How does managed care manage care? Evidence from public insurance

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A dissertation presented

by

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to

The Harvard Ph.D. Program in Health Policy

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

Health Policy

Harvard University

Cambridge, Massachusetts

May 2016

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Abstract

In the United States, the fraction of individuals with public insurance is growing and the policies and markets that serve them are changing. Over two-thirds of Medicaid recipients are now enrolled in managed care organizations (MCOs), but little is known about how these plans operate. To study this market, I use data from New York Medicaid where managed care recipients are randomly assigned to plans. In chapter one, I estimate how physician and hospital networks impact health care use and spending. In chapter two, I study how production and selection vary across the managed care plans participating in New York Medicaid. Finally, in the third chapter, I turn to the Medicare program and examine how health care use and spending change at age 65 as adults switch from private to public coverage. Taken together, my dissertation chapters provide new perspective on how differences within public programs and between public and private programs, impact consumers.

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Acknowledgments

This research would not have been possible without the assistance of my dissertation committee. Thomas G. McGuire was a fount of wisdom, practical advice, and friendly ping pong competition throughout my graduate studies. Michael Chernew provided countless hours of mentoring and research advice and, though he knows it not, is partially responsible for my purchase of a Fitbit which has changed my life and my view of nudges. David Cutler patiently and relentlessly guided me through innumerable obstacles and my research and thinking have benefitted tremendously from our weekly conversations. Benjamin D. Sommers was generous with his time and encyclopedic knowledge of Medicaid, without which this dissertation would not have been possible. I consider myself extremely fortunate to have worked with and learned from such kind and thoughtful mentors. Collectively, your advice and encouragement pushed me to work harder to understand and, hopefully, contribute to solving the challenges our health care system faces. These chapters and the rest of my career will reflect that effort.

Other faculty also provided excellent advice and mentoring. I have been the beneficiary of years of conversations with Joseph P. Newhouse, who influenced my teaching, thinking and research, all for the better. I am also indebted to the broader Harvard health policy community, including: Chris Afendulis, Katherine Baicker, David Grabowski, John Hsu, Haiden Huskamp, Mary Beth Landrum, Ateev Mehrotra, Laura Hatfield, Barbara McNeil, Sherri Rose, Meredith Rosenthal, and Kathy Swartz. Your advice, suggestions and encouragement over the years have made this process more productive and more enjoyable. Also a big thank you to my co-authors, who provided the insight, support and encouragement needed to tackle complex questions in health economics.

My former colleagues and friends at New York State Medicaid provided critical insight and assistance. In 2007, Deborah Bachrach took a chance on me as a health care analyst and her passion for Medicaid and health policy continue to inspire me to this day. Greg Allen was a source of countless hours of mentoring, sage advice and encouragement. Perhaps more importantly, he also taught me how to fish. Alan Maughan provided daily insights into the inner workings of the Medicaid program that continue to serve me today. Chang Byun and Hyun Jae Kang provided assistance in accessing and interpreting the data.

I can't imagine what graduate school would have been like without the support and friendship of my fellow graduate students. I am especially grateful to have benefited from the wisdom of a great group of economists in training: Caitlin Carroll, Katherine Donato, Paul Goldsmith-Pinkham, Ellen Montz, Hannah Neprash, Nilesh Fernando, Abby Friedman, Daria Pelech, Tim Layton, Adam Sacarny, Aaron Schwartz, Mark Shepard, Zirui Song, Karen Stockley, Boris Vabson, Annetta Zhou and Eric Zwick. Special thanks to the amazing Harvard Ph.D. Program in Health Policy staff members, especially Debbie Whitney and Ayres Heller, who always made me feel welcome and fostered a sense of community among the graduate students in the program.

Generous funding was provided by the National Science Foundation Graduate Research Fellowship (Grant No. DGE 1144152), an Agency for Healthcare Research & Quality (AHRQ) Graduate Fellowship, and a Harvard Graduate School of Arts and Sciences (GSAS) Summer Research Grant.

To my parents, without whom this would (literally and figuratively) not have been possible. And to my wife, Julia, whose infectious passion for life and public service fueled me throughout this process.

Introduction

In the United States, the fraction of individuals with public insurance is growing and the policies and markets that serve them are changing. Over two-thirds of Medicaid recipients are now enrolled in managed care organizations (MCOs), but little is known about how these plans operate. To study this market, I use data from New York Medicaid where managed care recipients are randomly assigned to plans. In chapter one, I estimate how physician and hospital networks impact health care use and spending. In chapter two, I study how production and selection vary across the managed care plans participating in New York Medicaid. Finally, in the third chapter, I turn to the Medicare program and examine how health care use and spending change at age 65 as adults switch from private to public coverage. Taken together, my dissertation chapters provide new perspective on how differences within public programs and between public and private programs, impact recipients.

In chapter one, "Do Provider Networks Impact Health Care Use and Spending? Evidence from Random Assignment in Medicaid Managed Care," I use the random assignment of Medicaid recipients to health plans to assess the causal impact of provider network breadth on health care outcomes. A key distinction between the managed care plans that serve Medicaid recipients is the breadth of their provider networks (i.e. the number of providers they contract with). To study this, I use data from New York Medicaid where managed care recipients are randomly assigned to plans. Each plan contracts with a different set of physicians and hospitals, providing an ideal setting to study the impact of network breadth. To measure network breadth, I use estimates from a structural model of demand for physicians and hospitals. Combining these measures with administrative health records for over 100,000 randomly assigned Medicaid recipients, I find that the effects of physician and hospital networks differ significantly. Using variation in networks within plans, I find that broader physician networks are associated with increases in utilization and spending, fewer avoidable hospitalizations, and greater plan loyalty. Although also associated with greater plan loyalty, broader hospital networks have no effect on utilization or spending. Regulations that encourage broad networks should account for the tradeoff between improved access and higher spending.

In chapter two, “Are all Managed Care Plans Created Equal? Evidence from Random Plan Assignment in Medicaid,” (with Mike Geruso and Tim Layton) I open the black box of managed care and study how health plans competing in the same market may vary in their approaches—and ultimately in their ability—to constrain healthcare spending. I examine this issue in the context of Medicaid Managed Care in New York City, in which some beneficiaries make active plan choices across a large number of privately-operated managed care plans, and other beneficiaries are randomly auto-assigned to the same set of plans. I exploit the random assignment to identify plan effects that are purged of selection, which I show would be an important confounder in this setting. My findings reveal significant variation in both “production” and “selection” across ostensibly similar managed care plans competing in the same local market. Specifically, I find that plans differ in spending on identical beneficiaries by as much as 33%. These differences are even larger for enrollees with high baseline spending. I show that differences in negotiated upstream prices explain only a fraction (3-10%) of this difference, and that plan characteristics significantly affect the healthcare consumption of enrollees. These findings are important for the continued reform of healthcare, in which managed care is often touted as the single most important tool for constraining healthcare spending growth.

In chapter three, “Traditional Medicare Versus Private Insurance: How Spending, Volume, and Price Change at Age 65 for the Previously Insured,” (with Zirui Song) I examine how spending, volume and price differ between public and private insurance. To slow the

growth of Medicare spending, policymakers have advocated raising the Medicare eligibility age. Despite its policy importance, little is known about how this proposal would impact national health care spending. For the majority of affected adults, the proposal would delay entry into Medicare and leave them with private insurance. We examine how health care spending differs between Medicare and private insurance by exploiting longitudinal data on imaging and surgery for a national cohort of individuals that switch from private insurance to Medicare at age 65. Using a regression-discontinuity design, we find that spending falls by $38.56 per beneficiary per quarter (32.4%) upon entry into Medicare at age 65 ($P<0.001$). In contrast, we find no changes in volume at age 65. For the previously insured, entry into Medicare leads to a large drop in spending driven by lower provider prices, which may reflect Medicare's purchasing power as a large insurer. For the majority of adults affected, these findings imply that an increase in the Medicare eligibility age would raise national health care spending by replacing their Medicare coverage with private insurance where health care spending is higher due to higher provider prices.

Chapter 1

Do Provider Networks Impact Health Care Use and Spending? Evidence from Random Assignment in Medicaid Managed Care

1.1 Introduction

Do provider networks impact health care use and spending? Previous attempts to answer this important policy question have been hindered by limitations in data and study design. Using a randomized controlled design set in New York Medicaid, I present evidence that broader networks increase both access and spending.[1]

Access to health care providers is a perennial concern in Medicaid. With over two-thirds

[1] There are only two other large-scale randomized controlled trials that evaluate health insurance in the United States. In the 1970s the RAND Health Insurance Experiment evaluated the impact of cost-sharing using a large-scale randomized controlled trial in which approximately 6,000 individuals were assigned to plans with different cost sharing features (Newhouse, 1993; Manning *et al.*, 1987). More recently, the state of Oregon used a lottery to select which uninsured, low-income individuals would be eligible to apply for Medicaid. This randomization allowed the researchers to study the impact of insurance coverage itself on health, health care utilization and financial well-being (Finkelstein *et al.*, 2012b).

of Medicaid recipients enrolled in managed care organizations (MCOs), access for the Medicaid population now depends largely on the provider networks employed by these plans. Despite this, little is known about how these networks are formed or how they affect recipients. A typical justification for network restrictions is that they steer patients to high-value providers, thereby reducing cost. This reduction, however, comes at the expense of consumer choice. Moreover, policymakers and researchers have expressed concern that plans narrow their networks for other less socially desirable reasons, such as to attract healthier individuals (Frank *et al.*, 2000) or exploit consumer inattention (Corlette *et al.*, 2014). In response to these concerns, policymakers have set network adequacy requirements—rules for how many and what types of providers plans must contract with—but these standards vary widely by state (HHS, 2014). One explanation for this variation is a lack of evidence about how provider network breadth affects patients.[2]

In this paper, I use administrative health records and plan choice data for over 100,000 Medicaid recipients in New York State to estimate the causal impact of provider network breadth on health care use and spending.[3] The empirical strategy exploits the fact that New York randomly assigns a subset of Medicaid recipients to managed care organizations (MCOs). While the state enforces minimum network adequacy standards, there is significant variation in the physician and hospital networks offered by the different plans. As a result, otherwise identical recipients are assigned to different physician and hospital networks based on where they live and the plan they are assigned to. I use variation in those networks

[2]When regulating cost-sharing or benefit design, policymakers can draw on a large body of well-identified work (e.g. Manning *et al.*, 1987; Newhouse, 1993). When regulating networks, no such evidence exists. Gruber and McKnight (2014a) and Lo Sasso and Atwood (2015) do provide estimates of the impact of narrow networks, but they rely on plan-level variation rather than constructing network measures. However, state policymakers regulate networks by geography and provider type. Hence, they will benefit from estimates of the impact of provider networks that use measures which vary locally.

[3]New York's Medicaid program provides an ideal setting to study provider networks for several reasons. First, Medicaid MCOs in New York offer standard benefit packages. Second, the state collects and validates detailed administrative data on plans' provider networks, enrollment, and claims. Finally, Medicaid is of interest in its own right as it now covers more individuals (70 million) than any other insurance program in the United States. The New York Medicaid program covers over five million New Yorkers and is representative of the national trend toward managed care in Medicaid (Sparer, 2012). Hence, lessons from New York will help inform the regulation of Medicaid managed care plans nationally, a program that serves roughly 40 million low-income and disabled Americans (Medicaid 2013).

both within and across plans to separate the effect of networks from other characteristics that vary by plan.

Estimating the impact of provider networks has been hindered by three empirical challenges. First, measurement is difficult. Networks are multi-dimensional and measuring them requires taking into account provider type, location and quality. To date, these limitations have led researchers to focus on measuring hospital networks (Town and Vistnes, 2001; Gaynor and Vogt, 2003; Capps *et al.*, 2003; Ho, 2006), but physician networks are arguably more important, as my work demonstrates. Second, comparisons across individuals suffer from selection and endogeneity issues, as prior work establishes (Shepard, 2015). Third, researchers must separate the impact of networks from other plan characteristics which may be correlated with network breadth (Lo Sasso and Atwood, 2015).

The analysis proceeds in two steps. First, I construct measures of network breadth using techniques adapted from industrial organization (Town and Vistnes, 2001; Gaynor and Vogt, 2003; Capps *et al.*, 2003). Using a specification similar to Ho (2006), I estimate hospital and physician demand systems using micro-data on health care utilization. The main covariates are distance and provider characteristics. I include out-of-network providers in the choice set since nine percent of hospital and five percent of physician visits in the data are to out-of-network providers. Consistent with prior work, the model estimates a significant "hassle" cost for recipients that seek care from out-of-network providers (Shepard, 2015).[4] These estimates are combined with information on the frequency of health care use to produce network utility measures that capture the value of different health plan networks to consumers. Relative to past work, a key methodological contribution of this paper is to apply to physician networks methods previously used to measure hospital networks—a task made difficult by the large number of physicians relative to hospitals.

With these measures in hand, I use data from over 100,000 randomly assigned Medicaid

[4]Estimated hassle costs are significantly higher for physicians as compared to hospitals. The model implies a hassle cost for out-of-network physicians that reduces their share by 91% on average. For hospitals, the implied hassle cost is lower, only reducing their share by 48% on average. Plan officials indicated that recipients seeking care out-of-network must obtain "super-referalls" from their primary care providers or plans. This may be more likely in the hospital setting where specialty services are more likely to be unavailable in network.

recipients in New York City to estimate the impact of physician and hospital network breadth on health care use and spending. As noted earlier, a key empirical challenge is to control for the direct impact of health plans on the outcomes I study. MCOs may impact health care use directly in a number of ways, including unique provider payment arrangements or management techniques. If differences in these approaches are related to decisions about which providers to contract with, estimates of the impact of provider network breadth will be biased.[5] To address this, I include flexible controls for plan and zip code and estimate the impact of networks using the rich variation that remains at the plan-zip level. Intuitively, this identification strategy compares: (i) the outcomes of individuals within a zip code randomly assigned to different plans; and (ii) the outcomes of individuals in different zip codes randomly assigned to the same plan.

I examine the effects of provider network breadth on health care use, quality of care, and recipient satisfaction (as measured by plan loyalty).[6] After assignment, a one standard deviation increase (roughly equivalent to adding ten percent of the physicians in a market to the network) in the size of physician networks was associated with substantial and statistically significantly higher health care utilization and spending, improvements in health care access and an increase in recipient satisfaction. Broader physician networks are associated with a 14.3 log point (15%) increase in health care spending and a 2.7 percentage point (10%) increase in the probability of using health care, a reduction of 1.2 avoidable hospitalizations per 1000 recipient months (40%), and a 5.0 percentage point reduction (30%) in the probability of recipients switching plans after assignment.

In contrast, hospital network breadth is not associated with changes in utilization or spending. Estimated confidence intervals rule out an effect size larger than 1.5 percentage

[5]For example, Metroplus covers fewer hospitals on average than other MCOs, but it is also the only vertically-integrated plan. Metroplus is a wholly-owned subsidiary of the New York City Health and Hospitals Corporation (HHC), a public benefit corporation that operates the public hospitals and clinics in New York City.

[6]I remove pharmacy and dental services from the main analyses since pharmacy was carved out of the managed care benefit until October 2011 and dental is an optional service for MCOs. If MCOs elect not to cover dental, recipients access dental services through Medicaid fee-for-service. I also remove services carved out of the managed care benefit throughout the study period (primarily behavioral health). These services are analyzed separately.

points (5%) for the probability of using any health care and 10 log points (10%) in monthly spending. Both of these upper bounds are smaller than the estimated impact of physician networks. However, broader hospital networks are associated with increased plan satisfaction, with a one standard deviation increase in breadth (roughly equivalent to adding twenty percent of the hospitals in New York City to the network) leading to a reduction of 2.1 percentage points (15%) in the probability of recipients switching plans after assignment.

This paper extends the literature on provider choice and network measurement. The methods now in use can be traced back to the pioneering work of Town and Vistnes (2001) and Capps *et al.* (2003). They use discrete choice models to estimate consumer demand for hospital characteristics, including their preferences for specific hospitals. To approximate hospital bargaining power, they estimate each hospital's contribution to the expected utility of the overall hospital network. This measure was predictive of negotiated prices and hospital profits. Ho (2006, 2009) extended this work to study heterogeneity in insurer-hospital bargaining, finding that prestigious hospitals command large markups. Recent work found that consumers value broad networks when choosing plans and are willing to switch plans to retain access to preferred providers (Ericson and Starc, 2015; Shepard, 2015). In this paper, I make two contributions to this literature. First, I model demand for physicians and use the estimates to construct a measure of physician network utility. These are the first and only estimates of this kind. Second, I explore how plan switching (rather than initial choice) relates to physician and hospital network breadth. The results of this analysis offer insight into how consumers learn about and value networks.

This research is also related to recent work on the impact of narrow networks on health care use and spending. Gruber and McKnight (2014a) examine the impact of a premium subsidy offered to Massachusetts state employees who switch to narrow network plans and find evidence of reduced spending and utilization for the compliers. The authors attribute these effects to narrow networks, but due to the study design cannot control for other differences across the plans that may explain some of their results. For example, narrow network plans which must more likely to be health maintenance organizations (HMOs) than preferred provider organizations (PPOs). Lo Sasso and Atwood (2015) adopt a different approach. They examine data from a single insurer that operates in the small-group market, exploiting the fact that some of the employers the insurer works with offer their employees narrow network plans and others do not. Consistent with Gruber and McKnight (2014a), they find reduced spending and utilization associated with narrow networks. However, the identification relies on the strong assumption that employees at firms that offer narrow network plans are similar to employees at firms that do not.

The remainder of the paper is structured as follows. Section 1.2 provides background on the New York Medicaid program and its auto-assignment policy. Section 2.4 describes the primary data sources and Section 1.4 details the research design, including how I measure networks. Section 1.5 presents estimates of the impact of physician and hospital networks on health care use and spending. Section 1.6 concludes.

1.2 New York's Medicaid Auto Assignment Policy

New York experimented with managed care in Medicaid as early as 1967, expanding to a voluntary program in the 1980s and a mandatory program in the 1990s and 2000s. In New York, mandatory Medicaid managed care meant that most recipients were required to join a managed care plan. The shift to mandatory managed care took place via county-by-county "enrollment mandates." The mandates initially applied only to children and TANF adults, but were subsequently expanded to include disabled Medicaid recipients (Sparer, 2008).[7] New York still operates a Medicaid alternative to managed care, known as "Medicaid Fee-For-Service", in which the government is responsible for paying providers directly for Medicaid services.[8]

[7] The Temporary Assistance for Needy Families (TANF) program provides temporary financial assistance to pregnant women and families with dependent children.

[8] As of 2011, 23.3% of Medicaid recipients were enrolled in Fee-For-Service (FFS) in New York. FFS recipients either reside in counties that do not mandate managed care or are exempt/restricted from joining managed care. Examples of exempt recipients include Native Americans, those with a traumatic brain injury (TBI), or those with a chronic condition who are being treated by a specialist that does not accept payment from any of the managed care plans. Examples of recipients restricted from joining Medicaid managed care include recipients

New York encourages Medicaid recipients in mandatory managed care counties to actively choose their health plans. Recipients that fail to choose within the required timeframe are automatically enrolled in a plan, a policy known as "auto assignment".[9] When this happens, the enrollees and plans are notified of the effective date of enrollment. At this point, the enrollee has three months to switch plans without cause before a nine-month lock-in period begins. The details of New York's auto assignment policy depend on how much time has elapsed since mandatory managed care was instituted in a county. In the first year after mandatory managed care, 25% of auto assignments are randomly distributed to Prepaid Health Service Plans (PHSPs), plans that primarily serve beneficiaries of government health programs.[10] The remaining 75% are randomly distributed to all plans. In subsequent years, the share of recipients automatically assigned to PHSPs declines to zero percent and the state introduces performance-based assignment that, by the fourth year of mandatory managed care, accounts for 100% of auto assignments. 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.[11] Performance-based assignments are distributed randomly to the plans that qualify each year.

There are two exceptions to the auto assignment policies described above. First, New York takes into account where family members are enrolled. If family members are enrolled

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enrolled in Medicare, recipients in the Medicaid spend-down or excess income programs, recipients receiving hospice services at the time of enrollment, or children in foster care. Many of these exemptions/restrictions were lifted subsequently.

[9] As of October 1, 2011 newly mandated Medicaid recipient had 30 days to choose a plan, regardless of disability status. Prior to this policy, Supplemental Security Income (SSI) and SSI-related recipients had 90 days and non-SSI/SSI-related recipients had 60 days to choose a plan prior to auto-assignment. The authority for auto-assignment is granted pursuant to N.Y. Soc. Servs. L. §364-j(4)(f)(i).

[10] A PHSP is a managed care organization specifically authorized by New York State law. These plans are subject to the same structure and operating criteria as HMOs, but at least 90% of the enrollees must be beneficiaries of government health programs such as Medicaid. The total number of PHSPs is limited by law.

[11] Prevention Quality Indicators (PQIs) are a set of measures developed by the Agency for Healthcare Research and Quality to evaluate the quality of care for "ambulatory care sensitive conditions". These are conditions for which good outpatient care can prevent hospitalizations or complications.

in a managed care plan at the time of auto assignment, the recipient defaults to the family member's plan. For recipients without family members in managed care, there is a second exception to auto assignment. This exception applies to recipients that were enrolled in a managed care plan in the year prior to assignment. These recipients are reassigned to their previous plan.[12] Recipients assigned on the basis of family members or prior enrollment are removed from the analysis.[13]

1.3 Data

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

1.3.1 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 2007 to 2012.[14] For each recipient, I observe demographic data, monthly enrollment data and claims for medical services covered by Medicaid.[15] The enrollment data include an indicator that I use to identify recipients

[12]Preferential assignment to a prior plan does not apply if the recipient's prior plan was a partial capitation plan, a low quality plan, or a plan without further capacity.

[13]Auto assignments on the basis of family members of prior enrollment are not separately identified in the data. I adopt a conservative approach to remove these recipients, dropping individuals with family members on file at the time of auto assignment or any managed care enrollment in the year prior to auto assignment.

[14]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 recipients.

[15]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).

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. 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.

1.3.2 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 recipients.[16] 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

[16] 42 CFR §§438.310-370.

network data with fee-for-service claims and managed care encounter data. While the PNDS data is reported quarterly, I construct an indicator for 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 us to isolate 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.[17] 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 recipients. 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.

1.3.3 Key Outcome Measures

The available data allows me to construct a range of recipient-level, monthly outcome measures. These measures are grouped into three domains: health care use and spending, access to care, and recipient satisfaction.

Health care use and spending. To measure health care spending for the Medicaid managed care population I use variables provided by the New York State Department of Health (NYSDOH) to construct the following categories of service: physician office, hospital outpatient, emergency department, hospital inpatient, and other. The other category includes independent laboratory, transportation, diagnostic & treatment centers, and other

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

miscellaneous services.[18]

Because monthly health care spending is a highly skewed limited dependent variable, I use log expenditures and conduct robustness tests using general linear models (GLM) and two-part models similar to past work on health care expenditures (Manning *et al.*, 1987; Newhouse, 1993; Buntin and Zaslavsky, 2004). For the two-part model, the extensive margin (first part) is an indicator variable for whether or not an individual had any medical claims in a month, estimated using a Probit model. The intensive margin (second part) is a continuous measure of monthly health care spending conditional on an individual having had at least one medical claim in a month. The impact of network size on the intensive margin is estimated using a general linear model (GLM) with a log-link function.[19]

Access to Care. I measure access by adapting measures developed by the Secretary of Health and Human Services (HHS) specifically for adult Medicaid enrollees, a requirement of the Affordable Care Act (Section 113B). I focused on measures that could be constructed using administrative data since I do not have access to individual-level survey data or electronic medical records. I examined two domains of access: compliance with recommended preventive care and the frequency of avoidable hospitalizations.[20] For preventive care, I examine the frequency of flu vaccination for adults ages 18 to 64, breast cancer screening, cervical cancer screening and chlamydia screening in women. For avoidable hospitalizations, I examine the admission rates for four conditions: (i) diabetes short-term complications; (ii) chronic obstructive pulmonary disease (COPD) or asthma in adults; (iii) heart failure and (iv) asthma in younger adults.

A common concern related to network adequacy is the time or distance patients must

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[18]I remove pharmacy and dental services from the main analyses since pharmacy was carved out of the managed care benefit until October 2011 and dental is an optional service for MCOs. If MCOs elect not to cover dental, recipients access dental services through Medicaid fee-for-service. I also remove services carved out of the managed care benefit throughout the study period (primarily behavioral health).

[19]For inference with the two two-part model, these estimates are combined and standard errors are calculated using the delta method. Robustness tests are available upon request.

[20]My limited sample and frequent churning in Medicaid managed care makes it difficult to implement the lookback periods for each measure. To simplify, I instead measure the frequency with which recipients use the recommended preventive care measures.

travel to providers. I measure access—as proxied by distance traveled—for four categories of service: physician office, hospital outpatient departments, emergency department, and hospital admissions. For each of these services I measure distance as the driving time (in minutes) from the centroid of the recipient's home zip code to the centroid of the providers zip code. For providers with multiple locations of service I use the modal zip code (since I don't observe the specific zip code reliably on each claim).

Recipient Satisfaction. The final outcome I study is satisfaction as proxied by plan loyalty. Here, I assume recipients' preferences are revealed through their subsequent plan choices ("voting with their feet"). This is possible because randomly assigned recipients are not "locked-in" to their assigned plans. For the first three months after assignment they may switch for any reason, after which a nine-month lock-period begins. Hence, they have ample opportunity to switch plans if they are dissatisfied.[21]

1.3.4 Sample Selection

I restrict my estimation sample in six ways. First, I drop recipients that live outside the five boroughs of New York City. Approximately one-third (35%) of Medicaid recipients reside outside New York City (either in Long Island or "upstate"). The focus on New York City allows us to identify the impact of provider networks while controlling flexibly for geography with zip code fixed effects.[22] Second, I restrict the sample to recipients 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 managed care (Vabson, 2015). I remove recipients 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 recipients with family members enrolled in a Medicaid managed care plan at the time of auto assignment. Since preference is given to plans that enroll family members,

[21]Moreover, recipients can switch plans during the lock-in period for good cause.

[22]The randomly assigned Medicaid recipients in my sample reside in 170 different zip codes in New York City.

these auto assignments are non-random. Fourth, I exclude individuals enrolled in managed care plans within a year prior to assignment since they are preferentially placed into their prior plans. Fifth, to keep the sample reasonably well-balanced I exclude recipients auto-assigned to plans that were not in operation throughout the study period. Sixth, I remove individuals who qualify for Medicaid because they receive Supplemental Security income (SSI).[23] Excluding disabled Medicaid recipients makes the sample more comparable to the general population.

These sample restrictions leave me with 104,885 recipients in five boroughs and ten plans. The final sample includes 285 county by year by month observations. Since the unit of randomization is the county by year by month all analyses are clustered at this level. The median number of patient-month observations in each cluster is 5,211.

Because loss of Medicaid coverage is common and recipients can switch plans on a monthly basis, our main specification uses months as the unit of analysis. Since recipients exit and re-enter Medicaid during the study period, I define each continuous enrollment period as a separate "eligibility episode." Recipients that are auto assigned are included in the sample until the eligibility episode in which they were assigned ends. Additional eligibility spells are only included if the recipient is auto assigned multiple times.[24] For some eligibility episodes, recipients drop out of managed care and return to fee-for-service for brief periods. For our main analyses, I include these recipients but exclude spending and utilization during the fee-for-service months.

[23]Supplemental Security Income (SSI) is a federal government program that provides income to low-income individuals that are aged (65 or older), blind, or disabled. To qualify as "disabled" individuals must be unable to participate in gainful employment by reason of a physical or mental impairment expected to result in death or last for a continuous period of 12 months or more.

[24]This is rare and the results are not sensitive to the exclusion of these recipients.

1.4 Research Design

Consider a model that relates recipient outcomes such as health care use to measures of the physician and hospital network size:

$$Y_{it} = \alpha + \beta X_i + \delta \text{Physician}_{it} + \gamma \text{Hospital}_{it} + \epsilon_{it} \tag{1.1}$$

where i denotes recipients, t is time, Physician_{it} is a measure of physician network size, Hospital_{it} is a measure of hospital network size, δ is the causal impact of physician network size, γ is the causal impact of hospital network size, X_i includes controls such as age, race and gender, and ϵ_{it} is noise. The problem for inference is that OLS estimates of δ and γ may be biased if a recipients' network is correlated with unobservable determinants of the outcome: $E[\epsilon_{it}|\text{Physician}_{it}] \neq 0$ or $E[\epsilon_{it}|\text{Hospital}_{it}] \neq 0$. One possibility is that individuals who expect to consume a lot of health care choose plans with broader networks, biasing our estimates of δ and γ in equation (1.1). To address selection, I instrument for physician and hospital network size using the network of the randomly assigned plan.

A second empirical challenge is to separate the impact of networks from other plan features. Medicaid MCOs in New York adopt different strategies to serve the Medicaid population and these are correlated with the networks they construct. For example, Metroplus—one of the largest Medicaid MCOs in New York City—is owned by the local safety net hospital chain, Health & Hospitals Corporation (HHC), and restricts its hospital network to HHC hospitals and a small number of other facilities.[25] To separately identify network and plan effects, I include plan fixed effects in all specifications and use variation within plans between zip codes to identify the impact of network breadth.

Since auto assignment is not binding I estimate the causal impact of network size through a two-stage least squares regression that uses the assigned plan and network (which varies within plan between zip codes) to instrument for plan and network. The second stage

[25]If consumers randomly-assigned to Metroplus consume fewer hospital services is this attributable to a narrower hospital network, differences in organizational incentives due to vertical integration or some other unobserved plan feature?

estimating equation is:

$$Y_{it} = \alpha + \beta X_i + \phi_{ct} + \zeta_z + \text{Plan}_j + \delta \text{Physician}_{jzt} + \gamma \text{Hospital}_{jzt} + \epsilon_{izjct} \tag{1.2}$$

where i denotes recipients, z is the zip code, j is the plan, c is the county, t is time in months, ϕ_{ct} is a set of county by year by month of assignment dummies, ζ_z contains zip code dummies, X_i includes individuals controls such as race, age, gender and a baseline measure of the outcome being studied, Plan_j is a vector of plan fixed effects, δ is the causal impact of physician network size, γ is the causal impact of hospital network size, and ϵ_{izjct} is noise. Given J plans, there are $J+2$ first stage estimating equations associated with equation (2.13). Since there are ten plans that receive auto assignments in multiple years of the study period, I estimate the following first stage equations for plan:

$$\begin{aligned}
\text{Plan}_{1t} &= \phi_{1ct} + \zeta_{1z} + \beta_1 X_i + \sum_{j=1}^{10} \pi_{1j} \widetilde{\text{Plan}}_i^j + \pi_{1np} \widetilde{\text{Physician}}_{jzt} + \pi_{1nh} \widetilde{\text{Hospital}}_{jzt} + \epsilon_{iz1ct} \\
&\vdots \\
\text{Plan}_{10t} &= \phi_{10ct} + \zeta_{10z} + \beta_{10} X_i + \sum_{j=1}^{10} \pi_{10j} \widetilde{\text{Plan}}_i^j + \pi_{10np} \widetilde{\text{Physician}}_{zjt} + \pi_{10nh} \gamma \widetilde{\text{Hospital}}_{jzt} + \epsilon_{iz10ct}
\end{aligned} \tag{1.3}$$

where Plan_{1it} is an indicator that recipient i is enrolled in plan 1 at time t, $\widetilde{\text{Plan}}_i^j$ is a dummy variable that indicates individual i was auto assigned to plan j, $\widetilde{\text{Physician}}_{zjt}$ and $\widetilde{\text{Hospital}}_{jzt}$ are measures of the size of the assigned physician and hospital networks for individual i in zip code z at time t (even if individuals switch plans the assigned network measures are always based on the network of the plan assigned). For plan 1, π_{1j} captures the effect of the plan instruments and π_{1np} π_{1nh} captures the effects of the network instruments on plan choice. I also estimate two first-stage equations for network size. For physician network size I estimate

$$\text{Physician}_{jzt} = \alpha_p + \beta_p X_i + \phi_{p,ct} + \zeta_{p,z} + \widetilde{\text{Plan}}_{p,j} + \pi_{pp} \widetilde{\text{Physician}}_{jzt} + \pi_{ph} \widetilde{\text{Hospital}}_{jzt} + \epsilon_{it} \tag{1.4}$$

and for hospital network size I estimate

$$\text{Hospital}_{jzt} = \alpha_h + \beta_h X_i + \phi_{h,ct} + \zeta_{h,z} + \widetilde{\text{Plan}}_{h,j} + \pi_{hp}\widetilde{\text{Physician}}_{jzt} + \pi_{hh}\widetilde{\text{Hospital}}_{jzt} + \epsilon_{it} \quad (1.5)$$

where Physician_{jzt} and Hospital_{jzt} are scalars that vary based on zip code, plan, and time. Here, the parameters of interest are π_{pp} and π_{hh} which capture the first-stage effects of the physician and hospital network instruments on the size of recipients actual networks after assignment (allowing for switching). 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-year-month of assignment level in both the first and second stage regressions.

1.4.1 Network measure construction

In this section, I outline my approach to measuring the breadth of physician and hospital networks. There is not a consensus on how to measure network breadth, or provider access more generally. I use methods from the hospital merger literature (Town and Vistnes, 2001; Gaynor and Vogt, 2003; Capps *et al.*, 2003) to infer the breadth of provider networks based on recipient's physician and hospital choices. The measure I construct varies by zip code based on the distance to nearby providers and differences in provider quality. In addition, for each plan the measure takes into account which providers are in-network. To account for differences in provider type, I do this separately for physicians and hospitals.

In the first step I assess how important different provider characteristics are to recipients using discrete choice models of the demand for physicians and hospitals. In the second step, these estimates are combined with data on the frequency of health care use for Medicaid recipients to measure the expected utility from the networks offered by each plan. Since networks differ over time and between zip codes, this measure varies at the plan-zip-year level. Relative to past work, the main innovation is to extend the methods used in the hospital setting to measure physician networks. To provide a transparent base for comparison, I extend methods from Ericson and Starc (2015) to construct a second network measure that is based on the share of total claims or discharges in a zip code covered by each plan's network (for more details see Appendix Section A.1).

To construct the hospital network measure, I use micro-data on inpatient hospitalizations to estimate a model of hospital choice (Ho, 2006).[26] The main covariates are distance and hospital characteristics, which are allowed to vary with patient observables in some specifications to capture preference heterogeneity. Unlike past work, I do not include coinsurance (or hospital prices) as covariates since Medicaid recipients in New York are not charged cost sharing for hospital admissions.[27] Following Shepard (2015) I include out-of-network hospitals in the choice set. This is appropriate in New York Medicaid where recipients can get a "super referall" to see an out-of-network provider and approximately nine percent of hospital admissions are out-of-network. To capture the cost of seeking care from out-of-network providers, I include an out-of-network hassle cost term in the hospital choice model.

With some probability consumer i in plan j is hospitalized with diagnosis d. Their utility from visiting hospital h at time t is given by:

$$u_{i,j,d,t,h} = \underbrace{\delta(\text{Dist}_{i,h} \times Z_{i,d,t})}_{\text{Distance}} + \underbrace{\lambda(X_h \times Z_{i,d,t}) + \xi_h}_{\text{Hospital Characteristics}} + \underbrace{\psi_j \cdot 1\{h \notin N_{j,t}\}}_{\text{Out-of-Network Cost}} + \epsilon_{i,d,t,j,h} \tag{1.6}$$

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), and $1\{h \notin N_{j,t}\}$ 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.

[26]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 from the dataset had little effect on her coefficients.

[27]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.

The model is estimated using maximum likelihood. Table 1.1 shows the 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 for out-of-network 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 recipient observables, plan-specific out-of-network hassle costs and hospital fixed effects. In both cases, the model fit is good.[28] 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.[29] 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 71% on average. The model also estimates a hassle cost for out-of-network hospitals that reduces their share by 77% on average.[30] Appendix Table A.4 provides a list of the largest 25 hospital fixed effects recovered in the model, sorted by magnitude. As expected, the top hospitals tend to be large academic medical centers (e.g. Montefiore Medical Center and New York-Presbyterian Hospital).

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 (2015), is that if recipients 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 (1.6) using a sample of recipients randomly assigned to their plans. The results of this analysis are presented in Appendix Table A.5. The estimated hassle cost is now lower, suggesting the original estimates were biased by selection on unobservable hospital preferences. However, the model still estimates a negative and statistically significant hassle

[28] McFadden's R^2 for the basic and full models are 0.399 and 0.405, respectively. Both indicate good fit (McFadden, 1977).

[29] I will test the sensitivity of the results to the model specification by re-running the main analyses using network measures constructed with the full model. I anticipate that the findings will be qualitatively similar.

[30] A reduction that is similar in magnitude to what was reported for commercially-insured, low-income residents in Massachusetts (Shepard, 2015). The table also presents the largest coefficients for hospital service x diagnosis interactions in the full model; the remaining coefficients are mostly significantly positive.

Table 1.1: *Hospital Choice Model*

	Simple Model		Full Model	
	Coeff.	Std. Error	Coeff.	Std. Error
	(1)	(2)	(3)	(4)
Distance to Hospital				
Distance (Minutes)	-0.416^{***}	(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			-0.130^{***}	(0.008)
Distance x Mental Illness			0.056^{***}	(0.009)
Distance x Circulatory			-0.024^{**}	(0.011)
Distance x Digestive			-0.050^{***}	(0.018)
Out-of-Network Disutility				
Out-of-Network x Affinity	-1.574^{***}	(0.008)	-0.940^{***}	(0.022)
Out-of-Network x Amerigroup	*Single Coefficient*		-1.004^{***}	(0.024)
Out-of-Network x Healthfirst			-1.309^{***}	(0.026)
Out-of-Network x Health Plus			-1.126^{***}	(0.021)
Out-of-Network x HIP			-0.556^{***}	(0.084)
Out-of-Network x United			-1.136^{***}	(0.043)
Out-of-Network x Metroplus			-2.086^{***}	(0.013)
Out-of-Network x Neighborhood			-1.548^{***}	(0.027)
Out-of-Network x Fidelis			-0.897^{***}	(0.029)
Out-of-Network x Wellcare			-0.572^{***}	(0.034)
Hospital Characteristics				
Hospital Fixed Effects	✓		✓	
Pregnancy x Obstetrics			2.325^{***}	(0.029)
Injury x Trauma Center			0.568^{***}	(0.018)
Mental Illness x Psych			0.339^{***}	(0.023)
Circulatory x Card Surg			0.298^{***}	(0.017)
Circulatory x Cath Lab			0.135^{***}	(0.016)
Model Statistics				
Pseudo R-Squared (McFadden)	0.399		0.405	
Choice Instances	697,803		697,803	

Notes: This table reports results from the multinomial logit hospital choice model described in section 4.A. 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. In addition to these new variables, the full model also includes distance (and distance-squared) interacted with five-year age x gender bins. The logit coefficients shown are interpretable as entering the latent utility function in equation (1.8). *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

cost that reduces out-of-network hospital's shares by 48% on average.

I now turn to modeling the demand for physicians, using micro-data on physician office visits for Medicaid MCO recipients to estimate a model of how patients choose doctors. Similar to the methods and specification used to model hospital demand, the main covariates are distance, physician characteristics and network status.

The method and specification for estimating physician demand differ from the hospital model in two ways. First, due to the large physician choice set 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).[31] Instead, I estimate separate physician demand models in each of forty-two neighborhoods, pooling data across the study period. For each neighborhood, I estimate fixed effects for the largest five percent of practices serving the recipients 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.[32] The remaining physicians are undifferentiated in the model beyond their observed characteristics.[33]

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 select one random alternative (in addition to the chosen physician) and proceed with the estimation using these subsets.[34] To estimate the model I assume that with some probability consumer i in neighborhood n enrolled in plan j seeks out a physician for services s. Their

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[31]Relative to the 63 hospitals that serve Medicaid recipients in New York City, the physician choice set includes 22,983 doctors (even after I exclude doctors with fewer than five claims.

[32]One limitation of this approach is that the designation of large practice is based on the data, since practice volume is based on both observed and unobserved characteristics. Unfortunately, the available data on physicians do not include exogenous measures of practice size. Given the skewness in the distribution of claims across physicians it is likely that some physician identifiers are being used to bill for multiple physicians. See Appendix Figure A.1.

[33]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.

[34]McFadden (1978) demonstrates that the likelihood function for multinomial logit with a subset of alternatives reduces to the 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.

utility from visiting physician p at time t is given by:

$$u_{i,j,s,t,p,n} = \underbrace{\delta_n(\text{Dist}_{i,p} \times Z_{i,s,t})}_{\text{Distance}} + \underbrace{\lambda_n(X_p \times Z_{i,s,t}) + \xi_{p,n}}_{\text{Physician Characteristics}} + \underbrace{\psi_n \cdot 1\{p \notin N_{j,t}\}}_{\text{Out-of-Network Cost}} + \epsilon_{i,s,t,j,p,n} \tag{1.7}$$

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),[35] 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.

Since all the covariates are observed, I estimate the model using maximum likelihood. Table 1.2 shows the results. Unlike the hospital choice model, I estimate the physician model without plan-specific hassle costs.[36] Columns (1) and (2) report results for comparison from the simple model which includes distance (and distance squared), an indicator for out-of-network and physician fixed effects for large practices. Columns (3) and (4) report the result of the full model, which includes distance (and distance squared) interacted with services and recipient observables, plan-specific out-of-network hassle costs and physician fixed effects for large practices. Similar to the hospital setting, there is a disutility associated with travel distance. The estimates imply that an extra 10 minutes travel time reduces a physician's share by 48% on average. The model also estimates a significant hassle cost for out-of-network physicians that reduces their share by 92% on average, a reduction that is significantly larger than in the hospital setting. Finally, the table presents the largest coefficients for physician characteristics x service interactions; the remaining coefficients are mostly significantly positive.

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

[36]Preliminary analyses found minimal differences across plans in the hassle cost for physicians.

Table 1.2: *Physician Choice Model*

	Simple Model		Full Model	
	Avg. Coef.	# Sig.	Avg. Coef.	# Sig.
	(1)	(2)	(3)	(4)
Distance to Physician				
Distance (Miles)	-0.206^{***}	42	-0.199^{***}	42
Distance Squared	0.002^{***}	42	0.000^{***}	42
Distance x DME			-0.033^{***}	18
Distance x Imaging			0.024^{***}	33
Distance x Eval. and Manage. (E&M)			-0.026^{***}	40
Distance x Procedures			0.003^{***}	27
Distance x Test			0.006^{***}	30
Out-of-Network Disutility				
Out-of-Network	-2.861^{***}	42	-2.808^{***}	42
Physician Characteristics				
Large Practice Fixed Effects	✓		✓	
DME x Optometry			3.654^{***}	38
Imaging x Radiology			3.231^{***}	42
Procedure x Phys. Med.			2.464^{***}	42
Procedure x Dermatology			1.838^{***}	42
Tests x Pathology			1.494^{***}	42
Tests x OB/GYN			1.419^{***}	42
Tests x Cardiology			1.363^{***}	42
Tests x Urology			1.133^{***}	42
E&M x Allergy			0.906^{***}	41
E&M x Primary Care			0.742^{***}	42
E&M x Ophthalmology			0.669^{***}	42
Neighborhoods	42		42	

Notes: This table reports results from 42 multinomial logit physician choice models for each neighborhood in New York City described in section 4.A. Columns 1 and 2 report the average coefficients and the number of neighborhoods for which the coefficient was significant for a simple physician choice model. Columns 3 and 4 report average coefficients and the number of neighborhoods for which the coefficient was significant for a full physician choice model which includes interactions of distance with service and physician characteristics with service. In addition to these new variables, the full model also includes distance (and distance-squared) interacted with five-year age x gender bins. The logit coefficients shown are interpretable as entering the latent utility function in equation (1.7). *** = significant at 1 percent level in at least one neighborhood, ** = significant at 5 percent level in at least one neighborhood, * = significant at 10 percent level in at least one neighborhood.

Similar to the hospital analysis, I test for bias in the hassle cost by re-estimating the model in equation (1.7) at the city-level using a sample of recipients that made active choices and a sample of recipients randomly assigned to their plans. The results are presented in Appendix Tables A.6 and A.7. Unlike the hospital setting, the hassle cost is similar for the active choice and random assignment samples. The model estimates that being out-of-network reduces a physician's share by 91% on average in the random assignment sample (as compared to 92% in the active choice sample). The likeliest explanation for this is that there is less selection on unobservable preferences in the physician setting.

I use the estimated coefficients from the physician and hospital choice models to predict the utility provided by each plan's network for each consumer type. In the hospital case, for example, I follow Ho (2006, 2009) and Shepard (2015) and define the expected utility of the network for an individual in plan j hospitalized with diagnosis d as:

$$\text{HospitalEU}_{i,d,t,j} \equiv E[\max_h(V_{i,j,d,t,h}(N_{j,t}) + \epsilon_{i,d,t,j,h})] = \log\left(\sum_h \exp(V_{i,j,d,t,h}(N_{j,t}))\right) \quad (1.8)$$

where representative utility $V_{i,j,d,t,h}(N_{j,t})$ is defined as $u_{i,j,d,t,h} - \epsilon_{i,d,t,j,h}$. Since rates of hospitalization vary systematically by consumer type and diagnosis I multiply $\text{HospitalEU}_{i,d,t,j}$ by the probability consumer i is hospitalized with diagnosis d:

$$\text{Hospital}_{i,t,j} = \sum_d p_{i,d} \times \text{HospitalEU}_{i,d,t,j} \quad (1.9)$$

where $p_{i,d}$ is the probability that consumer i is hospitalized with diagnosis d. Equation (1.9) yields a measure of the hospital network for each consumer type x zip code x plan x year. The measure incorporates both observable and unobservable hospital characteristics, including quality, distance (and distance squared) as well as between patient diagnoses and hospital services. Because the scale of network utility is arbitrary I normalize the measure to have mean zero and standard deviation one.

