugs (%)	1.436	0.091	0.077	0.030
		(0.061)	(0.049)	(0.064)
Any contraceptives (%)	0.905	0.008	0.016	0.000
		(0.049)	(0.043)	(0.056)
Any potentially high-value drugs (%)	9.343	0.306	0.213	0.065
		(0.148)	(0.115)	(0.178)
Observations	295,728	295,728	295,728	349,044
Baseline Controls		X	X	X
Enrollee Controls			X	X
Plan Controls				X

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II for details on primary sample construction). The independent variable is overall network breadth as measured by the normalized covered share of simulated visits. The dependent variables are a specific set of potentially high-value services. Columns 2 and 3 report the main two-stage least squares (2SLS) results from estimating Equation 5 for overall networks breadth with and without enrollee-level controls. Column 4 reports 2SLS results based on a model with plan fixed effects (see Appendix C) estimated on a broader sample that includes enrollees in the provider-owned plan. All standard errors are clustered at the county × month of assignment level.

Appendix Table 14—: Estimates of the Impact of Network Breadth on Potentially Low-Value Care

	Any use (%)			
	Mean	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
Any potentially low-value care (%)	16.007	0.651	0.664	0.645
		(0.164)	(0.151)	(0.194)
Imaging and lab (%)	13.462	0.291	0.294	0.731
		(0.159)	(0.145)	(0.193)
Emergency department visits (%)	5.362	−0.138	−0.110	−0.076
		(0.085)	(0.083)	(0.117)
Panel A. Avoidable hospitalizations				
Diabetes short-term complications (%)	0.029	−0.008	−0.006	−0.010
		(0.007)	(0.007)	(0.009)
COPD or Asthma, age 40 and older (%)	0.203	−0.019	−0.011	0.000
		(0.017)	(0.016)	(0.022)
Congestive Heart Failure (%)	0.091	0.012	0.004	0.013
		(0.011)	(0.011)	(0.015)
Asthma, ages 18 to 39 (%)	0.116	0.007	0.004	−0.012
		(0.015)	(0.014)	(0.019)
Any avoidable hospitalizations (%)	0.407	−0.013	−0.014	−0.015
		(0.027)	(0.025)	(0.031)
Panel B. Designated low-value care				
Thorax CT (%)	0.006	0.004	0.003	0.003
		(0.003)	(0.003)	(0.003)
Head imaging for syncope (%)	0.173	0.003	0.002	−0.010
		(0.014)	(0.014)	(0.021)
Head imaging for uncomplicated headache (%)	0.005	0.002	0.002	−0.003
		(0.001)	(0.001)	(0.003)
Abdomen CT (%)	0.027	0.000	−0.000	−0.005
		(0.005)	(0.005)	(0.007)
Any clinically-designated low-value care (%)	0.207	0.004	0.003	−0.017
		(0.016)	(0.015)	(0.023)
Observations	295,728	295,728	295,728	349,044
Baseline Controls		X	X	X
Enrollee Controls			X	X
Plan Controls				X

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II for details on primary sample construction). The independent variable is overall network breadth as measured by the normalized covered share of simulated visits. The dependent variables are a specific set of potentially low-value services. Columns 2 and 3 report the main two-stage least squares (2SLS) results from estimating Equation 5 for overall networks breadth with and without enrollee-level controls. Column 4 reports 2SLS results based on a model with plan fixed effects (see Appendix C) estimated on a broader sample that includes enrollees in the provider-owned plan. All standard errors are clustered at the county × month of assignment level.

Appendix Table 15—: 2SLS Overall Network Results by Enrollee Characteristics

	Gender		Age		Quartiles of Lasso-predicted spending				Health condition		
	Male	Female	Under 40	40 and older	1st	2nd	3rd	4th	BH	Diab	CVD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A. Healthcare spending											
Log spending	0.059	0.088	0.050	0.119	0.065	0.071	0.063	0.073	0.079	0.061	0.086
	(0.018)	(0.022)	(0.015)	(0.028)	(0.017)	(0.018)	(0.029)	(0.040)	(0.041)	(0.090)	(0.082)
Any spending (%)	0.718	1.363	0.928	1.187	1.042	1.197	0.665	0.890	1.018	0.321	−0.072
	(0.250)	(0.337)	(0.236)	(0.358)	(0.290)	(0.296)	(0.429)	(0.514)	(0.550)	(1.045)	(0.983)
Panel B. Potentially high-value care											
Any high-value medical care (%)	0.523	1.052	0.508	1.213	0.471	0.558	0.592	1.309	1.141	1.665	3.378
	(0.155)	(0.247)	(0.144)	(0.271)	(0.165)	(0.210)	(0.255)	(0.392)	(0.389)	(0.891)	(0.900)
Any recommended preventive care (%)	0.022	0.250	0.073	0.198	0.199	0.052	0.077	0.175	0.089	0.794	0.337
	(0.038)	(0.099)	(0.054)	(0.088)	(0.066)	(0.093)	(0.102)	(0.109)	(0.075)	(0.394)	(0.318)
Any high-value prescription drugs (%)	0.023	0.488	0.054	0.586	−0.005	0.092	0.280	0.415	0.114	0.198	0.683
	(0.163)	(0.189)	(0.111)	(0.257)	(0.127)	(0.163)	(0.241)	(0.371)	(0.346)	(1.019)	(1.155)
Any potentially high-value care (%)	0.389	1.074	0.459	1.144	0.514	0.531	0.648	0.954	0.800	1.979	1.664
	(0.205)	(0.284)	(0.161)	(0.336)	(0.189)	(0.249)	(0.308)	(0.473)	(0.445)	(1.122)	(1.173)
Panel C. Potentially low-value care											
Any imaging or lab (%)	0.036	0.637	0.184	0.518	0.255	0.277	0.128	0.374	0.726	0.165	0.558
	(0.195)	(0.230)	(0.161)	(0.276)	(0.169)	(0.217)	(0.303)	(0.407)	(0.452)	(0.894)	(0.812)
Any emergency department use (%)	−0.169	−0.049	−0.026	−0.301	0.080	0.024	−0.411	−0.113	−0.151	−1.009	−0.289
	(0.113)	(0.119)	(0.098)	(0.141)	(0.121)	(0.125)	(0.148)	(0.232)	(0.234)	(0.513)	(0.556)
Any avoidable hospitalizations (%)	−0.053	0.046	−0.003	−0.037	0.011	0.006	0.025	−0.116	−0.055	−0.000	0.007
	(0.032)	(0.035)	(0.025)	(0.058)	(0.014)	(0.025)	(0.033)	(0.094)	(0.085)	(0.277)	(0.312)
Any designated low-value care (%)	−0.003	0.003	−0.012	0.026	0.001	0.008	0.011	0.003	0.002	−0.041	0.092
	(0.020)	(0.024)	(0.017)	(0.033)	(0.017)	(0.019)	(0.028)	(0.049)	(0.044)	(0.121)	(0.130)
Any potentially low-value care (%)	0.369	1.052	0.608	0.761	0.486	0.647	0.410	1.036	1.272	0.740	0.873
	(0.201)	(0.242)	(0.179)	(0.275)	(0.200)	(0.220)	(0.306)	(0.423)	(0.460)	(0.881)	(0.856)
Panel D. Consumer satisfaction											
In assigned plan (%)	1.145	1.022	1.007	1.335	0.368	0.848	1.177	2.127	1.667	1.835	1.096
	(0.137)	(0.167)	(0.114)	(0.211)	(0.151)	(0.165)	(0.229)	(0.287)	(0.256)	(0.686)	(0.641)
Baseline Controls	X	X	X	X	X	X	X	X	X	X	X
Enrollee Controls	X	X	X	X	X	X	X	X	X	X	X

