Essays on the Industrial Organization of Health Care Markets

by

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Dissertation submitted in partial fulfillment of the
requirements for the degree of Doctor of Philosophy
in the Department of Economics
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ABSTRACT

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Abstract

The way in which health care providers make treatment decisions and the incentives which drive these choices are the subject of much policy and research discussion. Financial incentives have been used to steer provider treatments to more cost-effective options, such as in Medicare's recent Accountable Care Organization model, while acquisition of providers by a firm can provide incentives for providers to treat patients differently than prior to acquisition. In this dissertation, I use a variety of administrative data sources to study the effects of these financial incentives on both physicians and dialysis clinics.

In Chapter 1, I study the effects of integration between referring physicians and specialists in cardiology. To address concerns of endogeneity of integration, I exploit a change in Medicare payment rates which increased the financial benefit to vertically integrating for cardiologists. Instrumental variables estimates show that cardiologists who work in the same practice as cardiac surgeons are 7.7% more likely to refer patients for surgery rather than more conservative options. Patients diagnosed by integrated cardiologists in turn have worse mortality and readmission outcomes, with 18.7% higher mortality risk and 13.4% higher risk of readmission for AMI within 180 days. This is in spite of the fact that patients diagnosed by integrated cardiologists have 7.8% higher medical spending in the 180 days following diagnosis. I provide evidence that these effects are not driven by inherent risks of invasive surgery or selection on patient observables, but worse outcomes for patients receiving the most conservative treatment option.

In Chapters 2 and 3, which are joint with Paul Eliason, Ryan McDevitt, and James Roberts, we use a rich panel of Medicare claims data for nearly one million

dialysis patients to advance the literature on the effects of mergers and acquisitions by studying the precise ways in which providers change their behavior following an acquisition and the effects of bundled payment reforms. We base our empirical analysis on more than 1,200 acquisitions of independent dialysis facilities by large chains over a twelve-year period and find that chains transfer several prominent strategies to the facilities they acquire. Most notably, acquired facilities converge to the behavior of their new parent companies by increasing patients' doses of highly reimbursed drugs, replacing high-skill nurses with less-skilled technicians, and waitlisting fewer patients for kidney transplants. We then show that patients fare worse as a result of these changes: outcomes such as hospitalizations and mortality deteriorate, with our long panel allowing us to identify these effects from within-facility or within-patient variation around the acquisitions. Because overall Medicare spending increases at acquired facilities, mostly as a result of higher drug reimbursements, this decline in quality corresponds to a decline in value for payers. We conclude the paper by considering the channels through which acquisitions produce such large changes in provider behavior and outcomes, finding that increased market power cannot explain the decline in quality. Rather, the adoption of the acquiring firm's strategies and practices drives our main results, with greater economies of scale for drug purchasing responsible for more than half of the change in profits following an acquisition.

Chapter 3 studies the effect of a bundled payment reform in 2011 for dialysis providers. Using an instrumental variables strategy, exploiting a biological interaction between a patient's elevation of residence and their health outcomes, we show that bundled payment reform yielded better hospitalization outcomes for patients, but worse transfusion outcomes. This is consistent with the decreased use of drugs to prevent blood transfusions observed after the reform. In addition, we find significant patient and firm heterogeneity in responses.

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Chapter 1

Introduction

Over the last two decades, healthcare markets in the United States have become increasingly consolidated, while payers have experimented with a variety of alternative payment schemes intended to alter provider behavior. This integration has been both horizontal, such as the hospital mergers, and vertical, such as the rapid acquisition of physician practices by hospitals and multispecialty practices. While there is a long literature studying the effects of horizontal market concentration and mergers in health care, particularly in the hospital industry, other forms of integration are not as well understood.

Of particular interest to both researchers and policymakers is the integration of physicians with other types of providers, due to the unique role physicians play in health care. One of the primary functions of many physicians, especially those serving in specialty fields, is to provide care to patients. However, physicians also serve as the primary source of information for patients about the types of care they need and which providers to seek care from, providing referrals to appropriate providers. For many patients, a physician's recommendation is second only to inclusion in a patient's insurance network when choosing a provider. (Arrow, 1963; Ziemba et al., 2017)

Partly because physicians serve multiple functions within the health care system, effects of such forms of integration are ambiguous. Proponents argue that integration of different types of providers will enable physicians to coordinate care more effec-

tively, improving patient outcomes, while reducing costs.[1] This principle underpins several attempts at payment reform, such as Medicare's Accountable Care Organizations and the Alternative Quality Contract in Massachusetts. In contrast, opponents argue that integration will lead to higher prices, by enhancing providers' market power and lead to patients being steered to in-system providers and treatments.[2] In Chapter 1, I use an instrumental variables strategy to investigate the effects of such integration on the choice of treatments for patients in the cardiology sector.

Another form of integration which has been the subject of much scrutiny is in the market for outpatient dialysis. Over the last two decades, the two largest chains, DaVita and Fresenius, have acquired a number of facilities and now control roughly two-thirds of all facilities in the United States. In Chapter 2, which is joint work accepted for publication in *The Quarterly Journal of Economics* with Paul Eliason, Ryan McDeVitt, and James Roberts, we study how these acquisitions changed facility treatment decisions.

Chapter 3 studies the effects of a recent reform in the dialysis industry which bundled all payments for a dialysis session. Medicare has recently experimented with using bundled payments to restrain the costs of reimbursing providers. Under a bundled payment model, health care providers receive a single payment for the care they provide during the course of a patients treatment, such as a joint replacement or cardiac rehabilitation, irrespective of the actual costs incurred. By holding multiple parties accountable for the cost and quality of care, proponents claim that a bundled payment system will encourage coordination among providers and reduce unnecessary expenses. At the same time, providers that receive a fixed payment irrespective

[1] Cutler (2010)

[2] King and Brown (2016)

of the amount of care they provide may face an incentive to under-treat patients because additional expenses do not yield additional reimbursements, in stark contrast to Medicares traditional fee-for-service model. Despite the inherent tradeoffs associated with a bundled payment system, and despite its growing prominence in Medicares push for alternative payment models, little empirical work has examined the precise channels through which bundled payments alter providers behavior. In this paper, we use detailed claims data from dialysis patients to show how the allocation of resources changes following the adoption of a bundled payment system and the resulting effect on health outcomes and providers' profits.

We use the differences in elevation and wholesale prices for EPO to isolate the causal effect of bundled payments on providers' behavior and patients' outcomes. Our empirical strategy offers several advantages over previous studies of this topic that have mostly used observational data to analyze the effect of bundled payments on a small number of hospitals that voluntarily participated in the program. Although these studies typically find large savings associated with the payment reform, they cannot determine causality because the hospitals that selectively opt into bundled payments may have been particularly well suited to achieve savings, biasing their estimates. Our identification strategy allows us to overcome such confounds because (i) facilities faced different incentives to change their EPO doses following the reform due to differences in costs and (ii) patients faced different changes in EPO doses due to their elevations. As both of these sources of variation are independent of the policy reform, we can use them to cleanly identify the causal impact of bundled payments on behavior and outcomes.

Chapter 2

Vertical Integration and Treatment Choices: Evidence from Cardiologists

In the United States, financial integration is one of the only tools for health care providers to incentivize referring physicians, due to regulations known as Stark Law. These regulations prohibit physicians from referring to health care entities in which they have a financial interest. However, Stark Law provides an exception for "bona fide employment arrangements" (42 CFR 411.357(c)). In other words, physicians are not barred from referring to an entity if they are directly employed by it. While they are still not able to provide direct financial incentives, in the form of payments for referring patients to in-system providers, firms can impose indirect and informal incentives. For instance, physicians may be paid partly in revenue sharing or profit sharing agreements, which provides an indirect incentive to refer patients in-house. Alternatively, physicians may come under informal social pressure for not referring enough patients in-house. At their most extreme, health care systems may even attempt to prohibit referrals to outside providers. For instance, Steward Health was accused of cancelling a patient's scheduled treatment at an out-of-system hospital for a treatment Steward did not provide in a lawsuit filed in 2018. (Kowalczyk, 2018)

This paper empirically examines the role of integration between cardiologists and cardiac surgeons in the choice of treatment for cardiac patients. Using Medicare data on the diagnosis and treatment of cardiac patients, I find that patients are 7.7% more

likely to receive surgical interventions following diagnosis by an integrated cardiologist. Consistent with prior literature, this shift to surgical interventions increases healthcare utilization. However, my results also show that patients have 18.7% higher mortality risk and 13.4% higher readmission risk. Lastly, I show that these are not driven by inherent risks to undergoing surgery. Instead, my results suggest medically managed patients receive less effective care at integrated cardiologists.

One of the primary difficulties in studying integration of any sort is the lack of plausibly exogenous variation. To ameliorate these concerns, I use an instrumental variables strategy to show my results are robust to the most likely sources of endogeneity. To do so, I exploit a change in Medicare payment rates in 2010, which changed the financial benefits to billing in hospital outpatient departments for many physicians. This variation is driven by an update to the underlying cost estimates used by CMS to set physician payment rates in 2010, an update also studied in Dranove and Ody (2019).

Using the predicted probability of vertical integration from the logit first-stage as an instrument for integration, I first investigate how integration affects choice of treatment. Conditional on patient risk characteristics, I find the likelihood of surgical intervention increases by approximately 7.7% due to vertical integration. While likelihood of surgery increases, patients are steered away from medical management, the most conservative treatment option, rather than reallocated between interventional treatment options.

After establishing that patients receive more surgery overall, I show that inpatient utilization is $\$1,879$ higher for patients diagnosed by vertically integrated cardiologists. Decomposing this spending into separate categories, I find that this increase is largely driven by an increase in inpatient spending due to higher hospital

readmission rates.

Next, I turn my attention to how patient hospitalization and mortality outcomes differ, finding worse outcomes for both, with patients approximately $0.83pp$ more likely to die and $0.51pp$ more likely to be readmitted to the hospital for a heart attack within 180 days of diagnosis. There are a number of possible explanations for this finding. First, since interventional procedures carry inherent risks, this may be simply a side effect of increased interventions. However, I find that outcomes worsen conditional on the type of treatment, suggesting this is not the primary driver. Another possibility is that the patient mix diagnosed by vertically integrated cardiologists is different than that seen by non-integrated cardiologists, however they appear similar along observable dimensions.

Despite concern by policy makers and industry insiders about the effects of vertical integration on patient choice, academic literature has largely focused on its price and outcome effects. For instance, it is well documented that vertical mergers are associated with higher spending and prices (Capps et al., 2017a; Baker et al., 2014). Despite increased spending, there is limited evidence of improvement in patient outcomes due to integration (Koch et al., 2018).

While these issues are undoubtedly important, relatively little work has been done studying the referral effects of vertical acquisitions. Two recent works in this small literature are Baker et al. (2016), who find that ownership of a patient's primary physician by a hospital increases the likelihoodof choosing that hospital and reduces the cost-sensitivity of the choice function to zero. The other recent work in this literature is Brot-Goldberg and de Vaan (2018), who uses Massachusetts data to study how integration impacts the choice of orthopedist and patient utilization after diagnosis. They find that patients referred in practice have lower utilization,

but that steering effects are extremely strong, accounting for approximately half of all in-practice referrals. In contrast, cost efficiencies have almost no impact on specialist choice, due to cost insensitivity of primary care physicians and consumers when choosing providers. In contrast to this work, I focus on the initial choice of treatment, rather than the choice of provider. While they have a section on the margin whether to receive surgery, the primary focus of the work is on selection of surgeon conditional on getting surgery.

The most closely related work to this one is Afendulis and Kessler (2007), who study how treatment decisions of cardiologists differ when they perform interventional services in addition to diagnostic services. Their results show that diagnosis by an interventional cardiologist increases a patient's utilization post-diagnosis, but also improves outcomes, suggesting they are better able to allocate sicker patients to appropriate care. My work differs from theirs in two fundamental ways. First, their study uses geographic variation in a two-stage procedure[1] to identify the effect of integration, while I use variation in integration generated by changes in Medicare payment rates to identify the effect of integration. At a more fundamental level, the form of integration they study is arguably less relevant to policymakers because it is not driven acquisitions and other areas of health care research than the integration I study.

This paper also contributes to the sizable literature showing financial incentives to healthcare providers has large impacts on patient steering. Much of this literature has focused on alternative payment contracts, such as Ho and Pakes (2014), who study capitation contracts in California, showing that such contracts increase cost sensitivity without reducing outcomes. Similarly, Song et al. (2011) find that the Alternative

[1]Their procedure is similar to two-stage least squares, but differs in that the second stage of their choice model is a multinomial logit, rather than a linear model.

Quality Contract in Massachusetts reduced beneficiary spending, primarily by shifting specialist referrals to lower-cost providers.

Lastly, this paper builds on a large literature documenting provider responses to Medicare payment rules. For instance, Capps et al. (2017a) find that physicians who are acquired by hospitals shift their billing to facility based settings in order to take advantage of higher facility-based payment rates. In closely related work, Dranove and Ody (2019) find that a 2010 update to data used to Medicare physician payments led to an increase in hospital employment in physicians, by increasing the gap between facility and office based payment amounts. This is the same variation I exploit as exogenous variation in vertical integration for cardiologists. Studying long-term care hospitals, Eliason et al. (2018) and Einav et al. (2017) both study the impact of a discontinuity in provider reimbursements, finding that hospitals disproportionately discharge patients immediately after receiving a lump sum payment for care.

2.1 Empirical Setting

2.1.1 Cardiac Catheterizations

Coronary angiography, or diagnostic cardiac catheterization, is a procedure developed in the 1950's in which a doctor inserts a catheter into a patient's groin and uses injectable dye to diagnose blockages in a patient's heart. Once the catheter is inserted, the doctor uses dye and an imaging machine to locate blockages in the patient's arteries.

After the doctor has located any potential blockages, they recommend a further

course of treatment. After diagnosis, there are three treatment options available to patients. The most inexpensive and least invasive option is medical management of symptoms. This course of treatment consists primarily of using prescription drugs to break up any blockages and reduce the potential negative effects of future blockages, should they reoccur. This is the treatment choice for apprximately two-thirds of patients, especially those diagnosed in an elective setting.

The most common alternative to medical treatment for arterial blockages is an interventional catherization,[2] called percutaneous coronary intervention (PCI). This procedure involves inserting a catheter into a patients arteries, in a similar fashion to diagnostic catheterizations. Afterwards, the physician removes the blockage by inserting a small balloon and expanding it and potentially inserting a stent. Frequently, these are performed in the same procedure and by the same physician as the diagnostic catheterization. Cardiologists who perform both PCI's and diagnostic catheterizations are called interventional cardiologists, though in recent years, the vast majority of cardiologists are interventional.

Finally, patients may be referred to a cardiac surgeon for cardiac arterial bypass graft (CABG) surgery. CABG is a surgical procedure in which arteries from a different location of the body, such as the leg, are grafted to bypass blockages in the arteries surrounding the heart. For many years, this was the treatment of choice for patients requiring cardiac intervention. Since the mid-1990's, however, U.S. CABG volumes have been on a consistent decline. Importantly, the vast majority of patients do not regularly interact with cardiac surgeons. Instead, they are advised on their options by their cardiologist, who would recommend one of the available treatment options.

[2]For the purposes of this paper, I will use "cardiac catheterizations" or "catheterizations" to refer to diagnostic catheterizations, rather than interventional catheterizations.

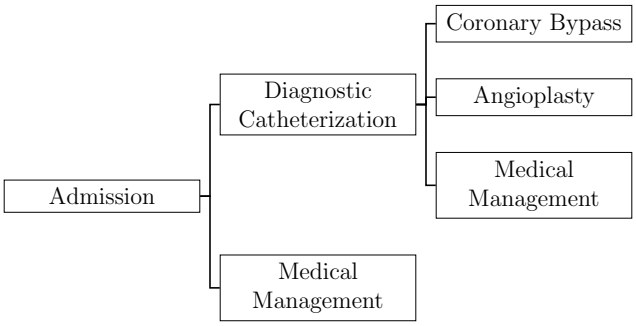


Figure 2.1: Cardiac treatment decision tree. Adapted from Cutler, McClellan, and Newhouse (2000).

Guidelines for the treatment of heart attack and angina are regularly published by the American College of Cardiology and American Heart Association. (Antman et al., 2008; Anderson et al., 2007) Medical management is preferred for low-risk female patients and patients who have high risk of adverse clinical events, for example. Angioplasty is the most common form of intervention chosen, though several factors can make bypass more appropriate, such as multivessel coronary artery disease or diabetes mellitus.

Despite the detailed nature of these guidelines, there is no set algorithm for selecting a treatment. This lack of a set algorithm leaves significant latitude for physician judgement in the choice of treatment. This is made clear in the guidelines: "Although general guidelines can be offered, individual judgment is required." (Anderson et al., 2007) It is unlikely physicians frequently recommend wholly unsuitable treatments. Physicians treating patients on the margin, however, may be swayed by the underlying financial incentives.

2.1.2 Vertical Integration in Cardiology

This setting also provides the distinct advantage that vertical integration has increased substantially during the study period. In my data, the portion of procedures performed by cardiologists who work in the same practice as surgeons increased from 15.0% in Q1 2008 to 27.3% in Q4 2012. This is in line with other studies, such as Nikpay et al. (2018) which found that hospital employment of cardiologists nearly tripled from 2007-2017.

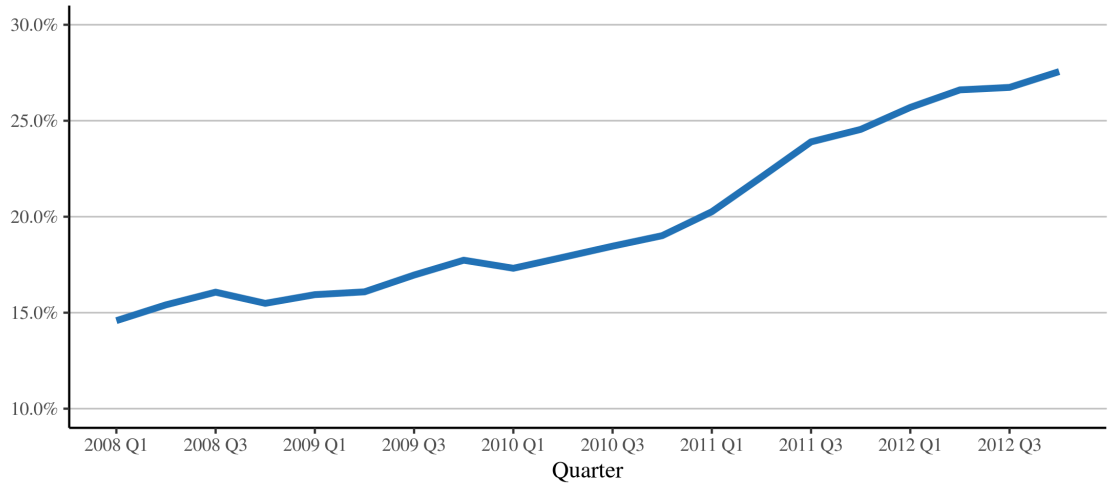


Figure 2.2: Share of Catheterizations Performed by Vertically Integrated Cardiologists

All physician specialties have increased their level of integration, similar to all of healthcare, but cardiology experienced a uniquely high rate of change, largely due to changes in Medicare fee structures. In 2010, the Center for Medicare and Medicaid Services (CMS) introduced a change in physician payments which substantially increased the benefit to billing in a hospital outpatient department relative to a physician office. There is substantial evidence suggesting this led to cardiologists selling their practices to hospitals to take advantage of higher hospital payment rates.(Song

et al., 2015; Dranove and Ody, 2019; American College of Cardiology, 2010)

2.2 Data

The primary data for this analysis are the Medicare Carrier 20% file from 2008-2012. These data consist of all physician services claims for a 20% sample of enrolled Medicare Part B beneficiaries. Each claim contains identifiers for the physician and practice billing for the procedure, along with detailed diagnosis and procedure information. These data are well suited to this study for a number of reasons. First, diagnostic catheterizations are an extremely common procedure in the Medicare population, with roughly half of all procedures being performed on Medicare patients. This not only provides a large sample size, but allows me to accurately determine the set of cardiologists and cardiac surgeons performing at a given point in time. Second, detailed diagnostic information allow me to include detailed controls for differences in patients' health history. I supplement these data with medical claims from non-physician sources of care (e.g. inpatient claims) in order to construct outcome and risk measures for each patient.

2.2.1 Primary Variable Construction

Treatments Using Common Procedural Terminology (CPT) codes, I identify all diagnostic catheterization, PCI, and CABG claims in the Carrier claims file.[3] Each catheterization is assigned to the treatment they receive in the month following diagnosis. If a diagnosis is followed by both PCI and CABG claims, they are assigned

[3]See Appendix A.2 for a list of CPT codes used to identify each procedure.

to CABG.[4] I exclude any claims which are denied.

Physician Employment Included on each claim in the Carrier file is the Tax Identification Number (TIN) of the practice billing for the claim, along with the National Provider Identifier (NPI) for the performing physician. Following other work on physician acquisitions, I use the TIN as the firm definition for this study (e.g. Capps et al., 2017a; Walden, 2016). I leverage these two identifiers to construct a panel of physician employment by assigning each physician to the practice in which they bill the most claims in each quarter.[5]

I define any practice which employs both cardiologists and cardiac surgeons in the current quarter as vertically integrated. In order to be classified as a cardiac surgeon, a physician must report a specialty of general, cardiac, or thoracic surgery and perform at least 5 cardiac bypass procedures during the entirety of my sample.[6] This differs slightly from the definition used in Dranove and Ody (2019), which defines vertical integration as ownership of a practice by a hospital. Since surgical specialists are among the most likely specialties to work for hospitals, these two measures will be correlated, but not identical.[7]

This measure of integration is subject to notable measurement error, because two practices which are owned by the same firm do not necessarily bill under the same Tax ID. One common example of this is for hospitals to have different Tax ID's for the practices of each of its specialties. It is difficult to directly determine by how much this measure of integration understates the true value. However, Baker et al. (2018)

[4]This is consistent with the assignment used in Culler et al. (2015).

[5]This is not a problematic assumption, as $> 90\%$ of physician-quarters bill under a single TIN.

[6]Specifically, I require they report a Healthcare Provider Taxonomy Code (HPTC) of 03, 33, or 78.

[7]Nikpay et al. (2018)

compares measures of HHI using TIN's in Medicare claims with those calculated using hospital and system ownership information from a commercial dataset from SK&A, yielding very similar values, especially in cardiology. While not the exact measure of integration I am using, this suggests measurement error is likely small. Nevertheless, using an IV addresses such concerns.

Patient And Hospital Characteristics If integrated cardiologists see patients who are systematically different than those seen by non-integrated cardiologists, it would be natural for them to recommend different treatment options and have different outcomes. To account for this, I follow the medical literature to adjust for patient risk.(Elixhauser et al., 1998) Specifically, I calculate the Elixhauser Index for each diagnosis, along with indicators for the existence of Elixhauser comorbidities using the International Classification of Diseases (ICD-9) codes from all claims for the patient in the 12 months leading up to diagnosis.[8] These variables, along with patient sex, risk, and race are used to risk-adjust for patient characteristics.

Additionally, I control for regional sociodemographic characteristics using data from the Area Health Resources File (AHRF). Specifically, I control for the percent of adults with at least a bachelor's degree, median age, and median income for the county in which a patient resides. Lastly, using data from the Provider of Services file provided by CMS, I include the bed count, for-profit status and teaching status of the hospital at which the cath occurs.

Patient Outcomes I focus on three primary patient outcome measures: hospital readmission, healthcare utilization, and mortality. I measure utilization as total payments for the patient from the date of diagnosis and until the end of the observation

[8]This is consistent with prior studies, such as Afendulis and Kessler (2007) and Brot-Goldberg and de Vaan (2018).

window. Because this includes the diagnosis and any treatments, this will include any spending differences attributable to treatment choices. Hospital readmissions are identified ignoring the month following diagnosis, as these admissions are potentially part of the original course of treatment.

2.2.2 Sample Restrictions

After constructing the overall panel of diagnoses and treatments, I restrict the sample in a number of ways. I first drop any diagnoses which are performed by cardiac surgeons, to remove potential issues arising from integration of the same physician. Second, I drop any observations where the billing provider is not a cardiologist. This excludes claims where a group practice is listed as the billing provider or where a non-cardiologist physician performs the procedure, such as a radiologist.[9] Lastly, I drop any claims with a diagnosis code indicating ST-elevated myocardial infarction[10], as there is clear guidance for many of these patients to receive interventional catheterization upon reaching the hospital, leaving less scope for physician decision making.[11] Lastly, to ensure accurate construction of risk and outcome measures, I restrict attention to those patients who were continuously enrolled in Medicare Parts A & B for the 12 months prior to diagnosis in order to accurately construct measures of patient severity. Lastly, when considering 180-day outcomes, I restrict attention to those who are continuously enrolled in Medicare for 180 days following diagnosis or until their death.

In Table 2.1, I show baseline summary statistics for this sample. Imposing

[9]Specifically, providers must report an HPTC code of 06 or C3 and an entity type 01.

[10]ICD codes 410.x, other than 410.7x.

[11]ACC/AHA Guidelines state: "Primary PCI should be performed in patients with STEMI and ischemic symptoms of less than 12 hours duration." Antman et al. (2008)

these sample restrictions generates a sample of 675,789 catheterizations performed by 12,308 cardiologists. Of these, I observe 2,155 perform catheterizations before and after integration with a surgeon. From this table, it is notable that mortality and readmission are both rare events, with 4.45% of patients dying and 3.77% of patients being readmitted within 180 days. Additionally, spending is very high for these patients, at $24,063 on average in the 180 days following diagnosis.

Table 2.1: Summary Statistics

Treatment	
Medical Management (%)	64.7
PCI (%)	27.0
CABG (%)	8.3
180-Day Outcomes	
Mortality (%)	4.5
Heart Attack Readmission (%)	3.8
Spending ($)	24,063
	(30,328)
Full Sample Observations	675,789
180-Day Sample Observations	589,166
Cardiologists	12,308
Cardiologist Switchers	2,115

Notes: An observation is a Medicare patient undergoing diagnostic catheterization. The 180-Day sample consists of patients who survive and are continuously enrolled in Medicare for 180 days following diagnosis. Switchers are those cardiologists who perform at least one in-sample catheterization before and after integrating with a surgeon.

2.3 Empirical Strategy

Identifying the effect of integration on treatment choices is difficult, as there is substantial heterogeneity in both patients and physicians. While I posess very detailed data on patient characteristics, this does not control for all potential sources of het-

erogeneity. There are two very likely sources of endogeneity. First, patients with a high propensity for surgery may seek out integrated physicians, biasing the estimate of β upwards. Second, physicians who are more likely to refer patients for surgery may be more likely to integrate with surgeons. While this is by no means an exhaustive list of potential sources of endogeneity, these examples illustrate the cause for concern. Such concerns are exceedingly common in studies of integration effects, both in healthcare and industrial organization more broadly, with a variety of methods utilized to address them.[12]

To address these concerns, I instrument for integration with a surgeon using a physician's exposure to a 2010 update to the Medicare Physician Fee Schedule.

2.3.1 Medicare Physician Fee Schedule

The Center for Medicare and Medicaid Services (CMS) administratively sets prices for physician services each year according to their estimate of the relative cost of providing each procedure.[13][14] To do so, CMS assigns each procedure a number of Relative Value Units (RVUs) based on four factors: the physician work required to perform the procedure, estimates of the practice expenses associated with a procedure, the malpractice risk associated with the procedure, and the location in which the procedure is perfrmed. While a procedure's malpractice and work RVU's do not vary by location, CMS assigns different RVU's to many procedures when they are

[12]For such examples in healthcare: Afendulis and Kessler (2007); Dunn and Shapiro (2014); Kessler and McClellan (2000a); Gaynor (2004); Dafny (2009a); Capps (2005a); Hayford (2012a); Cutler et al. (2017a). For examples in non-healthcare settings, see: Prince and Simon (2017); Fan (2013); Hortasu and Syverson (2007); Januszewski Forbes and Lederman (2009)

[13]These fees are occasionally updated mid-year. In years where there are multiple, I use the January MPFS file.

[14]For the purposes of this section, procedure will be used to mean CPT code.

billed in a facility setting rather than an office setting. The sum of these RVU's are then multiplied by an RVU-to-dollar conversion factor to to generate the physician payment rate for a procedure. Thus, for procedure p in location l, the payment made under the Physician Fee Schedule is:[15][16]

$$PFS_{lp} = \left[RVU_p^{Work} + RVU_{lp}^{PE} + RVU_p^{MP}\right] ConvFac \tag{2.1}$$

where RVU^{Work} denotes work RVUs, RVU^{PE} practice xpense RVUs, and RVU^{MP} malpractice RVUs. $ConvFac$ is the RVU-to-dollar conversion factor. All of these values are updated annually according to established CMS methodology that is described in the Federal Register.[17] For the purposes of this paper, I take RVU^{Work} and RVU^{MP} as given, ignoring methodology for calculating them, as there are no substantive changes during this time period. Instead, I focus on variation in RVU^{PE} generated by CMS updating its estimates of practice expenses for providing procedures.

Practice expense RVU's can be decomposed into two portions: direct expense RVUs and indirect expense RVUs. Direct expense RVU's for each procedure are estimated by the American Medical Association (AMA) and may vary between office and facility settings. Indirect expenses assigned to a procedure are a multiple of its direct practice expenses. Each multiplier is calculated using survey estimates of the ratio of indirect to direct expenses for each specialty, where the multiplier is equal to the weighted average of the indirect-to-direct expense ratios for the specialties which perform the procedure.

[15] All RVU's are adjusted for geography, which I ignore for expositional purposes.

[16] Throughout the section, I ignore CMS's multiple rounds of budget balancing to ensure statutory spending requirements are met.

[17] See CMS-1403-FC for an example.

From 1999 through 2009, CMS used estimates of these indirect-to-direct cost ratios from the AMA's Socioeconomic Monitoring System (SMS) survey, along with supplemental surveys from approximately a dozen specialties. These data had two large issues. First, by 2009, they were extremely out of date, having been collected from 1995-1999. To the extent the costs of practicing medicine (in particular, the ratio of direct to indirect costs) changed since the survey was conducted, they did not provide an accurate representation of the cost to physicians of providing service to patients. Second, the specialty data were extremely coarse. Supplemental data were provided to allocate specialty-specific costs to approximately 30 specialties, but many specialties did not have specialty specific cost data and were crosswalked to a similar specialty. To address both these concerns, CMS conducted the Physician Practice Information Survey (PPIS) from 2007-2008, which collected new data on the cost of providing medical service for many specialties. Expense ratio estimates under the PPIS were significantly different than the SMS for many specialties, which in turn generated large changes in the fees paid for many procedures. Of particular interest for this paper, procedures commonly performed by cardiologists were among the most affected by this update; for example, from 2009 to 2013, the payment rate for an echocardiogram performed in an office setting fell by 16%.