Similarly, I construct physician network utility as:

$$\text{Physician}_{i,t,j,n} = \sum_s p_{i,s} \times \text{PhysicianEU}_{i,s,t,j,n} \quad (1.10)$$

where $p_{i,s}$ is the probability that consumer i receives services s from a physician in neighborhood n. Unlike in the hospital setting where there is a single primary diagnosis, consumers may receive multiple services during the course of a single physician visit. Similar to the hospital setting, the scale of network utility is arbitrary so I normalize the measure to have mean zero and standard deviation one.

Appendix Table A.1 reports the average value of the network utility measures for each managed care organization in the sample. For comparison, I include an alternative measure of network breadth that is based on the shared of physicians or hospitals, weighted by volume, that each plan covers (the "covered share" measure). One limitation of the covered share measure is that it does not include physicians that don't see Medicaid patients (all hospitals saw some Medicaid patients). Hence, the physician covered share measures are inflated relative to what they would be if all New York City physicians were accounted for. With this caveat in mind, the descriptive statistics show that the average Medicaid MCO covered 69.3% of Medicaid physicians and 78.1% of hospitals in New York City. The network utility and covered share measures track each other closely (particularly for physician networks).

To help visualize the MCO networks, Appendix Figures A.2 and A.3 map relative network breadth in each New York City zip code for a subset of the MCOs in 2012. I residualize network breadth on zip code before mapping, since geographic variation would otherwise dominate comparisons across plans. What emerges from these figures is the fact that some MCOs operate networks that are similar in breadth (either consistently broader or narrower than other plans), while others operate networks that are relatively broad in some zip codes and relatively narrow in others. This variation is crucial to the identification of the network parameters in equation (2.13).

1.4.2 Specification Checks

Using my measure of network breadth and indicators for plan assignment to instrument for network breadth and plan, the identified two-stage least squares parameters from equation

(2.13) measure the local average treatment effect of physician and hospital network breadth for recipients whose outcomes are altered by auto assignment. The conditions necessary to interpret these two-stage least squares estimates as the causal impacts of network size are: (1) the assigned network size is associated with the actual networks recipients experience, (2) assignment only impacts recipient outcomes through its impact on network size, and (3) the impact of network assignment is monotonic across recipients.

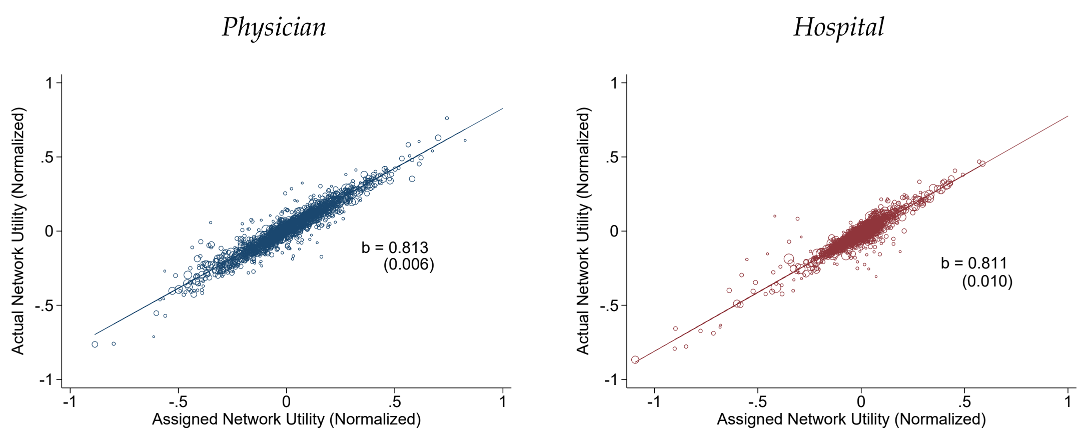

Figure 1.1: *First Stage: Assigned and Actual Network Utility*

Notes: These figures plot recipients' actual physician and hospital network utility against their assigned physician and hospital network utility. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months). To construct the binned scatter plot, I separately regress assigned and actual network utility against our base controls: recipient zip, half year fixed effects (based on time since assignment), five-year age x gender bins, race bins, plan assignment, and county x year x month of assignment. I then take the mean residuals in each plan by zip bin, adding the mean network size (actual or assigned) back in to facilitate interpretation of the results. Bins with fewer than 100 observations are omitted. The solid line is the best linear fit estimated on the micro data using OLS. The coefficient shows the estimated slope of the best-fit line including our base controls, with standard errors in parentheses clustered at the county by year by month level, the unit of randomization.

The first assumption is empirically testable. Figure 1.1 plots the actual network size vs. the assigned network. The estimation sample includes the 104,885 Medicaid recipients in New York City that are auto assigned to managed care plans from April 2008 to December 2012. To construct the plot, I residualize both the instrumented network size and the actual network size on the full set of controls. I then take the average residual by plan-zip (the primary source of variation) and add back in the overall means to aid in interpretation. The line of best fit and corresponding coefficient show the best linear fit estimated on the micro-data, controlling for demographics, plan assignment, zip code and county-year-month of assignment with standard errors clustered at the county-year-month of assignment level. Table 2.2 presents analogous recipient-level estimates with and without plan controls. To examine heterogeneity in the first stage, Appendix Figures A.4 and A.5 plot the assigned physician and network measures against actual provider networks separately by gender, age and race. The trends are similar across subgroups.

The second assumption is that the assigned network breadth only impacts recipient outcomes through its impact on actual network breadth. If assigned network breadth is correlated with unobservable determinants of the outcomes I study, my estimates will be biased. Table 1.4 presents a series of randomization tests to partially assess this exclusion restriction. Columns 2 and 3 present the estimates from regressions of baseline characteristics on the network instruments, with controls for plan, zip code and county by year by month of assignment, the unit of randomization. Using this specification, none of the baseline variables are significantly related to the assigned physician or hospital networks at the five percent level. Column 4 presents results from an additional test of randomization. Each baseline characteristic is regressed on the full set of plan fixed effects. Each regression controls for county by year by month of assignment fixed effects. I report the p-value from an F-test that the plan effects are jointly different than zero, which is a test of the null hypothesis that the baseline characteristics do not differ significantly among recipients assigned to different plans. None of the baseline variables are significantly related to plan assignment at the five percent level. The joint F-tests for male and inpatient utilization were significant at the ten percent level, with p-values of 0.055 and 0.051, respectively. For comparison, a similar set of tests were performed on a sample of Medicaid recipients that made active plan choices. For recipients that made active choices the results imply considerable selection on plan and, within plan, on network breadth (See Appendix Table

Table 1.3: *First Stage Estimates of the Impact of Assigned Network Size on Actual Network Size*

	Actual Physician Network		Actual Hospital Network	
	(1)	(2)	(3)	(4)
Assigned Physician Network	0.842***	0.813***	0.014***	0.006*
	(0.004)	(0.006)	(0.002)	(0.003)
Assigned Hospital Network	0.010**	−0.017**	0.828***	0.811***
	(0.004)	(0.008)	(0.006)	(0.010)
Assigned Plan: Amerigroup		0.001		−0.002
		(0.004)		(0.004)
Assigned Plan: Health First		0.030***		0.032***
		(0.005)		(0.004)
Assigned Plan: Health Plus		0.062***		0.004
		(0.008)		(0.004)
Assigned Plan: HIP		0.036***		0.030***
		(0.004)		(0.004)
Assigned Plan: Metroplus		−0.020***		−0.003
		(0.007)		(0.007)
Assigned Plan: Neighborhood		0.004		0.004
		(0.003)		(0.003)
Assigned Plan: Fidelis		0.049***		0.018***
		(0.005)		(0.004)
Assigned Plan: United		0.020***		0.023***
		(0.004)		(0.004)
Assigned Plan: Wellcare		0.030***		0.000
		(0.007)		(0.006)
F-Statistic (Excluded Instruments)	29,629	7,025	12,252	3,381
Observations	909,759	909,759	909,759	909,759

Notes: This table reports first stage results. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months). In addition to the variables listed, all regressions control for recipient zip code, six-month episode buckets (how long since the recipient was auto-assigned), five-year age x gender fixed effects, race fixed effects and the county-year-month of assignment. All standard errors are clustered on the county x year x month of assignment, the unit of randomization.

A.3). This highlights the importance of using a randomized controlled design to estimate the impact of provider network breadth.

The third identifying assumption is that there is a monotonic impact of network assignment on actual provider network. The monotonicity assumption implies that being assigned to a broader (narrower) network does not result in being in a narrower (broader) network. This assumption would be violated if recipients assigned to the narrowest networks ended up in broader networks than their counterparts assigned to average networks, for example. If the monotonicity assumption is violated, equation (2.13) is not guaranteed to estimate a well defined local average treatment effect. To partially test the monotonicity assumption, Appendix Figures A.4 and A.5 plot the assigned physician and network measures against actual provider networks separately by gender, age and race. Consistent with my monotonicity assumption, being assigned a broader network leads to being in a broader network for each of the subgroups.[37]

1.4.3 Multiple inference correction

When examining the impact of provider networks on health care use and spending I examine related outcomes within several domains. In each domain, I adjust for multiple inference using the Holm-Bonferroni stepwise algorithm to control the familywise error rate (Holm, 1979).[38] Following Finkelstein *et al.* (2012b), I present per comparison p-values and "family-wise" p-values adjusted to account for multiple outcomes tested in the domain and summarize the related outcomes by collapsing each domain to estimate an overall treatment effect.

[37]This is only a partial test of monotonicity. In a future version of the paper I will use estimates from the full physician and hospital choice models, which allow coefficients on provider characteristics to vary by observables, to examine whether networks that are broader for males (or other subgroup) in a given plan-zip-year are also broader for females.

[38]The Holm-Bonferroni algorithm is uniformly more powerful than the Bonferroni correction with no further assumptions on the data.

Table 1.4: *Test of Randomization*

	Baseline Mean	Physician Network	Hospital Network	F-Test p-value
	(1)	(2)	(3)	(4)
Age	34.190	0.000035	0.000008	[0.657]
	(12.034)	(0.000058)	(0.000037)	
Male	0.516	−0.000362	0.000228	[0.055]
	(0.500)	(0.001420)	(0.000903)	
Black	0.512	0.001981	0.001373	[0.562]
	(0.500)	(0.001462)	(0.000929)	
Baseline Office Spending	16.638	0.000022	−0.000005	[0.377]
	(200.574)	(0.000028)	(0.000018)	
Baseline OPD Spending	60.756	0.000011	−0.000002	[0.469]
	(171.558)	(0.000029)	(0.000019)	
Baseline Clinic Spending	77.711	0.000007	0.000014	[0.459]
	(208.539)	(0.000030)	(0.000019)	
Baseline Inpatient Spending	486.837	0.000018	−0.000003	[0.163]
	(1947.551)	(0.000028)	(0.000018)	
Baseline Office Quantity	0.064	−0.004055	0.004244	[0.941]
	(0.240)	(0.008524)	(0.005417)	
Baseline OPD Quantity	0.321	−0.001131	0.003573	[0.417]
	(1.025)	(0.008051)	(0.005117)	
Baseline Clinic Quantity	0.635	−0.002648	0.001246	[0.499]
	(2.041)	(0.008032)	(0.005105)	
Baseline Inpatient Quantity	0.051	0.000002	0.005721	[0.051]
	(0.150)	(0.010359)	(0.006584)	
Joint F-Test		[0.1342]	[0.2543]	
Observations	104,885	104,885	104,885	

Notes: This table reports reduced form results testing the conditional random assignment of recipients to networks and the random assignment to health plans. The auto assignment sample consists of Medicaid recipients aged 18 to 65 auto-assigned to managed care plans from April 2008 to December 2012 in the five counties in New York City (N=104,885 recipients). Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded. Recipients with family members in managed care at the time of assignment or who were enrolled in Medicaid managed care in the year prior to assignment are excluded because their assignments are nonrandom. For some individuals in the sample, less than a year of prior data is available. Baseline spending and utilization variables are formed by calculating the monthly mean in the two years prior to assignment for each recipient. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

1.5 Results

1.5.1 Health Care Use and Spending

In this section I present results of the impact of broader physician and hospital networks on health care use and spending. As a benchmark for evaluating the results, Table 1.5 presents descriptive statistics. The main sample of recipients randomly assigned to managed care organizations contains 909,759 patient months. On average, recipients accessed health care services in 28.6% of the sample months and spent $263. The hospital inpatient setting accounted for the largest share of spending, at $134 a month.

Reduced form estimates of the impact of physician and hospital networks on health care use and spending are presented in Figures 1.2 and 1.3.[39] To construct this plot, I estimate the reduced form version of equation (2.13) in an event study framework. I plot coefficients recovered from the interaction between indicators for each six month period around assignment and the assigned measures of physician and hospital network breadth. By allowing the coefficients on physician and hospital network breadth to vary flexibly in each period, I can assess whether network breadth is associated with the outcomes prior to assignment and observe how the impact of being assigned to a broader network changes over time. To ease interpretation, a one standard deviation increase in the physician network measure is roughly equivalent to adding ten percent of the Medicaid physicians in New York City to a network. For hospitals, a one standard deviation increase in the network measure is equivalent to adding twenty percent of the hospitals to a network. For both physician and hospital networks, network breadth was not associated with spending or utilization prior to assignment. However, as Figure 1.2 clearly demonstrates, broader physician networks are associated with an immediate increase in spending and utilization after assignment.

[39]These analyses exclude services carved out (paid for directly by the state) of the managed care benefit package during the sample period. For example, behavioral health services, hospice and orthodontia. Pharmacy services are included since they were moved into the Medicaid managed care benefit package beginning October 2011.

Table 1.5: *Descriptive Statistics*

	Mean	Std. Dev.	Observations
	(1)	(2)	(3)
Any monthly utilization			
All Settings	0.286	0.452	909,759
Physician Office	0.104	0.305	909,759
Hospital Outpatient	0.072	0.258	909,759
Emergency Department	0.056	0.231	909,759
Hospital Inpatient	0.020	0.139	909,759
Other Settings	0.206	0.405	909,759
Spending			
All Settings	263.000	1,843.835	909,759
Physician Office	25.602	222.192	909,759
Hospital Outpatient	25.039	228.825	909,759
Emergency Department	15.838	138.608	909,759
Hospital Inpatient	134.742	1,627.217	909,759
Other Settings	61.779	391.507	909,759
Avoidable Hospitalizations†			
Any avoidable hospitalization	3.234	148.202	2,719,973
Diabetes short term complications	0.272	37.284	909,759
COPD or asthma (age 40 and older)	13.203	311.118	320,789
Congestive heart failure	0.432	62.289	909,759
Asthma (ages 18 to 39)	6.670	201.236	579,666
Preventive Care			
Any preventive care	0.017	0.128	1,525,056
Cervical cancer screening	0.025	0.156	416,120
Chlamydia test	0.060	0.238	137,713
Breast cancer screening	0.023	0.150	61,464
Flu vaccination	0.006	0.078	909,759

Notes: This table reports descriptive statistics. The auto assignment sample consists of Medicaid recipients aged 18 to 65 auto-assigned to managed care plans from April 2008 to December 2012 in the five counties in New York City (N=104,885 recipients). Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded. Recipients with family members in managed care at the time of assignment or who were enrolled in Medicaid managed care in the year prior to assignment are excluded because their assignments are nonrandom. For some individuals in the sample, less than a year of prior data is available. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

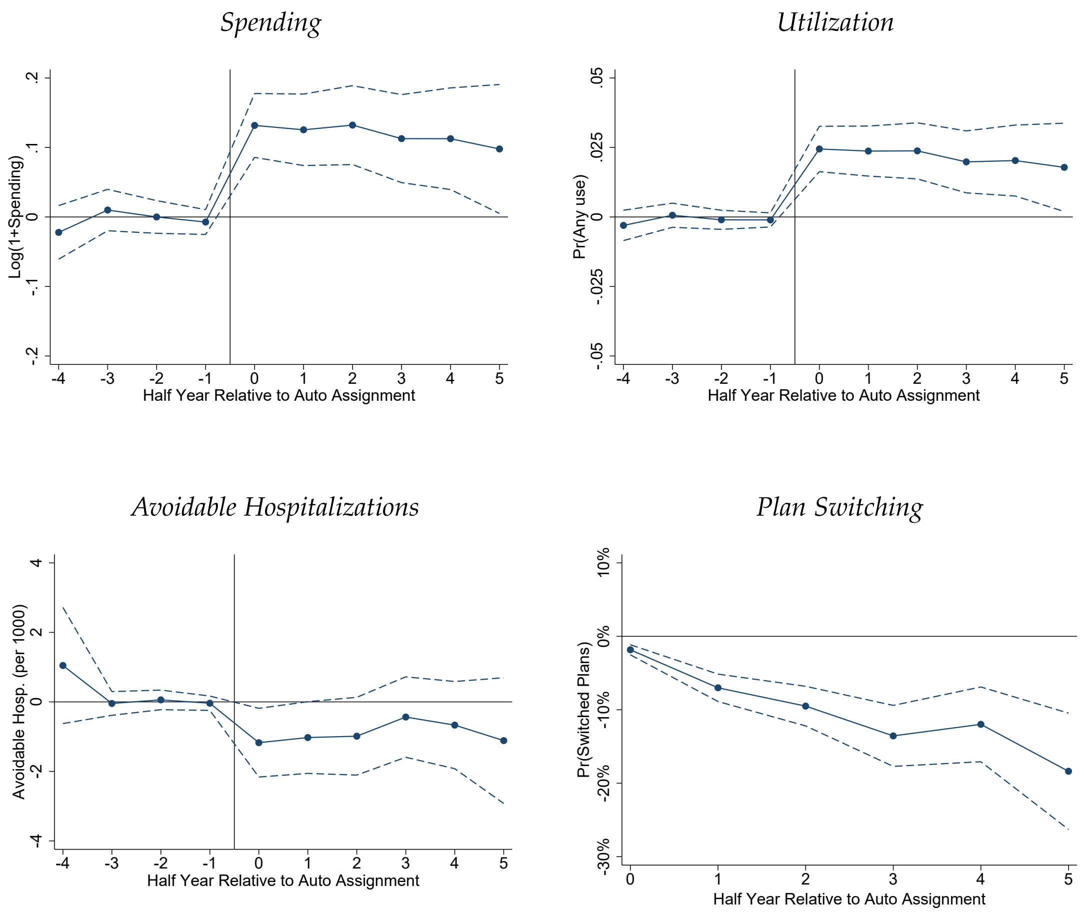

Figure 1.2: *Reduced Form Estimates of the Impact of Physician Network Size on Spending, Utilization, Avoidable Hospitalizations, and Plan Choice*

Notes: These figures plot reduced from estimates of the impact of physician network size on spending, utilization, avoidable hospitalizations and plan choice. I instrument for physician network size using the size of the network of the assigned plan (measured at the zip x plan x year level). The regressions control for a full set of plan assignment dummies, zip code dummies, indicator variables for each half year of an episode, five-year age x gender dummies, race dummies and county-year-month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment, the unit of randomization. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months).

Figures 1.4 and 1.5 plot the relationship between assigned network breadth and the outcomes being studied. In the case of physician networks, we observe a strong, linear relationship between breadth and health care use and spending. Interestingly, we do not see evidence of curvature in the relationship. This implies that adding additional physicians to

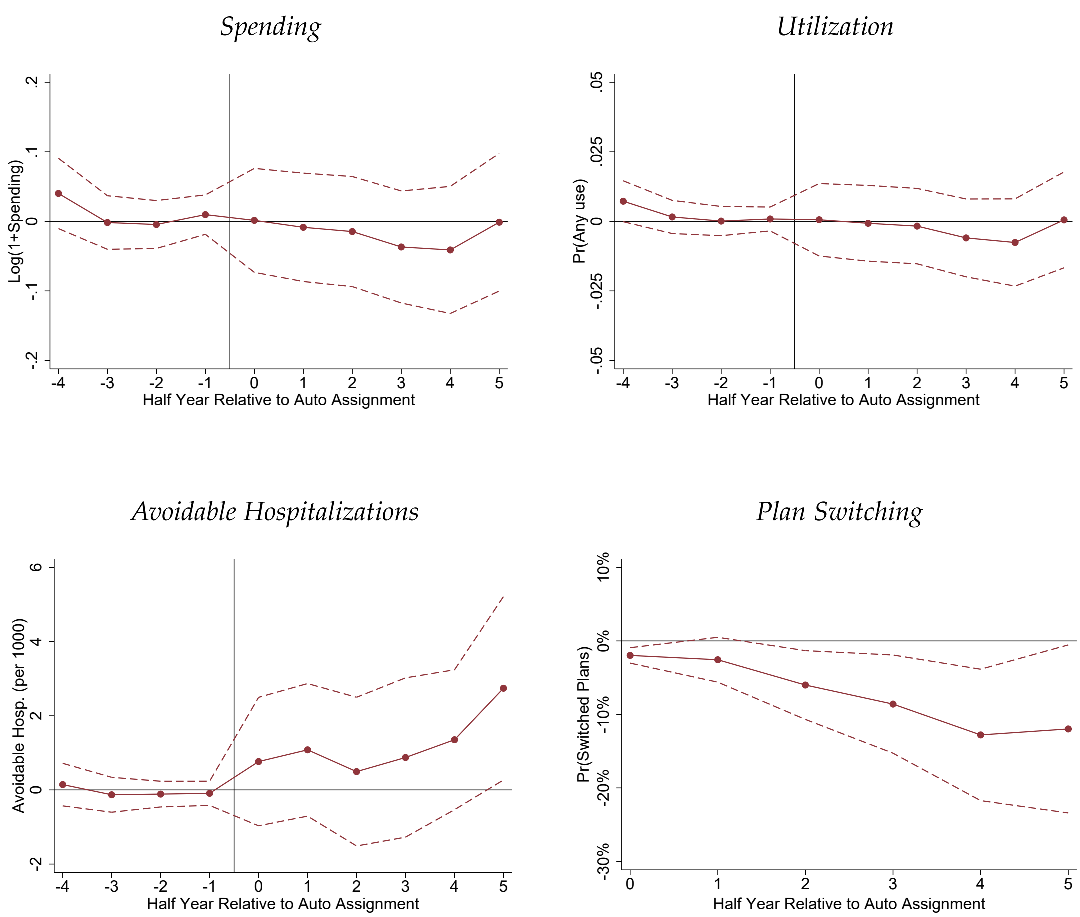

Figure 1.3: *Reduced Form Estimates of the Impact of Hospital Network Size on Spending, Utilization, Avoidable Hospitalizations, and Plan Choice*

Notes: These figures plot reduced from estimates of the impact of physician network size on spending, utilization, avoidable hospitalizations and plan choice. I instrument for physician network size using the size of the network of the assigned plan (measured at the zip x plan x year level). The regressions control for a full set of plan assignment dummies, zip code dummies, indicator variables for each half year of an episode, five-year age x gender dummies, race dummies and county-year-month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment, the unit of randomization. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months).

a network increases health care use in both narrower and broader networks over the range of networks we observe. In contrast, the relationship between hospital network breadth and health care use is noisy and flat.

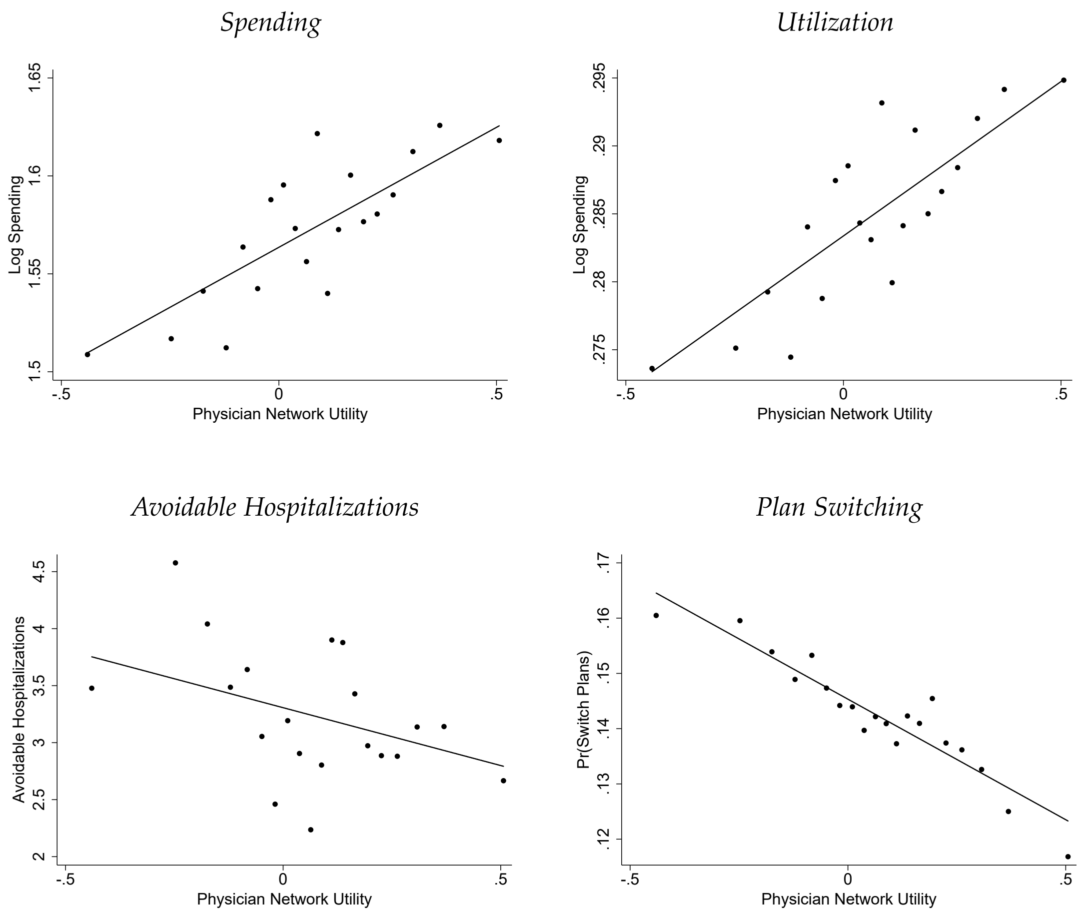

Figure 1.4: *Assigned Physician Network Size and Spending, Utilization, Avoidable Hospitalizations, and Plan Choice*

Notes: These figures plot reduced from estimates of the impact of physician network size on spending, utilization, avoidable hospitalizations and plan choice. I instrument for physician network size using the size of the network of the assigned plan (measured at the zip x plan x year level). The regressions control for a full set of plan assignment dummies, zip code dummies, indicator variables for each half year of an episode, five-year age x gender dummies, race dummies and county-year-month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment, the unit of randomization. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months).

Table 1.6 presents our primary instrumental variable estimates. Column (2) presents our primary estimates with the full set of controls. A one standard deviation increase in the size of the physician network increased total spending by 14.3 log points (std. err. = 0.029), or roughly 15%, and an increase of 2.7 percentage points in the probability of using care,

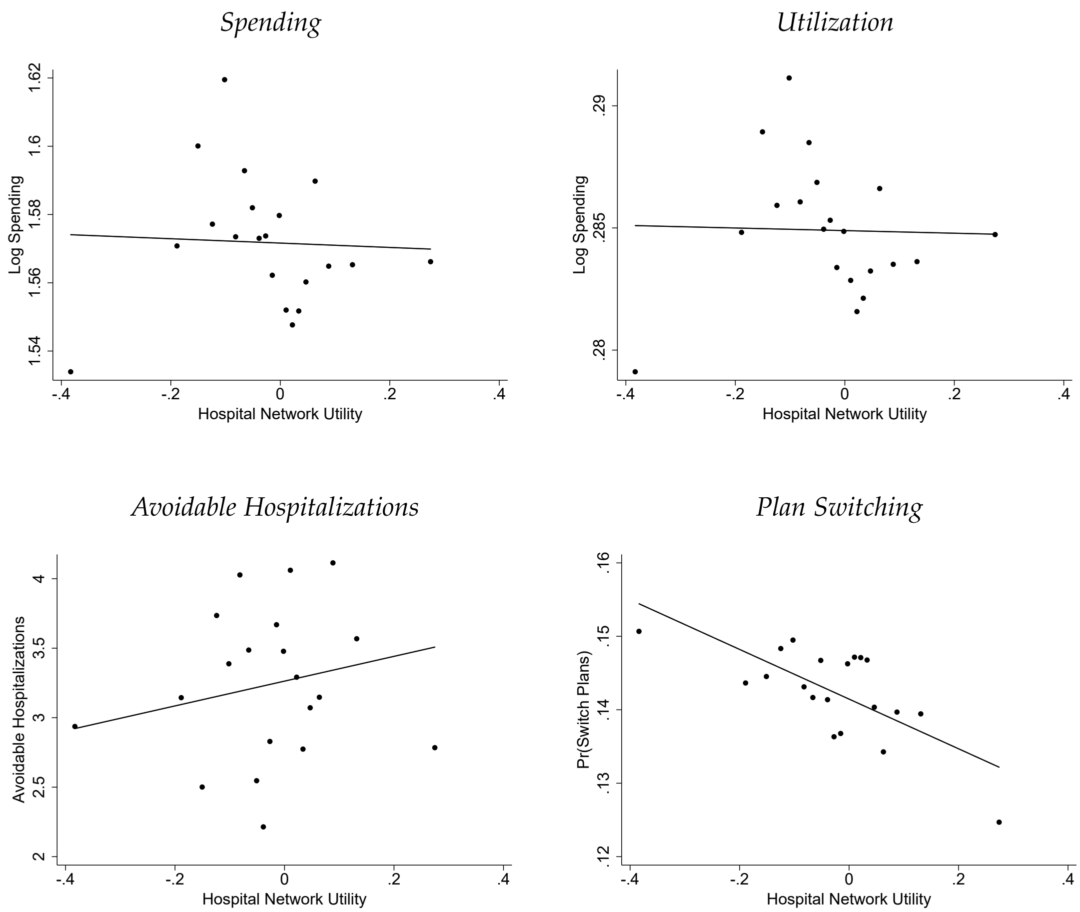

Figure 1.5: *Assigned Hospital Network Size and Spending, Utilization, Avoidable Hospitalizations, and Plan Choice*

Notes: These figures plot reduced from estimates of the impact of physician network size on spending, utilization, avoidable hospitalizations and plan choice. I instrument for physician network size using the size of the network of the assigned plan (measured at the zip x plan x year level). The regressions control for a full set of plan assignment dummies, zip code dummies, indicator variables for each half year of an episode, five-year age x gender dummies, race dummies and county-year-month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment, the unit of randomization. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (*N*=900,759 patient months).

or roughly a 10%. In column (3) we demonstrate that these estimates are not sensitive to excluding recipient-level controls for age, gender, race and mean baseline spending. Likewise, estimates of the impact of hospital network breadth were robust to the exclusion of recipient controls. I found no statistically significant association between hospital network breadth and spending or health care use.

Table 1.6: *IV Estimates of the Impact of Networks on Health Care Use, Access, and Plan Loyalty*

		Physician Network		Hospital Network	
	Mean	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)
Panel A: Health Care Use					
Log Spending	1.577	0.143***	0.163***	−0.009	0.013
		(0.029)	(0.033)	(0.046)	(0.049)
Utilization (any)	0.286	0.027***	0.030***	−0.001	0.003
		(0.005)	(0.006)	(0.008)	(0.009)
Panel B: Access					
Preventive Care	0.017	0.001	0.001	0.001	0.001
		(0.001)	(0.001)	(0.001)	(0.001)
Avoidable Hospitalizations†	3.234	−1.209*	−1.426**	1.041	0.680
		(0.623)	(0.628)	(1.076)	(1.218)
Panel C: Plan Loyalty					
% time in other plan	0.142	−0.051***	−0.050***	−0.041***	−0.043***
		(0.007)	(0.007)	(0.012)	(0.012)
Observations		909,759	909,759	909,759	909,759
Recipient Controls		Yes	No	Yes	No

Notes: Standard errors in parentheses. The dependent variables are based on five analyses across three domains: health care use; access; and recipient satisfaction. The independent variables are physician and hospital network generosity. Columns (2) and (4) report the main two-stage least squares (2SLS) results from estimating equation (2.13) for physician and hospital networks, respectively. Columns (3) and (5) present the results of estimating the same specification using a sample of recipients that made active plan choices. All regressions include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since assignment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months). For per comparison significance for the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

†Avoidable hospitalizations are measured per 1000 recipient months.

The above findings mask significant heterogeneity in the impact of networks by the site of service (e.g. physician office vs. hospital outpatient department). Appendix Table A.8 presents IV estimates of the impact of broader physician and hospital networks on log spending by site of service. For broader physician networks, the impact on spending was concentrated in the outpatient setting and among miscellaneous services. In column (3) I show that broader physician networks were associated with increases in spending of 6.6 log points (std. err. = 0.018) in physician offices ($\approx$ 7.5%) and 7.7 log points (std. err. = 0.14) in hospital outpatient departments ($\approx$ 7.5%). A broader physician network was also associated with an increase of 10.9 log points (std. err. = 0.022) in spending on "other" services ($\approx$ 10%), a category that includes freestanding clinics, independent lab, transportation, and other miscellaneous settings. The data do not allow me to reject the null of no change in emergency room and inpatient utilization and the estimates are precise enough to rule out substantial effects in either direction.

Column (6) presents analogous estimates of the impact of hospital network size on spending. There was no statistically significant association between a broader hospital network and overall spending. However, A one standard deviation increase in the size of the hospital network was associated with an increase of 5.0 log points (std. err. = 0.026) in spending in the hospital outpatient department ($\approx$ 5%), but a decrease of 5.4 log points (std. err. = 0.031) in spending in the physician office setting ($\approx$ 5%). As in the case of physician network breadth, I cannot reject the null of no change in emergency room and inpatient spending. Here, a marked difference with the physician network results emerges. Whereas broader physician networks serve as a complement to the hospital outpatient department (increasing spending there), a broader hospital network appears to drive a substitution of outpatient care away from the office and into the hospital setting. This result is highlighted in Appendix Figures A.6 and A.7 which present event study estimates of the impact of physician and hospital networks by setting. These figures also provide an additional set of falsification tests, allowing us to observe the relationship between the assigned network breadth and spending by setting prior to assignment. Reassuringly, there is little evidence of a consistent relationship before assignment.

Similar heterogeneity is present when examining the impact of networks on health care use. Appendix Table A.9 presents our estimates of the impact of networks on the probability of using care in a month, a measure of the extensive margin of utilization. The findings are qualitatively similar to the spending results. Column (3) of Appendix Table A.9 contains estimates of the impact of physician networks on the probability recipients seek care in a month. A one standard deviation increase in the size of the physician network is associated with a 2.7 percentage (std. err. = 0.5) increase in the overall probability of using care ($\approx$ 10%), driven by increased use in the physician office ($\approx$ 15%), hospital outpatient department ($\approx$ 20%), and other ($\approx$ 10%) settings. Column (5) presents the more muted impacts of a broader hospital network, with a one standard deviation increase in the size of the hospital network associated with a 1.0 percentage point (std. err. = 0.5) increase the probability of using hospital outpatient care ($\approx$ 10%), and null results in all other settings. Additionally, broader hospital networks were associated with a 1.0 percentage point (std. err. = 0.6) decrease in the probability of using care in the physician office setting, but it was insignificant with a p-value of 0.12. These results are reinforced by Appendix Figures A.8 and A.9 which present event study estimates of the impact of physician and hospital networks on health care use by setting.

Tables 1.7 and 1.8 present two-stage least squares results from our primary specification separately by recipient gender, ethnicity, age and baseline spending. Table 1.7 demonstrates that the effects of broader physician networks on health care use and spending were somewhat larger for females, non-blacks and older recipients, however none of these differences is statistically significant. The impact of broader physician networks on health care use and spending was significantly larger for recipients with above median baseline spending, possibly because lower spenders rarely seek care and are thus unaffected by network breadth. In Table 1.8, I do not find evidence that hospital networks are associated with health care use or spending for any of the recipient subgroups.

Table 1.7: *2SLS Physician Network Results by Recipient Characteristics*

	Gender		Ethnicity		Age		Baseline Spending	
	Male	Female	Black	Non-Black	Under 40	40 and older	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Health Care Use								
Spending	0.117***	0.175***	0.110***	0.173***	0.127***	0.179***	0.085***	0.230***
	(0.037)	(0.042)	(0.039)	(0.042)	(0.030)	(0.056)	(0.032)	(0.051)
Utilization (any)	0.023***	0.031***	0.021***	0.032***	0.024***	0.032***	0.015***	0.043***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.010)	(0.006)	(0.009)
Panel B: Access								
Preventive Care	−0.000	0.002	−0.001	0.002**	0.002*	−0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Avoidable Hospitalizations†	−0.661	−1.912**	−2.364**	−0.089	−1.225*	−0.748	−1.593***	−0.821
	(0.892)	(0.886)	(1.104)	(0.654)	(0.719)	(1.158)	(0.456)	(1.294)
Panel C: Plan Loyalty								
% time in other plan	−0.043***	−0.058***	−0.043***	−0.058***	−0.050***	−0.048***	−0.040***	−0.067***
	(0.008)	(0.012)	(0.009)	(0.011)	(0.008)	(0.013)	(0.009)	(0.014)
Observations	448,942	451,513	458,846	441,608	579,666	320,789	485,500	414,955
Recipient Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors in parentheses. The dependent variables are based on five analyses across three domains: health care use; access; and recipient satisfaction. The independent variables are physician and hospital network generosity. Columns (2) and (4) report the main two-stage least squares (2SLS) results from estimating equation (2.13) for physician and hospital networks, respectively. Columns (3) and (5) present the results of estimating the same specification using a sample of recipients that made active plan choices. All regressions include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since assignment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months). For per comparison significance for the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

†Avoidable hospitalizations are measured per 1000 recipient months.

Table 1.8: *2SLS Hospital Network Results by Recipient Characteristics*

	Gender		Ethnicity		Age		Baseline Spending	
	Male	Female	Black	Non-Black	Under 40	40 and older	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Health Care Use								
Spending	0.035	−0.064	−0.018	0.013	0.049	−0.109	−0.031	0.028
	(0.057)	(0.070)	(0.060)	(0.066)	(0.048)	(0.097)	(0.050)	(0.082)
Utilization (any)	0.006	−0.008	−0.003	0.003	0.012	−0.023	−0.005	0.007
	(0.010)	(0.012)	(0.011)	(0.011)	(0.008)	(0.016)	(0.009)	(0.014)
Panel B: Access								
Preventive Care	0.001	0.001	−0.000	0.003*	0.002	0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
Avoidable Hospitalizations†	1.122	1.104	1.702	0.450	−1.299	5.370**	0.690	1.387
	(1.565)	(1.310)	(1.510)	(1.482)	(0.968)	(2.415)	(0.807)	(2.239)
Panel C: Plan Loyalty								
% time in other plan	−0.017	−0.065***	−0.047***	−0.033*	−0.046***	−0.032	−0.018	−0.071***
	(0.013)	(0.019)	(0.016)	(0.018)	(0.013)	(0.021)	(0.015)	(0.020)
Observations	448,942	451,513	458,846	441,608	579,666	320,789	485,500	414,955
Recipient Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors in parentheses. The dependent variables are based on five analyses across three domains: health care use; access; and recipient satisfaction. The independent variables are physician and hospital network generosity. Columns (2) and (4) report the main two-stage least squares (2SLS) results from estimating equation (2.13) for physician and hospital networks, respectively. Columns (3) and (5) present the results of estimating the same specification using a sample of recipients that made active plan choices. All regressions include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since assignment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months). For per comparison significance for the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

†Avoidable hospitalizations are measured per 1000 recipient months.

1.5.2 Access

In this section, I examine the impact of physician and hospital network breadth on two measures of access: the rate of avoidable hospitalizations and compliance with recommended preventive care. I use avoidable hospitalizations because these measures are included in the 2015 Medicaid Adult Core Set, a set of access measures developed by the Secretary of Health and Human Services (HHS) for adult Medicaid recipients. In the core set, there are four measure of avoidable hospitalization: (i) diabetes short-term complications; (ii) chronic obstructive pulmonary disease (COPD) or asthma in adults; (iii) heart failure and (iv) asthma in younger adults. In addition to these measures, I examine four markers of compliance with recommended preventive care in the Medicaid Adult Core Set: (i) cervical cancer screenings; (ii) chlamydia testing; (iii) breast cancer screenings; and (iv) flu vaccinations. For the recipients in my sample, the use of preventive care and the prevalence of avoidable hospitalizations is low (See Table 1.5). To ease interpretation, I rescale the measure of avoidable hospitalizations to be the rate of hospitalizations per 1,000 recipient months. For both avoidable hospitalizations and preventive care, I create summary measures by stacking the individual measures with standard errors clustered at the county-by-year-by-month of assignment level.

Reduced form estimates of the impact of physician and hospital networks on avoidable hospitalizations are presented in Figures 1.2 and 1.3. As described earlier, I construct these plots by estimating the reduced form version of equation (2.13) in an event study framework and allowing the coefficients on network breadth to vary flexibility by time relative to assignment. I find no effect of a broader physician or hospital network on the use of preventive care and do not include reduced form estimates of this relationship for the sake of brevity. For physician and hospital networks, network breadth was not associated with the rate of avoidable hospitalizations prior to assignment. However, as can be seen in Figure 1.2, a broader physician network appears to reduce the rate of avoidable hospitalizations after assignment. Interestingly, in Figure 1.3 we see a slight (thought insignificant) increase in the rate of avoidable hospitalizations associated with a broader hospital network.

In Figures 1.2 and 1.3 we plot the rate of avoidable hospitalizations against the assigned physician and hospital network breadth. The relationship is noisier than in the case of health care use and spending, however, Figure 1.2 provides suggestive evidence that being assigned to a broader physician network is associated with a reduction in avoidable hospitalizations. To test this formally, table 1.6 presents instrumental variable estimates of the impact of networks on the use of preventive care and rate of avoidable hospitalizations. Stacking the data for the four different measures of avoidable hospitalizations, I find that a one standard deviation increase in the size of the physician network reduces the number of avoidable hospitalizations by 1.2 (std. err. = 0.6) per 1000 recipient months (40%). Despite the large point estimate, the 95% confidence intervals overlap zero due to imprecision in the estimates. Column (3) shows that this estimate is robust to the exclusion of recipient controls. I found no effect of a broader physician network on the use of preventive care, possibly attributable to the fact that churn in the Medicaid population made it infeasible to construct these measures exactly as indicated in the Medicaid Adult Core Set. Columns (4) and (5) contain estimates of the impact of hospital networks on access. I was unable to reject the null of no change in preventive care or avoidable hospitalizations associated with broader hospital networks.

Appendix Figures A.10 and A.11 plot coefficients from an event study of the impact of physician and hospital network breadth on each of the four avoidable hospitalization measures. Due to the rarity of avoidable hospitalizations, these plots are noisier than the corresponding plots for health care use or spending. In Appendix Figure A.10 a broader physician network is associated with a slight reduction in avoidable hospitalizations in three of the four measures (all except congestive heart failure), however these are generally not significant. The exception is a statistically significant reduction in avoidable hospitalizations for asthma in recipients under 40 associated with a broader physician network in the period after assignment. The estimated hospital coefficients are noisier. In Appendix Figure A.11 I see little effect of broader hospital networks on avoidable hospitalizations for short-term diabetes complications or congestive heart failure. For avoidable hospitalizations for asthma in recipients under 40 a broader hospital network is associated with a slight and consistent (albeit insignificant) reduction, a result similar to the physician network findings. Surprisingly, however, i find the opposite effect for chronic obstructive pulmonary disease (COPD) or asthma in recipients above 40. Broader hospital networks seem to increase hospitalizations for these conditions, perhaps due to the ease of access to hospital emergency departments or inpatient wards. Appendix Table A.10 presents IV estimates of the impacts of broader physician and hospital networks. To reiterate, the physician network results are driven by a reduction of 3.7 (std. err. = 2.0) hospitalizations for asthma (18 to 39 year olds) per 1000 recipient months (although the result is not significant after adjusting for multiple inference). Column (6) presents results for the hospital network. Here, I find no impact of a larger hospital network on the overall rate of avoidable hospitalizations, but an increase of 15.1 visits per 1000 recipient months in the rate of hospitalizations for chronic obstructive pulmonary disease (COPD) or asthma for Medicaid recipients 40 or older (over 100%). This result is not significant after adjusting for multiple inference.[40]

1.5.3 Plan Loyalty

One measure of recipient dissatisfaction is the decision to switch managed care plans. Medicaid managed care recipients have three months from the time of assignment to switch plans without cause before they face a nine-month "lock-in" period during which they may only switch plans if they have "good cause" to do so. Even among the auto assigned population, plan switching occurs infrequently. To measure plan switching I construct an indicator variable equal to one if a recipient's current plan is not their assigned plan. The mean for this measure of plan loyalty is 15.3%. Higher rates of plan switching indicate lower plan loyalty. If recipients prefer broader networks, our increased values of our physician and hospital networks should be associated with a reduction in plan switching.