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II for details on primary sample construction). The independent variable is overall network breadth as measured by the normalized covered share of simulated visits. The dependent variables include measures of healthcare use and spending, specific high-value and low-value services, and an ex-post demand measure of enrollee satisfaction. Panel D presents reduced form, rather than 2SLS, estimates as they measure the likelihood that enrollees remain in their assigned plans. The table contains two-stage least squares (2SLS) results from estimating Equation 5 for overall networks breadth with enrollee-level controls. Each estimate is a subgroup analyses that subsets the data to a particular group (e.g., males) before running the analysis. A subset of enrollees are assigned to one of three health condition subgroups based on hierarchical condition codes (HCCs) assigned in the pre-assignment period. I use HCCs to group enrollees into those with behavioral health (BH) conditions, diabetes ("Diab"), and cardiovascular disease (CVD). Appendix A describes these groupings in more detail. All standard errors are clustered at the county × month of assignment level.

Appendix Table 16—: Reduced Rorm Estimates of the Impact of Network Breadth on Consumer Satisfaction

	Share of sample (1)	Sample Mean (2)	RF (3)	RF (4)	RF (4)
In assigned plan (%)	1.00	96.153	1.004	1.112	0.918
			(0.097)	(0.095)	(0.169)
Panel A. Satisfaction by enrollee characteristics					
Male	0.60	96.608	1.051	1.145	0.899
			(0.138)	(0.137)	(0.241)
Female	0.40	95.483	0.887	1.022	0.894
			(0.166)	(0.167)	(0.281)
18-39	0.65	96.897	0.921	1.007	0.876
			(0.115)	(0.114)	(0.194)
40-64	0.35	94.791	1.197	1.335	1.005
			(0.211)	(0.211)	(0.366)
Panel B. Satisfaction by enrollee health status					
1st quartile predicted spending	0.25	97.956	0.279	0.346	0.305
			(0.150)	(0.150)	(0.260)
2nd quartile predicted spending	0.25	97.043	0.878	0.955	0.798
			(0.163)	(0.163)	(0.257)
3rd quartile predicted spending	0.25	95.961	0.942	1.044	1.033
			(0.231)	(0.230)	(0.398)
4th quartile predicted spending	0.25	93.652	1.933	2.091	1.658
			(0.284)	(0.281)	(0.456)
Baseline Controls			X	X	X
Enrollee Controls				X	X
Plan Controls					X

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II for details on primary sample construction). The independent variable is overall network breadth as measured by the normalized covered share of simulated visits. The dependent variable is an ex-post demand measure of enrollee satisfaction. Columns 3 and 4 report the results of estimating a reduced form (RF) version of Equation 5 for overall networks breadth with and without enrollee-level controls. Column 5 reports reduced form results based on a model with plan fixed effects (see Appendix C) estimated on a broader sample that includes enrollees in the provider-owned plan. All standard errors are clustered at the county × month of assignment level.

Appendix Table 17—: Heterogeneity by Network Characteristics: Model Without Plan Controls

	Main sample			Usual source of care sample		
	Main Spec.	Alternative specification w/ physician and hospital		Alternative specification w/ physician and hospital and key provider		
	Overall Network (1)	Physician Network (2)	Hospital Network (3)	Physician Network (4)	Hospital Network (5)	Key provider in assigned (6)
Panel A. Healthcare use and spending						
Log spending	0.071	0.046	0.035	0.041	0.009	0.170
	(0.014)	(0.014)	(0.015)	(0.024)	(0.027)	(0.048)
Any spending (%)	1.002	0.610	0.541	0.480	0.112	1.839
	(0.201)	(0.194)	(0.209)	(0.323)	(0.380)	(0.720)
Panel B. Potentially high-value care						
Any high-value medical care (%)	0.742	−0.214	1.034	−0.105	1.197	1.369
	(0.135)	(0.151)	(0.149)	(0.281)	(0.275)	(0.485)
Any recommended preventive care (%)	0.118	0.062	0.073	0.077	0.047	0.154
	(0.047)	(0.045)	(0.051)	(0.077)	(0.095)	(0.169)
Any high-value prescription drugs (%)	0.213	0.072	0.170	0.084	0.043	1.099
	(0.115)	(0.142)	(0.137)	(0.249)	(0.255)	(0.411)
Any potentially high-value care (%)	0.680	−0.032	0.792	−0.067	0.809	1.625
	(0.161)	(0.166)	(0.177)	(0.291)	(0.320)	(0.581)
Panel C. Potentially low-value care						
Any imaging and lab (%)	0.294	0.087	0.246	−0.149	0.264	0.869
	(0.145)	(0.158)	(0.162)	(0.271)	(0.268)	(0.468)
Any emergency department use (%)	−0.110	−0.031	−0.094	−0.012	−0.024	−0.554
	(0.083)	(0.083)	(0.097)	(0.147)	(0.188)	(0.289)
Any avoidable hospitalizations (%)	−0.014	−0.008	−0.008	−0.009	−0.050	0.021
	(0.025)	(0.026)	(0.029)	(0.056)	(0.067)	(0.096)
Any designated low-value care (%)	0.003	0.032	−0.027	0.041	−0.060	0.031
	(0.015)	(0.014)	(0.016)	(0.025)	(0.033)	(0.049)
Any potentially low-value care (%)	0.664	0.307	0.451	0.203	0.629	0.752
	(0.151)	(0.163)	(0.171)	(0.286)	(0.303)	(0.503)
Panel D. Consumer satisfaction						
In assigned plan (%)	1.112	0.637	0.638	0.871	0.277	2.800
	(0.095)	(0.111)	(0.116)	(0.211)	(0.220)	(0.421)
Observations	295,728	295,728	295,728	130,896	130,896	130,896
Baseline controls	X	X	X	X	X	X
Enrollee Controls	X	X	X	X	X	X
Plan controls						
Usual source of care sample				X	X	X

