WORKING PAPERS

The Effects of Physician and Hospital Integration on Medicare Beneficiaries' Health Outcomes

Thomas G. Koch
Brett W. Wendling
Nathan E. Wilson

WORKING PAPER NO. 337

July 2018

BUREAU OF ECONOMICS
FEDERAL TRADE COMMISSION
WASHINGTON, DC 20580

The effects of physician and hospital integration on Medicare beneficiaries' health outcomes*

Thomas G. Koch
Federal Trade Commission
tkoch@ftc.gov

Brett W. Wendling
Federal Trade Commission
bwendling@ftc.gov

Nathan E. Wilson
Federal Trade Commission
nwilson@ftc.gov

July 23, 2018

Abstract

We consider whether hospital acquisitions of physicians lead to improved clinical outcomes for Medicare patients aged 65 and older. The analysis combines 2005-2012 Medicare fee-for-service and enrollment data with merger and physician affiliation information from the Levin Reports and SK&A, respectively. The analysis uses propensity score matching and a discrete-time hazard model to determine the effect of acquisitions on several health outcomes: mortality, acute myocardial infarctions, acute circulatory conditions, ischemic heart disease, glaucoma, symptomatic diabetes complications, and asymptomatic diabetes complications. These outcomes represent the progression of hypertension and diabetes into worse health states. Our results indicate that hospital acquisitions of existing physician practices have no statistically significant clinical benefits for the health outcomes we consider.

Key words. Integration, Industrial Organization, Physicians, Medicare, Mortality, Diabetes, Hypertension, Health Outcomes

JEL Codes: I11, L23, L40

*We thank Dan Hosken, John McAdams, Luke Olson, Elizabeth Schneirov, Nathan Petek, Devesh Raval, David Schmidt, Bob Town, Christina DePasquale, Brad Shapiro, and audiences at the 2016 ASHE, 2018 ASHE, and 2018 IOS Conferences for helpful comments. We thank Ilana Salant for excellent research assistance. All remaining errors are our own. The views expressed in this article are those of the authors, they do not necessarily represent those of the Federal Trade Commission or any of its Commissioners.

I Introduction

Policy makers and industry participants are exploring ways to integrate healthcare services in order to achieve higher quality care and lower costs. For example, the 2010 Affordable Care Act (ACA) includes provisions that incentivize healthcare providers to integrate through the formation of Accountable Care Organizations (ACOs).[1] In light of these developments, it is perhaps unsurprising that the proportion of U.S. physicians employed by hospitals nearly doubled from 16 to 29 percent between 2007 and 2013 (Kane and Emmons, 2013). Similarly, the percentage of solo physician practices continues a decline that began in the 1980s (Rebitzer and Votruba, 2011).[2]

The trend towards greater vertical integration among healthcare providers may theoretically lead to more efficient care and service, but could also lower incentives to innovate and result in higher prices. This ambiguity has led to a growing body of research that measures the effects of provider integration and seeks to identify the forms of integration that yield the best clinical and financial outcomes (see e.g., McWilliams (2013), Wenke Hwang and Paz (2013), Burns et al. (2013), and Post et al. (2017) for reviews).[3] Despite this research, the effects of integration remain poorly understood. Key research challenges include effective measurement of healthcare quality and provider integration, both of which are necessary to causally attribute health outcomes to integration. Perhaps as a result of these difficulties, policy makers continue to debate the size and nature of integration effects (Gottlieb, 2013, Blankenthorn, 2009).[4]

[1] ACOs are "groups of doctors, hospitals, and other healthcare providers who come together voluntarily to give coordinated care." The center for Medicare and Medicaid Services (CMS) encourages the formation of ACOs by enacting payment and delivery reforms of Medicare and other programs, such as the *Medicare Shared Savings Program* and the *Pioneer ACO Model*. https://innovation.cms.gov/initiatives/aco/.

[2] The increase in the share of integrated systems has occurred due to both new physicians' preference for employment rather than solo practice and the integration of practicing physicians into hospital systems. Burns et al. (2013) provides a detailed characterization of the changes in physician market structure over time. For commentary on the changes, see, *inter alia*, the Advisory Board Company's discussion of recent evidence at http://www.advisory.com/Daily-Briefing/2012/07/10 or Dafny (2013).

[3] Provider integration ranges in form from full employment to complete independence and includes various intermediate contractual relationships.

[4] Firms sometimes motivate possible efficiencies associated with proposed mergers by referencing the

This paper uses a difference-in-differences estimation strategy to measure the effects of 28 physician practice acquisitions by hospitals on a variety of health outcomes related to the treatment of hypertension and diabetes among Medicare patients.[5] Our analysis extends the existing empirical literature on the effects of provider integration on healthcare quality in three ways.

First, we identify changes in integration through physician acquisitions by hospital systems, which sharply alter physicians "status" from independent providers to system employees.[6] Direct employment represents an extreme form of integration. Employers can directly manage clinical practices and financial incentives using strategies that may be unavailable through looser forms of integration.[7] Thus, our integration measure facilitates inference regarding the effect of integration in our difference-in-differences econometric framework, which relies on observing providers change status from one form of integration to another.

Second, we measure health using several direct health outcome measures that represent the progression of either diabetes or hypertension into a worse health state: mortality, acute circulatory conditions, acute myocardial infarction (AMI), ischemic heart disease, glaucoma, and diabetes complications. We consider diabetes and hypertension since they are prevalent and treatable. Medical science has correlated the outcomes that we consider with the progression of these conditions, and providers track these outcomes to manage a patient's health progression.[8] As such, these outcome measures are direct and prevalent measures of

potential for large efficiencies before the antitrust authorities and the Courts. In the proposed acquisition of the Saltzer medical group by St. Luke's health system, the merging parties claimed that any lost competition attributable to the merger would be offset by efficiencies associated with provider integration. http://www.ag.idaho.gov/consumerProtection/pendingActions/StLukesFTC&IdahoBrief.pdf

[5]Some researchers find that physician-owned practices are more likely to participate in ACOs than physician practices in hospital-based systems (Casalino et al., 2014).

[6]Health services researchers are also interested in the effects of other types of "clinical integration," which is any form of provider coordination that occurs independently of financial integration. Clinical integration may include full financial integration, but also includes other forms of provider coordination that does not necessitate formal mergers and acquisitions. See, e.g., http://www.thecamdengroup.com/thought-leadership/blog/clinical-integration-an-overview/.

[7]For example, firms may offer financial incentives to comply with clinical practice standards.

[8]For example, the 33 Accountable Care Organization quality performance measures include "Percent of beneficiaries with diabetes whose HbA1c in poor control (>9 percent)" and "Percent of beneficiaries with hypertension whose BP $<$ 140/90." For more information, see https://www.cms.gov/Medicare/Medicare-

disease progression into severe outcomes for important chronic conditions.

The extensive use of mortality by the healthcare quality literature implicitly validates our set of health metrics. While we, too, consider mortality as an outcome of interest, we do not limit our analysis to it because it is only rarely observed and may be weakly related to ambulatory care. The prior literature acknowledges both the benefits and concerns of mortality as an analytical measure and addresses these issues by considering mortality alongside alternative outcomes. The literature extensively uses hospital utilization metrics, such as ER visits and readmissions, as alternative outcomes (see Post et al. (2017)) since they are more prevalent than mortality and are likely related to firm quality outcomes such as health and healthcare costs. However, hospital utilization measures are only indirect measures of health since they measure the services used to treat a diagnosed medical condition rather than the condition itself. In some settings, this is a desired attribute since the metric implicitly captures both clinical health and resource intensity. However, in our setting, this attribute threatens our identification. We want to isolate the clinical benefits associated with integration. Medicare provider-based billing (PBB) policies may interact with acquisitions to change the financial incentives for choosing whether to treat patients in a hospital or an office (Koch et al., 2017). If so, these measures could confound the clinical benefits associated with acquisitions with the changed financial incentives they also cause. Therefore, we employ a set of outcome measures that are severe and verifiable, similar to mortality, but are more prevalent and more closely related to ambulatory care, similar to hospital utilization. However, unlike hospital utilization, our outcomes are independent of PBB and do not suffer from the potential conflation of interpretations that those metrics may invite.

Third, we employ propensity score matching techniques to identify a valid control group of similar patients who visit non-acquired providers based on observable demographic, utilization, health, and provider characteristics. This more careful approach to identification reduces the possibility that our results are driven by a failure to control for unobservable

Fee-for-Service-Payment/sharedsavingsprogram/Quality_Measures_Standards.html

confounders or selection bias.

Overall, our analyses find that vertical integration rarely leads to better health outcomes, and sometimes results in worse outcomes. We typically find negligible average effects across acquisitions that do not change much in the several years following an acquisition.[9] These findings are robust, as we do not find evidence of improved health in any of our specifications or with any of our outcome measures. Instead, our results indicate that vertical integration is not associated with improvements in health, despite the fact that the literature has found it to be associated with increased expenditures (Koch et al., 2017, Capps et al., 2015, Baker et al., 2014a).

The rest of the paper is organized as follows: Section II reviews the existing literature on the relationship between acquisitions and health outcomes. Section III describes our data. Section IV presents our empirical strategy for estimating acquisition effects and Section V provides motivating summary and balance statistics. Section VI discusses our findings. We conclude in Section VII.

II Background and literature review

Economic theory suggests that integration effects are ambiguous and may depend upon on a variety of conditions including agency concerns (Cooper et al., 2005), transactions costs (Bresnahan and Levin, 2012), information asymmetries (Wolinsky, 1993, Afendulis and Kessler, 2007), and competitive incentives (Gaynor and Vogt, 2000, Whinston, 2006). Since integration may be welfare increasing or decreasing, determination of its net effects requires empirical assessment. However, empirical assessment depends crucially on the valid measurement of both integration and outcome measurement. Integration takes many forms that vary in degree and scope and may affect a variety of outcomes including quality, costs, prices, and output.

[9] We focus on the average effect of a vertical merger and not the effect of the average merger. As discussed in recent work by Gibbons et al. (2014), these are not necessarily the same.

The empirical literature considers the effects of a variety of integration forms on numerous outcomes. An old literature considers the relationship between firm size, a form of integration, and outcomes such as costs and quality. Some studies find increasing returns, whereas others do not.[10] A related literature considers the effects of provider concentration on economic and clinical outcomes and generally finds that higher concentration leads to higher costs (Cooper et al., 2015) and weakly negative health effects (see, e.g., Gaynor and Town (2012) and Koch et al. (2018)).

A growing literature considers the effects of integration between hospitals and physicians on costs, prices, and utilization (Neprash et al., 2015, Keating et al., 2004, Burns and Muller, 2008, Baker et al., 2014a, 2016, Cuellar and Gertler, 2006, Ciliberto and Dranove, 2006, Capps et al., 2015). A related literature evaluates the performance of ACOs on cost and quality outcomes. Although this literature generally finds that hospital and physician integration result in higher prices or increased costs, Ciliberto and Dranove (2006) finds that integration between hospitals and physicians does not lead to higher charges.

Our analysis is most closely related to the literature that considers the relationship between hospital and physician integration on the quality of care, broadly defined (see Post et al. (2017) for a comprehensive review). However, some of the literature expresses concern that existing evidence has not determined the causal relationship between provider integration and quality since it frequently relies on cross-sectional relationships between the variables of interest. A survey by Burns et al. (2013) suggests that the existing literature has not settled the debate about whether integration of provider services increases welfare.[11]

By bringing clear measures of changes in integration status together with rich, well-powered measures of health outcomes, we hope to provide new clarity on the relationship

[10] See, for example, Kimbell and Lorant (1977), Gaynor and Pauly (1990), Hillson et al. (1992), Hough (2001), Kralewski et al. (1999), Hough et al. (2011), Casalino et al. (2014), and Ketcham et al. (2007). See, also, Medpac's March 2012 Report to the Congress.

[11] The difficulty of estimating the impact of form is not specific to healthcare. Mullainathan and Scharfstein (2001) discusses that the causal effect of firm structure is unknown except for a small literature focusing carefully on identification issues (Novak and Stern, 2008, Forbes and Lederman, 2010, Kosova et al., 2013, Wilson, 2015).

between vertical integration and health outcomes.

III Data

Our analysis combines ambulatory and hospital claims from Medicare during the period 2005-2012 with provider acquisitions identified using data from SK&A and the Levin Health Care Acquisition Reports (Levin Reports).[12] We describe these data below.

III.1 Provider acquisitions

The Levin Reports are annual lists of mergers and acquisitions in the healthcare sector compiled by *Irving Levin Associates*, a private company. The lists are generated from public announcements, SEC filings, and interviews with industry management. The Levin Reports provide information for each transaction regarding the parties involved (i.e., firm names), the announcement date, and (sometimes) the closing date.[13] We use this information to generate a list of 28 provider acquisitions that occurred between the third quarter of 2005 and the second quarter of 2010. All 28 transactions involve a physician group and a hospital, where the hospital typically acquires the physician group. These transactions are identical to the ones considered in Koch et al. (2017), which finds that these transactions led to increased expenditures and a greater number of services billed through the hospital.[14]

Unfortunately, the Levin Reports do not report the identity of the physicians employed by the firms affected by acquisitions. In order to determine the identity of those physicians, we combine the list of acquired practices from the Levin Reports with information about a physician's group affiliation from SK&A.[15] SK&A has person-level information on provider

[12] Throughout the rest of the document we refer to transactions as provider acquisitions, although the transaction may involve a physician group acquiring a hospital. The distinction does not matter for the analysis or its interpretation.

[13] See http://www.levinassociates.com/hardescription.

[14] Although some of our analyses consider an acquisition involving a children's hospital and a women's clinic not considered by Koch et al. (2017), this acquisition contributes few observations to our analysis.

[15] We merge the data together using firm name and address.

affiliation for a near census of physician groups in the United States. SK&A provides the National Provider Identifier (NPI) and the Unique Physician Identifier Number (UPIN) code for the members of each physician group. These identifier codes are person-level identifiers that uniquely identify a single provider.[16] SK&A collects this information through biannual surveys of registered physicians. Although the timing of the SK&A data collection process does not facilitate the identification of mergers and acquisitions, the detail SK&A provides regarding physician identity is useful in constructing metrics of industry structure. For example, Dunn and Shapiro (2014) and Baker et al. (2014b) use these data to associate physicians with firms for measures of physician-market concentration.

We combine the SK&A physician-level information with the Levin Report transaction data to identify physicians affected by transactions involving a hospital-provider system. Overall, matching SK&A data to the Levin Reports yields 1,485 individual providers involved in 28 group acquisitions.[17] Table A-4 in Appendix A details the names of the target and acquiring firms, the number of hospitals involved, the number of physicians employed by the target, and the transaction date. We also list whether the acquired practice is a cardiologist group (Card), a multi-specialty group (Multi), a group of surgeons (Surg), or another specialist group (e.g., Family practice, Obstetrician/Gynecologist, or Internal medicine). We combine this provider information with the Medicare claims data to perform our analysis of quality effects.

[16] See http://www.skainfo.com/.

[17] The physician count includes only those physicians identified in SK&A, and does not include physicians associated with the same TAXID within the Medicare claims data as an acquired physician. Some acquired physicians identified within the SK&A data perform few services in our Medicare sample since their specialties treat younger patients. For example, some acquired multi-specialty groups employ pediatricians and obstetricians who serve children and pregnant women, respectively.

III.2 Medicare claims

We combine acquisition information with Medicare healthcare claims from a 5% sample of Medicare beneficiaries during the period 2005 to 2012.[18] The 5% sample of Medicare beneficiaries represents a census of beneficiary claims from inpatient admissions, hospital outpatient visits, and office-based visits for approximately 2.5 million persons per year.