Reduced form estimates of the impact of physician and hospital networks on plan

[40]Moreover, the large magnitude is likely due to the use of linear probability models rather than non-linear models like probit or logit that may be more appropriate for rare outcomes like avoidable hospitalizations.

switching are presented in Figures 1.2 and 1.3. Unlike the other event study figures, there is no baseline measure of plan switching since recipients are in Medicaid Fee-For-Service prior to assignment. Hence, to construct this plot I estimate the reduced form version of equation (2.13) separately for each six year period after assignment. The coefficients should be interpreted as the impact of one standard deviation increase on the probability of a recipient being in their assigned plan, controlling for the average rate of plan switching in each period. Both physician and hospital networks are associated with significantly lower probabilities of recipients switching plans. This "protective effect" increases over time, with recipients assigned to broader networks becoming less likely (relative to other recipients) to switch plans. Perhaps more intuitively, recipients in narrower networks become likelier to switch plans over time, likely a function of them learning about their network (and the alternatives) over time.

Figures 1.4 and 1.5 depict a strong (and negative) relationship between the breadth of the assigned physician and hospital networks and the probability of switching plans. As in the case of health care use and spending, at these relationships are approximately linear. Table 1.6 presents our reduced form estimates of the impact of networks on plan loyalty. Broader physician and hospital networks both increase estimated plan loyalty. Columns (2) and (4) present estimates for my preferred specification, where I find that a one standard deviation increase in the breadth of the physician network of a recipient's assigned plan is associated with a decrease in the probability of switching plans of 5.1 percentage points (std. err. = 0.7), or about 35%, and a one standard deviation increase in the breadth of hospital networks reduce the probability of plan switching by 4.1 percentage points (std. err. = 0.7), or about 30%.

In tables 1.7 and 1.8, I present reduced form results from my primary specification separately by recipient gender, ethnicity, age and baseline spending. For both physician and hospital networks, the plan loyalty results differ markedly for recipients that were low and higher spenders at baseline. For high spenders at baseline a one standard deviation increase in the breadth of the physician and hospital networks are associated with a decrease in plan switching of 6.7 percentage points (std. err. = 1.4) and 7.1 percentage points (std. err. = 2.0), respectively (about a 50% reduction in both cases). For low baseline spenders, however, plan switching was less responsive to network breadth. For these recipients, a broader physician network was associated with a decrease in plan switching of 4.0 percentage points (std. err. = 0.9), or about 30%, and a broader hospital network was not associated with plan switching. This may reflect the fact that high baseline spenders, who interact with the health care system more frequently after assignment, are likelier to learn about and respond to the breadth of their provider network.

1.5.4 Distance Traveled

In addition to the three primary domains (health care use, access and plan loyalty), I examine the impact of provider networks on distance. These are results are presented separately for two reasons. First, the estimates are not well-identified since I only observe "distance traveled" if a recipient consumes care. Since I establish earlier that broader networks impact health care use, changes in distance that are associated with network breadth may be the result of compositional changes in care.[41] Second, and related, it is not obvious how to interpret the sign of these effects. For example, if the results suggest that distance traveled for office-based specialty care increases in broader physician networks is that desirable? On the one hand, recipients are traveling further and bearing higher time and transportation costs to access providers. On the other hand, by revealed preference further away providers must be more desirable then those nearby.

With these caveats in mind, Appendix Table A.11 presents estimates of the impact of physician and hospital networks on distance traveled. I examine the association between networks and distance traveled to physicians and hospitals—two settings where I can reliably measure provider location. The results are mixed. A one standard deviation increase in the size of the physician network is associated with a reduction of 0.8 miles

[41]For example, the reduction in travel time for primary care visits in the office setting may be driven by an increase in low-intensity primary care visits that can be accommodated closer to home.

(std. err. = 0.5) traveled for primary care in the physician office setting (12%), however, it is insignificant after adjusting for multiple inference. Interestingly, a broader physician network was associated with a modest decrease in the distance traveled for hospital care (5%). The hospital network was not associated with differences in the distance traveled when seeking care except in the case of pregnant women traveling to deliver a baby, where the distance traveled was reduced by 0.9 miles (std. err. = 0.2) or 15%. This result is robust to adjusting for multiple inference and is unlikely to be driven by changes in the composition of care.

1.6 Discussion

1.6.1 Comparison with Other Estimates

First, I compare the main IV estimates to a specification that omits plan effects. Plan effects were included in the primary specification to control flexibly for the impact of plans on health care use and spending.[42] While random assignment addresses differences across plans based on selection, operational differences may exist (e.g. differences in provider payment) that impact the outcomes being studied. To quantity the extent to which these differences exist and to understand how they interact with network size, I re-estimate equation (2.13) without plan fixed effects.

Table 1.9 presents a comparison of the IV results to estimates that exclude plan effects. In the case of health care use and spending, the two exercises yield qualitatively similar estimates for physician network breadth. Omitting plan controls, however, does qualitatively change estimates of the impact of physician networks on health care access. Without plan controls, there is a significant and positive (but modest) impact of physician networks on the use of the preventive care and no association between physician networks and avoidable hospitalizations. The hospital results are even more striking. For both spending and

[42]The extent to which differences across plans other than benefit design or provider network impact health and health care is not well-understood. Appendix Table A.13 provides evidence that these effects are economically and statistically significant.

Table 1.9: *IV Estimates of the Impact of Networks With and Without Plan Controls*

		Physician Network		Hospital Network	
	Mean of Dep. Var (1)	Main Results (2)	No Plan Controls (3)	Main Results (4)	No Plan Controls (5)
Panel A: Health Care Use					
Spending	1.577	0.143***	0.099***	−0.009	−0.200***
		(0.029)	(0.021)	(0.046)	(0.027)
Utilization (any)	0.286	0.027***	0.018***	−0.001	−0.027***
		(0.005)	(0.004)	(0.008)	(0.005)
Panel B: Access					
Preventive Care	0.049	0.001	0.001**	0.001	−0.001
		(0.001)	(0.000)	(0.001)	(0.001)
Avoidable Hospitalizations†	3.234	−1.209*	0.225	1.041	−1.083
		(0.623)	(0.409)	(1.076)	(0.680)
Panel C: Plan Loyalty					
% recipients that switch plans	0.142	−0.051***	−0.054***	−0.041***	0.032***
		(0.007)	(0.005)	(0.012)	(0.007)
Observations		909,759	909,759	909,759	909,759
Recipient Controls		Yes	Yes	Yes	Yes

Notes: Standard errors in parentheses. The dependent variables are based on five analyses across three domains: (i) health care use; (ii) access; and (iii) satisfaction. The independent variables are physician and hospital network generosity. Columns (2) and (4) report the main two-stage least squares (2SLS) results from estimating equation (2.13) for physician and hospital networks, respectively. Columns (3) and (5) present the results of estimating the same specification without controlling for plan effects. All regressions include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since assignment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months). For per comparison significance for the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

†Avoidable hospitalizations are measured per 1000 recipient months.

utilization, the results in column (5) suggest that broader hospital networks are associated with economically and statistically significant reductions in spending and health care use (as compared to the null effect in our main specification). In addition, the sign of the impact of hospital networks on recipient satisfaction is flipped when plan controls are omitted, implying somewhat implausibly that a broader hospital network is associated with lower recipient satisfaction. These findings highlight the fact that meaningful differences exist across plans (See Appendix Table A.13) and that these differences are correlated with measures of the physician and hospital network (See Appendix Figure A.12).[43]

Second, as in Finkelstein *et al.* (2012b), I compare my quasi-experimental estimates to observational estimates constructed using New York Medicaid data. To do this I construct a second sample of equal size with recipients that made active plan choices. This approach is preferable to re-estimating equation (2.13)) without instruments since plan assignment explains most of the variation in networks in that sample. Appendix Table A.14 presents the results of this analysis. In columns (2) and (3), I present estimates of the impact of a broader physician network on health care use, access and distance traveled.[44] The OLS estimates of the impact of physician networks on log spending and utilization are larger than the 2SLS coefficients. These findings are consistent with adverse selection on physician networks—individuals with greater health care needs sorting into broader physician networks. Interestingly, the opposite result emerges when comparing the estimated impact of hospitals on health care use and spending. The OLS point estimates are negative (and highly significant) as compared to a null effect using the 2SLS approach. A possible explanation for this is the way facilitated enrollers operate in the hospital setting. Health plans employee these enrollers to provide decision support to Medicaid recipients, and concerns have been raised that enrollers seek to disproportionately sign-up healthy recipients. If health plans locate enrollers in areas where their hospital network is broader this may explain the discrepancy

[43]For example, Metroplus—one of the largest MCOs operating in New York City—has the narrowest hospital network (Appendix Figure A.12) and folks assigned to Metroplus are much less likely to switch out of the plan (Appendix Table A.13).

[44]As expected, the variance of the OLS estimates are lower.

between the OLS and 2SLS results.

1.6.2 External Validity

The results should be evaluated in context and several caveats are warranted when extrapolating the main findings to other populations, settings or time periods. First, the findings speak only to the effect of physician and hospital networks using within-plan variation. As discussed, this assumption is important to separate the impact of network breadth from other characteristics of plans (e.g. ownership structure). This approach does, however, impact the generalizability of the results. For example, if broader Medicaid MCOs with broader networks (on average) are including more expensive physicians and hospitals this will have the effect of increasing health care spending by raising prices. Our provider network estimates will not reflect this, since any plan level impacts are absorbed by our plan fixed effects. However, as Table 1.9 makes clear, omitting plan effects is not a feasible alternative.

Second, the results are based on Medicaid managed care recipients living in New York City from 2008 to 2012. When generalizing these results to other Medicaid populations, it is important to consider state variation in eligibility rules, procurement policies and the characteristics of the health care sector. For example, the results may not generalize to rural areas where provider markets are not as saturated as New York City. Finally, the sample of approximately 100,000 New York Medicaid recipients selected for the study failed to make an active plan choice. Auto assignment is an important source of random variation, however, recipients who actively choose their plans may differ in meaningful ways from those who don't.

Third, the results measure the partial equilibrium effects of assigning Medicaid recipients to different physician and hospital networks. The effect of broader policy changes like adjustments to network adequacy requirements may differ because insurers and providers will react to large scale changes in health care markets (Finkelstein, 2007).

1.6.3 Conclusion

Using a randomized controlled design I examine how variation in physician and hospital networks affects Medicaid managed care recipients in New York City. Broader physician networks are associated with significantly higher health care utilization and spending, a reduction in avoidable hospitalizations, and increased plan loyalty. Broader hospital networks are also associated with increased plan loyalty, but have no impact on utilization, spending or access. These results provide insight into the costs and benefits of broader access to physicians and hospitals for an important low-income population. The findings inform the ongoing debate about the role of government in setting and enforcing network adequacy standards in Medicaid and elsewhere.

Chapter 2

Are all managed care plans created equal? Evidence from random plan assignment in Medicaid[1]

2.1 Introduction

Almost all health insurance contracts in the US today, public or private, exhibit some form of "managed care."[2] It is widely believed that managed care embodies the proper tools for addressing the moral hazard, information, and agency problems endemic to healthcare consumption, in which consumers choose among goods of unobservable quality and face low or zero marginal costs of consumption (Arrow, 1963). Broadly defined, managed care consists of an array of features targeted at cutting costs and generating productive efficiency, including selective contracting (i.e., network formation), utilization review, negotiation over provider prices, innovative provider payment models, patient gatekeeping and cost sharing, and active management of patient conditions. Given the potential of managed care

[1]Co-authored with Michael Geruso and Tim Layton.

[2]Features of managed care are found in virtually every health insurance contract available in the US, including both public and private coverage within Medicare and Medicaid, employer-sponsored coverage, and individual markets.

to address several of the fundamental market failures in healthcare consumption, it is no surprise that both in recent policy embodied in the Affordable Care Act and consistently over the last thirty years, the tools of managed care have been critical pieces of nearly every policy effort to stem the rising tide of healthcare cost growth.[3]

However, despite wide adoption of managed care features in public insurance programs and by insurance firms in private markets, it is no exaggeration to say that we know very little about how managed care enhances the productive efficiency of healthcare, or whether different managed care plans might vary importantly in their approach to cost minimization. Much of the literature on managed care focuses on binary comparisons either between public and private health plans in Medicare or Medicaid or between Health Maintenance Organizations (HMOs) and other non-HMO plans in an employer setting.[4,5] While this binary focus has allowed for some progress toward understanding how managed care plans differ from non-managed care plans, non-managed care plans are increasingly rare, making variation *within* the world of managed care the more relevant area of inquiry. Indeed, there is considerable speculation that the internal workings of managed care organizations—and the incentives they impart on providers—may vary considerably across managed care plans (Gaynor *et al.*, 2004), but there has been little systematic study of either the extent of variation in healthcare spending or the drivers of such variation across plans falling under the "managed care" umbrella.

In this paper, we open the black box of managed care and study how health plans vary in their ability to influence productivity by lowering health care spending. In doing so, we must address a fundamental challenge to identifying an MCO's impact on healthcare

[3]For example, in the recent set of Medicare reforms embodied in the Affordable Care Act, new managed care pilot programs were touted as important contributors to efforts to "bend the cost curve" of Medicare spending.

[4]For public/private comparisons see Duggan and Hayford (2013). For the seminal work comparing HMOs to other plans see Cutler *et al.* (2000)

[5]Throughout the paper, we refer to health plans that are managed by the government as public plans, rather than the more conventional "fee-for-service" plans. We do this because most public plans include some features of managed care, making the public/private management structure the key difference between these plans rather than the use of care management. Some examples of features of managed care used in public plans are bundled payments, Accountable Care Organization contracts, and pay-for-performance.

spending: Consumers sort non-randomly across plans on unobservables that are correlated with their healthcare utilization patterns. Although selection in insurance markets has attracted significant independent research attention,[6] the problem of selection has generally not been integrated into the managed care productivity literature. But as we show in a simple model of the MCO's profit objective, selection is an important potential confounder when attempting to measure plan productivity: Firms must compete to attract low-cost enrollees, in addition to competing on their ability to lowering costs for any fixed enrollee type. The problem is analogous to separately identifying the supply-side and demand-side factors contributing to the geographic variation in health care spending (Finkelstein *et al.*, 2015).

Separating the effects of selection and production on a health plan's spending level is critical for policy. If plans with the lowest spending levels achieve "savings" just by attracting a healthier group of consumers, there is little to be gained by adopting their care management practices. If, on the other hand, the variation in spending across health plans is explained by plan, rather than consumer, characteristics, then adopting the care management techniques of the plans with the lowest spending levels could help slow the rapid growth in healthcare spending. Additionally, in markets that lack traditional market mechanisms such as prices to direct consumers to more efficient plans (like the Medicaid Managed Care market we study in this paper), regulators may desire to adopt policies that encourage consumers to join these "low-cost" plans.

Here, we overcome the obstacles to studying production and selection in managed care with a research design that relies on random assignment of consumers to plans. Specifically, we exploit random assignment of Medicaid beneficiaries to Medicaid Managed Care plans in New York City. In this setting, privately-operated MCO plans compete for Medicaid beneficiaries making active plan choices, but a large number of beneficiaries are also randomly auto-assigned to the same set of plans, enabling us to identify plan

[6]See, e.g., Einav *et al.* (2010); Geruso (2012); Layton (2014) for studies of selection in employer plans; Hackmann *et al.* (2013) in the Marketplaces; and Newhouse *et al.* (2012); Brown *et al.* (2014); Cabral *et al.* (2014) in Medicare.

effects purged of the typical selection concerns. To our knowledge, ours is the first study since the RAND Health Insurance Experiment to make use of random assignment across health plans in any market.[7] In the state's administrative data, we observe information on randomized plan assignments, realized enrollments, and the universe of claims for the roughly 100,000 Medicaid enrollees who are randomly assigned from 2007 to 2012. We also observe plan characteristics, including networks and negotiated upstream insurer-provider prices, allowing us to decompose spending differences and gain insights into what drives those differences.

Our setting offers two additional advantages for uncovering how plans differ in their ability to constrain healthcare spending: First, restricting attention to a single, competitive geographic market allows us to compare plans which have the opportunity to contract with the same set of providers and which are competing over the same population of potential enrollees. Second, the Medicaid setting has the added benefit that all plans are required to have identical demand-side cost sharing parameters and would typically be characterized as having "narrow" provider networks. This implies that the plans are restricted to vary *only* in the least understood aspects of managed care.

We find significant variation in both production and selection across ostensibly similar managed care plans. Indeed, simple mean spending comparisons among randomly-assigned beneficiaries in our data reveal that if an individual enrolls in the lowest spending plan in her local area, she will incur monthly healthcare costs about 33% lower (about $600 per member per year) than what she would have incurred had she enrolled in the highest spending plan. This variation across plans is not restricted to outliers: Other plans competing in the same local market are approximately uniformly distributed between these extremes in terms of effects on spending. To put this dispersion in context, Cutler *et al.* (2000) find that HMOs reduce spending on heart disease treatment by 30-40% relative to traditional insurance plans, implying that the variation in spending we estimate across managed care

[7]The Oregon Health Insurance Experiment (Finkelstein *et al.*, 2012a) randomized assignment into Medicaid, but did not exploit any random variation in assignment across health plans.

plans is approximately equivalent to the variation in spending between managed care plans and non-managed care plans.

To study selection, we compare OLS spending estimates from the active plan choosers to the IV estimates from the individuals randomly assigned to plans ("auto-assignees"). Removing the plan productivity differences (estimated via IV), we find that the difference between the average cost of the individuals actively choosing the most adversely selected plan is about 60% greater than the average cost of the individuals choosing the least adversely selected plan, suggesting substantial selection across plans in the market. This selection would otherwise confound estimates of the effect of plan choice on spending. Similarly, without accounting for variation in productivity across plans, average costs alone would generate misleading conclusions about the extent of selection in the market. Both of these facts highlight the importance of our research design.

We next ask what drives productivity differences across managed care plans. First, we decompose productivity differences into price and quantity effects, finding that a significant portion of the spending differences across plans are driven by differences in utilization, rather than price differences. This is important, given that the MCO literature has—until now—largely examined only the upstream price advantages of MCOs relative to traditional plans. We also decompose spending differences across health care spendings, finding that over 70% of the difference in spending between low-spending plans and high-spending plans is due to differences in inpatient hospital spending. This suggests that a key way the most efficient MCOs generate lower spending is by limiting inpatient stays. This result is consistent with recent evidence on the effects of Medicare Advantage on inpatient hospital utilization in Duggan *et al.* (2015).

Our findings are important for understanding how managed care can limit cost growth (or indeed, may fail to do so). In this way, our study connects to a long literature interested in the role managed care in particular, and competitive insurance markets more generally, in constraining healthcare spending growth. Specifically, our results on the extent and determinants of productivity differences across plans fit into the literature focusing on how different insurance plan characteristics affect spending. Examples of recent papers in this literature are Brot-Goldberg *et al.* (2015) who examine the effect of a change in deductibles on spending, Gruber and McKnight (2014b) who show how consumers use less healthcare in managed care plans with narrower provider networks, and Gaynor *et al.* (2004) who study the effect of capitated provider payments on overall healthcare spending. Additionally, by illuminating price and quantity channels, our paper connects to work by Dafny *et al.* (2016) and others who analyze the importance of MCO versus provider bargaining power in driving the prices that MCOs negotiate. Finally, as discussed above, our paper also naturally builds on the literature focusing on binary differences across contract types, such as public versus private plans (Duggan and Hayford, 2013) and HMOs versus non-HMOs (Cutler *et al.*, 2000). However, whereas most of the prior literature has treated managed care as a singular alternative to fee-for-service, our study is rare in documenting and explaining differences across managed care plans. Only a handful of studies, including Marton *et al.* (2014), are able to draw clear distinctions between the impacts of different organizational structures, contracting arrangements, or cost-reducing strategies in comparisons of managed care plans. And our study is unique in comparing managed care plans operating in the same market. This feature, plus random assignment of enrollees, allows us to draw comparisons robust to a variety of concerns about unobservables.

By shedding light on heterogeneity in spending among plans competing in the same local market our paper also contributes to the literature examining health care "exceptionalism," the notion that healthcare is special in the sense that productively inefficient firms are not driven out of the marketplace. Moral hazard and poor information dampen consumer responsiveness to quality and price. As Chandra *et al.* (2015) point out in their recent article, the concept dates back to Arrow (1963), and has attracted attention in the context of competition in healthcare markets (Cutler, 2010), and in the context of the regional variations of healthcare spending (see Skinner, 2012 for an overview). Our work shows that plans with identical patient pools, competing in the same markets, and contracting with a largely overlapping set of providers can have dramatically different production functions and total costs. This suggests that the literature focusing on explaining regional variation in healthcare spending has necessarily missed important *within*-region variation, a result that is consistent with other recent work (Cooper *et al.*, 2015).

Finally, our results have implications for the regulation of Medicaid Managed Care markets. Regulations prohibit the use of premiums in these markets, making it difficult for productively efficient plans to pass through savings to consumers and increase market share as they would in typical markets. In a setting where spending differed very little across plans, this lack of premiums would likely have only a small effect on the overall level of efficiency in the market. However, our results show that spending varies substantially across plans, suggesting a possibly important role for prices in these markets.

2.2 Conceptual Framework: Productivity and Profits in an MCO

In this section, we develop a simple conceptual framework for analyzing productive efficiency in a managed care organization (MCO). We begin by defining productive efficiency in an MCO as cost minimization conditional on generating a quality unit of output. We then describe the firm's profit objective and consider two classes of explanations of why competition among MCO plans may fail to achieve the productive efficiency frontier. First, we show that even abstracting away from the information, agency, and moral hazard problems of healthcare consumption, an MCO's profit objective diverges from the cost minimization (subject to quality) goal because in health insurance markets, firms can take actions to induce favorable selection. Second, even if the firm's profit objective were solely aligned with minimizing costs, it is possible that the various tools of managed care, such as gatekeeping and selective contracting, simply cannot induce efficient healthcare consumption by consumers in a setting in which consumers face a low or zero marginal cost of consumption.

2.2.1 Productive Efficiency as Cost Minimization

As Glied (2000), Gaynor *et al.* (2004), and Gaynor *et al.* (2015) outline in their overviews of the managed care literature, MCOs may generate productive efficiency via selective contracting, including network formation; via innovative provider payment models, such as capitation and revenue sharing; and by leveraging bargaining power to reduce prices paid to upstream providers. Implicit in such discussions is the notion that the productive efficiency of an MCO is closely tied to its ability to reduce medical spending. We link our definition of productivity to this notion.

Consider an MCO plan j with a general vector of characteristics ϕ_j that capture both supply-side features of plans like organizational structure and negotiated provider prices, as well as consumer-side features like cost-sharing schedules and patient gatekeeping. Consumers of type k select plans, and consumer characteristics ψ_k describe all consumer traits that enter plan choice and health spending decisions, such as chronic illnesses, preferences over medical care, and price sensitivity to cost sharing. Without loss of generality, j's paid-claims cost of enrolling consumer i of type k is $c_i(\phi_j, \psi_k) \equiv c_{ijk}$.

Let u represent a quality unit of a plan's output. Below, we will consider operationalizing u as either a consumer's valuation (of a plan year) or a regulator's assessment of plan quality. We define productivity A_{kj} as simply the negative of the average cost in plan j of providing a quality unit to consumers of type k:

$$A_{kj}(u) \equiv -\frac{1}{N_k} \sum_{i \in k} c_{ikj}(u). \tag{2.1}$$

Intuitively, a plan will rank as more productively efficient in our framework if it can produce a quality unit at a lower average cost than a competitor. We allow for productivity to differ across consumer types k because a plan that is efficient at treating pregnant women may not be efficient at managing chronic disease.

The definition in Eq (2.1) is identical, up to an intercept, to an output-minus-costs definition of productive efficiency.[8] This productivity definition tracks the general notion of productivity (in any industry) as related to the residual of output over input costs (Syverson, 2011). Inputs here are the hired services of physicians, hospitals, and other care providers.

[8]We could alternatively define $A_{kj}(u)$ as output minus costs, $u - \overline{c_{kj}}(u)$, though measuring u in dollars would require more stringent assumptions and structure, and in any case, comparisons of average costs across MCOs delivering a fixed-quality contract will be straightforward to interpret.

Our framework builds on Chandra *et al.* (2015), who consider productive efficiency in hospitals, and is consistent with earlier treatments of productivity in managed care plans, such as in Cutler *et al.* (2000), where productivity is linked to the managed care plan's quality adjusted cost of delivering services.[9]

2.2.2 The MCO Firm's Objective

Naturally, there is a close correspondence between the definition of productive efficiency in Eq (2.1) and an MCO's incentive to minimize $\overline{c_{jk}}$, subject to some demand constraint. Clemens *et al.* (2015), for example, model the MCO's objective as minimizing its paid claims while providing care of sufficient quality to satisfy the patient's reservation utility.

Nonetheless, the firm's profit objective may diverge from the efficiency condition in Eq (2.1) for several reasons, in particular the incentives firms face to manipulate the risk-adjusted payments they receive from regulators and to induce favorable selection. Let R_{ikj} represent plan j's risk-adjusted payment for enrolling consumer i of type k. Costs of delivering a quality unit u are $c_{ikj}(u)$ as above. Firm profits for enrolling a consumer of health type k are:

$$\pi_{kj}(u) = \sum_{i \in k} [R_{ikj} - c_{ikj}(u)] \times I_{ikj} \, . \tag{2.2}$$

If plans could affect only costs—a typical, but restrictive, assumption implicit in much of the MCO productivity literature—then maximizing the profit condition in Eq (2.2) would correspond exactly to maximizing our stylized productivity objective in Eq (2.1). In practice however, plans can affect the risk-adjusted payment R_{ikj} by investing resources towards generating higher intensity of diagnosis coding or towards services more likely to generate compensated diagnoses. For example, Geruso and Layton (2015) show that private plans

[9] We differ from Chandra *et al.* (2015) in a few ways. First we are examining MCOs rather than hospitals. Second, the nature of our setting naturally lends itself to consider holding output fixed at some quality level, and considering the corresponding input costs. This is the dual of the framework of Chandra *et al.* (2015), which holds input costs fixed and considers variation in output. Third, because the literature is explicitly interested in the role of MCOs in lowering provider prices, we consider alternative definitions of productivity that remove or leave in place the contribution of input prices to overall costs.

in Medicare generate 6%-15% higher payments (about $600-$1,500 per member per year) than are paid out under the public fee-for-service system. Plans generate these higher payments by reporting patients as relatively sicker than these patients would appear under fee-for-service.

The second wedge between the productive efficiency condition and the firm's objective is driven by selection. Even though plans in our empirical setting must accept all applicants (just as in essentially all US health insurance markets), they can nonetheless affect who chooses to enroll in their plans (I_{ikj}). For example, plans can advertise services that are differentially appealing to consumers who are low cost relative to the risk adjusted payment, and plans can set high shadow prices of care for treating conditions among enrollees who would be high cost relative to the risk adjusted payment (Ellis and McGuire, 2007). In equilibrium, this implies that $E[R_{ikj} - c_{ikj} | I_{ikj}] \neq E[R_{ikj} - c_{ikj}]$. In most settings, this selection on unobservables would complicate the measurement of productivity as $A_{kj} = -\overline{c_{kj}}$, because consumer characteristics that affect plan costs will be correlated with preferences and choices over plans. In contrast, we exploit random assignment across plans to eliminate the possibility of this type of bias when measuring productivity.

2.2.3 Market Failures and Productivity

Even setting aside the selection and diagnosis coding incentives that firms face, there is no guarantee that competitive pressures will actually deliver productive efficiency in terms of cost minimization. The information and agency problems well-known to be endemic to healthcare utilization (Arrow, 1963) have a direct bearing on a firm's *ability* to minimize costs. For example, one of the most basic ideas in managed care is to structure copays and deductibles to steer patients to more cost-effective care. But if consumers respond only weakly to such incentives, the insurer may not be able to induce optimal utilization, in terms of costs conditional on quality/utility. In the Medicaid setting, in which firms are prohibited from charging any cost sharing at all, the problem of finding appropriate tools to drive down costs is exacerbated. Indeed, whether competition works to drive down costs in this setting is an open question of considerable interest (e.g., Cutler *et al.*, 2000; Chandra *et al.*, 2015).

2.2.4 Private and Social Costs when Measuring Productivity

Cost in terms of paid claims will be affected both by quantities of healthcare resources consumed as well as factor prices paid to physicians, hospitals, and pharmacies. This raises a question about which measure of inputs are appropriate. For example, should one measure physician inputs as number of physician visits or spending on physician visits? The prior literature has largely considered price negotiation with providers as an (or possibly *the*) important role of an MCO, which suggests examining plan expenditure differences $\overline{c_{kj}}(u)$ across plans, without adjusting for differences in upstream prices paid.

In contrast, from a social welfare perspective, negotiated prices—to a first approximation—simply divide producer surplus between insurers and providers, without changing the consumption of real resources.[10] From this perspective the relevant cost variable is real resource utilization. Eq (2.1) can be recast to capture this alternative notion of productive efficiency by substituting resources (Q_{ikj}) in place of spending (c_{ikj}). With an appropriate price index, P_{kj}, quantities are just $Q_{ikj} = \frac{c_{ikj}}{P_{kj}}$. Below, we describe how we create plan-specific price indices to translate between these measures.

Given the focus on spending in the prior literature, we begin in Section 2.6 by treating the MCO's ability to negotiate low prices as a marker of productive efficiency—that is, we begin by reporting spending as our measure of productivity. We then show productivity estimates purged of price effects. Combining these results allows a decomposition that examines the share of productivity dispersion among MCOs that can be accounted for by differences in MCO's negotiated provider rates. Finally, as our definition of productive efficiency relates both to spending and to quality, we present variation across plans in

[10]The social welfare considerations are somewhat more nuanced than this. If MCO market power generates a check on, say, hospital market power, final prices paid by consumers will be affected. This will affect affect quantity of care consumed and will therefore impact social welfare, in addition to determining the split of producer surplus.

quality-adjusted spending.

2.3 Background and Setting

New York State experimented with managed care in Medicaid as early as 1967, expanding to a voluntary program in the 1980s and a mandatory program in the 1990s and 2000s.[11] Under mandatory managed care, recipients are required to join a managed care plan operated by a for-profit or not-for-profit third party organization.[12] The state still operates a fee-for-service Medicaid option for certain exempted groups, such as Native Americans, though the vast majority of beneficiaries are required to enroll in a managed care plan.[13]

We focus on New York City, which is comprised of five counties where enrollment in managed care is mandatory, and contains about two-thirds of the state's Medicaid population. The state encourages Medicaid recipients in mandatory managed care counties to actively choose their health plans. In excess of 90% of enrollees make an active choice during our study period, 2008-2012. Recipients who fail to choose within the required timeframe are automatically enrolled in a plan, a policy known as "auto assignment".[14] After notification of auto-assignment, the beneficiary has three months to switch plans

[11]The shift to mandatory managed care took place via county-by-county "enrollment mandates." The mandates initially applied only to children and TANF adults, but were subsequently expanded to include disabled Medicaid recipients (Sparer, 2008).

[12]The Temporary Assistance for Needy Families (TANF) program provides temporary financial assistance to pregnant women and families with dependent children.

[13]"Medicaid Fee-For-Service", in which the government is responsible for paying providers directly for Medicaid services. As of 2011, 23.3% of Medicaid recipients were enrolled in Fee-For-Service (FFS) in New York. FFS recipients either reside in counties that do not mandate managed care or are exempt/restricted from joining managed care. Examples of exempt recipients include Native Americans, those with a traumatic brain injury (TBI), or those with a chronic condition who are being treated by a specialist that does not accept payment from any of the managed care plans. Examples of recipients restricted from joining Medicaid managed care include recipients enrolled in Medicare, recipients in the Medicaid spend-down or excess income programs, recipients receiving hospice services at the time of enrollment, or children in foster care. Many of these exemptions/restrictions were lifted subsequently.

[14]As of October 1, 2011 newly mandated Medicaid recipient had 30 days to choose a plan, regardless of disability status. Prior to this policy, Supplemental Security Income (SSI) and SSI-related recipients had 90 days and non-SSI/SSI-related recipients had 60 days to choose a plan prior to auto-assignment. The authority for auto-assignment is granted pursuant to N.Y. Soc. Servs. L. §364-j(4)(f)(i).

without cause before a nine-month lock-in period begins. The details of how long a recipient has to actively choose a plan prior to being auto-assigned depends on how much time has elapsed since mandatory managed care was instituted in a county. For our study period in New York City, recipients had either 30 or 60 days to make an active choice. Conditional on auto-assignment, enrollees are randomly assigned across eligible plans with equal probability. Plans qualify as eligible for 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.[15]

There are two exceptions to the auto assignment policies described above. First, New York takes into account where family members are enrolled. If family members are enrolled in a managed care plan at the time of auto assignment, the recipient defaults to the family member's plan. Second, recipients who were enrolled in a managed care plan in the year prior to assignment are reassigned to their previous plan.[16] Recipients assigned on the basis of family members or prior enrollment are removed from the analysis.[17]

2.4 Data

To estimate variation in MCO productivity, we merge administrative health records from the New York State Department of Health (NYSDOH), managed care provider directories and hospital characteristics from the American Hospital Association. We briefly describe each data source here.

[15]Prevention Quality Indicators (PQIs) are a set of measures developed by the Agency for Healthcare Research and Quality to evaluate the quality of care for "ambulatory care sensitive conditions". These are conditions for which good outpatient care can prevent hospitalizations or complications.

[16]Preferential assignment to a prior plan does not apply if the recipient's prior plan was a partial capitation plan, a low quality plan, or a plan without further capacity.

[17]Auto assignments on the basis of family members of prior enrollment are not separately identified in the data. We adopt a conservative approach to remove these recipients, dropping individuals with family members on file at the time of auto assignment or any managed care enrollment in the year prior to auto assignment.

2.4.1 Administrative Enrollment and Claims Data

We obtained de-identified administrative data on enrollment, plan choice and insurance claims for the entire New York Medicaid population from 2007 to 2012.[18] For each recipient, we observe demographic data, monthly enrollment data and claims for medical services covered by Medicaid.[19] The enrollment data include an indicator that we use to identify recipients 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. New York State Department of Health staff have standardized the fee-for-service and managed care data. For the outpatient data, we 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, we 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.

2.4.2 Key Outcome Measures

The available data allows us to construct a range of recipient-level, monthly outcome measures. These measures are grouped into three domains: health care use and spending, access to care, and a measure of recipient satisfaction that we term "Willingness to Stay."

Health care use and spending. To measure health care spending for the Medicaid managed care population we use variables provided by the New York State Department of Health

[18]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 recipients.

[19]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).

(NYSDOH) to construct the following categories of service: physician office, hospital outpatient, emergency department, hospital inpatient, and other. The other category includes independent laboratory, transportation, diagnostic & treatment centers, and other miscellaneous services.[20]

Because monthly health care spending is a highly skewed limited dependent variable, we use log expenditures and will conduct robustness tests using general linear models (GLM) and two-part models similar to past work on health care expenditures (Newhouse, 1993; Buntin and Zaslavsky, 2004). For the two-part model, the extensive margin (first part) is an indicator variable for whether or not an individual had any medical claims in a month, estimated using a Probit model. The intensive margin (second part) is a continuous measure of monthly health care spending conditional on an individual having had at least one medical claim in a month. The impact of plans on the intensive margin is estimated using a general linear model (GLM) with a log-link function.[21]

Access to Care. We measure access by adapting measures developed by the Secretary of Health and Human Services (HHS) specifically for adult Medicaid enrollees, a requirement of the Affordable Care Act (Section 113B). We focused on measures that could be constructed using administrative data since we do not have access to individual-level survey data or electronic medical records. We examined two domains of access: compliance with recommended preventive care and the frequency of avoidable hospitalizations.[22] For preventive care, we examine the frequency of flu vaccination for adults ages 18 to 64, breast cancer screening, cervical cancer screening and chlamydia screening in women. For avoidable hospitalizations, we examine the admission rates for four conditions: (i) diabetes

[20] I remove pharmacy and dental services from the main analyses since pharmacy was carved out of the managed care benefit until October 2011 and dental is an optional service for MCOs. If MCOs elect not to cover dental, recipients access dental services through Medicaid fee-for-service. We also remove services carved out of the managed care benefit throughout the study period (primarily behavioral health).

[21] For inference with the two two-part model, these estimates are combined and standard errors are calculated using the delta method. Robustness tests are available upon request.

[22] Our limited sample and frequent churning in Medicaid managed care makes it difficult to implement the lookback periods for each measure. To simplify, we instead measure the frequency with which recipients use the recommended preventive care measures.

short-term complications; (ii) chronic obstructive pulmonary disease (COPD) or asthma in adults; (iii) heart failure and (iv) asthma in younger adults.

A common concern related to network adequacy is the time or distance patients must travel to providers. We measure access—as proxied by distance traveled—for four categories of service: physician office, hospital outpatient departments, emergency department, and hospital admissions. For each of these services we measure distance as the driving time (in minutes) from the centroid of the recipient's home zip code to the centroid of the providers zip code. For providers with multiple locations of service we use the modal zip code (since we don't observe the specific zip code reliably on each claim).

Willingness to Stay. The final outcome we study is a measure of recipient satisfaction that we term "willingness to stay". Here, we assume recipients' preferences are revealed through their subsequent plan choices ("voting with their feet"). This is possible because randomly assigned recipients are not "locked-in" to their assigned plans. For the first three months after assignment they may switch for any reason, after which a nine-month lock-period begins. Hence, they have ample opportunity to switch plans if they are dissatisfied.[23]

2.4.3 Sample Selection

To estimate the impact of MCO productivity, we construct two analysis samples. First, we construct a sample based on recipients that are automatically assigned (randomly) to their health plan, this is the "auto assignment sample." Figure 2.1 provides a broad overview of the auto-assignment process. Second, we draw a random sample of recipients that made active plan choices to evaluate variation across MCOs among a set of recipients that made active choices when signing up for Medicaid managed care, this is the "active choice sample."

Because loss of Medicaid coverage is common and recipients can switch plans on a monthly basis, our main specification uses months as the unit of analysis. Since recipients exit and re-enter Medicaid during the study period, we define each continuous enrollment

[23]Moreover, recipients can switch plans during the lock-in period for good cause.

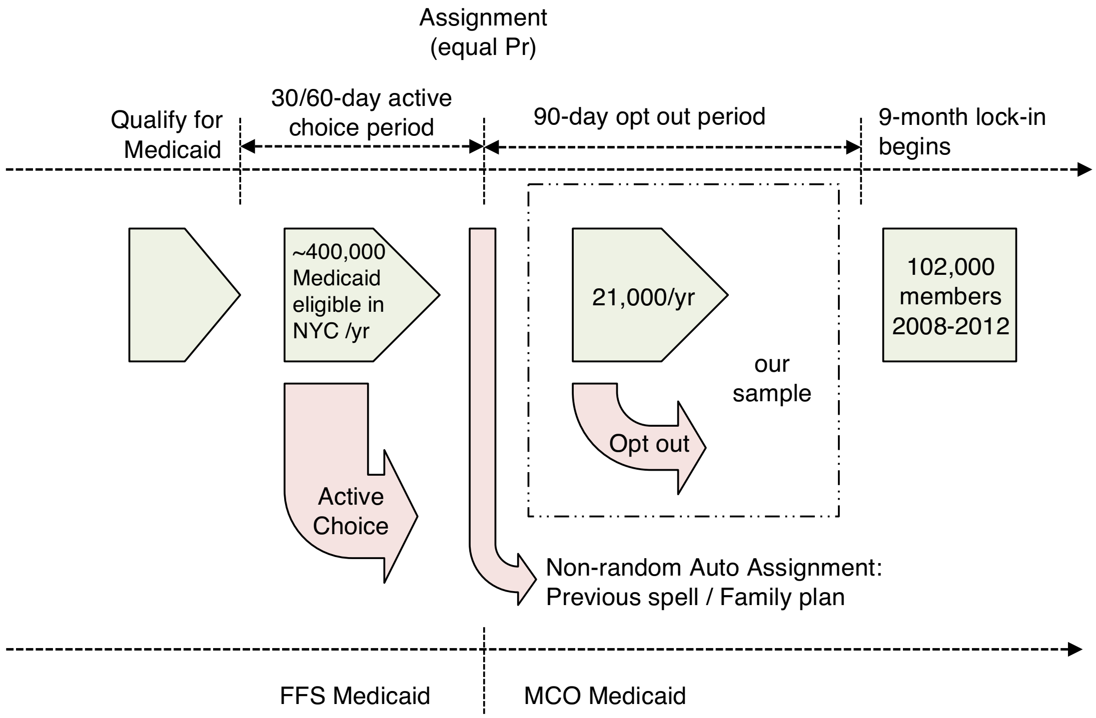

Figure 2.1: *Random Assignment*

Notes: This figure provides a flowchart of the auto assignment process in New York State Medicaid. Our sample consists of all Medicaid recipients aged 18 to 65 enrolled from April 2008 to December 2012 in the five counties in New York City. We exclude recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI).

period as a separate "eligibility episode." Recipients that are auto assigned are included in the sample until the eligibility episode in which they were assigned ends.[24] Additional eligibility spells are only included if the recipient is auto assigned multiple times.[25] For some eligibility episodes, recipients drop out of managed care and return to fee-for-service for brief periods. For our main analyses, we include these recipients but exclude spending and utilization during the fee-for-service months.

[24]If, for example, they re-enter Medicaid later and make an active plan choice that could be included as a Medicaid episode in the "active choice" sample. In practice, this is very rare.

[25]This is rare and the results are not sensitive to the exclusion of these recipients.

Auto Assignment Sample

To construct our auto assignment sample we make six restrictions. First, we drop recipients that live outside the five boroughs of New York City. Approximately one-third (35%) of Medicaid recipients reside outside New York City (either in Long Island or "upstate"). The focus on New York City allows us to identify variations in MCO productivity for plans operating in the same local market.[26] Second, we restrict the sample to recipients aged 18 to 65. We exclude individuals aged 65 and older because they become eligible for Medicare (often referred to as "dual eligibles") and are excluded from managed care (Vabson, 2015). We remove recipients below age 18 because we study the impact of plans on choice and switching behavior and it is difficult to interpret plan choice behavior for children. Third, we exclude Medicaid recipients with family members enrolled in a Medicaid managed care plan at the time of auto assignment. Since preference is given to plans that enroll family members, these auto assignments are non-random. Fourth, we exclude individuals enrolled in managed care plans within a year prior to assignment since they are preferentially placed into their prior plans. Fifth, to keep the sample reasonably well-balanced we exclude recipients auto-assigned to plans that were not in operation throughout the study period. Sixth, we remove individuals who qualify for Medicaid because they receive Supplemental Security income (SSI).[27] Excluding disabled Medicaid recipients makes the sample more comparable to the general population.

These sample restrictions leave us with 102,862 recipients in five boroughs and ten plans. The final sample includes 285 county by year by month observations. Since the unit of randomization is the county by year by month all analyses are clustered at this level. The median number of patient-month observations in each cluster is 5,211. Table 2.1 provides descriptive statistics.

[26]The randomly assigned Medicaid recipients in our sample reside in 170 different zip codes in New York City.

[27]Supplemental Security Income (SSI) is a federal government program that provides income to low-income individuals that are aged (65 or older), blind, or disabled. To qualify as "disabled" individuals must be unable to participate in gainful employment by reason of a physical or mental impairment expected to result in death or last for a continuous period of 12 months or more.

Table 2.1: *Descriptive Statistics*

	Mean	Std. Dev.	Obs.
	(1)	(2)	(3)
Any monthly utilization:			
All Services	0.285	0.451	900,449
Physician Office	0.103	0.304	900,449
Hospital Outpatient	0.072	0.258	900,449
Emergency Department	0.056	0.231	900,449
Hospital Inpatient	0.020	0.139	900,449
Other	0.205	0.404	900,449
Spending:			
All Services	261.887	1,842.572	900,449
Physician Office	25.203	206.181	900,449
Hospital Outpatient	24.849	225.141	900,449
Emergency Department	15.822	139.600	900,449
Hospital Inpatient	134.611	1,632.105	900,449
Other	61.401	385.104	900,449
Avoidable Hospitalizations:			
Any avoidable hospitalization	3.234	148.202	2,768,754
Diabetes short term complications	0.272	37.284	900,449
COPD or asthma (age 40 and older)	13.203	311.118	328,943
Congestive heart failure	0.432	62.289	900,449
Asthma (ages 18 to 39)	6.670	201.236	593,975
Preventive Care:			
Any preventive care	16.792	128.492	1,538,215
Cervical cancer screening	25.072	156.344	416,120
Chlamydia test	60.067	237.612	137,713
Breast cancer screening	23.087	150.180	61,464
Flu vaccination	6.183	78.386	900,449

Notes: This table reports summary statistics for the sample of Medicaid beneficiaries who were auto-enrolled in a managed care plan (Auto Assignee Sample). The summary measure of avoidable hospitalizations (any avoidable hospitalizations) and preventive care (any preventive care) are done by stacking the data on the individual measures that comprise them.

Active Choice Sample

To construct our active choice sample we make four restrictions. First, we drop recipients that live outside the five boroughs of New York City. Approximately one-third (35%) of Medicaid recipients reside outside New York City (either in Long Island or "upstate"). The focus on New York City allows us to identify variations in MCO productivity for plans operating in the same local market. Second, we restrict the sample to recipients aged 18 to 65. We exclude individuals aged 65 and older because they become eligible for Medicare (often referred to as "dual eligibles") and are excluded from managed care (Vabson, 2015). We remove recipients below age 18 because we study the impact of plans on choice and switching behavior and it is difficult to interpret plan choice behavior for children. Third, to keep the sample reasonably well-balanced and to match our auto assignment sample we exclude recipients from plans that did not receive auto-assigneees throughout the study period. Fourth, we remove individuals who qualify for Medicaid because they receive Supplemental Security income (SSI).[28] Excluding disabled Medicaid recipients makes the sample more comparable to the general population. These sample restrictions leave us with approximately 100,000 recipients in five boroughs and ten plans. The final sample includes 285 county by year by month observations.