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II for details on primary sample construction), excluding enrollees in the provider-owned plan. None of the regressions include plan controls (i.e., fixed effects). The dependent variables include measures of healthcare use and spending, specific high-value and low-value services, and an ex-post demand measure of enrollee satisfaction. Panel D presents reduced form, rather than 2SLS, estimates of the likelihood that enrollees remain in their assigned plans. In Column 1, the independent variable is overall network breadth (normalized covered share of simulated visits). Columns 2 and 3 report the main two-stage least squares (2SLS) results from estimating Equation 6 using physician and hospital network breadth in the same model. Columns 4-6 restrict the sample to enrollees who could be attributed to a physician or hospital based on care they sought prior to assignment (the "usual source of care sample"). The column reports the results of estimating Equation 7 on this restricted sample. All standard errors are clustered at the county × month of assignment level.

Appendix Table 18—: Balance Test of Assigned Physician and Hospital Provider Network Breadth

	Separate Multivariate OLS Regressions			Joint Multivariate OLS Regression	
	Overall Network (1)	Physician Network (2)	Hospital Network (3)	Physician Network (4)	Hospital Network (5)
Age	0.0092	0.0125	0.0048	0.0093	−0.0020
	(0.0144)	(0.0145)	(0.0134)	(0.0110)	(0.0102)
Male	−0.0001	−0.0013	0.0009	−0.0019	0.0016
	(0.0114)	(0.0116)	(0.0104)	(0.0088)	(0.0079)
Black	−0.0029	−0.0019	−0.0030	0.0001	−0.0019
	(0.0061)	(0.0060)	(0.0057)	(0.0045)	(0.0042)
Outpatient spending	0.0114	0.0223	0.0010	0.0217	−0.0112
	(0.0155)	(0.0154)	(0.0149)	(0.0122)	(0.0118)
Inpatient spending	0.0010	0.0028	−0.0005	0.0031	−0.0020
	(0.0041)	(0.0039)	(0.0041)	(0.0033)	(0.0034)
Pharmacy spending	0.0087	0.0134	0.0034	0.0111	−0.0039
	(0.0102)	(0.0103)	(0.0094)	(0.0080)	(0.0073)
Other spending	0.0042	0.0117	−0.0021	0.0132	−0.0085
	(0.0092)	(0.0087)	(0.0092)	(0.0071)	(0.0075)
Any high-value medical care (%)	0.0077	0.0090	0.0051	0.0055	0.0003
	(0.0168)	(0.0171)	(0.0155)	(0.0129)	(0.0117)
Any recommended preventive care (%)	0.0028	−0.0011	0.0051	−0.0045	0.0056
	(0.0075)	(0.0074)	(0.0071)	(0.0058)	(0.0055)
Any high-value prescription drugs (%)	0.0266	0.0229	0.0236	0.0070	0.0112
	(0.0426)	(0.0431)	(0.0388)	(0.0317)	(0.0285)
Any lab or imaging (%)	−0.0001	0.0187	−0.0138	0.0281	−0.0240
	(0.0186)	(0.0173)	(0.0186)	(0.0139)	(0.0149)
Any emergency department use (%)	−0.0034	0.0015	−0.0063	0.0057	−0.0071
	(0.0067)	(0.0064)	(0.0068)	(0.0053)	(0.0056)
Any avoidable hospitalizations (%)	0.0003	−0.0016	0.0016	−0.0027	0.0025
	(0.0052)	(0.0052)	(0.0049)	(0.0041)	(0.0039)
Any low-value care visits (%)	0.0036	0.0009	0.0048	−0.0023	0.0043
	(0.0040)	(0.0038)	(0.0038)	(0.0029)	(0.0030)
Predicted spending	−0.0012	0.0014	−0.0029	0.0033	−0.0036
	(0.0064)	(0.0065)	(0.0059)	(0.0050)	(0.0046)
Predicted any potentially HVC (%)	−0.0373	−0.0258	−0.0375	−0.0006	−0.0235
	(0.0675)	(0.0692)	(0.0605)	(0.0503)	(0.0438)
Predicted any potentially LVC (%)	−0.0056	−0.0419	0.0220	−0.0567	0.0448
	(0.0429)	(0.0399)	(0.0425)	(0.0311)	(0.0333)
Predicted share in assigned plan (%)	0.0018	0.0045	−0.0005	0.0049	−0.0030
	(0.0353)	(0.0357)	(0.0330)	(0.0277)	(0.0257)
P-value on joint F-test	0.82	0.30	0.96	0.19	0.73
Observations	58,172	58,172	58,172	58,172	58,172
Baseline Controls	X	X	X	X	X
Plan Controls					

Notes: This table reports reduced form results testing the conditional random assignment of enrollees to physician and hospital networks. Results are based on my primary sample (see Section II for details on primary sample construction), including enrollees in the provider-owned plan. Baseline outcomes are the average for each enrollee in the three months prior to assignment. Predicted spending, high-value care (HVC), and low-value care (LVC) are formed using the other baseline variables. Detailed descriptions of the outcome measures are included in Appendix A. Columns 1-3 present the results of multivariate OLS models with enrollee characteristics as the independent variables and the assigned network breadth as the dependent variable. Column 1 reproduces results from Table 2 for reference. Columns 4 and 5 presents bivariate OLS regressions with enrollee characteristics as the independent variable and assigned physician or hospital network breadth as the dependent variable. Standard errors are clustered at the county × month of assignment level.