The claims data contain detailed patient, provider, and service information. Patient information includes 5-digit International Classification of Disease (ICD-9) diagnosis codes and basic demographic information such as the age, sex, and 9-digit postal ZIP code for each Medicare member. Provider information includes facility identifiers for hospitals and person-level NPI and UPIN identifiers for individual practitioners.[19] The data distinguish between physician and non-physician individual providers and indicate the "specialty" for physicians and non-physicians. The "Carrier files," which represent office-based Medicare claims, also identify the the tax identification number (TAXID) of individual providers which serves as a coarse measure of group affiliation. All claim information includes the date associated with the "event," or service, as well as the ZIP code and state of the location.

We identify Medicare claims involving an acquired physician group using the set of physicians identified from the Levin Reports and SK&A data. We match the list of acquired physicians to the Medicare claims data using the NPI/UPIN provider identifiers listed in both sets of data. For providers that match, we identify firm affiliation and acquisition timing from the Levin Reports and SK&A data. We use the TAXID to identify firm affiliation for providers that we cannot match to the Levin Reports and SK&A data (and are thus defined as not being involved in acquisition transactions).

[18] The sample represents 5% of Medicare recipients. The sample does not address selection associated with Medicare Advantage plans, which compete with traditional Medicare. Unfortunately, claims for Medicare Advantage plans, and privately-insured plans more generally, are not widely available. We use the 2005 data to construct health conditions of patients (e.g., hypertension and diabetes), but perform the principal analysis using the 2006-2012 data.

[19] Medicare has transitioned from tracking physicians by UPIN at the beginning of the sample to tracking physicians by NPI at the end of the sample.

We associate patients with the characteristics of their geographic area of residence using Area Resource Files (ARF) from 2009 and 2015. ARF include indices that measure the urbanicity of the county and the proximity of the county to a hospital. We associate the relevant claim quarter with the most recent "nearest" year of the ARF.[20]

We aggregate the resultant claims data to the patient-quarter and track all of the provider groups that the patient visited during the quarter, as identified using the TAXID.[21] Patients that are not observed with claims during the quarter are assigned the values of relevant time-varying characteristics, such as ICD-9 chapter conditions, assigned to the previously observed quarters.[22] We define a patient-quarter as involving an acquisition if any of the claims during the quarter involve acquired or acquiring physicians.[23] Similarly, we define the patient-quarter as post-acquisition if the contemporaneous quarter of the claim occurred following the acquisition date of the acquired physician group.

III.3 Health conditions and outcomes

We consider health outcomes that enable us to measure the effects of physician acquisitions on provider treatment for diabetes and hypertension. In addition to mortality, we construct several outcome measures from the 5-digit ICD-9 code listed on patient claims. The specificity associated with a 5-digit ICD-9 code allows us to identify the progression of relatively mild

[20] For example, we assign 2009 values to the years 2009, 2010, and 2011 for variables that change in 2009 and 2015. We assign 2015 values to 2012 since 2015 is the most recent "nearest" year.

[21] Some patients see more than 70 groups within a quarter, but fewer than 10 percent of patient quarters involve visits to more than two groups.

[22] We observe beneficiaries' birth dates and death dates regardless of whether the patient is observed with a claim during the quarter. As described above, many demographic characteristics of the individual are assigned in 2005, which is not included in our analysis sample. We assume that patients who are first observed in the claims data after March 2006 entered the sample during the period in which they are first observed.

[23] We exclude from our analysis the claims associated with physicians that exclusively bill through the hospital since provider characteristics for those physicians are excluded from the Carrier files. This procedure produces a provider set of physicians that are disproportionately represented by providers who are independent of hospital systems or who are acquired by hospital systems during the sample period. We argue that this group of providers is the appropriate control group for our analysis. However, our analysis does include physicians that bill both from an office and through the hospital. For those physicians we observe all claims, including claims billed through the hospital.

chronic conditions into more severe outcomes related to that condition.

We believe our most credible measures relate to diabetes. Table A-3 provides the descriptions for the ICD-9 codes that we use as diabetes outcome metrics, 250.00-250.93. In our analysis, and in Table A-3, we categorize the ICD-9 codes as either "symptomatic" or "asymptomatic."[24] These codes explicitly identify conditions that are related to the progression of diabetes. For example, the description for 250.10-250.13 is not simply "ketoacidosis," but rather "diabetes with ketoacidosis." In addition to providing a relationship between the outcome and the underlying chronic condition, we note that the existence of the code suggests that providers monitor this complication in order to assess the progression of diabetes. Indeed, the Agency for Healthcare Research and Quality (AHRQ) uses readmissions associated with these ICD-9 codes as quality measures in other applications. Overall, the explicit relationship between the underlying chronic condition and the complication that follows from it is ideal for relating health outcomes to changes in treatments that arise from changes in provider ownership.

We also use the ICD-9 diagnosis codes to identify health outcomes related hypertension progression. We consider "any" acute cardiac condition, which we define as 5-digit ICD-9 diagnosis codes that are not defined as "chronic" by the National Household Interview Survey (NHIS) and are in the "conditions of the circulatory system" chapter heading of the ICD-9 code list.[25] We replicate the NHIS table of chronic conditions as Table A-7. We identify patients as having ischemic heart disease and heart attacks, or acute myocardial infarctions (AMI), using the beneficiary summary files. We also use the summary files to identify whether a beneficiary died.

The validity of all of our health outcome metrics rely on accurate, detailed, and consistent coding by the healthcare claims processors. If claims reporting is strategic or even correlated

[24]We categorize the conditions based on the descriptions. "Symptomatic" complications have descriptions that suggest that the patient likely observes the condition (e.g., blindness). The "asymptomatic" conditions typically relate to the patient's blood sugar levels, but may be unknown to patient monitoring their bloodwork.

[25]The relevant ICD-9 codes in the chapter covering circulatory conditions range from 390-459.

with unobserved determinants of firm size or profitability, then these metrics may bias our analysis. For example, some of our metrics rely on physician diagnoses. If an acquisition results in increased monitoring and thus more diagnoses, our health outcome measure might suggest that acquisitions result in worse health when, in fact, the acquisition resulted in better monitoring and more diagnoses.

To address these potential issues, we consider a set of conditions with different strengths and weaknesses. In particular, mortality and acute myocardial infarction (AMI) represent severe outcomes with obvious symptoms that are unlikely to be coded inconsistently across claims processors. Moreover, these outcomes are unlikely to be disproportionately observed as a result of increased physician monitoring.[26] These conditions have also been used extensively by the previous literature in a variety of contexts, effectively validating their use.

We treat all of our health conditions as absorbing states for the beneficiary. Individuals observed with a health outcome are defined as having the condition for the rest of the sample period regardless of whether the contemporaneous period contains a claim with the ICD-9 diagnosis listed. We impose this restriction for all health outcomes, including acute conditions. In addition, we omit data from 2005 from our analysis, limiting the analysis to the period 2006 - 2012, so that we can have an entire year of observed claims to determine the health conditions and (potentially) outcomes of beneficiaries observed in 2006.

IV Econometric Approach

When using non-experimental data to identify the impact of an event, the possible confounding factors associated both with the incident and the outcomes of interest are important concerns. In our application, Table 1 reveals important differences in the means of relevant covariates, such as firm size, across the samples of patients of acquired and non-acquired

[26] We argue that our "symptomatic" diabetes metric has a similar property since it involves symptoms obvious to the patient, such as vision impairment or tingling in the extremities.

physicians in our data.[27] To address concerns related to these differences, our principal econometric specification employs propensity score matching techniques to identify a set of relevant control-group patients. We then use these matched pairs in a fixed-effects discrete time hazard model to consider the relationship between health outcomes and physician acquisitions.

IV.1 Propensity score matching

Matching methods are useful for measuring average "treatment" effects, such as the acquisition effect in our application, if covariate distributions differ substantially by "treatment" status (i.e., "acquisition" status).[28] As is evident in Table 1, the Medicare data is a good candidate for a matching procedure. We have a large set of potential control-group patients, and the samples of patients treated by acquired and non-acquired physicians differ substantially along firm size. We use propensity score matching to ensure that we have a good control group for the set of patients treated by acquired physicians.

We match patients that visit acquired physicians to patients that visit non-acquired physicians using a single nearest neighbor propensity score match without replacement within an exact match. We define the exact match categories using the combination of patient sex-patient birth cohort-physician specialty.[29] For example, one of our "exact-match" categories includes a female that is born in 1935 and visits a family practice physician. We select potential matches for each female beneficiary born in 1935 that visits an acquired family practice physician from the set of potential control female beneficiaries that were also born in 1935 and who also visited a family practice physician - but a physician not acquired during the sample period. The physician specialty that defines the exact-match category is refined such that we have different categories for patients that visit any combination of

[27] The samples also differ with respect to age and race, although to a much smaller extent.

[28] Henceforth, we use "acquisition" effects to refer to the "treatment" effects discussed in the econometric literature. We distinguish acquisition effects from treatment effects since clinical "treatments" have a role in our discussion of the results, and thus create a potential source of confusion.

[29] Physician specialty is determined by the combination of specialties visited by the patient.

specialties. For example, we have separate categories for beneficiaries that visit a family practice physician, beneficiaries that visit a cardiologist, and beneficiaries that visit both a family practice physician and a cardiologist. Within the defined exact match category, the selected patient match is determined by the potential control beneficiary with the propensity score "nearest" to the propensity score of the relevant beneficiary that visits an acquired physician. Ties favor acquired beneficiaries with higher propensity scores.

We define beneficiaries who are patients of multiple physicians as patients of an acquired physician if any of the visited physicians were acquired during sample period.[30] For example, if a patient visits a cardiologist acquired during the sample period and also visits a family practice physician that remains independent, we define the beneficiary as the patient of an "acquired" physician. In this way, potential control-group beneficiaries never visit an acquired physician at any time during the sample period.

We estimate the propensity score using a probability model that relates "acquisition" group characteristics to potential control-group characteristics using provider characteristics, d, group characteristics, g, and patient characteristics, i. The following person-level equation represents the probability equation that we use to match patients:

$$Pr(A_i = 1|X; \Theta_A) = f(\alpha + \beta_i + X_i^i \beta_d X_i^d + \beta_g X_i^g + \epsilon_i), \tag{1}$$

where X_i^i are patient characteristics, X_i^d are provider characteristics, and X_i^g are the physician's group characteristics.

We account for patient heterogeneity, X_i^i, using the patient's age at the beginning of the sample (i.e., their birth cohort), the race, sex, health conditions, and urbanicity characteristics of the patients seeking care from the provider during the sample period.[31] Patient health

[30] In addition, the timing of mergers are set such that patients who visit multiple acquired groups are associated with the group that was acquired first.

[31] State is determined by the location of the claim as defined by Medicare. State is interacted with urbanicity, group size, and proximity to hospitals. Age is binned into the following categories: 65-70, 71-75, 76-80, 81-85, 86-90, > 90. Race is defined as non-Hispanic white, black, and other.

conditions are measured using indicators for 18 chronic conditions, defined as any chronic condition in each of the major ICD-9 chapter heading categories.[32] We associate patient demographic conditions that change during the sample, such as urbanicity and proximity to a hospital, with the values observed at the beginning of the sample. We measure urbanicity and proximity to a hospital using the 2009 and 2015 Area Health Resource Files (AHRF) and associate with the patient's county of residence.[33] We associate patient with the health conditions observed at the end of the sample period (i.e., 4Q2012) to ensure that patient matches are treated for the same conditions observed in our sample of acquired patients.

For provider characteristics, X_i^d, we include state fixed-effects and 24 discrete and non-mutually exclusive specialty categories of the physician. For patients that visit multiple physicians, we associate the patient with all of the relevant criteria such that each of the categorical variables are not mutually exclusive.[34]

Finally, we control for variation in physician group characteristics, X_i^g, using the size of the practice as measured by the number of providers with the same TAXID.[35] We interact firm size with the hospital proximity measures and urbanicity. We also interact urbanicity with state fixed-effects and the hospital proximity measures. Again, we associate beneficiaries that visit multiple physicians with all of the relevant practice variables for those physicians.[36]

Equation (1) is used to estimate the probability of whether the patient's provider is

[32] We produce the ICD-9 chapter headings as Table A-1.

[33] Urbanicity takes on four values: a county in a metro area with $>$ 1 million people, a metro area with 500,000 - 1 million, a metro area with 250,000 - 500,000 people, and a non-metro area. Urbanicity also includes a separate measure of population density that enters linearly.

[34] For example, if a beneficiary visits two different providers both located in New York, the patient is associated with a single dummy variable for New York. However, if a beneficiary visits a provider in New York and a second provider in New Jersey, we associate the patient with providers in both New York and New Jersey (i.e., both the New York and the New Jersey dummy variables are set to one for the patient).

[35] We use the minimum firm size observed during the period, and bin the sizes into 5 categories: $<$ 5, 5-24, 25-49, 50-99,100-199, $>$=200. We choose the minimum since it likely better reflects the size of the acquired group at the time of the acquisition.

[36] For example, if a beneficiary visits two small practices (i.e., $<$ 5 physicians), then we associate the beneficiary with a small physician practice. If, however, the beneficiary visits two physicians, one at a small practice and another at a large practice, then we associate the beneficiary with both the large and the small practice. However, both the large and the small practice are defined as such using the minimum number of providers at the firm during the sample period.

acquired at some point during the sample period, which we represent as $A = 1$. We assume a logistic functional form for the acquisition probability. The predicted probability from the logit conditional on the estimated parameters and observed characteristics provides the propensity score, $e(x)$, for each Medicare beneficiary in the sample. The estimated propensity score takes the form:

$$\hat{e}(x) = \frac{exp(\hat{\alpha} + \hat{\beta}_d X_i^d + \hat{\beta}_g X_i^g + \hat{\beta}_i X_i^i)}{1 + exp(\hat{\alpha} + \hat{\beta}_d X_i^d + \hat{\beta}_g X_i^d + \hat{\beta}_i X_i^i)}. \tag{2}$$

We use the estimated propensity score, $\hat{e}(x)$, evaluated at the estimated parameter values, $\hat{\beta}$, and conditional on the observed patient and provider characteristics to construct the log-odds ratio for every beneficiary in the sample:

$$\hat{r}(x) = \ln(\frac{\hat{e}(x)}{1 - \hat{e}(x)}). \tag{3}$$

Next, we match the patients of acquired physicians to potential control-group patients of non-acquired physicians using the minimum absolute value of simple differences between the log-odds ratios of patients having the same cohort-sex-specialty groupings, without replacement. If we index the patients of acquired physicians within a cohort-sex-specialty group by $i = 1, ..., N_a$, where a represents a patient of an acquired physician with a matched cohort-sex-specialty and $\hat{r}(X_i) \geq \hat{r}(X_{i+1})$, then the index of the matched control patient within a cohort-sex-specialty, $j(1)$, is represented as:

$$j(1) = arg \min_{i:A=0} |\hat{r}(X_i) - \hat{r}(X_1)|. \tag{4}$$

We proceed iteratively using this procedure until each of the N_a patients of acquired physicians within an cohort-sex-specialty are matched. We perform this procedure for each of the cohort-sex-specialty groups observed in our sample of patients choosing acquired physicians. The result is N_A pairs of matched patients, where A represents the full sample

of patients choosing acquired physicians across all age-sex-specialty groups. The resulting sample is the set of patients that visit an acquired physician and each of the matched patients of non-acquired physicians. Our analysis sample is a quarterly dataset that follows each of the treated patients and their matched pair over time.