Unsurprisingly, the active choice sample differs from the auto assignment sample in a number of ways (see Appendix Table B.1). The two populations have different demographics, with the auto assignees more likely to be young, male and black. Mean utilization and spending also differ considerably across the two samples. The auto assignees consume more of all types of care except services in the physician's office. The same pattern holds for spending, with the auto assignees spending, on average, \$263 per month as compared to \$154 per month for the active choice recipients. A large driver of these differences is the shorter Medicaid tenure of the auto assignees relative to the active choice recipients.

[28]Supplemental Security Income (SSI) is a federal government program that provides income to low-income individuals that are aged (65 or older), blind, or disabled. To qualify as "disabled" individuals must be unable to participate in gainful employment by reason of a physical or mental impairment expected to result in death or last for a continuous period of 12 months or more.

2.5 Research Design

We separately identify the impacts of plan productivity and consumer selection on a plan's overall spending level by exploiting the random plan assignment of auto-assignees. In this section, we describe the procedure, which makes use of both the auto-assignee (randomization) subsample and the subsample of active choosers in the same market.

2.5.1 Framework

To provide motivation for our empirics, consider a simple setup where there are two plans and enrollees are categorized into types: $t = 1, 2, ..., T$. Now assume the two plans are differentiated by how much they would spend on a randomly-assigned recipient. Designate the high-spending plan as "High" (H) and the low-spending plan as "Low" (L). In this context, individual health care spending can be written as:

$$c_i = b_i + \gamma_t^H \mathbf{1}[H = 1] \tag{2.3}$$

where b_i is i's spending in L and varies at the level of the individual, γ_t^H is the "treatment effect" on spending of a type-t individual enrolling in H. This varies at the level of the recipient type (these categories could be based on demographics or risk scores). For recipients that select their plans, we observe average costs in both plans which we can express as:

$$\begin{aligned} AC^L &= E[c_i|H = 0] = E[b_i|H = 0] \\ AC^H &= E[c_i|H = 1] = E[b_i|H = 1] + E[\gamma_t^H|H = 1] \end{aligned} \tag{2.4}$$

We can then decompose the difference in spending between recipients in the low and high plans as:

$$AC^H - AC^L = \underbrace{E[\gamma_t^H|H = 1]}_{\text{Treatment Effect}} + \underbrace{(E[b_i|H = 1] - E[b_i|H = 0])}_{\text{Selection}} \tag{2.5}$$

As we demonstrate in the following section, we estimate the expected value of the treatment effect conditional on (random) auto-assignment:

$$\hat{\gamma} = E[\gamma_t^H | A = 1] \tag{2.6}$$

where A is an indicator of auto assignment. If we assume that $E[\gamma_t^H|A = 1]$ equals $E[\gamma_t^H|H = 1]$ the decomposition simplifies to:

$$AC^H - AC^L = \hat{\gamma} + (E[b_i|H = 1] - E[b_i|H = 0]) \tag{2.7}$$

If the assumption that $E[\gamma_t^H|A = 1]$ equals $E[\gamma_t^H|H = 1]$ is inappropriate, we have a misspecification where:

$$AC^H - AC^L = \underbrace{E[\gamma_t^H|H = 1] - E[\gamma_t^H|A = 1]}_{\text{Uncaptured Treatment Effect Heterogeneity}} + \underbrace{(E[b_i|H = 1] - E[b_i|H = 0])}_{\text{Selection}} \tag{2.8}$$

This misspecification may arise if the types of recipients that are auto assigned are different than those that make active choices (Appendix Table B.1). Now we can rewrite the decomposition as:

$$AC^H - AC^L = \frac{1}{N^H} \sum_{t=1}^{T} N_t^H \gamma_t^H + (E[b_i|H = 1] - E[b_i|H = 0]) \tag{2.9}$$

with this setup we can further decompose the average cost difference between H and L as follows:

$$AC^H - AC^L = \underbrace{\overline{\gamma}}_{\text{ATE}} + \underbrace{\frac{1}{N_h N} \sum_{t=1}^{T} \gamma_t^H (N_t N^H - N_t^H N)}_{\text{Selection on Production}} + \underbrace{(E[b_i|H = 1] - E[b_i|H = 0])}_{\text{Selection}} \tag{2.10}$$

where the first term $\overline{\gamma}$ is equivalent to $\frac{1}{N} \sum_{t=1}^{T} N_t \gamma_t^H$, or the average treatment effect (ATE) across the population. The second term, $\frac{1}{N_h N} \sum_{t=1}^{T} \gamma_t^H (N_t N^H - N_t^H N)$ represents "selection on production", or the portion of the difference in costs between L and H due to sorting on treatment effect heterogeneity. The final term, $(E[b_i|H = 1] - E[b_i|H = 0])$, represents classic selection on health status.

2.5.2 Measuring Productivity

We now move from our stylized example to a detailed discussion of how we estimate productivity differences across the ten plans in our sample. First, we regress individual-level healthcare spending on indicators for enrollment in one of the ten MCO plans competing in the New York City market:

$$Y_{ict} = \alpha + \phi_{ct} + \delta X_{ict} + \sum_{j=1}^{10} \gamma_j \mathbf{1}[\text{Plan j}_{\ ict}] + \epsilon_{ict}. \tag{2.11}$$

Here, i denotes recipients, j denotes plans, t is time in months, and c is county. The variable $\mathbf{1}[\text{Plan j}_{\ ict}]$ indicates enrollment in plan j during month t, the coefficients γ_j capture the average impact of each plan on spending. X is a vector of individual-level controls such as race, age, gender and a baseline measure of the outcome being studied. For parsimony below, we denote the vector of plan indicators as $\mathbf{Plan}_{ict}$.

The problem for inference is that OLS estimates of γ are biased if a recipient's plan choice is correlated with unobservable determinants of the outcome: $E[\epsilon_{ict}|\mathbf{Plan}_{ict}] \neq 0$. This would be the case, for example, if individuals sort into plans on the basis of expected health care costs. To address this problem, we instrument for the vector $\mathbf{Plan}_{ict}$ with indicators for random assignment to each plan. Given our source of variation, the X variables serve to reduce variance but are not necessary for unbiased estimation. We include a set of county $\times$ year $\times$ month of assignment fixed-effects (ϕ_{ct}) because this is the unit at which randomization occurs.

Since the auto assignment is not permanent (i.e., individuals can switch out of the plan to which they are auto-assigned) we use two-stage least squares to obtain unbiased estimates of the plan effects. There are ten plans that receive auto-assigned enrollees during our time period, requiring ten first-stage regressions, one for each plan. For each first-stage regression, we predict the probability that an individual enrolls in plan j as a function of a full set of plan assignment dummy variables. For all auto-assigned enrollees, only one of the plan assignment variables will be equal to one. Formally, we estimate the following first-stage equations:

$$
\begin{aligned}
\text{Plan 1}_{\,ict} &= \phi_{1ct} + \delta_1 X_{ict} + \sum_{j=1}^{10} \lambda_{1j} \mathbf{1}[\text{Assigned j}_{\,ict}] + \eta_{1,ict} \\
&\vdots \\
\text{Plan 10}_{\,ict} &= \phi_{10ct} + \delta_{10} X_{ict} + \sum_{j=1}^{10} \lambda_{10j} \mathbf{1}[\text{Assigned j}_{\,ict}] + \eta_{10,ict}
\end{aligned}
\tag{2.12}
$$

where c is the county; and Assigned j $_{ict}$ is a dummy variable that indicates whether individual i was auto assigned to plan j. Note that the regressors in each of the first-stage equations are identical. Intuitively, λ_{kj} captures the probability that an individual auto-assigned to plan j will be enrolled in plan k during any given month. We expect that empirically for $k = j$, λ_{kj} will be large, while for $k \neq j$, λ_{kj} will be close to zero.

The second stage estimating equation is then:

$$
Y_{ict} = \alpha + \phi_{ct} + \delta X_{ict} + \sum_{j=1}^{10} \gamma_j \mathbf{1}[\widehat{\text{Plan j}}_{\,ict}] + \epsilon_{ict}. \tag{2.13}
$$

This IV strategy will result in estimates of the plan effects, γ, that use only variation in enrollment due to auto-assignment, which we can show is uncorrelated with other individual characteristics related to spending.

2.5.3 Measuring Selection

We now turn to measuring selection. The first step is to generate predicted spending for each recipient based on the estimated 2SLS regressions above. Revisiting the second stage equation from the previous section, we now plug in for the values of the estimated plan effects to generate predicted spending as follows:

$$
\widehat{Y}_{ict} = \widehat{\alpha} + \widehat{\phi_{ct}} + \sum_{j=1}^{10} \widehat{\gamma}_j \mathbf{1}[\text{Plan j}_{\,ict}], \tag{2.14}
$$

where notation is as above and hats indicate estimated values of the coefficients.

We use the estimates of γ and the other fixed effects from 2.13 to predict $\widehat{Y}_{ict}$ for each observation in the data, including both auto-assigned enrollees and enrollees making active plan choices. This produces a measure of individual spending purged of cross-plan differences in "production." We then calculate a selection residual for each recipient as:

$$R_{ict} = Y_{ict} - \widehat{Y}_{ict} \tag{2.15}$$

The residual measures the difference between observed spending (Y_{ict}) and predicted spending ($\widehat{Y}_{ict}$). Intuitively, a positive residual indicates that a given individual would have high spending in *any* plan, not just in the plan in which they were enrolled. This residual can be thought of as the portion of an individual's spending that is unaffected by their plan choice. This is a highly policy relevant varible, in that policymakers typically design risk adjustment systems to compensate plans for this portion of spending and not for the portion of spending that plans influence. To determine how this (purged of production differences) portion of spending varies across plans in the market, we estimate an OLS regression of R_{ict} on the plan dummies:

$$R_{ict} = \sum_{j=1}^{10} \theta_j \mathbf{1}[\text{Plan j }_{ict}] + \psi_{ict} \tag{2.16}$$

Here, the θ_j's capture the average residualized cost for each plan. We will use this vector to illustrate the extent of selection across plans in the market.

2.5.4 Decomposing Production Differences into Price and Quantity

In this section we outline our approach to decomposing differences in spending across plans into differences in quantity and price. In the first step, we estimate median price for each setting × service × year in our sample. We then construct quantity by "re-pricing" each claim in our sample at the median price for that location × service × year (McKellar *et al.*, 2014). We begin with the well-known equation that relates spending to price and quantity:

$$\mathrm{s}_j = p_j * q_j \tag{2.17}$$

where s_j , p_j, and q_j denote spending, price and quantity for plan j respectively. We then replace plan-specific prices by location x service x year with estimates of the median price across all plans. This removes the price variation across plans and yields a dollardenominated measure of quantity that holds price fixed ($\overline{s}$):

$$\overline{s_j} = \overline{p}_j * q_j \tag{2.18}$$

Now, armed with our quantity measure we can construct price differences across the plans by dividing spending by quantity as follows:

$$\overline{p} = \frac{s_j}{\overline{s}_j} \tag{2.19}$$

Intuitively, when we divide unadjusted spending (s_j) by our measure of quantity ($\overline{s}_j$), the result is price. In other words, once we control for quantity differences across the plans, any remaining variation in total spending is attributable to differences in price. To decompose the variation of spending into the shares attributable to price and quantity (or production and selection) we use the approach outlined by Fields (2003).

2.5.5 First Stage Results and Identifying Assumptions

The conditions necessary to interpret these two-stage least squares estimates as the causal effect of enrolling in a given health plan on spending for this group of individuals are: (1) random plan assignment is associated with enrollment in the actual plan that covers a recipient, (2) assignment only impacts recipient outcomes through its impact on the health plan they are enrolled in, and (3) the impact of auto assignment is monotonic across recipients.

The first condition is empirically testable. Figure 2.2 plots for each plan the probability that an individual who is auto-assigned to the plan is enrolled in the plan during any given month. Across the sample, recipients assigned to a plan spend approximately eighty percent of the months in a Medicaid episode in the plan to which they were assigned. An alternative way to view the strength of the instrument is examine the probability that a recipient is in a plan, conditional on not being assigned to that plan. These probabilities, drawn as lighter bars in Figure 2.2, are much lower and almost not visible in the graph because of their relatively smaller size. In Figure 2.3, we present unadjusted probabilities that recipients are

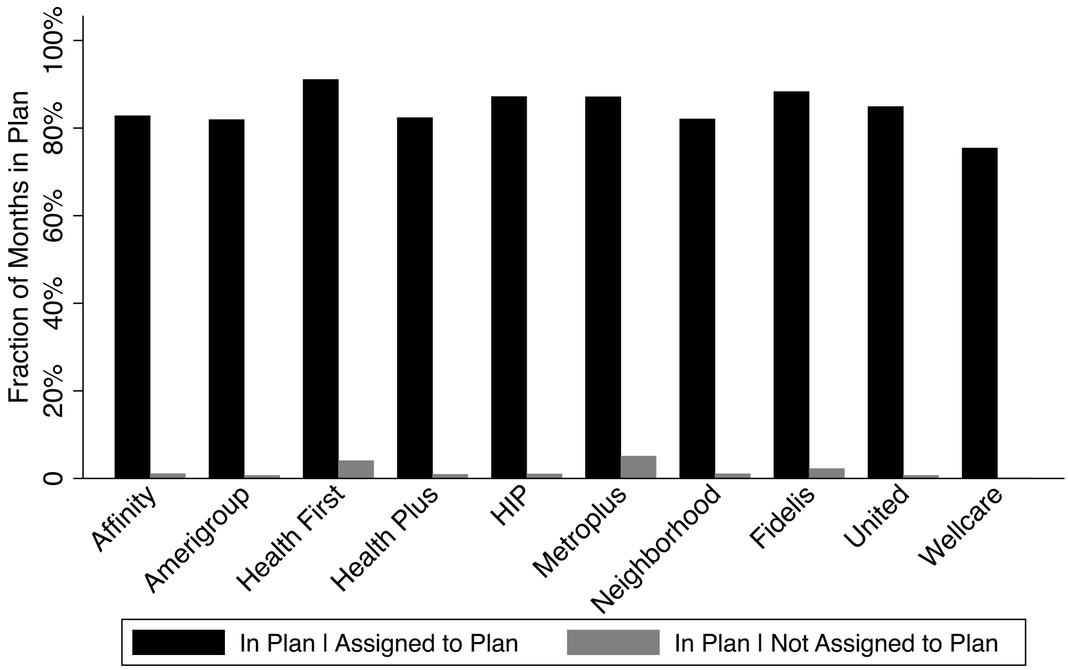

Figure 2.2: *First Stage: Plan Enrollment Conditional on Random Assignment*

Notes: Figure displays evidence on the strength of the first stage. Darker bars correspond to the probability of being observed in a plan, conditional on being randomly assigned to that plan. Lighter bars correspond to the probability of being observed in a plan, conditional on *not* being randomly assignment to that plan.The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (*N*=900,449 patient months).

in each of the ten plans conditional on defying their original assignment—i.e., switching out of the plan they were originally assigned to. In other words, we plot the distribution of plan choices among non-compliers. This previews some of our later results which show that auto-assigned recipients tend to favor certain plans with approximately half of the switchers going into either Health First or Metroplus. Table 2.2 presents analogous recipient-level estimates with and without plan controls.

The second assumption is that plan assignment only impacts recipient outcomes through its impact on the plan a recipient is enrolled in. Table 2.3 presents a series of randomization tests to partially assess this exclusion restriction. In column (2), each baseline (i.e., pre-randomization) characteristic is regressed on the full set of plan fixed effects. Each regression controls for county-by-year-by-month of assignment fixed effects. We report the p-value

Table 2.2: *First Stage Estimates of the Impact of Plan Assignment on Plan Enrollment ("Auto Assignees")*

	Controls	
	No	Yes
	(1)	(2)
Assigned Plan: United	0.854***	0.853***
	(0.006)	(0.006)
Assigned Plan: Wellcare	0.771***	0.770***
	(0.011)	(0.011)
Assigned Plan: Neighborhood	0.825***	0.825***
	(0.005)	(0.005)
Assigned Plan: HIP	0.876***	0.876***
	(0.006)	(0.006)
Assigned Plan: Affinity	0.832***	0.832***
	(0.005)	(0.005)
Assigned Plan: Fidelis	0.869***	0.870***
	(0.004)	(0.004)
Assigned Plan: Health First	0.879***	0.881***
	(0.008)	(0.008)
Assigned Plan: Health Plus	0.827***	0.825***
	(0.006)	(0.006)
Assigned Plan: Metroplus	0.823***	0.823***
	(0.006)	(0.006)
Kleibergen-Paap F-Stat	1,673	1,677
Cragg-Donald F-Stat	86,418	87,349
Observations	900,449	900,449

Notes: This table reports first stage results. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,449 patient months). Baseline controls are fixed effects for the county-year-month of assignment, the unit of randomization. Full controls also includes fixed effects for recipient zip code, six-month episode buckets (how long since the recipient was auto-assigned), five-year age x gender fixed effects, and race fixed effects. All standard errors are clustered on the county x year x month of assignment, the unit of randomization.

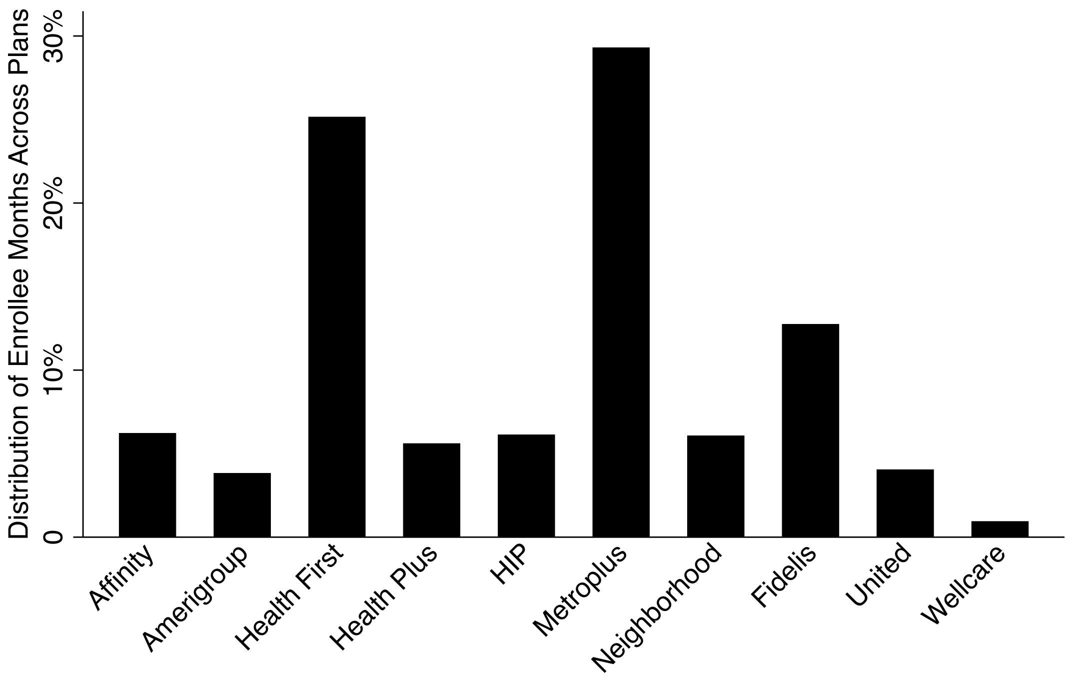

Figure 2.3: *Plan Enrollment Outcomes of Non-Compliers (Switchers)*

Notes: Figure shows the distribution of realized plan enrollments among non-compliers. Non-compliers are defined as auto-assignees who are observed in any plan other than their randomly assigned plan. The figure indicates that Health First and Metroplus are popular destinations for switchers. As in Figure 2.2, the sample is additionally limited to recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,449 patient months).

from an F-test that the plan effects are jointly different than zero, which is a test of the null hypothesis that the baseline characteristics do not differ significantly among recipients assigned to different plans. None of the baseline variables are significantly related to plan assignment at the five percent level. The joint F-tests for male and inpatient utilization were significant at the ten percent level, with p-values of 0.054 and 0.050, respectively. For comparison, column (3) presents an identical set of tests on a sample of Medicaid recipients that made *active* plan choices. We choose an active-chooser random subsample that is equal in size to our auto-assignee sample in column (2) to facilitate comparison. For recipients that made active choices, the results reveal considerable selection on both demographics and baseline spending. These findings highlight the value of our identification strategy for disentangling the role of the Medicaid MCOs from the role of selection by consumers.

Table 2.3: *Test of Randomization: Auto Assignee and Active Chooser Subsamples*

		F-Test P-value	
	Baseline Mean (1)	Auto Assigned (2)	Self Selected (3)
Age	37.462	[0.656]	[0.000]**
	(13.807)		
Male	0.399	[0.054]†	[0.000]**
	(0.490)		
Black	0.285	[0.560]	[0.000]**
	(0.451)		
Baseline Office Spending	33.891	[0.375]	[0.000]**
	(150.862)		
Baseline OPD Spending	38.674	[0.467]	[0.000]**
	(143.230)		
Baseline Clinic Spending	24.162	[0.457]	[0.000]**
	(115.524)		
Baseline Inpatient Spending	156.306	[0.161]	[0.000]**
	(1152.549)		
Baseline Office Quantity	0.281	[0.941]	[0.000]**
	(0.642)		
Baseline OPD Quantity	0.218	[0.415]	[0.000]**
	(0.743)		
Baseline Clinic Quantity	0.199	[0.497]	[0.000]**
	(1.025)		
Baseline Inpatient Spending	0.019	[0.050]†	[0.000]**
	(0.093)		
Observations		102,862	102,862

Notes: This table reports reduced form results testing the conditional random assignment of recipients to health plans. The active chooser sample was randomly sampled from the larger active chooser population to match the auto-assignee sample size. The auto assignment sample consists of Medicaid recipients aged 18 to 65 auto-assigned to managed care plans from April 2008 to December 2012 in the five counties in New York City (N=102,862 recipients). Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded. Recipients with family members in managed care at the time of assignment or who were enrolled in Medicaid managed care in the year prior to assignment are excluded because their assignments are nonrandom. For some individuals in the sample, less than a year of prior data is available. Baseline spending and utilization variables are formed by calculating the monthly mean in the two years prior to assignment for each recipient. ** = significant at 1 percent level, * = significant at 5 percent level, † = significant at 10 percent level.

The third identifying assumption is that there is a monotonic impact of plan assignment on on plan enrollment. The monotonicity assumption implies that being assigned to a plan does not result in a reduced probability of being in that plan. This assumption would be violated if recipients assigned to a specific plan were less likely to be in that plan than those not assigned. Reassuringly, Figure 2.2 demonstrates that for each plan in the sample random assignment results in increased enrollment. Given the magnitude of first stage effects, sub-analyses by recipient type are unnecessary.

2.6 Results

2.6.1 Health Care Use and Spending

Table 2.4 presents our main spending results. Columns (1) through (3) present results for the sample of individuals who were auto-assigned to a plan and column (4) presents results for the sample of individuals who actively chose a plan. In all regressions, the dependent variable is log monthly spending (+1), standard errors are clustered at the county-by-month of assignment level, and Amerigroup is the "left-out" plan, so that all estimated effects are relative to Amerigroup levels. Table 2.4 describes the results from similar regressions using spending in dollars as the dependent variable. Column (1) presents the OLS results for the auto-assignee sample. It is clear that there is wide variation in average spending across plans, even conditioning on our set of control variables. The average cost in Wellcare is only about 7% higher than the average cost in Amerigroup, while the average cost in Metroplus is over 60% higher.

While the OLS results in column (1) are estimated only on the group of individuals auto-assigned to plans, they do not represent unbiased estimates of the causal effect of enrolling in a plan on an individual's spending (i.e., production). This is due to the fact that individuals need not remain in the plan to which they are auto-assigned. We address this issue using the instrumental variables strategy we describe in Section 2.5. Column (3) describes the results using two-stage least squares, instrumenting for plan enrollment using

Table 2.4: *OLS and IV Estimates of Plan Effects on Log Spending*

	Auto Assignment Sample			Active Choice Sample
	OLS	RF	2SLS	OLS
Plan Name	(1)	(2)	(3)	(4)
United	0.086^{***}	0.035	0.049	0.556^{***}
	(0.025)	(0.026)	(0.030)	(0.082)
Wellcare	0.069^{*}	0.046	0.051	-0.020
	(0.041)	(0.042)	(0.054)	(0.030)
HIP	0.182^{***}	0.068^{***}	0.093^{***}	0.036
	(0.026)	(0.023)	(0.028)	(0.061)
Affinity	0.151^{***}	0.091^{***}	0.110^{***}	0.039
	(0.025)	(0.024)	(0.029)	(0.043)
Neighborhood	0.136^{***}	0.087^{***}	0.105^{***}	0.378^{***}
	(0.023)	(0.023)	(0.028)	(0.083)
Fidelis	0.339^{***}	0.146^{***}	0.181^{***}	0.551^{***}
	(0.024)	(0.024)	(0.028)	(0.035)
Health First	0.632^{***}	0.220^{***}	0.265^{***}	0.138^{**}
	(0.032)	(0.033)	(0.038)	(0.060)
Health Plus	0.336^{***}	0.231^{***}	0.277^{***}	0.520^{***}
	(0.024)	(0.025)	(0.030)	(0.026)
Metroplus	0.615^{***}	0.268^{***}	0.325^{***}	0.281^{***}
	(0.021)	(0.025)	(0.031)	(0.051)
Full Controls	YES	YES	YES	YES
Observations	900,449	900,449	900,449	3,034,035
Joint F-Test	[0.000]	[0.000]	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variable is log monthly spending. The independent variables are plan effects relative to the reference plan (Amerigroup). Columns (1), (2), and (3) present specifications for the sample of individuals who were auto-assigned to a plan. Column (1) presents results from an OLS regression of log spending on plan dummies. Column (2) presents results from reduced form regressions of log spending on dummies indicating the plan to which the enrollee was auto-assigned. Column (3) presents IV results estimated using two-stage least squares (2SLS) from the estimating equation (2.13). Column (4) presents results from an OLS regression of log spending on plan dummies for the population of enrollees who actively chose a plan. Controls include baseline spending, five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The active choice sample is randomly drawn from recipients that were not auto-enrolled. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

plan assignment. Here, the variation in spending across plans is smaller, though still sizable and statistically significant.

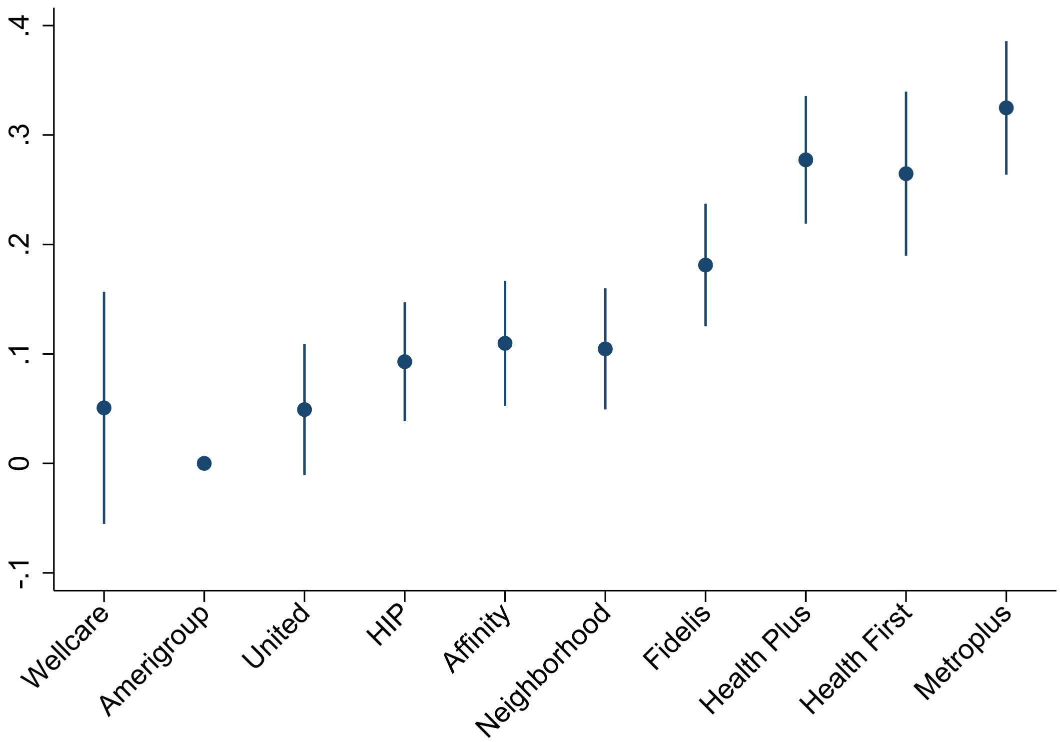

Figure 2.4: *Main Results: IV Estimates of Plan Effects on Spending*

Notes: The figure plots coefficients on instrumented plan indicators from 2SLS regressions in which the dependent variable is log spending. Regressions are run at the recipient $\times$ month level, and the sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,449 patient months).

These IV results from column (3) are presented graphically in Figure 2.4. The figure illustrates the extent of variation in spending across plans. These coefficients can be interpreted as the causal effect on spending of enrolling in a given plan rather than the left-out plan: If an individual enrolls in Metroplus, she will spend over 30% more than she would have spent had she enrolled in Amerigroup. Appendix Table B.2 presents the same results using (winsorized) spending in dollars rather than log spending as the dependent variable. The coefficients show that if an individual enrolls in Metroplus, she will spend over \$50 more per month, or over \$600 more annually, than she would have spent had she enrolled in Amerigroup. This is a substantial spending difference given that cost-sharing is identical in all plans, and all plans receive (almost) the same payment for a given individual. This variation across plans is not restricted to outliers: The eight other plans competing in the market are approximately uniformly distributed between these extremes in terms of effects on spending.

Comparing columns (1) and (3) shows the importance of instrumenting for plan enrollment using plan assignment, even among the sample of individuals auto-assigned to plans. The differences between the coefficients in these columns are driven by individuals switching plans after being auto-assigned. The patterns of the coefficients suggest that there is selection on the plan's true level of spending, with sicker enrollees choosing plans with higher spending levels. This pushes the OLS coefficients higher than the TSLS coefficients for the highest spending plans. This is particularly interesting, given that it is the first evidence of which we are aware of sicker consumers selecting plans based on unobserved (to the econometrician) components of plan generosity.

The first evidence of selection among the active choosers is given in Column (4) of Table 2.4, which presents the results from an OLS regression of log spending on indicators for each plan among the population of individuals actively choosing their plans. The coefficients in this column include variation across plans in both production and selection, and they are quite different from the TSLS estimates, indicating substantial selection across plans among the active choosers. Below, we will explore selection more thoroughly by purging overall levels of spending of differences across plans in "production," focusing only on differences in selection. The spending estimates in this section are based on estimating equation 2.13 with controls for a series of recipient-level variables including five-year age $\times$ gender fixed effects, race fixed effects, six-month episode buckets (measuring time since assignment), and zip code fixed effects. These variable primarily serve to absorb variation. To evaluate their impact on our results we compare our main estimates to main plan effects estimated without recipient-level controls. These results are presented in Appendix Table

B.3. Reassuringly, the point estimates are similar with and without controls. Two exceptions are Wellcare and Health Plus, where the introduction of controls increases the magnitude of their point estimates (although not significantly).

The TSLS findings regarding average spending, or production, differences across plans mask significant heterogeneity in the impact plans have on different recipient types. Figure B.2 plots coefficients from our main estimating equation estimated separately by recipient gender, age and baseline spending. These results are also presented in Table 2.5. The most notable result is that the distribution of plan effects differs for recipients with low and high baseline (pre-randomization) spending. More specifically, there is significantly more dispersion in spending for recipients with high baseline health care use. If an individual with low baseline spending enrolls in Metroplus, she will spend only 13% more than if she had enrolled in Amerigroup. On the other hand, if an individual with high baseline spending enrolls in Metroplus, she will spend around 46% more than if she had enrolled in Amerigroup. Overall, the variation in spending across plans for recipients with high baseline spending was considerably higher (standard deviation of 15.1 log points) than those with low baseline spending (standard deviation of 6.2 log points). This suggests that the more efficient Medicaid MCOs may play a more important role in managing care for recipients with higher baseline needs.

In addition to heterogeneity by recipient type, there are also meaningful differences in services impacted by different plans. Appendix Table B.4 presents estimates of the plan effects on log spending by setting. To make the decomposition simpler, we focus on results from regressions with spending in dollars rather than log spending as the dependent variable. These results are presented in Appendix Table B.5.[29] We examined five categories of spending: (i) physician office; (ii) hospital outpatient department; (iii) emergency department; (iv) hospital inpatient; and (v) other. For each of the five categories the F-test for the joint significance of the plan effect coefficients had p-values of less than

[29] All regressions include a "winsorized" version of monthly spending in dollars as the dependent variable in order to eliminate extreme outliers that represent errors in the administrative claims data.

Table 2.5: *IV Estimates of Plan Effects on Log Spending by Recipient Characteristics*

	Gender		Ethnicity		Age		Baseline Spending	
	Male	Female	Black	Non-Black	Under 40	40 and older	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
United	0.035	0.065	0.036	0.070	0.023	0.097	0.032	0.040
	(0.039)	(0.042)	(0.038)	(0.044)	(0.028)	(0.062)	(0.027)	(0.054)
Wellcare	0.063	0.065	0.016	0.117	0.072	0.037	0.088	−0.044
	(0.067)	(0.085)	(0.085)	(0.080)	(0.056)	(0.117)	(0.063)	(0.086)
Neighborhood	0.078**	0.142***	0.079**	0.145***	0.117***	0.100*	0.023	0.155***
	(0.036)	(0.037)	(0.034)	(0.039)	(0.027)	(0.054)	(0.027)	(0.048)
HIP	0.078**	0.102**	0.069*	0.119***	0.097***	0.088	0.053*	0.118**
	(0.038)	(0.040)	(0.037)	(0.041)	(0.028)	(0.054)	(0.028)	(0.049)
Affinity	0.065*	0.149***	0.092**	0.127***	0.109***	0.107*	0.035	0.146***
	(0.037)	(0.039)	(0.039)	(0.040)	(0.028)	(0.057)	(0.027)	(0.050)
Fidelis	0.225***	0.131***	0.132***	0.241***	0.121***	0.293***	0.092***	0.219***
	(0.038)	(0.040)	(0.034)	(0.043)	(0.028)	(0.054)	(0.025)	(0.048)
Health First	0.275***	0.245***	0.215***	0.314***	0.236***	0.312***	0.147***	0.287***
	(0.057)	(0.052)	(0.049)	(0.056)	(0.042)	(0.073)	(0.040)	(0.058)
Health Plus	0.281***	0.252***	0.214***	0.331***	0.232***	0.348***	0.191***	0.271***
	(0.039)	(0.043)	(0.040)	(0.043)	(0.030)	(0.060)	(0.032)	(0.054)
Metroplus	0.325***	0.312***	0.311***	0.341***	0.294***	0.379***	0.134***	0.460***
	(0.040)	(0.042)	(0.040)	(0.043)	(0.032)	(0.060)	(0.026)	(0.052)
Full Controls	YES	YES	YES	YES	YES	YES	YES	YES
Joint F-Test	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variable is log monthly spending. The independent variables are plan effects relative to the reference plan (Amerigroup). The table presents IV results estimated using two-stage least squares (2SLS) from the estimating equation (2.13) separately by gender, ethnicity, age and baseline spending. For the baseline spending measure, recipients were grouped into low and high based on whether they were below or above median baseline monthly spending in the sample. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

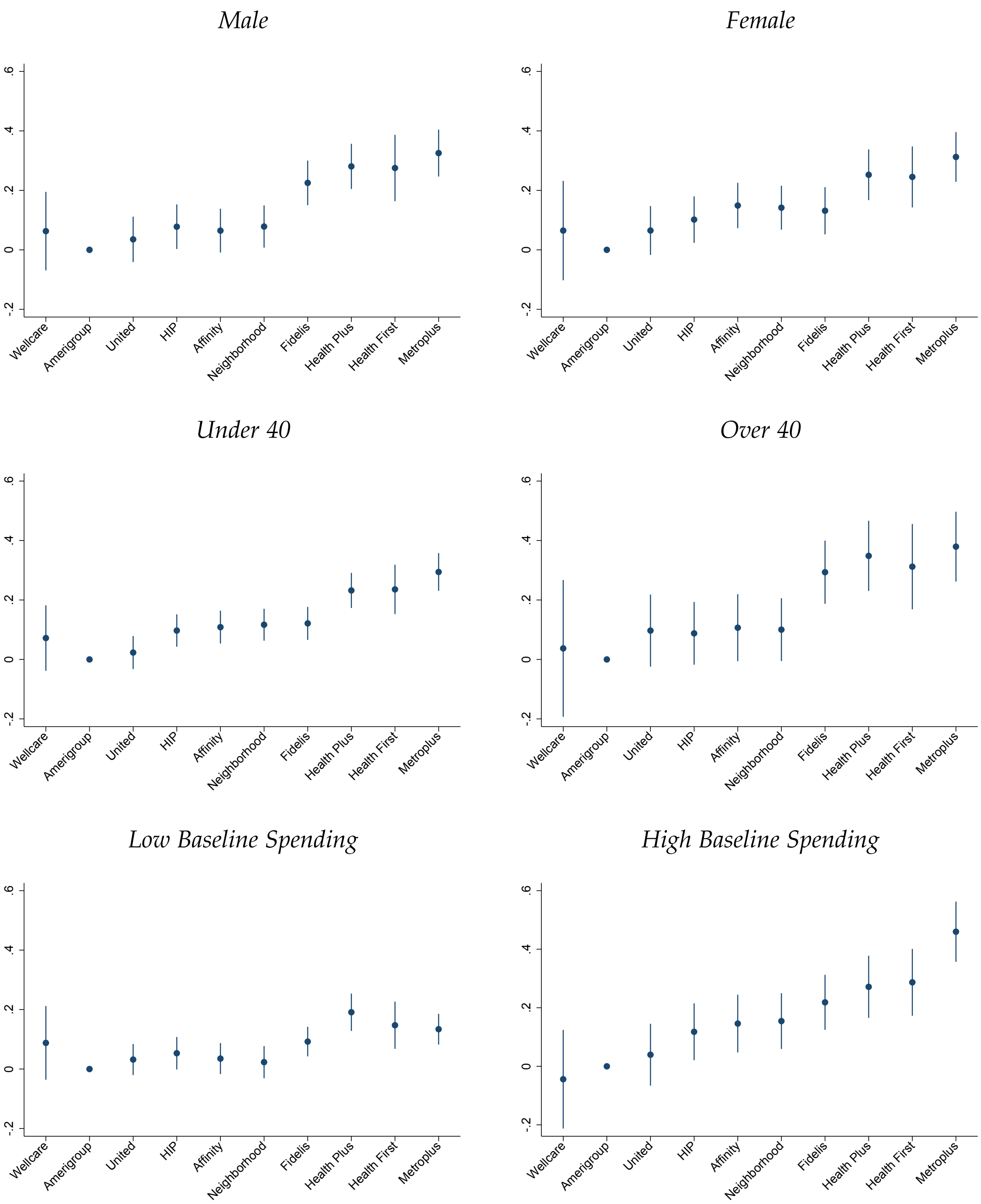

Figure 2.5: *Main Results: IV Plan Spending Effects by Recipient Characteristics*

Notes: The figure plots coefficients on instrumented plan indicators from 2SLS regressions in which the dependent variable is log spending. Subsamples are restricted by sex, age and baseline spending, according to the subfigure heading.

0.0001, allowing us to reject the null hypothesis that the plan fixed effects were jointly zero in each setting.

The coefficients in Appendix Table B.5 are somewhat difficult to interpret, but it is instructive to compare the coefficients of the high overall spending plans (bottom of the table) to the coefficients of the low overall spending plans (top of the table) to observe where they differ most. One obvious pattern is that the lowest spending plans reduce spending relative to the highest spending plans in the inpatient hospital setting: An individual enrolling in one of the top 5 highest spending plans will incur on average about $35 more per month in inpatient hospital spending than if she enrolls in one of the 5 lowest spending plans. The average monthly total difference in spending an individual would incur when enrolling in the top 5 highest spending plans vs. the 5 lowest spending plans is only $45, implying that about 78% of the total difference is due to differences in hospital inpatient spending.

We now turn to the question of what drives the variation in spending across MCOs in our sample. The first step is to decompose the spending variation into price and quantity (resource use). In addition to providing insight into how cost functions vary by MCOs, measuring resource use will provide additional insight into productivity differences across the plans. We begin by examining the impact of plans on a simple measure of quantity, the probability that a recipient seeks care in a given month (Appendix Table B.6). Similar to our estimates of the impact of plans on log spending, significant differences emerge across plans in the monthly probability that recipients use care. Panel (A) of Figure 2.6 plots our IV estimates of plan effects on health care use against our measure of production (log spending) differences across plans. The two measures are highly correlated (0.97) a preview of our finding that the variation in spending across plans is primarily explained by differences in utilization.

Using the methods outlined in Section 2.5.4 we construct a measure of quantity that is based on spending standardized by re-pricing each claim with the median price for that service $\times$ provider type $\times$ year. This approach purges differences that arise across plans because of different provider prices for the same services. Appendix Table B.7 presents our main estimates of plan effects (OLS and IV) on a logged version of this quantity measure. Column (3) contains the IV estimates where the variation in quantity (≈30%) is similar to the variation in log spending in Table 2.4. In Figure 2.6 we plot our IV estimates of plan effects on production (log spending) against the two quantity measures introduced in this section. The strong correlation between plan effects on spending and quantity is apparent when looking at either measure of quantity. In Panel (B) the adjusted R-squared of the underlying regression was 97%. Appendix Figure B.3 presents the same scatterplot using levels. The results are broadly similar, but noisier.

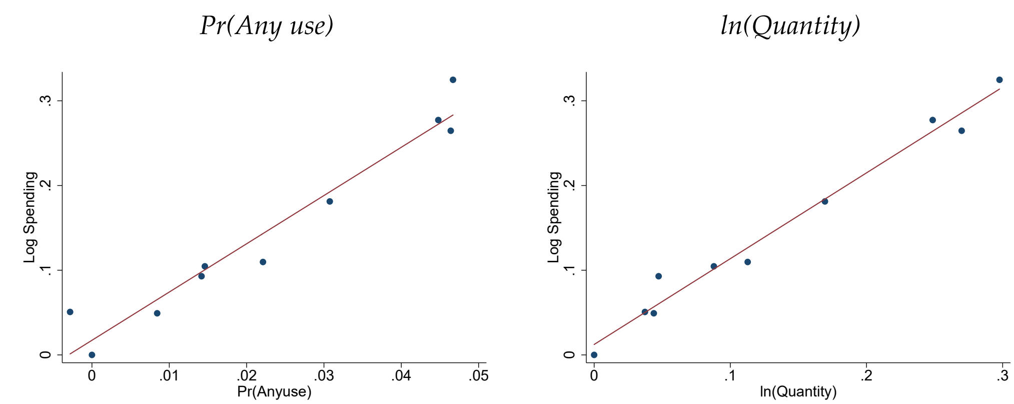

Figure 2.6: *Variation in Spending Driven by Quantity Differences across Plans*

Notes: The figure plots coefficients on instrumented plan indicators from 2SLS regressions in which the dependent variables are log spending and two measurs of quantity (any use in a month and a repriced claims measure) relative to the reference plan (Amerigroup). Panel (A) plots the probability of any use in a month against log spending. Panel (B) plots our measure of log quantity against log spending. Regressions are run at the recipient × month level, and the sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,449 patient months).

This approach underestimates the variation across plans attributable to price due to sparsely-populated service × provider type × year cells. If a plan dominates one of these cells than the median price will be entirely (or largely) based on that plans negotiated prices. When using our (price-purged) measure of quantity we will be attributing differences in price to differences in quantity. To address this, we re-run our IV estimates of plan effects on a measure of "adjusted quantity" (or "adjusted log quantity") in which we drop any months where standardized spending in that month is equal to actual spending. This is conservative but only results in dropping 1.5% of the recipient-month observations. Appendix Table B.8 presents OLS and IV estimates of plan effects on this adjusted log quantity measure. The broad ordering of plans remains similar, but several of the point estimates are different.[30] A regression of estimated log spending plan effects against adjusted log quantity yields an R-squared of 88%. Appendix Tables B.9 and B.10 present analogous estimates for unlogged versions of unadjusted and adjusted quantity.

2.6.2 Health Care Access and Recipient Satisfaction

This section presents results on the impact of managed care plans on measures of health care access and recipient satisfaction. First, we examine the impact of Medicaid MCOS on two measures of access: the rate of avoidable hospitalizations and compliance with recommended preventive care. I use avoidable hospitalizations because these measures are included in the 2015 Medicaid Adult Core Set, a set of access measures developed by the Secretary of Health and Human Services (HHS) for adult Medicaid recipients. In the core set, there are four measure of avoidable hospitalization: (i) diabetes short-term complications; (ii) chronic obstructive pulmonary disease (COPD) or asthma in adults; (iii) heart failure and (iv) asthma in younger adults. In addition to these measures, I examine four markers of compliance with recommended preventive care in the Medicaid Adult Core Set: (i) cervical cancer screenings; (ii) chlamydia testing; (iii) breast cancer screenings; and (iv) flu vaccinations Since health care encounters with these preventive services or avoidable hospitalizations are rare, we rescale each measure to be based on the rate per 1,000 recipient months. For both avoidable hospitalizations and prevention we create summary measures by stacking the individual measures with standard errors clustered at the county-by-year-by-month of assignment level.