Appendix Table 19—: Summary statistics for Auto-Assignees in the "Usual Source of Care" Sample

	Mean	Std. Dev.	Observations
	(1)	(2)	(3)
Demographics			
Age	36.746	12.301	157,536
Male	0.564	0.496	157,536
Black	0.513	0.500	157,536
Assigned network breadth			
Covered share of simulated visits (%)	59.103	15.117	157,536
Covered share of simulated physician visits (%)	56.662	14.215	157,536
Covered share of simulated hospital visits (%)	61.692	20.488	157,536
Network covers primary provider (%)	67.365	46.888	157,536
Healthcare spending			
Total spending	614.540	3,003.766	157,536
Outpatient spending	104.189	446.117	157,536
Inpatient spending	284.041	2,696.346	157,536
Pharmacy spending	94.808	450.343	157,536
Other spending	131.503	536.027	157,536
Healthcare use			
Any spending (%)	43.280	49.546	157,536
Any outpatient spending (%)	24.624	43.082	157,536
Any inpatient spending (%)	3.021	17.116	157,536
Any pharmacy spending (%)	26.525	44.147	157,536
Any other spending (%)	27.259	44.529	157,536
Potentially high-value care			
Any high-value medical care (%)	16.373	37.004	157,536
Any recommended preventive care (%)	2.830	16.582	157,536
Any high-value prescription drugs	13.891	34.586	157,536
Any potentially high-value care (%)	24.917	43.254	157,536
Potentially low-value care			
Any lab or imaging (%)	17.643	38.119	157,536
Any emergency department use (%)	7.878	26.939	157,536
Any avoidable hospitalization (%)	0.708	8.387	157,536
Any designated low-value care (%)	0.315	5.608	157,536
Any potentially low-value care (%)	21.285	40.932	157,536
Satisfaction			
In assigned plan (%)	92.001	27.129	157,536

Notes: This table reports summary statistics. Summary statistics are based on my primary sample (see Section II), but further restricted to enrollees who could be attributed to a physician or hospital based on care they sought prior to assignment (the "usual source of care sample"). Observations are at the enrollee-month level and limited to the six months post-assignment (my primary sample). Details on the construction of the measures of network breadth are included in Section III. Additional details on the broad service categories or specific services identified as potentially high-value or low-value care are included in Appendix A.

Appendix Table 20—: Balance Test of Assignment to Plans on the Basis of In-Network Status of Enrollees' Usual Source of Care

	Mean (SD)	Multivariate OLS	
	(1)	(2)	(3)
Age	36.538	0.0001	−0.0015
	(12.300)	(0.0002)	(0.0009)
Male	0.564	−0.0075	0.0155
	(0.496)	(0.0056)	(0.0143)
Black	0.513	−0.0076	−0.0056
	(0.500)	(0.0053)	(0.0061)
Outpatient spending	165.237	−0.0000	−0.0001
	(317.124)	(0.0000)	(0.0000)
Inpatient spending	512.633	0.0000	−0.0000
	(2785.248)	(0.0000)	(0.0000)
Pharmacy spending	105.895	−0.0000	−0.0000
	(461.494)	(0.0000)	(0.0000)
Other spending	172.981	0.0000	−0.0000
	(491.016)	(0.0000)	(0.0000)
Any high-value medical care (%)	29.125	0.0000	−0.0003
	(45.435)	(0.0001)	(0.0002)
Any recommended preventive care (%)	7.115	−0.0000	−0.0002
	(25.707)	(0.0001)	(0.0002)
Any high-value prescription drugs (%)	23.073	0.0001	−0.0006
	(42.131)	(0.0001)	(0.0006)
Any lab or imaging (%)	33.699	−0.0000	−0.0002
	(47.269)	(0.0001)	(0.0002)
Any emergency department use (%)	30.275	−0.0000	−0.0000
	(45.946)	(0.0001)	(0.0001)
Any avoidable hospitalizations (%)	2.476	0.0001	0.0002
	(15.538)	(0.0002)	(0.0003)
Any low-value care visits (%)	1.047	0.0002	0.0002
	(10.181)	(0.0003)	(0.0003)
Predicted spending	615.140		0.0000
	(844.414)		(0.0000)
Predicted any potentially HVC (%)	24.917		0.0010
	(17.857)		(0.0023)
Predicted any potentially LVC (%)	21.285		0.0012
	(10.374)		(0.0021)
Predicted share in assigned plan (%)	92.001		−0.0072
	(4.758)		(0.0049)
P-value on joint F-test		0.30	0.19
Observations	26,256	26,256	26,256
Baseline Controls		X	X
Plan Controls			

Notes: This table reports reduced form results testing the conditional random assignment of enrollees to plans that cover their usual sources of care. Results are based on my primary sample (see Section II), but further restricted to enrollees who could be attributed to a physician or hospital based on care they sought prior to assignment (the "usual source of care sample"). Baseline outcomes are the average for each enrollee in the three months prior to assignment. Predicted spending, high-value care (HVC), and low-value care (LVC) are formed using the other baseline variables. Detailed descriptions of the outcome measures are included in Appendix A. Columns 2-3 present the results of multivariate OLS models with enrollee characteristics as the independent variables and an indicator for whether the assigned network covers an enrollees usual source of care as the dependent variable. In addition to the baseline controls, all regressions include fixed effects for the individual providers that were enrollees' attributed usual sources of care. Standard errors are clustered at the county × month of assignment level.