Many of the procedures which were subject to large fee reductions after the PPIS did not experience similarly large fee reductions when billed in a hospital outpatient department. For many procedures, CMS allows location-based billing. A procedure when performed in an office-setting would be paid PFS_{op} while that same procedure perfomed in a facility setting would be paid $PFS_{fp}+OPPS_p$ where PFS_{lp} is the PFS rate for procedure p in location $l \in \{\text{office}, \text{facility}\}$ and $OPPS_p$ is the payment for procedure p under the Outpatient Prospective Payment System (OPPS). Typically, it is more attractive to bill for a procedure in a facility setting, because total payments

are higher, even though the cost of providing the procedure can be similar (or identical).[18] Since these total facility-based payments did not decrease as substantially, the relative gains to billing in a outpatient department increased for many cardiac procedures.[19]

Many observers, including the American College of Cardiology, predicted this would lead to rapid vertical integration in the cardiology specialty, as payments to cardiologists decreased and the relative value of working for a hospital increased. Consistent with this, cardiology experienced a large uptick in the percentage of procedures billed in an outpatient deparment (Song et al (2015)) and physicians directly employed by hospitals, rising from 8% in 2007 to 42% in 2017. (Nikpay et al (2018))

I define the price paid for procedure p in location l to be:

$$p_{lp} = \begin{cases} PFS_{op}, & l = \text{office} \\ PFS_{fp} + OPPS_p, & l = \text{facility} \end{cases} \tag{2.2}$$

I also define the facility markup for procedure p as

$$\eta_p := p_{fp} - p_{op}$$

The facility markup, η_p, is the additional amount of Medicare payments a physician could generate by billing for procedure p in a facility setting rather than an office based setting. Combining this with a physician's observed procedure mix, I calculate

[18] For example, in 2009, CMS paid 43.9% more for an echocardiogram performed in an outpatient department than an office based setting.

[19] In 2013, CMS paid 141% more for an echocardiogram performed in a facility setting than an office based setting.

the mount of additional Medicare payments physician j could generate by shifting their billing entirely from an office-based setting to a facility-based one:

$$\pi_j := \sum_p \eta_p q_{jp}$$

where q_{jp} is volume of procedure p physician j performs. I collect data from CMS on prices paid for each procedure paid under the old survey (SMS) and the new survey (PPIS), which I use to estimate π_j under each survey, π_j^{new} and π_j^{old}, respectively. Given these, I estimate the amount physician j's benefit to billing in a facility changed as a result of the survey update as:

$$\Delta\pi_j := \pi_j^{new} - \pi_j^{old}$$

Intuitively, this is the amount the survey changed how much a physician could gain from billing entirely in a facility-setting. Key to the exogeneity of this variable is that the fee update is exogenous to a cardiologist's choice of treatment for a patient. This is for two primary reasons. First, as noted above, this change in the fee structure had no relationship to actual costs of doing business, since it was an update in the data collection. Second, the price CMS pays for a procedure is influenced by the costs of all specialties performing that procedure. In other words, updates in the cost estimates for radiology could affect the prices paid to cardiologists for a given procedure. Given that these are an entirely separate specialty of physicians, it is unlikely changes in their costs would affect cardiologists' practice of medicine. Figures 2.3 and 2.4 present scatter plots of $\Delta\pi_p$ and $\Delta\pi_j$, respectively.

In Figure 2.5, I explore the validity of this instrument, plotting the rate of inte-

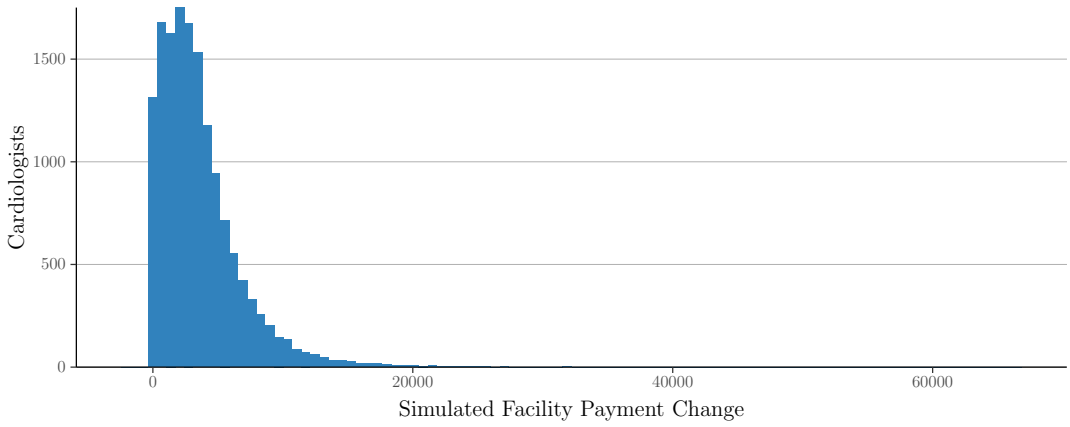


Figure 2.3: Distribution of Cardiologist Facility Payment Change

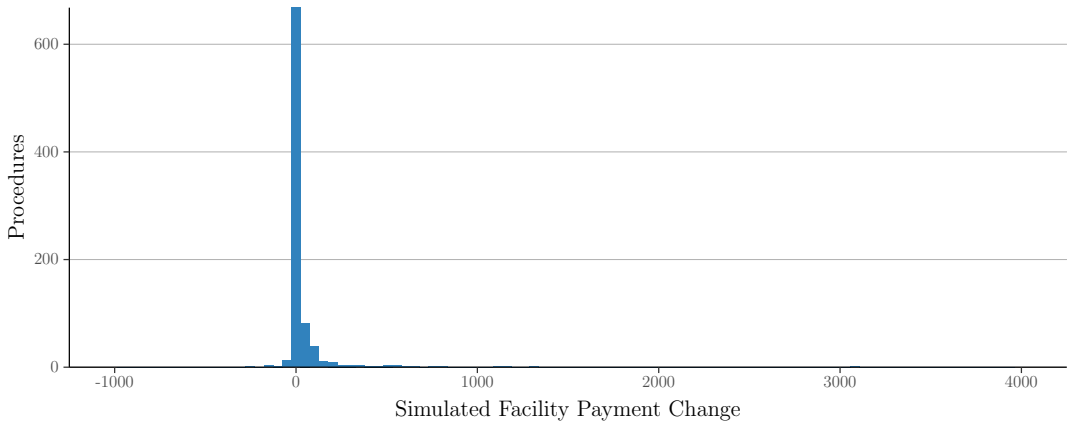


Figure 2.4: Distribution of Procedure Facility Payment Change

gration for physicians in each decile of $\Delta\pi$, which shows cardiologists in the top decile are more than twice as likely as those in the bottom decile to be integrated with a surgeon.

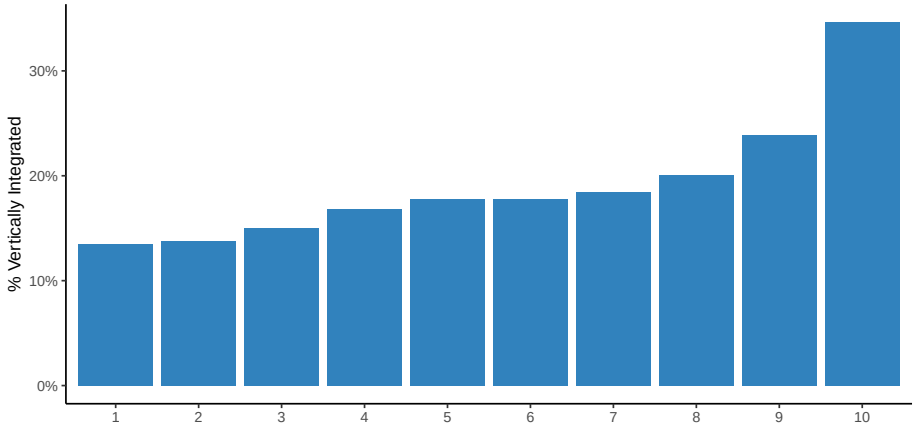


Figure 2.5: Share of Integrated Cardiologists by $\Delta\pi$ Decile

Notes: $\Delta\pi$ is the simulated change in a physician's facility markup, defined in Section 2.3.1.

2.3.2 Predicted Integration

Given each physician's π_{jt}, I use a logistic regression to predict the probability a physician is integrated in quarter t. Formally, I estimate the logistic regression:

$$log\left(\frac{Pr[VI_{jt}=1]}{1-Pr[VI_{jt}=1]}\right) = \Delta\pi_{jt} + \gamma_t + \delta_{s(j)t} + \eta_{jt}$$

where $\delta_{s(j)t}$ and γ_t are state and time fixed effects, respectively. I then predict $\hat{VI}_{jt}$, which I use in an instrumental variables framework. Using the predicted probability of integration improves instrument strength over using π_{jt} directly in an IV framework. As described in (Wooldridge, 2010, p. 262-268), this is due to the binary nature of the endogenous regressor. When using π_{jt} directly, the first stage regression is a linear probability model, which is known to have worse fit than binary dependent

variable regressions, such as the logistic regression. In this paper, this improves the strength of the instrument, providing more credible and precise estimates of β.[20]

2.3.3 Primary Estimating Equation

With the instrument in hand, I estimate the following regression:

$$Y_{ijt} = \beta VI_{jt} + \alpha X_{it} + \varepsilon_{ijt}$$

where X includes detailed patient and geographic controls as described in Section 2.2 along with quarter and state fixed effects.

2.4 Results

My empirical strategy reveals three facts about the effects of integration between cardiologists and surgeons. First, after integration, cardiologists shift patients from angioplasty and medical treatment to cardiac bypass. This does not improve patient hospitalization or mortality outcomes. Due to the shift from lower cost treatment options to surgery, total utilization for patients at vertically integrated cardiologists increases in the near-term. However, future spending is lower, largely due to lower risk of revascularization for medically treated patients.

[20] A similar approach is used and discussed in Adams et al. (2009).

2.4.1 Treatment Choices

Table 2.2 presents instrumental variables results from Equation (2.3.3) with the three treatment types (medical treatment, angioplasty, and surgery) as the dependent variables. Notably, the OLS and IV estimates of the effect of integration on treatment choice are not substantially different from one another. The physician fixed effects estimate of the effect of integration on the likelihood a patient receives CABG is $0.48pp$ in column (6) compared to $0.64pp$ from the IV in column (9). Though point estimates are different I cannot reject they are identical at reasonable confidence levels. Given the relatively low likelihood of CABG in the entire sample of 8.3%, an increase of $0.64pp$ is quite large, 7.7%.

Table 2.2: Effect of Vertical Integration on Treatment Choice

	OLS						IV		
	(1) Med.	(2) PCI	(3) CABG	(4) Med.	(5) PCI	(6) CABG	(7) Med.	(8) PCI	(9) CABG
VI	−0.522	0.285	0.237	−0.918*	0.460	0.459*	−1.491*	0.852	0.639*
	(0.318)	(0.331)	(0.132)	(0.381)	(0.375)	(0.197)	(0.654)	(0.681)	(0.300)
N	675,789	675,789	675,789	675,789	675,789	675,789	675,789	675,789	675,789
First Stage F							2,597	2,597	2,597
Treatment Mean	64.7	27.0	8.3	64.7	27.0	8.3	64.7	27.0	8.3
Quarter FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Physician FE				Y	Y	Y			

Notes: *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively. Dependent variable is 0 if the patient has no bypass claim and 100 otherwise. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Patients are matched to interventional treatments which occur within 1 month of their diagnosis. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. VI is a dummy for vertical integration. Vertical integration is defined as a cardiologist and surgeon working in the same practice. Physician-clustered standard errors are in parentheses.

An important consideration for the increase in CABG likelihood is where patients are redirected from. Cardiologists may be redirecting interventional patients away from angioplasty to CABG. Alternatively, they may shift more patients to CABG in total, without reallocating between interventional treatments. IV estimates in column (7) are 1.49*pp* less likely to be referred for medical management at integrated cardiologists. In contrast, I see no significant effect on the likelihood of receiving PCI. Though the point estimate of 0.852*pp* in column (8) is both large and positive, it is very imprecisely estimated, with a standard error of (0.681).

These findings show that integrated cardiologists do not reallocate patients between interventional treatments, rather they steer patients from the most conservative option into more intensive options. This is suggestive evidence that integrated cardiologists are not better informed about which patients would be most suitable for CABG. This stands in stark contrast to the findings of Afendulis and Kessler (2007) who found that interventional cardiologists steer patients towards PCI rather than both alternatives.

2.4.2 Patient Outcomes

The overall effect of steering patients to CABG on patient outcomes is unclear. Given the increased cost of CABG, it is expected overall medical spending will increase. However, if patients referred by integrated cardiologists have better access to surgery, they may experience better mortality and readmission rates.

In Table 2.3, I show that total 180-day spending increases, as should be expected from my estimates on treatment choices. In column (9), I show that integration increases patient medical spending by $\$1,879$ in the 180-days following diagnosis. This

amounts to an increase of 7.8% in total medical spending. To put this into perspective, an increase of $\$1,879$ for each of the approximately 1 million catheterizations performed on Medicare patients each year would amount to an additional $\$1.9bn$ in spending each year.

Table 2.3: Effect of Integration on Medical Spending

	OLS						IV		
	(1) 60 Days	(2) 90 Days	(3) 180 Days	(4) 60 Days	(5) 90 Days	(6) 180 Days	(7) 60 Days	(8) 90 Days	(9) 180 Days
VI	$1,141^{***}$	$1,301^{***}$	$1,473^{***}$	112	98	8	$1,591^{***}$	$1,872^{***}$	$1,879^{***}$
	(126)	(139)	(163)	(167)	(189)	(238)	(240)	(267)	(301)
N	655,560	637,384	589,166	655,560	637,384	589,166	655,560	637,384	589,166
First Stage F							2,576	2,556	2,479
Spending Mean	18,072	20,041	24,063	18,072	20,041	24,063	18,072	20,041	24,063
Quarter FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Physician FE				Y	Y	Y			

Notes: *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively. Dependent variable is denominated in thousands of dollars. Observations for which the patient is not continuously enrolled in Medicare for the entire observation window or where the observation window extends past the end of the sample period are dropped. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. VI is a dummy for vertical integration. Vertical integration is defined as a cardiologist and surgeon working in the same practice. Physician-clustered standard errors are in parentheses.

As described above, an increase in spending may be welfare enhancing if patient health outcomes improve as a result. However, in Table 2.4, I find that patients diagnosed by integrated cardiologists are, in fact, more likely to have an AMI readmission within 180-days. As in the case on treatment choices, this effect is consistent across identification strategies. In column (9), I estimate that patients diagnosed by integrated cardiologists are $0.51pp$, or 13.4%, more likely to be readmitted for AMI between 30 and 180 days following diagnosis by an integrated cardiologist.

Table 2.4: Effect of Integration on Readmission

	OLS						IV		
	(1) 60 Days	(2) 90 Days	(3) 180 Days	(4) 60 Days	(5) 90 Days	(6) 180 Days	(7) 60 Days	(8) 90 Days	(9) 180 Days
VI	0.124**	0.168**	0.273***	0.052	0.141	0.304*	0.219*	0.433***	0.510***
	(0.047)	(0.058)	(0.077)	(0.094)	(0.116)	(0.155)	(0.095)	(0.119)	(0.154)
N	656,501	641,691	605,144	656,501	641,691	605,144	656,501	641,691	605,144
First Stage F							2,571.776	2,557.997	2,496.149
Hospitalization Mean	1.7	2.5	3.8	1.7	2.5	3.8	1.7	2.5	3.8
Quarter FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Physician FE				Y	Y	Y			

Notes: *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. VI is a dummy for vertical integration. Vertical integration is defined as a cardiologist and surgeon working in the same practice. Dependent variable is 100 if the patient has an inpatient claim within the observation window after discharge, 0 otherwise. Observations for which the patient is not continuously enrolled in Medicare for the entire observation window or where the observation window extends past the end of the sample period are dropped. Physician-clustered standard errors are in parentheses.

Similarly, I see substantially worse mortality outcomes for patients diagnosed by integrated cardiologists. In Table 2.5, I estimate that patients diagnosed by integrated cardiologists are $0.83pp$ more likely to die within 180-days of diagnosis. Again, this is a very large effect relative to the overall mortality rate, which is only 4.45%. Thus, my estimates imply an increase in mortality risk of 18.7%. In aggregate, this would translate into $8,300$ additional deaths each year for Medicare catheterization patients.

Table 2.5: Effect of Integration on Mortality

	OLS						IV		
	(1) 60 Days	(2) 90 Days	(3) 180 Days	(4) 60 Days	(5) 90 Days	(6) 180 Days	(7) 60 Days	(8) 90 Days	(9) 180 Days
VI	0.144**	0.168**	0.270***	0.046	0.032	0.040	0.380***	0.557***	0.830***
	(0.047)	(0.058)	(0.078)	(0.096)	(0.117)	(0.156)	(0.096)	(0.114)	(0.157)
N	663,810	651,110	614,158	663,810	651,110	614,158	663,810	651,110	614,158
First Stage F							2,584.872	2,566.310	2,498.844
Mortality Mean	1.8	2.6	4.4	1.8	2.6	4.4	1.8	2.6	4.4
Quarter FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Physician FE				Y	Y	Y			

Notes: *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Dependent variable is 100 if the patient dies in the observation window after discharge, 0 otherwise. Observations differ from baseline due to excluding patients whose observation window extends past the end of the sample period. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. VI is a dummy for vertical integration. Vertical integration is defined as a cardiologist and surgeon working in the same practice. Physician-clustered standard errors are in parentheses.

On net, it is clear that patients diagnosed by cardiologists who are integrated with cardiac surgeons do not have improved outcomes. Instead, they have higher medical spending, despite having worse mortality and readmission outcomes.

2.5 Mechanisms

Though my results show a clear pattern of integrated cardiologists shifting patients to more intensive treatment options, increasing patient spending, and yielding worse mortality and readmission outcomes, it is unclear what mechanism drives these changes, which I address here. There are several possible explanations I explore.

2.5.1 Selection

The first possible explanation for my estimates is that cardiologists who are more likely to be integrated with surgeons diagnose different types of patients on unobservable dimensions, rendering my instrument invalid. While I cannot reject that patients differ on unobservable dimensions, I can show that patients do not differ along observable dimensions. I explore this possibility in Figure 2.6, which displays binned scatterplots of the Elixhauser risk score and predicted mortality risk[21] relative to predicted probability of integration. In Figure 2.6a, I examine the Elixhauser risk score, finding that, if anything, physicians who are highly likely to be integrated with a surgeon see healthier patients than others, though this effect is extremely small. From the fit line in this plot, moving from the 5th to the 95th percentile of predicted integration is associated with a lower Elixhauser risk score of approximately 0.2, while

[21] See Appendix A.1 for details on the construction of this variable.

the standard deviation of risk scores in the overall sample is 9.36. In Figure 2.6b, I see a very similar pattern for predicted mortality.

2.5.2 Surgical Mortality Risk

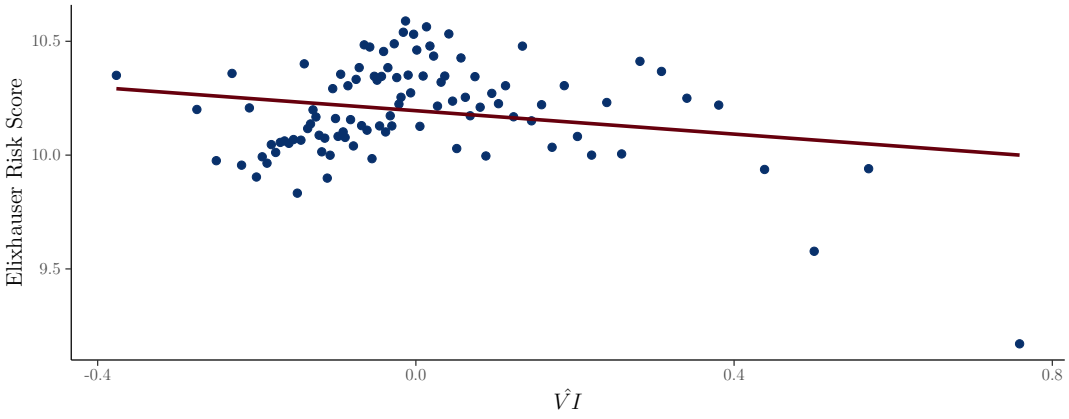
Given that cardiologists who are very likely to be integrated do not have significantly different patient mixes from those who are less likely, the most immediate explanation for increased mortality is that coronary bypass simply has higher mortality risk than other treatment options. However, a simple back of the envelope calculations shows this is extremely unlikely. To illustrate, consider the expected mortality due to shifting patients to CABG:

$$\underbrace{(Pr[CABG|VI=1] - Pr[CABG|VI=0])}_{0.0064}\underbrace{\mathbb{E}[Death|CABG, VI=1]}_{0.062} \approx .0004 \tag{2.3}$$

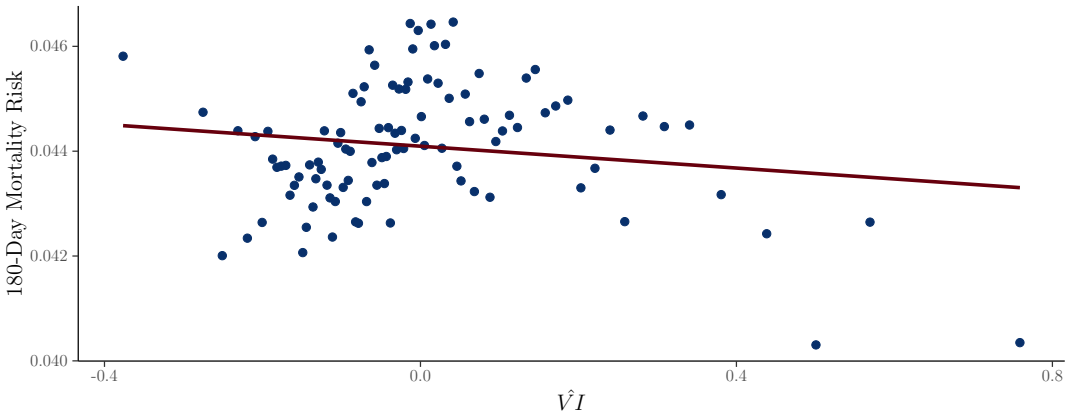
The first term is the increase in the likelihood of CABG due to integration, which I estimate at $0.64pp$ in Table 2.2, while the second term is the average overall mortality for CABG patients diagnosed by integrated cardiologists. This yields an estimated increase in mortality of $0.04pp$ due to shifting patients to CABG. [22] Given that I estimate an increase in 180-day mortality of $0.83pp$, shifting patients to CABG accounts for less than 5% of the increased mortality risk.

As an alternative to illustrate this point, I consider how large the increase in CABG likelihood would need to be in order to generate the observed increase in

[22] This likely is an overestimate of the increase in mortality due to shifting to CABG. The number calculated here would be the increase assuming patients who did not receive CABG had 0 mortality risk, while in reality they would have the mortality risk associated with the alternate treatment option they received.



(a)



(b)

Figure 2.6: Binned Scatterplots of Patient Severity vs. Predicted Integration

Notes: All variables are residualized by state and quarter and grouped into 50 equally sized bins. $\hat{VI}$ is the predicted integration from Section 2.3.

mortality. I do so by rearranging Equation (2.3) as follows:

$$(Pr[CABG|VI = 1] - Pr[CABG|VI = 0]) = \frac{\mathbb{E}[Death|VI = 1] - \mathbb{E}[Death|VI = 0]}{\mathbb{E}[Death|CABG, VI = 1]}$$

The right hand side of this equation is the increase in mortality associated with integration divided by the sample CABG mortality, which yields an estimate of $13.2pp$. In other words, integrated cardiologists would need to be more than twice as likely to refer patients for coronary bypass than non-integrated ones, much larger than my estimates suggest is reasonable. Considering the results of both of these calculations, I find no evidence to support the hypothesis that increased mortality risk is driven by higher propensity for surgery by integrated cardiologists.

2.5.3 Reputational Concerns

A further driver of mortality is that patients who are referred for surgery are lower risk, while patients who may benefit from surgery, but are at high mortality risk are not referred. This could occur due to cardiologists partially internalizing the downside risk of adverse patient outcomes on surgeon reputation. There is some evidence that surgeons reject risky patients to avoid negative shocks to their reputation (Jauhar, 2003), which could be internalized by referring physicians.

First, if this were the case, we should expect to see patients at high mortality risk be less likely to receive CABG at integrated cardiologists. As a first way to address this, I estimate a modification of Equation (2.3.3) by interacting the integration dummy with a dummy for whether a patient is high risk, which I define as having

above median Elixhauser risk score. Formally, I estimate:

$$Y_{ijt} = \beta VI_{jt} + \delta(VI_{jt} \times HighRisk_i) + \gamma HighRisk_i + \alpha X_{it} + \varepsilon_{ijt} \tag{2.4}$$

where δ indicates how Y differs for high-risk patients diagnosed by integrated cardiologists. In column 1 of Table 2.6, I find that, while low risk patients are more strongly steered towards CABG by integrated cardiologists, patients in the high-risk category are no more likely to be referred to CABG by integrated cardiologists. This suggests that cardiologists are not internalizing the reputational risk of these patients.

Table 2.6: Reputational Effects

	(1) Med.	(2) PCI	(3) CABG	(4) Med.	(5) PCI	(6) CABG
VI	-2.895^{***}	1.731^{*}	1.164^{***}	-2.650^{***}	1.503	1.148^{**}
	(0.702)	(0.728)	(0.347)	(0.740)	(0.770)	(0.369)
$HighRisk$	0.206	-0.223	0.017	-0.047	0.166	-0.119
	(0.217)	(0.205)	(0.126)	(0.234)	(0.221)	(0.133)
$VI \times HighRisk$	3.087^{***}	-1.932^{***}	-1.155^{***}	3.312^{***}	-2.421^{***}	-0.891^{**}
	(0.551)	(0.526)	(0.301)	(0.587)	(0.558)	(0.317)
$HighRisk \times ReportCard$				0.993^{*}	-1.657^{***}	0.664^{**}
				(0.409)	(0.401)	(0.238)
$VI \times ReportCard$				-4.556^{*}	4.762^{*}	-0.206
				(2.305)	(2.255)	(1.004)
$VI \times HighRisk \times ReportCard$				1.665	0.105	-1.770
				(1.982)	(1.962)	(1.116)
N	675,789	675,789	675,789	675,789	675,789	675,789
First Stage F	1,302.461	1,302.461	1,302.461	682.412	682.412	682.412
Treatment Mean	64.7	27.0	8.3	64.7	27.0	8.3
Quarter FE	Y	Y	Y	Y	Y	Y

Notes: *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. VI is a dummy for vertical integration. Vertical integration is defined as a cardiologist and surgeon working in the same practice. Dependent variable is 100 if the patient receives coronary bypass, 0 otherwise. Observations for which the patient is not continuously enrolled in Medicare for the entire observation window or where the observation window extends past the end of the sample period are dropped. Physician-clustered standard errors are in parentheses.Estimates are from Equation (2.4) and (2.5), respectively. The instrument described in Section 2.3 is used to instrument for VI in all cases.

Another way to consider this is to examine how the likelihood of a patient being referred for CABG differs in states where there are public report cards for surgeon mortality. If integrated physicians internalize reputational risks, we should expect to see high risk patients in states with report cards be less likely to receive CABG. These report cards are intended to inform patients about the quality of surgeons and have been the subject of much policy discussion and research. (Brown et al., 2012) I address this by modifying Equation (2.4) by interacting the high risk indicators with indicator variables for whether the physician is located in a state with report cards.[23][24]

$$Y_{ijt} = \beta VI_{jt} + \delta_1(VI_{jt} \times HighRisk_i) + \delta_2(VI_{jt} \times HighRisk_i \times ReportCard_{s(j)}) + \gamma_1 HighRisk_i + \gamma_2 HighRisk_i \times ReportCard_{s(j)} + \alpha X_{it} + \varepsilon_{ijt} \quad (2.5)$$

In column 2 of Table 2.6, I show that estimates of the likelihood of surgery do not differ for patients in these states relative to others. Given that I find no evidence that high risk patients are less likely to be referred for coronary bypass, even in the presence of public report cards, it is extremely unlikely that reallocating patients to avoid high risk patients is driving the observed mortality results.

[23] I do not include *ReportCard* on its own, as it is collinear with state fixed effects.

[24] There are five states which have cardiac surgeon report cards: California, Massachussetts, New Jersey, New York, and Pennsylvania.

2.5.4 Worse Care

The final remaining explanation I explore for mortality increasing is that care worsens conditional on treatment choice. In Table 2.7, I estimate Equation (2.3.3) on the subsample of patients receiving each treatment type. This tells us how mortality differs for patients diagnosed by integrated cardiologists conditional on the type of treatment they receive. In column (1), I find that mortality increases substantially for medically managed patients, while PCI and CABG patients do not have statistically significant increases in mortality.[25] It's noteworthy that this result is similar in direction and magnitude to a result noted in Afendulis and Kessler (2007), who found an increase in mortality for medically managed patients diagnosed by an interventional cardiologist. They do not, however, attempt to explain the mechanism behind this result.

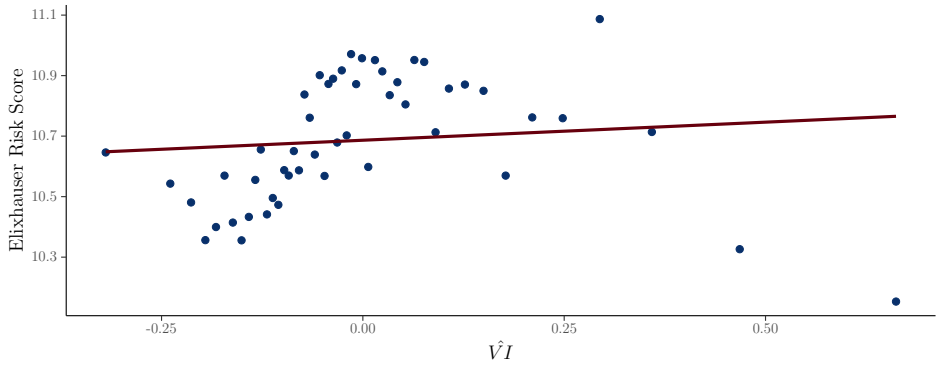
Intuitively, there are two possible explanations for increase in mortality for medically managed patients. First, riskier patients may be more likely to be steered towards medical management by integrated cardiologists. I repeat the exercise from Section 2.5.1 using only the sample of patients who are referred for medical management, finding no differences among patients along observable dimensions. These results are displayed in Figure 2.7. As in the overall sample, I see no meaningful relationship between the instrument and patient severity, as measured by predicted mortality and Elixhauser risk score. The fit line has a slight upward slope in both instances, however, the magnitude of this effect is extremely small - moving from the 5th to 95th percentile of the instrument is associated with an increase in risk score of less than 0.1. In addition, increasing the probability of integration by 0.50 increases

[25] It should be noted that the point estimates of each represent economically significant increases in mortality, despite the lack of statistical power.