Table 2.6 presents formal tests of the impact of plans on these measures using instrumen-

[30]Recipients in Metroplus, for example, consumed 30% more services than those in Amerigroup as measured by unadjusted quantity but only 21% more services as measured by adjusted quantity.

Table 2.6: *2SLS Estimates of Plan Impacts on Quality and Satisfaction*

	Quality of Care		
	Prevention (1)	Avoid. Hosp. (2)	Willingness To Stay (3)
United	-2.262^{***}	-0.424	0.038^{***}
	(0.704)	(0.373)	(0.007)
Wellcare	-1.032	1.237	-0.002
	(1.296)	(0.784)	(0.013)
HIP	0.358	4.367^{***}	0.065^{***}
	(0.690)	(0.923)	(0.007)
Affinity	0.217	0.564	0.030^{***}
	(0.681)	(0.499)	(0.007)
Neighborhood	-0.414	0.667	0.024^{***}
	(0.662)	(0.575)	(0.006)
Fidelis	-0.056	2.474^{***}	0.081^{***}
	(0.611)	(0.604)	(0.007)
Health First	1.781^{**}	4.101^{***}	0.113^{***}
	(0.844)	(1.357)	(0.009)
Health Plus	3.425^{***}	0.289	0.057^{***}
	(0.860)	(0.563)	(0.007)
Metroplus	0.317	3.483^{***}	0.072^{***}
	(0.682)	(0.752)	(0.006)
Full Controls	YES	YES	YES
Observations	1,538,215	2,768,754	900,449
Joint F-Test	[0.000]	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variables are two measures of access (prevention and avoidable hospitalizations) and one measure of consumer satisfaction, "willingness to stay." The independent variables are plan effects relative to the reference plan (Amerigroup). The plan effects are estimated using two-stage least squares (2SLS) from the estimating equation (2.13). Columns (1) and (2) present the results of estimating this specification using two measures of access built by stacking observations for the underlying measures. Controls include baseline spending, five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment (N=900,449 patient months). For per comparison significance for the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

tal variables. Column (1) presents two-stage least squares estimates of the impact of each plan on a summary measure of preventive care, created by stacking the four underlying measures. Relative to the reference plan (Amerigroup), recipients get significantly fewer preventive care services in one plan (United) and significantly more preventive care in two plans (Health First and Health Plus). Column (2) presents estimates of the impact of plans on avoidable hospitalization rates, a measure designed to evaluate the quality of ambulatory care. For avoidable hospitalizations, there are four plans with significantly higher rates than the reference plan. For both measures, we were unable to reject the null of no difference from the reference plan for the majority of plans in this market. However, given the magnitude of the differences in avoidable hospitalizations are very large, with some plans having double to triple the rate of avoidable hospitalizations among the auto assignees. This is a marked difference from our log spending estimates but partially reflect the noisier quality measures given a set of relatively rare conditions and services.

Appendix Tables B.11 and B.12 present IV estimates of the impact of plans on the underlying preventive and avoidable hospitalization measures, respectively. Due to the rarity of these types of services, the measures are noisier than those for health care use or spending. In figure 2.7 we plot our IV production estimates against summary measures for each of these quality measures. Interestingly, we find that higher spending on recipients is correlated with both an increase in the use of preventive care (good) and and an increase in the rate of avoidable hospitalizations (bad).

In addition to claims-based access measures, we also examine a measure of recipient satisfication based on plan choice behavior. Medicaid managed care recipients have three months from the time of assignment to switch plans without cause before they face a nine-month "lock-in" period during which they may only switch plans if they have "good cause" to do so. Even among the auto assigned population, plan switching occurs infrequently. For each plan that receives randomly assigned recipients we evaluate the stickiness of that assignment, terming this measure "willingness to stay." It takes on a value of one if recipients are in the plan they were originally assigned to.

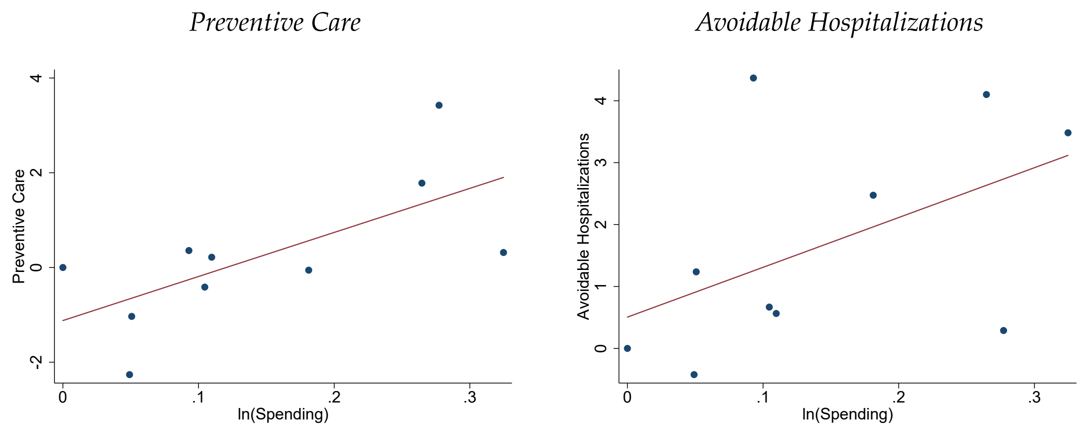

Figure 2.7: *Higher Spending Correlated With More Appropriate and Inappropriate Care*

Notes: The figure plots coefficients on instrumented plan indicators from 2SLS regressions in which the dependent variables are log spending and two measures of quality (preventive care and avoidable hospitalizations) relative to the reference plan (Amerigroup). Panel (A) plots preventive care use in a month against log spending. Panel (B) plots avoidable hospitalizations against log spending. Regressions are run at the recipient × month level, and the sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,449 patient months).

Column (3) of Table 2.6 presents instrumental variable estimates of the impact of plans on willingness to stay. Relative to recipients assigned to the reference plan (Amerigroup), recipients assigned to eight of the nine other plans had statistically significantly higher willingness to stay. Of interest is the relationship between production and willingness-to-stay. If plans that spend more on services for recipients are doing so without generating any value for recipients there is no reason to suspect a relationship between production and willingness-to-stay. Figure 2.8 is a scatterplot of our IV estimated plan effects on log spending on the x-axis and IV estimates of plan effects on willingness-to-stay on the y-axis. The visual presents a clear and positive relationship between the two sets of estimates, suggesting that plans which spend more on their recipients (primarily, as we know, through increased quantity) increase recipient satisfaction and reduce the likelihood of recipients switching out of their plan. Appendix Figure B.5 presents an analogous scatter plot using spending levels (instead of logs). Although the relationship is noisier, it remains positive and significant with only ten data points.

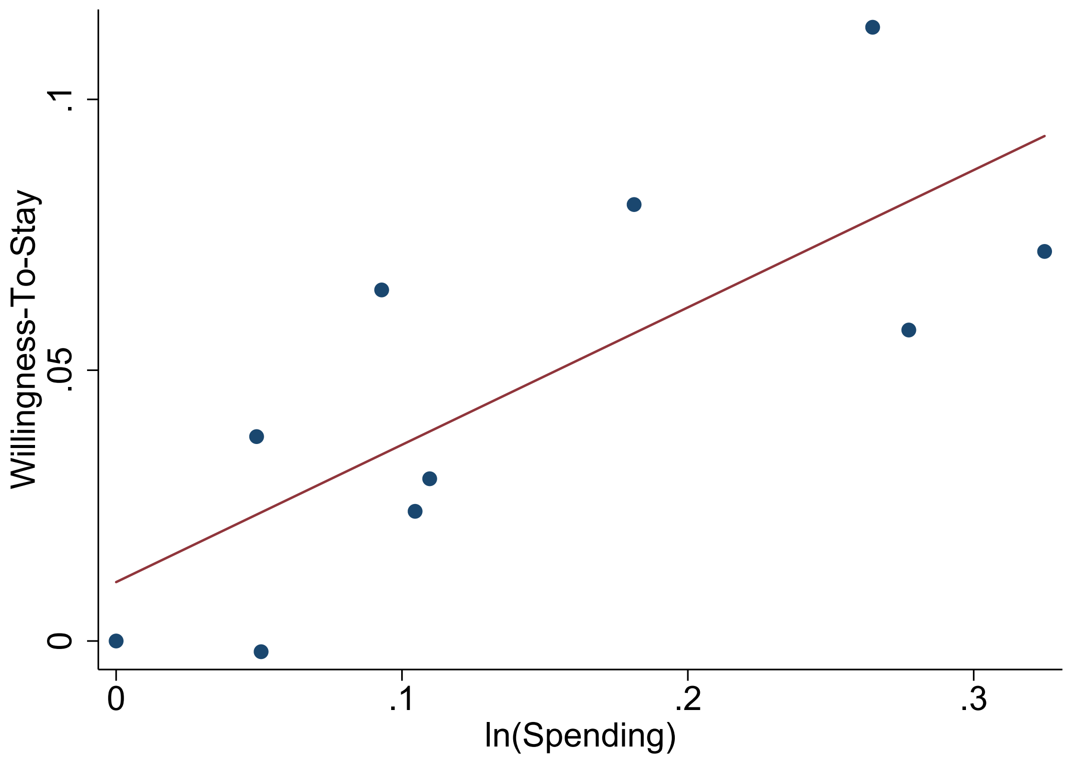

Figure 2.8: *Higher Spending Plans Improve Recipient Satisfaction*

Notes: The figure plots coefficients on instrumented plan indicators from 2SLS regressions in which the dependent variables are log spending and willingness-to-stay (a measure of recipient satisfaction) relative to the reference plan (Amerigroup). Regressions are run at the recipient $\times$ month level, and the sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,449 patient months).

2.6.3 Selection

We now turn to investigating the extent of selection present in this market. To do so, we use the methods laid out in Section 2.5.3. First, we focus on the population of recipients that were automatically assigned to plans. Since the assignment is not binding, we measure the extent of any selection driven by the switchers in this market. We then turn to the larger population of individuals actively choosing plans. To estimate selection we generate a residual that removes the plan effects (estimated via IV) from the individual-level spending variable. We then estimate an OLS regression of the residualized spending on plan dummies (see Eq 2.16), so that the coefficients on those dummies represent the average level of residualized spending for each plan.

Even among the auto assignees, we find that (production-purged) selection across plans is substantial. Table 2.7 presents our estimates of selection for the sample of "auto assignees". For the sample of recipients automatically assigned to their plans we find that the average cost of individuals choosing Health First is about 40% larger than the average cost of individuals choosing Amerigroup (the "left-out" plan). Adverse selection of this extent is surprising in this sample given the considerable inertia in the initial assignment.

For the recipients making active choices we also find substantial evidence of selection, although the patterns of selection differ from those among the auto assignees (Figure 2.9). We find that the average cost of individuals choosing United is about 60% larger than the average cost of individuals choosing Health First (Table 2.8). Adverse selection of this extent is likely to be important for the functioning (or failure) of this market. We note, however, that the differences in these coefficients represent average levels of selection rather than estimates of selection on the margin which may be more relevant for both policy and welfare (see Einav *et al.* (2010)). At the same time, in this setting where there are no plan prices, it is not even clear on which margin (e.g. network size, plan quality, plan spending levels, etc.) selection is relevant.

We then examine whether there is a connection between selection and production. In other words, do sicker recipients select into plans that allow (or encourage) recipients to access more services? Here, the pattern is also unclear. Figure 2.10 plots IV estimated plan effects on production (log spending) against selection. Panel (A) presents this relationship for the auto assignee sample, and a positive relationship is clear visually. In Panel (B), the same scatterplot is presented for recipients that have made active plan choices. Here, the slope is smaller and the relationship much noisier. For example, among the active choosers the plan that is the most adversely selected is one with a relatively small IV estimate for production. We explore the same question using levels and present the results in Appendix Figure B.7. Here the differences are even more stark. Again, there appears to be a positive correlation between production and selection on health status in the auto assignee sample.

Table 2.7: *Selection Estimates (Log $) for Auto Assignees*

	OLS (1)	OLS (2)
United	0.039	0.036
	(0.026)	(0.025)
Wellcare	0.020	0.018
	(0.045)	(0.041)
HIP	0.091***	0.089***
	(0.027)	(0.026)
Affinity	0.040	0.041
	(0.026)	(0.025)
Neighborhood	0.039	0.031
	(0.024)	(0.023)
Fidelis	0.165***	0.158***
	(0.025)	(0.024)
Health First	0.403***	0.367***
	(0.033)	(0.032)
Health Plus	0.061**	0.059**
	(0.025)	(0.024)
Metroplus	0.316***	0.290***
	(0.022)	(0.021)
Observations	900,449	900,449
Demographic Controls	No	Yes
Joint F-Test	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variable is a residual measuring selection as the difference between observed and predicted spending based on our two-stage least squares (2SLS) coefficients from estimating equation (2.13). The independent variables are plan effects relative to the reference plan (Amerigroup). Columns (1) and (2) present results with and without additional controls (all specifications control for the county x year x month of assignment). Baseline controls are just the county x year x month of assignment. Additional controls include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), and zip code fixed effects. All standard errors are clustered on the county x year x month of assignment (N=900,449 patient months). For per comparison significance for the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table 2.8: *Selection Estimates (Log $) for Active Choosers*

	OLS (1)	OLS (2)
United	0.325***	0.273***
	(0.038)	(0.035)
Wellcare	−0.143***	−0.122***
	(0.041)	(0.041)
HIP	0.136***	0.137***
	(0.032)	(0.032)
Affinity	0.028	0.023
	(0.031)	(0.031)
Neighborhood	0.174***	0.192***
	(0.036)	(0.034)
Fidelis	0.236***	0.226***
	(0.029)	(0.028)
Health First	0.119***	0.092***
	(0.026)	(0.026)
Health Plus	0.139***	0.115***
	(0.026)	(0.026)
Metroplus	0.121***	0.107***
	(0.027)	(0.027)
Observations	960,500	960,474
Demographic Controls	No	Yes
Joint F-Test	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variable is a selection residual using log dollars that measures the difference between observed spending and predicted spending based on the two-stage least squares (2SLS) from the estimating equation (2.13). The independent variables are plan effects relative to the reference plan (Amerigroup). Columns (1) and (2) present the results of estimating equation (2.16) with and without additional controls (all specifications control for the county x year x month of assignment) on residualized spending. Baseline controls are just the county x year x month of assignment. Additional controls include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), and zip code fixed effects. All standard errors are clustered on the county x year x month of assignment. The sample consists of recipients aged 18 to 65 making active choices about their Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=960,474 patient months). For per comparison significance for the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

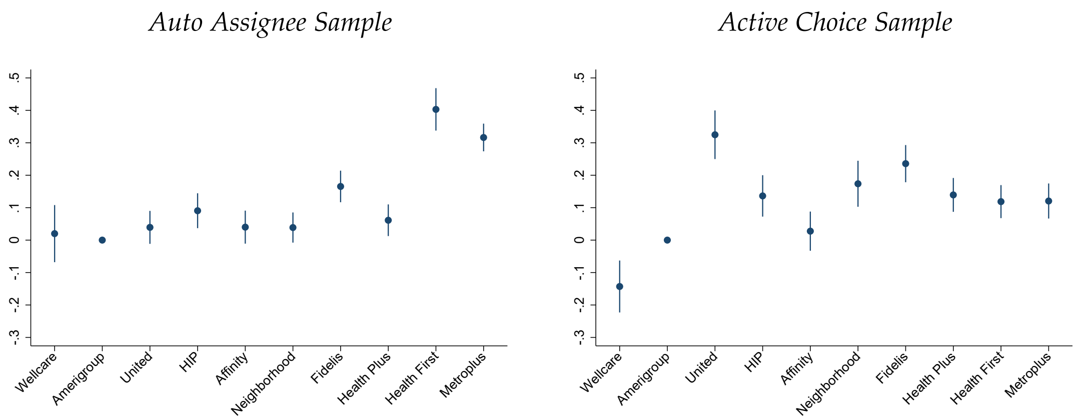

Figure 2.9: *Selection Differs for Auto Assignees and Active Choosers*

Notes: The figure plots coefficients on instrumented plan indicators from 2SLS regressions in which the dependent variable is a residual that measures (production-purged) selection based on log spending relative to the reference plan (Amerigroup). Panel (A) plots these coefficients for the auto assignee sample. Panel (B) plots these coefficients for the active choice sample. Regressions are run at the recipient $\times$ month level, and the sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,449 patient months).

However, in the active choice sample the slope is now negative (but again the relationship is noisy).

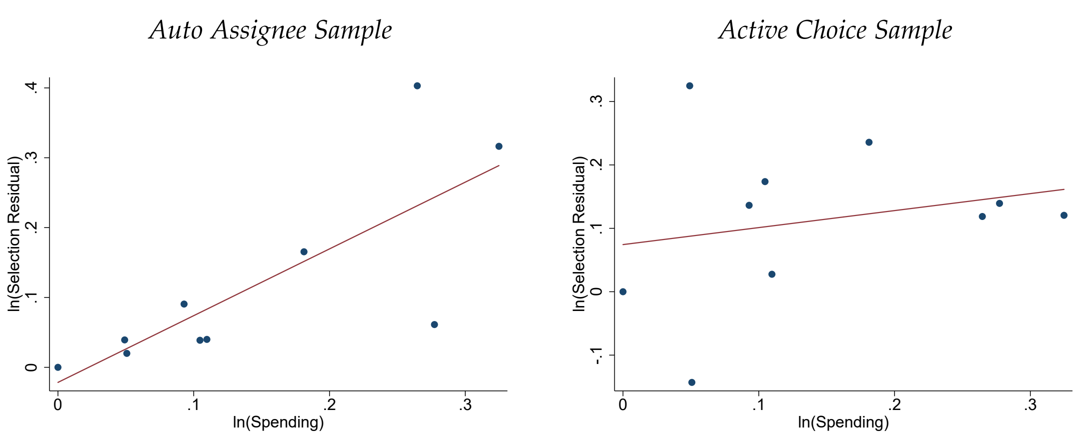

Figure 2.10: *Sicker Auto Assignee Recipients Select Into More Generous Plans*

Notes: The figure presents scatter plots of plan production (as measured by log spending) against a residual that measures (production-purged) selection. Panel (A) plots these coefficients for the auto assignee sample. Panel (B) plots these coefficients for the active choice sample. Regressions are run at the recipient $\times$ month level, and the sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded.

2.7 Discussion

In this paper, we examine how health care spending and quality vary for plans that are identical in their premiums, cost-sharing parameters, and "HMO" designation. Separating the effects of selection and production on a health plan's output is critical for policy. To do this, we use a research design that relies on random assignment of consumers to plans. We find significant variation in both production and selection across Medicaid managed care plans. Among randomly-assigned beneficiaries in our data, monthly healthcare costs vary by 33% (about $600 per member per year) between the lowest and highest spending plans. Importantly, this variation is not restricted to outliers as the other plans in the market are distributed evenly between the extremes. This dispersion is similar in magnitude to the size of the reduction in spending (30-40%) HMOs achieve relative to traditional insurance plans in treating heart disease (Cutler *et al.*, 2000). We also find significant differences in quality and recipient satisfaction, although these do not correlate perfectly with spending differences across plans suggesting some may be more efficient at producing quality.

We also use our unique setting to study the extent of selection in the market. Removing the plan productivity differences (estimated via IV), we find that the difference between the average cost of the individuals actively choosing the most adversely selected plan is about 60% greater than the average cost of the individuals choosing the least adversely selected plan, suggesting substantial selection across plans in the market. This selection would otherwise confound estimates of the effect of plan choice on spending. Similarly, without accounting for variation in productivity across plans, average costs alone would generate misleading conclusions about the extent of selection in the market. Both of these facts highlight the importance of our research design. Additionally, we find considerable selection even within the sample of recipients that are randomly assigned to their plans. Despite the considerable inertia associated with the original plan assignment, simple comparisons of average cost for this sample would also produce biased measures of how production varies across plans.

We also ask what drives productivity differences across managed care plans. First, we decompose productivity differences into price and quantity effects, finding that a significant portion of the spending differences across plans are driven by differences in utilization, rather than price differences. This is important, given that the MCO literature has—until now—largely examined only the upstream price advantages of MCOs relative to traditional plans. In the Medicaid market we find that the majority of the differences in spending across plans are attributable to differences in utilization, rather than unit prices. We also decompose spending differences across health care spendings, finding that over 70% of the difference in spending between low-spending plans and high-spending plans is due to differences in inpatient hospital spending. This suggests that a key way the most efficient MCOs generate lower spending is by limiting inpatient stays.

2.8 Conclusion

Using a randomized controlled design we examine how Medicaid managed care plans affect recipients in New York City. We exploit the random assignment to identify plan effects that are purged of selection, which we show would be an important confounder in this setting. Our findings reveal significant variation in both "production" and "selection" across ostensibly similar managed care plans competing in the same local market. Specifically, we find that plans differ in spending on identical beneficiaries by as much as 33%. These differences are even larger for enrollees with high baseline spending. We show that differences in negotiated upstream prices explain only a fraction of this difference, and that plan characteristics significantly affect the healthcare consumption of enrollees. Our findings are important for the continued reform of healthcare, in which managed care is often touted as the single most important tool for constraining healthcare spending growth.

Chapter 3

Traditional Medicare Versus Private Insurance: How Spending, Volume, And Price Change At Age Sixty-Five[1]

3.1 Introduction

As the US economy recovers and increasing numbers of Americans gain health insurance, health care spending growth is expected to accelerate (Sisko *et al.*, 2014; Cuckler *et al.*, 2013; Keehan *et al.*, 2012; Roehrig, 2014). Amid mounting pressure to control Medicare spending, some lawmakers have proposed raising the Medicare eligibility age (Capretta, 2012; MacGuineas and A.E., 2012). The Congressional Budget Office estimates that if the Medicare eligibility age changed from sixty-five to sixty-seven, most of those who would need to wait longer to receive Medicare coverage would stay in their private plans until age sixty-seven (Office, 2013a, 2014). Therefore, to evaluate this proposal it is crucial to understand how health care spending and use differ between Medicare and private insurance.

While most Americans transition to Medicare from private coverage, little is known about

[1]Co-authored with Zirui Song.

how health care use and spending change during this transition. Earlier work examining the transition to Medicare at age sixty-five has focused primarily on previously uninsured adults entering Medicare, a significantly smaller share of Medicare entrants compared to the previously insured, and on a limited set of outcomes including mortality (Card *et al.*, 2009), self-reported health (McWilliams *et al.*, 2007a), and utilization (Card *et al.*, 2008; McWilliams *et al.*, 2003, 2007c; Decker, 2005).

In contrast, we examined how health care spending, utilization, and price differ for adults with private insurance before and after they gain Medicare coverage at age sixty-five. We measured spending as the sum of insurer-paid amounts and enrollee cost sharing.

We focused on outpatient imaging and procedures, which are categories of services with fully reported data from Truven Health Analytics for adults before and after they turn sixty-five. The detailed claims information and longitudinal nature of the data allowed us to control flexibly for age (we used age in months), time, and individual characteristics, which would otherwise threaten to confound comparisons of traditional Medicare and private insurance.

We used a regression discontinuity design that adjusted for age trends and quarter, year, and individual fixed effects. With this design and a continuously enrolled national cohort of individuals, our study provides the first causal estimates of the difference in health care spending, volume, and price between traditional Medicare and private insurance.

Previous studies have shown that Medicare prices are lower than those of private insurers (Gottlieb and Clemens, 2014). However, little is known about how or whether providers respond toMedicare's lower prices. If providers respond by seeing fewer Medicare patients, we would expect to see a drop in health care use at age sixty-five, as Medicarebecomes the primary payer. Instead, our results indicate that Medicare beneficiaries do not face reduced access upon entry to the program, at least as measured by health care use—which is unchanged before and after age sixty-five. One possible explanation for this is providers' willingness to contract withMedicare despite its lower rates, which may be a result of Medicare's purchasing power as a large insurer (Chernew *et al.*, 2010).

3.2 Study Data and Methods

3.2.1 Population

We acquired data on 1,387,534 individuals who transitioned from commercial insurance to Medicare at age sixty-five in Truven Health Analytics' 2007–13 Medicare and Commercial Claims and Encounters database, a large nationwide convenience sample of employer-sponsored health insurance enrollees at large employers. These data contain fully integrated patient-level claims and enrollment information, including benefit design and cost sharing (Adamson *et al.*, 2008).

We excluded individuals who did not have at least one year of continuous enrollment before and after entering Medicare at age sixty-five and individuals with Social Security Disability Insurance (SSDI) or end-stage renal disease, who are eligible for Medicare before age sixty-five.

We restricted the sample to retired individuals, since active workers often retain employer-sponsored insurance as their primary coverage after age sixty-five. We limited our sample to individuals whose commercial plan before age sixty-five was a broad network plan (indemnity, preferred provider organization, or point-of-service plan) and who entered traditional Medicare instead of MedicareAdvantage. We excluded Medicare Advantage because of concerns that capitated claims are not always reported to the former employer, and hence we might not observe all of those claims. Finally, we excluded individuals with any capitated claims or claims for which the final paid amount was negative.[2]

Our final sample consisted of 200,870 continuously enrolled individuals. Importantly, each individual's claims and enrollment information could be tracked from when he or she

[2]These sample restrictions had the following effect on our study population. From the original 1,387,534 individuals, we removed 149,335 who had capitated claims or claims with negative paid amounts, 858 with missing plan information, 5,368 with end-stage renal disease, 52,935 receiving SSDI, 121,421 for whom the only data available were during months in which they switched plans and for whom we did not observe all spending and utilization, 852,765 who were not coded as retired and who may have remained on private insurance past age sixty-five, and 17,739 who were not in broad network plans. The resulting sample size was 200,780 because these populations were not mutually exclusive, so the sum of each item does not equal 200,780. In analyses available upon request, we examined how our overall spending result would have changed if we had not restricted our sample to individuals coded as retired and those in broad network plans. For this alternative sample, we found that overall spending fell by 20.2 percent after entry into Medicare at age sixty-five.

had private insurance to when he or she was covered by Medicare—a unique advantage of using these longitudinal data that was lacking in the previous literature.

3.2.2 Data and Variables

Our primary outcomes were spending, volume, and price at the person-quarter level. For our spending analyses, the dependent variable was claims payments, which reflect negotiated prices among commercial enrollees, administratively set prices for Medicare beneficiaries, and patient cost sharing. Cost sharing for a given service was the sum of its copayment, coinsurance, and deductible. For utilization analyses, we constructed counts of services. Services with both professional and facility claims on the same day were counted once. For prices, the actual paid amounts reflect negotiated rates between private insurers and providers. We calculated a price index by dividing spending by utilization, using mean prices at the person-quarter level.

We decomposed health care spending by type of service, and we focused on services for which we had complete payment data from both the payer and patient. Since Medicare supplemental coverage applies only to Medicare-covered services that contain cost sharing, we restricted our analyses to the following two categories of services that consistently have cost sharing in traditional Medicare: imaging and procedures.

In our main analyses we examined total health care spending, which included the actual amount paid by payers that reflected payer-provider negotiated prices (or administered prices in the case of traditional Medicare) and enrollee cost sharing. We also analyzed the use of and average prices for these services.

In subgroup analyses we analyzed a number of sentinel services with large sample sizes, including x-rays, computed tomography (CT) scans, and magnetic resonance imaging (MRI); and ophthalmologic, dermatologic, respiratory, and other procedural services.

Most laboratory tests and many office visits, such as preventive visits and "Welcome to Medicare" visits, do not require cost sharing from Medicarebeneficiaries. Therefore, they are not consistently reported to employers that offer supplemental coverage and do not consistently appear in the data. Similarly, deductibles for inpatient care are set on an episode basis and extend to sixty days after the end of the most recent inpatient or skilled nursing facility stay. As a result, if a readmission occurs within this benefit period, there is no additional cost sharing, and the readmission may not appear in the Truven claims data. Thus, we focused on services that were reliably reported each time a claim was generated.

Lastly, because of data confidentiality protections, patient age was reported only at the year and month level. Since our claims included exact dates of service, we could not reliably assign claims to a given month for individuals who switched coverage. To address this, we excluded any month immediately before a plan switch from the data, as was done in a previous study on the transition into Medicare (Card *et al.*, 2009).

3.2.3 Statistical Analysis

We used a regression discontinuity design to identify the causal effect of entry into Medicare on total health care spending, volume, and price for previously insured adults. Regression discontinuity—an increasingly common method in empirical research—makes it possible to identify effects caused by a policy change (Imbens and Lemieux, 2008; Thistlethwaite and Campbell, 1960; Linden and Adams, 2012). We compared changes in spending, volume, and price from shortly before to shortly after the Medicare eligibility threshold at age sixty-five. As was done in previous studies, we modeled age trends as continuous polynomials and attributed discontinuities in outcomes at age sixty-five to the effect of entry into Medicare.

Base regression models were estimated using ordinary least squares with second-order polynomials (quadratic form) that allowed for different age trends before and after age sixty-five (Lee and Lemieux, 2009). Our independent variable of interest was an indicator for age sixty-five, which captured sharp changes in the outcomes of interest at the Medicare eligibility threshold. Because our treatment-determining variable was discrete (age), the treatment effect of Medicare was not identified without assuming a parametric functional form (Lee and Card, 2008). We adjusted for individual fixed effects, age, and quarter and year fixed effects. Standard errors were clustered by individual and by age in quarters and reported with two-tailed p values. More details about our model specification are provided in the Appendix.

In sensitivity analyses we altered various aspects of the model, including functional form, covariates, and bandwidth on either side of the discontinuity at age sixty-five. Our base model used a quadratic functional form because lower-order polynomial functions have been shown to be preferable (Gelman and Imbens, 2014). All analyses used Stata, version 13.

3.2.4 Limitations

Our study had several limitations. First, our regression discontinuity analyses relied on the assumption that the dependent variables of interest would evolve smoothly with age in the absence of the transition to Medicare at age sixty-five (Angrist and Pischke, 2009). Although this assumption could not be fully tested, we partially tested it by examining spending trends for individuals with SSDI. These beneficiaries enroll in Medicare early and thus experience no change in coverage at age sixty-five. If outcomes were discontinuous for these individuals at age sixty-five, that would suggest that other factors in addition to the switch to Medicare might have biased our results. Reassuringly, spending, volume, and price trended smoothly across age sixty-five for adults in the sample with SSDI (Appendix Figure C.1).

Second, data limitations precluded the assessment of all types of services. Since all of the people in our sample of Medicare beneficiaries received some form of supplemental retiree coverage, we did not consistently observe claims for services covered by Medicare with no cost sharing. To address this limitation conservatively, we restricted our analysis to outpatient imaging and surgery services subject to Medicare Part B cost sharing. Nevertheless, Medicare coverage may have different implications for those services that we did not study, including laboratory tests, office visits, and inpatient care. We were also unable to analyze the quality of care. Although we found little change in utilization, we could not observe whether lower Medicare prices drove beneficiaries to receive care from lower-quality providers or to receive lower-quality care.

Third, our study sample was not representative of all adults who enter Medicare at age sixty-five. Because of data limitations, we observed only individuals who had private insurance before entering Medicare. Although this represents the bulk of adults entering Medicare, Medicare beneficiaries may have previously been covered by Medicaid, enrolled in a plan listed on a health insurance exchange (or Marketplace), or uninsured. Our results thus are not generalizable to these populations. Nonetheless, they complement results of earlier work that examined how outcomes change at age sixty-five for the previously uninsured (Card *et al.*, 2009; McWilliams *et al.*, 2009, 2007b,c). In addition, our study did not include Medicare Advantage plans, which now cover about 30 percent of Medicare beneficiaries.

Instead, we focused on the transition from broad network plans (indemnity plans and preferred provider organizations) to traditional Medicare, in an effort to compare insurance with similar benefit designs before and after age sixty-five. Because Medicare Advantage plans use different benefits, provider networks, and managed care strategies than traditional Medicare does, changes in health care spending and use at age sixty-five for enrollees in Medicare Advantage may differ from our findings (Landon *et al.*, 2012; Stevenson *et al.*, 2013; Ayanian *et al.*, 2013). Relatedly, we did not include new beneficiaries who were previously covered by private plans on exchanges, since data on those individuals are not yet broadly available.

3.3 Study Results

3.3.1 Population

The 200,870 unique enrollees were ages 62–68 during the study period and were predominantly from the North Central and South regions of the United States; 56.7 percent of the enrollees were male. About 80 percent entered Medicare from a fee-for-service plan or a preferred provider organization; the remainder entered from a point-of-service plan. In total, the sample contained 3,911,685 unique enrollee quarters within a twelve-quarter (three-year) bandwidth of the Medicare eligibility age.

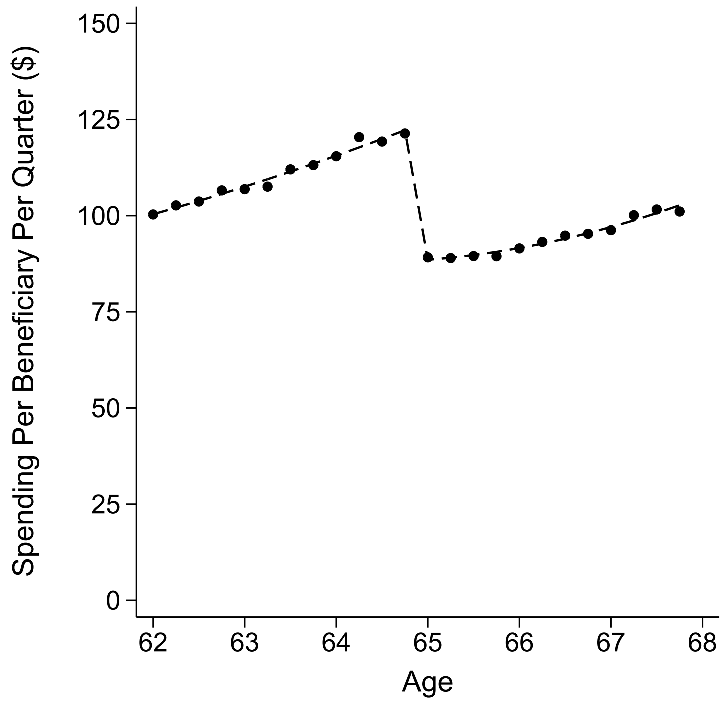

Figure 3.1: *Unadjusted spending before and after the transition from private insurance to Medicare at age 65*

Notes: Authors' analysis of data for 2007–13 from Truven Health Analytics' Medicare and Commercial Claims and Encounters database. NOTES Average spending on analyzed services (in 2013 US dollars) per beneficiary per quarter. The line was generated from regressions of spending on age and age squared, with a binary variable for ages sixty-five and older. The model allowed for different age coefficients in the private insurance and Medicare samples.

3.3.2 Spending

Unadjusted spending per person per quarter for the services we analyzed fell sharply after entry into Medicare, while spending trends were similar before and after age sixty-five (Figure 3.1). After spending an average of $119.12 per quarter in their last year with private insurance, new Medicare beneficiaries spent $89.28 per quarter, on average, in their first year in Medicare. In an adjusted analysis, average spending was $38.56 (or 32.4 percent) lower after transitioning into Medicare, compared to before the transition (Table 3.1).

Table 3.1: *Adjusted spending before and after transition from private insurance to Medicare at age 65*

	Age 64	Age 65	Adjusted difference		
			Amount	Percent change	p value
Spending ($)					
Total spending	119.12	89.28	−38.56	−32.4	< 0.001
Imaging	84.06	57.15	−33.75	−40.2	< 0.001
X-ray	15.86	11.43	−5.58	−35.2	< 0.001
CT and MRI scans	57.03	38.42	−24.20	−42.4	< 0.001
Electrocardiogram	11.18	7.30	−3.98	−35.6	< 0.001
Procedures	35.05	32.13	−4.82	−13.8	< 0.001
Cataract removal	15.15	15.31	−0.26	−1.7	0.70
Opthtalmologic	7.75	6.15	−2.28	−29.4	< 0.001
Dermatologic	6.29	5.88	−0.87	−13.8	< 0.001
Respiratory	2.49	1.99	−0.81	−32.5	< 0.001
Arthrocentesis	3.37	2.80	−0.60	−17.8	< 0.001
Utilization (Per 1,000)					
Total volume	608.28	631.59	−3.24	−0.5	0.52
Imaging	476.62	491.03	−2.94	−0.6	0.54
X-ray	215.34	220.26	−3.33	−1.5	0.28
CT and MRI scans	89.24	93.87	2.01	2.3	0.24
Electrocardiogram	172.04	176.90	−1.62	−0.9	0.48
Procedures	131.66	140.56	−0.29	−0.2	0.85
Cataract removal	8.97	12.02	2.58	28.8	< 0.001
Opthtalmologic	10.25	11.48	0.43	4.2	0.30
Dermatologic	72.95	75.02	−3.77	−5.2	0.007
Respiratory	8.58	9.24	0.00	0.0	0.99
Arthrocentesis	30.91	32.79	0.46	1.5	0.43
Average Price ($)					
Total volume	193.69	146.42	−56.48	−29.2	< 0.001
Imaging	167.30	115.41	−63.20	−37.8	< 0.001
X-ray	72.68	52.50	−23.55	−32.4	< 0.001
CT and MRI scans	660.44	429.64	−278.01	−42.1	< 0.001
Electrocardiogram	55.65	35.88	−17.32	−31.1	< 0.001
Procedures	279.34	239.56	−37.07	−13.3	< 0.001
Cataract removal	1,676.24	1,239.85	−183.72	−11.0	0.22
Opthtalmologic	721.51	539.64	−202.25	−28.0	< 0.001
Dermatologic	93.31	84.51	−8.14	−8.7	< 0.001
Respiratory	251.51	194.04	−72.34	−28.8	< 0.001
Arthrocentesis	108.51	83.88	−20.52	−18.9	< 0.001

Notes: Authors' analysis of data for 2007–13 from Truven Health Analytics' Medicare and Commercial Claims and Encounters database. Our sample contained data on 200,870 people when they were ages sixty-four and sixty-five. All spending and prices are in 2013 US dollars. Adjusted differences are based on a regression discontinuity model that controlled for age, quarter, year, and individual fixed effects. A detailed description of the regression specification is available in the Appendix. Categories of services were designated according to the Medicare procedure groups, a procedure commonly used by commercial insurers. CT is computed tomography. MRI is magnetic resonance imaging.

Decomposition of spending by service category highlighted this result. Unadjusted spending for imaging decreased from $84.06 per beneficiary per quarter at age sixty-four to $57.15 at age sixty-five, with adjusted savings of $33.75 (40.2 percent) attributable to entry into Medicare. Subgroup analyses demonstrated savings of $5.58 for x-rays, $24.20 for CT scans and MRIs, and $3.98 for electrocardiograms (Exhibit 2). Analogously, spending for procedures decreased from $35.05 to $32.13, with adjusted savings of $4.82 attributable to Medicare. This was reflected in subgroup analyses as well, with most procedural subcategories showing a significant reduction in spending after entry into Medicare.

The results from our sensitivity analyses were consistent with our general findings. Of note, results were very similar across alterations in the model's bandwidth, functional form, and control variables, including the removal of individual, year, or quarter fixed effects. In addition, Medicare beneficiaries who were SSDI recipients and therefore experienced no change in coverage at age sixty-five exhibited no change in spending at that age (Appendix Figure C.1). This strengthens our inference that the sharp decrease in spending for the main sample of individuals without SSDI at age sixty-five is attributable to entry into Medicare.

3.3.3 Volume

Unadjusted total volume of services showed no discontinuous changes at age sixty-five (Exhibit 3). This was consistent with adjusted analyses, which found a reduction in quarterly volume of 3.24 services per 1,000 beneficiaries per quarter after the transition into Medicare (Figure 3.2. Similarly, the changes in use of imaging and procedures were not significant, which suggests that changes in spending were driven primarily by lower prices in Medicare. These findings were broadly consistent with the results of our sensitivity analyses.

3.3.4 Price

The average price across all services decreased sharply after entry into Medicare, from $193.69 to $146.42 (Table 3.1 and Figure 3.3). In adjusted analyses, the average price decrease attributable to Medicare was $56.48, or 29.2 percent (Table 3.1).

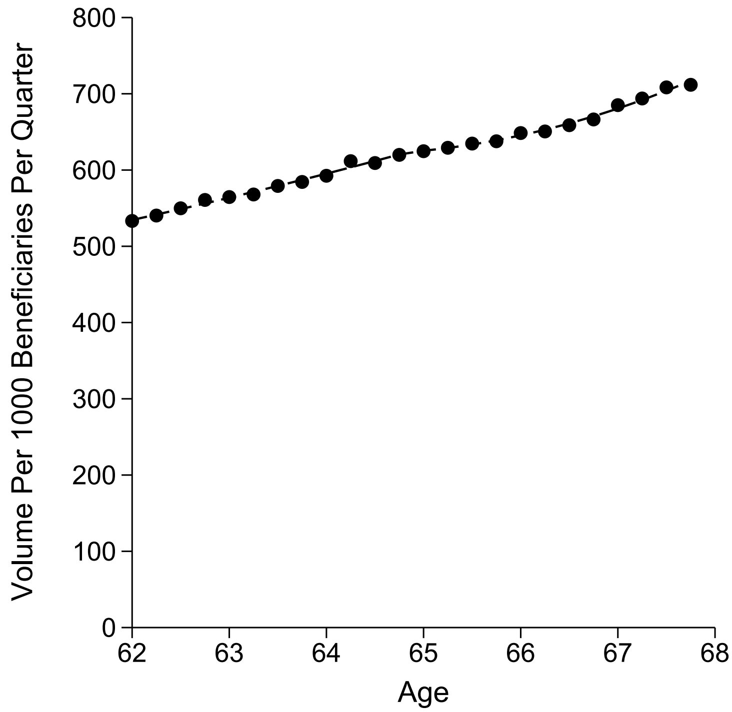

Figure 3.2: *Unadjusted volume before and after the transition from private insurance to Medicare at age 65*

Notes: Authors' analysis of data for 2007–13 from Truven Health Analytics' Medicare and Commercial Claims and Encounters database. NOTES Average volume per 1,000 beneficiaries per quarter on analyzed services. The line was generated from regressions of volume on age and age squared, with a binary variable for ages sixty-five and older. The model allowed for different age coefficients in the private insurance and Medicare samples.

The price decline was greater for imaging (37.8 percent) than for procedures (13.3 percent). In subgroup analyses, imaging price reductions ranged from 31.1 percent for electrocardiograms to 42.1 percent for CT scans and MRIs. Price reductions for procedures ranged from 8.7 percent for dermatologic procedures to 28.8 percent for respiratory procedures. Results from sensitivity analyses were consistent with these findings. Of note, prices for cataract removal behaved differently from prices for other services: While cataract prices were lower after age sixty-five, the decline was not significant.

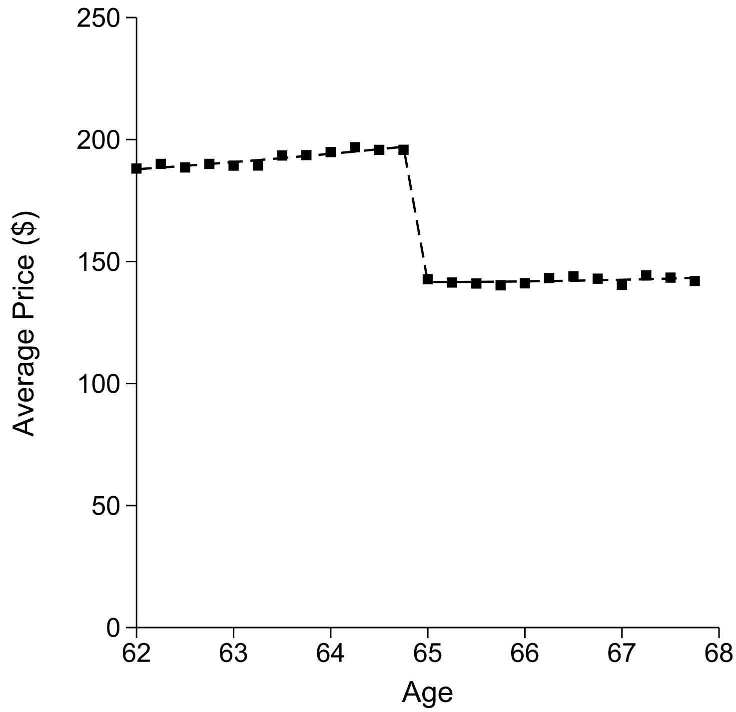

Figure 3.3: *Unadjusted price before and after the transition from private insurance to Medicare at age 65*

Notes: Authors' analysis of data for 2007–13 from Truven Health Analytics' Medicare and Commercial Claims and Encounters database. NOTES Average price for analyzed services. The line was generated from regressions of price on age and age squared, with a binary variable for ages sixty-five and older. The model allowed for different age coefficients in the private insurance and Medicare samples.

3.4 Discussion

Entry into Medicare decreased health care spending by 32 percent for new beneficiaries previously covered by private insurance. Lower Medicare prices, rather than changes in health care use, drove the decrease in spending. While previous work has shown that providers' prices are lower in Medicare than in private insurance (Gottlieb and Clemens, 2014), we found no evidence that these prices reduced health care utilization. One possible explanation for providers' willingness to see Medicare patients at lower rates is Medicare's purchasing power as a large insurer.

To our knowledge, this study is the first attempt to measure differences in health care spending and use between private insurance and traditional Medicare using claims data from a national sample of continuously enrolled adults. Instead of relying on ecological methods, cross-sectional data, or simulations, we used unique longitudinal data and a regression discontinuity design to provide causal estimates of changes in health care spending, volume, and price at age sixty-five (Hadley and Waidmann, 2006; Joyce *et al.*, 2005). Our study leveraged data that followed individuals as they aged into Medicare at sixty-five. This permitted us to control for a key confounder in studies that compare outcomes for individuals with Medicare to those with private insurance: namely, unobserved differences in the characteristics of the groups being compared.