Appendix Table 21—: Heterogeneity by Provider Network Characteristics: Analyses Restricted to Enrollees Assigned to Plans That Cover Their Usual Source of Care

	Usual source of care sample		
	Physician Network (1)	Hospital Network (2)	Key provider in assigned (3)
Panel A. Healthcare use and spending			
Log spending	0.083	−0.037	-
	(0.047)	(0.039)	-
Any spending (%)	0.866	0.308	-
	(0.691)	(0.521)	-
Panel B. Potentially high-value care			
Any high-value medical care (%)	0.854	−0.684	-
	(0.453)	(0.416)	-
Any recommended preventive care (%)	0.072	−0.062	-
	(0.168)	(0.126)	-
Any high-value prescription drugs (%)	−0.107	−0.070	-
	(0.479)	(0.387)	-
Any potentially high-value care (%)	0.048	−0.437	-
	(0.561)	(0.471)	-
Panel C. Potentially low-value care			
Any imaging and lab (%)	1.277	−0.347	-
	(0.530)	(0.383)	-
Any emergency department use (%)	−0.154	−0.106	-
	(0.320)	(0.268)	-
Any avoidable hospitalizations (%)	−0.021	−0.079	-
	(0.103)	(0.076)	-
Any designated low-value care (%)	0.086	−0.025	-
	(0.051)	(0.040)	-
Any potentially low-value care (%)	1.005	−0.449	-
	(0.562)	(0.426)	-
Panel D. Consumer satisfaction			
In assigned plan (%)	0.340	0.813	
	(0.395)	(0.310)	
Observations	106,124	106,124	106,124
Baseline controls	X	X	X
Enrollee Controls	X	X	X
Plan controls	X	X	X
Usual source of care sample	X	X	X

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II), but further restricted to enrollees who could be attributed to a physician or hospital based on care they sought prior to assignment (the "usual source of care sample"). The dependent variables include measures of healthcare use and spending, specific high-value and low-value services, and an ex-post demand measure of enrollee satisfaction. Panel D presents reduced form, rather than 2SLS, estimates of the likelihood that enrollees remain in their assigned plans. Columns 1-3 report the results of estimating Equation 7 after first restricting to enrollees whose usual sourc of care is in their assigned plan (hence, there is no variation with which to identify the usual source of care effect in Column 3). All standard errors are clustered at the county × month of assignment level.

Appendix Table 22—: Heterogeneity by Provider Network Characteristics, Extended Sample 6 Months Post-Assignment

	Main sample			Usual source of care sample		
	Main Spec.	Alternative specification w/ physician and hospital		Alternative specification w/ physician and hospital and key provider		
	Overall Network (1)	Physician Network (2)	Hospital Network (3)	Physician Network (4)	Hospital Network (5)	Key provider in assigned (6)
Panel A. Healthcare use and spending						
Log spending	0.053	0.045	0.024	0.047	−0.015	0.180
	(0.017)	(0.019)	(0.015)	(0.033)	(0.026)	(0.037)
Any spending (%)	0.717	0.443	0.420	0.640	0.001	1.974
	(0.244)	(0.290)	(0.228)	(0.486)	(0.369)	(0.544)
Panel B. Potentially high-value care						
Any high-value medical care (%)	0.367	0.452	0.078	0.816	−0.426	1.802
	(0.172)	(0.177)	(0.161)	(0.324)	(0.300)	(0.429)
Any recommended preventive care (%)	0.083	−0.025	0.095	−0.019	0.014	0.274
	(0.063)	(0.073)	(0.058)	(0.122)	(0.097)	(0.125)
Any high-value prescription drugs (%)	0.087	−0.143	0.171	−0.232	−0.054	1.096
	(0.163)	(0.203)	(0.169)	(0.354)	(0.297)	(0.382)
Any potentially high-value care (%)	0.456	0.453	0.163	0.503	−0.388	2.082
	(0.218)	(0.256)	(0.206)	(0.444)	(0.360)	(0.510)
Panel C. Potentially low-value care						
Any imaging and lab (%)	0.634	0.550	0.275	0.708	−0.038	1.207
	(0.189)	(0.215)	(0.184)	(0.354)	(0.281)	(0.400)
Any emergency department use (%)	−0.081	−0.003	−0.076	−0.100	−0.153	−0.184
	(0.102)	(0.121)	(0.090)	(0.224)	(0.180)	(0.229)
Any avoidable hospitalizations (%)	−0.021	−0.013	−0.012	−0.070	−0.080	0.158
	(0.027)	(0.036)	(0.027)	(0.073)	(0.058)	(0.060)
Any designated low-value care (%)	−0.009	0.021	−0.022	0.059	−0.034	0.035
	(0.023)	(0.022)	(0.020)	(0.040)	(0.038)	(0.042)
Any potentially low-value care (%)	0.550	0.491	0.230	0.559	−0.161	1.385
	(0.195)	(0.217)	(0.188)	(0.366)	(0.309)	(0.422)
Panel D. Consumer satisfaction						
In assigned plan (%)	1.069	0.601	0.662	0.565	0.200	5.091
	(0.176)	(0.194)	(0.158)	(0.391)	(0.287)	(0.378)
Observations	320,226	320,226	320,226	142,572	142,572	142,572
Baseline controls	X	X	X	X	X	X
Enrollee Controls	X	X	X	X	X	X
Plan controls	X	X	X	X	X	X
Usual source of care sample				X	X	X

Notes: Standard errors in parentheses. Results are based on enrollee-months in the 6 months post-assignment for an imbalanced sample of enrollees (Appendix A describes the construction of this sample. Because additional restrictions are imposed on enrollees—i.e., enrollees had to remain in New York City for at least 12 months following assignment (rather than 6 as in my primary sample)—the enrollee-month counts are lower in this sample even in the 6 months post-assignment. The dependent variables include measures of healthcare use and spending, specific high-value and low-value services, and an ex-post demand measure of enrollee satisfaction. Panel D presents reduced form, rather than 2SLS, estimates of the likelihood that enrollees remain in their assigned plans. In Column 1, the independent variable is overall network breadth (normalized covered share of simulated visits). Columns 2 and 3 report the main two-stage least squares (2SLS) results from estimating Equation 6 using physician and hospital network breadth in the same model. Columns 4-6 restrict the sample to enrollees who could be attributed to a physician or hospital based on care they sought prior to assignment (the "usual source of care sample"). The column reports the results of estimating Equation 7 on this restricted sample. All standard errors are clustered at the county × month of assignment level.