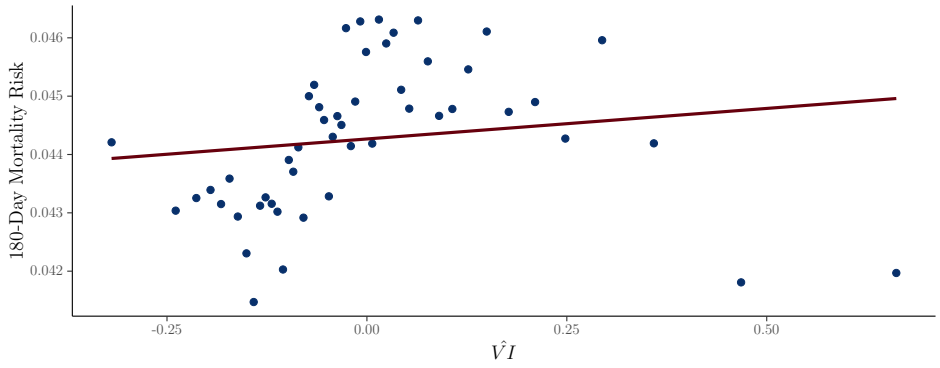
Table 2.7: Effect of Integration on 180-Day Mortality By Treatment

	180-Day Mortality		
	Med.	PCI	CABG
VI	0.61***	0.34	0.49
	(0.15)	(0.22)	(0.50)
N	396,585	166,092	51,481
First Stage F	2,603	2,157	2,442
Mortality Mean	4.47	3.91	6.05
Quarter FE	Y	Y	Y
Physician FE			

Notes: *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. VI is a dummy for vertical integration. Vertical integration is defined as a cardiologist and surgeon working in the same practice. Dependent variable is 100 if the patient dies within 180 days of diagnosis, 0 otherwise. Observations for which the patient is not continuously enrolled in Medicare for the entire observation window or where the observation window extends past the end of the sample period are dropped. Physician-clustered standard errors are in parentheses.



(a)



(b)

Figure 2.7: Binned Scatterplots of Patient Severity vs. Predicted Integration - Medically Managed Patients

Notes: All variables are residualized by state and quarter.

the expected mortality risk by less 0.05*pp*.

In Table 2.8, I estimate Equation (2.3.3) with the number of Evaluation & Management visits a patient has within 180-days as the dependent variable on the sample of patients who receive medical management.[26] In column (1), I show that patients diagnosed by integrated cardiologists have 0.30 fewer visits for management services. I see a similar pattern in columns (2) and (3), when I split the sample by whether a patient survives 180-days or not. Among patients who die, those diagnosed by

[26]I classify a claim as E&M if any procedure has a BETOS code beginning with "M".

integrated cardiologists have significantly fewer (2.28) E&M visits.

Table 2.8: Effect of Integration on Evaluation & Management Visits For Medically Managed Patients

	(1) All	(2) Survivors	(3) Non-Survivors
VI	-0.30^{**}	-0.25^{*}	-2.28^{***}
	(0.11)	(0.12)	(0.58)
N	437,163	378,869	17,716
First Stage F	2,526	2,402	1,692
Visits Mean	6.39	6.25	13.35
Quarter FE	Y	Y	Y

Notes: *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. VI is a dummy for vertical integration. Vertical integration is defined as a cardiologist and surgeon working in the same practice. Dependent variable is medical spending after diagnosis in U.S. dollars. Observations for which the patient is not continuously enrolled in Medicare for the entire observation window or where the observation window extends past the end of the sample period are dropped. Physician-clustered standard errors are in parentheses.

These findings suggest integrated cardiologists do not provide effective management for conservatively treated patients, with patients having fewer routine management visits.

Chapter 3

The Impact of Acquisitions on Firm Strategy, Patient Outcomes, and the Cost of Dialysis Care

Health-care markets have become increasingly concentrated through mergers and acquisitions (Gaynor et al., 2015a). Proponents of this industry trend cite several potential benefits of consolidation, including lower costs through economies of scale and better patient outcomes through coordinated care. But greater concentration may also result in higher prices or lower quality (Gaynor and Town, 2012; Cuellar and Gertler, 2006; Dafny et al., 2012). Previous studies of this topic typically consider only broad notions of market structure and outcomes — by showing, for instance, that more-concentrated hospital markets have higher mortality rates. Comparatively less work has examined the precise channels through which mergers and acquisitions ultimately lead to changes in outcomes. In this chapter, which is joint with Paul Eliason, Ryan McDevitt, and James Roberts and accepted for publication at *The Quarterly Journal of Economics*, we use detailed claims and facility data from the U.S. dialysis industry to show directly how large chains transfer their corporate strategies to the independent facilities they acquire and leverage their greater economies of scale, seen most prominently in larger drug doses, which substantially affect the cost and quality of care they provide.

We focus our study on the U.S. market for outpatient dialysis — a medical proce-

dure that cleans the blood of patients suffering from end-stage renal disease (ESRD) — because it offers several distinct advantages as an empirical setting for this topic. First, dialysis is a fairly standardized treatment that allows for a direct comparison of providers. Second, the dialysis industry has become increasingly concentrated following a series of mergers and acquisitions: today, dialysis is provided primarily by multi-establishment, for-profit firms, with the share of independently owned and operated dialysis facilities falling from 86% to 21% over the past three decades and the two largest publicly traded corporations, DaVita and Fresenius, now owning over 60% of facilities and earning over 90% of the industry's revenue (United States Renal Data System, 2014; Baker, 2019). Third, detailed Medicare claims and clinical data allow us to identify important changes in providers' behavior and patients' outcomes following an acquisition. Finally, the dialysis industry is an important market to study in and of itself, with total Medicare reimbursements for treating the nation's 430,000 dialysis patients amounting to about $33 billion each year, or 6% of total Medicare expenditures.

We find that acquired facilities alter their treatments in ways that increase reimbursements and decrease costs. For instance, facilities capture higher payments from Medicare by increasing the amount of drugs they administer to patients, for which Medicare paid providers a fixed per-unit rate during our study period. The most noteworthy of these is Epogen (EPO), a drug used to treat anemia, which represented the single largest prescription drug expenditure for Medicare in 2010, totaling $2 billion (U.S. Government Accountability Office, 2012). Perhaps reflecting the profits at stake, patients' EPO doses increase 129% at independent facilities acquired by large chains. Similarly, acquired facilities increase their use of the iron-deficiency drug Venofer relative to Ferrlecit, a perfect substitute that offers lower reimbursements. On the cost side, large chains replace high-skill nurses with lower-skill technicians

at the facilities they acquire, reducing labor expenses. Facilities also increase the patient-load of each employee by 11.7% and increase the number of patients treated at each dialysis station by 4.5%, stretching resources and potentially reducing the quality of care received by patients.

Adopting the acquiring firm's operational strategies directly affects patients' outcomes and Medicare's expenditures. Patients at acquired facilities are 4.2% more likely to be hospitalized in a given month, while the survival rate for new patients falls by 1.3-2.9% depending on the time horizon. In addition, new ESRD patients who start treatment at an acquired facility are 8.5% less likely to receive a kidney transplant or be added to the transplant waitlist during their first year on dialysis, a reflection of worse care because transplants provide both a better quality of life and a longer life expectancy than dialysis. Other measures of clinical quality are mixed, at best. We find, for example, that patients are 5.1% less likely to have hemoglobin values within the recommended range and 10.0% more likely to have values that are too high, an indication of poor anemia treatment. The only outcome for which we find unequivocal evidence of improved quality at acquired facilities is the urea reduction ratio, a measure of the waste cleared during dialysis, with patients at acquired facilities becoming 1.8% more likely to have adequate clearance levels. And although patients receive worse care on these measures following an acquisition, acquired facilities increase per-treatment Medicare reimbursements by 6.9%, resulting in $301.7 million in additional Medicare spending throughout our sample on a base of $4.5 billion.

As in much of the merger-effects literature, our findings may face multiple threats to identification, as acquisitions do not occur randomly and acquired facilities likely differ from those not acquired in important, potentially unobservable ways. We over-

come these challenges by using detailed claims data that allow us to observe patients with the same characteristics being treated at the same facility before and after acquisition, which allows us to identify the effects of an acquisition solely from within-facility changes in ownership. In many cases, we can also estimate specifications that include patient fixed effects and identify the acquisition effects from within-patient changes in outcomes, a particularly conservative approach.

We then examine the mechanisms through which acquisitions affect firm behavior. We first consider whether an acquisition's effect on market power can explain the changes we observe for patient outcomes, as would be predicted by standard models of regulated markets with endogenous product quality (e.g., Gaynor (2004) and the models discussed therein). With prices set administratively for Medicare patients, these models predict that a facility facing more competition in its market would offer higher-quality care to attract patients, given the assumption that demand is elastic with respect to quality. In dialysis, however, this assumption fails to hold: patients are not very responsive to changes in quality and rarely switch facilities, mainly due to high travel costs. We therefore find similar qualitative and quantitative results across all outcomes when comparing acquisitions that increased market concentration to those that did not. As such, changes in market power cannot explain the decline in dialysis quality that occurs after a takeover, which implies that the strategy of the acquiring chain, rather than the subsequent concentration of the market, largely determines how patients fare following an acquisition.

Since an increase in local market power does not explain the changes we observe following an acquisition, we conclude our analysis by considering other explanations for why independent facilities do not typically imitate the more-profitable strategies of the large chains before being acquired. Although we assess a host of possible

reasons, only two withstand scrutiny. First, and most importantly, the largest for-profit chains benefit from greater economies of scale, such as the volume discounts they receive for purchasing injectable drugs, which influences their behavior. Second, we find some limited evidence that non-profit facilities change more following an acquisition than for-profit facilities do, suggesting that for-profit acquirers' explicit mandate to maximize profits may lead them to sacrifice patient outcomes in favor of higher reimbursements.

Our paper contributes to several bodies of literature. The first studies the effects of mergers and acquisitions, both in health care and more generally.[1] Much of this literature has focused on how mergers affect prices through changes in market power.[2] The literature examining the effects of mergers and acquisitions on quality is more limited.[3] Even in regulated markets, the net effect is theoretically ambiguous. On the one hand, standard models without merger efficiencies (e.g., Gaynor, 2004) show that acquisitions leading to increased market power reduce the incentive to deliver high-quality care.[4] On the other hand, mergers that result in efficiency gains, such as through economies of scale, may lead to better outcomes.

Our paper also contributes to the somewhat limited literature on how "roll-

[1]This is an extensive literature that cannot be fully reviewed here. For a thorough review in the context of health care, see Gaynor et al. (2015a).

[2]In health care, these studies have primarily considered hospital mergers, broadly finding that they result in higher prices paid by insurers (e.g., Dafny et al., 2016; Dafny, 2009b; Gowrisankaran et al., 2015).

[3]A number of papers study the effect of market concentration on hospital quality but do so without focusing explicitly on mergers and acquisitions (e.g., Kessler and McClellan, 2000b; Gaynor et al., 2013).

[4]Bloom et al. (2015) find empirical support for this by showing that U.K. public hospitals improve their quality when patients can more easily switch from low-quality to high-quality providers. More directly, Ho and Hamilton (2000) compare quality measures at hospitals before and after being acquired or merging with another hospital, finding that quality deteriorates along some dimensions following acquisition, especially in more-concentrated markets. Hayford (2012b) and Capps (2005b) also investigate the direct impact of mergers on hospital quality.

up" strategies, where large firms gradually increase their market share by acquiring many of their much-smaller competitors, affect industry performance and outcomes. This "whale eats krill" pattern of consolidation has occurred in industries as varied as physician practices (Capps et al., 2017b) and funeral homes (Wollmann, 2019), as well as packaged ice companies, breweries, hairdressers, vending machines, medical devices (Dunn, 2016), automotive suppliers (Kocourek et al., 2000), solar power (*Seeking Alpha*, 2015), and many others (*The Economist*, 2015).

Finally, our paper contributes to a recent literature specifically focused on the economics of the dialysis industry (e.g., Dai, 2014; Cutler et al., 2017b; Dai and Tang, 2015; Grieco and McDevitt, 2017; Eliason, 2019; Gaynor et al., 2018; Wilson, 2016a,b). Within this literature, our paper is most closely related to Cutler et al. (2017b), who study how market concentration in the dialysis industry impacts quality and the price charged to privately insured patients. Using data from the Health Care Cost Institute and Dialysis Facility Compare (DFC), they exploit mergers of national dialysis chains as shifters in local market concentration and find no effect of concentration on quality and a weakly positive effect on prices. This differs substantially from our paper in a number of ways. First, they perform their analysis at an aggregate level because they do not observe patient-level data and are unable to match data from private insurers to facilities from DFC. By contrast, much of our analysis is performed at the patient level, allowing us to control for a large set of patient covariates and to observe how quality and treatment change within a facility — and even within a patient — over time. Moreover, our paper focuses on the role of a chain's strategy in treatment decisions, which is less likely to be influenced by local market competition. Also, some prior work has studied the effect of facility ownership on patients' treatments, but to our knowledge ours is the first to directly consider how acquisitions change firm

strategies and the causal mechanisms through which they affect patient outcomes.[5]

The rest of the paper proceeds as follows. Section 3.1 summarizes important institutional details of the dialysis industry. Section 3.2 describes our data. Section 3.3 presents our main results on the effects of dialysis facility acquisitions. Section 3.4 shows that these effects do not vary based on market concentration. Section 3.5 considers other explanations for why independent facilities behave differently than chains. Section 3.6 concludes. The online appendices contain further details on the data, the sample construction, and analyses that illustrate the robustness of our findings.

3.1 Background on the Dialysis Industry

3.1.1 Medical Background

The kidneys perform two primary functions in the human body: they filter wastes and toxins out of the blood and produce erythropoietin, a hormone that stimulates red blood cell production. The diagnosis for patients experiencing chronic kidney failure, where their kidneys no longer adequately perform these functions, is called end-stage renal disease (ESRD). To survive, ESRD patients must either receive a kidney transplant or undergo dialysis, a medical treatment that mechanically filters wastes and toxins from a patient's blood. Although a transplant is considered the

[5]Garg et al. (1999), Zhang et al. (2014), and Thamer et al. (2007) study the effect of facility ownership on patients' treatments. The first two of these papers provide descriptive evidence that for-profit facilities and chain-owned facilities, respectively, are less likely to refer patients to the transplant waitlist, with Garg et al. also finding lower mortality rates at for-profit facilities. Zhang et al. (2011) further show that chain-owned facilities have higher mortality rates than independent facilities, while Thamer et al. (2007) find that patients at non-profit dialysis facilities receive lower EPO doses than those at for-profit chain facilities.

best course of treatment, it is often not possible, either due to a lack of available kidneys or the patient's poor physical condition. Fewer than 20% of dialysis patients are currently on a kidney waitlist, and for those who are, the median wait time for a transplant is 3.6 years (United States Renal Data System, 2014). As a result, most patients with kidney failure rely on dialysis, either permanently or for an extended period.

Those with ESRD may receive one of two types of dialysis, hemodialysis or peritoneal dialysis. Hemodialysis uses a machine (also referred to as a station and designed to treat one patient at a time) to circulate blood through a filter outside the body, which can be performed at the patient's home or at a dialysis center, whereas peritoneal dialysis uses the lining of the patient's abdomen to filter blood inside the body.[6] Because over 90% of dialysis patients choose in-center hemodialysis, we focus on this modality for our analysis.

In addition to dialysis, most ESRD patients also receive treatment for anemia because they do not naturally produce enough erythropoeitin, which leads to a deficiency of red blood cells (Besarab et al., 1998). Anemia is treated with a cocktail of injectable drugs, most commonly the erythropoietin stimulating agent (ESA) known as Epogen (EPO), along with an intravenous iron analog, such as Venofer or Ferrlecit. Patients most commonly receive these drugs while being treated at a dialysis facility.

A dialysis facility's quality of care may be assessed through both clinical indicators and patient outcomes. Among the clinical measures, the two most prominent are the urea reduction ratio (URR) and hemoglobin (HGB) levels. The first, URR, measures the percent of primary waste (i.e., urea) filtered out of a patient's blood during dialysis, which increases as a patient spends more time on a machine. Al-

[6]For more information, see `https://www.niddk.nih.gov`.

though patients vary in how long it takes them to achieve a given URR, the standard of care is that a dialysis session should continue until a patient achieves a URR of at least 0.65 (Owen et al., 1993; NIH, 2009).

The second, a patient's HGB level, measures the onset or severity of anemia. During the period of our study, the FDA recommended EPO doses be such that HGB levels fall between 10 and 12 grams per deciliter (g/dL) (Manns and Tonelli, 2012). On the lower end, patients with HGB below 10g/dL are anemic and suffer from symptoms such as fatigue, dizziness, headaches, and, in some severe cases, death. On the other side of this range, high levels of HGB can result in serious complications, such as cardiovascular events (Besarab et al., 1998; Singh et al., 2006).

Along with these clinical measures, patient outcomes such as mortality and hospitalization represent additional indicators of a facility's quality. Of particular concern are hospitalizations for septicemia and cardiovascular events (Schrier and Wang, 2004). Septicemia, an infection of the blood for which dialysis patients are especially susceptible due to their weakened immune systems and frequent connection between the dialysis machine and their bloodstream, poses a severe risk for patients. Providers can reduce infections by properly cleaning machines between patients (Patel et al., 2013), but this is costly since it takes up to an hour to adequately sanitize a dialysis station (Grieco and McDevitt, 2017). ESRD patients also face an elevated risk for cardiovascular events such as myocardial infarction and stroke, a risk made worse through excessive use of EPO (Besarab et al., 1998; Singh et al., 2006).

3.1.2 The Role of Medicare in Dialysis

A defining feature of the dialysis industry is that 90 days after being diagnosed with ESRD, all patients become eligible for Medicare coverage, regardless of age, which makes Medicare the primary payer for most ESRD patients. In 2014, over 80% of the 460,000 ESRD patients receiving dialysis treatments in the U.S. were enrolled in Medicare. As a result, Medicare spends more than $33 billion each year for costs associated with ESRD, approximately 1% of the entire federal budget (Ramanarayanan and Snyder, 2014).

Throughout the time period of our study, Medicare used a blended payment policy to reimburse dialysis providers.[7] Specifically, Medicare paid a composite rate of around $128 per dialysis treatment, up to three times per week for each patient. For injectable drugs, providers were reimbursed separately on a fee-for-service basis depending on the quantity of drug administered, a crucial feature of the industry that we study below.[8]

Prior to 2011, fee-for-service injectable drugs generated considerable revenue for dialysis providers. In our analysis, we focus on the three most prevalent injectable anemia drugs: EPO, Venofer, and Ferrlecit. More than 90% of dialysis patients received EPO in the mid 2000s, and annual expenditures reached $2 billion in 2010,

[7] Beginning in 2011, Medicare made a number of changes to the way it reimburses dialysis providers. In particular, it substantially changed its reimbursement policy by bundling dialysis and anemia treatment (including injectable drugs) into a single prospective payment, changing the case-mix adjustments to those payments, and introducing the Quality Incentive Program. Because these reforms likely had many confounding effects on the dialysis industry, in this paper we restrict our analysis of facility acquisitions to the years spanning 1998 to 2010 and study the effects of the 2011 reform in a separate paper (Eliason et al., 2019a).

[8] For these drugs, providers were reimbursed at a rate equal to 95% of their average wholesale price prior to 2005. This was reduced to 85% in 2004. After investigations by the Centers for Medicare and Medicaid Services (CMS) found that providers were being reimbursed much more than they were spending, Congress altered the payment scheme to be 106% of the average sales price, a more accurate reflection of the drugs' true costs for providers.

making it the largest prescription drug expense for CMS (U.S. Government Accountability Office, 2012). Administering EPO proved lucrative for providers, accounting for as much as 25% of DaVita's revenue and up to 40% of its accounting profits (DaVita, 2005). Many patient advocates questioned such pervasive use of EPO, however, as several studies linked excessive EPO doses to an increased risk of mortality and cardiovascular events (Besarab et al., 1998; Singh et al., 2006; Brookhart et al., 2010).

The other two anemia drugs, Ferrlecit and Venofer, are intravenous iron supplements used to treat iron-deficient anemia patients; they are essentially substitutable (Kosch et al., 2001) and both offered generous reimbursements. In 2007, total Medicare expenditures for these two drugs were $68 million and $166 million, respectively, making them the fourth and sixth most highly reimbursed drugs under Medicare Part B. Both are sold by their manufacturers in single-use vials, and any amount of the drug left over in a vial must be discarded to reduce the risk of infection, with CMS reimbursing facilities for the amount in the vial rather than the amount actually administered to the patient. Although Ferrlecit and Venofer had nearly identical per-milligram reimbursement rates during our study period, Venofer was produced exclusively in 100mg vials, while Ferrlecit was produced in 62.5mg vials. As a result, facilities could effectively receive higher reimbursements per vial for Venofer because they could, for example, use 25mg from four vials rather than one 100mg vial but still bill CMS for four 100mg vials, discarding 75mg from each of the four (i.e., under this scheme they could bill for 400mg of Venofer as opposed to 250mg of Ferrlecit). One company accused of engaging in this practice paid $450 million to settle a whistleblower lawsuit (Pollack, 2011; Stempel, 2015).

Although Medicare covers the vast majority of dialysis patients in the U.S., those

who have private insurance and become eligible for Medicare solely due to ESRD retain that coverage for the first 30 months of treatment before Medicare becomes the primary payer.[9] Reimbursements from private insurers tend to be much higher than those from Medicare, with estimates suggesting that the average private insurance rates are anywhere from 2.1 times (United States Renal Data System, 2013) to 4.5 times (Boyd, 2017) as generous as Medicare.[10]

3.1.3 The Market for Dialysis

Dialysis patients choose their provider much like they do in other segments of the U.S. health-care system, with those covered under Medicare able to receive treatment at any facility that has an opening. Patients primarily receive dialysis at one of the more than 6,000 dedicated dialysis facilities across the country, where they typically go three times per week for treatment that lasts 3-4 hours each visit.[11] These facilities are run by a mix of for-profit and non-profit firms, and over the past three decades the two largest for-profit chains, DaVita and Fresenius, have grown to the point where they now control over 60% of facilities and earn 90% of the industry's revenue (United States Renal Data System, 2014; Baker, 2019). The remainder of the market comprises smaller chains as well as independent facilities that are often run by nephrologists.

Dialysis chains potentially have a number of advantages over independent facilities. Large chains, for example, may have lower average costs due to volume

[9] Including the 90-day waiting period for Medicare eligibility, private insurance coverage may last up to 33 months.

[10] According to DaVita's 2007, 10-K the average patient with private insurance generated 3.8 times more revenue than the average Medicare patient.

[11] Unless otherwise specified, for the rest of the paper when we use the term "dialysis" we are referring to in-center hemodialysis.

discounts for pharmaceuticals as well as centralized clinical laboratories; they may have a stronger bargaining position with commercial insurance companies (Pozniak et al., 2010); and their national brand and network may make them more attractive to patients.

Chains also stand apart from independent facilities by having firm-wide standards that they implement across their facilities. Notably, large chains have operation manuals that dictate each of their facilities' procedures during treatment. We see evidence of this standardization in the predictability of a patient's EPO dose: an acquired facility's use of EPO becomes nearly twice as predictable — and twice as high — compared to its pre-acquisition doses.[12] The use of these manuals represents a clear channel through which an acquisition could alter patients' treatments and outcomes, which we study at length below.

Chains' system-wide standards may not universally lead to higher-quality care, however, as anecdotal evidence presented by the media, as well as some governmental reports, have raised concerns about practices and outcomes at both independent and chain facilities. For example, an investigative journalist from ProPublica examined the inspection records of more than 1,000 facilities and found that surveyors came across filthy or unsafe conditions in almost half the units they checked (Fields, 2010).[13] Similarly, *The New York Times* and *Washington Post* have both reported on the excessive use of injectable drugs at dialysis facilities, noting that despite the billions spent on anemia drugs, there is little evidence that they improve patients'

[12]These statements about predictability are based on comparing R^2 from regressions of EPO dose per patient on patient characteristics interacted with year fixed effects estimated separately using observations from facilities that are acquired either pre- or post-acquisition. See results in Appendix B.15.

[13]At some facilities, blood was found encrusted on patients' treatment chairs or even splattered around the room. At a unit in Durham, N.C., ants were reportedly so common that staffers would simply hand a can of bug spray to patients who complained.

quality of life (Berenson and Pollack, 2007; Whoriskey, 2012). Multiple reports by the Office of the Inspector General have also scrutinized dialysis facilities' drug use and acquisitions.[14] In addition to bad press, extreme cases of poor conditions and treatment quality have led to a number of lawsuits against providers.[15] Moreover, the media has reported claims that chains potentially provide worse care by discouraging their patients from seeking kidney transplants (Matthews, 2017; Oliver, 2017).[16] In the analysis below, we will move beyond such anecdotes by using our comprehensive claims data to consider directly how a firm's strategy affects patient outcomes.

3.2 Data and Descriptive Statistics

A primary contribution of our paper is to show how acquisitions affect the quality of care provided by dialysis facilities, which we accomplish in part by tracking patients' treatments and tests before and after their facilities are acquired. The micro-level data we use in our analysis are essential for observing any changes in a facility's strategic choices and how these choices subsequently impact patients' outcomes and overall Medicare spending. In this section, we describe our data and provide descriptive results for the most-prominent changes in firm strategy.

[14] See OEI-03-06-00200 or OEI-03-06-00590 for two examples.

[15] As an example, in 2008 Fresenius Medical Care North America agreed to settle a wrongful-death lawsuit brought by a deceased patient's survivors. According to a federal inspection report, during treatment the patient's bloodline became disconnected and, contrary to emergency standing orders, the dialysis technician reconnected the line to the patient's catheter, "infusing him with 'potentially contaminated blood'." He was later taken to a hospital where tests showed that his catheter had become infected with antibiotic-resistant staph. The infection moved to his heart and brain and he died a few days later.

[16] Although patients can self-refer for a transplant, they often lack adequate information about the procedure and fail to understand its risks and benefits. Facilities thus play an important role in a patient's decision to pursue a transplant, and some have allegedly discouraged patients from seeking one out to avoid losing their reimbursements (OPTN Minority Affairs Committee, 2015).

3.2.1 Data Sets

For our analysis, we use patient- and facility-level data from the United States Renal Data System (USRDS). The USRDS is a data clearing house funded by the National Institute of Diabetes and Digestive and Kidney Diseases and the National Institute of Health that collects and stores data related to chronic kidney disease. They combine data from a variety of sources, including Medicare administrative files, Medicare claims, annual facility surveys, and clinical surveillance data, to create the most-comprehensive data set for studying the U.S. dialysis industry.[17] Appendices B.1 and B.14 provide more details on the data sets and how we constructed our sample.

The USRDS uses a number of data sources to create an exhaustive treatment history for almost all dialysis patients in the U.S. since at least 1991, allowing us to observe each patient's sex, race, BMI, cause of ESRD, payer, measures of kidney failure, comorbidities (e.g., diabetes and hypertension), residential ZIP Code, facility of treatment, and mortality data. We combine these data with institutional claims from Medicare, which provide a more granular view of the dialysis treatments received by Medicare patients. Providers submit line-item claims for services other than dialysis. These include all injectable drugs administered during treatment and clinical measures related to dialysis care (urea reduction ratio) and anemia treatment (hemoglobin levels) at a monthly frequency, making them among the more-detailed claims data available to researchers. These data also identify if and when a patient is hospitalized. Finally, we observe a patient's transplant and waitlist status, including their listing date and the transplant center.

Detailed data on dialysis facilities come from the Annual Facility Survey, which

[17] For a more thorough description of USRDS, see the *Researcher's Guide to the USRDS System* at USRDS.org.

is required by CMS to maintain certification and receive Medicare reimbursements for ESRD treatment. From these surveys, we observe a facility ID, address, chain affiliation, labor inputs, number of dialysis stations, for-profit status, and types of treatment offered (e.g., hemodialysis, peritoneal dialysis, or transplant). These data allow us to construct a yearly panel of chain ownership for each facility. We enrich this panel and construct a monthly panel of chain ownership using precise acquisition dates for each facility from the Provider of Service files and annual cost reports submitted to CMS. This process enables us to find precise acquisition dates for 1,055 of the 1,236 acquisitions we observe.[18]

In addition to the Annual Facility Survey, providers must submit certified financial statements to CMS each year that detail their costs of providing care as part of the Healthcare Cost Reporting Information System (HCRIS), which CMS reserves the right to audit. We use these reports to construct measures of per-treatment variable costs and per-unit EPO acquisition costs.[19]

We combine these data sets and drop any patient who is missing demographic or comorbidity data. We also drop observations at facilities that are acquired but do not have reliable dates of acquisition, as well as the 12-month window surrounding an acquisition to reduce measurement error in the timing of acquisition.[20]

[18] A more-detailed description of this matching process is available in Appendix B.14.

[19] These data allow us to net rebates out of the total acquisition costs for EPO. We validated the fidelity of these data by comparing them to an independent audit of dialysis facility drug costs conducted by the Office of the Inspector General (OIG Report OEI-03-06-00590) in 2006 and found that the mean acquisition costs for EPO was very similar in the two sources.

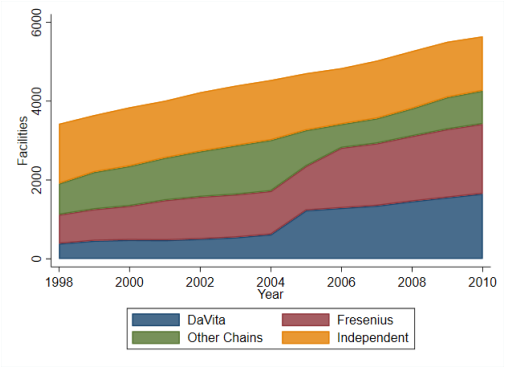
[20] Our qualitative results are robust to the inclusion of this time period, though quantitative results are somewhat attenuated due to the introduction of measurement error in the timing of acquisitions. See Appendix B.8.