IV.2 Health outcome "acquisition" effects

We begin implementation of our analysis of "acquisition" effects using a simple difference-in-differences among the sample constructed from our propensity score analysis. Our primary specification estimates health outcome acquisition effects using a discrete-time hazard model from the matched estimation sample. The logit regression controls for a rich set of patient demographic characteristics, provider characteristics, and quarter dummies during the period 2006-2012. Our preferred specification also includes controls for the patient's health. We represent this specification with the following equation:

$$Pr(h_{it} = 1|X_{it}, \Theta) = \Lambda(\theta_{PM} PM_{it} + \theta_{fPM}(fem_i \cdot PM_{it}) + \beta_M M_i + \beta_{fM}(fem_i \cdot M_i) + \beta_i X_{it} + \beta_y yr_{it} + \beta_{M*y}(M_i \cdot yr_{it}) + \delta_{ZIP} + \delta_t + u_{it}). \quad (5)$$

The discrete time hazard model considers the probability that we observe a positive realization of our health outcome variable in the contemporaneous period, $h_{it} = 1$. Since our health outcomes represent adverse health conditions, we interpret negative coefficients as improving health and positive coefficient estimates as worsening health. The discrete choice hazard model specifies a logit probability (i.e., $\Lambda(\cdot) = exp(\cdot)/(1 + exp(\cdot))$) for patient i in quarter t, conditional on characteristics X and estimable paramaters Θ. Each health outcome that we consider (i.e., acute cardiac conditions, AMI, death, diabetes complications, glaucoma, and ischemic heart disease) is a discrete indicator that specifies whether the patient is observed with the outcome during the quarter, or not. We model each outcome, separately. As is standard for discrete time hazard models, health outcomes are defined as absorbing

states. Patients are assigned a condition for the duration of the sample period following its first observance for all conditions.[37]

The effect of interest in equation (5) is the post acquisition coefficient, θ_{PM}, which we interact with sex to allow different outcomes for male and female patients. We report these coefficients, separately, alongside "marginal treatment effects" on the treated for a specified beneficiary. We associate a beneficiary-quarter with an acquired group if any physician visited by the beneficiary during the quarter is a member of an acquired group. The variable PM effectively represents the interaction of the acquisition indicator, M, with an indicator that is equal to one if the contemporaneous quarter follows the acquisition for patients of acquired physicians.[38] We interpret the coefficients as the clinical benefits (or harm) attributable to the acquisition.

The model controls for patient and provider characteristics, X_{it}, quarter-year fixed effects, δ_t, and 3-digit ZIP code fixed effects, δ_{ZIP}, which are similar to a county but better account for population.[39] The demographic controls include ICD-9 major category chronic condition indicators, sex, age fixed effects, and interactions between sex and 7 age categories.[40] The specification includes separate indicators for each age up to 95, and an indicator for whether the patient is older than 95. For most of our health outcomes we limit the sample to patients with diabetes or hypertension, as identified using the ICD-9 indicators in the contemporaneous quarter.[41] The provider controls include indicators for the seven most prevalent physician specialties in our sample, including family medicine, as well as an indicator for whether the

[37] We drop all observations following realization of the modeled health outcome. However, we also treat the control chronic conditions as absorbing states that persist until the outcome variable is observed.

[38] As described in the data section, we use the closing dates to determine the acquisition timing, when available, but use the announcement dates when the closing date is unavailable. The implied omitted period is the observed period prior to the acquisition. The corresponding post-acquisition interaction for patients of non-acquired physicians is always zero and thus not included.

[39] ZIP codes are geographically smaller in densely populated areas and larger in less dense areas. In each specification, we pool ZIP codes with less than 1250 observations. For each of those ZIP codes, we control for the state of the patient's residence.

[40] The age categories are 65-69, 70-74, 75-79, 80-84, 85-89, 90-94, 95+.

[41] We limit the sample to patients with hypertension when we consider the acute heart conditions, ischemic heart disease, and AMI health outcomes. We limit the sample to patients with diabetes when we consider the two diabetes complications and glaucoma. We consider mortality among the entire sample population.

beneficiary visited another specialist not included in the list of the most frequently observed specialties in our data.[42] Our preferred specification allows for an acquisition-specific time trend to differ from the jointly estimated quarter-year fixed effects.[43]

We do not control for matched-pairs in equation (5), and thus do not take full advantage of the propensity score matching procedure. Rather, we limit the full sample to a "trimmed" sample that is more balanced in the covariates than the original full sample, but which may have some remaining bias. However, estimating this relatively parsimonious specification allows us to recover marginal effect estimates since estimation of equation (5) does not require controlling for or recovering fixed effect estimates. In the next specification we do account for the matched sample procedure by replacing controls for ZIP code with fixed-effects for matched pairs:

$$Pr(h_{it} = 1|X_{it}, \Theta) = \Lambda(\theta_{PM} PM_{it} + \theta_{fPM}(fem_i \cdot PM_{it}) + \beta_M M_i + \beta_{fM}(fem_i \cdot M_i) + \beta_i X_{it} + \beta_y yr_{it} + \beta_{M*y}(M_i \cdot yr_{it}) + \delta_{match} + \delta_t + u_{it}). \quad (6)$$

Equation (6) (i.e., our "matching estimator") takes full advantage of the nearest-neighbor matching procedure through the inclusion of matched pair fixed-effects, δ_{match} . These fixed-effects represent separate indicators for whether each of the benefiaries is a member of a specific matched-pair identified using the nearest neighbor matching procedure. There are, conceptually, $N_A - 1$ of these δ_{match} indicators, one for each matched acquisition and control beneficiary pair. We use the procedure outlined in Chamberlain (1980) to account for the fixed-effects in the estimation of the post-merger effects. Implementation of the Chamberlain (1980) procedure effectively takes differences of h and X within each matched pair to identify the relevant coefficients, similar to a simple matching estimator. The Chamberlain (1980) procedure results in consistent estimates of the relevant coefficients and is more flexible than

[42] We define a specialist as a family medicine if they are defined as general practice, family practice, or internal medicine. We also define visits to physician assistants as family medicine.

[43] We do not include this variable in our more limited specifications.

simple matching estimators since it also controls for changes in time-varying covariates and other characteristic differences between matched beneficiaries. Indeed, Imbens (2015) does not recommend simply estimating the average treatment effect for the treated by differencing average outcomes between groups in this sample. Unfortunately, the Chamberlain (1980) differencing procedure does not allow for the recovery of marginal effects, which results in interpretation difficulties. In order to interpret the estimates from the "fixed-effects matching estimator," we compare the resultant difference-in-differences coefficient estimates from this analysis to that of the more parsimonious equation, equation (5). The comparison enables us to determine whether controlling for matched-pair fixed effects attenuates or otherwise changes the coefficient estimates relative to the more parsimonious equation.

In all of these specifications, causal interpretation requires that the standard difference-in-difference assumptions hold. In our setting, these assumptions imply, among other considerations, that changes in acquired providers treatment practices follow similar paths over time as changes in non-acquired providers treatment practices, conditional on observables. In particular, we require that changes in referral patterns, patient demographics or other firm practices that change in response to integration status do not also change the outcomes of the patient population.[44]

In addition to equation (5), we consider alternative specifications that estimate differential acquisition effects across time and acquisitions. We estimate separate effects for each acquisition to allow for different effects across acquisitions, which we represent in the following specification equation (7):

$$Pr(h_{it} = 1|X_{it}, \Theta) = \Lambda(\sum_{g \in G} \theta_p^g Post_{it}^g + \beta_x X_{it} + \beta_M M_i + \beta_y yr_{it} + \beta_{M*y}(M_i \cdot yr_{it}) + \delta_{ZIP} + \delta_t + u_{it}). \quad (7)$$

[44] As we note in the data section, some might have particular concerns regarding changes in coding practices resulting in violations of the relevant identification assumptions. However, the lack of any significant pre-trends in Figure 4-Figure 6 suggests that these assumptions likely hold for most of the health outcome measures that we consider.

Equation (7) allows the post-acquisition effect to vary by each of the G acquired groups. However, we pool some smaller acquisitions together and estimate a single effect for them. We also do not estimate separate acquisition effects for men and women, although we continue to control for separate age effects for men and women.[45]

In our third specification, we allow the acquisition effects to depend on the time since the acquisition occurred, including periods prior to consummation:

$$Pr(h_{it} = 1|X_{it}, \Theta) = \Lambda(\sum_{l=-15}^{15} \theta_l l_{it} + \sum_{l=-15}^{15} \theta_{fl}(l_{it} \cdot fem_i) + \beta_M M_i + \beta_{fM}(fem_i \cdot M_i) + \beta_x X_{it} + \beta_y yr_{it} + \beta_{M*y}(M_i \cdot yr_{it}) + \delta_{m(i)} + \delta_t + u_{it}). \quad (8)$$

Equation (8) is nearly identical to the primary specification represented as equation (5), except that we allow the effect of the acquisition to vary by the time since consummation, l_{it}. Each l_{it} is a dummy variable that is equal to one if the contemporaneous quarter was l months since the earliest observed acquisition, and zero otherwise.[46] We omit the period of the acquisition, and set all acquisition variables to zero if the relevant group did not experience an acquisition during our sample period. The data enable identification of quarter-specific pre- and post-merger effects for quarters more than three years before and after acquisitions. We think of the time profile drawn from the quarter-specific pre- and post-merger coefficient estimates as providing the relevant information for an event-study. We cautiously use the quarter-specific post-merger coefficient estimates to determine whether merger benefits take time to become realized.

We estimate all of our model specifications using the same data sample, which we describe in the previous section.[47] The models are estimated using a weighted logit procedure

[45] Due to few occurrences of death in our data, we note that all of our mortality specifications are somewhat less flexible than our other specifications. In all of our mortality specifications, the primary age effects are estimated for the seven age categories, and we do not allow for a acquisition-specific time trend.

[46] We represent periods prior to an acquisition as negative values in the summation.

[47] The full sample differs from the diabetes and hypertension samples. However, we estimate the quarter-specific effects and the merger-by-merger effects using the same respective data.

recommended by Hirano et al. (2003) to achieve efficient coefficient estimates.[48] We also cluster the standard errors by the matched group (i.e., the treated patient and her matched pair) to allow for correlations over time and across birth cohort-sex-specialty-propensity score matches.

V Descriptive and motivating statistics

In Table 1, we provide summary statistics characterizing a select set of important characteristics for our Medicare sample. Each observation represents a patient-quarter combination. Table 1 provides all information separately for patients of acquired physicians, patients of potential control group physicians, and the sample of matched patients using our propensity score methodology.[49] The sample of non-matched potential controls contains over 40 million patient-quarters, and both the matched and the acquisition samples have more than a million observations. For each sub-sample, Table 1 provides the average propensity score, average demographic characteristics such as age, sex, race, urbanicity, and health condition of the beneficiaries. Table 1 also provides characteristics of the providers seen by the benefiaries, including the specialties of the providers and whether the patient visited providers of various sizes. We also provide the estimates for every beneficiary in the full sample.[50] Beneficiaries in the sample of potential controls are, on average, 76.2 years old, 86.1% white, 38.3% male, and live predominantly in metro areas with more than 500 thousand people. Beneficiaries often visit multiple providers during a quarter. 74.4% of potential control beneficiaries visited a provider in a group that had fewer than 5 physicians and 70.3% visited physicians employed by groups with between 5-24 physicians. Perhaps not surprisingly, the vast majority of ben-

[48] We estimate a weighted logit regression where the weights are equal to $1/\hat{e}(x)$ for acquired observations and $1/(1-\hat{e}(x))$ for non-acquired observations and $\hat{e}(x)$ is the propensity score estimate for the beneficiary with characteristics x. The unreported fixed-effects conditional logit estimates are estimated unweighted.

[49] The sample of potential controls excludes states and specialties that were not involved in acquisitions. We provide the list of acquisitions and some information regarding physician specialties in Appendix Table A-4.

[50] Patients visit multiple physicians during a quarter, such that the physician specialties and firm size variables are not mutually exclusive.

eficiaries have some health condition, as 71.7% of potential controls have at least a chronic circulatory condition.

In order to facilitate comparisons across samples, Table 1 also reports "normalized differences" that provide a metric of the compositional differences between the samples for each of the variables that we report. The normalized differences are a "balance" statistic, a standard measure reported in the propensity score literature that is used to evaluate the performance of the matching methodology (see, e.g., Imbens and Wooldridge (2009)). The normalized difference between the acquisition sample A and the comparison sample C for variable k is calculated as $\frac{\bar{X}_{A,k}-\bar{X}_{C,k}}{\sqrt{(S^2_{A,k}+S^2_{C,k})/2}}$, where $\bar{X}_{A,k}$ is the mean of variable k in the acquisition sample and $S^2_{A,k} = \frac{1}{N_A-1}\sum_{i\in A}(X_{i,k} - \bar{X}_{A,k})^2$ is the sample variance of variable k in sample A.[51]

In Table 1, the normalized differences labeled "P.C." compare the sample of patients visiting acquired physicians to the sample of potential controls. The normalized difference column labeled "Match" compares the sample of patients visiting acquired physicians against the sample of patients matched using the propensity score. Normalized differences closer to zero suggest more sample "balance," and thus a better control group, than normalized differences that are different from zero. Rosenbaum and Rubin (1985) suggest that a normalized difference with absolute value of less than 0.2 likely provides good "balance" between the sample and control group, although the 0.2 threshold is a rule of thumb.

The normalized differences analysis suggests important differences between the full sample of potential controls and the sample of patients visiting acquired physicians. These differences are immediately apparent in the comparison of the propensity score estimates, where normalized differences between the two samples are greater than one. The samples also differ substantially with respect to provider characteristics. Again, the normalized differences between the acquired sample and the potential control sample are greater than 0.2 with respect to several firm characteristics including whether the beneficiary visits a provider during the quarter, whether the beneficiary visits a family medicine provider, and the firm size of the

[51] The sample variance for the control sample is analogous. See page 24 of Imbens and Wooldridge (2009).

providers visited. The acquisition sample is far more likely to visit a physician than the sample of potential controls. Moreover, patients of acquired physicians visit physicians employed by larger firms than potential control group beneficiaries. Thus, the samples are not well-balanced along these dimensions.

The matching procedure is effective in substantially reducing the normalized differences between samples for every variable that we consider. The normalized differences between the patients of acquired physicians and the matched sample are always less than 0.1. Thus, the matching procedure results in a matched sample that is well-balanced against the acquired sample for all of the variables that we consider, including the provider characteristic variables that resulted in sample imbalance between the acquired sample and the full sample of potential controls. The matching procedure also results in even better balance among the demographic and health variables such as age, sex, race, and health for which the potential control sample was relatively similar to the acquisition sample. Thus, the matched sample results in better balance than the sample of potential controls for all of the observable characteristics that we consider in Table 1.

Appendix Table A-5 and Table A-6 provide similar summary and balance statistics as Table 1, but among diabetic and hypertensive beneficiaries, respectively.[52] The comparison patterns for these subsamples are similar to those found when we do not condition on health. The acquired and potential control samples differ significantly with respect to provider characteristics, especially firm size. However, the matching procedure results in well-balanced samples, and results in better balance than the full sample for all of the variables that we consider.

Next, we consider the average health outcomes that we use as the dependent variable in our analyses. The health outcomes that we consider are indicators for whether the beneficiary transitioned into the health condition during the quarter. Table 2 provides the mean outcome variables for patients of acquired physicians prior to the acquisition, patients of acquired

[52]Note that diabetes is a chronic endocrine condition and hypertension is a chronic circulatory condition.

physicians after the acquisition, and the matched patients that we use as a control-group for the patients of acquired physicians.