Appendix Table 23—: Heterogeneity by Provider Network Characteristics, Extended Sample 1 Year Post-Assignment

	Main sample			Usual source of care sample		
	Main Spec.	Alternative specification w/ physician and hospital		Alternative specification w/ physician and hospital and key provider		
	Overall Network (1)	Physician Network (2)	Hospital Network (3)	Physician Network (4)	Hospital Network (5)	Key provider in assigned (6)
Panel A. Healthcare use and spending						
Log spending	0.069	0.081	0.016	0.088	−0.018	0.151
	(0.017)	(0.018)	(0.016)	(0.030)	(0.027)	(0.038)
Any spending (%)	0.955	0.999	0.301	1.261	0.062	1.639
	(0.244)	(0.271)	(0.220)	(0.430)	(0.366)	(0.549)
Panel B. Potentially high-value care						
Any high-value medical care (%)	0.349	0.639	−0.061	0.983	−0.470	1.327
	(0.184)	(0.179)	(0.166)	(0.309)	(0.308)	(0.426)
Any recommended preventive care (%)	0.085	−0.045	0.111	−0.029	0.066	0.111
	(0.058)	(0.066)	(0.053)	(0.112)	(0.083)	(0.113)
Any high-value prescription drugs (%)	0.076	−0.016	0.084	−0.180	−0.013	0.947
	(0.185)	(0.214)	(0.184)	(0.366)	(0.311)	(0.427)
Any potentially high-value care (%)	0.515	0.641	0.099	0.606	−0.269	1.695
	(0.239)	(0.263)	(0.216)	(0.422)	(0.370)	(0.535)
Panel B. Potentially low-value care						
Any imaging and lab (%)	0.719	0.637	0.298	0.677	0.084	0.952
	(0.183)	(0.208)	(0.183)	(0.364)	(0.289)	(0.388)
Any emergency department use (%)	−0.068	−0.031	−0.047	−0.132	−0.108	0.022
	(0.098)	(0.112)	(0.085)	(0.193)	(0.165)	(0.234)
Any avoidable hospitalizations (%)	−0.031	−0.028	−0.012	−0.106	−0.064	0.213
	(0.029)	(0.041)	(0.025)	(0.078)	(0.050)	(0.056)
Any designated low-value care (%)	−0.017	−0.003	−0.015	0.016	−0.004	−0.001
	(0.021)	(0.019)	(0.019)	(0.034)	(0.035)	(0.036)
Any potentially low-value care (%)	0.659	0.600	0.264	0.648	−0.073	1.292
	(0.189)	(0.206)	(0.187)	(0.363)	(0.312)	(0.406)
Panel D. Consumer satisfaction						
In assigned plan (%)	1.388	0.563	0.993	0.449	0.198	7.308
	(0.240)	(0.268)	(0.216)	(0.491)	(0.383)	(0.514)
Observations	431,990	431,990	431,990	198,611	198,611	198,611
Baseline controls	X	X	X	X	X	X
Enrollee Controls	X	X	X	X	X	X
Plan controls	X	X	X	X	X	X
Usual source of care sample				X	X	X

Notes: Standard errors in parentheses. Results are based on enrollee-months in the 12 months post-assignment for an imbalanced sample of enrollees (Appendix A describes the construction of this sample. Because additional restrictions are imposed on enrollees—i.e., enrollees had to remain in New York City for at least 12 months following assignment (rather than 6 as in my primary sample)—the enrollee-month counts are lower in this sample even in the 6 months post-assignment. The dependent variables include measures of healthcare use and spending, specific high-value and low-value services, and an ex-post demand measure of enrollee satisfaction. Panel D presents reduced form, rather than 2SLS, estimates of the likelihood that enrollees remain in their assigned plans. In Column 1, the independent variable is overall network breadth (normalized covered share of simulated visits). Columns 2 and 3 report the main two-stage least squares (2SLS) results from estimating Equation 6 using physician and hospital network breadth in the same model. Columns 4-6 restrict the sample to enrollees who could be attributed to a physician or hospital based on care they sought prior to assignment (the "usual source of care sample"). The column reports the results of estimating Equation 7 on this restricted sample. All standard errors are clustered at the county × month of assignment level.

Appendix Table 24—: Heterogeneity by Provider Network Characteristics, Extended Sample 2 Years Post-Assignment

	Main sample			Usual source of care sample		
	Main Spec.	Alternative specification w/ physician and hospital		Alternative specification w/ physician and hospital and key provider		
	Overall Network (1)	Physician Network (2)	Hospital Network (3)	Physician Network (4)	Hospital Network (5)	Key provider in assigned (6)
Panel A. Healthcare use and spending						
Log spending	0.064	0.073	0.017	0.083	−0.022	0.152
	(0.019)	(0.018)	(0.017)	(0.028)	(0.028)	(0.040)
Any spending (%)	0.776	0.874	0.205	1.217	−0.069	1.763
	(0.260)	(0.281)	(0.228)	(0.434)	(0.360)	(0.564)
Panel B. Potentially high-value care						
Any high-value medical care (%)	0.240	0.612	−0.150	0.950	−0.650	1.345
	(0.203)	(0.187)	(0.176)	(0.322)	(0.314)	(0.455)
Any recommended preventive care (%)	0.052	−0.015	0.059	0.027	0.015	0.159
	(0.054)	(0.063)	(0.052)	(0.099)	(0.085)	(0.114)
Any high-value prescription drugs (%)	0.053	−0.047	0.080	−0.139	0.058	0.929
	(0.214)	(0.236)	(0.189)	(0.393)	(0.317)	(0.493)
Any potentially high-value care (%)	0.435	0.534	0.087	0.525	−0.272	1.612
	(0.263)	(0.278)	(0.222)	(0.436)	(0.369)	(0.603)
Panel B. Potentially low-value care						
Any imaging and lab (%)	0.562	0.533	0.211	0.508	−0.066	0.998
	(0.181)	(0.196)	(0.183)	(0.327)	(0.288)	(0.397)
Any emergency department use (%)	−0.042	0.008	−0.046	−0.017	−0.134	0.066
	(0.100)	(0.104)	(0.089)	(0.183)	(0.165)	(0.227)
Any avoidable hospitalizations (%)	0.000	−0.012	0.008	−0.051	−0.053	0.208
	(0.028)	(0.039)	(0.023)	(0.072)	(0.045)	(0.058)
Any designated low-value care (%)	−0.026	−0.015	−0.016	−0.011	−0.013	0.014
	(0.019)	(0.017)	(0.017)	(0.030)	(0.032)	(0.031)
Any potentially low-value care (%)	0.551	0.546	0.193	0.560	−0.190	1.308
	(0.190)	(0.194)	(0.192)	(0.332)	(0.312)	(0.418)
Panel D. Consumer satisfaction						
In assigned plan (%)	1.816	0.942	1.171	0.904	0.238	8.832
	(0.284)	(0.320)	(0.274)	(0.549)	(0.435)	(0.627)
Observations	523,194	523,194	523,194	246,095	246,095	246,095
Baseline controls	X	X	X	X	X	X
Enrollee Controls	X	X	X	X	X	X
Plan controls	X	X	X	X	X	X
Usual source of care sample				X	X	X