3.2.2 Descriptive Statistics

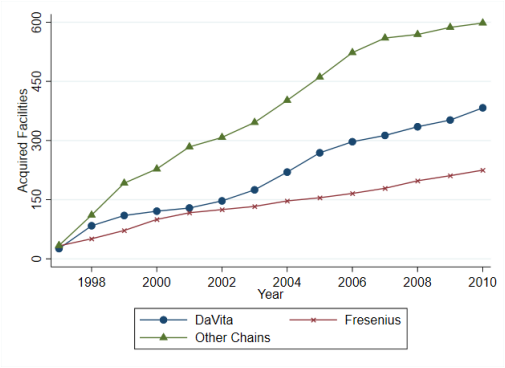
Figures 3.1a-3.1b illustrate the significant change in the dialysis industry's market structure over our sample period. The number of acquisitions has varied between 50 to 150 each year, and by 2010 we observe over 1,200 first-time acquisitions of independent facilities, providing us a large sample to conduct our analysis. Consolidation increased sharply during our sample period. Figure 3.1a shows the extent of this change, with DaVita and Fresenius owning the majority of facilities by 2010 and the other chains collectively commanding a somewhat smaller market share. The two biggest mergers during this time period are DaVita and Fresenius' acquisitions of the large chains Gambro and Renal Care Group, respectively. We exclude these large acquisitions from our analysis, however, because we are primarily interested in understanding how the transition from a fragmented industry comprised of independently owned and operated facilities to one predominately controlled by large chains affects both patients' outcomes and Medicare's expenditures. Moreover, by focusing exclusively on the acquisitions of independent facilities, we can cleanly link changes in ownership to the resulting changes in behavior and outcomes, whereas any effects stemming from an acquisition of one large chain by another may be confounded by issues such as the integration of different corporate cultures or policies, as well as the impact of nationwide market forces. Figure 3.1b depicts how the acquisitions of independent facilities have contributed to each chain's overall growth during our sample period.[21] Despite the large number of acquisitions during this time, the number of independent facilities has declined only modestly, from approximately 1,500 in 1998 to 1,300 in 2010. The vast majority of this decline came from acquisitions: only 404

[21] As Wollmann (2019) points out, one reason why such consolidation is possible is that most of the acquisitions that led to these firms' growth were exempt from the Hart-Scott-Rodino's pre-merger notification program due to the relatively small size of the target firms.

independent facilities exited, fewer than half the number acquired by chains.



(a) Market Evolution, 1998-2010



(b) Acquisitions by Major Chains, 1998-2010

Figure 3.1: Dialysis Market Evolution and Facility Acquisitions by Major Chains Over Time

Table 3.1 presents descriptive statistics at a patient-month level, split by acquisition status (Appendix B.4 includes an expanded version of this table that shows, for example, more of the clinical characteristics that we use in our analysis below). The first three panels describe the patient population in some detail. In addition to comorbid conditions, our data include important blood chemical tests that indicate the severity of each patient's kidney failure, such as the glomerular filtration rate (GFR) that measures residual kidney function. Specifically, it measures how much blood passes through the glomeruli, tiny filters in the kidneys, each minute, with a GFR below 15 possibly indicating kidney failure (Stevens et al., 2006). Of the comorbid conditions, cardiovascular conditions are widespread among dialysis patients. In total, approximately 50% of patients have at least one cardiovascular condition, with congestive heart failure the most common. The prevalence of such conditions makes any increase in EPO doses especially hazardous due to the concern that it elevates a patient's risk of cardiovascular events (Besarab et al., 1998; Singh et al., 2006).

Dialysis patients are also disproportionately African-American, comprising over 30% of our sample compared to less than 15% of the U.S. population. In our analysis, we also include demographic characteristics that vary both across ZIP Codes and within a ZIP Code over time. In our regressions we control for the age of the facility and, in specifications without facility fixed effects, the facility's elevation, as medical evidence suggests that elevation influences a patient's need for EPO.[22] We also note that acquirers are more like to be for-profit firms. To summarize patient health, in the fifth panel we combine the clinical characteristics into a measure of predicted mortality by taking the fitted values from regressing an indicator for patient death on patient characteristics.[23] The table shows that, according to this measure, patient health is fairly constant across the four types of facility ownership.

Table 3.1 also allows us to investigate the potential identification challenges that we must address with our empirical strategy. Namely, patients at acquired facilities may be inherently different from patients at facilities that are not acquired, and the patient mix at acquired facilities could change after an acquisition. For many attributes, we observe no systematic differences across facility-types (e.g., GFR and congestive heart disease). We also see no meaningful difference in the share of privately insured patients across each type of facility. We do observe differences in racial composition and the rates of ischemic heart disease, however, with these differences largely coming from long-run trends in patient characteristics, as the pre-acquisition column tends to sample from earlier years and the post-acquisition column from later years. For example, the prevalence of ischemic heart disease among dialysis patients

[22] At higher elevations, the richness of oxygen in the blood decreases and tissue-hypoxia sets in, which causes the body to produce more endogenous erythropoietin (Brookhart et al., 2011) reducing the need for ESAs. Eliason et al. (2019a) exploit this feature of anemia treatment to study the effect of the 2011 dialysis payment reforms on patient and market outcomes.

[23] See Appendix B.5 for details on the construction of this measure.

Table 3.1: Patient and Treatment Descriptive Statistics by Facility Type

	Always Independent	Pre-Acquisition	Post-Acquisition	Always Chain
Clinical Characteristics				
GFR	7.92	7.74	7.99	7.71
Hemoglobin	7.68	7.67	7.73	7.56
Atherosclerotic Heart Disease (%)	5.74	7.18	4.76	4.77
Peripheral Vascular Disease (%)	13.44	14.33	12.53	11.47
Ischemic Heart Disease (%)	17.25	20.58	14.84	13.75
Congestive Heart Failure (%)	31.07	32.04	30.29	28.56
Demographics				
Male (%)	53.87	53.18	52.93	52.15
Non-Hispanic White (%)	48.56	53.42	44.41	40.44
Black (%)	32.30	30.65	36.23	39.98
Hispanic (%)	13.06	10.03	13.79	14.77
Asian (%)	3.33	2.57	2.62	2.41
Other Race (%)	5.61	5.33	4.91	4.52
Age (Years)	64.31	64.53	64.02	63.38
Months With ESRD	35.83	31.75	37.06	36.88
Distance (Mi.)[b]	4.93	5.36	5.11	5.00
Area Demographics				
% 18-24 with only High School	31.79	33.24	33.19	32.90
% 18-24 with only Bachelors	9.10	7.81	7.46	7.76
Median Income ($)	50,404.87	48,202.46	47,441.34	47,637.76
Facility Characteristics				
Facility Age (Years)	14.08	12.02	10.10	13.86
Facility Elevation (ft.)	195.54	198.65	211.42	192.58
For-Profit (%)	40.99	64.09	96.40	88.70
Patient Health				
Predicted Mortality (%)	1.03	1.07	1.06	1.17
Treatment				
EPO Per Session ('000 IU's)	4,495.66	4,728.87	6,223.04	6,259.82
Venofer Per Session (mg)	7.95	7.60	15.93	14.86
Ferrlecit Per Session (mg)	6.49	7.22	4.65	4.86
Payments Per Session	179.22	171.79	184.58	183.15
Waitlist or Transplant[a] (%)	10.92	9.63	9.76	9.52
Patient-Months	2,880,503	1,483,917	1,960,286	7,836,538
Incident Patients	235,144	142,815	126,582	400,161

Notes: See text for more detail.
[a] Dummy variable for being waitlisted or transplanted within 1 year for incident patients only.
[b] Median distance is displayed instead of mean.

has declined from 21.8% in 1998 to 10.6% in 2010. Reflecting this, when we consider only those patients treated within 12 months of the acquisition window, we find no meaningful difference between the pre- and post-acquisition groups (see Appendix B.6). This further suggests that any meaningful differences in demographics are driven by time trends, not changes in the mix of patients treated at facilities following an acquisition.

Nevertheless, in the analysis that follows, we directly consider the possibility that an acquisition may affect the mix of patients in ways that could bias our results. To ensure that time trends and selection bias do not confound our analysis, we control for detailed patient characteristics and include month-year fixed effects in our regressions. To further address any concerns that our findings may be driven by changes in patient unobservables, we show that our results are robust to including patient fixed effects in Appendix B.9. Additionally, in Section 3.3.4 we present evidence that patients starting dialysis at acquired facilities may be healthier than those beginning treatment at the same facility before acquisition, suggesting that the deterioration in outcomes we estimate may actually be understating the true decline.

These descriptive statistics also highlight stark differences in the treatments received by patients at each type of facility. As the bottom panel of Table 3.1 clearly shows, patients at chain-owned facilities receive substantially more EPO per session and are much more likely to receive Venofer than Ferrlecit. As a result, payments per session (all Medicare payments to the dialysis facility including injectable drugs per session) jump by about 7% at facilities acquired by a chain.

Facilities' operations also change following an acquisition. Table 3.2 shows that chain-owned facilities have more stations per facility, substitute towards lower-cost technicians and away from higher-cost nurses, and generally stretch resources further

Table 3.2: Facility Summary Statistics

	Always Independent	Pre-Acquisition	Post-Acquisition	Always Chain
Stations	14.30	16.63	18.39	17.92
	(8.63)	(7.82)	(8.13)	(7.39)
Hemodialysis (%)	89.90	91.69	92.36	94.22
	(19.25)	(15.92)	(14.76)	(13.06)
Privately Insured (%)	6.52	7.43	6.66	6.79
	(6.17)	(5.85)	(4.12)	(5.38)
Nurses	5.61	5.14	4.23	3.70
	(4.06)	(3.76)	(2.63)	(2.26)
Technicians	4.95	6.20	6.65	6.22
	(5.09)	(4.77)	(4.53)	(4.12)
Nurses/Techs	1.62	1.08	0.77	0.72
	(2.21)	(1.17)	(0.70)	(0.59)
Patients/Employee	4.14	4.75	5.84	5.52
	(2.76)	(2.14)	(2.09)	(2.34)
Has Night Shift (%)	24.85	23.85	23.88	18.47
	(43.22)	(42.62)	(42.64)	(38.81)
For-Profit (%)	35.15	66.48	94.12	88.10
	(47.75)	(47.21)	(23.53)	(32.37)
Facility Elevation (ft.)	251.24	205.88	209.83	229.52
	(359.41)	(242.46)	(282.05)	(342.04)
Facility Age (Years)	12.93	9.11	9.74	10.98
	(9.71)	(8.61)	(7.11)	(8.50)
Facility-Years	7,824	4,063	4,137	16,459

Notes: An observation is a facility-year. Standard deviations are in parentheses.

by treating more patients per employee. All of these differences are consistent with a firm strategy that prioritizes profits over patient outcomes, which we consider in greater detail in the next section.

3.3 The Impact of Acquisitions on Firm Strategy, Patient Outcomes, and the Cost of Dialysis Care

In this section, we show how independent facilities change their behavior after being acquired by a chain and how these changes then impact the quality and cost of care. To do so, we use a differences-in-differences research design that compares

independent facilities acquired by chains to those that are never acquired:

$$Y_{ijt} = \beta^{Pre} D_{jt}^{Pre} + \beta^{Post} D_{jt}^{Post} + \beta^{Chain} D_{jt}^{Chain} + \alpha X_{ijt} + \epsilon_{ijt}, \tag{3.1}$$

where Y_{ijt} is the outcome of interest for patient i at facility j in month t; D_{jt}^{Pre} and D_{jt}^{Post} are indicators for whether facility j in month t will be acquired in the future or has already been acquired; and D_{jt}^{Chain} is an indicator for whether facility j is always owned by a chain. The excluded category comprises independent facilities that are not acquired during our sample period. Although X_{ijt} varies by specification, in our preferred specification it includes a host of facility and patient controls, including age, comorbidities, race, sex, time on dialysis, and facility age[24]; X also includes year, state, and facility fixed effects. Without facility fixed effects, β^{Post} would capture the mean difference in Y for facilities that have been acquired relative to facilities that are never acquired in our sample, conditional on other covariates. To avoid measurement error in the date of acquisition, and to allow enough time for a firm's strategy to be fully implemented at an acquired facility, we exclude all observations within a six-month window on either side of the acquisition date.[25] In all specifications, we cluster standard errors at the facility level.[26]

The primary threat to identification in this setting is that chains may acquire independent facilities whose patients have certain characteristics that affect Y through

[24]Specifically, controls include: sex, race, BMI, kidney function, diabetes, hypertension, cancer, drug use, alcoholism, smoker, requiring assistance with daily activities, chronic obstructive pulmonary disease, atherosclerotic heart disease, peripheral vascular disease, ischemic heart disease, congestive heart failure, facility for-profit status, income quintile, % of those between 18-24 with just a college degree, % of those between 18-24 with just a high-school diploma, patient age, facility elevation, and facility age.

[25]As shown in Appendix B.8, however, our main results are robust to including this period, although slightly attenuated due to the introduction of measurement error in the timing of acquisitions.

[26]Clustering at the patient level yields standard errors 25-75% smaller than those clustered at the facility level, so we report standard errors clustered by facility as the more conservative of the two approaches.

channels other than a change in ownership. As shown in Table 3.1, however, patients treated at independent facilities acquired by chains are not systematically different along observable characteristics than those treated at other independent facilities. Additionally, the richness of our data allows us to control for all clinically relevant covariates, making this an even smaller concern. Lastly, to make a causal claim about acquisitions from a specification that includes facility fixed effects requires only that chains do not systematically change the mix of patients along unobservable dimensions when they acquire a facility, a relatively weak assumption. Moreover, our results are robust to the inclusion of patient fixed effects, which further limits this concern. Nevertheless, in Section 3.3.4 we also explore the possibility that patient selection may be a part of the strategy chains implement post acquisition and find that new patients at acquired facilities may be slightly healthier than those who were at the same facilities before they were acquired. These findings, if anything, suggest that our results may understate the true effects of an acquisition. In short, the rich data of our empirical setting allow us to cleanly identify the effects of acquisitions on facilities' practices and patients' outcomes, affording us a unique opportunity to disentangle the otherwise opaque nature and effects of firms' corporate strategies. It is also worth noting that even though our research design exploits within-patient changes at the same facility before and after acquisition, chains may target certain areas for potential growth, and so it is possible that acquisitions in these areas may not be independent of one another. We have explored whether there is any noticeable change in the behavior of facilities of the same chain when a nearby independent joins the chain and failed to find any. Relatedly, we have found that neighboring competing facilities do not noticeably change their behavior in response to a nearby acquisition. Thus, there do not appear to be spillover effects from acquisitions on neighboring facilities. Additionally, in our discussions with nephrologists, we have

been told that independent acquisitions are often driven by idiosyncratic reasons on the part of facility owners, such as retirement.

3.3.1 Drug Doses

We first consider the use of EPO at dialysis facilities due to its importance for firms' profits, its outsize effect on Medicare's total spending on drugs, and its potential for abuse by providers. The first two columns of Table 3.3 presents estimates of equation (3.1) where the dependent variable is the log of EPO doses per treatment.[27] Column (1) shows that although acquired facilities were already using slightly more EPO per treatment than independent facilities that are never acquired, they experience such a substantial increase following an acquisition that their levels converge to those of facilities always owned by a chain. Column (2) adds facility fixed effects and suggests that acquisitions cause EPO doses to more than double for patients at the same facility with the same observable characteristics.

[27]Dependent variable is log(1+Dose) in cases where the dose is 0.

Table 3.3: Acquisition Effects on Drug Doses

	(1) Epogen	(2) Epogen	(3) Ferrlecit	(4) Ferrlecit	(5) Venofer	(6) Venofer
Pre-Acquisition	0.270*		-0.0188		0.0650	
	(0.124)		(0.0558)		(0.0604)	
Post-Acquisition	1.350***	0.829***	-0.351***	-0.303***	0.784***	0.612***
	(0.0822)	(0.0725)	(0.0466)	(0.0627)	(0.0555)	(0.0751)
Always Chain	1.343***		-0.335***		0.722***	
	(0.0775)		(0.0391)		(0.0454)	
Observations	14,161,244		12,473,162		11,595,400	
Dep. Var. Mean	7.538		0.589		1.337	
Units	log(IU)		log(mg)		log(mg)	
Year x Month FE	X	X	X	X	X	X
Controls	X	X	X	X	X	X
Facility FE		X		X		X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Venofer and Ferrlecit specifications have different observations due to the availability of the two drugs. Ferrlecit was introduced in 1999 and Venofer in late 2000. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Drug doses are winsorized at the 99th percentile. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively. Controls include patient and facility characteristics.

By interpreting this estimate as the causal effect of an acquisition on EPO doses, we are relying on the assumption that an acquisition creates a discontinuous change in facility behavior and that any trends in dosing during the period surrounding an acquisition are common to all of the facilities in the control group. To support this assumption, in Figure 3.2 we plot EPO doses during the time period around acquisition, where the horizontal axis has the quarters relative to acquisition, quarter 0 is the quarter of acquisition denoted by a vertical dashed line, and the omitted category is the quarter prior to acquisition. The graph plots coefficients from estimating

$$Y_{ijt} = \sum_{s} \delta^s D^s_{jt} + \alpha X_{ijt} + \epsilon_{ijt}, \tag{3.2}$$

where D^s_{jt} is a dummy variable for facility j being acquired at time $t + s$ and X_{ijt} includes the same set of controls as equation (3.1), including facility fixed effects. We find no evidence of a pre-trend. We do see a short adjustment period of approximately 6 months following acquisition where facilities gradually adjust EPO doses upwards before leveling off. For this phenomenon to arise due to selection bias (in the sense that chains acquire facilities that were going to increase EPO doses irrespective of being acquired), acquiring firms would need to observe some indication of a looming increase in doses when negotiating the sale of the facility. This strikes us as implausible given that negotiations occur many months prior to the date of acquisition.

We extend our baseline analysis to study the effect of acquisitions on the use of two other commonly used intravenous drugs given to patients with anemia, the iron-supplement drugs Ferrlecit and Venofer. The last four columns of Table 3.3 repeats the research design to focus on these drugs, with the number of observations differing across the columns because Ferrlecit and Venofer did not receive FDA approval until 1999 and 2000, respectively, whereas EPO was in use at the start of our sample in

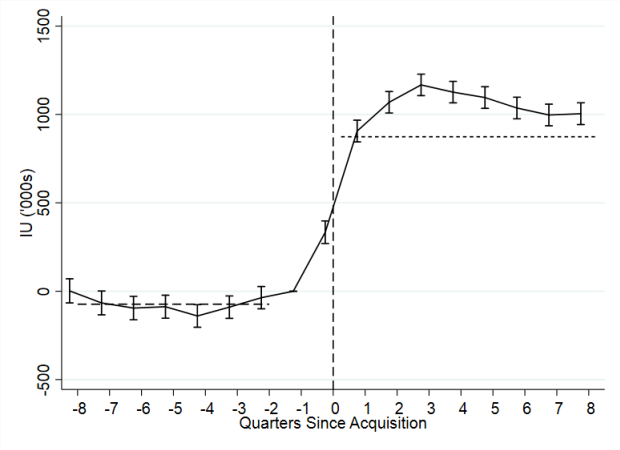


Figure 3.2: EPO Dosing Dynamics at Acquired Firms

Notes. Months outside the 48 month window are included in the regression but not shown here. Observations are binned by quarter to reduce noise. Error bars are 95 percent confidence intervals. Observations within 6 months of acquisition are included in this plot.

1998. Due to delays in the creation of HCPCS codes, we have Ferrlecit doses since 2001 and Venofer doses since 2002. The results in Table 3.3 show that acquired facilities substantially increase their use of Venofer and decrease their use of Ferrlecit.

The switch from Ferrlecit to Venofer reflects the profits at stake. As discussed in Section 3.1.2, Ferrlecit and Venofer are essentially substitutes for one another and are reimbursed by Medicare at nearly the same per-unit rate, but differences in how manufacturers package the two drugs make Venofer a potentially more lucrative drug for providers because it allows them to bill for more "unavoidable" waste. To illustrate the onset of these strategies at newly acquired firms, we replicate Figure 3.2 for both Venofer and Ferrlecit in Appendix B.13.[28]

[28]This appendix also contains event studies for the other dependent variables analyzed in this section.

3.3.2 Facility Inputs

The results in Section 3.3.1 clearly show that chains strategically alter the drug doses of patients at newly acquired facilities. In this subsection, we investigate how they alter the input choices of their targets following takeovers in ways that reduce costs. To do so, we modify our baseline specification (3.1) to analyze data at the facility-year level, as data for many of the inputs (e.g., staff and the number of dialysis stations) are only available annually. Specifically, we include facility fixed effects and estimate specifications of the form

$$Y_{ijt} = \gamma^{Post} D_{jt}^{Post} + \delta X_{jt} + \nu_{jt}. \tag{3.3}$$

Aside from the change in the unit of observation, this analysis is very similar to our patient-level analysis and relies on similar identifying assumptions. Namely, for a causal interpretation of γ^{Post}, we require that the acquisition results in a discrete change in the environment determining facilities' input choices. With annual data, measurement error for the timing of acquisitions is an even greater concern because some inputs (e.g., staff) may change part way though the year, but we would not observe the new levels until the following year's report. To remedy this, we drop the entire year of acquisition for each facility that changes ownership, keeping only observations where a facility has the same ownership for the entire year.

Table 3.4 displays the effect of acquisitions on facility-level labor and capital decisions. These estimates show a consistent shift in the use of certain inputs by chains, with acquired facilities decreasing their use of nurses while increasing their use of dialysis technicians. Such a switch reduces facilities' costs because technicians have

less training and are therefore paid less than nurses.[29] Upon acquisition, the target firm decreases its nurse-technician ratio by roughly 15.1%. Newly acquired facilities also stretch their resources by increasing their patient-to-employee ratio by 11.7% and their patient-to-station ratio by 4.5%. Taken together, we find that acquiring firms adjust the inputs of their targets by substituting away from more-experienced, higher-cost labor and by increasing both the number of patients per employee and station.

Although these changes reduce the acquired facilities' operating costs, patients may have worse outcomes if being treated by busier employees with less training diminishes their quality of care. Moreover, if the number of patients per station increases because the time each patient spends on a machine decreases, or because machines are not adequately cleaned between patients, this, too, may result in worse outcomes for patients, as shown in Grieco and McDevitt (2017).

[29]Dialysis technicians typically require only 12 months of training, much of which is done on the job. By contrast, nurses are typically required to pass an RN licensure exam.

Table 3.4: Acquisition Effects on Facility Input Choices

	(1) Nurses	(2) Technicians	(3) HD Patients	(4) Total Stations	(5) Nurses per Tech	(6) Patients per Employee	(7) Patients per Station	(8) Employees per Station
Post-Acquisition	-0.0204	0.0456*	0.134***	0.0210	-0.146***	0.599***	0.179*	-0.0289
	(0.0194)	(0.0230)	(0.0187)	(0.0410)	(0.0410)	(0.107)	(0.0825)	(0.0185)
Observations	24,868	24,868	42,944	43,046	23,217	24,868	43,046	24,868
Dep. Var. Mean	1.548	1.703	61.554	18.574	0.969	5.129	3.992	0.814
Units	log(FTE)	log(FTE)	log(Patients)	log(Stations)	-	-	-	-
Year FE	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a facility-year. Sample includes facilities involved in an independent-to-chain acquisition and facilities which are independent or owned by the same chain for the entirety of our sample. We drop observations in the year of acquisition. FTE are Full-Time Equivalents. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

3.3.3 Patient Outcomes

The richness of our data, along with the clinical and operational links between drugs and facility inputs, allows us to connect the changes in strategy at an acquired facility to its effects on patient outcomes. In this way, we can demonstrate how acquisitions directly impact the quality of care received by patients and the cost of this care for Medicare.

We begin by considering a number of clinical outcomes. The first three columns of Table 3.5 show the effect of acquisitions on patients' urea reduction ratio (URR) and hemoglobin (HGB) levels, two important diagnostic measures for dialysis patients. The dependent variable in column (1) of Table 3.5 measures the probability that a patient's URR reaches 0.65, the lower bound of how much urea should be removed from a patient's blood during a dialysis session according to accepted standards of care (see Section 3.1.1 for details). We find a 2.1% increase in the probability that a patient has an adequate URR following acquisition, one of the few cases where quality improves at independent facilities after being acquired by a chain.

In columns (2) and (3) of Table 3.5, we examine how acquisitions affect patients' management of anemia. Consistent with patients' higher doses of EPO, we find that hemoglobin levels at acquired facilities rise, with a 10.0% increase in the likelihood that patients have HGB in excess of the recommended range and a 5.1% decrease in the likelihood that patients have HGB within the recommended range. In Appendix B.7, we expand Table 3.5 to show that the average HGB level increases and the number of patients with low HGB declines post acquisition.

Hospitalizations represent another indicator of a facility's overall quality. Column (4) of Table 3.5 shows the results from estimating our primary specification where

Table 3.5: Acquisition Effects on Outcomes

	(1) URR Good	(2) HGB Good	(3) HGB High	(4) Hospitalized Any Cause	(5) Payments Per-Session
Post-Acquisition	0.0183***	-0.0266**	0.0382***	0.00599***	0.0665***
	(0.00496)	(0.00825)	(0.00899)	(0.00170)	(0.00617)
Observations	14,161,244	13,271,104	13,271,104	14,161,244	14,161,243
Dep. Var. Mean	0.881	0.523	0.382	0.141	5.150
Units	pp	pp	pp	pp	log($)
Year x Month FE	X	X	X	X	X
Pat. & Fac. Controls	X	X	X	X	X
Facility FE	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Hemoglobin specifications have different observations because it is not submitted with non-ESA claims for some of our sample. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Payments are winsorized at the 99th percentile. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

the dependent variable is equal to 1 if a patient was hospitalized for any reason during the month and 0 otherwise.[30] Hospitalizations increase 4.2% after acquisition, with patients becoming specifically more likely to be hospitalized for septicemia and cardiac events (see Appendix B.7). For septicemia, the blood infection common among dialysis patients, we find that patients are 10.0% more likely to be hospitalized following an acquisition. Because these infections are avoidable through the proper cleaning and disinfecting of dialysis machines between patients (Patel et al., 2013), we consider the two most likely explanations for the higher rate of infections following a takeover to be (i) the decrease in per-patient staffing levels at acquired facilities, which leave employees with less time to properly clean machines between patients (column (7) of Table 3.4) and (ii) the relative increase in the use of lower-skilled employees who may be less likely to follow proper cleaning and treatment protocols (column (5) of Table 3.4). Patients are also 2.1% more likely to be hospitalized for an adverse cardiac event following acquisition, although this effect is not statistically significant (p-value of 0.298).[31] Such an increase would be expected given the larger EPO doses received by patients post acquisition (Table 3.3), as the principal risk of elevated hemoglobin values (Table 3.5) is a higher incidence of adverse cardiovascular events.

The number of patients referred for a kidney transplant represents another important measure of a facility's quality.[32] The first two columns of Table 3.6 presents results from estimating equation (3.1) with an indicator for whether an incident patient was waitlisted or transplanted within a given time frame as the dependent

[30] Episodes of hospitalization are assigned to the month in which they begin.

[31] It is worth noting that the estimate is statistically significant when we include patient fixed effects, suggesting that unobservable patient characteristics play an important role in cardiac events. See Table B.91 in Appendix B.9.

[32] See Patzer et al. (2015) for much more on the relationship between kidney transplants and dialysis facilities.

variable. After acquisition, new patients are less likely to be placed on a transplant waitlist or to receive a transplant during any of the time frames we study. One year after starting dialysis, a new patient at an acquired facility is 8.5% less likely to receive a transplant or be on the waitlist for a transplant than he or she would have been at the same facility before it was acquired. After 730 days patients are 9.0% less likely to be placed on the waitlist or receive a transplant.

As a final measure of quality, we consider patients' survival rates. The last two columns of Table 3.6 presents estimates of an acquisition's effect on patients' survival rates after 365 and 730 days since starting dialysis. We restrict our attention to patients starting dialysis at facilities that do not change ownership or for whom the entire observation window is before or after acquisition (e.g., to be included in the 365-day specification, a patient must start dialysis more than 365 days prior to the acquisition date). We further restrict our attention to those patients who remain at the same facility until their date of death or the end of the observation window.[33] We find that patients' 365-day survival rate decreases by 1.27 percentage points, or 1.7%. After 730 days patient survival rates fall by 2.9%.

When considering the totality of our results for clinical outcomes, hospitalizations, transplants, and survival, the overarching finding is that acquisitions result in worse care for patients. But providing high-quality care is costly, so it remains possible that these acquisitions could reduce overall spending on dialysis, making the overall impact on welfare inconclusive. We do not find evidence that acquisitions reduce Medicare expenditures in the dialysis industry, however, as the final column of Table 3.5 shows that acquired facilities increase their per-session Medicare reimbursements by 6.9%, amounting to $252.4 million in additional spending for Medicare

[33] We have done robustness checks estimating these effects including all patients as well as those who return to the facility within 30 or 60 days, finding similar results.

Table 3.6: Acquisition Effects on Transplants and Mortality

	Waitlisted or Transplanted Within:		Survives for:	
	(1) 365 Days	(2) 730 Days	(3) 365 Days	(4) 730 Days
Post-Acquisition	-0.0108*	-0.0188*	-0.0127**	-0.0174**
	(0.00468)	(0.00738)	(0.00476)	(0.00654)
Observations	610,955	498,056	539,487	457,184
Dep. Var. Mean	0.127	0.208	0.746	0.597
Units	pp	pp	pp	pp
Year FE	X	X	X	X
Pat. & Fac. Controls	X	X	X	X
Facility FE	X	X	X	X

Notes. Estimates from OLS regression. Facility-clustered standard errors in parentheses. An observation is a new dialysis patient. Sample includes new patients starting dialysis at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. For the mortality specifications we drop any patients who start dialysis at facilities acquired within six months of acquisition. We only include those patients who remain at their original facility until death or the end of the observation window. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

throughout our sample. In short, we find that acquisitions lead to clear changes in firm strategy that substantially worsen the quality of care received by patients and increase the cost of care borne by Medicare.

3.3.4 Patient Selection

Although the results above are robust to controlling for patient observables and (where feasible) patient fixed effects, we also consider whether a facility changes its mix of patients following an acquisition for two reasons. First, if observable patient attributes at a facility change post acquisition, it may suggest that selection on unobservables could be biasing our results. Second, the ability of chains to selectively treat desirable patients may be an important strategy in and of itself, often referred to as "cream

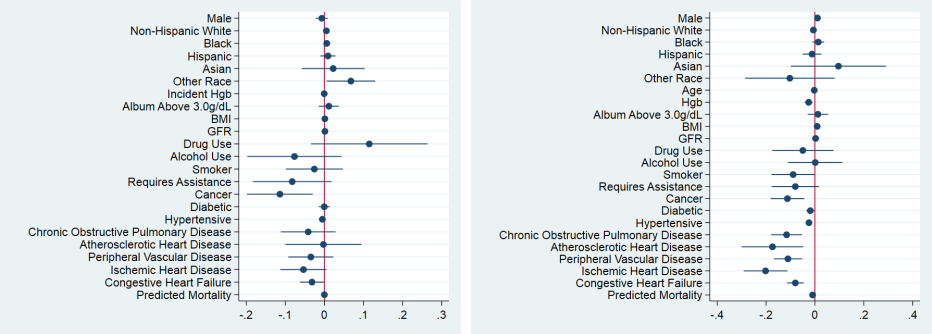
skimming."

To conduct this analysis, we estimate a series of differences-in-differences specifications with facility and time fixed effects, where the dependent variables are the patient-level controls from the previous specifications, as displayed in equation (3.4):

$$X_{ijt} = \beta^{Post} D_{jt}^{Post} + \gamma_j + \delta_t + \epsilon_{ijt}. \tag{3.4}$$

We estimate this specification for both the main patient-month sample as well as a sample restricted to patients in their first month on dialysis, with the results presented in Figure 3.3. Each plot displays the coefficient estimates of β^{Post} along with 95% confidence bands, all rescaled by the mean of their respective variables.