3.4.1 Changes Attributable to Medicare Entry At Age 65

Our findings add to the evidence on changes in health care use and outcomes attributable to entry into Medicare at age sixty-five. First, we analyzed a national cohort of previously insured adults who switched from private insurance to traditional Medicare as their primary source of coverage at age sixty-five—a path followed by the majority of working US adults. Earlier studies have focused on previously uninsured individuals entering Medicare, who differ from continuously insured adults on measures of race, education, income, geography, health status, and health behaviors (McWilliams *et al.*, 2007b).

Second, earlier studies that included previously insured beneficiaries used them as controls instead of focusing on their outcomes, and they analyzed measures of health care utilization constructed from household surveys or hospital discharge data (Card *et al.*, 2009; McWilliams *et al.*, 2009, 2007b,c). In contrast, we used detailed claims data integrated with enrollment information. Finally, our estimates of how spending and volume change for imaging and surgery at age sixty-five complement findings from earlier studies, which report no decreases (and some increases) in the volume of office visits and certain tests upon entry to Medicare (McWilliams *et al.*, 2007a; Card *et al.*, 2008).

3.4.2 Increasing the Medicare Eligibility Age

Our results also help forecast how proposals to increase the Medicare eligibility age would affect health care spending. In this discussion we consider national health care spending and government health care spending separately.

National Health Spending

National health care spending is the sum of health care spending for all consumers and payers (including government payers). The Congressional Budget Office projects that the majority of people who would lose Medicare coverage if the eligibility age were increased would continue to be covered by private insurance (Office, 2013a, 2014). For these individuals, our study suggests that between ages sixty-five and sixty-seven, national health care spending would increase by roughly 30 percent for the services we examined. This increase would be because the use of outpatient imaging and surgery would be unlikely to change for adults who retained private insurance, but health care providers would be reimbursed by private insurers at negotiated prices that are higher than what Medicare would pay.

Our findings are consistent with an analysis by the Henry J. Kaiser Family Foundation arguing that increasing the Medicare eligibility age would raise overall health care spending (Neuman, 2011). For example, the Kaiser Family Foundation estimated that $5.7 billion in net savings to the federal government in the first year would be offset by a net increase of $3.7 billion in out-of-pocket spending for people ages sixty-five and sixty-six and an increase of $4.5 billion in employers' medical costs for retirees. According to the foundation's estimates, health care spending would be 20 percent higher for individuals who had private employer-sponsored coverage instead of Medicare—a spending difference that is a third lower than what we found.

Government Spending

Of particular interest to policy makers is how an increase in the Medicareeligibility age would affect government spending. To account for the full effect of such a change on government spending, one must consider several additional factors. These include increased federal subsidies to individuals with private insurance from the exchanges, increased spending on beneficiaries eligible for Medicaid alone or with Medicare, and changes in premiums for the remaining Medicare beneficiaries (Neuman, 2011).

The Congressional Budget Office estimates that if the Medicare eligibility age were raised to sixty-seven, Medicare spending would be $17.1 billion lower than projected in 2023 but that offsetting subsidy spending, Medicaid spending, and revenue losses would sum to $10.4 billion, resulting in a net decrease in the deficit of $6.7 billion (Office, 2013b). By 2038 Medicare spending would be 3 percent lower than projected, decreasing its share of the gross domestic product from 4.9 to 4.7 percent, but two-thirds of the savings would be offset by the above factors.

Finally, our regression discontinuity estimates were based on changes in health care use and spending that happen right around age sixty-five. Some proposals, as we have noted, would raise the Medicare eligibility age to sixty-seven. Such a large increase could trigger broader changes in health care markets that would limit the generalizability of our findings. For example, if more individuals retained private insurance after age sixty-five, this might have the effect of increasing insurers' market power and altering providers' negotiated prices. In addition or alternatively, private insurers and self-insured employers might adjust their benefits to account for their risk pools' getting older. Lastly, the expansion of private insurance might reduce the pressure for health care providers to cross-subsidize—the practice of raising rates charged to private insurers to cover losses from government payers.

3.5 Conclusion

Our finding that Medicare's lower prices reduce health care spending but not utilization as people transition from private insurance to Medicare has important policy implications. First, it provides evidence on how spending and access differ between Medicare and private insurance, thereby helping policy makers forecast the impact of proposed changes in the Medicare eligibility age on health care spending and beneficiary access.

Second, and more broadly, it provides new insight into the effect of Medicare's market power on health care prices and access. If this finding is generalizable to other health care services, populations, and payers, it suggests that insurers with market power can pay lower rates than insurers that lack market power without losing access to providers.

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Appendix A

Appendix to Chapter 1

A.1 "Covered Share" Network Measure

In addition to the network utility measures, I extend methods from Ericson and Starc (2015) to construct a measure of network size at the plan by year by zip code level. The measure is based on the fraction of visits (eg. admissions or office visits) covered by each managed care network. Ideally, I would use a distribution of visits from Medicaid managed care recipients facing no network restrictions. Unfortunately, this is not possible given the available data. I can use the empirical distribution of visits for the Medicaid fee-for-service population but this approach has two limitations. First, fee-for-service recipients are less healthy and more disabled than the managed care population. Second, because of low Medicaid fees the fee-for-service population does not face unrestricted choice.[1] Instead, I use the distribution of visits for the managed care population. For example, for general acute care hospitals this would be calculated as:

$$\mathrm{ND}^{Hosp}_{j,z,t} = \frac{\sum_h Q_{z,h} \cdot \{h \in N_{j,t}\}}{\sum_h Q_{z,h}} \tag{A.1}$$

where $1\{h \notin N_{j,t}\}$ is an indicator that hospital h is out-of-network for plan j in time t and Q_{zh} is the number of admissions to hospital h for Medicaid recipients that reside in zip

[1] Fee-for-service Medicaid payment rates are often lower than Medicaid MCO payment rates. As a result, the fee-for-service Medicaid population often faces more restrictive provider networks.

z. I construct a similar measure for physician networks using the fraction of office visits covered by each managed care network. I pool claims across the sample period (April 2008 to December 2012) to construct these measures. This measure will vary within plan across zip codes based on systematic differences in where recipients in different zip codes receive hospital care and which hospitals are in network.

A.2 Supplementary Tables and Figures

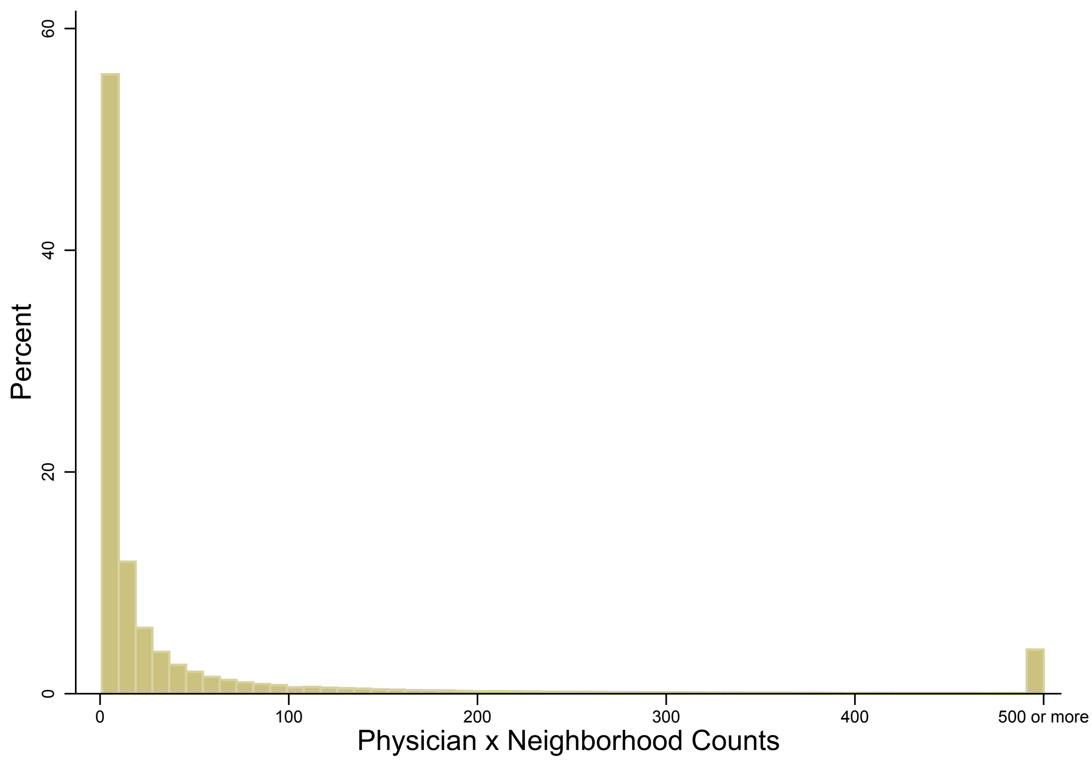

Figure A.1: *Skewed Distribution of Visits By Physician*

Notes: This figure plots the distribution of visits at the physician by neighborhood level. Each observation represents the number of Medicaid managed care visits to each physician license number for recipients living in each of 42 neighborhoods in New York City. The sample consists of all Medicaid recipients aged 18 to 65 enrolled from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded.

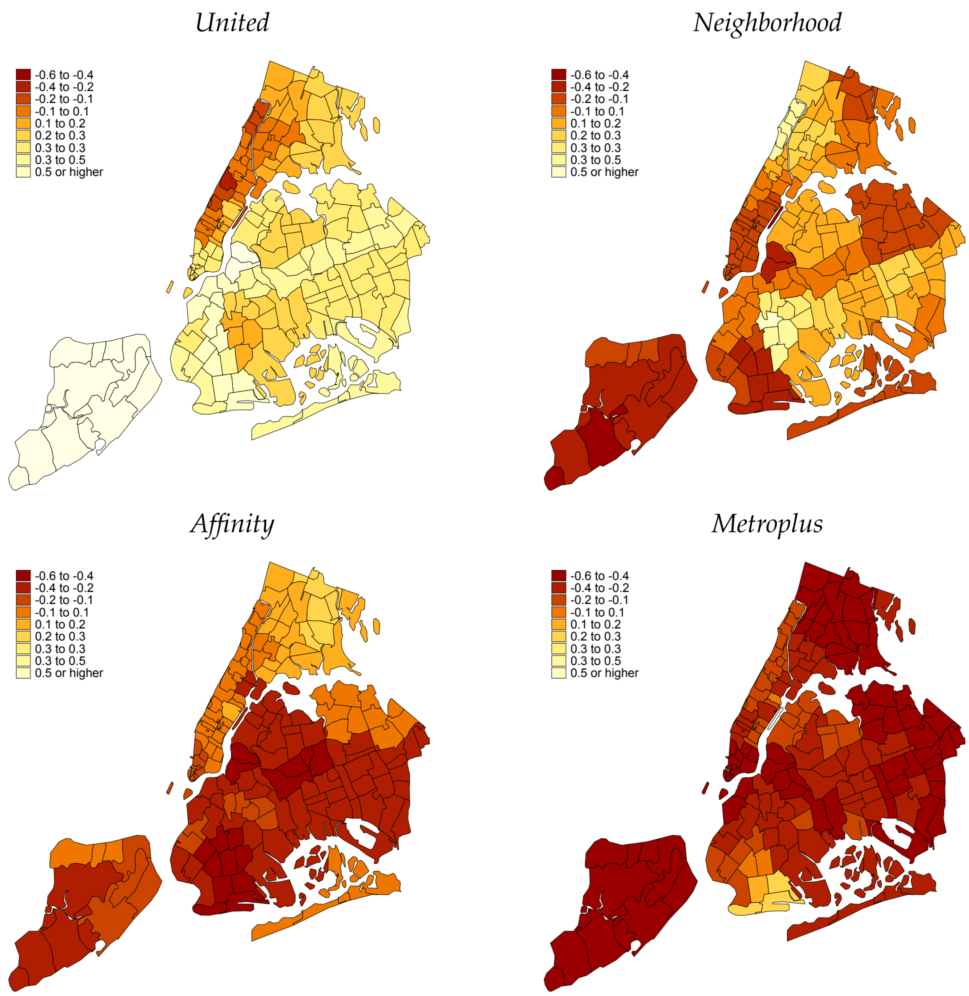

Figure A.2: *Variation in Physician Networks in 2012*

Notes: This figure plots physician network breadth for a subset of Medicaid MCOs in New York City in 2012. To construct these maps, I residualize the physician network breadth measure on zip code fixed effects before plotting. Intuitively, this exercise identifies plans that are narrow or broad in each zip code relative to the other MCOs.

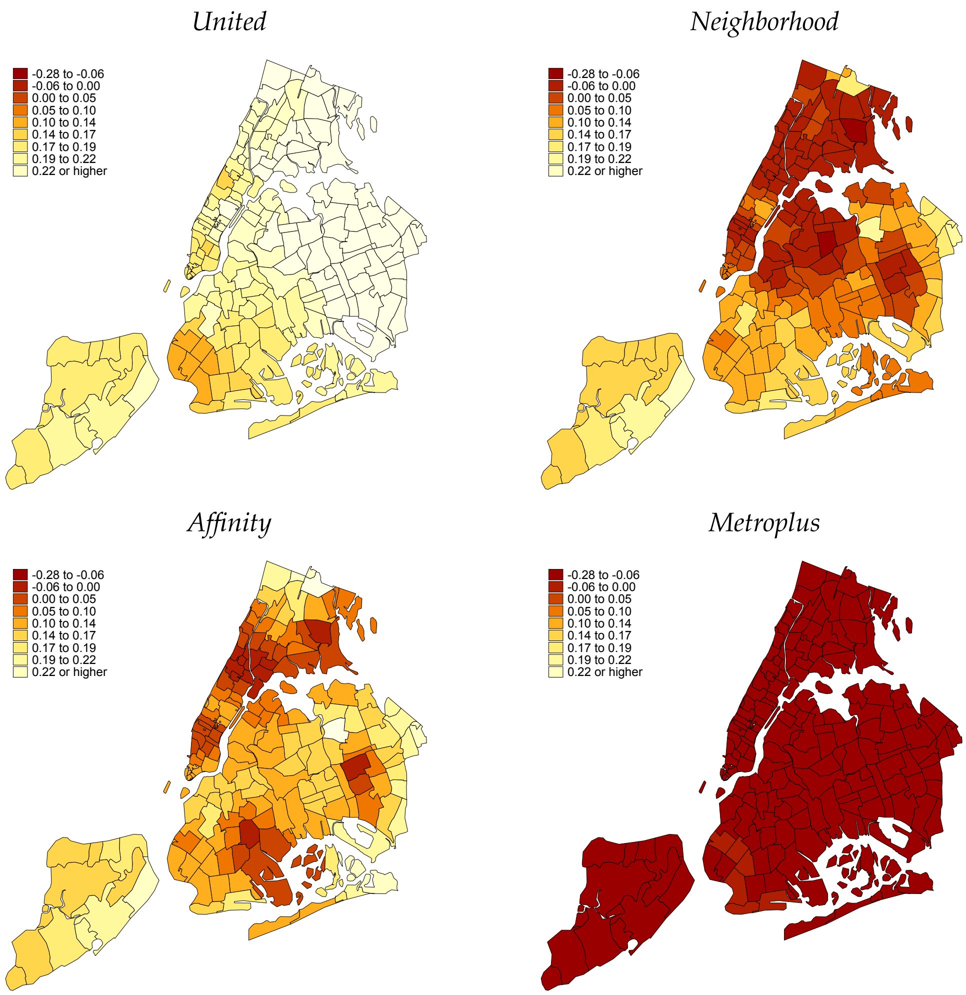

Figure A.3: *Variation in Hospital Networks in 2012*

Notes: This figure plots hospital network breadth for a subset of Medicaid MCOs in New York City in 2012. To construct these maps, I residualize the hospital network breadth measure on zip code fixed effects before plotting. Intuitively, this exercise identifies plans that are narrow or broad in each zip code relative to the other MCOs.

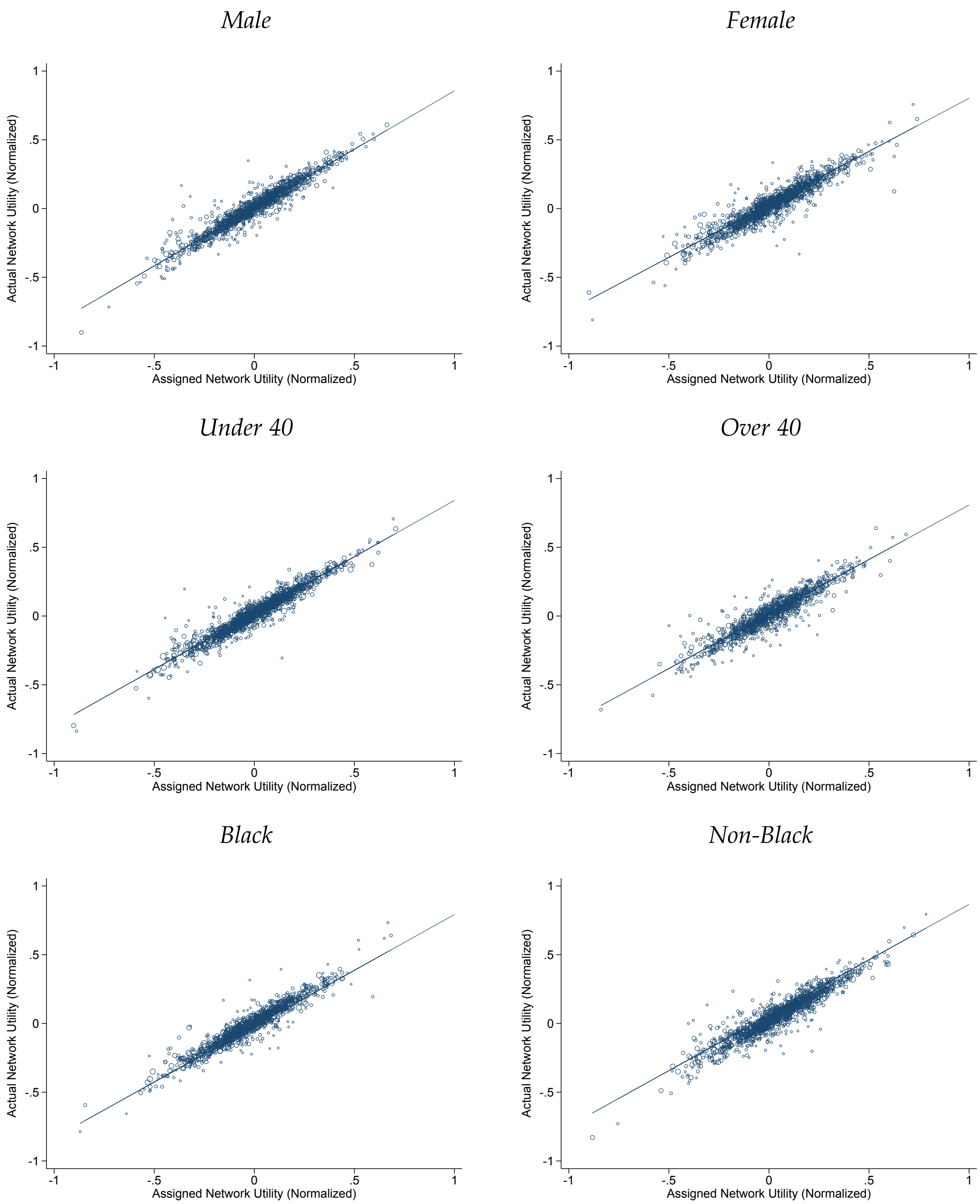

Figure A.4: *Variation by Subgroup in Assigned and Actual Physician Network Utility*

Notes: These figures plot recipients' physician network size against the assigned physician network size for different subgroups. The full sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (*N*=900,759 patient months). To construct the binned scatter plot, I separately regress assigned and actual network size against our base controls: recipient zip, episode quarter, five-year age x gender bins, race bins, plan assignment, and county x year x month of assignment. I then take the mean residuals in each plan by zip bin, adding the mean network size (actual or assigned) back in to facilitate interpretation of the results. The solid line is the best linear fit estimated on the micro data using OLS.

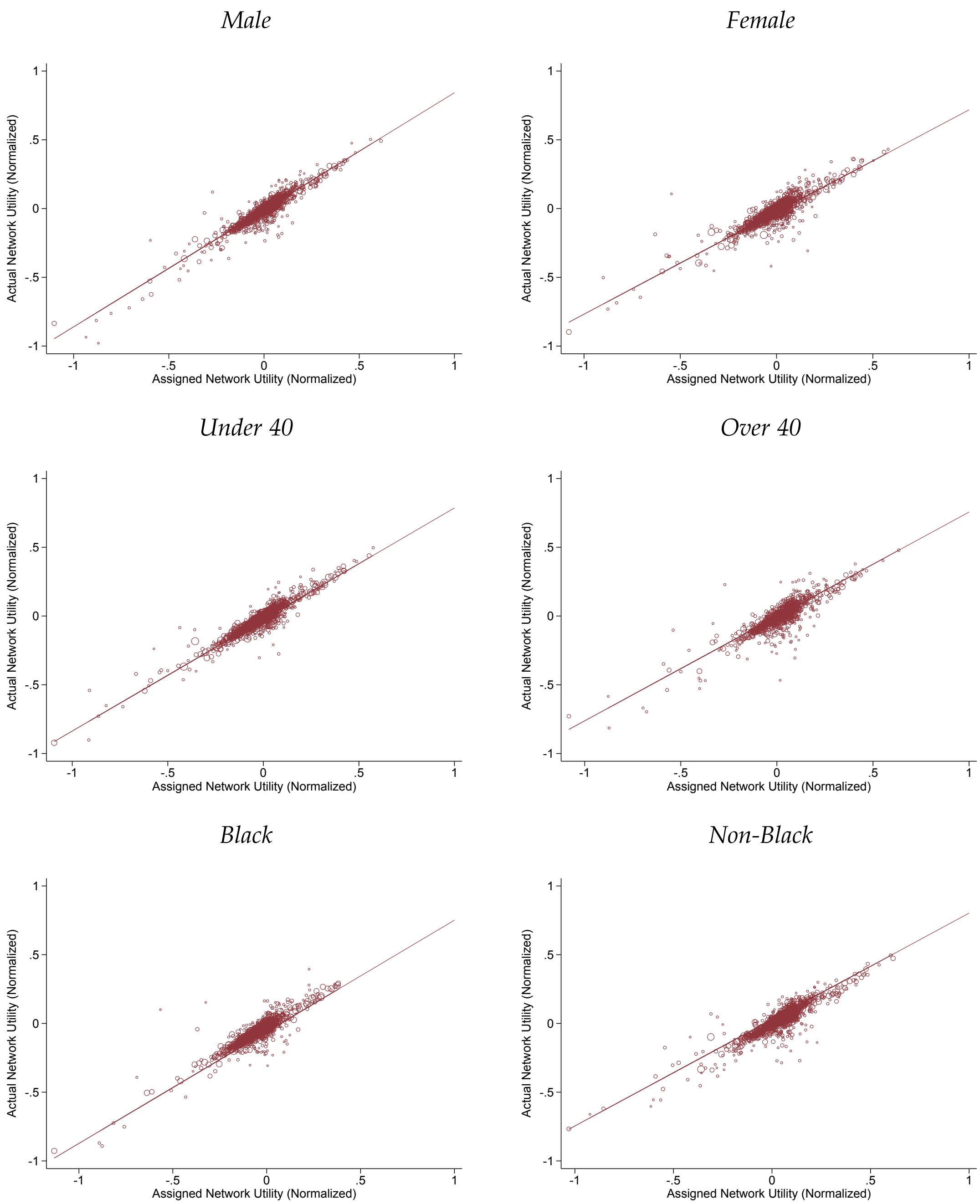

Figure A.5: *Variation by Subgroup in Assigned and Actual Hospital Network Utility*

Notes: These figures plot recipients' hospital network size against the assigned hospital network size for different subgroups. The full sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (*N*=900,759 patient months). To construct the binned scatter plot, I separately regress assigned and actual network size against our base controls: recipient zip, episode quarter, five-year age x gender bins, race bins, plan assignment, and county x year x month of assignment. I then take the mean residuals in each plan by zip bin, adding the mean network size (actual or assigned) back in to facilitate interpretation of the results. The solid line is the best linear fit estimated on the micro data using OLS.

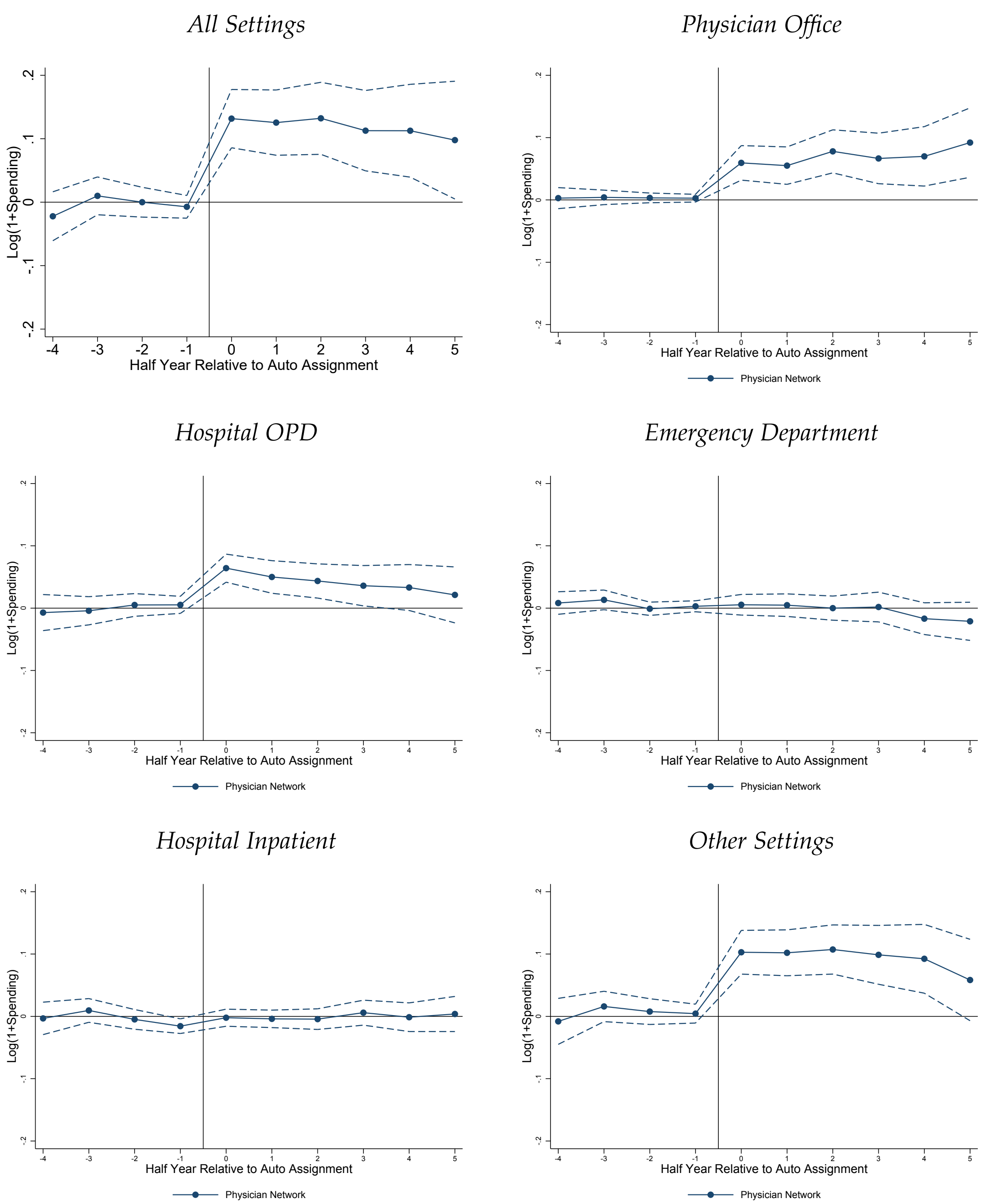

Figure A.6: *Reduced Form Estimates of the Impact of Physician Networks on Spending*

Notes: These figures plot reduced from estimates of the impact of physician network size on spending by site of service. I instrument for physician network size using the size of the network of the assigned plan (measured at the zip x plan x year level). The regressions control for a full set of plan assignment dummies, zip code dummies, indicator variables for each half year of an episode, five-year age x gender dummies, race dummies and county-year-month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment, the unit of randomization. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months).

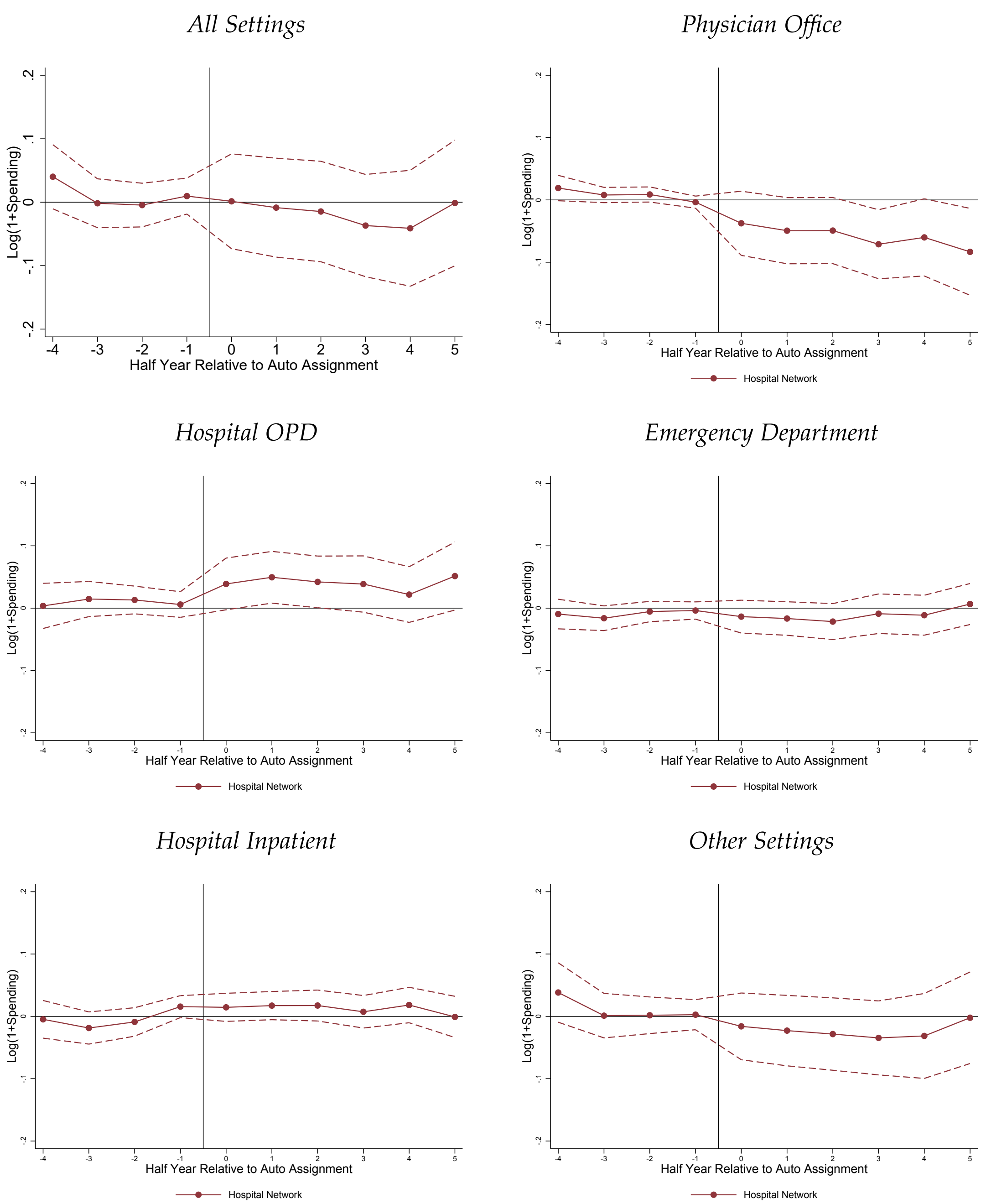

Figure A.7: *Reduced Form Estimates of the Impact of Hospital Networks on Spending*

Notes: These figures plot reduced from estimates of the impact of hospital network size on spending by site of service. I instrument for hospital network size using the size of the network of the assigned plan (measured at the zip x plan x year level). The regressions control for a full set of plan assignment dummies, zip code dummies, indicator variables for each half year of an episode, five-year age x gender dummies, race dummies and county-year-month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment, the unit of randomization. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months).

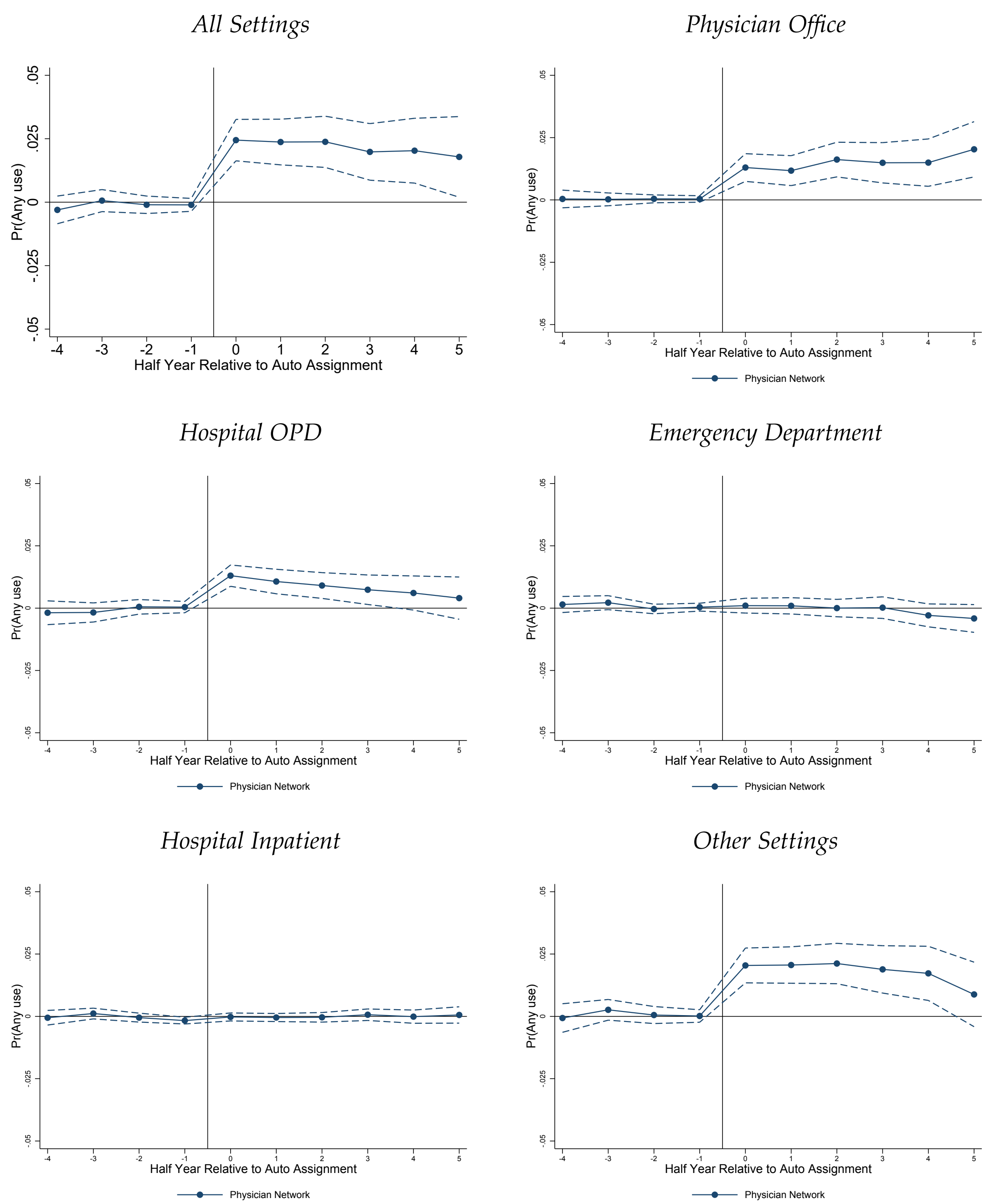

Figure A.8: *Reduced Form Estimates of the Impact of Physician Networks on Utilization*

Notes: These figures plot reduced from estimates of the impact of physician network size on utilization by site of service. I instrument for physician network size using the size of the network of the assigned plan (measured at the zip x plan x year level). The regressions control for a full set of plan assignment dummies, zip code dummies, indicator variables for each half year of an episode, five-year age x gender dummies, race dummies and county-year-month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment, the unit of randomization. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months).

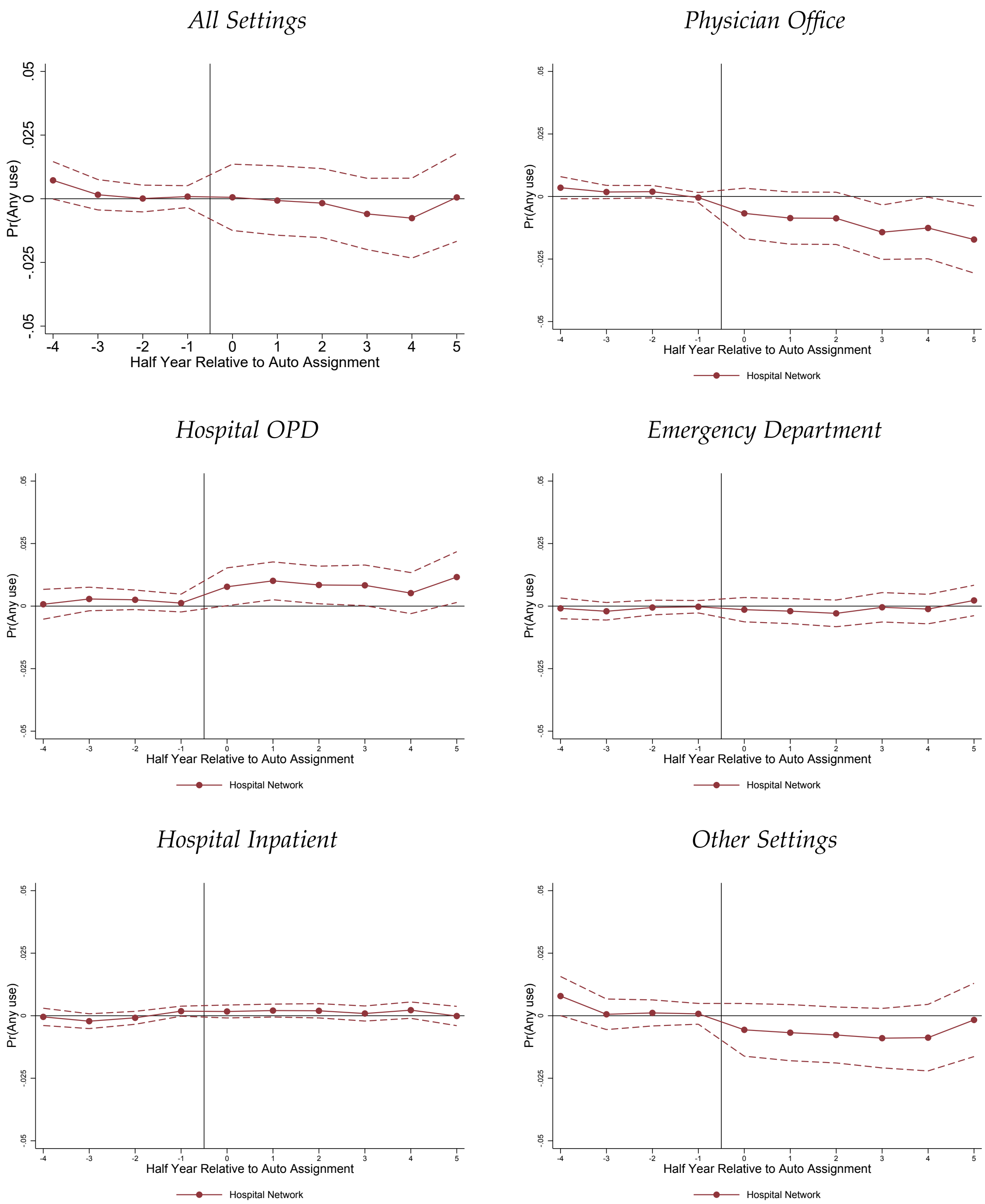

Figure A.9: *Reduced Form Estimates of the Impact of Hospital Networks on Utilization*

Notes: These figures plot reduced from estimates of the impact of hospital network size on utilization by site of service. I instrument for hospital network size using the size of the network of the assigned plan (measured at the zip x plan x year level). The regressions control for a full set of plan assignment dummies, zip code dummies, indicator variables for each half year of an episode, five-year age x gender dummies, race dummies and county-year-month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment, the unit of randomization. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months).

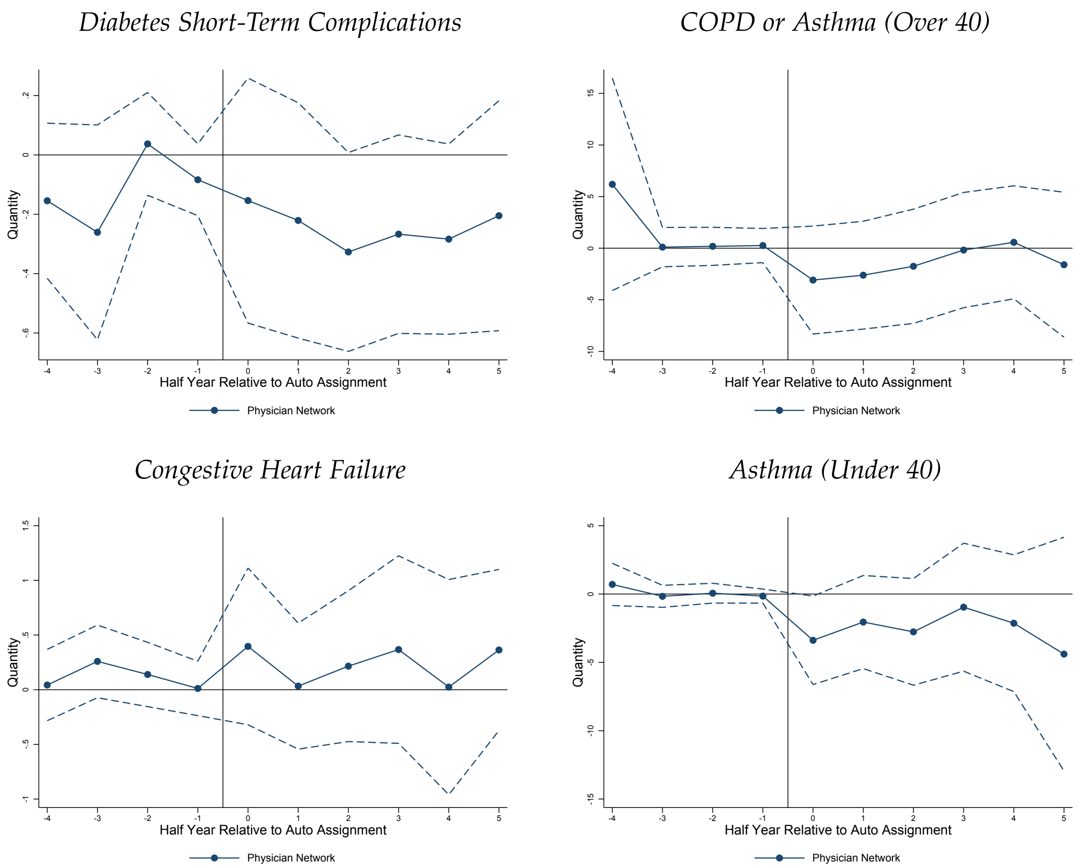

Figure A.10: *Reduced Form Estimates of the Impact of Physician Networks on Avoidable Hospitalizations*

Notes: These figures plot reduced from estimates of the impact of physician network size on avoidable hospitalizations by site of service. I instrument for physician network size using the size of the network of the assigned plan (measured at the zip x plan x year level). The regressions control for a full set of plan assignment dummies, zip code dummies, indicator variables for each half year of an episode, five-year age x gender dummies, race dummies and county-year-month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment, the unit of randomization. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months).