Notes: Standard errors in parentheses. Results are based on enrollee-months in the 24 months post-assignment for an imbalanced sample of enrollees (Appendix A describes the construction of this sample. Because additional restrictions are imposed on enrollees—i.e., enrollees had to remain in New York City for at least 12 months following assignment (rather than 6 as in my primary sample)—the enrollee-month counts are lower in this sample even in the 6 months post-assignment. The dependent variables include measures of healthcare use and spending, specific high-value and low-value services, and an ex-post demand measure of enrollee satisfaction. Panel D presents reduced form, rather than 2SLS, estimates of the likelihood that enrollees remain in their assigned plans. In Column 1, the independent variable is overall network breadth (normalized covered share of simulated visits). Columns 2 and 3 report the main two-stage least squares (2SLS) results from estimating Equation 6 using physician and hospital network breadth in the same model. Columns 4-6 restrict the sample to enrollees who could be attributed to a physician or hospital based on care they sought prior to assignment (the "usual source of care sample"). The column reports the results of estimating Equation 7 on this restricted sample. All standard errors are clustered at the county × month of assignment level.

Appendix Table 25—: Impact of Alternative Assignment Policies on Outcomes and Mean Network Breadth

	Counterfactual outcomes (Δ)		Counterfactual network size (Δ)		
	Consumer Satisfaction (pp)	Total Cost (log points)	Physician Breadth (pp)	Hospital Breadth (pp)	Key provider in assigned (pp)
Alternative policy	(1)	(2)	(3)	(4)	(5)
Minimize cost	−2.77	−9.54	−5.74	−6.83	−48.70
	(0.21)	(1.94)			
Minimize cost without reducing satisfaction	0.04	−2.20	−7.35	1.08	5.15
	(0.20)	(1.81)			
Maximize satisfaction without increasing cost	0.71	−0.16	−7.00	1.85	17.78
	(0.20)	(1.92)			
Maximize satisfaction	1.84	6.74	5.87	8.22	30.55
	(0.17)	(1.62)			

Notes: This table reports the effects of select counterfactual assignment policies described in Section VII. Columns 1 and 2 contain point estimates and standard errors for the predicted differences in mean consumer satisfaction and log spending, respectively, relative to the state's current (random) assignment policy. Columns 3-5 describe the change in mean physician and hospital network breadth for each counterfactual, as well as differences in what share of enrollees are assigned to a plan where their usual source of care is in-network. These simulations are based on a randomly-selected subset of 4000 auto-assignees from a sample of enrollees who could be attributed to a physician or hospital based on care they sought prior to assignment (the "usual source of care" sample).

Appendix Table 26—: OLS Estimates of the Impact of Overall Network Breadth on Health Care Use and Spending Among Enrollees That Made Active Plan Choices

	Share of sample (1)	Sample Mean (2)	OLS (3)	OLS (4)	OLS (5)
Any spending (%)	1.00	50.591	1.032	1.437	0.841
			(0.220)	(0.197)	(0.241)
Log spending	1.00	548.975	0.052	0.097	0.072
			(0.014)	(0.012)	(0.015)
Observations		454,668	454,668	454,668	575,328
Panel A. Spending by enrollee characteristics					
Male	0.42	457.057	0.113	0.110	0.051
			(0.021)	(0.018)	(0.024)
Female	0.58	616.568	0.028	0.089	0.087
			(0.017)	(0.015)	(0.019)
18-39	0.65	486.562	−0.014	0.076	0.059
			(0.016)	(0.013)	(0.018)
40-64	0.35	666.315	0.133	0.139	0.088
			(0.023)	(0.021)	(0.026)
Panel B. Spending by enrollee health status					
1st quartile predicted spending	0.25	164.776	0.074	0.081	0.054
			(0.018)	(0.017)	(0.027)
2nd quartile predicted spending	0.25	237.931	0.135	0.137	0.063
			(0.022)	(0.021)	(0.029)
3rd quartile predicted spending	0.25	400.439	0.120	0.130	0.082
			(0.025)	(0.026)	(0.030)
4th quartile predicted spending	0.25	1,392.818	−0.038	0.017	0.043
			(0.028)	(0.026)	(0.031)
Baseline Controls			X	X	X
Enrollee Controls				X	X
Plan Controls					X

Notes: Standard errors in parentheses. Results are based on an alternative sample of enrollees that made active plan choices (see Appendix A for details on sample construction). The independent variable is actual network breadth as measured by the normalized covered share of simulated visits. The dependent variable is log spending for Panels B and C. Columns 3 and 4 report the main ordinary least squares (OLS) results from estimating a version of Equation 5 that uses actual network breadth (with no instrumentation), with and without enrollee-level controls. Unsurprisingly, the OLS estimates are more sensitive to the inclusion of controls than the IV estimates which are based on randomly assigned enrollees. Column 5 reports OLS results based on a model with plan fixed effects (see Appendix C) estimated on a broader sample that includes enrollees in the provider-owned plan. All standard errors are clustered at the county × month of assignment level.