As shown in Figure 3.3a, we do not find any systematic evidence of cream skimming in the monthly data. In Figure 3.3b, however, we do find some slight evidence that the characteristics of new patients change following an acquisition. In both cases, the changes are unequivocally in the direction of facilities treating healthier patients despite our finding of worse patient outcomes overall. For example, new patients at acquired facilities are less likely to have a variety of comorbid conditions, such as diabetes, hypertension, cancer, and heart disease. If these observable patient attributes are correlated with unobservable attributes, then our results suggest that selection would likely bias our findings of worse outcomes towards zero, making them conservative.



(a) Monthly Patients (b) New Patients

Figure 3.3: Changes in Patient Mix After Acquisition

Notes. Depicts differences-in-differences estimates of the changes in covariates after acquisition. Estimates are acquisition effects from equation (3.4). All values are rescaled by the sample mean of their respective covariates. Bars are 95% confidence bands. Standard errors are clustered at the facility level.

3.4 The Effect of Competition on Firm Behavior

In this section, we investigate whether competition from other dialysis firms can discipline the behavior of newly acquired facilities. With the price for most dialysis treatments fixed by Medicare, facilities may compete for patients by offering higher-quality treatments or other services. Such competition may prevent the acquirer from implementing its strategies to increase profits if patients respond to the corresponding decline in quality by defecting to a rival facility. In what follows, we find no evidence that market concentration mitigates the transference of firm strategy in the dialysis industry. In this way, our findings echo those of Cutler et al. (2017b), who, using a different identification strategy and more-aggregate data, also find no evidence that increased concentration from national mergers affects the quality of care received by dialysis patients. We argue below that a key reason that competition does not affect facilities' behavior is that patients rarely respond to differences in quality, as reflected in the low number of patients who switch facilities each year.

To investigate the effect of concentration on firm behavior, we must first establish

a relevant geographic market and then select an appropriate measure of concentration. The existing literature lacks a clear consensus on how to define markets for the dialysis industry — Cutler et al. (2017b) and Grieco and McDevitt (2017) define markets as Hospital Service Areas (HSAs); Wilson (2016a) and Dai (2014) use counties; and Wilson (2016b) and Eliason (2019) develop facility-specific markets using distance bands around each facility. In light of this, we focus below on a specification that defines markets as HSAs and uses HHI to measure concentration but show in Appendix B.11 that our results are robust to a variety of other market definitions and measures of concentration.

3.4.1 Most Acquisitions Do Not Change Market Concentration

We begin by examining whether the acquisitions of independent facilities by chains actually affect market concentration. We first locate market-months where an acquisition will occur in the following month, finding 891 such instances.[34] We then calculate the HHI for that market and what the HHI would have been if the acquisition had already occurred.[35]

Figure 3.4 shows a scatterplot of pre- and post-acquisition HHI for each HSA-month where an acquisition is about to occur (we reduced the transparency of each dot to 30% so that darker regions imply more overlapping markets or more mass in that area). HHI increases in only 34.4% of HSA-months following an acquisition.[36]

[34] This is less than the total acquisitions due to HSA-months where multiple facilities are acquired.

[35] We use this as our definition of post-acquisition HHI to avoid confounding the effect of acquisition with the entry of new dialysis facilities.

[36] Note that 32.6% of markets where acquisitions occur have only one facility, denoted by the mass at (1,1) in the figure.

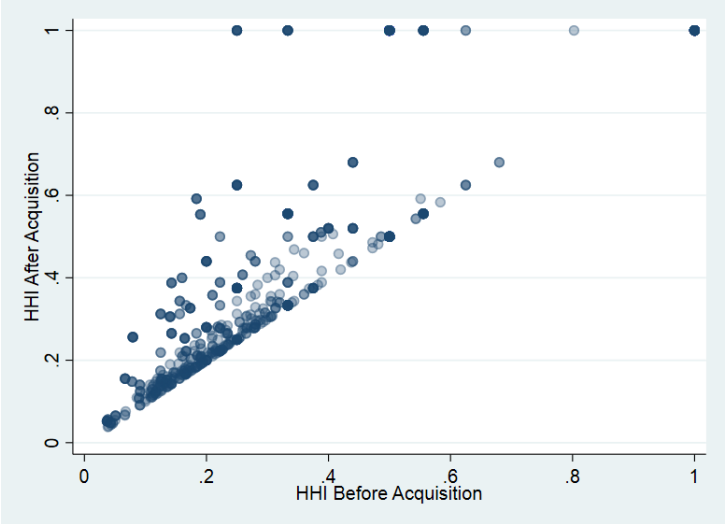


Figure 3.4: Changes in Concentration Across Markets

Notes. An observation is an acquisition. The horizontal axis depicts the Hospital Service Area's HHI before acquisition. The vertical axis depicts what the Hospital Service Area's HHI would have been in the month before acquisition had the facility already been acquired. Opacity is reduced to 30%, so darker regions represent regions of more mass.

That HHI increases in so few markets following a takeover strongly suggests that changes in facility behavior and patient outcomes are not driven by changes in market concentration. To this point, we find that our results are quantitatively very similar to those in Section 3.3 when we restrict our sample to markets with only one facility, meaning that the results for these markets could not possibly be explained by changes in concentration.[37] Rather, firm strategy appears to be the main determining factor.

3.4.2 Acquisitions That Increase HHI Have Similar Effects

Next, we show in Table 3.7 that the outcomes in markets where an acquisition increased concentration do not differ from those where an acquisition did not affect market concentration. To do so, we modify our baseline specification by interacting

[37]See Table B.119 in Appendix B.11.

our post-acquisition dummy variable with a dummy variable for whether the acquisition of that facility increased HHI in the market, defined here as an HSA.[38] Formally, this estimating equation is

$$Y_{ijt} = \beta^{Post} D_{jt}^{Post} + \gamma D_{jt}^{Post} \times IncreasesHHI_j + \alpha X_{ijt} + \epsilon_{ijt}, \tag{3.5}$$

where $IncreasesHHI_j$ is a dummy variable indicating if the acquisition of facility j increased the market's HHI. The effects in Table 3.7 are not substantially different from our baseline results, either qualitatively or quantitatively. In addition, we see no effect on the indicator variable for acquisitions that increase HHI, implying that the changes in outcomes we see after an acquisition are not driven by changes in market concentration, leaving changes in management practices as the most likely explanation. As mentioned above, we provide further support for this result in Table B.119 in Appendix B.11, which shows in a sample restricted to markets with only one facility (so that there can be no change in concentration following an acquisition) that the effects of acquisitions are very similar to the baseline results. Further, Table B.1110 in Appendix B.11 shows that even in markets that are deemed "non-worrisome" by antitrust agencies due to either their low levels of concentration or small changes in HHI post acquisition, we find very similar effects.

A noteworthy implication of these results is that consolidation can have detrimental effects irrespective of market concentration. As acquisitions lead to fewer active firms nationwide, the strategies and management practices of the expanding firms may increasingly affect aggregate outcomes. In this case, acquisitions drive both concentration and a decrease in the quality of care, but the channel through which the latter occurs is the transference of firm strategy, not an increase in market power.

[38] In Appendix B.11, we show that our results are robust to other measures of concentration beyond HHI, a continuous measure of the change in HHI, and other market definitions.

Table 3.7: Acquisition Effects By Concentration Increase: HSA Markets

	Drugs			Clinical Outcomes			Hospitalized
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Epogen	Venofer	Ferrlecit	HGB High	HGB Good	URR Good	Any Cause
Post-Acquisition	0.808***	0.553***	-0.286**	-0.0313**	-0.0123*	0.0174*	0.00800**
	(0.0752)	(0.123)	(0.100)	(0.0112)	(0.00533)	(0.00708)	(0.00250)
Post-Acquisition ×	-0.0486	0.0891	-0.0267	0.00747	0.00120	0.00156	-0.00318
Increases HSA HHI	(0.0823)	(0.151)	(0.124)	(0.0153)	(0.00614)	(0.00893)	(0.00324)
Patient-Months	14,161,244	11,595,400	12,473,162	13,271,104	13,271,104	14,161,244	14,161,244
Units	log(UI)	log(mg)	log(mg)	pp	pp	pp	pp
Pat. & Fac Controls	X	X	X	X	X	X	X
Year x Month FE	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Drug doses are winsorized at the 99th percentile. The number of observations differs from the baseline specification due to missing ZIP Code-to-market crosswalk data. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

3.4.3 Why Competition Does Not Discipline Provider Behavior

In regulated markets, standard models of competition with endogenous provider quality predict that quality will increase with the extent of competition in the market (e.g., Gaynor (2004) and the models discussed therein). This theoretical result relies on the assumption that demand increases with product quality, which in our setting would mean that patients are more likely to choose a high-quality facility, all else equal, and thus facilities would compete for patients by offering higher-quality care. In practice, patient demand in the U.S. dialysis market does not respond to the decline in quality following an acquisition. As suggested in column (7) of Table 3.4, acquired facilities actually increase the number of patients they treat per machine despite providing lower-quality treatments.

We look more directly at this result by considering whether patients are more likely to switch away from a facility after it is acquired, finding that they are not. In general, it is uncommon for dialysis patients to switch providers, with 98.4% of patient-months in our sample such that the patient visits the same facility the following month. For patients who have completed 12 months of dialysis, only 1.3% of patient-months represent a permanent switch away from a facility.

In addition to the low absolute levels of switching among patients, we show in Table 3.8 that patients do not become more likely to switch after their facility is acquired. For the full sample of patients, our point estimate of the effect of acquisition on switching is -0.06 percentage points, which is small economically and not statistically significant at conventional levels. In addition, we find that acquisitions do not have a meaningful impact on patients' likelihood of switching in their first year or if

Table 3.8: Effect of Acquisition on Facility Switching

	All		First Year	
	(1) Any	(2) Never Return	(3) Any	(4) Never Return
Post-Acquisition	-0.000707	-0.000467	-0.000384	-0.000300
	(0.000507)	(0.000454)	(0.000847)	(0.000772)
Observations	13,898,240	13,898,240	3,416,860	3,416,860
Dep. Var. Mean	0.016	0.013	0.024	0.020

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Columns (3) and (4) include only patients in their first 12 months on dialysis. The dependent variable in columns (1) and (2) is 1 if the patient is on dialysis the next month at a different facility and 0 if they remain on dialysis at their current facility. The dependent variable in columns (2) and (4) is 1 only for those patients who do not return to the initial facility at any point in our sample. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

we only include facility switches where the patient does not return to his or her initial facility.

A host of institutional and behavioral factors explain why patients do not switch from low-quality providers. In many markets, patients may not have a valid outside option, as one-third of markets in our sample have only 1 facility. Our findings are unchanged, however, if we repeat the analysis in Table 3.8 but restrict our sample to include only markets with at least two facilities.[39] Moreover, even patients who live in markets with multiple facilities face significant travel costs due to the frequency of visits required for dialysis, as documented in Eliason (2019). These travel costs are exacerbated by comorbidities that make travel difficult as well as the low income of most dialysis patients. As such, travel costs may outweigh concerns about a facility's

[39]Results available upon request.

quality for most patients. Behavioral inertia likely also plays a significant role in this market, as it does in other health-care settings (e.g., Handel, 2013; Tilipman, 2018).

3.5 Understanding Pre-Acquisition Differences Across Chain and Independent Facilities

In the analysis above, we find that the effects of an acquisition persist even when it is not accompanied by a change in market structure. In almost all cases, independent facilities acquired by chains had better patient outcomes but lower profits than chain facilities prior to being acquired. Shortly after acquisition, the chains implement new policies regarding, for instance, the facilities' drug doses and staffing levels, which then lead to higher profits but worse outcomes for patients. Because competitive pressure does not explain why independent facilities do not imitate the behavior of the more-profitable chain facilities before acquisition, as shown in Section 3.4, in this section we explore several alternative explanations. We find the most prominent reason relates to differences in the potential tradeoffs facilities face regarding maximizing their own profits and maintaining high standards of care, which stem primarily from differences in economies of scale for purchasing injectable drugs.

To conduct this analysis, we supplement the USRDS data with data from HCRIS that include accounting costs for key facility inputs, such as EPO, which allows us to better understand the differences in facilities' costs and why some might behave differently. We estimate the impact of an acquisition on total variable profits per dialysis session and several variables related to EPO using the following specification:

$$Y_{jt} = \beta^{Pre} D_{jt}^{Pre} + \beta^{Post} D_{jt}^{Post} + \beta^{Chain} D_{jt}^{Chain} + \alpha X_{jt} + \epsilon_{jt}, \tag{3.6}$$

where X includes state and year fixed effects. From the results presented in Table 3.9, we find no evidence that chains disproportionately acquire the least-sophisticated or worst-performing independent facilities to turn around, unlike in other settings where sharply declining financial performance prompts an ownership change (Brav et al., 2015). That is, acquired independents are no less profitable than the independents that are not acquired.[40] Column (1) shows that, on average, independent facilities that are eventually acquired earned a statistically insignificant \$1.36 more in variable profits per session (these exclude fixed costs such as rent) before acquisition compared to the omitted group, independent facilities that are never acquired. After acquisition, per-session variable profits increase by \$16.81 (= \$18.17 – \$1.36) at the acquired independent facilities.[41] This suggests that chains do not selectively acquire low-performing independent facilities; rather, both acquired and not-acquired independent facilities had similar profits prior to acquisition. After acquisition, the new owners then improve the financial performance of their targets, similar to what Braguinsky et al. (2015) found for Japanese cotton mills and Natividad (2014) found for a large fishing firm that acquired some of its suppliers.

Most of the increase in per-session profits at acquired facilities comes from EPO. Column (2) of Table 3.9 shows that the profits from EPO increase by \$8.43 per session, or 50.1% of the total increase in profits shown in column (1). EPO is more profitable for chains in part because they pay lower prices for the drug, as shown in column (3), which reflects the volume discounts they negotiate with drug suppliers. For example, in DaVita's 2005 Annual Report, the company writes, "Our agreement with Amgen

[40] If anything, acquired independents were behaving slightly more like for-profit chains prior to acquisition with respect to EPO doses, as shown in column (1) of Table 3.3, meaning that there were likely fewer profitable opportunities to increase patients' doses following acquisition.

[41] The difference is \$17.73 based on a specification with facility fixed effects. We focus on the specifications without facility fixed effects because they allow us to compare the pre-acquisition profits of acquired facilities to the profits of facilities that were never acquired.

Table 3.9: Effect of Chain Acquisition on Profit Measures

	(1) Variable Profits per Session	(2) EPO Margin	(3) EPO Cost Per 1000 IUs	(4) EPO Units per Session	(5) Total EPO Costs
Pre-Acq	1.360	-0.581	-0.371**	222.5	-0.451
	(2.497)	(1.652)	(0.141)	(204.1)	(1.723)
Post-Acq	18.17***	7.851***	-1.237***	778.8***	0.965
	(2.205)	(1.334)	(0.145)	(171.9)	(1.464)
Always Chain	22.16***	7.975***	-1.340***	812.2***	0.745
	(2.344)	(1.626)	(0.156)	(193.4)	(1.724)
Constant	30.60***	1.113	9.190***	3835.8***	35.36***
	(3.704)	(3.399)	(0.205)	(265.7)	(2.833)
Year FE	X	X	X	X	X
State FE	X	X	X	X	X
Observations	25,934	25,934	25,934	25,934	25,934
Post - Pre	16.81	8.432	-0.866	556.3	1.416
P-value	[0.000]	[0.000]	[0.000]	[0.000]	[0.0720]
Always Chain - Post	3.993	0.123	-0.103	33.42	-0.220
P-value	[0.002]	[0.880]	[0.000]	[0.732]	[0.806]

Notes. Facility-clustered standard errors in parentheses. An observation is a facility-year. Sample includes facilities involved in an independent-to-chain acquisition and facilities that are independent or owned by the same chain for the entirety of our sample. We drop observations in the year of acquisition and those cost reports that are for fewer than 365 days. EPO margin is calculated as the average national payment rate per 1000 IU less the costs from the cost reports. Top panel shows coefficient estimates from equation (3.6). Bottom panel shows estimated difference between post-acquisition and pre-acquisition coefficients and always chain and post-acquisition coefficients, along with p-values. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

for the purchase of EPO includes volume discounts and other thresholds which could negatively impact our earnings if we are unable to meet those thresholds." Also, "Our contract with Amgen provides for specific rebates and incentives that are based on ... purchase volume growth." Facing lower costs for EPO, chains use more of it, as shown in column (4), so total EPO expenditures differ little after acquisition, as shown in column (5).

The scale economies stemming from buyer power are not available to smaller independent facilities, and this is a key reason why their behavior differs from chains' prior to acquisition. If independent providers treated patients with the same doses that the largest chains do, they would earn only 55% of the profits due to their higher wholesale costs for EPO.[42] If providers balance the financial gains of giving patients larger EPO doses against the risks and non-pecuniary costs of doing so, such as an elevated risk of cardiac events for patients, this difference in per-unit costs may lead chains to administer more EPO to their patients. That is to say, these cost differences may induce different dosing strategies even if chains and independents are both seeking to maximize their profits.[43]

Another possible explanation for why independent and chain facilities behave differently is that chains may have different underlying objectives, perhaps focusing more on financial performance than on patient outcomes. One way to proxy for the incentives that a firm faces is its for-profit status. Because the largest chains are for-profit firms and many independent facilities are non-profit, the change in for-profit status following an acquisition may explain the changes in behavior and outcomes rather than the change in ownership itself. A related argument is made by Eaton et al. (2018), who show that the high-powered incentives introduced by private equity owners following takeovers in higher education lead to better financial performance but worse student outcomes. We explore this possibility in Appendix B.10, finding in Tables B.101 and B.102 that the post-acquisition changes across most of our measures are largely the same for all acquired independent facilities, regardless of whether they

[42]This assumes that Medicare reimburses \$10 per 1000 IUs of EPO, which is a close approximation of the actual rate during the study period: $\frac{10-(9.19-0.37)}{10-(9.19-1.34)} = 0.5488$.

[43]The result that newly acquired independents benefit from chain-level economies of scale contrasts somewhat with the results of Blonigen and Pierce (2016), who find little evidence of merger efficiencies in U.S. manufacturing.

were previously non-profit or for-profit. There are a few notable exceptions to this: the effects of acquisition on EPO and Venofer doses, as well as the use of technicians, are all smaller in the case of for-profit independent facilities. The effect is diminished primarily because for-profit independent facilities were already behaving more like chains along these dimensions before they were acquired, suggesting that changes in for-profit status may account for some portion of our results. At the same time, these differences are relatively small, suggesting that the effects arising from a change in for-profit status are secondary to the effects from a change in ownership.[44]

In addition, chains' behavior might seem risky given the potential negative impacts on patient care resulting from excessive drug doses or low staffing levels. Chains may be more willing than independent facilities to accept this risk if they have large financial reserves to pay for any future litigation, allowing them to behave in ways that increase their profits even if it makes it more likely they will face malpractice lawsuits. Perhaps reflecting this, DaVita has made at least four settlements exceeding $100 million in the last 10 years.

Other possible explanations for the differences in behavior between independent and chain facilities lack empirical support. For example, we showed above in Section 3.3.4 that chain and independent facilities treat a very similar distribution of patients, so it is unlikely that a change in patient mix following a takeover alters a target's behavior. Another possible explanation is that chains may be subject to different regulations than independent facilities, but both types of facilities face the same regulatory environment, such as Medicare reimbursement rates and certification standards. Given the lack of support for these alternative explanations, we conclude that a leading explanation for why independent facilities do not employ the same

[44]These results are in line with those of Duggan (2000), who finds evidence that non-profit hospitals are no more altruistic than for-profit ones.

strategies as chains is that they face different tradeoffs when balancing profits and patient care, the majority of which arise from differences in economies of scale.

3.6 Conclusion

Changes in ownership affect the treatment and outcomes of patients at independent dialysis facilities acquired by chains. Our results show that acquired facilities change their behavior in three broad ways, each of which either increases their revenue or decreases their operating costs. First, acquired facilities capture higher per-session reimbursements from Medicare by increasing drug doses and shifting to more-lucrative drugs. Second, acquired facilities stretch their resources by treating more patients relative to the number of staff and stations at the facility. Third, acquired facilities reduce their costs of providing dialysis by replacing high-skill nurses with lower-skill technicians.

Adopting the acquirer's strategies causes the acquired facility's quality of care to decline. Along almost every dimension we measure, patients fare worse at the target facility after acquisition, most prominently in terms of fewer kidney transplants, more hospitalizations, and lower survival rates. Because Medicare spends more after acquired facilities implement their strategic changes, we interpret the diminished quality to represent an unambiguous decline in the overall value of dialysis treatments,

at least in the short run.[45] More research is needed to understand the implications for total welfare, as these acquisition may promote access to dialysis in underserved markets.

Our findings have important policy implications, as most of the acquisitions we study fall outside the scope of current antitrust laws, which prohibit acquisitions if "the effect of such acquisition may be substantially to lessen competition, or to tend to create a monopoly" (U.S. Department of Justice and Federal Trade Commission, 2010). To the extent that the diffusion of firm strategy, rather than a change in market concentration, causes the quality of dialysis care to decline, minor adjustments to the current antitrust statutes may do little to prevent the harmful effects of these acquisitions.

One policy prescription would be to avoid enacting regulations that could unintentionally spur consolidation, such as certificate of need laws that make new entry more difficult for expanding health-care providers and lead them to favor acquisitions instead (Pozniak et al., 2010). Others have raised concerns that policies that increase the administrative burdens for facilities may inadvertently increase consolidation (Gaynor, 2018), along with certain aspects of Medicare's reimbursement policies. By tying each firm's reimbursements to the costs of comparable firms, regulators encourage cost minimization through "yardstick competition" (Shleifer, 1985), which may increase the pressure to consolidate if greater economies of scale are necessary to decrease costs and maintain high profit margins. Similarly, the uniform fee-for-service

[45] A possible benefit from the cost-cutting strategies of chains is that they may be eventually incorporated into the reimbursement rate, resulting in lower costs for Medicare. Although we cannot rule out this possibility due to the fact that we do not observe the counterfactual reimbursement rate, this effect seems likely to be small. Over the period of our study, the dialysis reimbursement rate rose steadily. Furthermore, when Medicare combined payments for injectable drugs and dialysis into a single prospective payment in 2011, the new payment rate was designed to be approximately budget neutral, based on historic EPO usage. Thus, the high use of EPO by chains resulted in higher prospective payments for all providers after 2011.

reimbursement policy for injectable drugs may also contribute to consolidation, as it favors large firms that can negotiate lower prices for drugs. Although each of these policies likely has beneficial aspects, their tendency to drive consolidation should nevertheless be viewed as a tradeoff against those benefits.

Our results also illustrate the importance of well-designed payment systems in controlling health-care costs and improving patient outcomes. As we show in the case of EPO, poorly structured reimbursement schemes can induce provider behavior that not only wastes resources, but also harms patients. By improving the design of Medicare's payment systems, policymakers can simultaneously reduce costs and improve outcomes. Some changes in this direction have already occurred. In 2011, for example, Medicare bundled payments for dialysis treatments and their associated injectable drugs into a single Prospective Payment System, which effectively reduced providers' financial incentives to overuse EPO. To address the resulting incentive to use too little EPO, the Quality Incentive Program initiated in 2012 allows Medicare to penalize providers that fail to meet certain quality standards: providers that have too many patients below the benchmark for hemoglobin levels, for example, could lose up to 2% of their entire reimbursement from Medicare. Although these changes would seem to improve facilities' incentives for providing high-quality and cost-effective care, more research is needed to understand how they have changed the industry and affected patients (Eliason et al., 2019a).

Finally, because dialysis is a market in which the government, via Medicare, plays an outsize role in subsidizing care and in which patients may find it difficult to observe their facilities' quality, competition may be unlikely to discipline providers' behavior. Our findings are therefore likely to be applicable to similar settings in other areas of health care or higher education. Indeed, Eaton et al. (2018) show that private equity

buyouts in higher education lead to higher tuition and per-student debt, while at the same time resulting in lower graduation rates, loan repayment rates, and earnings among graduates. Complementing this result, Bernstein and Sheen (2016) find that private equity buyouts of restaurants lead to better health safety ratings, arguably a very visible measure of quality for consumers. As such, future work should consider how the effects of acquisitions differ in markets characterized by extensive government intervention, such as health care and education, compared to those without it, such as restaurants, as well as how these effects differ depending on how well patients or consumers can observe quality.

Chapter 4

The Effect of Bundled Payments on Provider Behavior and Patient Outcomes

4.1 Introduction

Health insurers in the United States have recently experimented with using bundled payments to restrain reimbursement costs. Prominent examples include Medicare's Bundled Payments for Care Improvementand Comprehensive Care for Joint Replacement initiatives. Under a bundled payment model, providers receive a single payment for a defined episode of patient care. Epidoes are often defined as individual procedures, such as joint replacements or cardiac rehabilitation, or providing treatment for a chronic disease for a specified period of time, such as cancer treatment. Importantly, reimbursements under bundled payment contracts are unrelated to the actual costs providers incur and services they provide. By holding multiple parties accountable for the cost and quality of care, proponents claim that a bundled payment system will encourage coordination among providers and reduce unnecessary expenses. At the same time, providers that receive a fixed payment irrespective of their costs may face an incentive to under-treat patients because additional expenses do not yield additional reimbursements, in stark contrast to Medicare's traditional fee-for-service model. Despite the inherent tradeoffs associated with a bundled payment system, and despite its growing prominence in Medicare's move towards alternative payment

models, little empirical work has examined the precise channels through which bundled payments alter providers' behavior. In this paper, we use detailed claims data from dialysis patients to show how the allocation of resources changes following the adoption of a bundled payment system and the resulting effects on patients' outcomes and providers' profits.

We focus on outpatient dialysis — a medical procedure that cleans the blood of patients suffering from end-stage renal disease (ESRD) — because it offers several distinct advantages as an empirical setting for this topic. First, dialysis is a fairly standardized treatment that allows for a direct comparison of providers and patients using detailed Medicare claims and clinical data. Second, the dialysis industry comprises a large share of U.S. health spending, with total Medicare reimbursements for treating the nation's 430,000 dialysis patients amounting to about $33 billion each year, or 6% of total Medicare expenditures. Third, Medicare transitioned to a bundled payment system for dialysis in 2011, providing us with a rich empirical setting to evaluate the effects of this reform and its implications for other parts of the U.S. health care system.

Before changing its payment model in 2011, Medicare reimbursed dialysis facilities with a hybrid composite and fee-for-service system, under which providers received a fixed payment for each dialysis session of approximately $128 and a fee-for-service reimbursement for any injectable drugs administered during treatment. Most of these injectable drugs were given to treat patients' anemia, as those suffering from ESRD often develop this condition because their kidneys fail to produce the hormone that stimulates red blood cell production. A variety of drugs have been approved by the FDA to treat anemia over the past few decades, with the most prominent being epoetin alfa marketed under the brand name EPOGEN (EPO) by the drug

company Amgen. Prior to the transition to bundled payments, EPO represented the single largest prescription drug expenditure for Medicare, totaling $2 billion in 2010 (U.S. Government Accountability Office, 2012). Administering EPO proved lucrative for providers, accounting for as much as 25% of revenue for the largest dialysis chain, DaVita, and up to 40% of its accounting profits (Healthcare, 2005). Many patient advocates questioned such extensive use of EPO, however, as several studies linked excessive EPO doses to an increased risk of mortality and cardiovascular events (Besarab et al., 1998; Singh et al., 2006; Brookhart et al., 2010).

Medicare's transition away from FFS affords us a unique opportunity to examine the ways in which providers change their behavior in response to bundling and quality-promoting incentives. In 2008, legislation set in motion an eventual payment reform of Medicare's ESRD program. This reform had two parts: first, in 2011 payments for anemia drugs were bundled together with payments for dialysis treatments under the new ESRD Prospective Payment System (herein referred to as the "bundle" or "PPS"). Additionally, to assuage concerns that these prospective payments may harm patients by incentivizing excessive cost cutting by providers CMS implemented the Quality Incentive Program (QIP) in 2012. This directly links payments to patient outcomes, by enabling Medicare to reduce payments to dialysis facilities which exhibit poor patient outcomes.

Although all providers faced the same policy changes, certain institutional details meant that not all providers faced the same change in incentives following the payment reform. First, patients residing at higher elevations require less EPO because their bodies naturally produce more red blood cells in response to lower oxygen levels. When injectable drugs received fee-for-service reimbursements, this physiological distinction made patients at higher elevations less profitable for dialysis facilities be-

cause clinical guidelines recommend that they receive smaller doses of EPO, and hence facilities received correspondingly lower fee-for-service reimbursements. Second, facilities pay different wholesale prices for EPO based on their negotiated contracts with Amgen and other drug manufacturers. Due to its larger scale, for instance, chains paid approximately 11% less for EPO than the typical independent facility before the introduction of PPS (Eliason et al., 2019b).

We use differences in elevation and wholesale prices for EPO to isolate the causal effect of bundled payments on providers' behavior and patients' outcomes. Our empirical strategy offers several advantages over previous studies of this topic that have mostly used observational data to analyze the effect of bundled payments on a small number of hospitals that voluntarily participated in the program. Although these studies typically find large savings associated with the payment reform, they cannot determine causality because the hospitals that selectively opt into bundled payments may have been particularly well suited to achieve savings, biasing their estimates. Our identification strategy allows us to overcome such confounds because (i) facilities faced different incentives to change their EPO doses following the reform due to differences in costs and (ii) patients faced different relative changes in EPO doses due to their elevations and underlying health. As both of these sources of variation are independent of the policy reform, we can use them to cleanly identify the causal impact of bundled payments on behavior and outcomes.

Using an interrupted time series approach we find evidence that this payment reform resulting in a 48 percent drop in the mean monthly Epo dose patients receive. This represents a decrease in resource utilization but has ambiguous implications for patient welfare. On the one hand it may improve patient welfare as patients may have been over treated prior to the reform. On the other is may be harmful for patients

if their anemia is under treated as a result of the reform. Our results suggest that patient outcomes improve because of the new policy. Two of the major risk factors associated with overuse of Epo are mortality and increased risk of cardiac events such as stroke and heart attack. We find that the reform results in a 15 percent decrease in hospitalizations for cardiac events and a 25 percent decrease in the likelihood of death in any given month. Additionally, overall hospitalizations drop by 12.3 percent. Blood transfusions, which can be required by patients who's anemia is not properly treated, initially rise as patient Epo doses plummet. However, over a two year horizon following the intervention transfusions gradually decrease and were 21 percent lower than they would have been absent the policy change in December of 2012.

That transfusions improve even while Epo use decreases, suggests that providers may be becoming more judicious in their use of EPO. We explore whether provider are becoming better at differentiating between patients who will benefit from Epo and those that will be harmed by it and are improving their allocative efficiency accordingly. We consider the possibility of a learning process that providers undertake as an explanation of the gradual transitions we see in our results.