A comparison of outcomes among patients of acquired physicians in the pre-acquisition period against outcomes of matched beneficiaries finds that average outcomes are somewhat better among the matched beneficiaries. For example, among male patients with hypertension, 5.2% of patients in the "matched" sample develop acute cardiac conditions, whereas 5.6% of patients in the pre-acquisition period develop acute cardiac conditions. These patterns appear for most of the outcomes that we consider. However, the comparison of averages does not control for other factors that may explain the differences such as patient demographics, provider characteristics, industry trends, or length of the sample.[53]

Among patients of acquired physicians, the comparison of average health outcomes before and after acquisitions provides some evidence that health outcomes improve following acquisitions. For example, among patients with hypertension, 5.6% of patients develop an acute cardiac condition in the pre-acquisition period, but only 5.1% of those patients develop an acute cardiac condition in the post-acquisition period. In the post-acquisition period, patients of acquired physicians also have weakly better outcomes than matched beneficiaries. We observe this pattern for most all of our health conditions for both men and women, except for mortality. Mortality outcomes are weakly better prior to acquisitions and among the matched patients than among patients of acquired physicians following acquisitions, except among men with diabetes. Diabetic male patients of acquired physicians differ from the other samples in that they have higher mortality in the post-acquisition period than in the pre-acquisition period, but are similar to the other samples in that they have higher mortality than their matched counterparts.

The goal of our analysis in the following sections is to determine whether the mean differences observed in Table 2 represent acquisition effects after controlling for potentially

[53]The sample length may be an important consideration in this comparison since patients in the pre-acquisition period are observed for a much shorter period than are their matched counterparts.

confounding factors, such as age and general trends in health and healthcare.[54]

VI Empirical results

In this section, we present the results from our specifications that derive from our propensity score estimation sample. We begin with the results from our difference-in-differences estimation, which we present in Table 3. This table presents coefficients, marginal effects, and their respective standard errors under two specifications. The reported marginal effect estimates effectively represent the change in the hazard rate attributed to the post-acquisition effect for each of our health outcomes, which we calculate as $ME = \Lambda(X\hat{\beta} + \hat{\theta}_{PM}) - \Lambda(X\hat{\beta})$ for a specific person with characteristics X.[55]

We split Table 3 into three panels from top to bottom and report all results separately for men and women. The top panel considers the effects of acquisitions on mortality separately for the full sample, the sample of hypertensives, and the sample of diabetics. The middle panel considers the effects of acquisitions on acute cardiac conditions, AMIs, and ischemic heart disease among hypertensives. The third panel considers the effects of acquisitions among diabetics on glaucoma, asymptomatic diabetes complications, and symptomatic diabetes complications. Each of the vertical panels are split into two horizontal panels representing different model specifications. The "full set of controls" specification includes all of the covariates that we identified in Section IV, including controls for the health condition of the beneficiary and the specialties of the physicians seen during the quarter. In contrast, the "Age and quarter only" specification includes only quarter dummies and controls for the patient's age. The results for these specifications are presented separately for men and

[54]The regression specification uses geographic controls to account for some relevant demographic characteristics such as wealth and education.

[55]Marginal effect estimates are evaluated for a 77-year old, non-white patient of a merged provider who did not visit a specialist or have a chronic condition in the 044 (Penobscot County, ME) 3-digit ZIP code 1Q2006. The fact that the beneficiary visits a merged provider is included in X, although the equation distinguishes whether the period represents a post-merger period. $\Lambda(\cdot)$, again, represents the logit probability function.

women. For example, Table 3 presents the marginal effect estimate from our "full set of controls" specification for women from the full propensity score sample in the top panel on the right. We interpret the −0.012 estimate to mean that the mergers we consider reduce the probability that women from this sample will die by 1.2%.[56] However, both the marginal effect estimate and the corresponding post-merger coefficient estimate, −0.049, are statistically insignificant at the 5% confidence level, which is consistent with no beneficial merger effect.

Overall, we find that acquisitions do not improve health outcomes in *any* of our specifications for men or for women. Indeed, simply controlling for age and time removes most of the before and after differential observed in the sample means and reported in Table 2. Within mortality, most of the coefficients and marginal effects are not statistically significant from zero. Although the acquisition coefficient estimates in our mortality equation are positive and statistically significant for both men and women among diabetics, the predicted marginal effects are not statistically significant at the 5% confidence level. However, as we suspected, the marginal effect estimates for mortality are imprecise. For example, the 95% confidence interval in one specification ranges from reducing men's mortality by approximately 50% to increasing men's mortality by more than 25%. Similarly for women, the 95% confidence interval ranges from reducing mortality by nearly 40% to increasing mortality by nearly the same amount in our preferred specification using the full sample.

All of our other health measures are much more precisely estimated than are our estimates for mortality. However, despite the greater precision in the estimates from these other health outcomes, none of the marginal effect estimates provide evidence that acquisitions improve health. Indeed, the marginal effect estimates across nearly all of our outcomes are statistically insignificant and none of the estimates imply marginal effect benefits, as we estimate them, to be greater than 2.1%. Moreover, 27 of the 36 estimated effects are positive, consistent with mergers resulting in worse health. Although we find some evidence that the post-

[56] We interpret the coefficient to mean that the merger reduces the probability of death since the coefficient is negative. Positive coefficients represent increases in mortality probabilities.

acquisition coefficient estimates reduce the probability that women have heart attacks, we do not find the same result for men. In addition, the resulting marginal effect estimates for heart attacks among women are not statistically significant. Thus, even where we find statistically significant coefficient estimates consistent with health improvements, we do not find corresponding statistically significant marginal effects.

None of the estimates in Table 3 fully take advantage of the information from our nearest neighbor matching procedure that we use to create our control sample. In Table 4, we report the coefficient estimates from from our nearest neighbor matching estimator against the corresponding coefficient estimates from these two relatively parsimonious specifications.[57] We present this comparison since the Chamberlain (1980) procedure that we use to estimate consistent estimates for the coefficients of interest in our most flexible specification does not allow for the calculation of marginal effects.

Table 4 shows that the matching estimator results are qualitatively similar, and often attenuated, relative to the estimates from our more parsimonious specifications. In these cases, the matching estimator provides more evidence that mergers of this type are unlikely to result in clinical benefits, as we measure them. However, several of the mortality results do not become attenuated when we control for the matched pair, especially among the sample of diabetics. Although our matching estimator finds larger coefficient estimates for mortality in some cases, we continue to find that the mortality results are imprecise such that these estimates are not statistically significant.[58] We also find that the coefficient estimates increase in magnitude when we consider progression into ischemic heart disease among hypertensives. However, these effects are ambiguous since the coefficient for men suggests worse outcomes than the parsimonious specifications while the coefficient for women suggests statistically

[57] The most parsimonious specification includes controls for age and the contemporaneous quarter of the data. The "preferred" specification adds controls for health condition, time, and patient ZIP code fixed-effects. The matching estimator replaces the ZIP code controls for fixed-effects associated with matched beneficiaries from the nearest neighbor propensity score matching procedure.

[58] Indeed, the coefficient estimates from the matched estimator suggest that vertical integration may result in statistically significant increases in mortality among women.

significant better outcomes, relative to the parsimonious specifications.

We interpret our matching estimator results to be similar and often attenuated relative to the preferred difference-in-difference specification, despite some findings of larger magnitude coefficient estimates for mortality and ischemic heart disease. Consequently, we feel confident that we can focus on the results from our more parsimonious difference-in-difference specifications in the sections that follow. These sections consider whether vertical interation has separate effects by merger or over time following an acquisition, respectively. Our focus on the specification that controls for the health and ZIP code of the beneficiaries allows us to discuss the results in terms of marginal effects.

VI.1 Effects separately by acquisition

Next, we consider whether acquisitions have differential effects, which might occur if some firms are better at integration than others.[59] We plot our acquisition-specific marginal effect estimates and the corresponding 95% confidence intervals in Figure 1, Figure 2, and Figure 3. In each figure, the coefficient estimates are sorted such that the smallest coefficient (i.e., largest negative estimate) is plotted on the leftmost part of the figure and the largest coefficient estimate is on the rightmost part of the figure.

Figure 1 considers the acquisition-specific effects of acquisitions on mortality for the full sample. Our estimates provide little evidence that any of the acquisitions that we consider have a beneficial effect on mortality relative to the control group of physicians, but the effects are imprecise. In several cases, we cannot reject marginal effects that are zero or as large as 50%. These findings may not be surprising since death is a rare event tangentially related to ambulatory physician care. Perhaps we should not expect that the clinical benefits from mergers between hospitals and ambulatory physicians will be realized as lower mortality.

Next, we consider health conditions that are more prevalent and are more closely related to ambulatory treatment. Figure 2 considers acquisition-specific marginal effects of outcomes

[59] We pool several of the smaller acquisitions from the sample into a single group.

related to diabetes. The top two charts, (a) and (b), correspond with the estimates for asymptomatic and symptomatic diabetes complications, respectively. Chart (c) considers glaucoma, an eye condition that results from the progression of diabetes.[60] The estimates are far more precise than they were for mortality, especially for our symptomatic diabetes complication measure. However, we again find that the vast majority of acquisitions have no statistically significant effect on these outcomes.

Next, we consider health outcomes associated with the treatment of hypertension, perhaps the most prevalent chronic condition in our sample. Figure 3 presents the effects of acquisitions on AMI (i.e., "heart attacks") in chart (a), ischemic heart disease in chart (b), and "acute cardiac conditions," which includes AMI and stroke, in chart (c). The estimates for all of our results are our most precisely estimates marginal effects, far more precisely estimated than our mortality estimates. Again, we rarely observe that mergers have statistically significant effects on any of our health outcomes. In chart (a), we observe that none of the acquisitions result in fewer heart attacks. In chart (b), we observe that none of the acquisitions reduce the hazard for ischemic heart disease. In chart (c), we do not find any evidece that acquisitions reduce the chance of having an acute cardiac condition. We do find some evidence that three acquisitions result in a statistically significant higher chance of having an acute cardiac conditions, and one acquisition has a marginal effect of between 5-10%. However, the largest effect is also the most imprecisely estimated effect and is stastically insignficant, whereas none of the statistically significant effects increase the chance of an acute cardiac condition by more than 5%.

The effects from our hypertension samples are especially relevant since our sample of acquisitions includes many cardiology groups. Although integration should theoretically affect any type of physician that treats their observed patients, one might expect that the largest effects would be realized among patients with conditions treated by the specialty of

[60] see, e.g., S. Bonovas and Filioussi (2004).

the acquired physicians.[61] That we do not observe beneficial acquisition effects for any of our transactions involving hypertensive patients suggests that our overall findings are unlikely due to choosing irrelevant health conditions for the specialties involved in the acquisitions that we consider.

VI.2 Quarter-specific effects

Although our overall and acquisition-specific results provide little evidence that full financial integration of hospitals and physicians lead to improved health outcomes, perhaps clinical efficiencies from acquisitions take time to be realized. Clinical integration may involve the development of best practices, training, and the acculturation of physicians. If so, the average effect for the period following an acquisition might mask potential benefits that do not become realized until months or years following the acquisition. We consider the potential for these types of period-specific effects by plotting quarter-specific acquisition marginal effects from before and after an acquisition. We also provide the respective 95% confidence intervals to identify statistically significant effects. The period of the data and the timing of the acquisitions enable us to identify effects 12 quarters (i.e., 3 years) following acquisitions.

Figure 4 considers mortality estimates for each period in the full sample following a merger, separately for men and women. If integration takes time to achieve clinical benefits, we would expect that the quarter-specific acquisition coefficients would be increasing in magnitude (i.e., by becoming more negative) in the periods following the acquisition. We do not find any evidence of these period-specific effects. However, the estimates are often imprecise and volatile, especially for men.[62] Next, we consider the period-specific acquisition effects for diabetes complications, separately for men and women. Again, we would expect the same pattern in the coefficient estimates if mergers took time to achieve clinical benefits

[61] Conceptually, integration could allow providers of unlike services to coordinate care and ensure better outcomes across a variety of treatments and outcomes, regardless of whether the relevant specialist directly treats the condition.

[62] Men represent a much smaller fraction of our data than do women.

with respect to this measure. Similar to mortality, we do not find any such acquisition effects in the diabetes-related health outcomes. Although these results suggest that some periods for complication measures result in statistically significant health outcomes, these results hardly represent a pattern and do not suggest a trend towards better or worse health. Indeed, the marginal effect estimates are generally flat over the period for every outcome that we consider for both men and women.

Figure 6 considers the set of hypertension conditions. These results are by far the most precisely estimated, and perhaps the most relevant given the importance of cardiologists to our sample. Again, we find little evidence that clinical benefits take time to become realized in the treatment of hypertension. Although we observe some evidence that hazard rates experience some decline in the time since the merger for some of the outcomes that we consider, especially for women, beneficial effects are not statistically significant in any period. In addition, we find some evidence that any patterns in the hazard rates may have begun prior to the acquisitions.

VII Conclusion

Whether vertically integrated health systems are beneficial to patients, payers, or providers is of significant policy concern. Our analysis directly considers the health consequences of 28 physician acquisitions by hospitals using a sample of acquisitions and a database of Medicare hospital claims. We consider a broad range of health outcomes that each have different analytical strengths and weaknesses. Some measures, such as AMI and mortality, are easily observed and are consistently identified across time and providers. Other measures that are not as easily diagnosed, such as our diabetes outcomes, have the advantage that they are directly related to the treatment of the disease and are frequently diagnosed during our sample period. Our results find little evidence that physician integration into hospital systems affect health outcomes related to the treatment of diabetes and hypertension, regardless

of the nature of the outcome that we consider. We interpret our results as implying that acquisitions of this type have small clinical benefits related to the treatment of hypertension and diabetes.

One possible limitation of our study is that some of the "vertical" acquisitions that we consider may also increase physician concentration in the affected areas. If so, prior research has demonstrated that increased horizontal concentration may lessen competition for quality (Koch et al., 2017) and thus offset efficiencies associated with "vertical" integration. This effect may also be responsible for the finding that some acquisitions result in worse health outcomes for patients and is consistent with doctors' increased perception of consolidation as creating risks for patients (Haas et al., 2018).

Perhaps the most important limitation of our study is that we limit our analysis to clinical efficiencies related to specific health conditions. Efficiencies related to our acquisitions may appear for other health conditions, or along other dimensions of firm performance (i.e., cost efficiencies) that we do not consider. For this reason, our results do not allow us to reach definitive conclusions about the nature of efficiencies, in general.

Despite these limitations, we emphasize that the health outcomes that we consider are important and prevalent for our patient population. Nearly everyone in our Medicare sample becomes hypertensive at some point during the period, and over one-third of Medicare patients develop diabetes. These conditions can have serious health consequences if not properly managed. Moreover, these conditions can interact with the treatment and development of other types of health conditions suggesting potential benefits from integration. Despite the opportunities for these potential benefits, we find little evidence that the benefits were realized in the transactions that we consider.

References

Afendulis, Christopher C and Daniel P Kessler, "Tradeoffs from integrating diagnosis and treatment in markets for health care," *American Economic Review*, 2007, *97* (3), 1013–1020.

Baker, Laurence C, M Kate Bundorf, and Daniel P Kessler, "Vertical integration: hospital ownership of physician practices is associated with higher prices and spending," *Health Affairs*, 2014, *33* (5), 756–763.

_ , _ , and _ , "The effect of hospital/physician integration on hospital choice," *Journal of health economics*, 2016, *50*, 1–8.

_ , _ , Anne B Royalty, and Zachary Levin, "Physician practice competition and prices paid by private insurers for office visits," *Jama*, 2014, *312* (16), 1653–1662.

Blankenthorn, Dana, "Vertical integration the answer to health care costs," *SmartPlanet. com*, 2009.

Bonovas, V. Peponis S. and K Filioussi, "Diabetes mellitus as a risk factor for primary open-angle glaucoma: a meta-analysis," *Diabetic medicine*, 2004, *21* (6), 609–614.

Bresnahan, Timothy F and Jonathan D Levin, "Vertical integration and market structure," Technical Report, National Bureau of Economic Research 2012.

Burns, Lawton Robert and Ralph W Muller, "Hospital-physician collaboration: landscape of economic integration and impact on clinical integration," *The Milbank Quarterly*, 2008, *86* (3), 375–434.