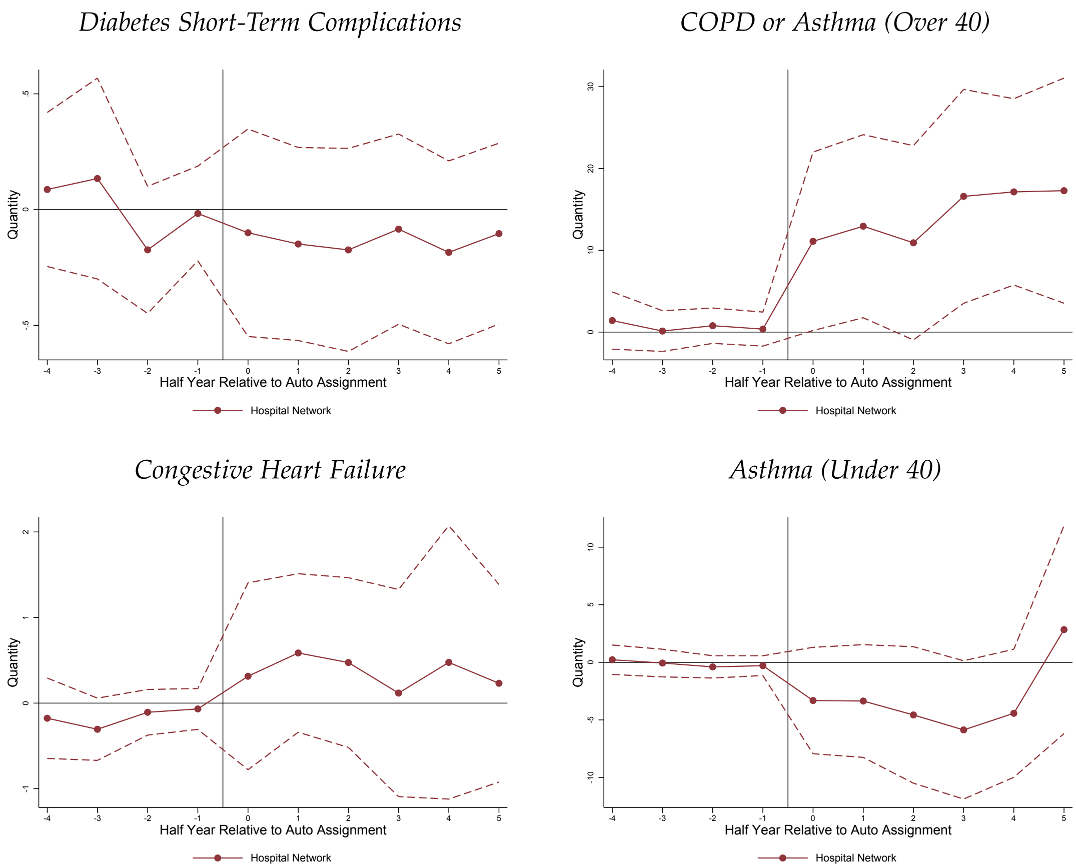

Figure A.11: *Reduced Form Estimates of the Impact of Hospital Networks on Avoidable Hospitalizations*

Notes: These figures plot reduced from estimates of the impact of hospital network size on avoidable hospitalizations by site of service. I instrument for hospital network size using the size of the network of the assigned plan (measured at the zip x plan x year level). The regressions control for a full set of plan assignment dummies, zip code dummies, indicator variables for each half year of an episode, five-year age x gender dummies, race dummies and county-year-month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment, the unit of randomization. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months).

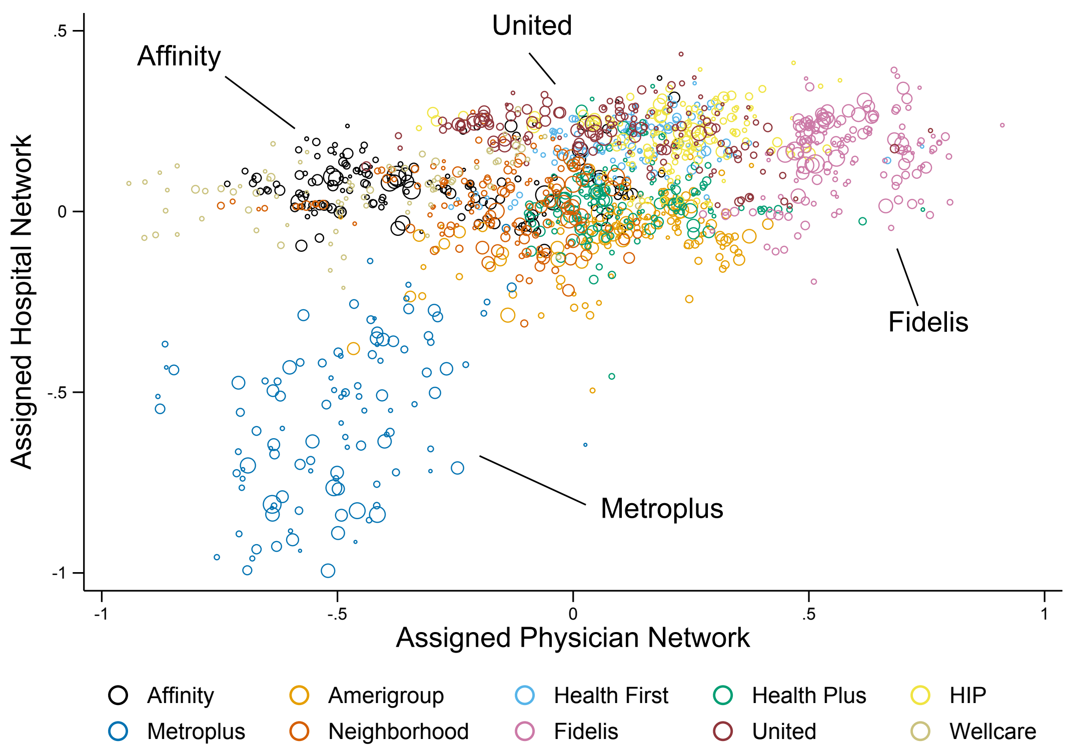

Figure A.12: *Physician and Hospital Network Size By Plan*

Notes: This figure plots recipients' physician and hospital network size against their assigned physician and hospital network size. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months). To construct the binned scatter plot, I separately regress assigned and actual network size against our base controls: recipient zip, indicator variables for each half year of an episode, five-year age x gender bins, race bins, plan assignment, and county x year x month of assignment. I then take the mean residuals in each plan by zip bin, adding the mean network size (actual or assigned) back in to facilitate interpretation of the results.

Table A.1: *Measuring Network Breadth Using Network Utility and the Covered Share of Providers*

	Physician		Hospital	
	Network Utility	Covered Share	Network Utility	Covered Share
	(1)	(2)	(3)	(4)
Wellcare	−0.53	55.0%	0.13	67.9%
Metroplus	−0.49	54.5%	−0.65	48.9%
Affinity	−0.27	62.6%	0.04	77.0%
Neighborhood	−0.12	66.1%	0.01	78.1%
United	0.05	73.6%	0.21	89.9%
Health First	0.07	73.2%	0.19	95.1%
Health Plus	0.07	75.2%	0.00	71.9%
Amerigroup	0.12	71.2%	−0.08	64.5%
HIP	0.23	74.0%	0.23	99.2%
Fidelis	0.60	81.1%	0.15	92.6%
Mean	0.00	69.3%	0.00	78.1%
Standard Deviation	1.00	0.13	1.00	0.21

Notes: This table reports raw means by plan for the network utility measures constructed in Section 1.4.1 and a "covered share" measure of network breadth. The "covered share" measure measures what share of Medicaid providers (physicians or hospitals) each MCO covers, weighted by their Medicaid volume. One limitation of this approach is that it does not include physicians that don't see Medicaid patients (all hospitals saw some Medicaid patients). Hence, the physician "covered share" measures are inflated relative to what they would be if all New York City physicians were accounted for. Columns (1) and (3) report the network utility measures, both of which have been normalized to have mean zero and standard deviation one in the full sample. Columns (2) and (4) report the average covered share by plan. On average, the Medicaid MCOs covered 69.3% of Medicaid physicians in New York City (with the caveat above). The Medicaid MCOs covered 78.1% of the hospitals in New York City (weighted by Medicaid volume). At the plan level, the network utility and "covered share" measures track each other closely.

Table A.2: *Test of Randomization of Network Assignment*

	Baseline Mean	Physician Network Instrument			Hospital Network Instrument		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age	34.364	0.000118	0.000115	0.000028	0.000021	0.000018	0.000018
	(12.074)	(0.000104)	(0.000103)	(0.000059)	(0.000041)	(0.000041)	(0.000041)
Male	0.512	−0.002201	−0.002132	0.000252	−0.000605	−0.000565	−0.000362
	(0.500)	(0.002527)	(0.002524)	(0.001432)	(0.001001)	(0.001000)	(0.000997)
Black	0.511	−0.000428	−0.000260	0.001068	−0.001483	−0.001475	−0.001477
	(0.500)	(0.002603)	(0.002600)	(0.001475)	(0.001031)	(0.001030)	(0.001026)
Baseline Office Spending	15.808	−0.000007	−0.000005	0.000015	−0.000017	−0.000017	−0.000016
	(192.283)	(0.000051)	(0.000051)	(0.000029)	(0.000020)	(0.000020)	(0.000020)
Baseline OPD Spending	62.697	−0.000008	−0.000006	0.000006	−0.000012	−0.000012	−0.000011
	(172.595)	(0.000052)	(0.000052)	(0.000030)	(0.000021)	(0.000021)	(0.000021)
Baseline Clinic Spending	80.979	−0.000040	−0.000038	0.000003	−0.000012	−0.000011	−0.000011
	(215.019)	(0.000053)	(0.000053)	(0.000030)	(0.000021)	(0.000021)	(0.000021)
Baseline Inpatient Spending	474.540	−0.000011	−0.000009	0.000011	−0.000017	−0.000017	−0.000016
	(1914.535)	(0.000051)	(0.000051)	(0.000029)	(0.000020)	(0.000020)	(0.000020)
Baseline Office Quantity	0.066	0.003185	0.002570	0.000051	0.005408	0.005352	0.005247
	(0.246)	(0.014787)	(0.014772)	(0.008380)	(0.005858)	(0.005852)	(0.005831)
Baseline OPD Quantity	0.344	0.005411	0.005327	0.001913	0.000735	0.000639	0.000641
	(1.076)	(0.013982)	(0.013968)	(0.007924)	(0.005539)	(0.005534)	(0.005514)
Baseline Clinic Quantity	0.661	0.007025	0.006924	0.000264	0.000894	0.000769	0.000724
	(2.064)	(0.013943)	(0.013929)	(0.007901)	(0.005524)	(0.005518)	(0.005499)
Baseline Inpatient Quantity	0.050	0.004131	0.004035	0.007894	0.000844	0.000770	0.000795
	(0.150)	(0.018109)	(0.018091)	(0.010263)	(0.007175)	(0.007167)	(0.007142)
Joint F-Test		[0.8055]	[0.7881]	[0.4056]	[0.1481]	[0.1396]	[0.1828]
Other network controls			✓	✓		✓	✓
Plan controls				✓			✓
Observations	104,885	104,885	104,885	104,885	104,885	104,885	104,885

Notes: This table reports reduced form results testing the random assignment of recipients to physician and hospital networks. The auto assignment sample consists of Medicaid recipients aged 18 to 65 auto-assigned to managed care plans from April 2008 to December 2012 in the five counties in New York City (N=104,885 recipients). Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded. Recipients with family members in managed care at the time of assignment or who were enrolled in Medicaid managed care in the year prior to assignment are excluded because their assignments are nonrandom. For some individuals in the sample, less than a year of prior data is available. Baseline spending and utilization variables are formed by calculating the monthly mean in the year prior to assignment for each recipient. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.3: *Test of Randomization Using Active Choosers*

	Baseline Mean	Physician Network	Hospital Network	F-Test p-value
	(1)	(2)	(3)	(4)
Age	37.462	-0.000167^{***}	0.000013	[0.000]
	(13.807)	(0.000045)	(0.000034)	
Male	0.399	-0.003598^{***}	-0.000780	[0.000]
	(0.490)	(0.001235)	(0.000949)	
Black	0.285	-0.008030^{***}	0.004624^{***}	[0.000]
	(0.451)	(0.001516)	(0.001165)	
Baseline Office Spending	33.891	-0.000012	-0.000039^{*}	[0.000]
	(150.862)	(0.000029)	(0.000022)	
Baseline OPD Spending	38.674	0.000002	-0.000054^{**}	[0.000]
	(143.230)	(0.000029)	(0.000023)	
Baseline Clinic Spending	24.162	-0.000012	-0.000026	[0.000]
	(115.524)	(0.000031)	(0.000024)	
Baseline Inpatient Spending	156.306	-0.000010	-0.000035	[0.000]
	(1152.549)	(0.000029)	(0.000022)	
Baseline Office Quantity	0.281	0.010053	-0.001668	[0.000]
	(0.642)	(0.007422)	(0.005701)	
Baseline OPD Quantity	0.218	0.003690	0.007269	[0.000]
	(0.743)	(0.007450)	(0.005722)	
Baseline Clinic Quantity	0.199	0.009265	0.000959	[0.000]
	(1.025)	(0.007442)	(0.005716)	
Baseline Inpatient Quantity	0.019	0.019984^{*}	-0.004461	[0.000]
	(0.093)	(0.012102)	(0.009296)	
Joint F-Test		[0.0000]	[0.0000]	
Observations	104,885	104,885	104,885	

Notes: This table reports reduced form results testing the random assignment of recipients to physician and hospital networks. The auto assignment sample consists of Medicaid recipients aged 18 to 65 auto-assigned to managed care plans from April 2008 to December 2012 in the five counties in New York City (N=104,885 recipients). Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded. Recipients with family members in managed care at the time of assignment or who were enrolled in Medicaid managed care in the year prior to assignment are excluded because their assignments are nonrandom. For some individuals in the sample, less than a year of prior data is available. Baseline spending and utilization variables are formed by calculating the monthly mean in the year prior to assignment for each recipient. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.4: *Hospital Choice Model (Top 25 Hospital Fixed Effects)*

	Simple Model		Full Model	
	Coeff.	Std. Error	Coeff.	Std. Error
	(1)	(2)	(3)	(4)
Hospital Name				
Montefiore Medical Center	0.000	(0.000)	0.000	(0.000)
Long Island Jewish Medical Center	-0.132^{***}	(0.019)	-0.215^{***}	(0.020)
New York-Presbyterian Hospital	-0.151^{***}	(0.014)	-0.226^{***}	(0.014)
North Shore University Hosp	-0.354^{***}	(0.026)	-0.453^{***}	(0.027)
Westchester Medical Center	-0.566^{***}	(0.127)	-0.725^{***}	(0.127)
Staten Island University Hospital	-0.998^{***}	(0.023)	-1.100^{***}	(0.023)
Beth Israel Medical Center	-1.106^{***}	(0.015)	-1.254^{***}	(0.015)
Maimonides Medical Center	-1.149^{***}	(0.016)	-1.300^{***}	(0.016)
Nassau Univ Medical Center	-1.197^{***}	(0.060)	-1.239^{***}	(0.060)
St Luke's-Roosevelt Hosp Ctr	-1.256^{***}	(0.015)	-1.409^{***}	(0.015)
Bellevue Hospital Center	-1.265^{***}	(0.016)	-1.482^{***}	(0.016)
New York Hospital Queens	-1.274^{***}	(0.015)	-1.361^{***}	(0.015)
Mount Sinai Hospital	-1.449^{***}	(0.015)	-1.614^{***}	(0.015)
New York Methodist Hospital	-1.462^{***}	(0.017)	-1.553^{***}	(0.018)
St. John's Riverside Hospital	-1.463^{***}	(0.028)	-1.508^{***}	(0.027)
North Shore University Hospital at Plainview	-1.577^{***}	(0.096)	-1.590^{***}	(0.095)
Jacobi Medical Center	-1.580^{***}	(0.015)	-1.685^{***}	(0.015)
Lutheran Medical Center	-1.605^{***}	(0.016)	-1.725^{***}	(0.017)
Bronx-Lebanon Hospital Center	-1.678^{***}	(0.015)	-1.721^{***}	(0.015)
Saint Vincent's Hosp-Manhattan	-1.694^{***}	(0.029)	-1.770^{***}	(0.029)
Flushing Hospital Medical Ctr	-1.696^{***}	(0.015)	-1.707^{***}	(0.015)
Elmhurst Hospital Center	-1.710^{***}	(0.014)	-1.929^{***}	(0.014)
Lincoln Med & Mental Hlth Ctr	-1.712^{***}	(0.014)	-1.903^{***}	(0.015)
Brooklyn Hospital Center	-1.728^{***}	(0.018)	-1.793^{***}	(0.018)
Winthrop-University Hospital	-1.823^{***}	(0.056)	-1.889^{***}	(0.056)
Model Statistics				
Pseudo R-Squared (McFadden)	0.399		0.405	
Choice Instances	697,803		697,803	

Notes: This table reports hospital fixed effects from the multinomial logit hospital choice model. The logit coefficients shown are interpretable as entering the latent utility function in equation (1.6). *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.5: *Hospital Choice Model (Randomly Assigned Enrollees)*

	Simple Model		Full Model	
	Coeff.	Std. Error	Coeff.	Std. Error
	(1)	(2)	(3)	(4)
Distance to Hospital				
Distance (Minutes)	-0.405^{***}	(0.004)	-0.407^{***}	(0.006)
Distance Squared	0.006^{***}	(0.000)	0.006^{***}	(0.000)
Distance x Pregnancy			-0.031^{***}	(0.008)
Distance x Respiratory			-0.088^{***}	(0.011)
Distance x Mental Illness			0.098^{***}	(0.008)
Distance x Circulatory			-0.010	(0.008)
Distance x Digestive			-0.041^{***}	(0.009)
Out-of-Network Disutility				
Out-of-Network x Affinity	-0.692^{***}	(0.020)	-0.477^{***}	(0.056)
Out-of-Network x Amerigroup	*Single Coefficient*		-0.381^{***}	(0.053)
Out-of-Network x Healthfirst			-0.861^{***}	(0.087)
Out-of-Network x Health Plus			-0.576^{***}	(0.053)
Out-of-Network x HIP			0.003	(0.200)
Out-of-Network x United			-0.747^{***}	(0.117)
Out-of-Network x Metroplus			-0.943^{***}	(0.037)
Out-of-Network x Neighborhood			-0.701^{***}	(0.055)
Out-of-Network x Fidelis			-0.394^{***}	(0.085)
Out-of-Network x Wellcare			-0.483^{***}	(0.081)
Hospital Characteristics				
Hospital Fixed Effects	✓		✓	
Pregnancy x Obstetrics			2.205^{***}	(0.109)
Injury x Trauma Center			0.461^{***}	(0.048)
Mental Illness x Psych			-0.100^{**}	(0.045)
Circulatory x Card Surg			0.289^{***}	(0.042)
Circulatory x Cath Lab			-0.027	(0.043)

Notes: This table reports results from the multinomial logit hospital choice model. 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. In addition to these new variables, the full model also includes distance (and distance-squared) interacted with five-year age x gender bins. The logit coefficients shown are interpretable as entering the latent utility function in equation (1.6). *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.6: *Citywide Physician Choice Model (Active Choice Enrollees)*

	Simple Model		Full Model	
	Coeff.	Std. Error	Coeff.	Std. Error
	(1)	(2)	(3)	(4)
Distance to Physician				
Distance (Minutes)	-0.281^{***}	(0.009)	-0.281^{***}	(0.009)
Distance Squared	0.001^{***}	(0.000)	0.001^{***}	(0.000)
Distance x DME			0.019^{*}	(0.011)
Distance x Imaging			0.103^{***}	(0.009)
Distance x E&M			0.012	(0.009)
Distance x Other			-0.016^{*}	(0.009)
Distance x Procedures			0.046^{***}	(0.009)
Distance x Tests			0.018^{**}	(0.009)
Out-of-Network Disutility				
Out-of-Network	-2.943^{***}	(0.010)		
Out-of-Network x Affinity			-2.769^{***}	(0.029)
Out-of-Network x Amerigroup			-4.099^{***}	(0.059)
Out-of-Network x Healthfirst			-2.231^{***}	(0.019)
Out-of-Network x Health Plus			-3.510^{***}	(0.040)
Out-of-Network x HIP			-2.314^{***}	(0.024)
Out-of-Network x United			-2.699^{***}	(0.030)
Out-of-Network x Metroplus			-3.493^{***}	(0.025)
Out-of-Network x Neighborhood			-2.803^{***}	(0.041)
Out-of-Network x Fidelis			-4.088^{***}	(0.062)
Out-of-Network x Wellcare			-3.474^{***}	(0.041)
Physician Characteristics				
Physician Fixed Effects	✓		✓	

Notes: This table reports results from the multinomial logit physician choice model. Columns 1 and 2 report the coefficients and standard errors for a simple physician choice model that includes distance, distance-squared, an out-of-network indicator and physician fixed effects. Columns 3 and 4 report the coefficients and standard errors for a full physician choice model which includes interactions of distance with service, network with plan and physician characteristics with diagnosis. In addition to these new variables, the full model also includes distance (and distance-squared) interacted with five-year age x gender bins. The logit coefficients shown are interpretable as entering the latent utility function in equation (1.7). *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.7: *Citywide Physician Choice Model (Randomly Assigned Enrollees)*

	Simple Model		Full Model	
	Coeff.	Std. Error	Coeff.	Std. Error
	(1)	(2)	(3)	(4)
Distance to Physician				
Distance (Minutes)	-0.253^{***}	(0.005)	-0.253^{***}	(0.006)
Distance Squared	0.001^{***}	(0.000)	0.001^{***}	(0.000)
Distance x DME			0.022^{***}	(0.007)
Distance x Imaging			0.107^{***}	(0.006)
Distance x E&M			0.023^{***}	(0.006)
Distance x Other			0.014^{**}	(0.006)
Distance x Procedures			0.060^{***}	(0.006)
Distance x Tests			0.049^{***}	(0.006)
Out-of-Network Disutility				
Out-of-Network	-2.424^{***}	(0.008)		
Out-of-Network x Affinity			-3.006^{***}	(0.027)
Out-of-Network x Amerigroup			-2.771^{***}	(0.037)
Out-of-Network x Healthfirst			-1.572^{***}	(0.020)
Out-of-Network x Health Plus			-2.283^{***}	(0.028)
Out-of-Network x HIP			-1.355^{***}	(0.018)
Out-of-Network x United			-3.042^{***}	(0.040)
Out-of-Network x Metroplus			-2.886^{***}	(0.016)
Out-of-Network x Neighborhood			-2.008^{***}	(0.024)
Out-of-Network x Fidelis			-3.979^{***}	(0.051)
Out-of-Network x Wellcare			-3.233^{***}	(0.053)
Physician Characteristics				
Physician Fixed Effects	✓		✓	

Notes: This table reports results from the multinomial logit physician choice model. Columns 1 and 2 report the coefficients and standard errors for a simple physician choice model that includes distance, distance-squared, an out-of-network indicator and physician fixed effects. Columns 3 and 4 report the coefficients and standard errors for a full physician choice model which includes interactions of distance with service, network with plan and physician characteristics with diagnosis. In addition to these new variables, the full model also includes distance (and distance-squared) interacted with five-year age x gender bins. The logit coefficients shown are interpretable as entering the latent utility function in equation (1.7). *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.8: *Health Care Spending*

	Mean of	Physician Network			Hospital Network		
	Dep. Var (1)	ITT (2)	LATE (3)	p-values (4)	ITT (5)	LATE (6)	p-values (7)
Physician Office	0.510	0.058	0.066***	[0.0003]	−0.045	−0.054*	[0.085]
		(0.015)	(0.018)	{0.001}	(0.026)	(0.031)	{0.340}
Hospital Outpatient	0.367	0.057	0.077***	[< 0.0001]	0.043	0.050**	[0.049]
		(0.012)	(0.014)	{< 0.0001}	(0.021)	(0.026)	{0.245}
Emergency Department	0.306	0.002	0.002	[0.860]	−0.013	−0.016	[0.326]
		(0.008)	(0.011)	{0.999}	(0.013)	(0.016)	{0.786}
Hospital Inpatient	0.167	−0.004	−0.005	[0.559]	0.013	0.016	[0.262]
		(0.007)	(0.008)	{0.999}	(0.012)	(0.014)	{0.786}
Other Settings	0.991	0.099	0.109***	[< 0.0001]	−0.020	−0.022	[0.504]
		(0.018)	(0.022)	{< 0.0001}	(0.027)	(0.033)	{0.786}
All Settings	1.577	0.122	0.143***	[< 0.0001]	−0.006	−0.009	[0.850]
		(0.024)	(0.029)		(0.038)	(0.046)	

Notes: Standard errors in parentheses; per comparison p-values in square brackets; family-wise p-values in curly brackets. The dependent variable is log(1+spending). The independent variables are physician and hospital network generosity. Column (1) reports the untransformed dependent variable means. Columns (2) and (5) report the intent-to-treat (ITT) estimates for the physician and hospital networks, respectively. Columns (3) and (6) report the local average treatment effects (LATE) for the physician and hospital networks, respectively. Columns (4) and (6) contain the unadjusted and adjusted p-values. Family-wise p-values are constructed using the Holm-Bonferroni method. All regressions include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since assignment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months). For per comparison significance of the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.9: *Health Care Utilization (Any)*

	Mean of	Physician Network			Hospital Network		
	Dep. Var (1)	ITT (2)	LATE (3)	*p*-values (4)	ITT (5)	LATE (6)	*p*-values (7)
Physician Office	0.104	0.013	0.014***	[< 0.0001]	−0.008	−0.010	[0.116]
		(0.003)	(0.004)	{< 0.0001}	(0.005)	(0.006)	{0.464}
Hospital Outpatient	0.072	0.012	0.016***	[< 0.0001]	0.009	0.010**	[0.027]
		(0.002)	(0.003)	{< 0.0001}	(0.004)	(0.005)	{0.135}
Emergency Department	0.056	0.001	0.001	[0.789]	−0.001	−0.002	[0.544]
		(0.002)	(0.002)	{0.999}	(0.002)	(0.003)	{0.732}
Hospital Inpatient	0.020	−0.000	−0.001	[0.566]	0.002	0.002	[0.244]
		(0.001)	(0.001)	{0.999}	(0.001)	(0.002)	{0.732}
Other Settings	0.206	0.019	0.022***	[< 0.0001]	−0.006	−0.007	[0.268]
		(0.004)	(0.004)	{< 0.0001}	(0.005)	(0.006)	{0.732}
All Settings	0.286	0.023	0.027***	[< 0.0001]	−0.001	−0.001	[0.936]
		(0.004)	(0.005)	{< 0.0001}	(0.007)	(0.008)	

Notes: Standard errors in parentheses; per comparison *p*-values in square brackets; family-wise *p*-values in curly brackets. The dependent variable is an indicator set to one if a recipient used any services in a month. The independent variables are physician and hospital network generosity. Column (1) reports the untransformed dependent variable means. Columns (2) and (5) report the intent-to-treat (ITT) estimates for the physician and hospital networks, respectively. Columns (3) and (6) report the local average treatment effects (LATE) for the physician and hospital networks, respectively. Columns (4) and (6) contain the unadjusted and adjusted *p*-values. Family-wise *p*-values are constructed using the Holm-Bonferroni method. All regressions include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since assignment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (*N*=900,759 patient months). For per comparison significance for the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.10: *Health Care Access*

	Mean of	Physician Network			Hospital Network		
	Dep. Var (1)	ITT (2)	LATE (3)	p-values (4)	ITT (5)	LATE (6)	p-values (7)
Panel A: Avoidable Hospitalizations							
Diabetes Short-Term Complications	0.272	−0.193	−0.228	[0.331]	−0.107	−0.132	[0.608]
		(0.190)	(0.235)	{0.993}	(0.214)	(0.214)	{0.940}
COPD or Asthma (age 40 and older)	13.203	−2.137	−1.843	[0.577]	12.135**	15.102**	[0.030]
		(2.580)	(3.299)	{0.993}	(5.608)	(5.608)	{0.120}
Congestive Heart Failure	0.432	0.299	0.382	[0.349]	0.375	0.457	[0.470]
		(0.328)	(0.407)	{0.993}	(0.524)	(0.524)	{0.940}
Asthma (ages 18 to 39)	6.670	−2.898*	−3.697*	[0.066]	−3.405	−4.191	[0.143]
		(1.636)	(2.002)	{0.264}	(2.376)	(2.376)	{0.429}
Any avoidable hospitalization		−1.015**	−1.209*	[0.053]	0.892	1.041	[0.334]
		(0.498)	(0.623)		(0.890)	(0.890)	
Panel B: Preventive Care							
Cervical Cancer Screening	0.025	0.002	0.002	[0.174]	0.001	0.002	[0.422]
		(0.001)	(0.001)	{0.696}	(0.002)	(0.002)	{0.999}
Chlamydia Screening in Women	0.060	0.005	0.006	[0.192]	0.008	0.009	[0.109]
		(0.003)	(0.004)	{0.696}	(0.005)	(0.005)	{0.436}
Breast Cancer Screening	0.023	−0.000	−0.000	[0.934]	0.004	0.004	[0.410]
		(0.003)	(0.004)	{0.999}	(0.004)	(0.004)	{0.999}
Flu Vaccinations	0.006	0.000	−0.000	[0.983]	−0.000	−0.000	[0.476]
		(0.000)	(0.001)	{0.999}	(0.001)	(0.001)	{0.999}
Any preventive service		0.001	0.001	[0.172]	0.001	0.001	[0.297]
		(0.001)	(0.001)		(0.001)	(0.001)	

Notes: Standard errors in parentheses; per comparison p-values in square brackets; family-wise p-values in curly brackets. The dependent variables are access measures drawn from the 2015 Medicaid Adult Core Set. The screening estimates are computed on subsamples defined in the technical specifications for each measure. The prevention quality indicators (PQIs) apply to the full sample. The domain treatment effects are estimated by stacking the data for each measure. The independent variables are physician and hospital network generosity. Column (1) reports the untransformed dependent variable means. Columns (2) and (5) report the intent-to-treat (ITT) estimates for the physician and hospital networks, respectively. Columns (3) and (6) report the local average treatment effects (LATE) for the physician and hospital networks, respectively. Columns (4) and (6) contain the unadjusted and adjusted p-values. Family-wise p-values are constructed using the Holm-Bonferroni method. All regressions include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since assignment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months). For per comparison significance of the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.11: *Distance Traveled*

	Mean of	Physician Network			Hospital Network		
	Dep. Var (1)	ITT (2)	LATE (3)	*p*-values (4)	ITT (5)	LATE (6)	*p*-values (7)
Panel A: Avoidable Hospitalizations							
Diabetes Short-Term Complications	0.272	−0.193	−0.228	[0.331]	−0.107	−0.132	[0.608]
		(0.190)	(0.235)	{0.993}	(0.214)	(0.214)	{0.940}
COPD or Asthma (age 40 and older)	13.203	−2.137	−1.843	[0.577]	12.135**	15.102**	[0.030]
		(2.580)	(3.299)	{0.993}	(5.608)	(5.608)	{0.120}
Congestive Heart Failure	0.432	0.299	0.382	[0.349]	0.375	0.457	[0.470]
		(0.328)	(0.407)	{0.993}	(0.524)	(0.524)	{0.940}
Asthma (ages 18 to 39)	6.670	−2.898*	−3.697*	[0.066]	−3.405	−4.191	[0.143]
		(1.636)	(2.002)	{0.264}	(2.376)	(2.376)	{0.429}
Any avoidable hospitalization		−1.015**	−1.209*	[0.053]	0.892	1.041	[0.334]
		(0.498)	(0.623)		(0.890)	(0.890)	
Panel B: Preventive Care							
Cervical Cancer Screening	0.025	0.002	0.002	[0.174]	0.001	0.002	[0.422]
		(0.001)	(0.001)	{0.696}	(0.002)	(0.002)	{0.999}
Chlamydia Screening in Women	0.060	0.005	0.006	[0.192]	0.008	0.009	[0.109]
		(0.003)	(0.004)	{0.696}	(0.005)	(0.005)	{0.436}
Breast Cancer Screening	0.023	−0.000	−0.000	[0.934]	0.004	0.004	[0.410]
		(0.003)	(0.004)	{0.999}	(0.004)	(0.004)	{0.999}
Flu Vaccinations	0.006	0.000	−0.000	[0.983]	−0.000	−0.000	[0.476]
		(0.000)	(0.001)	{0.999}	(0.001)	(0.001)	{0.999}
Any preventive service		0.001	0.001	[0.172]	0.001	0.001	[0.297]
		(0.001)	(0.001)		(0.001)	(0.001)	

Notes: Standard errors in parentheses; per comparison *p*-values in square brackets; family-wise *p*-values in curly brackets. The dependent variable is travel time in minutes. The independent variables are physician and hospital network generosity. Column (1) reports the untransformed dependent variable means. Columns (2) and (5) report the intent-to-treat (ITT) estimates for the physician and hospital networks, respectively. Columns (3) and (6) report the local average treatment effects (LATE) for the physician and hospital networks, respectively. Columns (4) and (6) contain the unadjusted and adjusted *p*-values. Family-wise *p*-values are constructed using the Holm-Bonferroni method. All regressions include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since assignment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The sample consists of physician and hospital claims for recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months). For per comparison significance of the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.12: *Comparison of Main Results to Separate Physician and Hospital Regressions*

		Physician Network		Hospital Network	
	Mean of Dep. Var (1)	Main Results (2)	No Hospital Controls (3)	Main Results (4)	No Physician Controls (5)
Panel A: Health Care Use					
Spending	1.577	0.143***	0.142***	−0.009	0.037
		(0.029)	(0.029)	(0.046)	(0.045)
Utilization (any)	0.286	0.027***	0.027***	−0.001	0.008
		(0.005)	(0.005)	(0.008)	(0.008)
Panel B: Access					
Preventive Care	0.017	0.001	0.001	0.001	0.001
		(0.001)	(0.001)	(0.001)	(0.001)
Avoidable Hospitalizations	3.234	−1.209*	−1.076*	1.041	0.655
		(0.623)	(0.609)	(1.076)	(1.051)
Panel C: Plan Loyalty					
% recipients that switch plans	0.142	−0.051***	−0.056***	−0.041***	−0.057***
		(0.007)	(0.007)	(0.012)	(0.012)

Notes: Standard errors in parentheses. The dependent variables are based on seven analyses across four domains: (i) health care use; (ii) access; and (iii) plan loyalty. The independent variables are physician and hospital network generosity. Columns (2) and (4) report the main two-stage least squares (2SLS) results from estimating equation (2.13) for physician and hospital networks, respectively. Columns (3) and (5) present the results of a sensitivity analysis in which the impact of physician and hospital networks are estimated separately (without controls for the other). All regressions include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since assignment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months). For per comparison significance for the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table A.13: *2SLS Estimates of the Impact of Plans on Spending, Utilization, Access and Plan Loyalty*

	Health Care Use		Access		Distance Traveled		Satisfaction
	Spending (1)	Any Use (2)	Prevention (3)	Avoidable Hospitalizations (4)	Physician Office (5)	Hospital (6)	% recipients that switch (7)
Affinity	0.003	0.015*	0.001	−0.540	−0.655	0.996***	−0.025**
	(0.053)	(0.009)	(0.001)	(0.929)	(0.781)	(0.332)	(0.012)
Amerigroup	−0.137**	−0.014	0.001	−0.705	−0.498	0.888**	0.017
	(0.056)	(0.009)	(0.001)	(0.925)	(0.862)	(0.353)	(0.013)
Health First	0.059	0.021**	0.002	3.358**	0.081	0.526	−0.086***
	(0.059)	(0.010)	(0.001)	(1.322)	(0.841)	(0.334)	(0.014)
Health Plus	0.073	0.019*	0.004***	−0.324	−1.711*	0.850**	−0.040***
	(0.059)	(0.010)	(0.001)	(1.016)	(0.901)	(0.407)	(0.013)
HIP	−0.070	−0.004	0.000	3.248***	−1.124	0.898***	−0.032**
	(0.059)	(0.010)	(0.001)	(1.226)	(0.831)	(0.334)	(0.013)
Metroplus	0.222***	0.042***	0.002	2.714**	−1.254	0.219	−0.104***
	(0.067)	(0.011)	(0.002)	(1.329)	(0.786)	(0.338)	(0.014)
Neighborhood	−0.030	0.003	0.000	−0.830	−1.125	1.059***	−0.013
	(0.055)	(0.009)	(0.001)	(0.910)	(0.792)	(0.338)	(0.012)
Fidelis	−0.053	−0.000	−0.000	2.035*	−0.713	1.137***	−0.034***
	(0.062)	(0.011)	(0.001)	(1.141)	(0.905)	(0.382)	(0.013)
United	−0.084	−0.004	−0.002	−1.115	−0.182	1.088**	−0.013
	(0.056)	(0.009)	(0.001)	(0.918)	(0.869)	(0.475)	(0.013)
Joint F-Test	[0.000]	[0.000]	[0.000]	[0.000]	[0.003]	[0.000]	[0.000]

Notes: Standard errors in parentheses. Estimated effects are relative to the omitted plan, Wellcare. The dependent variables are based on seven analyses across four domains: (i) health care use; (ii) access; (iii) distance traveled; and (iv) satisfaction. The independent variables are physician and hospital network generosity. Columns (2) and (4) report the main two-stage least squares (2SLS) results from estimating equation (2.13) for physician and hospital networks, respectively. Columns (3) and (5) present the results of estimating the same specification without controlling for plan effects. All regressions include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since assignment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months). For per comparison significance of the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.
†Avoidable hospitalizations are measured per 1000 recipient months.

Table A.14: *Comparison of 2SLS and OLS Results*

	Mean of	Physician Network		Hospital Network	
	Dep. Var	2SLS	OLS	2SLS	OLS
	(1)	(2)	(3)	(4)	(5)
Panel A: Health Care Use					
Spending	1.577	0.143***	0.202***	−0.009	−0.113***
		(0.029)	(0.016)	(0.046)	(0.020)
Utilization (any)	0.286	0.027***	0.037***	−0.001	−0.020***
		(0.005)	(0.003)	(0.008)	(0.003)
Panel B: Access					
Preventive Care	0.049	0.001	0.003	0.001	−0.001
		(0.001)	(0.000)	(0.001)	(0.001)
Avoidable Hospitalizations	3.234	−1.209*	−0.608	1.041	−0.092
		(0.623)	(0.223)	(1.076)	(0.326)
Panel C: Distance Traveled					
Physician Office	7.704	−0.173	−1.276***	−0.273	−0.485***
		(0.381)	(0.098)	(0.184)	(0.110)
Hospital	6.212	−0.361*	−0.040	0.108	−2.854***
		(0.190)	(0.084)	(0.097)	(0.099)

Notes: Standard errors in parentheses. The dependent variables are based on seven analyses across four domains: (i) health care use; (ii) access; and (iii) plan loyalty. The independent variables are physician and hospital network generosity. Columns (2) and (4) report the main two-stage least squares (2SLS) results from estimating equation (2.13) for physician and hospital networks, respectively. Columns (3) and (5) present the results of a sensitivity analysis in which the impact of physician and hospital networks are estimated separately (without controls for the other). All regressions include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since assignment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,759 patient months). For per comparison significance for the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix B

Appendix to Chapter 2

B.1 Supplementary Tables and Figures

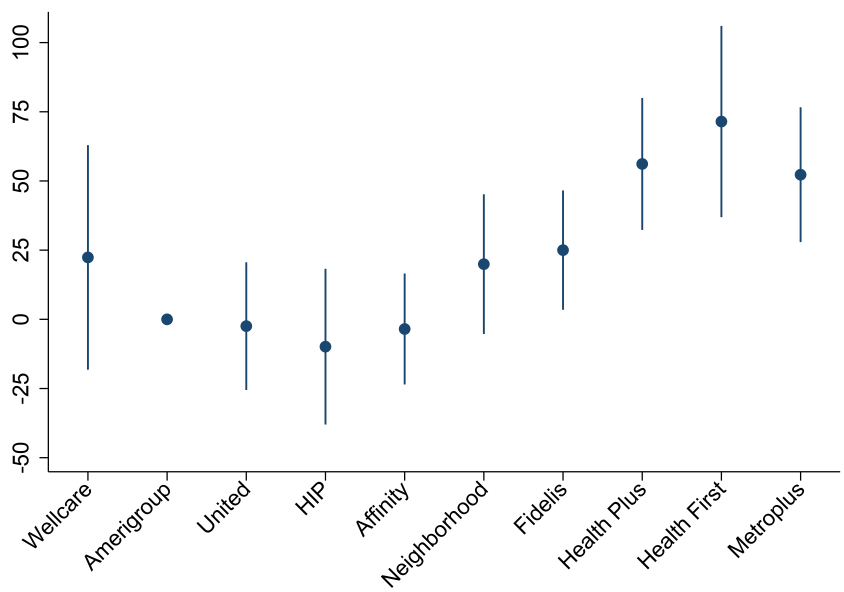

Figure B.1: *Main Results: IV Estimates of Plan Effects on Spending*

Notes: The figure plots coefficients on instrumented plan indicators from 2SLS regressions in which the dependent variable is spending (rather than log spending). Regressions are run at the recipient $\times$ month level, and the sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,449 patient months).

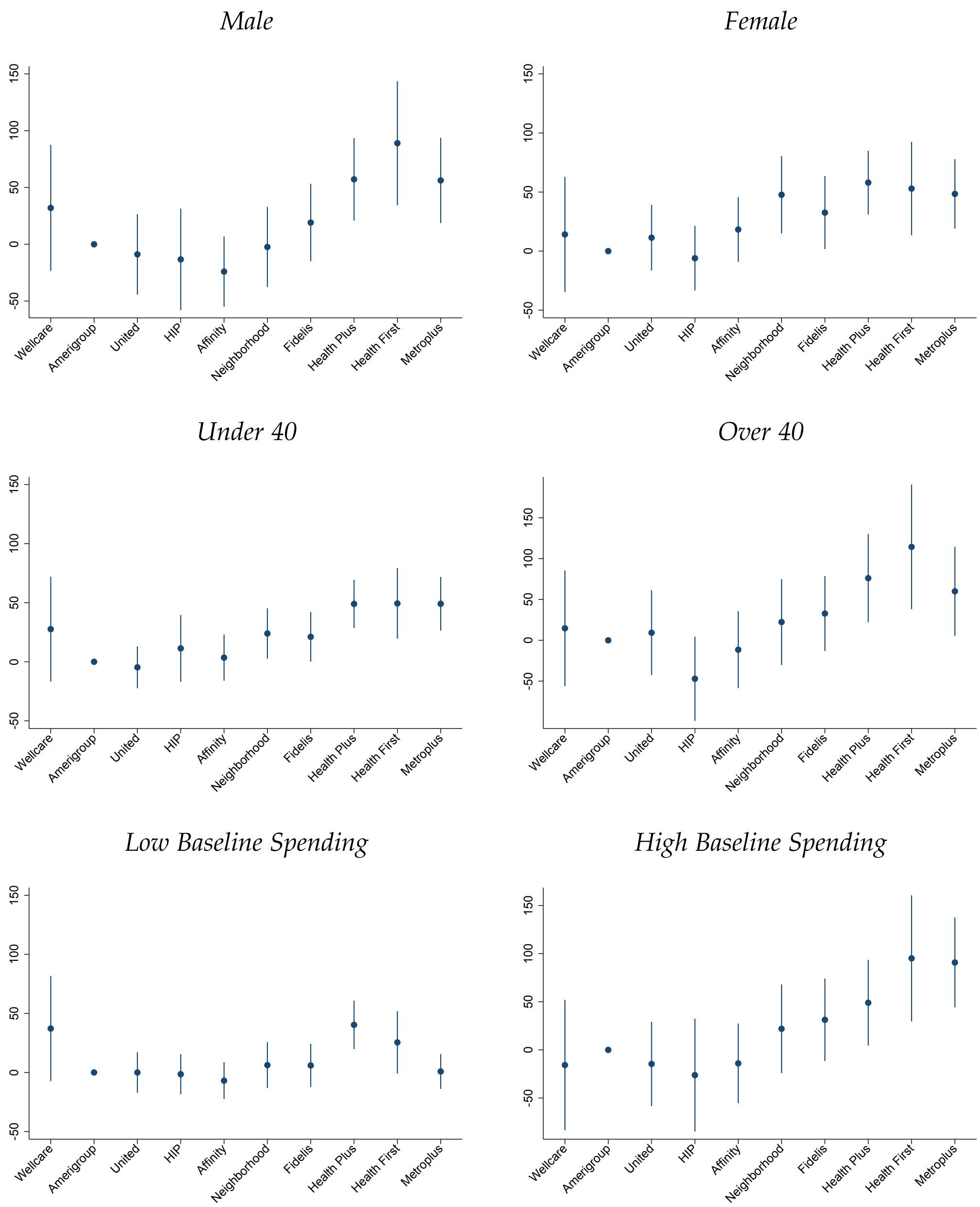

Figure B.2: *Main Results: IV Plan Spending Effects by Recipient Characteristics*

Notes: The figure plots coefficients on instrumented plan indicators from 2SLS regressions in which the dependent variable is level spending. Subsamples are restricted by sex, age and baseline spending, according to the subfigure heading. Regressions are run at the recipient $\times$ month level, and the sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,449 patient months).