Our paper contributes to previous work on the allocation of health care resources. A number of papers study misallocation of health care resources using cross-sectional data. Abaluck et al. (2016), for instance, study the use of CT scans to test for pulmonary embolisms. They document significant variation in the propensity of providers to order scans and find that providers ordering lots of scans have significantly lower pulmonary embolism yield rates, suggesting that physician practice styles drive the variation in the use of scans rather than patient selection. Chandra and Staiger (2017) note that differences in utilization rates across hospitals may also emerge from variation in comparative advantage. They develop a model to sepa-

rate the effects of misallocation and comparative advantage and estimate it using data from heart attack patients. They find that both channels contribute to the observed variation in treatment rates across hospitals. Other related papers include Currie and MacLeod (2013), Finkelstein et al. (2016), and Molitor (2018), although they do not look specifically at how that misallocation changes with a bundling payment reform.

Another large literature studies the effects of alternative payment systems, including bundled payment models. Many of these papers focus on Medicare's move in 1983 from cost-based reimbursements to the diagnoses related group (DRG) system for hospitals. Under this new system, hospitals are paid for treating patients based on their diagnoses rather than the costs incurred. Cutler (1995) shows that this change affected hospitals differently based on their exposure to Medicare patients. Exploiting this cross-sectional variation, he finds little evidence of sustained changes in patient outcomes resulting from the payment reform. In related work, Acemoglu and Finkelstein (2008) study whether the same reform affected the input mix and technology choices of hospitals. They find significant increases in the capital-labor ratio as well as increased adoption of new medical technologies.[1] In a non-Medicare setting, Ho and Pakes (2014) study capitation contracts by private insurers in California, finding that such contracts lead physicians to steer patients to lower cost hospitals, without worsening outcomes.

In dialysis, the focus of our study, the switch to a prospective payment system (PPS) has also been studied extensively in the medical and health services literatures. Much of this work has focused on anemia treatment and outcomes. For example, Chertow et al. (2016) document an abrupt decline in EPO doses beginning in late 2010 and look at related patient outcomes, finding that all-cause mortality, cardiovascular

[1]For more examples of papers examining the adoption of the DRG system, see Sloan et al. (1988a), Sloan et al. (1988b), Dafny (2005), and others.

mortality, and myocardial infarction did not change significantly after 2012. During 2012, however, the rate of stroke, venous thromboembolic disease, and heart failure were all lower than expected. Additionally, Hirth et al. (2014) find an uptick in blood transfusions following PPS. Another sizable literature focuses on changes in dialysis modality following PPS. Sloan et al. (2019) find an uptick in the number of patients using peritoneal dialysis, a form of dialysis associated with a similar mortality as hemodialysis but with a higher quality of life and lower costs, after adopting PPS in 2011. Similarly, Wang et al. (2018) find that following the 2011 reform more facilities offered peritoneal dialysis, while Lin et al. (2017) find an increase in home dialysis.[2]

A more recent literature has examined the effects of Medicare's Bundled Payments for Care Improvement Initiative. Starting in 2011, this initiative sought to control provider health care costs by paying providers a bundled rate for treatment of a patient, rather than a traditional fee-for-service payment methodology. From observational data, Maughan et al. (2019), for instance, find that hospitals participating in particular payment models had worse outcomes for average patients than similar non-participating hospitals, but no worse outcomes for the most vulnerable patients. Martin et al. (2018) document similar findings for lumbar fusion patients, where patients treated at participating hospitals had higher readmission rates and higher repeat surgery rates than patients at similar hospitals. In contrast, both Dummit et al. (2016) and Navathe et al. (2017) document lower costs for lower extremity joint replacement patients with no meaningful difference in quality at participating hospitals. This small literature suggests that bundled payment reforms could have significant consequences for patient oucomes. While other papers mentioned here have studied the relationship between payment reforms and patient out comes, this paper is, to our knowledge, the first to directly tie changes in patient outcomes to

[2]See also Chambers et al. (2013).

provider responses to payment reforms.

One important exception to the observational studies of bundled payments is finkelstein et al. (2018), who study a randomized trial of a bundled payment model for lower extremity joint replacements. They find that patients treated at participating hospitals were less likely to be discharged to post-acute care, yielding lower total cost of care, with no differences in readmission or ER visit outcomes. We build on the findings from that RCT by evaluating outcomes for longer than the first year following implementation of the bundled payment program, considering the effects on total Medicare spending for those affected, exploring heterogeneity across types of patients and providers (e.g., for-profit vs. not-for-profit), and assessing a number of relevant clinical measures (e.g., hematocrit levels and infection rates).

In addition, our paper is among the first to examine how regulations that restructure Medicare's reimbursement for drugs can affect allocative efficiency. In 2017, total drug expenditures in the U.S. were $324.4 billion (IQVIA 2018), and a large portion of this spending may be wasteful (Garber and Skinner, 2008; Kyle and Williams, 2017). We contribute to the understanding of how regulations can limit the unnecessary use of prescription drugs, particularly for Medicare Part B that paid $26 billion for drugs on a fee-for-service basis in 2015 (MEDPAC, 2017). A MEDPAC analysis found that over 75% of the volume for two-thirds of these drugs was sold below the Medicare reimbursement rate, suggesting the potential for misuse and over-treatment. Our paper informs the policy discussion related to these concerns.

Lastly, this paper also contributes to the broad literature on health care provider responses to financial incentives. Of particular relevance, Gaynor et al. (2018) study how dialysis providers balance patient health with financial incentives in EPO dosing using a structural model of dosing decisions. Their findings suggest that, as expected,

the traditional fee-for-service payment structure Medicare utilized until 2011 yielded overuse of EPO. In their counterfactual simulations, doses would be 30-40% lower under the optimal linear contract for EPO payment than the existing payment structure. The most relevant subsection of this literature studies the effects of Medicare reimbursement design, such as Capps et al. (2017a), who find that physicians who acquired by hospitals shift their billing to hospital based settings in order to take advantage of higher facility-based payment rates. In a similar vein, Eliason et al. (2018) and Einav et al. (2017) both study the impact of a discontinuity in reimbursements for long-term-care hospitals, finding that hospitals disproportionately discharge patients immediately after receiving a lump sum payment for care.[3]

Our paper proceeds as follows: Section 4.2 discusses the institutional details of the dialysis industry in the United States, Section 4.3 describes the data used in our study, Section 4.3 describes the data used in this study, Section 4.4 presents findings from a preliminary time-series analysis of the effects of the policy reforms, Section 4.5 decomposes the sources of these effects, and Section 4.6 presents instrumental variables estimates of the effects of the bundled payment reform.

4.2 Background

4.2.1 Medical Background on Anemia

Anemia is a medical condition in which a low red blood cell count prevents oxygen from being adequately delivered throughout the body. Two blood chemical tests

[3]The size of this literature precludes an exhaustive review here. For a more thorough discussion, see Gaynor et al. (2015b).

can be used to diagnose anemia and assess its severity: hematocrit and hemoglobin concentration. Hematocrit measures the volume of red blood cells as a percent of total blood volume, whereas hemoglobin concentration measures the amount of hemoglobin, a protein contained in red blood cells, in terms of grams per deciliter of blood (g/dL). The two measures are nearly isomorphic, with hematocrit being approximately equal to three times the measured hemoglobin levels (Bain et al., 2017). In this paper, we focus on hemoglobin levels.

According to the one common medical reference, anemia is defined as hemoglobin below 14 g/dL for men and 12 g/dL for women. Common symptoms relate to a patient's quality of life, including fatigue, weakness, headaches, difficulty concentrating, rapid heart beat, and insomnia. Anemia can also contribute to an increased risk of serious heart conditions, hospitalization, and mortality (Kliger et al., 2013).

Anemia is common among patients with kidney failure. The primary functions of kidneys are to filter waste water and toxins from the blood and to naturally produce erythropoietin, a factor that stimulates the production of red blood cells in the bone marrow. Naturally occurring erythorpoietin is much lower in patients with kidney failure, which is thought to be the main reason anemia is common among dialysis patients (Babitt and Lin, 2006). Among these patients, anemia is typically managed using a cocktail of drugs, with acute instances calling for blood transfusions.

4.2.2 Treatment of Anemia

Chief among the drugs used to treat anemia in dialysis patients is recombinant human erythropoeitin or epoetin alfa, commonly known as EPO. This biologic was approved by the Food and Drug Administration for the treatment of anemia in dialysis patients

in 1989 (Kalantar-Zadeh, 2017). Since then, EPO has been a standard of care for this condition. It is manufactured by Amgen under the brand name EPOGEN®.[4] Initial outcomes from using EPO were encouraging, as anemic patients treated with EPO required fewer blood transfusions and reported improved appetite, activity level, and sense of well-being (Eschbach et al., 1987; Valderrabano, 2000). By 2005, 99% of in-center hemodialysis patients regularly received EPO. It became so popular, in fact, that in some years it represented the largest share of drug spending in Medicare's budget (U.S. Government Accountability Office, 2012).

By the mid-2000s, randomized controlled trials found evidence that the use of EPO among certain populations may be harmful. In one study, Besarab et al. (1998) found that ESRD patients with congestive heart failure who were treated with EPO to achieve normal or high hematocrit levels had a higher probability of death and myocardial infarction. Similarly, Singh et al. (2006) found an increased risk of death and cardiovascular events among ESRD patients treated with EPO to normal or high hematocrit levels. Although these RCTs focused on specific patient populations, they raised concerns about the use of EPO more generally, and in March 2007 the FDA issued a public health advisory for EPO, mandating a black box warning and advising physicians to adjust doses to target hemoglobin levels between 10 to 12 g/dL Thamer et al. (2013). Over this time period, observational studies suggested similar adverse effects[5], although providers did not alter their doses much in response (Thamer et al., 2013). At the end of June 2011, the FDA amended the original black box warning, instructing providers to use the lowest dose required to avoid blood transfusions.

In addition to EPO, a broader class of drugs called erythropoeises stimulating

[4]Amgen also licenses epoetin alpha to Johnson & Johnson who market it for non-ESRD uses under the brand name, Procrit®.

[5]See Zhang et al. (2004), Bradbury et al. (2009), and Brookhart et al. (2010), among others.

agents (ESAs) help promote the production of red blood cells. These include intravaneous iron drugs Venofer®and Ferrlecit®, which enhance red blood cell production both naturally and when induced by recombinant EPO. There are short-acting ESAs such as Epogen (epoetin alfa) produced by Amgen that require multiple injections per week. On the other hand, long-acting ESAs such as Aaranesp (darbepoetin alfa) and Mircera (methoxy polyethylene glycol-epoetin beta, i.e PEG-EPO) are administered at one to four-week intervals. Many physicians report that apart from convenience, there are not distinctions between these ESAs in terms of efficacy or safety (Bernieh et al. 2014). Short-acting ESAs are more common in patients undergoing dialysis at facilities due to their frequent visits, while long-acting ESAs are more common in-home dialysis or non-dialysis environments. Despite physicians reporting that there are not distictions between these ESAs in term of efficacy or safety, there is a recent shift away from Epogen towards Aranesp and Mircera (DOPPS 2016). Convenience seems to drive this, as half-lives of Epogen, Aranesp, and Mircera are 4-13 hours, 21, and 134 hours respectively. Thus where Epogen is often administered with each dialysis treatment, Mircera can be administered 1-2 times a month. Mircera produced by Roche received US FDA approval in November 2007 but did not launch until mid-2014 due to a settlement with Amgen to delay release in the United States.

4.2.3 The Role of Elevation

ESRD patients do not respond uniformly to EPO. One documented source of this heterogeneity is the elevation at which a patient resides. Brookhart et al. (2008) show that patients living above 6000 ft receive 19% less EPO compared to patients at sea level, Brookhart et al. (2011) find that patients moving from low to high elevations exhibit large and persistent increases in observed hematocrits and decreases in EPO

doses relative to a control group. Sibbel et al. (2017) find that even in 2012, after the 2011 reform, patients at higher elevations were less likely to receive EPO or IV iron, had higher mean hemoglobin levels, and had lower mortality rates compared to patients at lower elevations.

At higher elevations, the richness of oxygen in the blood decreases, which activates hypoxia-inducible transcription factors (HIFs). For patients with healthy kidneys, HIFs trigger an increase in natural erythropoietin and increased availability of iron in the blood stream, with bone marrow stimulated by the erythropoietin to use available iron to produce red blood cells. In ESRD patients, higher elevation is associated with increased iron availability but little increase in erythropoietin (as the kidneys are not functioning properly). The increased availability of iron makes the available erythropoietin, whether naturally or artificially occurring, more productive. Consequently, patients at higher elevation tend to have higher baseline HGB levels and to be more responsive to EPO doses.[6]

4.2.4 Medicare Payment Reform

Since 1972, Medicare has extended full benefits to all patients suffering from ESRD, regardless of age. Individuals enrolled in an employer group health plan when they were diagnosed with ESRD retain their commercial insurance as a primary payer for 33 months, during which time Medicare acts as a secondary payor, after which time Medicare becomes the primary payor. Medicare pays for the dialysis and anemia treatment of ESRD patients jointly under Part B. Prior to 2011, Medicare paid for dialysis and anemia under different policies. Since the early 1980s, Medicare paid

[6]See Winkelmayer et al. (2009) and Brookhart et al. (2011) for a more complete discussion of these physiological relationships.

a composite rate of approximately $ 135, with very little variation over time. This single payment was intended to cover the labor, capital, supplies and routine lab tests associated with each dialysis treatment. In addition, injectable drugs covered by Medicare Part B, such as those used to treat anemia, were separately billable. This fact enabled facilities to increase their reimbursements by increasing drug dosages, potentially yielding higher than optimal doses for patients.

Fee-for-Service Injectable Drugs for Anemia

Beginning in 1991, Medicare reimbursed for EPO on a fee-for-service basis. In 2005, the reimbursement rate changed from being based on the Average Wholesale Price to the Average Sales Price plus a six percent markup. This resulted in a reimbursement rate of about $10 per 1000IUs. The fee-for-service era saw consistent increases in EPO doses and expenditures. In 2007, spending on ESAs was about $2.7 billion dollars (Whoriskey, 2012). Concerns that the distortionary incentives from fee-for-service were leading to excessive costs and harm to patients motivated ESRD payment reform to be included in the Medicare Improvements for Patients and Providers Act (MIPPA) of 2008.

Bundled Payments

MIPPA mandated the bundling of dialysis and anemia treatments into a single prospective payment. Under the new prospective payment system (PPS), which started in 2011, providers are paid a single payment (initially about $230) for each dialysis treatment. This single payment is supposed to cover the costs of both dialysis and previously separately billable injectable drugs, including EPO. The payment

level was picked to reduce total federal payments to dialysis providers by 2%.

Amgen Sourcing and Supply Agreements

The large kidney dialysis chains DaVita and Fresenius have at times partnered with biopharmaceutical industry leader and erythropoietin stimulating agent (ESA) producer Amgen to improve the profitability of administering ESAs. In 2011 DaVita entered into a sourcing and supply agreement with Amgen, providing DaVita with discounts and rebates for Amgens two ESAs, Epogen and Aranesp (DaVita Amgen Agreement 2011). In return, DaVita committed to purchasing Epogen and Aaranesp in sufficient amounts necessary to ensure at least 90% of DaVitas ESA use is supplied through Amgen. This 2011 contract ran through 2018, and was renewed in 2017 to extend through 2022 (DaVita Amgen Agreement 2017). Fresenius entered into a similar sourcing and supply agreement with Amgen in 2006 extending to 2011. (Fresenius Amgen Agreement 2006). Fresenius contract lacked minimum purchase commitments, but did secure discounts for Epogen and Aranesp. Following this Fresenius has entered into year-to-year contracts with Amgen.

Quality Incentive Program

To give providers further incentives to improve quality and to guard against the possibility that providers didn't respond to the bundle by cutting costs at the expense of clinical quality, MIPPA also mandated the development of a Quality Incentive Program (QIP). QIP potentially penalizes payments to providers that do not meet certain clinical standards, such as excessively high hemoglobin or hospitalization rates. In its inaugural year, 2012, QIP standards focused on patient's urea reduction ratio

(URR), a measure of the adequacy of dialysis filtration, and hemoglobin levels (HGB). If the HBG of too many patients at a given facility fell outside the regulated standards, Medicare could penalize the annual payments to that facility by up to 2%.

4.3 Data

The first data source used in this analysis is the U.S. Renal Data System (USRDS). USRDS is a clearing house that collects and manages data from a variety of sources relevant to ESRD and CKD patients, health care providers, and utilization. Included in these data are Medicare claims, treatment histories, patient attributes and residential data, and an annual facility survey. Additionally CMS Form 2728, known as the Medical Evidence Form, provides rich data on the health and clinical attributes of patients when initiating dialysis.

After geocoding facility addresses we extract the elevation of their locations using data from the U.S. Geological Survey (U.S. Geological Survey Center for Earth Resources Observation and Science, 2014).

We supplement this data using financial statements submitted by individual dialysis facilities to CMS each year, detailing their costs, as a part of the Healthcare Cost Reporting Information System (HCRIS). In addition to operating costs these data include acquisition costs for drugs such as EPO. While CMS reserves the right to audit these reports, there is some concern about the fidelity of these data, in particular drug acquisition costs. To address these concerns we compared them against an independent audit of dialysis facilities conducted by the Office of the Inspector General and determined that, at least in the aggregate, the costs reported in both

Table 4.1: Patient Descriptive Statistics

	(1)	
	Mean	Sd.
Age (Years)	63.35	14.74
Months With ESRD	47.34	38.97
Black	0.395	0.489
Male	0.531	0.499
Diabetic	0.549	0.498
Hypertensive	0.874	0.332
Facility Elevation (ft)	615.0	891.0
EPO Total (1000 IUs)	61.75	69.70
EPO Per Week (1000 IUs)	15.63	17.41
Hospitalized	0.131	0.337
Hosp. - Cardiac Event	0.0258	0.159
Hemoglobin (g/dL)	11.20	1.245
Hematocrit (%)	33.61	3.770
Log(Hb)	2.496	0.106
Log(HCRIT)	3.538	0.117
Dies	0.00820	0.0902
Transfusion	0.0343	0.264
Transfusion - ER	0.000163	0.0176
Transfusion - Outpatient	0.0135	0.204
Transfusion - Inpatient	0.0206	0.156
Observations	13,812,607	

sources were very similar.

4.3.1 Descriptive Statistics

Table 4.1 presents preliminary summary statistics for various variables of interest. After imposing sample restrictions, we are left with a sample of approximately 14 million patient-month observations. Importantly for our instrumental variables analysis later, there is a substantial amount of variation in the elevation of facilities, with a standard deviation of 819 feet.

4.4 Time Series Analysis

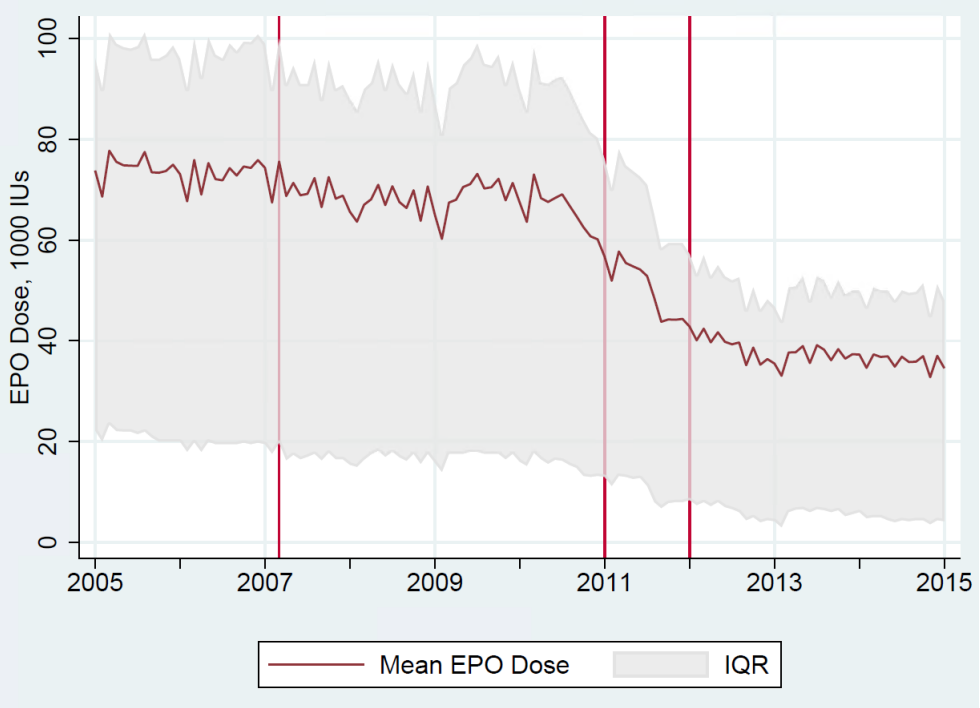
We begin by conducting a time-series analysis of the effect of the bundle payment reform on EPO doses. We begin with this outcome because it was the intended target of the policy and because it is likely a major mechanism through which the policy affects both resource utilization and patient outcomes. Figure 4.1 shows the evolution of average EPO doses over time, along with the interquartile range. The figure depicts a slow downward trend from 2005 to 2010 in mean EPO dose. Then, starting halfway through 2010 there is an abrupt acceleration in this downward trend as doses decline dramatically (albeit continuously) until leveling off around 2013. The observation that the decline in EPO pre-dates 2011 is suggestive of an anticipatory response to the payment reform.

Table 4.2 shows yearly averages of EPO dose, percent of patients receiving any dose, and the average EPO dose after conditioning on receiving any dose. This table suggests that the decline in doses comes from patients moving off of EPO altogether, as well as reductions in doses for those that continue to receive EPO.

Figure 4.2 shows the trends in HGB over time. EPO should directly increase HGB for patients. The figure shows similar trends to those of EPO. Leading up to 2011 there is a gradual lowering of HGB levels. In the month leading up to and through 2011 there is a more pronounced drop in HGB level, as we would expect if patients receive less of this drug.

Unpacking the effects of this decrease in EPO dose requires an examination of the heterogeneity of this response. Figure 4.3 shows total EPO utilization by patient HGB levels. As discussed above, the lower the hemoglobin the more beneficial EPO doses are and for patients with HGB above 10 or 12 g/dL the net effect of EPO on health

Figure 4.1: Monthly EPO Doses Over Time



is likely negative. This figure shows that between 2007 and 2010 EPO utilization was on the rise. However, in mid-2010 doses began to fall, reflecting Figure 4.1. Figure 4.3 shows that the decrease was concentrated among patients with HGB levels above 11g/dL. While this is suggestive of an improvement in allocative efficiency, this figure masks many things that may be going one (including the fact that patients may be changing categories as their dose and HGB levels decrease together).

Table 4.2: EPO Dosing Trends

	Average EPO Dose	Percent of Patients Receiving Any EPO	Average EPO Dose, Conditional on Any EPO
2005	74.80	92.02	81.28
2006	73.47	87.73	81.63
2007	70.40	86.75	79.17
2008	67.51	86.81	76.23
2009	69.08	86.91	77.51
2010	66.04	86.24	74.77
2011	50.54	83.20	59.52
2012	39.18	80.01	47.81
2013	36.99	79.77	45.60
2014	36.12	78.83	45.31

Figure 4.2: Hemoglobin Levels Over Time

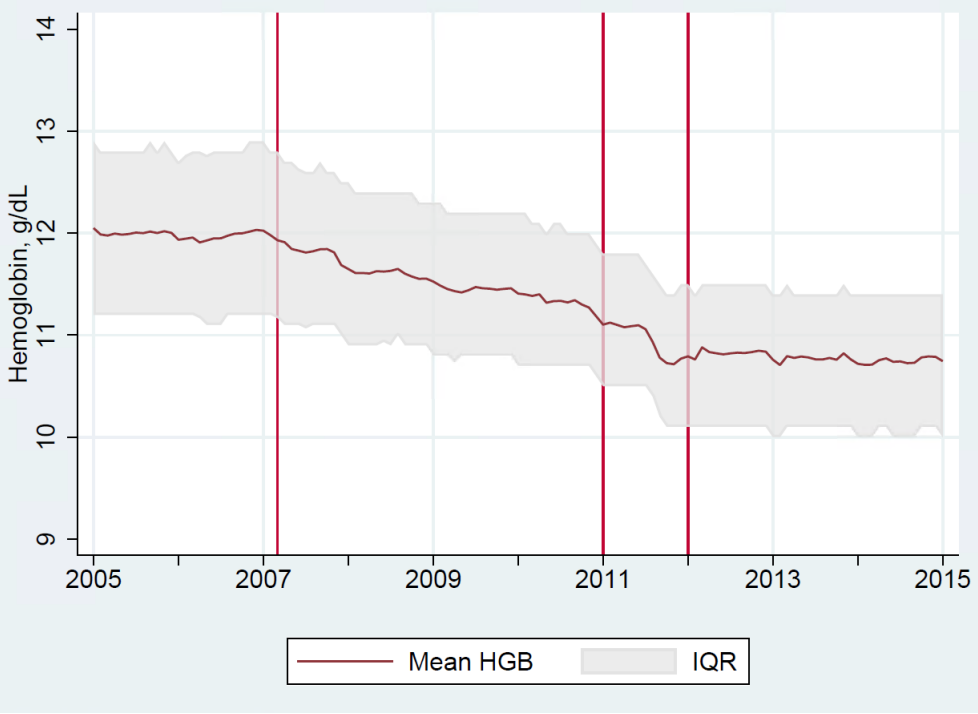
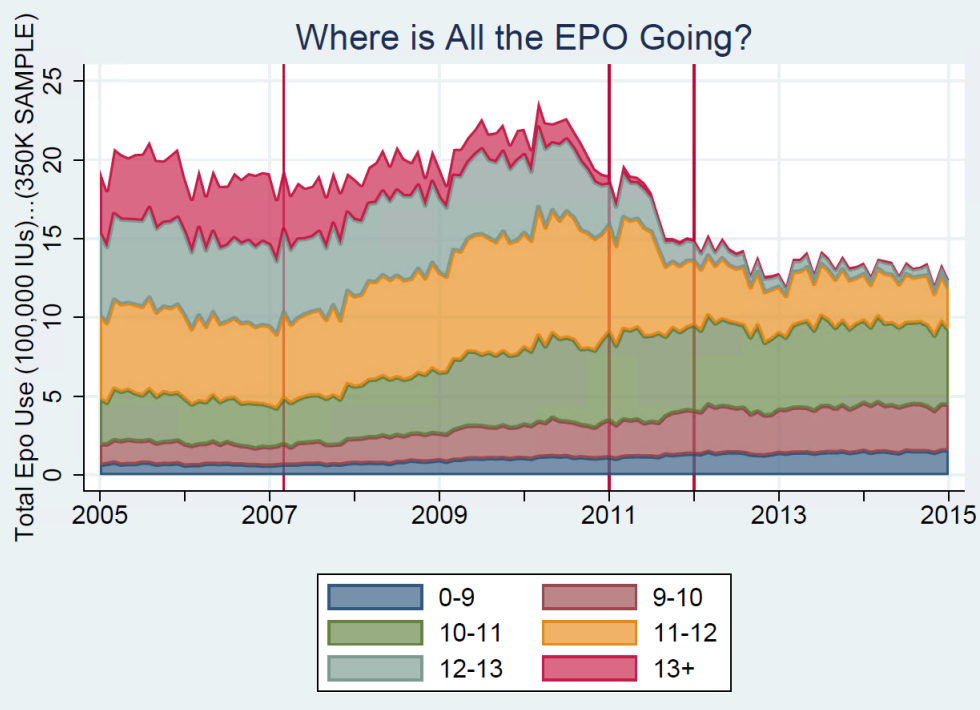


Figure 4.3: EPO Use by HGB Level



4.4.1 Interrupted Time Series

We perform an interrupted time series analysis to quantify the effect of the payment reform on provider practices and patient outcomes. We begin with a somewhat naive specification where we regress the dependent variable on a dummy variable for when PPS was implemented (January 2011) as well as a variety of controls:

$$y_{ijt} = \beta_0 + \beta_1 \mathbb{1}[PPS_t = 1] + X_{ijt}\Gamma + \varepsilon_{ijt}. \tag{4.1}$$

We show the results from estimating Equation 4.1 below in Table 4.3. The results are robust to controlling for patient and facility controls, as well as facility fixed effects, and suggest a decrease in EPO doses of close to 50%. Focusing on the specification in Column 3 (Month FEs, patient and facility controls, and facility fixed effects), we estimate this equation for a variety of additional outcomes and show the results

Table 4.3: Effect of Bundle on EPO Dose

	(1) EPO	(2) EPO	(3) EPO
PPS	-30.13***	-30.78***	-29.81***
	(0.280)	(0.262)	(0.258)
Month FE	1	1	1
Pat/Fac Controls	0	1	1
Facility FE	0	0	1
Dep. Var. Mean	61.75	61.75	61.75
R-squared	0.0474	0.0724	0.108
Observations	13812607	13812607	13812602

Notes: OLS estimates from (4.1). Dependent variable is total monthly EPO dose. Epo doses are measured in the thousands of IU's per-week. Doses are winsorized at the 99th percentile. *PPS* is an indicator variable for post-2011. Facility-clustered standard errors are in parentheses. An observation is a patient-month. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Data from 2008 to 2013 included. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

in Table 4.4. These results show dramatic changes. HBG levels demonstrate a six percent decline at the mean following the bundle, transfusions increase by 24 percent, overall hospitalizations drop by two percent, and hospitalizations for cardiac events drop by 18 percent. Finally, the likelihood that a patient dies in any given month decreases by 32 percent.

The magnitude of these results are quite large and should be taken with caution. Chief among potential confounding factors are time trends. Inspection of Figure 4.1 suggests a pre-existing trend in Epo doses that this specification does not account for and may bias our estimates. Similar trends likely exist for each of the dependent variables in Table 4.4. Additionally, Figure 4.1 suggests that the policy intervention may have had both an effect on the level of Epo doses as well as the trend. This

Table 4.4: Effect of Bundle on Other Outcomes

	(1) HGB	(2) Transfusions	(3) Hosp., Any Cause	(4) Hosp., Cardiac	(5) Death
PPS	-0.684***	0.00835***	-0.0167***	-0.00467***	-0.00265***
	(0.00447)	(0.000355)	(0.000400)	(0.000135)	(0.0000696)
Month FE	1	1	1	1	1
Pat/Fac Controls	1	1	1	1	1
Facility FE	1	1	1	1	1
Dep. Var. Mean	11.20	0.0343	0.131	0.0258	0.00820
R-squared	0.103	0.00665	0.0143	0.00576	0.00493
Observations	13812602	13812602	13812602	13812602	13812602

Notes: OLS estimates from (4.1). Dependent variable in column 1 is hemoglobin, measured in grams-per-deciliter. Dependent variables in columns 2-5 are binary outcome variables. PPS is an indicator variable for post-2011. Facility-clustered standard errors are in parentheses. An observation is a patient-month. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Data from 2008 to 2013 included. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

gradual adjustment following the implementation of PPS merits additional studies and consideration of what type of behavior or model can rationalize this gradual adjustment.