_ , Jeff C Goldsmith, and Aditi Sen, "Horizontal and vertical integration of physicians: A tale of two tails," *Advances in health care management*, 2013, *15*, 39–117.

Capps, Cory, David Dranove, and Christopher Ody, "The Effect of Hospital Acquisitions of Physician Practices on Prices and Spending," *mimeo*, 2015.

Casalino, Lawrence P, Michael F Pesko, Andrew M Ryan, Jayme L Mendelsohn, Kennon R Copeland, Patricia Pamela Ramsay, Xuming Sun, Diane R Rittenhouse, and Stephen M Shortell, "Small primary care physician practices have low rates of preventable hospital admissions," *Health Affairs*, 2014, *33* (9), 1680–1688.

Chamberlain, Gary, "Analysis of covariance with qualitative data," *The Review of Economic Studies*, 1980, *47* (1), 225–238.

Chang, Michelle LaClair Wenke Hwang Jongwha and Harold Paz, "Effects of Integrated Delivery System on Cost and Quality," *THE AMERICAN JOURNAL OF MANAGED CARE*, 2013, *19* (5), 175–184.

Ciliberto, Federico and David Dranove, "The effect of physician–hospital affiliations on hospital prices in California," *Journal of Health Economics*, 2006, *25* (1), 29–38.

Cooper, James C, Luke M Froeb, Dan O'Brien, and Michael G Vita, "Vertical antitrust policy as a problem of inference," *International Journal of Industrial Organization*, 2005, *23* (7-8), 639–664.

Cooper, Zack, Stuart V Craig, Martin Gaynor, and John Van Reenen, "The price aint right? hospital prices and health spending on the privately insured," Technical Report, National Bureau of Economic Research 2015.

Cuellar, Alison Evans and Paul J Gertler, "Strategic integration of hospitals and physicians," *Journal of health economics*, 2006, *25* (1), 1–28.

Dafny, Leemore, "Hospital Industry Consolidation - Still more to come?," *New England Journal of Medicine*, 2013.

Dunn, Abe and Adam Hale Shapiro, "Do Physicians Possess Market Power?," *Journal of Law and Economics*, 2014, *57* (1), 159–193.

Forbes, S.J. and M. Lederman, "Does Vertical Integration Affect Firm Performance? Evidence from the Airline Industry," *RAND Journal of Economics*, 2010, *41* (4), 765–790.

Gaynor, Martin and Mark V Pauly, "Compensation and productive efficiency in partnerships: evidence from medical groups practice," *Journal of Political Economy*, 1990, pp. 544–573.

_ **and Robert Town**, "The impact of hospital consolidationUpdate," *The Synthesis Project. Robert Wood Johnson Foundation. http://www. rwjf. org/content/dam/farm/reports/issue_briefs/2012/rwjf73261*, 2012.

_ **and William B Vogt**, "Antitrust and competition in health care markets," *Handbook of health economics*, 2000, *1*, 1405–1487.

Gibbons, Charles E., Juan Carlos Surez Serrato, and Michael B. Urbancic, "Broken or Fixed Effects?," Working Paper 20342, National Bureau of Economic Research July 2014.

Gottlieb, S, "The doctor won't see you now: he's clocked out," *The Wall Street Journal*, 2013.

Haas, S, A Gawande, and ME Reynolds, "The risks to patient safety from health system expansions," *JAMA*, 2018.

Hillson, Steven D, Roger Feldman, and Terence D Wingert, "Economies of scope and payment for physician services.," *Medical care*, 1992, *30* (9), 822–831.

Hirano, Keisuke, Guido W Imbens, and Geert Ridder, "Efficient estimation of average treatment effects using the estimated propensity score," *Econometrica*, 2003, *71* (4), 1161–1189.

Hough, DE, "Is bigger always better? The optimal size of a group practice.," *The Journal of medical practice management: MPM*, 2001, *17* (6), 312–318.

Hough, Douglas E, Kai Liu, and David N Gans, "Size matters: the impact of physician practice size on productivity," *Unpublished manuscript. Medical Group Management Association (CO)*, 2011.

Imbens, Guido W, "Matching methods in practice: Three examples," *Journal of Human Resources*, 2015, *50* (2), 373–419.

Imbens, Guido W. and Jeffrey M. Wooldridge, "Recent Developments in the Econometrics of Program Evaluation," *Journal of Economic Literature*, 2009, *47* (1), 5–86.

JM, Landon BE Chernew ME. McWilliams, "Changes in health care spending and quality for Medicare beneficiaries associated with a commercial ACO contract.," *JAMA*, 2013, *310* (8), 829–836.

Kane, C. K. and D. W. Emmons, "New Data on Physician Practice Arrangements: Private Practice Remains Strong Despite Shifts Toward Hospital Employment," Technical Report, AMA Policy Research Perspectives 2013.

Keating, Nancy L, Mary Beth Landrum, Bruce E Landon, John Z Ayanian, Catherine Borbas, Robert Wolf, and Edward Guadagnoli, "The influence of physicians practice management strategies and financial arrangements on quality of care among patients with diabetes," *Medical care*, 2004, *42* (9), 829–839.

Ketcham, Jonathan D, Laurence C Baker, and Donna MacIsaac, "Physician practice size and variations in treatments and outcomes: evidence from Medicare patients with AMI," *Health Affairs*, 2007, *26* (1), 195–205.

Kimbell, Larry J and John H Lorant, "Physician productivity and returns to scale.," *Health services research*, 1977, *12* (4), 367.

Koch, Thomas, Brett Wendling, and Nathan E Wilson, "Physician market structure, patient outcomes, and spendign: An examination of Medicare beneficiaries," *Health services research*, 2018.

Koch, Thomas G, Brett W Wendling, and Nathan E Wilson, "How vertical integration affects the quantity and cost of care for Medicare beneficiaries," *Journal of Health Economics*, 2017, *52*, 19–32.

Kosova, R., F. Lafontaine, and R. Perrigot, "Organizational Form and Performance: Evidence from the Hotel Industry," *Review of Economics and Statistics*, 2013, *95* (4), 1303–1323.

Kralewski, John E, Terence D Wingert, David J Knutson, and Christopher E Johnson, "The effects of medical group practice organizational factors on physicians' use of resources," *Journal of Healthcare Management*, 1999, *44* (3), 167.

Mullainathan, S. and D. Scharfstein, "Do firm boundaries matter?," *American Economic Review*, 2001, *91* (2), 195–199.

Neprash, Hannah T, Michael E Chernew, Andrew L Hicks, Teresa Gibson, and J Michael McWilliams, "Association of financial integration between physicians and hospitals with commercial health care prices," *JAMA internal medicine*, 2015, *175* (12), 1932–1939.

Novak, S. and S. Stern, "How Does Outsourcing Affect Performance Dynamics? Evidence from the Automobile Industry," *Management Science*, 2008, *54* (12), 1963–1979.

Post, Brady, Tom Buchmueller, and Andrew M. Ryan, "Vertical Integration of Hospitals and Physicians: Economic Theory and Empirical Evidence on Spending and Quality," *Medical Care Research and Review*, 2017, pp. 1–35.

Rebitzer, James B. and Mark Votruba, "Organizational Economics and Physician Practices," Working Paper 17535, National Bureau of Economic Research October 2011.

Rosenbaum, P.R. and D.B. Rubin, "Constructing a Control Group Using Multivariate Matched Sampling Methods that Incorporate the Propensity Score," *The American Statistician*, 1985, *39*, 33–38.

Whinston, Michael D., *Lectures on Antitrust Economics*, The MIT Press, Cambridge Massachusetts, 2006.

Wilson, Nathan E, "Vertical Separation Increases Gas Prices," *Economic Inquiry*, 2015, *53* (2).

Wolinsky, Asher, "Competition in a market for informed experts' services," *The RAND Journal of Economics*, 1993, pp. 380–398.

Table 1: Summary and balance statistics for the Medicare sample 2006-2012

	Potential Controls		Matched		Treated		Normalized Differences	
Variable	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Match**	**P.C.**
Propensity Score	0.018	0.039	0.137	0.118	0.144	0.128	0.054	1.329
				Patient demographics				
Age	76.21	8.10	77.34	7.60	77.39	7.60	0.007	0.150
Male	0.383	0.486	0.390	0.488	0.391	0.488	0.002	0.017
White	0.861	0.346	0.917	0.276	0.917	0.276	0.000	0.179
Metro >1 million	0.438	0.494	0.457	0.494	0.461	0.495	0.007	0.045
Metro 500k - 1 million	0.216	0.405	0.244	0.421	0.233	0.414	-0.026	0.041
Metro < 500k	0.116	0.310	0.096	0.283	0.096	0.278	-0.001	-0.068
Non-Metro area	0.229	0.413	0.203	0.394	0.210	0.395	0.019	-0.045
				Patient health				
Hypertension	0.649	0.477	0.711	0.453	0.704	0.456	-0.015	0.118
Diabetes	0.252	0.434	0.276	0.447	0.275	0.447	-0.002	0.052
Circulatory	0.717	0.450	0.790	0.407	0.790	0.407	-0.001	0.170
Musculoskeletal	0.492	0.500	0.556	0.497	0.556	0.497	0.000	0.128
Endocrine	0.388	0.487	0.421	0.494	0.420	0.494	-0.002	0.064
Sense organ diseases	0.314	0.464	0.344	0.475	0.344	0.475	0.000	0.064
Gastrointestinal	0.275	0.446	0.327	0.469	0.327	0.469	-0.001	0.113
Respiratory	0.215	0.411	0.262	0.440	0.258	0.438	-0.008	0.102
Signs/symptoms	0.200	0.400	0.248	0.432	0.249	0.432	0.001	0.117
Genito-urinary	0.224	0.417	0.251	0.433	0.248	0.432	-0.007	0.057
Blood disease	0.176	0.381	0.199	0.399	0.197	0.398	-0.005	0.055
Skin conditions	0.162	0.369	0.184	0.387	0.184	0.388	0.002	0.059
Neoplasms (cancer)	0.122	0.327	0.139	0.346	0.141	0.348	0.005	0.057
				Provider practice characeristics				
Any visit	0.657	0.475	0.841	0.365	0.837	0.369	-0.010	0.423
Family Practice	0.436	0.496	0.545	0.498	0.540	0.498	-0.010	0.210
Diagnostic radiology	0.166	0.372	0.224	0.417	0.224	0.417	0.000	0.145
Cardiology	0.124	0.329	0.179	0.383	0.189	0.392	0.026	0.181
Opthalmology	0.101	0.302	0.122	0.327	0.122	0.328	0.001	0.067
Podiatry	0.080	0.272	0.105	0.307	0.101	0.302	-0.012	0.073
Other	0.339	0.473	0.419	0.493	0.414	0.493	-0.009	0.156
Firm size <5	0.744	0.437	0.823	0.382	0.821	0.384	-0.005	0.187
Firm size 5 -24	0.703	0.457	0.819	0.385	0.819	0.385	0.001	0.274
Firm size 25 - 49	0.495	0.500	0.694	0.461	0.695	0.460	0.003	0.417
Firm size 50 - 99	0.409	0.492	0.633	0.482	0.632	0.482	-0.002	0.458
Firm size 100 - 200	0.306	0.461	0.564	0.496	0.553	0.497	-0.021	0.517
Firm size > 200	0.342	0.474	0.520	0.500	0.525	0.499	0.009	0.374
Obs	40,549,345		1,011,170		1,010,795			

Table 2: Summary of new condition diagnosis during quarter by sample 2006-2012

	Matched			Pre-Acquisition			Post-Acquisition		
Variable	**Obs**	**Mean**	**Std. Dev**	**Obs**	**Mean**	**Std. Dev**	**Obs**	**Mean**	**Std. Dev**
MEN					Full Sample				
Mortality	395,128	1.11E-04	1.06E-02	175,970	1.02E-04	1.01E-02	222,966	1.35E-04	1.16E-02
					Hypertension				
Mortality	292,641	1.50E-04	1.23E-02	127,573	1.41E-04	1.19E-02	163,306	1.72E-04	1.31E-02
Acute cardiac	147,317	0.052	0.222	74,406	0.056	0.230	69,283	0.051	0.220
AMI	277,023	0.004	0.066	122,351	0.005	0.071	151,643	0.004	0.066
Ischemic HD	103,512	0.037	0.189	46,931	0.048	0.214	51,346	0.033	0.178
					Diabetes				
Mortality	137,059	1.75E-04	1.32E-02	55,787	1.97E-04	1.40E-02	79,020	1.90E-04	1.38E-02
Asymptomatic	127,683	0.005	0.070	52,687	0.006	0.078	72,942	0.005	0.068
Symptomatic	87,588	0.029	0.168	37,294	0.038	0.191	46,724	0.027	0.161
Glaucoma	111,294	0.006	0.076	47,049	0.008	0.089	63,300	0.004	0.064
WOMEN					Full Sample				
Mortality	617,953	1.15E-04	1.07E-02	273,734	7.67E-05	8.76E-03	346,524	1.91E-04	1.38E-02
					Hypertension				
Mortality	475,549	1.49E-04	1.22E-02	208,545	9.59E-05	9.79E-03	263,868	2.46E-04	1.57E-02
Acute cardiac	248,590	0.046	0.210	127,749	0.049	0.216	116,077	0.046	0.210
AMI	458,434	0.003	0.057	202,136	0.004	0.065	250,673	0.003	0.058
Ischemic HD	241,406	0.027	0.163	112,424	0.034	0.182	125,990	0.021	0.144
					Diabetes				
Mortality	195,700	2.25E-04	1.50E-02	78,218	1.02E-04	1.01E-02	113,126	2.92E-04	1.71E-02
Asymptomatic	181,492	0.005	0.071	72,957	0.007	0.084	102,381	0.005	0.070
Symptomatic	126,739	0.026	0.161	51,812	0.034	0.181	66,793	0.025	0.156
Glaucoma	152,462	0.007	0.081	62,944	0.009	0.094	86,300	0.005	0.068

Table 3: Estimated effects of acquisitions on health-state transition probabilities

	Age and quarter only				**Full set of controls**				
	Men		**Women**		**Men**		**Women**		
Specification	Coeff (SE)	Marg Eff (SE)	Coeff (SE)	Marg Eff (SE)	Coeff (SE)	Marg Eff (SE)	Coeff (SE)	Marg Eff (SE)	N
					Mortality				
Full	-0.681	-0.166	0.120	0.011	-0.706	-0.172	-0.049	-0.012	1,945,801
	(0.816)	(0.204)	(0.739)	(0.068)	(0.767)	(0.171)	(0.752)	(0.188)	
Hyptertension	-0.794	-0.195	0.095	0.009	-0.749	-0.182	0.029	0.007	1,470,680
	(0.792)	(0.195)	(0.729)	(0.066)	(0.777)	(0.173)	(0.752)	(0.187)	
Diabetes	0.103	0.035	1.313*	0.035	0.628	0.270	1.898*	0.131	539,007
	(0.613)	(0.046)	(0.602)	(0.046)	(0.798)	(0.152)	(0.823)	(0.160)	
					Hypertension				
Acute Cardiac	0.092	0.005	0.125	0.013	0.104	0.015	0.112	0.019	783,422
	(0.077)	(0.020)	(0.074)	(0.007)	(0.073)	(0.010)	(0.069)	(0.012)	
Ishchemic HD	0.034	0.005	-0.051	-0.006	0.005	0.001	-0.073	-0.009	681,609
	(0.132)	(0.020)	(0.124)	(0.049)	(0.098)	(0.012)	(0.094)	(0.012)	
AMI	-0.164	-0.008	-0.246*	-0.011	-0.220	-0.011	-0.354*	-0.021	1,412,910
	(0.112)	(0.007)	(0.111)	(0.008)	(0.139)	(0.009)	(0.134)	(0.013)	
					Diabetes				
Asymptomatic compl	0.151	0.029	0.037	0.009	0.325	0.051	0.229	0.042	610,142
	(0.250)	(0.049)	(0.218)	(0.053)	(0.214)	(0.041)	(0.198)	(0.038)	
Symptomatic compl	0.214	0.051	0.240	0.048	0.126	0.031	0.177	0.043	416,950
	(0.143)	(0.035)	(0.122)	(0.023)	(0.122)	(0.030)	(0.113)	(0.027)	
Glaucoma	0.030	0.008	0.078	0.013	0.229	0.056	0.296	0.072	449,040
	(0.206)	(0.051)	(0.179)	(0.032)	(0.213)	(0.052)	(0.192)	(0.046)	