Appendix Table 27—: OLS estimates of the Impact of Overall Network Breadth on Potentially High-Value and Low-Value Care

	DV Mean (1)	OLS (2)	OLS (3)	OLS (4)
Panel A. Potentially high-value care				
Any high-value medical care (%)	26.920	0.252	0.777	0.657
		(0.190)	(0.169)	(0.222)
Any recommended preventive care (%)	5.025	0.031	0.158	0.127
		(0.053)	(0.049)	(0.073)
Any high-value prescription drugs (%)	14.330	0.982	0.382	0.260
		(0.157)	(0.133)	(0.150)
Any potentially high-value care (%)	34.659	0.372	0.765	0.511
		(0.210)	(0.181)	(0.230)
Panel B. Potentially low-value care				
Any imaging and lab (%)	24.031	−0.390	0.235	0.638
		(0.167)	(0.145)	(0.188)
Any emergency department use (%)	5.390	−0.248	−0.188	0.048
		(0.063)	(0.059)	(0.083)
Any avoidable hospitalizations (%)	0.286	0.006	−0.011	0.011
		(0.015)	(0.014)	(0.020)
Any designated low-value care (%)	0.302	0.022	0.019	0.026
		(0.014)	(0.014)	(0.018)
Any potentially low-value care (%)	26.303	−0.225	0.345	0.618
		(0.172)	(0.153)	(0.195)
Observations	454,668	454,668	454,668	575,328
Baseline Controls	—	X	X	X
Enrollee Controls	—		X	X
Plan Controls	—			X

Notes: Standard errors in parentheses. Results are based on an alternative sample of enrollees that made active plan choices (see Appendix A for details on sample construction). The independent variable is actual network breadth as measured by the normalized covered share of simulated visits. The dependent variables include specific high-value and low-value services. Columns 3 and 4 report the main ordinary least squares (OLS) results from estimating a version of Equation 5 that uses actual network breadth (with no instrumentation), with and without enrollee-level controls. Unsurprisingly, the OLS estimates are more sensitive to the inclusion of controls than the IV estimates which are based on randomly assigned enrollees. Column 5 reports OLS results based on a model with plan fixed effects (see Appendix C) estimated on a broader sample that includes enrollees in the provider-owned plan. All standard errors are clustered at the county × month of assignment level.

Appendix Table 28—: Reweighted Estimates of the Impact of Overall Network Breadth on Health Care Use and Spending

	Share of sample (1)	Sample Mean (2)	2SLS (3)	2SLS (4)	2SLS (4)
Log spending	1.00	371.916	0.079	0.080	0.076
			(0.018)	(0.017)	(0.022)
Any spending (%)	1.00	31.451	1.272	1.041	1.165
			(0.285)	(0.257)	(0.332)
Observations		295,722	295,722	295,722	349,038
Panel A. Spending by enrollee characteristics					
Male	0.60	406.086	0.052	0.060	0.090
			(0.020)	(0.017)	(0.023)
Female	0.40	321.607	0.097	0.093	0.059
			(0.027)	(0.026)	(0.034)
18-39	0.65	263.953	0.065	0.063	0.051
			(0.020)	(0.018)	(0.027)
40-64	0.35	569.574	0.103	0.116	0.126
			(0.034)	(0.032)	(0.044)
Panel B. Spending by enrollee health status					
1st quartile predicted spending	0.26	91.316	0.066	0.058	0.078
			(0.021)	(0.020)	(0.029)
2nd quartile predicted spending	0.25	140.561	0.080	0.086	0.084
			(0.022)	(0.022)	(0.032)
3rd quartile predicted spending	0.25	274.612	0.062	0.063	0.065
			(0.033)	(0.031)	(0.045)
4th quartile predicted spending	0.24	1,011.964	0.083	0.094	0.067
			(0.048)	(0.047)	(0.059)
Baseline Controls			X	X	X
Enrollee Controls				X	X
Plan Controls					X

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II for details on primary sample construction). Regressions are reweighted to balance the characteristics of the auto assignee and active choice Medicaid enrollee samples. The reweighting is done by defining cells at the age × sex × race × quartile of predicted spending level. The reweighted regression drops 6 observations due to lack of joint support. The independent variable is overall network breadth as measured by the normalized covered share of simulated visits. The dependent variable is log spending for Panels B and C. Columns 3 and 4 report the main two-stage least squares (2SLS) results from estimating Equation 5 for overall networks breadth with and without enrollee-level controls. Column 5 reports 2SLS results based on a model with plan fixed effects (see Appendix C) estimated on a broader sample that includes enrollees in the provider-owned plan. All standard errors are clustered at the county × month of assignment level.

Appendix Table 29—: Reweighted Estimates of the Impact of Overall Network Breadth on Potentially High-Value and Low-Value Care

	DV Mean (1)	2SLS† (2)	2SLS† (3)	2SLS† (4)
Panel A. Potentially high-value care				
Any high-value medical care (%)	11.729	0.861	0.862	0.785
		(0.187)	(0.174)	(0.241)
Any recommended preventive care (%)	2.137	0.169	0.176	0.192
		(0.064)	(0.062)	(0.087)
Any high-value prescription drugs (%)	9.343	0.382	0.315	0.262
		(0.167)	(0.141)	(0.221)
Any potentially high-value care (%)	17.697	0.897	0.811	0.808
		(0.215)	(0.195)	(0.288)
Panel B. Potentially low-value care				
Any imaging and lab (%)	13.462	0.377	0.374	0.935
		(0.176)	(0.167)	(0.225)
Any emergency department use (%)	5.362	−0.157	−0.127	−0.087
		(0.094)	(0.095)	(0.130)
Any avoidable hospitalizations (%)	0.407	0.015	0.012	−0.024
		(0.034)	(0.032)	(0.042)
Any designated low-value care (%)	0.207	−0.001	−0.003	−0.033
		(0.021)	(0.021)	(0.035)
Any potentially low-value care (%)	16.007	0.700	0.694	0.828
		(0.179)	(0.174)	(0.240)
Panel C. Satisfaction				
In assigned plan (%)	94.211	1.086	1.202	1.253
		(0.134)	(0.132)	(0.215)
Observations	295,722	295,722	295,722	349,038
Baseline Controls	—	X	X	X
Enrollee Controls	—		X	X
Plan Controls	—			X

Notes: Standard errors in parentheses. Results are based on my primary sample (see Section II for details on primary sample construction). Regressions are reweighted to balance the characteristics of the auto assignee and active choice Medicaid enrollee samples. The reweighting is done by defining cells at the age × sex × race × quartile of predicted spending level. The independent variable is overall network breadth as measured by the normalized covered share of simulated visits. The dependent variables include specific high-value and low-value services, and an ex-post demand measure of enrollee satisfaction. Panel C presents reduced form, rather than 2SLS, estimates as they measure the likelihood that enrollees remain in their assigned plans. Columns 2 and 3 report the main two-stage least squares (2SLS) results from estimating Equation 5 for overall networks breadth with and without enrollee-level controls. Column 4 reports 2SLS results based on a model with plan fixed effects (see Appendix C) estimated on a broader sample that includes enrollees in the provider-owned plan. All standard errors are clustered at the county × month of assignment level.