We enrich the former specification to allow for pre-existing time trends, as well as both an intercept shift and change in time-trend to be associated with both components of the Medicare payment reform (PPS and QIP). This new specification is:

$$y_{ijt} = \beta_0 + \beta_1 t + \beta_2 \mathbb{1}[PPS_t = 1] + \beta_3 t_{Post-PPS} + \beta_4 \mathbb{1}[QIP_t = 1] + \beta_5 t_{Post-QIP} + X_{ijt}\Gamma + \varepsilon_{ijt}. \tag{4.2}$$

This allows for an intercept shift in the dependent variable when the bundle was implemented (β_2). β_1 is the slope of a linear time trend in the pre-period that continues into the post-preriod. β_3 allows for a change in the slope of this trend

beginning with the bundle reform. $t_{post-PPS}$ is set equal to zero in every period through January 2011 after which it increases by one each month. We also control for patient characteristics to mitigate identification threats due to selection and a series of fixed effects (facility fixed effects and month fixed effects to address seasonality).

We first estimate Equation 4.2 using Epo dose as the dependent variable and show the results in Table 4.5. In the baseline specification we use data from 2007 to 2013. The results show that there was a pre-existing time trend as providers were decreasing average Epo doses by about 0.16 percent each month in the years preceding the bundle. When the bundle was implemented there was an immediate drop of close to $8,000$ units per month, a 13 percent drop. Additionally, the slope of the time trend increased by a factor of 13. During 2011 Epo doses dropped by around two percent each month. There is an additional drop in Epo dose following the implementation of QIP in 2012. At this point the slope of the time trend becomes less shallow and returns to just below the time trend in the per-period. Given these results, the total decrease in EPO doses attributable to PPS and QIP between January 2011 and January 2012 is 29.5 (or 48 percent of the mean Epo dose). This is quite similar to the estimates from the naive model.

In Table 4.6 we show the results of this specification for other outcomes. First consider blood transfusions. Starting with PPS there is a small increase in the trend of blood transfusions becoming more common over time. This is consistent with the reduction in Epo dosing that occurred together with this. However, following QIP there was an immediate and sizable drop in the frequency of transfusions, along with a reversal in the sign of the trend. Blood transfusions fell with QIP and continued to decline after that. This is consistent with the early performance criteria of QIP that penalized facilities for having patients with excessively low HGB because it put them

at risk and could require blood transfusions. It is also in line with QIP criteria in other years that specifically penalized facilities for having patients needing too many transfusions. Taken together these results describe a temporary increase in blood transfusions, followed by an overall decline. By the end of 2011 these reforms seem to have increased transfusions by 20 percent. However, by the end of 2012 blood transfusions were down by 21% and trending further down.

For hospitalizations for any cause there was a pre-existing downward trend. However, there is also a level adjustment in these hospitalizations following PPS. Additionally there was another level adjustment following the QIP and a post-QIP time trend that declines much more quickly than the pre-trend. Overall the payment reforms seem to have led to a 12.3 percent drop in overall hospitalizations. The frequency of cardiac hospitalizations declined quickly with PPS. This aligns with the concern that excessive Epo doses increased the risk of cardiac events. While there is evidence of a downward level-adjustment at QIP the coefficient is insignificant. However, after QIP there seems to be a sustained decline in these hospitalizations. By December of 2012 this works out to a 15 percent decrease in hospitalizations for cardiac events. Finally, the likelihood of mortality was decreasing in the pre-existing time trend and declined further with PPS. There was a small uptick in mortality with QIP but this does away within the year as the new post-QIP trend decreases mortality by more than the intercept shift. By the end of 2012 these policies appear to have decreased the likelihood of death by almost 25 percent.

Identification of these parameters relies on two key assumptions (see Baicker and Svoronos (2019) for further discussion). The first is that the pre-existing time trend would be the same whether or not the intervention occurred. This precludes the possibility of anticipatory effects.

The second key assumption is that in the absence of intervenions, the post-intervention trends would be equivalent to the extrapolated pre-trend (in expectation). In other words the pre-intervention time trend must provide a credible counterfactual for a world where the intervention never happened. This assumption is further complicated in this setting where we have two interventions (PPS and QIP). Here it must also be the case that the post-PPS trend would extend and be unchanged beyond 2012 if QIP had never happened. In some cases this may be problematic. For example consider the case that uses Epo as the dependent variable. The post-QIP trend returns the overall trend to close to the pre-PPS levels. To be able to interpret this is a causal effect of QIP this change must be attributable to QIP and not just the end of a transition to lower Epo doses that started with PPS.

Up to this point, our results have relied entirely on time-series variation in the regulatory environment for dialysis providers, which may not appropriately identify the effect of the reform. Providers may change their behavior in unobservable ways that could alter patient outcomes or attempt to alter their patient mix in order to attract the most profitable patients. To work around this, we exploit a novel biological aspect to anemia management in which patients living at different altitude will have different hemaglobin levels. At higher elevations, the human body more effectively produces red blood cells, which leads to lower EPO requirements for patients at higher altitudes.[7] Because elevation itself is potentially correlated with unobservables, we use the interaction between the payment reform and a facility's elevation as an instrument for a patient's EPO doses. This relies on the assumption that patients at different elevations experience different changes in their EPO doses after the payment reform and that this is the only mechanism which affects their subsequent outcomes.

[7]This has been studied in several academic papers, such as Brookhart et al. (2009).

Table 4.5: Effect of Bundle and QIP on epodose, Pre- and Post-Trends

	(1) Epo Dose	(2) Epo Dose	(3) Epo Dose
Time Trend	-0.103***	-0.0988***	-0.0884***
	(0.0112)	(0.0106)	(0.0100)
PPS	-7.379***	-7.917***	-7.972***
	(0.295)	(0.293)	(0.290)
Post-PPS Trend	-1.188***	-1.178***	-1.200***
	(0.0319)	(0.0315)	(0.0309)
QIP	-7.120***	-7.596***	-7.131***
	(0.227)	(0.229)	(0.226)
Post-QIP Trend	0.987***	0.961***	0.968***
	(0.0309)	(0.0308)	(0.0306)
Month FE	1	1	1
Pat/Fac Controls	0	1	1
Facility FE	0	0	1
Dep. Var. Mean	61.75	61.75	61.75
R-squared	0.0557	0.0811	0.116
Observations	13812607	13812607	13812602

Notes: OLS estimates from (4.2). Dependent variable is total monthly EPO dose. Epo doses are measured in the thousands of IU's per-week. Doses are winsorized at the 99th percentile. PPS is an indicator variable for post-2011. QIP is an indicator for post-2012. Post-PPS Trend and Post-QIP Trend are continous measures of time since the introduction of each policy. Facility-clustered standard errors are in parentheses. An observation is a patient-month. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Data from 2008 to 2013 are included. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

Table 4.6: Effect of PPS and QIP on Other Outcomes, Pre- and Post-Trends

	(1) HGB	(2) Transfusions	(3) Hosp., Any Cause	(4) Hosp., Cardiac	(5) Death
Time Trend	-0.0134***	0.000109***	-0.000142***	-0.0000494***	-0.0000265***
	(0.000168)	(0.0000124)	(0.0000161)	(0.00000615)	(0.00000311)
PPS	0.0174***	0.000168	-0.00278***	-0.000799**	-0.000587***
	(0.00509)	(0.000534)	(0.000671)	(0.000296)	(0.000157)
Post-PPS Trend	-0.0316***	0.000867***	0.000114	-0.00000254	-0.0000571**
	(0.000745)	(0.0000744)	(0.0000851)	(0.0000387)	(0.0000210)
QIP	0.101***	-0.00370***	-0.00269***	-0.000616	0.00114***
	(0.00566)	(0.000663)	(0.000718)	(0.000324)	(0.000176)
Post-QIP Trend	0.0423***	-0.00116***	-0.00111***	-0.000189***	-0.000101***
	(0.000943)	(0.0000820)	(0.0000890)	(0.0000393)	(0.0000210)
Month FE	1	1	1	1	1
Pat/Fac Controls	1	1	1	1	1
Facility FE	1	1	1	1	1
Dep. Var. Mean	11.20	0.0343	0.131	0.0258	0.00820
R-squared	0.118	0.00669	0.0147	0.00585	0.00502
Observations	13812602	13812602	13812602	13812602	13812602

Notes: OLS estimates from (4.2). Dependent variable in column 1 is hemoglobin, measured in grams-per-deciliter. Dependent variables in columns 2-5 are binary outcome variables. Epo doses are measured in the thousands of IU's per-week. Doses are winsorized at the 99th percentile. PPS is an indicator variable for post-2011. QIP is an indicator for post-2012. Post-PPS Trend and Post-QIP Trend are continous measures of time since the introduction of each policy. Facility-clustered standard errors are in parentheses. An observation is a patient-month. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Data from 2008 to 2013 are included. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

4.5 Decomposition

In this section we decompose the decrease in Epo doses to explore whether there is evidence of improved allocative efficiency. We explore potential heterogeneity in decreased Epo use and whether those who stood to gain the least or be harmed the most by Epo also experienced the largest drop in dosing.

To do this we decompose the change in Epo dosing from January 2010 to January 2012. Focusing on one year before and after PPS we avoid anticipatory and transition periods. The total change in EPO is:

$$\begin{aligned} TC &= EPO_{2012} - EPO_{2010} \\ &= \sum_i EPO_{i,2012} - \sum_i EPO_{i,2010} \end{aligned}$$

Over this period total Epo use dropped by 31.3 percent while the number of patients receiving dialysis increased by 8.2 percent.

Table 4.7: Total EPO Doses and Changes, 1000IUs

	January 2010	January 2012	Change	Pct Change
EPO Doses	11,675	8,026	-3,649	-0.313
Number of Patients	190879	176482	14397	0.082

We first decompose this change into patients were persist in both periods, new patients who were not treated in January 2010 but are treated in January 2012, and patients who were treated in January 2010 but exited the sample before January 2012 (most likely due to death or receiving a kidney transplant). The purpose of this exercise is to see if the decrease in Epo use is concentrated among patients from the extensive margin (old, high-Epo patients exiting and being replaced by new, low-Epo patients) or if it is coming from within patients—are persistent patients also

demonstrating declines in their doses. Table 4.8 shows that the per-patient decrease in Epo use was very similar for persisters and patients that entered or exited. In 2010 and 2012, persisters accounted for 54 percent of Epo use. They experienced a 31 percent drop in Epo over this period. The net affect of entry and exit was also a 31 percent drop in Epo dose. Of the 31 percent drop in total Epo use, 17 percent came from persisters and 14 percent came from new/old patients.

Table 4.8: EPO Decomposition: Persisters, Exiters and Entrants

	Total	Persisters	Exiters	Entrants	Entry/Exit
Percent of Total EPO 2010	1	0.540	0.460	0	0.460
Percent of Total EPO 2012	1	0.540	0	0.460	0.460
Change in EPO	-3649	-1965	-5376	3691	-1685
Percent Change in Own Group EPO	-0.313	-0.312	.	.	-0.313
Percent Change of Total EPO	-0.313	-0.168	-0.460	0.316	-0.144
Percent of Total Patients 2010	1	0.577	0.423	0	0.423
Percent of Total Patients 2012	1	0.533	0	0.467	0.467
Change in Total Patients	14397	0	-74681	89078	14397
Percent Change in Own Group Patients	0.082	0	.	.	0.193
Percent Change of Total Patients	0.082	0	-0.423	0.505	0.082

Persisters are patients who were treated in both January 2010 and January 2012. They account for 57.7% of patients in 2010 and 53.5% in 2012 and account for 53.8% of the decline in EPO. This means that much of the change is coming from within patients. Exiters are patients treated in 2010 but not 2012. Entrants are patients treated in 2012 but not 2010. Combined, these groups make up 42.3% of patients in 2010 and 46.7% of patients in 2012. They account for 46% of the decline in EPO. So it looks like the decline in EPO is spread pretty evenly across patients that persist and patients that enter/exit.

4.5.1 Decomposing by 2010 Hematocrit

Next, we decompose the change in Epo doses based on 2010 hematocrits. Patients with excessively high hematocrits likely benefit little from high Epo doses. Thus if we see that patients with high hematocrits in 2010 experience larger drops in Epo relative to patients with low hematocrits it is evidence of improving allocative efficiency.

Tables 4.9 and 4.10 below show the transition probabilities between HCRIT bins over two-year periods for patients that persisted over the entire period. Table 4.9 shows the transitions from January 2008 to January 2010. For all HCRIT bins in 2008 the modal destination bin was 33-36. Suggesting this was a common target. Looking at the transitions in January 2010 and 2012 the modal destination bin was 30-33, suggesting a downward shift in the target HCRIT. Note that the 2011 black box warning encourages keeping HCRIT even lower—below 30.

The other thing to notice is the general downward shift in destination HCRIT bins. Across all initial bins the likelihood of ending up with an HCRIT above 39 is 2.4 to 3.9 times greater in 2008-2010, relative to 2010-2012. The likelihood of ending

up with an HCRIT below 30 is 1.8-2.2 times greater in 2010-2012.

Table 4.9: Transition Matrix: HCRIT Bins in January 2008 and 2010

	HCRIT Bin 2010				
HCRIT Bin 2008	Below 30	$30-33$	$33-36$	$36-39$	39 or Over
$HCRIT < 30$	14.8	22.0	34.6	22.0	6.6
$30 < HCRIT < 33$	10.9	21.7	38.5	22.7	6.1
$33 < HCRIT < 36$	8.8	21.1	39.7	24.4	6.0
$36 < HCRIT < 39$	8.1	19.6	39.6	26.0	6.7
$39 < HCRIT$	7.9	19.5	37.5	26.6	8.5

Table 4.10: Transition Matrix: HCRIT Bins in January 2010 and 2012

	HCRIT Bin 2012				
HCRIT Bin 2010	Below 30	$30-33$	$33-36$	$36-39$	39 or Over
$HCRIT < 30$	25.9	35.5	28.5	8.4	1.7
$30 < HCRIT < 33$	20.8	38.2	30.6	8.7	1.7
$33 < HCRIT < 36$	17.7	38.2	33.1	8.8	2.1
$36 < HCRIT < 39$	16.9	37.0	33.3	10.0	2.9
$39 < HCRIT$	17.0	36.9	32.7	9.9	3.6

Table 4.11 shows the decomposition of Epo doses among persisting patients broken out into bins based on 2010 HCRIT levels. Except for the lowest bin (HCRIT<30), the higher the 2010 HCRIT the larger the reduction (in percentages and levels). Patients with HCRITs between 30 and 33 experienced a decline of 27 percent while patients with HCRITs over 39 saw a decrease in Epo of 42 percent.

Table 4.11: EPO Decomposition: Persisters by 2010 Hematocrits

		HCRIT Bin				
	All Persisters	Below 30	$30-33$	$33-36$	$36-39$	39 or Over
Percent of Total EPO 2010	1	0.125	0.206	0.341	0.230	0.078
Percent of Total EPO 2012	1	0.121	0.219	0.351	0.220	0.065
Change in EPO	-1965	-263	-353	-625	-496	-205
Percent Change in Own Group EPO	-0.312	-0.33	-0.27	-0.29	-0.34	-0.42
Percent Change of Total EPO	-0.312	-0.04	-0.06	-0.10	-0.08	-0.03
Percent of Total Patients 2010	1	0.07	0.17	0.33	0.22	0.06
Percent of Total Patients 2012	1	0.07	0.16	0.31	0.20	0.06
Change in Total Patients	0	-50	-585	-2157	-1759	-319
Percent Change in Own Group Patients	-0.01	-0.03	-0.06	-0.08	-0.05	
Percent Change of Total Patients	0	-0.0005	-0.06	-0.021	-0.017	-0.003

4.6 Instrumental Variables Approach

While the USRDS has nearly comprehensive data on dialysis patients due to the fact that most patients are on Medicare, there are challenges to identifying causal effects on EPO doses on dialysis patients. The endogeneity of EPO comes from two sources. First, EPO doses are nonrandomly assigned and are likely related to unobservable health factors. Any unobserverd factors associated with both EPO doses and patient outcomes will result in EPO doses being correlated with the error term. Second, measurement error due to incorrect EPO billing and reporting may bias naive estimates. Both of these factors may result in OLS being biased and inconsistent.

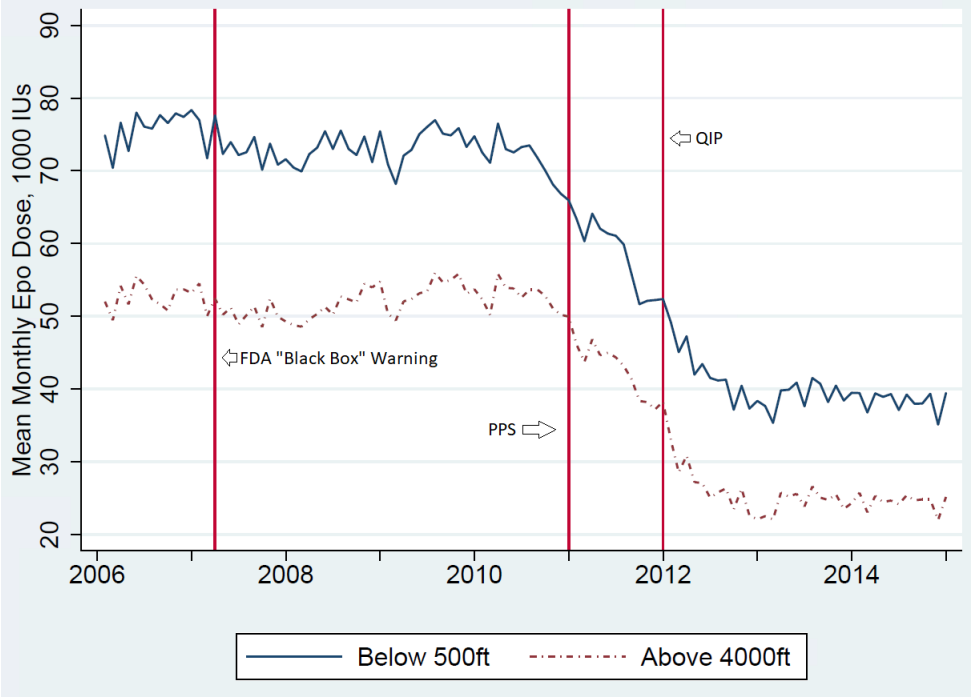
In order to identify the effect of EPO on patient outcomes we purpose an instrument. The instrument we put forth is the interaction between the elevation of the dialysis facility where patients are treated and an indicator for whether treatment occurs after the 2011 payment reform. This instrument exploits both cross-sectional variation (elevation) and intertemporal variation.

The estimating equation for the first stage of this approach has the following form:

$$EPO_{ijt} = \alpha_1 Elevation_j + \alpha_2 PPS_t + \alpha_3 Elevation_j * PPS_t + X_{ijt}\Gamma + u_{ijt}. \quad (4.3)$$

The instrument is the interaction between elevation and PPS_t, an indicator for whether t occurred under the Prospective Payment System. While additional controls including $Elevation_j$ and PPS_t must be included, they are likely inappropriate instruments. Elevation may be associated with health outcomes and other patient attributes. Additionally, PPS_t only demonstrates variation across time and hence

Figure 4.4: Mean EPO Dosage Per Month Over Time, by Elevation



will likely be correlated with other time trends. Inclusion of the interaction of these two terms is reminiscent of a difference-in-differences specification, except here the treatment (PPS) does not have an observable control group but demonstrates heterogeneity with some observable dimension—elevation. In the same spirit of the difference-in-differences, exclusion of this interaction from the second stage relies on the assumption trends pre-dating PPS did not vary across elevation. Additionally, it relies on the assumption that the only way the effect of PPS systematically varied across elevation was through the response of EPO doses.

Estimates from the first stage are found in Table 4.12. As expected EPO doses decrease with elevation but the rate of decrease is halved after PPS is introduced. Inference suggests that the instrument is relevant.

With the first stage in hand we turn toward estimating the desired effect: The

Table 4.12: First Stage Regression

Dependent Variable: Epo Per Week (1000 IUs)	OLS
Elevation (1000 ft)	-1.199***
	(0.0719)
Elevation*PPS_t	0.603***
	(0.0639)
N	2,070,937
F-Stat.	231.71
Dep. Var. Mean	15.78

Notes: Standard errors clustered at the facility level. An observation is a patient-year. Epo doses are measured in the thousands of IU's per-week. PPS_t is an indicator for year 2011 and later. Patient controls include dummies for comorbidities from Medical Evidence forms, facility elevation, patient demographics, and fixed effects for patient age, dialysis tenure, and year. Sample consists of hemodialysis patients at freestanding dialysis centers, aged 18 to 100 with Medicare as their primary payer. Data from 2008 to 2013 are included. Epo doses are trimmed at the 99th percentile. *, **, and *** indicate significance at the 5%, 1%, and 0.1% level, respectively.

effect on EPO use on patient outcomes. This second stage equation has the following form:

$$y_{ijt} = \beta_0 + \beta_1 EPO_{ijt} + X_{ijt}\Gamma + \varepsilon_{ijt}. \tag{4.4}$$

We estimate this equation using two stage least squares. In addition to instrumenting for EPO_{ijt} we control for a variety of observed patient covariates, year fixed effects, and potentially facility and patient fixed effects. We estimate this equation for a number of dependent variables including HGB, hemoglobin levels, blood transfusions, hospitalizations and mortality.

The first results considered are for HGB levels. This may be thought of as a sanity test. The FDA approved indication for EPO is to increase HGB levels, so we should expect the coefficient on EPO_{ijt} to be positive here. In Table 4.13 this is confirmed. Not that the OLS specification suggests that EPO decreases HGB levels.

This is consistent with endogeneity stemming from nonrandom assignment of EPO to patients—patients expected to have lower HGB levels may be prescribed higher EPO doses, inducing negative correlation if relevant patient attributes are not properly controlled for. However, employing the instrument seems to account for this. An increase in EPO dose of 1000IUs per week increases a patients HGB by 0.06g/DL, on average. This basically just confirms that EPO is an effective treatment for anemia. These results are further confirmed in Table 4.14. Similar to the HGB results, the OLS coefficient here would suggest that EPO induces a need for more blood transfusions. However, this result is likely biased and the IV column shows that EPO is effectie at reducing the need for blood transfusions.

Table 4.13: The Effect of Epo on Hemoglobin Levels

	OLS	IV
Epo Per Week (1000 IUs)	-0.018***	0.058***
	(0.0003)	(0.0092)
N	2,070,937	2,070,937
First Stage F		231.71
Dep. Var. Mean	11.02	11.02

Notes: Facility-clustered standard errors are in parentheses. An observation is a patient-year. Epo doses are measured in the thousands of IU's per-week. Doses are winsorized at the 99th percentile. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Data from 2008 to 2013 are included. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

After considering these surrogate outcomes, we turn our attention to more acute outcomes: hospitalizations and mortality. In Table 4.15 shows that while EPO does not seem to affect overall hospitalization rates, there is evidence that it increases the likelihood of hospitalization for cardiac diagnoses, a concern found in the descriptive

Table 4.14: The Effect of Epo on Transfusions

	OLS	IV
Epo Per Week (1000 IUs)	0.0046***	-0.0054**
	(0.00006)	(0.00171)
N	2,070,937	2,070,937
First Stage F		231.71
Dep. Var. Mean	0.161	0.161

Notes: Facility-clustered standard errors are in parentheses. An observation is a patient-year. Epo doses are measured in the thousands of IU's per-week. Doses are winsorized at the 99th percentile. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Data from 2008 to 2013 are included. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

medical literature. Similarly, EPO increases the risk of mortality.

Taken in concert, these results seem to suggest tradeoffs associated with the use of EPO. It does effectively treat anemia, increasing patients' HGB levels and decreasing their dependence on blood transfusions. These results may also suggest improved quality of life. However, these improvements come with the cost of increased risk of major adverse events such as hospitalizations and death.

Table 4.15: The Effect of Epo on Hospitalizations

	Any-Cause Hosp.		Cardiac Hosp.	
	OLS	IV	OLS	IV
Epo Per Week (1000 IUs)	0.006***	-0.0009	0.0029***	0.0022*
	(0.00008)	(0.00180)	(0.00004)	(0.00109)
N	2,070,937	2,070,937	2,070,937	2,070,937
First Stage F		231.71		231.71
Dep. Var. Mean	0.647	0.647	0.207	0.207

Notes: Facility-clustered standard errors are in parentheses. An observation is a patient-year. Epo doses are measured in the thousands of IU's per-week. Doses are winsorized at the 99th percentile. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Data from 2008 to 2013 are included. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

Table 4.16: The Effect of Epo on Mortality

	OLS	IV
Epo Per Week (1000 IUs)	0.004***	0.0031**
	(0.00005)	(0.0.00098)
N	2,070,937	2,070,937
First Stage F		231.71
Dep. Var. Mean	0.144	0.144

Notes: Facility-clustered standard errors are in parentheses. An observation is a patient-year. Epo doses are measured in the thousands of IU's per-week. Doses are winsorized at the 99th percentile. Unreported covariates consist of sex and race dummies, a quadratic control for patient age, Elixhauser risk score, dummies for Elixhauser comorbidities, a dummy for history of myocardial infarction, a dummy for whether the diagnosing cardiologist performs PCI's, along with county level median age, income, and share of adults with college degrees. Comorbidities and risk score are calculated using the previous 12 months of claims. An observation is a Medicare patient undergoing cardiac catheterization. Only patients continuously enrolled in traditional Medicare for 12 months prior are included. Data from 2008 to 2013 are included. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

Chapter 5

Conclusion

In this work, I have shown how financial incentives to health care providers, both through acquisition of providers by other firms and through direct changes in payment structure by payers, affects provider decision making. These findings provide further evidence for the need towards careful consideration when designing health care payment systems and antitrust policy.

Financial integration of referring physicians with other healthcare providers shifts the treatment patterns of patients. My results here show that when a cardiologist works with a cardiac surgeon, they respond to the additional financial incentives provided by integration by referring more patients to coronary bypass. They do this primarily by referring fewer patients for medical management, the most conservative option, rather than reallocating intensively treated patients.

This reallocation of patients yields worse outcomes along with higher spending. Increased spending is relatively straightforward to diagnose, as it is driven almost entirely from shifting patients to more expensive treatment options. Because coronary bypass is significantly more expensive than either alternative, shifting patients towards it has large implications for total patient spending. However, it is much more difficult to diagnose the driver of increased mortality and hospitalization. My results suggest that lower quality care, in the form of less oversight of medically managed patients, is what drives worse quality for patients diagnosed by integrated cardiologists.

There are important policy implications of this study. While much of the literature has focused on price effects of integration, my work adds to the growing body of literature showing that integration of health care providers can have significant effects on patient choices and outcomes, disregarding potential price effects. This is clearly demonstrated in Chapter 1, where cardiologists alter treatment choices even facing fixed prices, and in Chapter 2, where dialysis facilities change how they treat Medicare patients when acquired.

Appendix A

Chapter 2 Appendices

The following are supplemental appendices for Chapter 2.

Appendix A.2 contains a description of the method used to calculate predicted mortality risk.

Appendix A.2 contains a list of all CPT codes used to identify catheterization, PCI, and CABG procedures.

A.1 Predicted Mortality

To create a measure of predicted mortality, I estimate a logistic regression of 180-Day mortality on patient characteristics and use predicted values. Formally, I estimate:

$$log\left(\frac{Pr[Dies_{ijt}=1]}{1-Pr[Dies_{ijt}=1]}\right)=\alpha X_{ijt}+\eta_{jt}$$

where X includes the same patient controls as in the baseline analysis, excluding fixed effects. To avoid potential confounding from integration, I estimate this only on patients diagnosed by non-integrated cardiologists. Thus, this is a measure of a patient's mortality risk if they were diagnosed by a non-integrated cardiologist. Given these estimates, I use $\hat{Dies}$ as a measure of predicted mortality.

A.2 CPT Codes

The following table lists all CPT codes used to identify procedures of interest.

Procedure	CPT Codes
Diagnostic Catheterization	93501, 93508-93529, 93451-93468
Percutaeneous Coronary Intervention	92980-92982, 92984, 92995-92996
Coronary Bypass	33510-33536, 33508, 33572

Appendix B

Chapter 3 Appendices

The following are supplemental appendices for Chapter 3.

Appendix B.1 discusses the data we use in greater detail.

Appendix B.4 gives an expanded version of Table 3.1.

Appendix B.5 describes the construction of the predicted mortality index.

Appendix B.6 presents another version of Table 3.1 that includes only observations within 12 months of an acquisition.

Appendix B.7 presents expanded tables for some of our main results.

Appendix B.8 presents results from a robustness check that does not exclude observations from the year of acquisition.

Appendix B.9 presents results from a robustness check that includes patient fixed effects.

Appendix B.10 considers whether changes in for-profit status can explain changes in facilities' behavior following an acquisition.

Appendix B.11 presents results from a robustness check using alternative measures of markets and concentration, and also looks at markets that experience mergers to monopoly as well as markets that are "non-problematic" for antitrust authorities according to the Horizontal Merger Guidelines.

Appendix B.12 presents results from our main specifications but without patient covariates.

Appendix B.13 provides figures for the remainder of our event studies not included in the main body of the paper.

Appendix B.14 details our process for determining ownership changes and the dates of acquisition.

Appendix B.15 provides supporting evidence of standardization following an acquisition.

B.1 Detailed Description of Data Sets

This appendix gives a more detailed discussion of the underlying data sets used to construct our sample. For our analysis, we use patient- and facility-level data from the United States Renal Data System (USRDS). The USRDS is a data clearing house funded by the National Institute of Diabetes and Digestive and Kidney Diseases and the National Institute of Health that collects and stores data related to chronic kidney disease. They combine data from a variety of sources, including Medicare administrative files, Medicare claims, annual facility surveys, and clinical surveillance data, to create the most-comprehensive data set for studying the U.S. dialysis industry.[1] Appendix B.14 provides further details on how we constructed our sample.

[1]For a more thorough description of USRDS, see the *Researcher's Guide to the USRDS System* at USRDS.org.

B.2 Patient Data

USRDS uses a number of data sources to create an exhaustive treatment history for almost all dialysis patients in the U.S. since at least 1991. Patients' demographic information is obtained from the Medical Evidence form submitted to Medicare by providers at the patient's onset of ESRD, which CMS uses to determine eligibility for Medicare coverage.[2] Information collected at this time includes a patient's sex, race, BMI, cause of ESRD, payer, hemoglobin levels, measures of kidney failure, comorbidities (e.g., diabetes and hypertension), type of initial treatment, residential ZIP Code, and facility. After initiation, a patient's residence is updated over time in the CMS Medicare Enrollment Database.

Using a number of different sources, USRDS constructs the Treatment History Standard Analytical File (SAF), which details the complete ESRD treatment history for all patients included in the USRDS database. These data come primarily from the Consolidated Renal Operations in a Web-Enabled Network data system (CROWN-Web), a system established by CMS to track the treatment of ESRD patients. This system contains information submitted by the provider regarding treatments for each individual patient over the previous month.

We combine these data with institutional claims from Medicare, which provide a more granular view of the dialysis treatments received by Medicare patients. Providers submit line-item claims for services other than dialysis. These include all the injectable drugs administered during treatment, which we identify by their Health-

[2]The Medical Evidence form is used to establish the 90 day Medicare eligibility cutoff as well as the 30 month private insurance coordinating period. Consequently, it is required for all patients, regardless of payer.

care Common Procedure Coding System (HCPCS) codes.[3] Unique to this setting, the claims also include clinical measures related to dialysis care and anemia treatment at a monthly frequency, making them among the more-detailed claims data available to researchers.