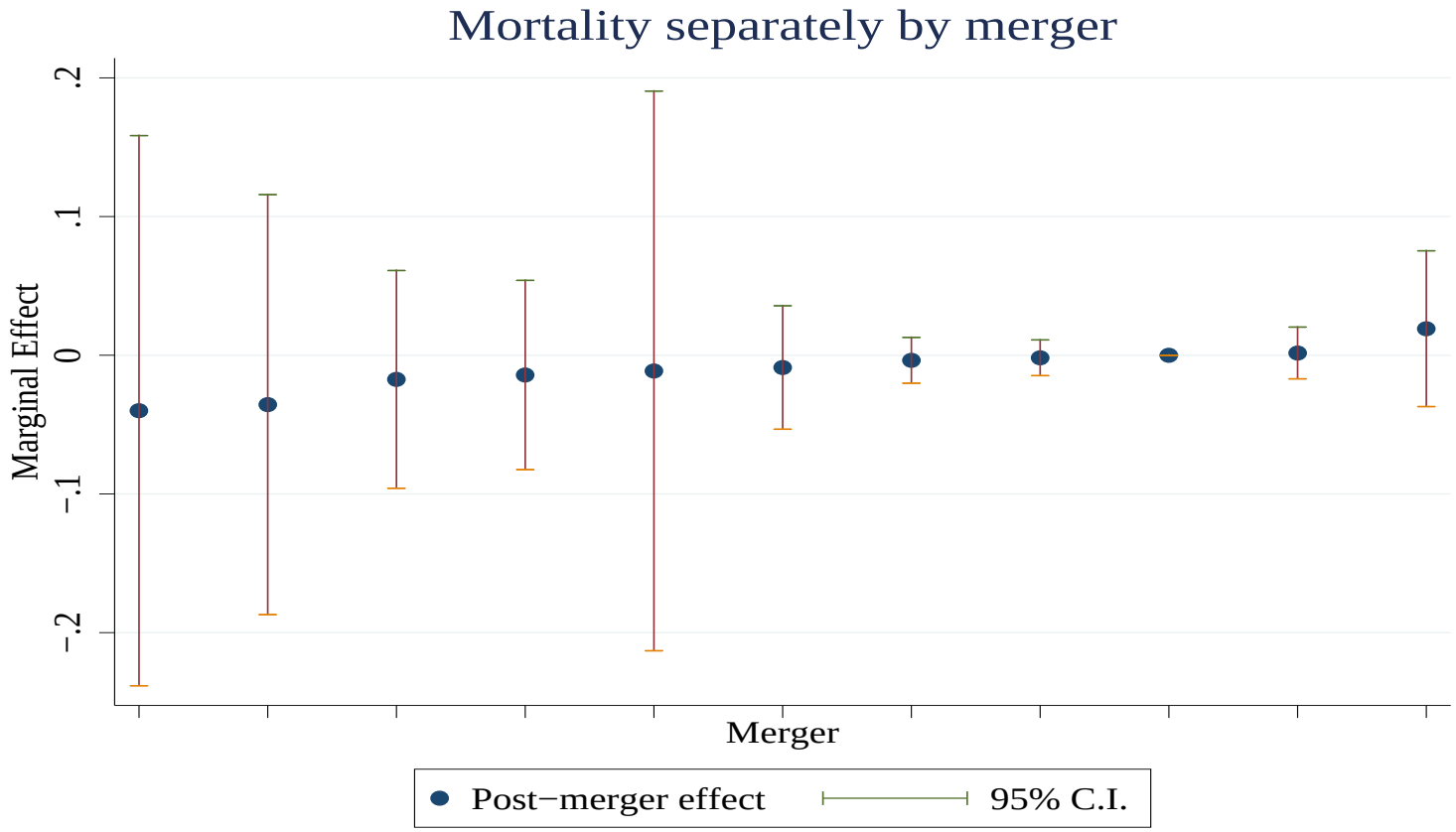
*Statistically significant at the 5% C.I.. Marginal effects evaluated for 77-year old in the 044 3-digit ZIP code among the treated 1Q2006.The full set of controls includes race and its gender interaction, physician specialty, patient ZIP code, and major ICD-9 condition characteristics in addition to age dummies, age category interactions with sex, and quarter dummies. Samples have different observation counts since we omit periods following the first observance of a condition. Observations represent counts in the "age and quarter only" specification. All estimates derive from a discrete-time hazard model. Marginal effect estimates are calculated as $ME = \Lambda(X\hat{\beta} + \hat{\theta}_{PM}) - \Lambda(X\hat{\beta})$ and reported as percentages/100.

Table 4: Coefficient and pseudo-R^2 comparisons across specifications.

Specification	Age and quarter only			Full set of controls			Matched-pair fixed effects		
	Men	Women	R^2	Men	Women	R^2	Men	Women	R^2
					Mortality				
Full	-0.681	0.120	0.158	-0.706	-0.049	0.408	-0.541	0.280	0.387
	(0.816)	(0.739)		(0.767)	(0.752)		(0.611)	(0.512)	
Hyptertension	-0.794	0.095	0.162	-0.749	0.029	0.395	-0.751	0.307	0.391
	(0.792)	(0.729)		(0.777)	(0.752)		(0.644)	(0.531)	
Diabetes	0.103	1.313*	0.207	0.628	1.898*	0.489	-2.364	3.821*	0.516
	(0.613)	(0.602)		(0.798)	(0.823)		(1.342)	(1.652)	
					Hypertension				
Acute Cardiac	0.092	0.125	0.039	0.104	0.112	0.183	0.043	0.037	0.263
	(0.077)	(0.074)		(0.073)	(0.069)		(0.051)	(0.043)	
Ishchemic HD	0.034	-0.051	0.268	0.005	-0.073	0.339	0.149	-0.187*	0.465
	(0.132)	(0.124)		(0.098)	(0.094)		(0.076)	(0.058)	
AMI	-0.164	-0.246*	0.221	-0.220	-0.354*	0.325	0.059	-0.038	0.399
	(0.112)	(0.111)		(0.139)	(0.134)		(0.106)	(0.093)	
					Diabetes				
Asymptomatic compl	0.151	0.037	0.093	0.325	0.229	0.201	0.116	-0.048	0.174
	(0.250)	(0.218)		(0.214)	(0.198)		(0.174)	(0.144)	
Symptomatic compl	0.214	0.240	0.059	0.126	0.177	0.162	0.006	0.190*	0.201
	(0.143)	(0.122)		(0.122)	(0.113)		(0.095)	(0.087)	
Glaucoma	0.030	0.078	0.281	0.229	0.296	0.379	-0.102	-0.056	0.490
	(0.206)	(0.179)		(0.213)	(0.192)		(0.177)	(0.153)	

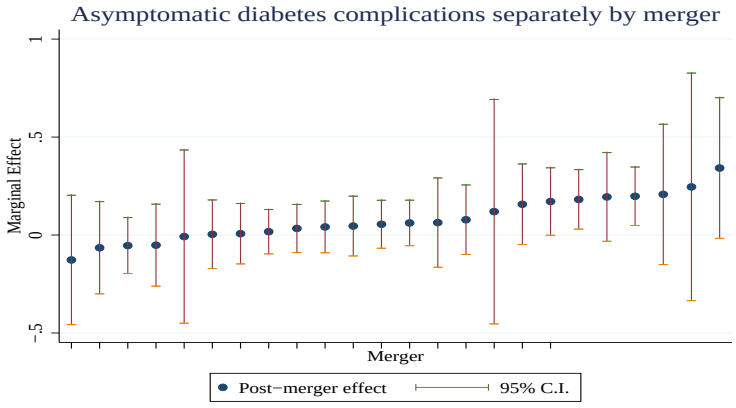
*Statistically significant at the 5% confidence interval. Matched-pair fixed-effects model estimated using procedure outlined by Chamberlain (1980). Fixed-effects in that model represent an acquired physician's patient and the matched patient from the propensity score procedure. Coefficients and standard errors in the "age and quarter only" and "full set of controls" specifications are estimated using propensity score weights, as described in Hirano et al. (2003).

Figure 1: Marginal effect estimates separately by acquisition - full sample mortality

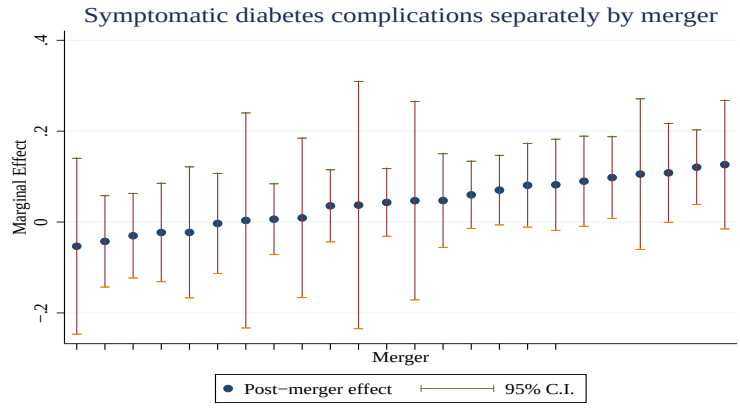


*Pooled mergers are Legacy-SW Community, Bridgeport-Radiation Oncology, Christ-Ohio Heart, Aurora-Comprehensive, Aurora-N. Lake, Jefferson-Jefferson Hills, Butler-DiCuccio, Good Samaritan Suffern-NY Day Surgery, Texas Childrens-Women's Health, Scripps-Penn Elm, and ThedaCare-Nelson Family. The pooled group has the largest marginal effect = 1.9%. Marginal effect estimates are reported as percentages/100.

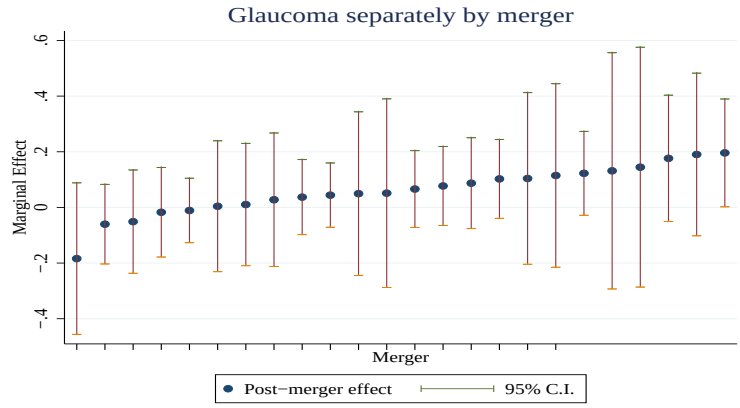
Figure 2: Marginal effects separately by acquisition - diabetes*



(a) Asymptomatic complications



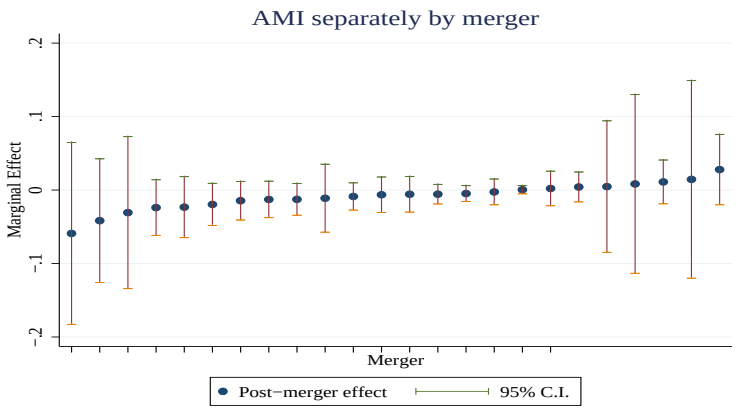
(b) Symptomatic complications



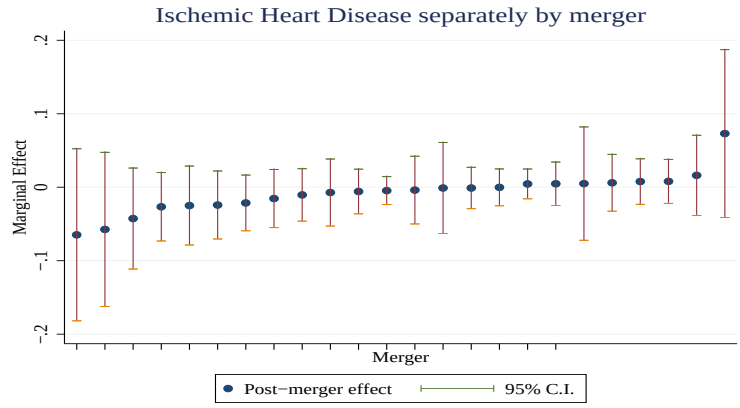
(c) Glaucoma

*Marginal effect estimates are reported as percentages/100.

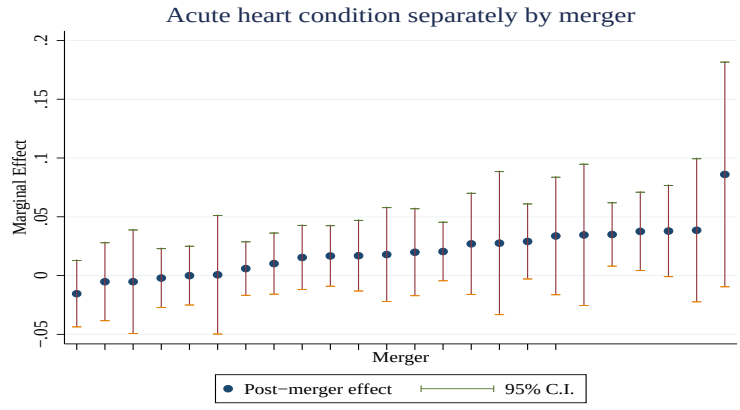
Figure 3: Marginal effects separately by acquisition - hypertension*