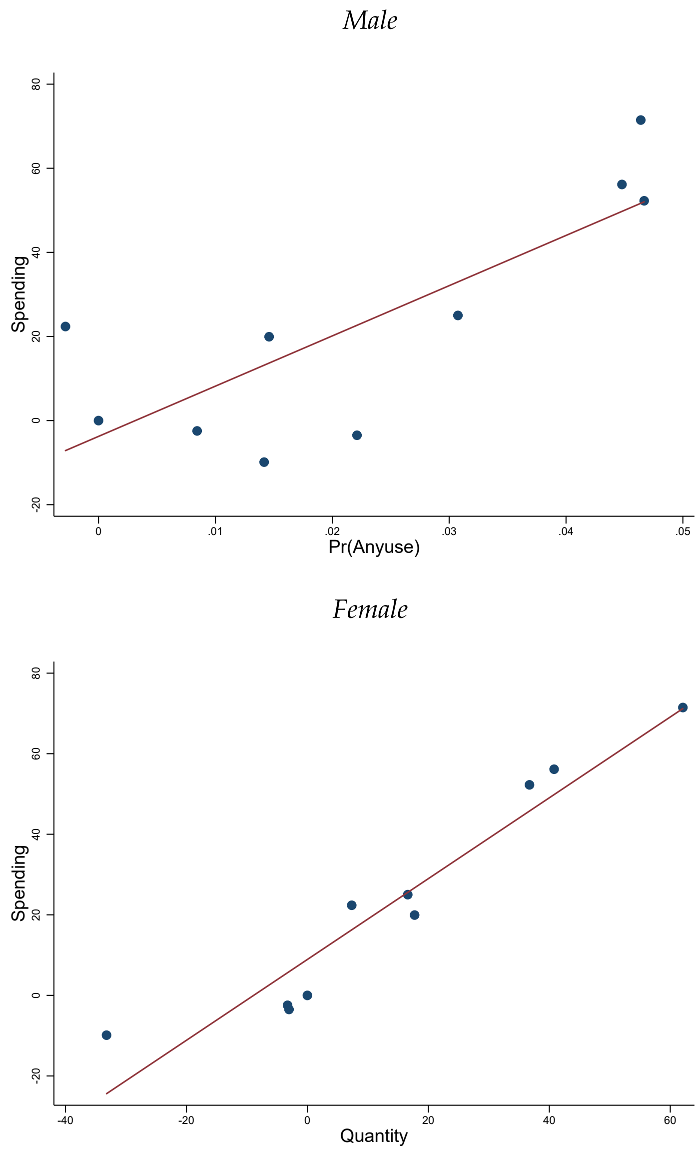

Figure B.3: *Variation in Spending Driven by Differences in Quantity*

Notes: The figure plots coefficients on instrumented plan indicators from 2SLS regressions in which the dependent variable is level spending against two measures of quantity: the probability of any health care use in a month, "Pr(Any Use)", or a measure of quantity based on repricing all claims at the median price for the market, "Quantity". Regressions are run at the recipient $\times$ month level, and the sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,449 patient months).

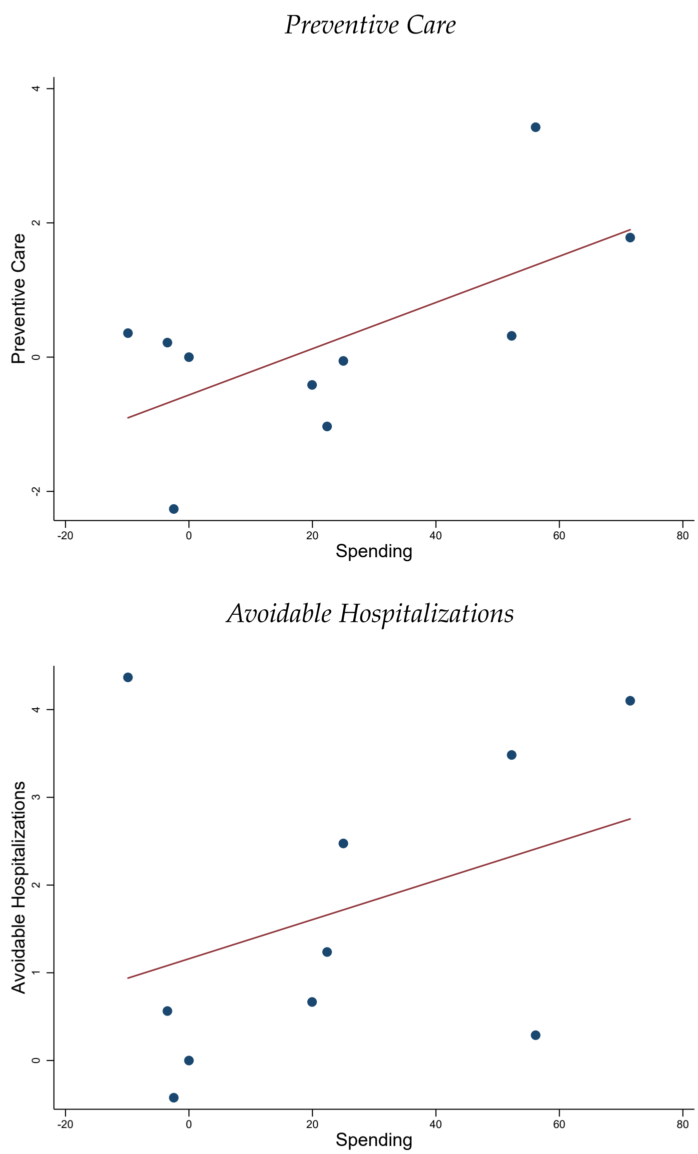

Figure B.4: *Higher Spending Correlated With More Appropriate and Inappropriate Care*

Notes: The figure plots coefficients on instrumented plan indicators from 2SLS regressions in which the dependent variables are level spending and two measures of quality (preventive care and avoidable hospitalizations) relative to the reference plan (Amerigroup). Panel (A) plots preventive care use in a month against log spending. Panel (B) plots avoidable hospitalizations against log spending. Regressions are run at the recipient $\times$ month level, and the sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,449 patient months).

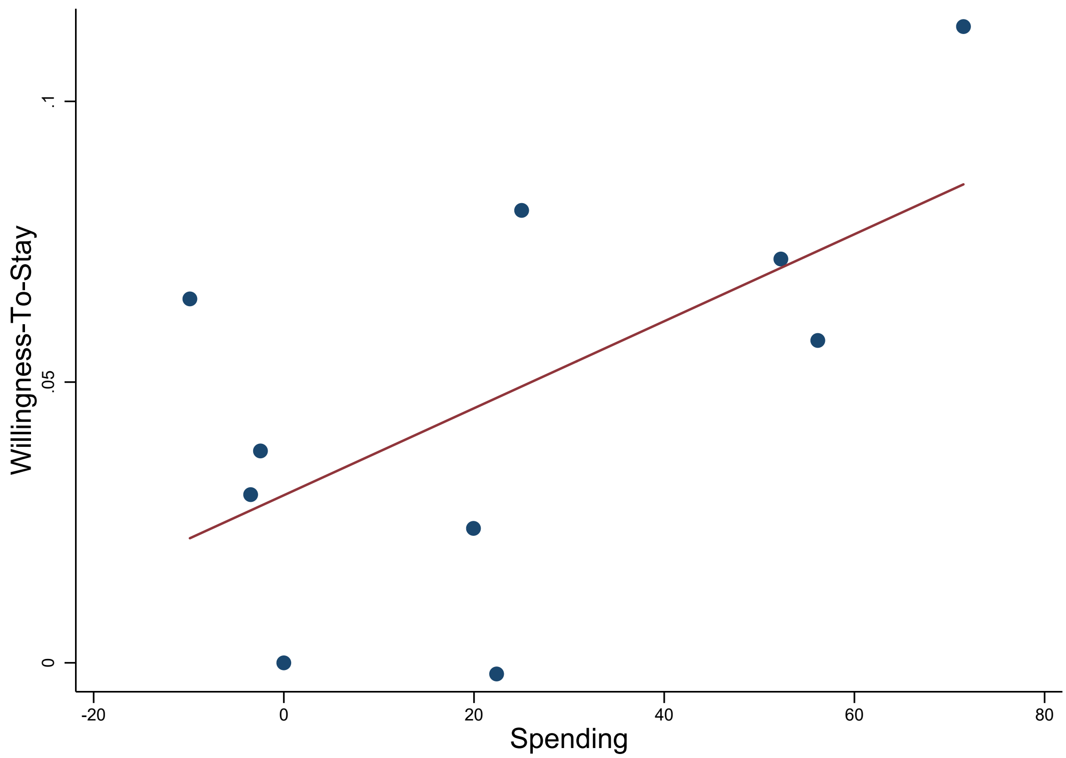

Figure B.5: *Higher Spending Plans Improve Recipient Satisfaction*

Notes: The figure plots coefficients on instrumented plan indicators from 2SLS regressions in which the dependent variables are level spending and willingness-to-stay (a measure of recipient satisfaction) relative to the reference plan (Amerigroup). Regressions are run at the recipient $\times$ month level, and the sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,449 patient months).

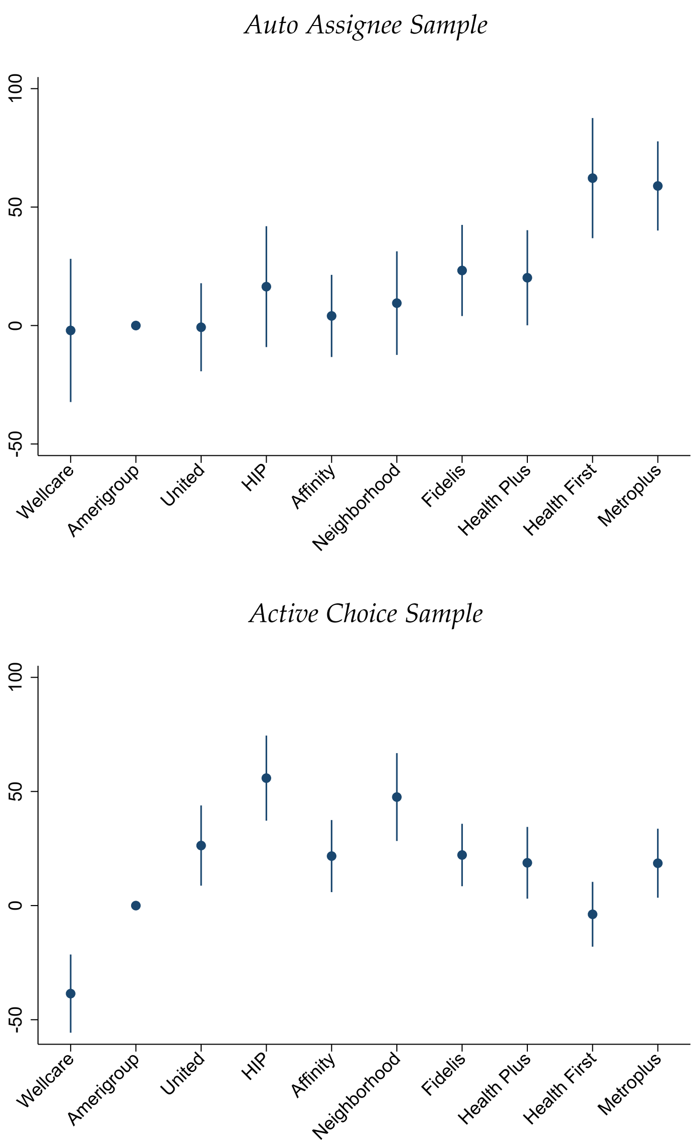

Figure B.6: *Selection Differs for Auto Assignees and Active Choice Recipients*

Notes: The figure plots coefficients on instrumented plan indicators from 2SLS regressions in which the dependent variable is a residual that measures (production-purged) selection based on level spending relative to the reference plan (Amerigroup). Panel (A) plots these coefficients for the auto assignee sample. Panel (B) plots these coefficients for the active choice sample. Regressions are run at the recipient $\times$ month level, and the sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded (N=900,449 patient months).

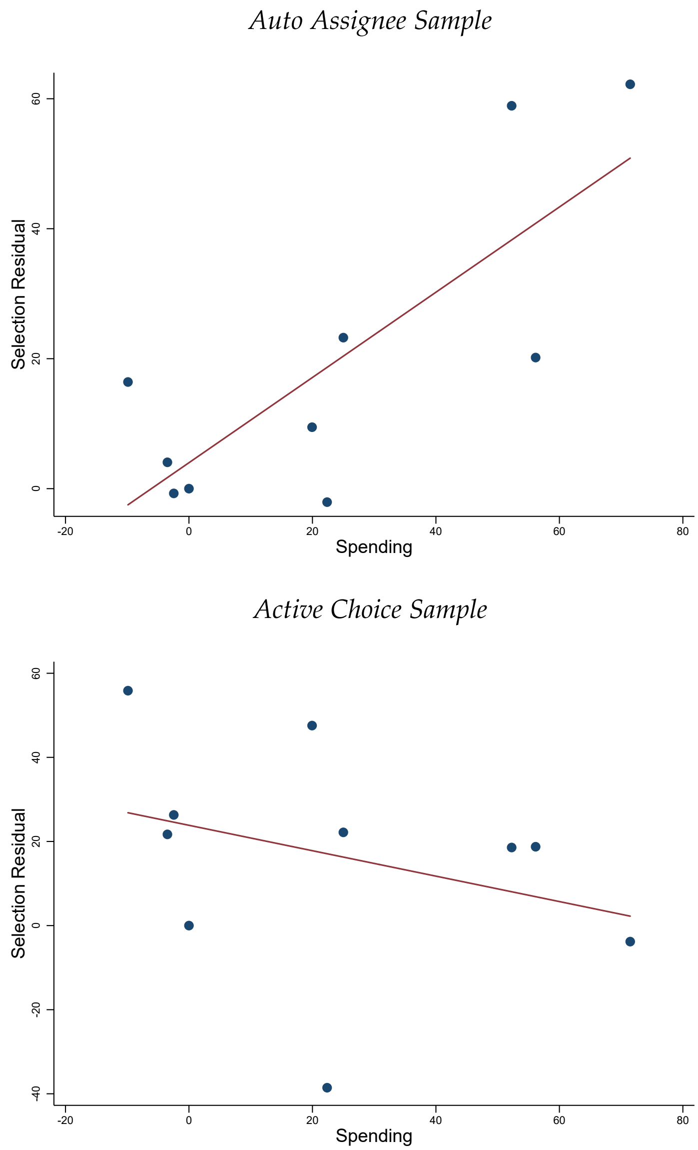

Figure B.7: *Selection ($) Vs. Production for Auto Assignees and Active Choosers*

Notes: The figure presents scatter plots of plan production (as measured by level spending) against a residual that measures (production-purged) selection based on level spending. Panel (A) plots these coefficients for the auto assignee sample. Panel (B) plots these coefficients for the active choice sample. Regressions are run at the recipient $\times$ month level, and the sample consists of recipients aged 18 to 65 auto-assigned to Medicaid Managed Care plans from April 2008 to December 2012 in the five counties in New York City. Recipients that qualify for Medicaid based on receipt of Supplemental Security Income (SSI) are excluded.

Table B.1: *Summary Statistics: Active Choosers and "Auto Assignees"*

	Active Choice Sample (1)	Auto Assign. Sample (2)	Difference (3)
Panel A: Demographic information			
Age	40.06	35.29	4.77***
Male	0.39	0.50	−0.11***
White	0.33	0.27	0.06***
Black	0.25	0.51	−0.26***
Other	0.43	0.22	0.21***
Panel B: Any monthly utilization			
Physician Office	0.15	0.10	0.04***
Hospital Outpatient	0.05	0.07	−0.02***
Emergency Department	0.02	0.06	−0.04***
Hospital Inpatient	0.01	0.02	−0.01***
Other	0.17	0.21	−0.04***
All Services	0.25	0.29	−0.03***
Panel B: Health care spending			
Physician Office	38.96	25.60	13.36***
Hospital Outpatient	17.33	25.04	−7.70***
Emergency Department	5.10	15.84	−10.74***
Hospital Inpatient	48.38	134.74	−86.37***
Other	43.98	61.78	−17.80***
All Services	153.76	263.00	−109.24***
Observations	3,034,035	900,449	3,934,484

Notes: This table reports summary statistics separately for the sample of Medicaid beneficiaries who actively chose a managed care plan (Active Choice Sample) and the sample of beneficiaries who were auto-enrolled in a managed care plan (Auto Assign. Sample). *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table B.2: *OLS and IV Estimates of Plan Effects on Spending ($)*

	Auto Assignment Sample			Active Choice Sample
	OLS	RF	2SLS	OLS
Plan Name	(1)	(2)	(3)	(4)
United	-4.6	-3.2	-2.5	50.3^{***}
	(9.4)	(9.9)	(11.7)	(8.3)
Wellcare	19.0	18.3	22.4	-0.6
	(15.5)	(16.1)	(20.6)	(4.6)
HIP	6.6	-11.0	-9.9	16.9^{*}
	(13.1)	(12.2)	(14.3)	(9.1)
Affinity	1.2	-3.0	-3.5	11.5^{*}
	(8.8)	(8.4)	(10.2)	(6.4)
Neighborhood	28.0^{**}	16.5	19.9	60.9^{***}
	(11.0)	(10.6)	(12.8)	(9.8)
Fidelis	47.0^{***}	19.6^{**}	25.0^{**}	67.7^{***}
	(9.7)	(9.3)	(11.0)	(4.5)
Health First	128.9^{***}	61.1^{***}	71.5^{***}	38.1^{***}
	(12.7)	(15.4)	(17.6)	(9.0)
Health Plus	76.2^{***}	46.8^{***}	56.2^{***}	72.5^{***}
	(10.2)	(10.0)	(12.1)	(4.0)
Metroplus	106.1^{***}	43.1^{***}	52.3^{***}	49.3^{***}
	(9.7)	(10.2)	(12.4)	(7.6)
Full Controls	YES	YES	YES	YES
Observations	900,449	900,449	900,449	3,034,035
Joint F-Test	[0.000]	[0.000]	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variable is monthly spending in dollars. The independent variables are plan effects relative to the reference plan (Amerigroup). Columns (1), (2), and (3) present estimates based on individuals who were auto-assigned to a plan. Column (1) presents results from an OLS regression of spending on plan dummies. Column (2) presents results from reduced form regressions of spending on dummies indicating the plan to which the enrollee was auto-assigned. Column (3) presents IV results estimated using two-stage least squares (2SLS) from the estimating equation (2.13). Column (4) presents results from an OLS regression of spending on plan dummies for the population of enrollees who actively chose a plan. Controls include baseline spending, five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), zip code fixed effects and county x year x month of assignment fixed effects. Standard errors clustered on the county x year x month of assignment. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table B.3: *Sensitivity of Main IV Spending Estimates to Controls*

	Health Care Spending		Log Spending	
	2SLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)
United	-5.875	-2.455	0.018	0.049
	(11.832)	(11.725)	(0.031)	(0.030)
Wellcare	13.294	22.376	-0.010	0.051
	(20.426)	(20.609)	(0.058)	(0.054)
HIP	-12.017	-9.878	0.070**	0.093***
	(14.177)	(14.295)	(0.029)	(0.028)
Affinity	-5.264	-3.490	0.087***	0.110***
	(10.258)	(10.180)	(0.030)	(0.029)
Neighborhood	17.787	19.944	0.087***	0.105***
	(12.849)	(12.824)	(0.028)	(0.028)
Fidelis	22.103**	25.009**	0.157***	0.181***
	(11.090)	(10.956)	(0.029)	(0.028)
Health First	69.483***	71.470***	0.239***	0.265***
	(17.391)	(17.558)	(0.038)	(0.038)
Health Plus	45.604***	56.150***	0.204***	0.277***
	(12.155)	(12.112)	(0.032)	(0.030)
Metroplus	49.326***	52.270***	0.294***	0.325***
	(12.179)	(12.371)	(0.033)	(0.031)
Observations	900,449	900,449	900,449	900,449
Full Controls	No	Yes	No	Yes
Joint F-Test	[0.000]	[0.000]	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variables are two measures of spending: health care spending winsorized at the one percent level (high only) and log health care spending. The independent variables are plan effects relative to the reference plan (Amerigroup). The plan effects are estimated using two-stage least squares (2SLS) from the estimating equation (2.13). Columns (1) and (2) present the results of estimating this specification with and without additional controls (all specifications control for the county x year x month of assignment) on winsorized spending. Columns (3) and (4) present analogous estimates for log spending with and without controls. Controls include baseline spending, five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. For per comparison significance for the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table B.4: *IV Estimates of Plan Effects on Log Spending by Provider Type*

	Total Spending	Physician Office	Hospital OPD	Emergency Department	Hospital Inpatient	Other Settings
	(1)	(2)	(3)	(4)	(5)	(6)
United	0.049	0.021	-0.233^{***}	-0.017^{*}	-0.010	0.158^{***}
	(0.030)	(0.017)	(0.015)	(0.009)	(0.008)	(0.023)
Wellcare	0.051	0.037	-0.120^{***}	0.029	0.010	0.100^{**}
	(0.054)	(0.037)	(0.031)	(0.021)	(0.017)	(0.044)
HIP	0.093^{***}	0.112^{***}	0.009	0.005	-0.029^{***}	-0.045^{**}
	(0.028)	(0.015)	(0.018)	(0.011)	(0.010)	(0.019)
Affinity	0.110^{***}	0.146^{***}	-0.210^{***}	0.014	-0.019^{**}	0.147^{***}
	(0.029)	(0.017)	(0.014)	(0.011)	(0.008)	(0.022)
Neighborhood	0.105^{***}	0.030^{**}	0.045^{***}	0.005	0.004	0.102^{***}
	(0.028)	(0.015)	(0.017)	(0.010)	(0.008)	(0.020)
Fidelis	0.181^{***}	0.118^{***}	-0.032^{**}	-0.000	0.015^{*}	0.130^{***}
	(0.028)	(0.016)	(0.015)	(0.009)	(0.008)	(0.020)
Health First	0.265^{***}	0.075^{***}	0.100^{***}	0.036^{**}	0.064^{***}	0.132^{***}
	(0.038)	(0.024)	(0.018)	(0.014)	(0.014)	(0.027)
Health Plus	0.277^{***}	0.140^{***}	-0.052^{***}	0.033^{***}	0.035^{***}	0.248^{***}
	(0.030)	(0.017)	(0.017)	(0.010)	(0.009)	(0.022)
Metroplus	0.325^{***}	0.135^{***}	-0.054^{***}	0.035^{***}	0.032^{***}	0.170^{***}
	(0.031)	(0.018)	(0.018)	(0.010)	(0.010)	(0.022)
Full Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	900,449	900,449	900,449	900,449	900,449	900,449
F-Test (P-val)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variable is the log of monthly spending in different care settings. The independent variables are plan effects relative to the reference plan (Amerigroup). The table presents IV results estimated using two-stage least squares (2SLS) from the estimating equation (2.13) separately for each care setting. The largest categories in the "other" setting include freestanding clinics, dental services, transportation and non-physician practitioner services. Controls include baseline spending, five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table B.5: *IV Estimates of Plan Effects on Level Spending by Provider Type*

	Total Spending	Physician Office	Hospital OPD	Emergency Department	Hospital Inpatient	Other Settings
	(1)	(2)	(3)	(4)	(5)	(6)
United	-2.455	-2.125^{**}	-15.294^{***}	0.611	-10.703	25.057^{***}
	(11.725)	(1.068)	(1.459)	(0.639)	(9.329)	(2.354)
Wellcare	22.376	2.835	-9.522^{***}	3.364^{**}	8.102	17.597^{***}
	(20.609)	(2.468)	(1.998)	(1.416)	(16.742)	(3.995)
HIP	-9.878	2.900^{***}	1.971	4.368^{***}	-22.792^{*}	3.675^{*}
	(14.295)	(1.007)	(1.724)	(0.768)	(12.133)	(2.079)
Affinity	-3.490	8.901^{***}	-13.320^{***}	0.199	-14.205^{*}	14.936^{***}
	(10.180)	(1.159)	(1.434)	(0.638)	(8.205)	(1.871)
Neighborhood	19.944	1.604	3.785^{**}	-0.111	3.960	10.705^{***}
	(12.824)	(1.032)	(1.849)	(0.589)	(10.380)	(1.848)
Fidelis	25.009^{**}	8.479^{***}	-3.043^{**}	1.179^{**}	7.367	11.027^{***}
	(10.956)	(1.112)	(1.508)	(0.586)	(9.002)	(1.665)
Health First	71.470^{***}	4.617^{***}	-1.258	-0.296	56.548^{***}	11.859^{***}
	(17.558)	(1.477)	(1.955)	(0.792)	(15.456)	(2.470)
Health Plus	56.150^{***}	6.814^{***}	-2.208	1.708^{***}	24.872^{**}	24.965^{***}
	(12.112)	(1.095)	(1.738)	(0.561)	(9.681)	(2.392)
Metroplus	52.270^{***}	8.767^{***}	-0.180	0.667	17.720^{*}	25.296^{***}
	(12.371)	(1.194)	(1.944)	(0.596)	(10.047)	(2.174)
Full Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	900,449	900,449	900,449	900,449	900,449	900,449
F-Test (P-val)	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variable is the level of monthly spending in different care settings. The independent variables are plan effects relative to the reference plan (Amerigroup). The table presents IV results estimated using two-stage least squares (2SLS) from the estimating equation (2.13) separately for each care setting. The largest categories in the "other" setting include freestanding clinics, dental services, transportation and non-physician practitioner services. Controls include baseline spending, five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table B.6: *OLS and IV Estimates of Plan Effects on Pr(Health Care Use)*

	Auto Assignment Sample			Active Choice Sample
Plan Name	OLS (1)	RF (2)	2SLS (3)	OLS (4)
United	0.015***	0.006	0.008	0.105***
	(0.004)	(0.004)	(0.005)	(0.017)
Wellcare	0.000	−0.001	−0.003	−0.012**
	(0.007)	(0.007)	(0.009)	(0.005)
HIP	0.030***	0.010**	0.014***	−0.001
	(0.005)	(0.004)	(0.005)	(0.011)
Affinity	0.029***	0.018***	0.022***	−0.000
	(0.005)	(0.004)	(0.005)	(0.008)
Neighborhood	0.019***	0.012***	0.015***	0.055***
	(0.004)	(0.004)	(0.005)	(0.015)
Fidelis	0.058***	0.025***	0.031***	0.088***
	(0.004)	(0.004)	(0.005)	(0.007)
Health First	0.112***	0.039***	0.046***	0.018*
	(0.006)	(0.006)	(0.007)	(0.011)
Health Plus	0.055***	0.037***	0.045***	0.086***
	(0.004)	(0.004)	(0.005)	(0.006)
Metroplus	0.094***	0.038***	0.047***	0.035***
	(0.004)	(0.004)	(0.005)	(0.008)
Full Controls	Yes	Yes	Yes	Yes
Observations	900,449	900,449	900,449	3,034,035
Joint F-Test	[0.000]	[0.000]	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variable is an indicator for any health care use in a month, a measure of quantity on the extensive margin. The independent variables are plan effects relative to the reference plan (Amerigroup). Columns (1), (2), and (3) present specifications for the sample of individuals who were auto-assigned to a plan. Column (1) presents results from an OLS regression of log spending on plan dummies. Column (2) presents results from reduced form regressions of log spending on dummies indicating the plan to which the enrollee was auto-assigned. Column (3) presents IV results estimated using two-stage least squares (2SLS) from the estimating equation (2.13). Column (4) presents results from an OLS regression of log spending on plan dummies for the population of enrollees who actively chose a plan. Controls include baseline spending, five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The active choice sample is randomly drawn from recipients that were not auto-enrolled. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table B.7: *OLS and IV Estimates of Plan Effects on Log Quantity*

	Auto Assignment Sample			Active Choice Sample
	OLS	RF	2SLS	OLS
Plan Name	(1)	(2)	(3)	(4)
United	0.081***	0.031	0.044	0.569***
	(0.025)	(0.025)	(0.030)	(0.083)
Wellcare	0.046	0.035	0.037	−0.039
	(0.038)	(0.041)	(0.052)	(0.028)
HIP	0.134***	0.029	0.047*	−0.001
	(0.025)	(0.022)	(0.026)	(0.056)
Affinity	0.153***	0.093***	0.113***	0.017
	(0.024)	(0.023)	(0.028)	(0.042)
Neighborhood	0.119***	0.073***	0.088***	0.324***
	(0.022)	(0.023)	(0.027)	(0.080)
Fidelis	0.324***	0.137***	0.169***	0.503***
	(0.023)	(0.024)	(0.028)	(0.035)
Health First	0.632***	0.226***	0.270***	0.123**
	(0.032)	(0.032)	(0.037)	(0.059)
Health Plus	0.306***	0.208***	0.249***	0.469***
	(0.023)	(0.024)	(0.029)	(0.028)
Metroplus	0.576***	0.245***	0.298***	0.241***
	(0.021)	(0.025)	(0.030)	(0.049)
Full Controls	Yes	Yes	Yes	Yes
Observations	900,449	900,449	900,449	3,034,035
Joint F-Test	[0.000]	[0.000]	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variable is log spending standardized by repricing all claims at the median price for that service $\times$ provider $\times$ year, a measure of (price-purged) quantity. The independent variables are plan effects relative to the reference plan (Amerigroup). Columns (1), (2), and (3) present specifications for the sample of individuals who were auto-assigned to a plan. Column (1) presents results from an OLS regression of log spending on plan dummies. Column (2) presents results from reduced form regressions of log spending on dummies indicating the plan to which the enrollee was auto-assigned. Column (3) presents IV results estimated using two-stage least squares (2SLS) from the estimating equation (2.13). Column (4) presents results from an OLS regression of log spending on plan dummies for the population of enrollees who actively chose a plan. Controls include baseline spending, five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The active choice sample is randomly drawn from recipients that were not auto-enrolled. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table B.8: *OLS and IV Estimates of Plan Effects on Adj. Log Quantity*

	Auto Assignment Sample			Active Choice Sample
	OLS	RF	2SLS	OLS
Plan Name	(1)	(2)	(3)	(4)
United	0.076***	0.030	0.041	0.565***
	(0.024)	(0.025)	(0.030)	(0.081)
Wellcare	−0.019	−0.016	−0.030	−0.067**
	(0.039)	(0.042)	(0.053)	(0.028)
HIP	0.131***	0.031	0.046*	0.001
	(0.025)	(0.023)	(0.026)	(0.056)
Affinity	0.132***	0.077***	0.093***	0.014
	(0.024)	(0.024)	(0.029)	(0.041)
Neighborhood	0.118***	0.073***	0.087***	0.324***
	(0.022)	(0.023)	(0.027)	(0.079)
Fidelis	0.322***	0.139***	0.169***	0.500***
	(0.023)	(0.024)	(0.028)	(0.035)
Health First	0.619***	0.219***	0.258***	0.118**
	(0.032)	(0.033)	(0.038)	(0.059)
Health Plus	0.311***	0.212***	0.254***	0.458***
	(0.023)	(0.024)	(0.029)	(0.027)
Metroplus	0.483***	0.178***	0.216***	0.161***
	(0.020)	(0.025)	(0.030)	(0.045)
Full Controls	Yes	Yes	Yes	Yes
Observations	887,343	887,343	887,343	3,007,676
Joint F-Test	[0.000]	[0.000]	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variable is adjusted log spending standardized by repricing all claims at the median price for that service $\times$ provider $\times$ year, a measure of (price-purged) quantity. We drop any claims where the median price equals the price paid by a plan because of concerns over sparse service $\times$ provider $\times$ year cells. The independent variables are plan effects relative to the reference plan (Amerigroup). Columns (1), (2), and (3) present specifications for the sample of individuals who were auto-assigned to a plan. Column (1) presents results from an OLS regression of log spending on plan dummies. Column (2) presents results from reduced form regressions of log spending on dummies indicating the plan to which the enrollee was auto-assigned. Column (3) presents IV results estimated using two-stage least squares (2SLS) from the estimating equation (2.13). Column (4) presents results from an OLS regression of log spending on plan dummies for the population of enrollees who actively chose a plan. Controls include baseline spending, five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The active choice sample is randomly drawn from recipients that were not auto-enrolled. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table B.9: *OLS and IV Estimates of Plan Effects on Quantity*

	Auto Assignment Sample			Active Choice Sample
	OLS	RF	2SLS	OLS
Plan Name	(1)	(2)	(3)	(4)
United	-2.329	-3.564	-3.277	57.219^{***}
	(10.467)	(10.244)	(12.156)	(7.436)
Wellcare	3.953	6.270	7.332	-3.209
	(13.202)	(13.114)	(16.725)	(4.705)
HIP	-19.715	-30.888^{***}	-33.210^{**}	6.191
	(12.133)	(11.130)	(13.043)	(6.751)
Affinity	-2.211	-2.572	-3.037	9.417
	(9.097)	(8.835)	(10.677)	(6.531)
Neighborhood	26.294^{**}	14.673	17.730	53.322^{***}
	(11.652)	(11.121)	(13.465)	(9.216)
Fidelis	36.386^{***}	12.863	16.593	60.750^{***}
	(9.417)	(9.053)	(10.721)	(4.779)
Health First	121.644^{***}	53.495^{***}	62.086^{***}	42.664^{***}
	(12.484)	(13.899)	(15.963)	(9.565)
Health Plus	58.669^{***}	34.002^{***}	40.796^{***}	59.187^{***}
	(9.808)	(9.458)	(11.435)	(4.399)
Metroplus	81.705^{***}	30.312^{***}	36.722^{***}	40.400^{***}
	(9.782)	(10.626)	(12.889)	(6.976)
Full Controls	Yes	Yes	Yes	Yes
Observations	900,449	900,449	900,449	3,034,035
Joint F-Test	[0.000]	[0.000]	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variable is spending standardized by repricing all claims at the median price for that service $\times$ provider $\times$ year, a measure of (price-purged) quantity. The independent variables are plan effects relative to the reference plan (Amerigroup). Columns (1), (2), and (3) present specifications for the sample of individuals who were auto-assigned to a plan. Column (1) presents results from an OLS regression of log spending on plan dummies. Column (2) presents results from reduced form regressions of log spending on dummies indicating the plan to which the enrollee was auto-assigned. Column (3) presents IV results estimated using two-stage least squares (2SLS) from the estimating equation (2.13). Column (4) presents results from an OLS regression of log spending on plan dummies for the population of enrollees who actively chose a plan. Controls include baseline spending, five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), zip code fixed effects and county x year x month of assignment fixed effects. All standard errors are clustered on the county x year x month of assignment. The active choice sample is randomly drawn from recipients that were not auto-enrolled. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table B.10: *OLS and IV Estimates of Plan Effects on Adj. Quantity*

	Auto Assignment Sample			Active Choice Sample
	OLS	RF	2SLS	OLS
Plan Name	(1)	(2)	(3)	(4)
United	-2.639	-3.491	-3.282	57.270^{***}
	(10.524)	(10.346)	(12.247)	(7.432)
Wellcare	0.013	3.760	3.961	-4.965
	(13.407)	(13.346)	(17.001)	(4.743)
HIP	-19.787	-31.006^{***}	-33.449^{**}	6.198
	(12.170)	(11.195)	(13.089)	(6.769)
Affinity	-0.351	-0.779	-0.945	9.812
	(9.135)	(8.914)	(10.751)	(6.587)
Neighborhood	26.310^{**}	14.788	17.803	53.519^{***}
	(11.691)	(11.185)	(13.505)	(9.307)
Fidelis	36.640^{***}	13.146	16.795	60.815^{***}
	(9.379)	(9.055)	(10.694)	(4.807)
Health First	124.127^{***}	54.704^{***}	63.285^{***}	43.045^{***}
	(12.507)	(13.917)	(15.962)	(9.641)
Health Plus	59.133^{***}	34.440^{***}	41.210^{***}	59.791^{***}
	(9.802)	(9.489)	(11.442)	(4.414)
Metroplus	80.228^{***}	28.161^{***}	33.996^{***}	37.738^{***}
	(9.896)	(10.764)	(13.037)	(6.986)
Full Controls	Yes	Yes	Yes	Yes
Observations	887,343	887,343	887,343	3,007,676
Joint F-Test	[0.000]	[0.000]	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variable is spending standardized by repricing all claims at the median price for that service $\times$ provider $\times$ year, a measure of (price-purged) quantity. We drop any claims where the median price equals the price paid by a plan because of concerns over sparse service $\times$ provider $\times$ year cells. The independent variables are plan effects relative to the reference plan (Amerigroup). Columns (1), (2), and (3) present specifications for the sample of individuals who were auto-assigned to a plan. Column (1) presents results from an OLS regression of log spending on plan dummies. Column (2) presents results from reduced form regressions of log spending on dummies indicating the plan to which the enrollee was auto-assigned. Column (3) presents IV results estimated using two-stage least squares (2SLS) from the estimating equation (2.13). Column (4) presents results from an OLS regression of log spending on plan dummies for the population of enrollees who actively chose a plan. Controls include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), zip code effects and county x year x month of assignment fixed effects. Standard errors clustered on the county x year x month of assignment. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table B.11: *2SLS Estimates of Plan Impacts on Use of Preventive Care*

	Cervical Screen (1)	Chlamydia Test (2)	Mammo-graphy (3)	Vaccine (4)
United	-0.596	-14.676^{***}	-0.410	-1.397^{***}
	(1.441)	(3.976)	(3.649)	(0.470)
Wellcare	-1.082	-9.570	-1.648	0.212
	(2.660)	(7.276)	(5.169)	(0.826)
HIP	0.116	4.633	-4.322	0.067
	(1.345)	(3.658)	(4.237)	(0.480)
Affinity	0.991	1.846	-1.963	-0.266
	(1.264)	(3.556)	(3.790)	(0.481)
Neighborhood	-1.790	1.632	-1.756	-0.002
	(1.309)	(3.471)	(3.950)	(0.466)
Fidelis	1.267	2.303	-2.020	-0.843^{*}
	(1.198)	(3.450)	(3.792)	(0.432)
Health First	0.352	5.693	2.968	1.599^{**}
	(1.602)	(4.610)	(4.991)	(0.732)
Health Plus	4.819^{***}	15.638^{***}	2.729	0.788
	(1.670)	(4.369)	(4.753)	(0.536)
Metroplus	0.707	5.852^{*}	-8.015^{**}	-0.378
	(1.301)	(3.385)	(3.964)	(0.517)
Full Controls	Yes	Yes	Yes	Yes
Joint F-Test	[0.002]	[0.000]	[0.232]	[0.000]

Notes: Standard errors in parentheses. The dependent variables are various quality measures related to prevention. The independent variables are plan effects relative to the reference plan (Amerigroup). Controls include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), zip code effects and county x year x month of assignment fixed effects. Standard errors clustered on the county x year x month of assignment. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table B.12: *2SLS Estimates of Plan Impacts on Avoidable Hospitalizations*

	Diabetes Compl (1)	COPD/Asthma (40+) (2)	CHF (3)	Asthma (18 to 39) (4)
United	-0.183	0.302	-0.029	-1.378
	(0.147)	(2.314)	(0.202)	(1.028)
Wellcare	0.129	11.823*	-0.038	0.780
	(0.211)	(6.382)	(0.253)	(1.671)
HIP	0.667	10.350***	1.081	12.361***
	(0.539)	(2.992)	(0.794)	(3.047)
Affinity	0.065	7.098*	-0.113	-0.346
	(0.176)	(3.868)	(0.176)	(1.125)
Neighborhood	0.110	4.607	-0.082	1.232
	(0.195)	(4.508)	(0.230)	(1.290)
Fidelis	-0.158	11.933***	0.022	5.849***
	(0.113)	(3.244)	(0.228)	(2.021)
Health First	-0.005	21.084*	0.298	8.402***
	(0.151)	(10.844)	(0.548)	(2.888)
Health Plus	-0.249*	-0.612	-0.095	3.097
	(0.132)	(2.597)	(0.274)	(2.122)
Metroplus	-0.144	18.921***	0.529	6.034***
	(0.115)	(4.978)	(0.524)	(1.767)
Full Controls	Yes	Yes	Yes	Yes
Joint F-Test	[0.210]	[0.000]	[0.731]	[0.000]

Notes: Standard errors in parentheses. The dependent variables are various quality measures related to avoidable hospitalizations. The independent variables are plan effects relative to the reference plan (Amerigroup). Controls include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), zip code effects and county x year x month of assignment fixed effects. Standard errors clustered on the county x year x month of assignment. *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table B.13: *Selection Estimates ($) for Auto Assignees*

	OLS (1)	OLS (2)
United	-0.727	-2.179
	(9.440)	(9.417)
Wellcare	-2.072	-3.387
	(15.352)	(15.506)
HIP	16.413	16.450
	(12.956)	(13.140)
Affinity	4.053	4.698
	(8.809)	(8.752)
Neighborhood	9.456	8.084
	(11.105)	(10.999)
Fidelis	23.239**	21.965**
	(9.771)	(9.655)
Health First	62.223***	57.463***
	(12.879)	(12.724)
Health Plus	20.175**	20.035**
	(10.195)	(10.174)
Metroplus	58.920***	53.792***
	(9.557)	(9.715)
Observations	900,449	900,449
Demographic Controls	No	Yes
Joint F-Test	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variable is a selection residual using dollars that measures the difference between observed spending and predicted spending based on the two-stage least squares (2SLS) from the estimating equation (2.13). The independent variables are plan effects relative to the reference plan (Amerigroup). Columns (1) and (2) present the results of estimating equation (2.16) with and without additional controls (all specifications control for the county x year x month of assignment) on residualized spending. Baseline controls are just the county x year x month of assignment. Additional controls include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), and zip code fixed effects. All standard errors are clustered on the county x year x month of assignment. For per comparison significance for the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Table B.14: *Selection Estimates ($) for Active Choosers*

	OLS (1)	OLS (2)
United	26.317***	17.340**
	(8.927)	(8.699)
Wellcare	-38.554^{***}	-34.336^{***}
	(8.703)	(9.070)
HIP	55.860***	53.244***
	(9.467)	(9.315)
Affinity	21.690***	21.201**
	(8.019)	(8.232)
Neighborhood	47.561***	46.381***
	(9.774)	(9.547)
Fidelis	22.168***	20.793***
	(6.945)	(6.824)
Health First	-3.809	-6.268
	(7.212)	(7.047)
Health Plus	18.752**	12.751*
	(7.978)	(7.649)
Metroplus	18.570**	15.005**
	(7.674)	(7.580)
Observations	960,500	960,474
Demographic Controls	No	Yes
Joint F-Test	[0.000]	[0.000]

Notes: Standard errors in parentheses. The dependent variable is a selection residual using dollars that measures the difference between observed spending and predicted spending based on the two-stage least squares (2SLS) from the estimating equation (2.13). The independent variables are plan effects relative to the reference plan (Amerigroup). Columns (1) and (2) present the results of estimating equation (2.16) with and without additional controls (all specifications control for the county x year x month of assignment) on residualized spending. Baseline controls are just the county x year x month of assignment. Additional controls include five-year age x gender fixed effects, race fixed effects, six-month episode buckets (measuring time since enrollment), and zip code fixed effects. All standard errors are clustered on the county x year x month of assignment. For per comparison significance for the LATE estimates, *** = significant at 1 percent level, ** = significant at 5 percent level, * = significant at 10 percent level.

Appendix C

Appendix to Chapter 3

C.1 Regression Discontinuity Design Specifications

Regression discontinuity design specifications were of the following form:

$$Y_{it} = \beta_1 \text{Age}_t \times \text{Post65}_{it} + \beta_2 \text{Age}_t^2 \times \text{Post65}_{it} + \beta_3 \text{Post65}_{it} + \omega_t + \phi_t + \mu_i + \epsilon_{it} \qquad \text{(C.1)}$$

where i denotes individuals and t age in quarters. Y is the outcome of interest (e.g. spending on imaging services). $\text{Age}_t \times \text{Post65}_{it}$ is a full interaction between an individual's age in quarters and an indicator for having reached age 65 and become eligible for Medicare. $\text{Age}_t^2 \times \text{Post65}_{it}$ is a full interaction between an individual's age in quarters squared and an indicator for having reached age 65 and become eligible for Medicare. Post65_{it} is an indicator that an individuals' age in quarters is at least 65 year olds (260 quarters), and β_3 is the coefficient of interest. We control for age in years, ω_t, age in quarters, ϕ_t, and individual fixed effects, μ_i.

In the regression discontinuity design framework used here, $\text{Age}_t \times \text{Post65}_{it}$ and $\text{Age}_t^2 \times \text{Post65}_{it}$ control for flexible age trends in our outcomes of interest and allow those trends to differ for individuals with and without Medicare coverage. The remaining term, Post65_{it}, identifies any sharp changes in the level of our outcome variable at age 65. Standard errors were clustered by individual and age in quarters to account for serial autocorrelation within individuals and correlation across individuals of the same age in quarters.

C.2 Supplementary Tables and Figures

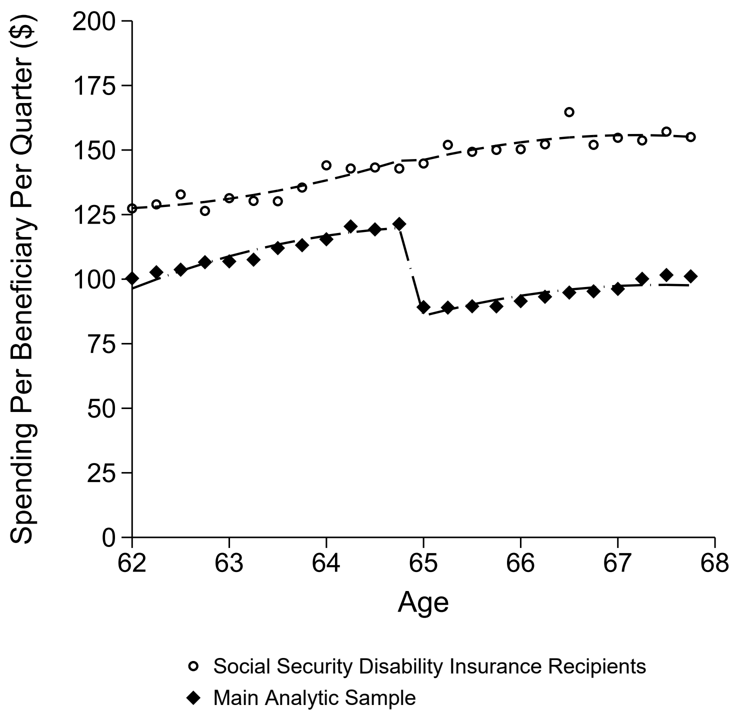

Figure C.1: *Falsification Test: Spending for Beneficiaries with Social Security Insurance*

Notes: Spending per Beneficiary per Quarter ($). Fitted lines are generated from regressions of spending on age and age-squared with a binary variable for age 65 or older. The model allows for different age coefficients in the Medicare and Commercial samples.