(a) AMI



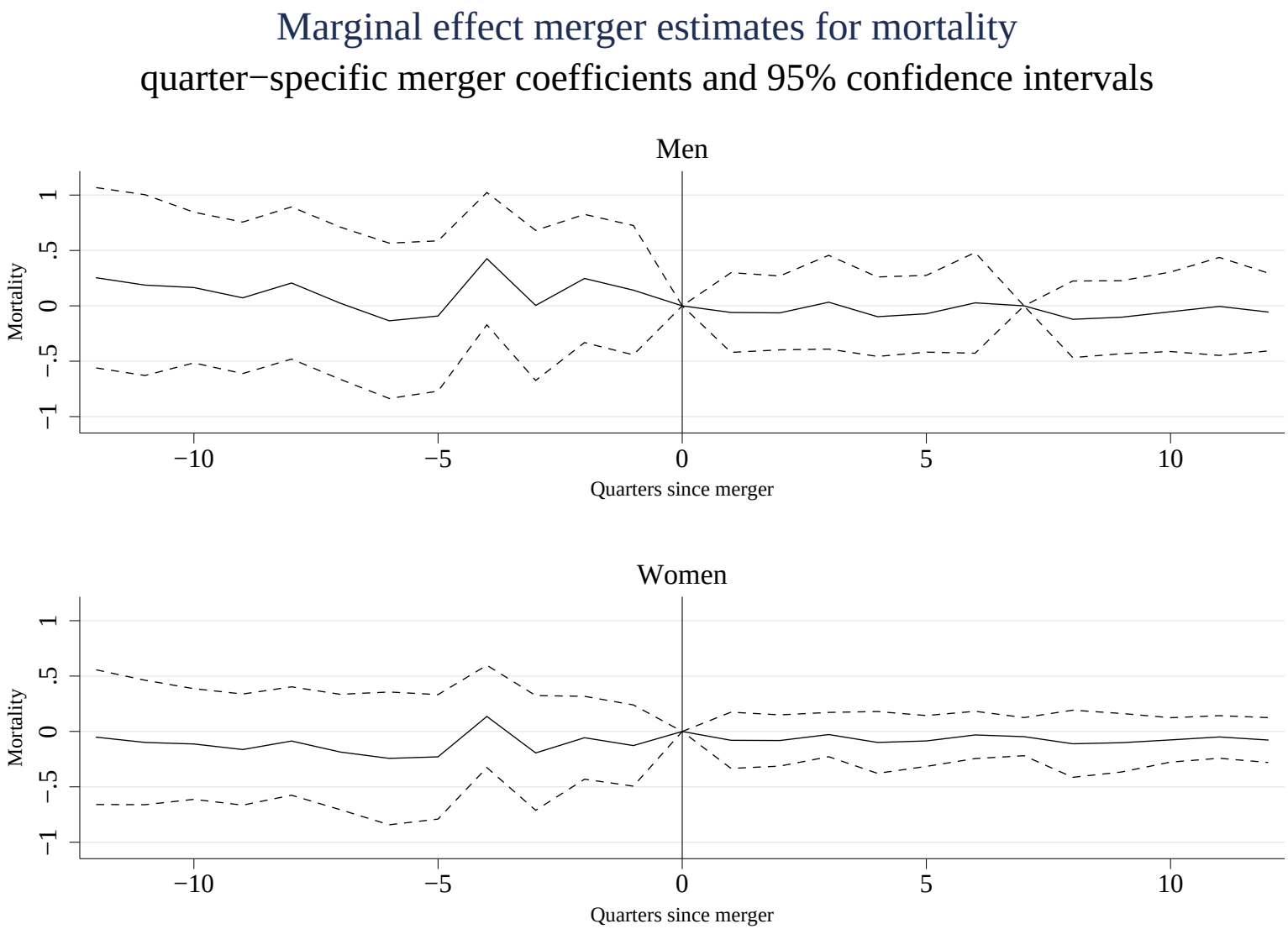
(b) Ischemic heart disease



(c) Acute cardiac conditions

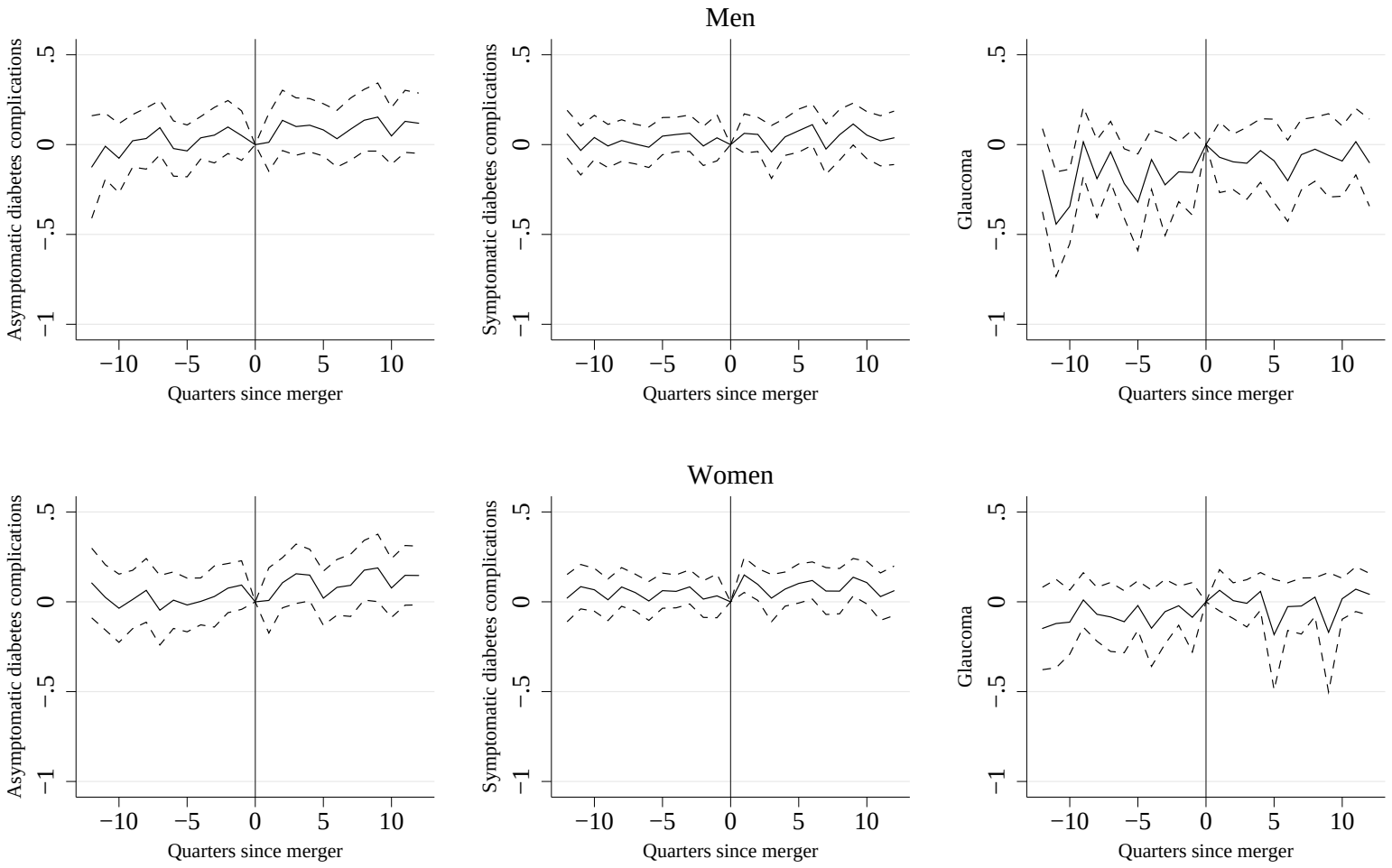
*Marginal effect estimates are reported as percentages/100.

Figure 4: Period-specific acquisition marginal effects - mortality*



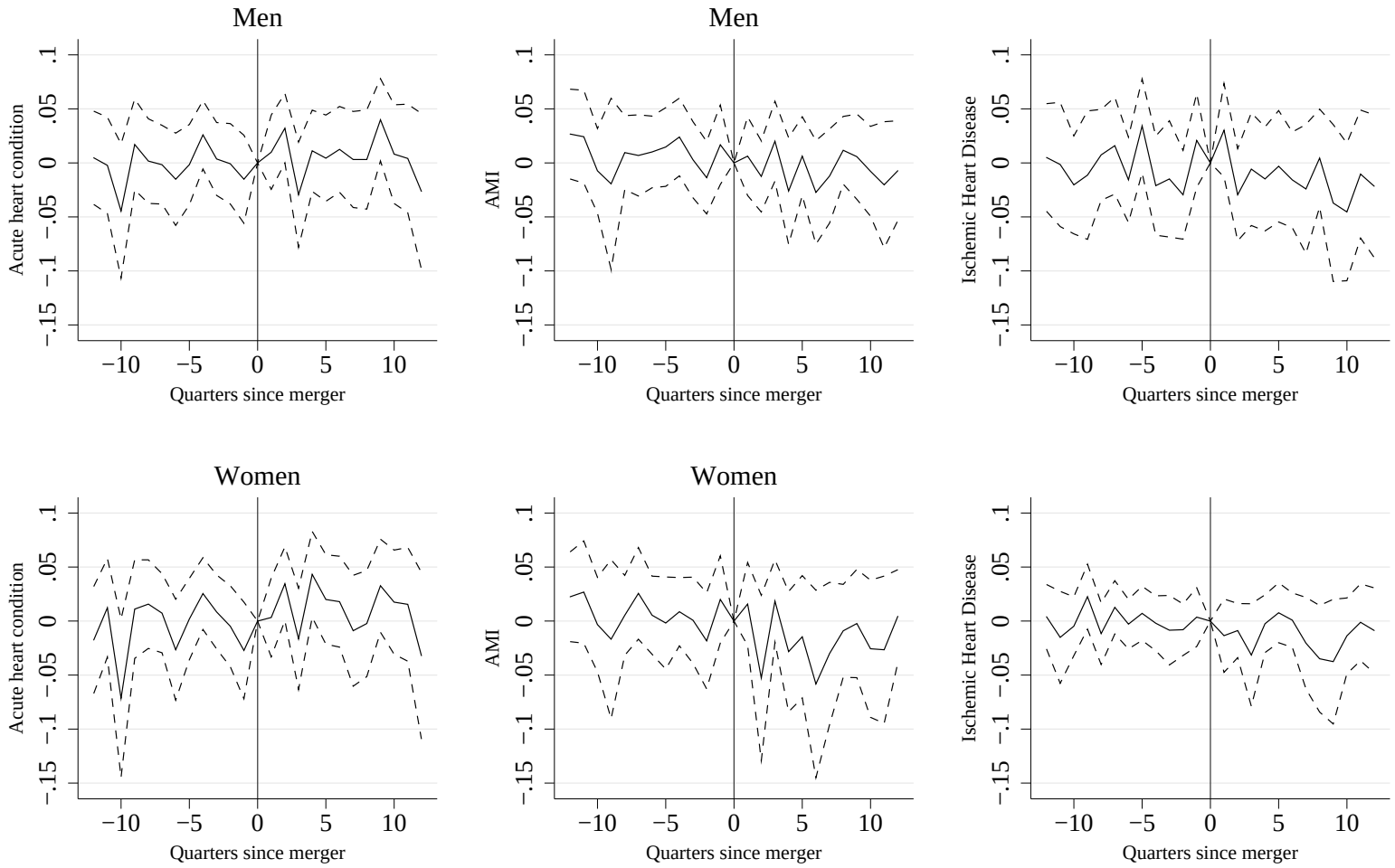
*Marginal effect estimates are reported as percentages/100 on the y-axis.

Figure 5: Period-specific acquisition marginal effects - diabetes*



*Marginal effect estimates are reported as percentages/100 on the y-axis.

Figure 6: Period-specific acquisition marginal effects - hypertension*



*Marginal effect estimates are reported as percentages/100 on the y-axis.

A Additional figures and tables

Table A-1: ICD9 Chapter headings

ICD9 Codes	Chapter Descriptions
001 − 139	Infectious And Parasitic Diseases
140 − 239	Neoplasms
240 − 279	Endocrine, Nutritional And Metabolic Diseases, And Immunity Disorders
280 − 289	Diseases Of The Blood And Blood-Forming Organs
290 − 319	Mental Disorders
320 − 359	Diseases Of The Nervous System
360 − 389	Diseases Of The Sense Organs
390 − 459	Diseases Of The Circulatory System
460 − 519	Diseases Of The Respiratory System
520 − 579	Diseases Of The Digestive System
580 − 629	Diseases Of The Genitourinary System
630 − 679	Complications Of Pregnancy, Childbirth, And The Puerperium
680 − 709	Diseases Of The Skin And Subcutaneous Tissue
710 − 739	Diseases Of The Musculoskeletal System And Connective Tissue
740 − 759	Congenital Anomalies
760 − 779	Certain Conditions Originating In The Perinatal Period
780 − 799	Symptoms, Signs, And Ill-Defined Conditions
800 − 999	Injury And Poisoning
E and V	External Causes Of Injury And Supplemental Classification

Table A-2: Hypertension and its complications

ICD	Description	Type
348.20	benign intracranial hypertension	Benign
401.10	benign essential hypertension	Benign
405.11	benign renovascular hypertension	Benign
405.19	other benign secondary hypertension	Benign
401.00	malignant essential hypertension	Malignant
365.04	ocular hypertension	Malignant
405.01	malignant renovascular hypertension	Malignant
405.09	other malignant secondary hypertension	Malignant
401.90	unspecified essential hypertension	Other
405.91	unspecified renovascular hypertension	Other
405.99	other unspecified secondary hypertension	Other
416.00	primary pulmonary hypertension	Other
459.30	chronic venous hypertension without complications	Other
459.31	chronic venous hypertension with ulcer	Other
459.32	chronic venous hypertension with inflammation	Other
459.33	chronic venous hypertension with ulcer and inflammation	Other
459.39	chronic venous hypertension with other complication	Other
572.30	portal hypertension	Other
796.20	elevated blood pressure reading without diagnosis of hypertension	Other

Table A-3: Diabetes and its complications

ICD Range	Description	Complication
250.00-250.03	diabetes mellitus without mention of complication	None
250.10-250.13	diabetes with ketoacidosis	Asymptomatic
250.20-250.23	diabetes with hyperosmolarity	Asymptomatic
250.30-250.33	diabetes with unspecified complication	Asymptomatic
250.40-250.43	diabetes with other coma	Symptomatic
250.50-250.53	diabetes with renal manifestations	Symptomatic
250.60-250.63	diabetes with ophthalmic manifestations	Symptomatic
250.70-250.73	diabetes with neurological manifestations	Symptomatic
250.80-250.83	diabetes with peripheral circulatory disorders	Symptomatic
250.90-250.93	diabetes with other specified manifestations	Symptomatic

Table A-4: Acquisitions considered

Announce Date	Buyer	Target	Tgt Type	Hosps	Physns	States
12-May-06	Butler Memorial Hospital	DiCuccio practice	Multi	1	9	PA
26-Mar-07**	Good Samaritan Hospital	NY Institute Same Day Surgery	Surg	1	2	NY, NJ
22-Jun-07	Presbyterian Healthcare	Mid Carolina Cardiology	Card	8	47	NC*
25-Jul-07	Aurora Health Care	Advanced Healthcare	Multi	12	253	WI*
7-Aug-07	Allina Hospitals and Clinics	Aspen Medical Group	Multi	12	165	MN*
15-Nov-07	ProHealth Care	Medical Assoc Health Ctr	Multi	2	95	WI*
30-Nov-07	Texas Children's Hospital	Women's Specialists Houston	OB/Gyn	1	11	TX
16-Jan-08	Essentia Health	Dakota Clinic	Multi	10	210	ND, MN, SD
3-Mar-08	Christ Hospital	Hyde Park Internists	Intern	1	9	OH
23-May-08	Jefferson Regional Mel Ctr	Jefferson Hills Surgical	Surg	1	7	PA
10-Jul-08	Aurora Health Care	Comprehensive Card Care Grp	Card	12	1	WI
4-Aug-08	North Memorial Health Care	Cardiovascular Consultants	Card	1	16	MN, WI*
25-Aug-08	Carilion Clinic	Consultants in Cardiology	Card	7	20	VA*
10-Sep-08	The Christ Hospital	Ohio Heart & Vascular Ctr	Multi	1	54	OH, KY, IN
26-Sep-08	OhioHealth Corp.	MidOhio Cardiology & Vascular	Multi	8	28	OH*
28-Nov-08	Bridgeport Hospital	Radiation Oncology South CT	Onc	1	10	CT*
8-Jan-09	Aurora Health Care	Northern Lake Medical	Fam	12	6	IL, WI
8-Jan-09	Scripps Health	Penn Elm Medical Group	Multi	4	11	CA*
1-Jun-09	ThedaCare	Nelson Family Clinic	Fam	3	2	WI
6-Jul-09	Spectrum Health System	Michigan Medical	Multi	5	216	MI*
27-Jul-09	Advocate Health Care	Midwest Physician Group	Multi	10	55	IL, IN*
27-Jul-09	Roper St. Francis Healthcare	Lowcountry Medical Assoc	Multi	2	146	SC*
1-Oct-09	Mission Medical Associates	Asheville Cardiology	Card	4	32	NC*
30-Oct-09	HCA Midwest Health System	Midwest Cardiology	Card	3	14	KS, MO*
1-Jan-10	St. David's HealthCare	Austin Heart	Card	4	6	TX*
21-Jan-10	Baptist Memorial Healthcare	NEA Clinic	Multi	1	26	AR, TX*
3-Feb-10	Legacy Comm Health Svcs	SW Community Health Ctr	Multi	2	20	CT*
26-May-10	St. Elizabeth Healthcare	Comprehensive Cardiology Cnsltnt	Card	3	14	OH, KY, IN*

*All state reported information is determined from Medicare claims data. Indicated rows have claims performed outside of listed areas.

Appendix Table A-4: NHIS codings for chronic conditions

NHIS Code	Chronic Disease Condition	ICD-9
100	SELECTED SKIN AND MUSCULOSKELETAL CONDITIONS	
101	Arthritis	714, 715, 716, 720.0, 721
102	Rheumatism, unspecified	729
103	Gout, including gouty arthritis	274
104	Sciatica (including lumbago)	724.2, 724.3
105	Intervertebral disc disorders	722
106	Bone spur/tendinitis NOS	726.9
107	Disorders of bone or cartilage	731.0, 731.2, 732, 733 730.9, 731.0, 731.2
108	Bunion	727.1
109	Bursitis, NEC	726.0, 726.1, 726.2, 726.3, 726.4, 726.5, 726.6, 726.7, 726.8, 727.0, 727.2, 727.3, 727.4, 727.5, 727.6, 727.7, 727.8, 727.9
110	Sebaceous skin cyst	706.2
111	Acne	706.0, 706.1
112	Psoriasis	696
113	Dermatitis	690-694
114	Dry (itching) skin NEC	698.9
115	Chronic ulcer of skin	707
116	Ingrown nails	703
117	Corns and calluses	700
118	Benign neoplasms of the skin	216
119	Malignant neoplasms of the skin	172, 173
200	IMPAIRMENTS	
201	Blind - Both eyes	X00
202	Other Visual Impairment	X01, X02, X03
203	Deaf - Both ears	X05
204	Other Hearing Impairments	X06, X07, X08, X09
205	Stammering and Stuttering	X10
206	Other Speech Impairments	X11
207	Impairment of Sensation	X12
208	Mental Retardation	X19
209	Absence - Both Arms/Hands	X20, X21
210	Absence - One Hand/Arm	X23, X24
211	Absence - Fingers - One or Both Hands	X22, X25
212	Absence - One or Both Legs	X26, X28
213	Absence - Feet/Toes - One or Both Limbs	X27, X29
214	Absence - Lung	X30
215	Absence - Kidney	X31
216	Absence - Breast	X32
217	Absence - Bone, Joint, Muscle of extremity	X34
218	Tips of Fingers, Toes	X35
219	Complete Paralysis - Entire Body	X40
220	Complete Paralysis- One Side of Body-Hemiplegia	X41
221	Complete Paralysis - Both Legs - Paraplegia	X46
222	Complete Paralysis - Other paralysis	X42, X43, X44, X45, X47, X48, X49
223	Partial Paralysis - Cerebral Palsy	X50
224	Partial Paralysis- One Side of Body-Hemiparesis	X51
225	Partial Paralysis-Legs - Both or Paraparesis	X56
226	Partial Paralysis - Other paralysis	X52, X53, X54, X55, X57, X58, X59
227	Paralysis - Complete or Partial - Other site	X60, X61, X62, X63, X64
228	Curvature or other deformity of back or spine	X70
229	Orthopedic Impairment	X80
230	Spina Bifida	X71
231	Hands, Fingers only	X74
232	Orthopedic Impairment-Shoulder(s)	X84
233	Other	X73
234	Flatfeet	X77

Continued on next page

Appendix Table A.4 – continued from previous page

NHIS Code	Chronic Disease Description	ICD-9
235	Clubfoot	X78
236	Other	X75, X76, X85, X86
237	Other Deformities/Orthopedic Impairment	X79, X89
238	Cleft Palate	X91
239	Color Blindness	368.5
240	Tinnitus	388.3
241	Cataracts	366
242	Glaucoma	365
243	Diseases of Retina	361, 362.1, 362.2, 362.3, 362.4, 362.5, 362.6, 362.7, 362.8, 362.9
300	SELECTED DIGESTIVE CONDITIONS	
301	Gallbladder stones	574
302	Liver diseases including cirrhosis	571, 572, 573.0, 573.3, 573.4, 573.5, 573.6, 573.7, 573.8, 573.9
303	Gastric ulcer	531
304	Duodenal ulcer	532
305	Peptic ulcer	533
306	Hernia of abdominal cavity	550-553
307	Disease of the esophagus	530
308	Gastritis and duodenitis	535
309	Indigestion	536.8
310	Other functional disorders of stomach and digestive system	536.0, 536.1, 536.2, 536.3, 536.4, 536.5, 536.6, 536.7, 536.9, 787
311	Enteritis and colitis	555, 556, 558
312	Spastic colon	564.1
313	Diverticula of intestines	562
314	Constipation	564
315	Other stomach and intestinal disorders	534, 537, 560, 569
316	Malignant neoplasms of stomach, intestines, colon, and rectum	151, 152, 153, 154
400	SELECTED CONDITIONS OF THE GENITOURINARY, ENDOCRINE, NERVOUS, METABOLIC AND BLOOD FORMING SYSTEMS	
401	Goiter	240, 241, 242.0, 242.1, 242.2, 242.3
402	Other disorders of the thyroid	242.4, 242.8, 242.9, 243, 244, 245, 246
403	Diabetes	250
404	Anemias	280-285
405	Epilepsy	345
406	Migraine headache	346
407	Other headache	784
408	Neuralgia or neuritis, unspecified	729.2
409	Kidney stones	592
410	Kidney infections	590
411	Other kidney trouble, NEC	581, 582, 583, 593
412	Bladder infections	595.0, 595.1, 595.2, 595.3, 595.8, 595.9
413	Other disorders of bladder	594.1, 596
414	Diseases of prostate	600, 601.0, 601.1, 601.2, 601.3, 601.5, 601.6, 601.7, 601.8, 601.9, 602
415	Multiple sclerosis	340
416	Inflammatory disease of female genital organs	614, 615, 616
417	Non-Inflammatory disease of female genital organs	620-624
418	Menstrual disorders	626
419	Other disorders of female genital organs	617-619, 625, 627, 628.0, 628.2, 628.3, 628.4, 628.5, 628.6, 628.7, 628.8, 28.9, 629.0, 629.1, 629.8
420	Female trouble, NOS	629.9
421	Malignant neoplasm of breast - female	174
422	Malignant neoplasms of female genital organs	179-184

Continued on next page

Appendix Table A.4 – continued from previous page

NHIS Code	Chronic Disease Description	ICD-9
423	Malignant neoplasm of prostate	185
424	Benign neoplasm of breast - female	217
425	Benign neoplasms of female genital organs	218-221
500	SELECTED CIRCULATORY CONDITIONS	
501	Rheumatic fever with or without heart disease	390, 392-399
502	Ischemic heart disease Heart rhythm disorders	413, 414, 410, 411, 412, 429.6
503	Tachycardia or rapid heart	427.0, 427.1, 427.2, 427.3, 785.0
504	Heart murmurs	785.2
505	Other and unspecified heart rhythm disorders	427.4, 427.5, 427.6, 427.8, 427.9, 785.1
506	Congenital heart disease	745, 746
507	Other selected diseases of heart	415-417, 420.9, 421.0, 421.9, 422.9, 423, 424, 425.0, 425.1, 425.2, 425.3, 425.4, 425.5, 425.9, 426, 428, 429.0, 429.1, 429.2, 429.3, 429.4, 429.5, 429.8, 429.9
508	High blood pressure (hypertension)	401-405
509	Cerebrovascular disease	430-435, 437
510	Hardening of the arteries	440
511	Aneurysm	441.0, 441.1, 441.2, 441.3, 441.4, 441.5, 441.6, 442
512	Phlebitis, thrombophlebitis	451
513	Varicose veins of lower extremities	454
514	Hemorrhoids	455
515	Poor circulation	459.8, 459.9
600	SELECTED RESPIRATORY CONDITIONS	
601	Chronic Bronchitis	490, 491
602	Asthma	493
603	Hay fever/allergic rhinitis without asthma	477
604	Nasal polyps	471
605	Chronic sinusitis	473
606	Deviated Nasal Septum	470
607	Chronic disease of tonsils and adenoids	474
608	Chronic laryngitis	476
609	Emphysema	492
610	Pleurisy	511
611	Pneumoconiosis and asbestosis	500-505
612	Tuberculosis (pulmonary)	011, 019
613	Malignant neoplasms of lung and bronchus	162.2, 162.3, 162.4, 162.5, 162.6, 162.7, 162.8, 162.9
614	Other diseases of lung	515, 518
615	Malignant neoplasms of other respiratory sites	160, 161, 162.0, 163

Table A-5: Summary and balance statistics for the sample of diabetics

	Pot Controls		Matched		Treated		Norm Diffs	
Variable	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Match**	**P.C.**
Propensity Score	0.020	0.041	0.139	0.117	0.147	0.126	0.064	1.352
				Patient demographics				
Age	75.867	7.775	76.880	7.369	76.761	7.241	-0.016	0.119
Male	0.416	0.493	0.428	0.495	0.426	0.494	-0.004	0.021
White	0.797	0.402	0.872	0.335	0.873	0.333	0.003	0.204
Metro >1 million	0.455	0.495	0.450	0.493	0.464	0.494	0.029	0.019
Metro 500k - 1 million	0.209	0.400	0.243	0.420	0.226	0.410	-0.041	0.041
Metro < 500k	0.111	0.304	0.098	0.285	0.088	0.268	-0.039	-0.082
Non-Metro area	0.224	0.409	0.209	0.398	0.222	0.401	0.033	-0.004
				Patient health				
Hypertension	0.831	0.375	0.858	0.349	0.858	0.349	0.000	0.074
Circulatory	0.893	0.309	0.920	0.271	0.924	0.265	0.013	0.107
Musculoskeletal	0.543	0.498	0.599	0.490	0.599	0.490	0.001	0.114
Sense organ diseases	0.348	0.476	0.371	0.483	0.368	0.482	-0.006	0.041
Gastrointestinal	0.317	0.465	0.360	0.480	0.363	0.481	0.005	0.096
Genito-urinary	0.271	0.444	0.295	0.456	0.294	0.456	-0.002	0.052
Blood disease	0.267	0.442	0.280	0.449	0.278	0.448	-0.004	0.026
Respiratory	0.258	0.438	0.307	0.461	0.305	0.460	-0.004	0.104
Signs/symptoms	0.245	0.430	0.295	0.456	0.292	0.455	-0.005	0.108
Skin conditions	0.216	0.411	0.241	0.428	0.241	0.428	0.001	0.061
Neoplasms (cancer)	0.123	0.329	0.136	0.343	0.139	0.346	0.007	0.046
				Provider characteristics				
Family Practice	0.624	0.484	0.711	0.453	0.706	0.455	-0.011	0.174
Other	0.438	0.496	0.508	0.500	0.502	0.500	-0.012	0.128
Diagnostic radiology	0.224	0.417	0.281	0.450	0.285	0.451	0.008	0.140
Cardiology	0.185	0.388	0.240	0.427	0.258	0.438	0.042	0.178
Podiatry	0.145	0.352	0.179	0.383	0.172	0.377	-0.018	0.075
Opthalmology	0.135	0.342	0.151	0.358	0.150	0.357	-0.003	0.042
Firm size <5	0.971	0.168	0.978	0.145	0.980	0.138	0.015	0.062
Firm size 5 -24	0.903	0.296	0.973	0.162	0.979	0.144	0.036	0.326
Firm size 25 - 49	0.637	0.481	0.831	0.375	0.838	0.368	0.020	0.470
Firm size 50 - 99	0.531	0.499	0.763	0.425	0.758	0.428	-0.011	0.488
Firm size 100 - 200	0.396	0.489	0.669	0.471	0.670	0.470	0.003	0.570
Firm size > 200	0.435	0.496	0.608	0.488	0.615	0.487	0.014	0.364
	10,236,345		279,205		278,375			

Table A-6: Summary and balance statistics for the sample of hypertensives

	Pot Controls		Matched		Treated		Norm Diffs	
Variable	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Match**	**P.C.**
Propensity Score	0.020	0.041	0.141	0.119	0.147	0.128	0.053	1.338
				Patient demographics				
Age	76.84	8.09	77.78	7.58	77.87	7.58	0.011	0.131
Male	0.367	0.482	0.379	0.485	0.378	0.485	-0.004	0.021
White	0.844	0.363	0.906	0.292	0.905	0.293	-0.003	0.184
Metro >1 million	0.444	0.494	0.459	0.494	0.469	0.495	0.019	0.051
Metro 500k - 1 million	0.214	0.402	0.244	0.421	0.230	0.412	-0.035	0.039
Metro < 500k	0.115	0.309	0.096	0.283	0.091	0.271	-0.018	-0.084
Non-Metro area	0.227	0.411	0.201	0.392	0.211	0.395	0.025	-0.040
				Patient health				
Diabetes	0.323	0.468	0.333	0.471	0.335	0.472	0.005	0.026
Musculoskeletal	0.556	0.497	0.610	0.488	0.610	0.488	0.000	0.109
Endocrine	0.480	0.500	0.494	0.500	0.497	0.500	0.005	0.033
Sense organ diseases	0.377	0.485	0.398	0.489	0.398	0.489	-0.001	0.043
Gastrointestinal	0.329	0.470	0.378	0.485	0.378	0.485	0.000	0.103
Genito-urinary	0.261	0.439	0.285	0.452	0.283	0.450	-0.005	0.049
Respiratory	0.255	0.436	0.299	0.458	0.296	0.457	-0.007	0.092
Signs/symptoms	0.245	0.430	0.292	0.455	0.293	0.455	0.002	0.108
Blood disease	0.222	0.415	0.239	0.426	0.239	0.426	-0.001	0.040
Skin conditions	0.186	0.389	0.204	0.403	0.206	0.405	0.004	0.052
Neoplasms (cancer)	0.132	0.338	0.147	0.354	0.149	0.356	0.008	0.051
				Provider characteristics				
Family Practice	0.604	0.489	0.683	0.465	0.681	0.466	-0.004	0.161
Other	0.428	0.495	0.495	0.500	0.491	0.500	-0.007	0.127
Diagnostic radiology	0.225	0.417	0.279	0.449	0.282	0.450	0.005	0.131
Cardiology	0.176	0.381	0.231	0.421	0.246	0.431	0.035	0.171
Opthalmology	0.138	0.345	0.153	0.360	0.155	0.361	0.005	0.046
Podiatry	0.101	0.301	0.125	0.331	0.121	0.326	-0.014	0.064
Firm size <5	0.968	0.176	0.975	0.155	0.977	0.150	0.010	0.054
Firm size 5 -24	0.904	0.294	0.970	0.170	0.974	0.158	0.024	0.297
Firm size 25 - 49	0.631	0.483	0.821	0.384	0.825	0.380	0.012	0.448
Firm size 50 - 99	0.522	0.500	0.747	0.435	0.749	0.434	0.003	0.485
Firm size 100 - 200	0.389	0.488	0.664	0.472	0.659	0.474	-0.011	0.561
Firm size > 200	0.431	0.495	0.607	0.488	0.611	0.488	0.008	0.365
	26,333,915		719,084		712,025			