Transplant and waitlisting events are available to us through the Transplant and Transplant Waiting List SAFs. The Transplant File includes a patient and provider ID for each kidney transplant received by a patient in the USRDS database. Similarly, the Transplant Waiting List SAF includes information on a patient's waitlist status, including their listing date and the transplant center where they are waitlisted.[4] Both of these files are populated using information from the Organ Procurement and Transplantation Network operated by the Department of Health and Human Services.

We focus primarily on four patient outcomes: mortality, hospitalization, urea reduction ratios, and hemoglobin levels. Mortality information comes from the USRDS Patient History File, which includes a date of death for patients. USRDS constructs this variable using information from the CMS Death Notification form, CROWNWeb, and the Social Security Death Master File. Hospitalization data come from institutional claims obtained from Medicare. We focus on three categories of hospitalizations, classified by their reported diagnoses: all cause, septicemia, and cardiovascular events. Urea reduction ratios and hemoglobin levels are reported in the claims data. Medicare required facilities to report urea reduction ratios for all dialysis claims and hemoglobin levels for ESA claims during our sample period, and for all

[3] We use all HCPCS codes for epoeitin alfa, ferric gluconate, and iron sucrose according to the CMS pricing guide at `https://www.cms.gov/Medicare/Medicare-Fee-for-Service-Part-B-Drugs/McrPartBDrugAvgSalesPrice/`.

[4] A patient is waitlisted at a particular transplantation center. They are able to be listed at multiple centers concurrently.

dialysis claims since 2008.[5] With the exception of mortality, we only observe these outcomes for patients for whom we have claims data.

B.3 Facility Data

Dialysis facilities must be certified by CMS to receive reimbursements for ESRD treatment, with the CMS ESRD Annual Facility Survey administered each year to all certified facilities. It records information including the facility ID, address, chain affiliation, labor inputs, number of dialysis stations, for-profit status, and types of treatment offered (e.g., hemodialysis, peritoneal dialysis, or transplant). Using these data, we construct a yearly panel of chain ownership for each facility in our sample. This allows us to examine, at a yearly level, how changes in ownership affect the treatment received by patients.

To construct a monthly panel of chain ownership, we first find all facility-years in our yearly facility panel where the facility listed no chain ownership in one year but did so the following year. We then obtain precise acquisition dates for each facility using data from the Provider of Services (PoS) data set and annual cost reports submitted to CMS, each of which lists certification and change of ownership dates. From this algorithm, we are able to find precise acquisition dates for 1,055 of the 1,236 acquisitions we observe.[6]

In addition to the Annual Facility Survey, providers are required each year to submit certified financial statements to CMS detailing their costs of providing care

[5] Prior to 2008, hemoglobin measures are missing for patients who were not treated with ESAs. But because more than 97% of patients received ESAs during this time period, we still have hemoglobin data for virtually all of our sample.

[6] A more-detailed description of this matching process is available in Appendix B.14.

as part of the Healthcare Cost Reporting Information System (HCRIS), which CMS reserves the right to audit. We use these reports to construct measures of per-unit EPO costs and per-treatment variable costs.

We combine these data sets and drop any patient who is missing demographic or comorbidity data. We also drop observations at facilities that are acquired but do not have reliable dates of acquisition, as well as the 12-month window surrounding an acquisition to reduce measurement error in the timing of acquisition.[7]

[7]Our qualitative results are robust to the inclusion of this time period, though quantitative results are somewhat attenuated due to the introduction of measurement error in the timing of acquisitions. See Appendix B.8.

B.4 Additional Summary Statistics

This appendix presents an expanded version of Table 3.1.

Table B.1: Patient and Treatment Descriptive Statistics by Facility Type

	Always Independent	Pre-Acquisition	Post-Acquisition	Always Chain
Clinical Characteristics				
Diabetic (%)	53.72	54.32	55.38	54.90
Hypertensive (%)	84.34	84.56	85.85	85.32
BMI	28.16	27.92	28.63	28.38
GFR	7.92	7.74	7.99	7.71
Albumin $\geq$ 3.0g/dL (%)	54.50	52.53	53.59	50.99
Hemoglobin	7.68	7.67	7.73	7.56
Cancer (%)	5.18	5.59	4.91	4.44
Drug Use (%)	1.22	1.13	0.98	1.15
Alcohol Use (%)	1.48	1.31	1.20	1.40
Smoker (%)	5.27	6.35	5.80	5.62
Requires Assistance (%)	5.85	4.46	4.88	4.81
Chronic Obstructive Pulmonary Disease (%)	6.79	7.59	6.66	5.78
Atherosclerotic Heart Disease (%)	5.74	7.18	4.76	4.77
Peripheral Vascular Disease (%)	13.44	14.33	12.53	11.47
Ischemic Heart Disease (%)	17.25	20.58	14.84	13.75
Congestive Heart Failure (%)	31.07	32.04	30.29	28.56
Demographics				
Male (%)	53.87	53.18	52.93	52.15
Non-Hispanic White (%)	48.56	53.42	44.41	40.44
Black (%)	32.30	30.65	36.23	39.98
Hispanic (%)	13.06	10.03	13.79	14.77
Asian (%)	3.33	2.57	2.62	2.41
Other Race (%)	5.61	5.33	4.91	4.52
Age (Years)	64.31	64.53	64.02	63.38
Months With ESRD	35.83	31.75	37.06	36.88
Distance (Mi.)[b]	4.93	5.36	5.11	5.00
Area Demographics				
% 18-24 with only High School	31.79	33.24	33.19	32.90
% 18-24 with only Bachelors	9.10	7.81	7.46	7.76
Median Income ($)	50,404.87	48,202.46	47,441.34	47,637.76
Facility Characteristics				
Facility Age (Years)	14.08	12.02	10.10	13.86
Facility Elevation (ft.)	195.54	198.65	211.42	192.58
For-Profit (%)	40.99	64.09	96.40	88.70
Patient Health				
Predicted Mortality (%)	1.03	1.07	1.06	1.17
Treatment				
EPO Per Session ('000 IU's)	4,495.66	4,728.87	6,223.04	6,259.82
Venofer Per Session (mg)	7.95	7.60	15.93	14.86
Ferrlecit Per Session (mg)	6.49	7.22	4.65	4.86
Payments Per Session	179.22	171.79	184.58	183.15
Waitlist or Transplant[a] (%)	10.92	9.63	9.76	9.52
Patient-Months	2,880,503	1,483,917	1,960,286	7,836,538
Incident Patients	235,144	142,815	126,582	400,161

Notes: See text for more detail.

[a] Dummy variable for being waitlisted or transplanted within 1 year for incident patients only.

[b] Median distance is displayed instead of mean.

B.5 Predicted Mortality Index

This appendix describes the construction of the predicted mortality index. To construct the predicted mortality measures in the body of the text, we first regress an indicator for patient death on patient controls, along with month-year fixed effects:

$$Dies_{ijt} = \alpha X_{ijt} + \delta_t + \epsilon_{ijt} \tag{B.1}$$

Here X contains the same variables as in our main specification. This is nearly identical to our main specification, except that it excludes facility fixed effects and acquisition dummies. We then take the fitted values from this as our predicted mortality measure. For incident patients, we use 1 year mortality and year fixed effects.

B.6 Patient Characteristics Around Acquisition

In this appendix we present two summary statistic tables. Table B.61, the second of the two tables, shows a version of Table B.1 including only observations within 12 months of an acquisition. The results show that the differences in patient characteristics we see between pre- and post-acquisition facilities largely disappear when only including observations within the 12 months of an acquisition.

Table B.61: Patient and Treatment Descriptive Statistics Around Acquisition

	Pre-Acquisition	Post-Acquisition
Clinical Characteristics		
Diabetic (%)	55.01	55.19
Hypertensive (%)	86.19	85.89
BMI	28.21	28.09
GFR	7.94	7.88
Albumin $\geq$ 3.0g/dL (%)	52.50	52.13
Hemoglobin	7.72	7.68
Cancer (%)	5.06	4.99
Drug Use (%)	1.08	1.06
Alcohol Use (%)	1.22	1.23
Smoker (%)	5.60	5.73
Requires Assistance (%)	5.05	4.68
Chronic Obstructive Pulmonary Disease (%)	6.88	6.84
Atherosclerotic Heart Disease (%)	5.86	6.08
Peripheral Vascular Disease (%)	13.93	14.03
Ischemic Heart Disease (%)	17.63	18.68
Congestive Heart Failure (%)	31.59	32.08
Patient Demographics		
Male (%)	52.35	52.74
Non-Hispanic White (%)	47.05	47.31
Black (%)	35.64	34.35
Hispanic (%)	11.35	11.97
Asian (%)	2.66	2.89
Other Race (%)	5.37	5.78
Area Demographics		
% 18-24 with only High School	33.08	33.23
% 18-24 with only Bachelors	7.78	7.70
Median Income	47,667.58	47,796.10
Age	64.28	64.38
Months With ESRD	34.16	32.66
Facility Characteristics		
Facility Age	10.65	7.59
Facility Elevation	190.89	194.75
For-Profit (%)	84.03	94.05
Patient-Months	122,500	153,894

Notes. See text for more detail.
[a] Dummy variable for being waitlisted or transplanted within 1 year for incident patients only.

B.7 Expanded Analysis of Outcomes

This appendix presents expanded tables for some of our main results.

Table B.71: Acquisition Effects on Outcomes

	Clinical Outcomes					Hospitalized			
	(1) URR Good	(2) HGB	(3) HGB Good	(4) HGB Low	(5) HGB High	(6) Any Cause	(7) Sept.	(8) Cardiac Event	(9) Payments Per-Session
Post-Acquisition	0.0152***	0.00885***	-0.0283***	-0.00866**	0.0369***	0.00481***	0.000617**	0.000182	0.0551***
	(0.00429)	(0.00259)	(0.00737)	(0.00280)	(0.00796)	(0.00145)	(0.000226)	(0.000502)	(0.00551)
Observations	14,437,638	13,520,140	13,520,140	13,520,140	13,520,140	14,437,638	14,437,638	14,437,638	14,437,637
Baseline Estimate	0.0183	0.0959	-0.0266	-0.0116	0.0382	0.00599	0.000746	0.000616	0.0665
Units	pp	log(g/dL)	pp	pp	pp	pp	pp	pp	log($)
Year x Month FE	X	X	X	X	X	X	X	X	X
Pat. & Fac. Controls	X	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Hemoglobin specifications have different observations because it is not submitted with non-ESA claims for some of our sample. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Payments are winsorized at the 99th percentile. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

B.8 Including the 12 Months Surrounding Acquisition

In this appendix, we present our main results from a sample that includes observations from the year of acquisition. The reason we excluded observations surrounding the year of acquisition in the main body of the paper is that the precise date when a facility is acquired may be measured with error, as discussed in Section 3.2. The results here show that our results are robust to including the year of acquisition. As expected, however, the measurement error we introduce somewhat attenuates the estimated magnitudes.

Table B.81: Effect of Acquisition on Per-Treatment EPO Dose

	(1) Epogen	(2) Epogen	(3) Epogen
Pre-Acquisition	0.300*	0.300*	
	(0.131)	(0.121)	
Post-Acquisition	1.466***	1.337***	0.762***
	(0.0863)	(0.0816)	(0.0663)
Always Chain	1.508***	1.345***	
	(0.0841)	(0.0775)	
Observations	14,437,638	14,437,638	14,437,638
Baseline Estimate	1.485	1.350	0.829
Units	log(IU)	log(IU)	log(IU)
Year x Month FE	X	X	X
Pat. & Fac. Controls		X	X
Facility FE			X

Notes: Facility-clustered standard errors in parentheses. An observation is a patient-month. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We do not drop observations within 6 months of the month of acquisition. Drug doses are winsorized at the 99th percentile. Baseline estimates are post-acquisition coefficients from Table 3.3. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

Table B.82: Acquisition Effects on Outcomes

	Clinical Outcomes					Hospitalized			
	(1) URR Good	(2) HGB	(3) HGB Good	(4) HGB Low	(5) HGB High	(6) Any Cause	(7) Sept.	(8) Cardiac Event	(9) Payments Per-Session
Post-Acquisition	0.0152***	0.00885***	-0.0283***	-0.00866**	0.0369***	0.00481***	0.000617**	0.000182	0.0551***
	(0.00429)	(0.00259)	(0.00737)	(0.00280)	(0.00796)	(0.00145)	(0.000226)	(0.000502)	(0.00551)
Observations	14,437,638	13,520,140	13,520,140	13,520,140	13,520,140	14,437,638	14,437,638	14,437,638	14,437,637
Baseline Estimate	0.0183	0.0959	-0.0266	-0.0116	0.0382	0.00599	0.000746	0.000616	0.0665
Units	pp	log(g/dL)	pp	pp	pp	pp	pp	pp	log($)
Year x Month FE	X	X	X	X	X	X	X	X	X
Pat. & Fac. Controls	X	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Hemoglobin specifications have different observations because it is not submitted with non-ESA claims for most of our sample. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We do not drop observations within 6 months of the month of acquisition. Payments are winsorized at the 99th percentile. Baseline estimates are post-acquisition coefficients from Table 3.5. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

B.9 Adding Patient Fixed Effects

In this appendix, we repeat our analysis of the patient-month variables in specifications that include patient fixed effects. Table B.91 shows results for patients who stay at a single facility and are treated there both before and after acquisition. These specifications do not include facility fixed effects because they are not separately identified given that each patient receives treatment from only one facility in this sample. We find results consistent with our main specification, with identification of the acquisition effect coming solely from within-patient changes following acquisition.

Table B.91: Robustness: Including Patient Fixed Effects

	Drugs			Clinical Outcomes					Hospitalized		
	(1) Epo	(2) Venofer	(3) Ferrlecit	(4) HGB	(5) HGB Good	(6) HGB Low	(7) HGB High	(8) URR Good	(9) Any Cause	(10) Sept.	(11) Cardiac Event
Post-Acquisition	0.845***	0.563***	-0.292***	0.00104	0.00629	-0.0393***	-0.000331	0.0390**	0.0371***	0.00241***	0.00813***
	(0.178)	(0.0937)	(0.0774)	(0.00605)	(0.00386)	(0.0112)	(0.00410)	(0.0123)	(0.00281)	(0.000426)	(0.00106)
Observations	475,694	387,410	418,449	475,694	428,857	428,857	428,857	428,857	475,694	475,694	475,694
Dep. Var. Mean	7.552	1.116	0.676	0.893	2.455	0.534	0.079	0.388	0.108	0.005	0.023
Baseline Estimate	0.829	0.612	-0.303	0.0183	0.0959	-0.0266	-0.0116	0.0382	0.00599	0.000746	0.000616
Dep. Var. Units	log(IU)	log(mg)	log(mg)	pp	log(g/dL)	pp	pp	pp	pp	pp	pp
Year x Month FE	X	X	X	X	X	X	X	X	X	X	X
Pat. & Fac. Controls	X	X	X	X	X	X	X	X	X	X	X
Patient FE	X	X	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Hemoglobin specifications have different observations because it is not submitted with non-ESA claims for most of our sample. Sample includes hemodialysis patients who only ever visit a single facility treated at facilities involved in an independent-to-chain acquisition and bridge the date of acquisition. We drop observations within 6 months of the month of acquisition. Specifications include patient, but not facility, fixed effects. Baseline estimates are post-acquisition coefficients from Tables 3.3-3.5. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

B.10 Acquisition Effects by For-Profit Status

A possible explanation for why acquisitions affect firm behavior is that chains may have different objectives, perhaps focusing more on financial performance than on patient outcomes. That is, their organizational structure may lead them to focus more on profits because, unlike chains, many independent facilities are non-profit entities. A related argument is made by Eaton et al. (2018), who show that the high-powered incentives introduced by private equity acquisitions in higher education lead to better financial performance for the school but worse outcomes for students. At the start of our data, 30.11% of all facilities are non-profit, and there is only 1 non-profit chain, Dialysis Clinic, Inc. Therefore, if the majority of acquisitions were characterized by for-profit chains buying non-profit facilities, one might expect to see the change in firm incentives manifest itself in patterns like the ones we document above, with 11.88% of the 1,065 acquisitions in our data involving a non-profit facility being acquired by a for-profit chain.

To investigate this, we modify our primary specification to interact acquisition status with the for-profit status of a facility when it was independent:

$$Y_{ijt} = \beta Acquired_{jt} + \eta Acquired_{jt} \times ForProfit_j^{Pre} + \alpha X_{ijt} + \epsilon_{ijt}, \tag{B.2}$$

where $ForProfit_j^{Pre}$ is a dummy variable for whether facility j was a for-profit facility prior to being acquired. As shown in Tables B.101 and B.102, the post-acquisition changes across all of our measures are largely the same for acquired independent facilities, regardless of whether they were previously non-profit or for-profit.[8] There are a few notable exceptions to this: the effects of acquisition on EPO and Venofer

[8]These results are in line with those of Duggan (2000), who finds evidence that non-profit hospitals are no more altruistic than for-profit ones.

doses, as well as the use of technicians, are all smaller in the case of for-profit independent facilities. The effect is diminished primarily because for-profit independent facilities were already behaving more like chains along these dimensions, suggesting that changes in for-profit status may account for some portion of our results. Still, these differences are small relative to the acquisition effects experienced by both types of independent facilities. This, together with the fact that for most outcomes we see no significant differences, suggests that the effect of a change in for-profit status is secondary to the overall acquisition effect.

Table B.101: Heterogeneity in Treatment and Outcome Response by For-Profit Status

	Drugs & Payments				Clinical Outcomes					Hospitalized	
	(1) Epo	(2) Venofer	(3) Ferrlecit	(4) Payments Per-Session	(5) URR Good	(6) HGB	(7) HGB Good	(8) HGB Low	(9) HGB High	(10) Any Cause	(11) Cardiac Event
Post-Acquisition	1.149***	1.007***	-0.433**	0.0482***	-0.00297	0.0123	-0.0192	-0.00896	0.0282	0.0131**	0.00274*
	(0.195)	(0.198)	(0.137)	(0.0142)	(0.0101)	(0.00901)	(0.0176)	(0.0104)	(0.0167)	(0.00421)	(0.00134)
Post-Acquisition x Prev. For-Profit	-0.442*	-0.444*	0.144	0.0258	0.0249*	-0.00224	-0.00757	-0.00399	0.0116	-0.00785	-0.00221
	(0.181)	(0.206)	(0.149)	(0.0153)	(0.0112)	(0.00914)	(0.0203)	(0.0106)	(0.0199)	(0.00452)	(0.00143)
Observations	13,820,539	11,349,206	12,190,138	13,820,538	13,820,539	12,955,204	12,955,204	12,955,204	12,955,204	13,820,539	13,820,539
Dep. Var. Mean	7.531	1.336	0.588	5.151	0.881	2.449	0.524	0.096	0.381	0.141	0.030
Units	log(IU)	log(mg)	log(mg)	log($)	pp	log(g/dL)	pp	pp	pp	pp	pp
Year x Month FE	X	X	X	X	X	X	X	X	X	X	X
Pat. & Fac. Controls	X	X	X	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X	X	X	X
Full Effect p-value	0.000	0.000	0.000	0.000	0.000	0.001	0.006	0.000	0.000	0.005	0.414

Notes: Facility-clustered standard errors in parentheses. An observation is a patient-month. Hemoglobin specifications have different observations because it is not submitted with non-ESA claims for most of our sample. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Payments and drug doses are winsorized at the 99th percentile. Prev. For-Profit is an indicator for whether the facility was for-profit in the last year prior to acquisition. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

Table B.102: Heterogeneity in Facility Inputs, Patient Survival, and Transplantation by For-Profit Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Facility Inputs							Survives		Tx or Waitlist	
	Nurses	Technicians	HD Patients	Nurses per Technician	Patients per Employee	Patients Per Station	Employees per Station	180 Days	365 Days	180 Days	365 Days
Post-Acquisition	-0.0159	0.175***	0.115*	-0.369**	0.916***	0.0910	0.0257	-0.0178*	-0.0156	0.00308	0.00423
	(0.0516)	(0.0494)	(0.0512)	(0.127)	(0.244)	(0.166)	(0.0412)	(0.00852)	(0.0114)	(0.00711)	(0.0101)
Post-Acquisition x	-0.00554	-0.157**	0.0237	0.272*	-0.384	0.112	-0.0664	0.00662	0.00345	-0.0118	-0.0209
Prev. For-Profit	(0.0554)	(0.0551)	(0.0540)	(0.132)	(0.267)	(0.188)	(0.0456)	(0.00918)	(0.0123)	(0.00748)	(0.0108)
Observations	24,766	24,766	42,457	23,116	24,766	42,559	24,766	594,343	525,516	674,853	597,882
Dep. Var. Mean	1.548	1.702	3.830	0.970	5.125	3.984	0.814	0.844	0.746	0.067	0.127
Units	log(FTE)	log(FTE)	log(Patients)	-	-	-	-	pp	pp	pp	pp
Year FE	X	X	X	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a facility-year in columns (1)-(8), a new patient starting dialysis in all other columns. Sample includes facilities involved in an independent-to-chain acquisition and facilities that are independent or owned by the same chain for the entirety of our sample or patients starting dialysis at those facilities. We drop facility-years in the year of acquisition and new patients whose observation windows overlap with the date of acquisition. FTE are full-time equivalents. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

B.11 Robustness of Competition Results

This appendix demonstrates the robustness of our results from chapter 3.4 to alternative market definitions, continuous changes in HHI, and different measures of competition. We also present results from monopoly markets and markets that the Horizontal Merger Guidelines deem "non-worrisome" that corroborate our baseline results.

Table 3.7 investigates acquisitions that increase HHI. Table B.111 shows that these results are robust to using a continuous measure of how much HHI changes.

We have shown that acquisitions that increase HHI at the HSA level do not differ in their effects on firm behavior. In line with Eliason (2019) and Wilson (2016b), however, many patients seek treatment outside of their HSA, suggesting that these may not be relevant market definitions. Tables B.112-B.115 show that the results are robust to alternative market definitions, including CBSA and several distance-based measures.

The presence of a direct competitor may also affect an acquirer's ability to alter a target's behavior following acquisition. In Tables B.116-B.118, we show that the presence of a competitor within 1, 5, and 10 miles and the number of competitors within 1, 5, and 10 miles do not affect our qualitative findings.

Table B.111: Acquisition Effects By HHI Change

	Drugs			Clinical Outcomes					Hospitalized		
	(1) Epo	(2) Venofer	(3) Ferrlecit	(4) HGB	(5) HGB Good	(6) HGB Low	(7) HGB High	(8) URR Good	(9) Any Cause	(10) Sept.	(11) Cardiac Event
Post-Acquisition	0.746***	0.583***	-0.290***	0.00976***	-0.0120***	0.0369***	-0.0249**	0.0182***	0.00561**	0.000394	0.000847**
	(0.0692)	(0.0772)	(0.0627)	(0.00277)	(0.00320)	(0.00852)	(0.00783)	(0.00502)	(0.00178)	(0.000631)	(0.000267)
Post-Acquisitions × Δ HHI	0.331	0.259	-0.122	0.00180	0.00462	0.0142	-0.0189	0.00137	0.00405	0.00236	-0.00108
	(0.189)	(0.256)	(0.189)	(0.00509)	(0.00739)	(0.0173)	(0.0162)	(0.0123)	(0.00411)	(0.00165)	(0.000670)
Observations	14,161,244	11,595,400	12,473,162	13,271,104	13,271,104	13,271,104	13,271,104	14,161,244	14,161,244	14,161,244	14,161,244
Dep. Var. Mean	7.538	1.337	0.589	2.449	0.095	0.382	0.523	0.881	0.141	0.030	0.007
Units	log(IU)	log(mg)	log(mg)	log(g/dL)	pp	pp	pp	pp	pp	pp	pp
Year x Month FE	X	X	X	X	X	X	X	X	X	X	X
Pat. & Fac. Controls	X	X	X	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Drug doses are winsorized at the 99th percentile. HHI is calculated at the HSA level. Change in HHI is only non-zero for acquired facilities. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

Table B.112: Acquisition Effects By Concentration Increase - CBSA Markets

	Drugs			Clinical Outcomes					Hospitalized		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Epo	Venofer	Ferrlecit	HGB	HGB Good	HGB Low	HGB High	URR Good	Any Cause	Sept.	Cardiac Event
Post-Acquisition	0.854***	0.582***	-0.270*	0.00149	-0.00247	0.0270*	-0.0246*	0.00717	0.00871**	0.000931	0.00153**
	(0.0966)	(0.167)	(0.137)	(0.00385)	(0.00484)	(0.0125)	(0.0118)	(0.00639)	(0.00306)	(0.00102)	(0.000569)
Post-Acquisitions × Increases CBSA HHI	-0.101	0.0388	-0.0432	0.0110*	-0.0119*	0.0145	-0.00263	0.0145	-0.00354	-0.000409	-0.00102
	(0.0944)	(0.184)	(0.151)	(0.00474)	(0.00580)	(0.0158)	(0.0149)	(0.00821)	(0.00353)	(0.00118)	(0.000622)
Patient-Months	14,161,244	11,595,400	12,473,162	13,271,104	13,271,104	13,271,104	13,271,104	14,161,244	14,161,244	14,161,244	14,161,244
Units	log(UI)	log(mg)	log(mg)	log(g/dL)	pp	pp	pp	pp	pp	pp	pp
Pat. & Fac Controls	X	X	X	X	X	X	X	X	X	X	X
Year x Month FE	X	X	X	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Drug doses are winsorized at the 99th percentile. Observations differ from baseline due to missing ZIP Code-to-market crosswalk data. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

Table B.113: Acquisition Effects For HHI Increases - 1 Mile Radius Markets

	Drugs			Clinical Outcomes					Hospitalized		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Epo	Venofer	Ferrlecit	HGB	HGB Good	HGB Low	HGB High	URR Good	Any Cause	Sept.	Cardiac Event
Post-Acquisition	0.765***	0.601***	-0.253***	0.00691*	-0.00983**	0.0261**	-0.0163*	0.0169**	0.00497**	0.000783	0.000664*
	(0.0755)	(0.0875)	(0.0693)	(0.00309)	(0.00357)	(0.00818)	(0.00753)	(0.00588)	(0.00191)	(0.000648)	(0.000323)
Post-Acquisition ×	0.0266	0.0303	-0.139	0.00887	-0.00518	0.0354	-0.0303	0.00415	0.00297	-0.000489	0.000221
Increases 1 Mile HHI	(0.0969)	(0.152)	(0.129)	(0.00548)	(0.00612)	(0.0202)	(0.0190)	(0.00933)	(0.00358)	(0.00128)	(0.000496)
Observations	14,161,244	11,595,400	12,473,162	13,271,104	13,271,104	13,271,104	13,271,104	14,161,244	14,161,244	14,161,244	14,161,244
Dep. Var. Mean	7.538	1.337	0.589	2.449	0.095	0.382	0.523	0.881	0.141	0.030	0.007
Units	log(IU)	log(mg)	log(mg)	log(g/dL)	pp	pp	pp	pp	pp	pp	pp
Year x Month FE	X	X	X	X	X	X	X	X	X	X	X
Pat. & Fac. Controls	X	X	X	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Drug doses are winsorized at the 99th percentile. HHI is facility specific. Each facility denotes a separate market made up of facilities within 1 mile of that facility. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

Table B.114: Acquisition Effects For HHI Increases - 5 Mile Radius Markets

	Drugs			Clinical Outcomes					Hospitalized		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Epo	Venofer	Ferrlecit	HGB	HGB Good	HGB Low	HGB High	URR Good	Any Cause	Sept.	Cardiac Event
Post-Acquisition	0.833***	0.566***	-0.242**	0.00787*	-0.0134**	0.0218*	-0.00839	0.0202**	0.00588*	0.000765	0.000653
	(0.0857)	(0.0999)	(0.0784)	(0.00375)	(0.00439)	(0.00974)	(0.00915)	(0.00677)	(0.00233)	(0.000803)	(0.000392)
Post-Acquisition ×	-0.111	0.0800	-0.109	0.00391	0.00334	0.0311	-0.0344*	-0.00358	0.000200	-0.000281	0.000162
Increases 5 Mile HHI	(0.0912)	(0.139)	(0.115)	(0.00502)	(0.00570)	(0.0163)	(0.0151)	(0.00903)	(0.00315)	(0.00110)	(0.000489)
Observations	14,161,244	11,595,400	12,473,162	13,271,104	13,271,104	13,271,104	13,271,104	14,161,244	14,161,244	14,161,244	14,161,244
Dep. Var. Mean	7.538	1.337	0.589	2.449	0.095	0.382	0.523	0.881	0.141	0.030	0.007
Units	log(IU)	log(mg)	log(mg)	log(g/dL)	pp	pp	pp	pp	pp	pp	pp
Year x Month FE	X	X	X	X	X	X	X	X	X	X	X
Pat. & Fac. Controls	X	X	X	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Drug doses are winsorized at the 99th percentile. HHI is facility specific. Each facility denotes a separate market made up of facilities within 5 miles of that facility. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

Table B.115: Acquisition Effects For HHI Increases - 10 Mile Radius Markets

	Drugs			Clinical Outcomes					Hospitalized		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Epo	Venofer	Ferrlecit	HGB	HGB Good	HGB Low	HGB High	URR Good	Any Cause	Sept.	Cardiac Event
Post-Acquisition	0.784***	0.537***	-0.245**	0.00656	-0.0104*	0.0303**	-0.0199*	0.0118	0.00392	0.000214	0.000501
	(0.0794)	(0.109)	(0.0907)	(0.00473)	(0.00506)	(0.00997)	(0.00995)	(0.00615)	(0.00268)	(0.000933)	(0.000448)
Post-Acquisition ×	-0.0159	0.112	-0.0872	0.00514	-0.00180	0.0120	-0.0102	0.00991	0.00314	0.000612	0.000362
Increases 10 Mile HHI	(0.0841)	(0.142)	(0.118)	(0.00544)	(0.00599)	(0.0151)	(0.0145)	(0.00842)	(0.00330)	(0.00115)	(0.000521)
Observations	14,161,244	11,595,400	12,473,162	13,271,104	13,271,104	13,271,104	13,271,104	14,161,244	14,161,244	14,161,244	14,161,244
Dep. Var. Mean	7.538	1.337	0.589	2.449	0.095	0.382	0.523	0.881	0.141	0.030	0.007
Units	log(IU)	log(mg)	log(mg)	log(g/dL)	pp	pp	pp	pp	pp	pp	pp
Year x Month FE	X	X	X	X	X	X	X	X	X	X	X
Pat. & Fac. Controls	X	X	X	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Drug doses are winsorized at the 99th percentile. HHI is facility specific. Each facility denotes a separate market made up of facilities within 10 miles of that facility. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

Table B.116: Acquisition Effects By Potential Competitors Within 1 Mile

	Drugs			Clinical Outcomes					Hospitalized		
	(1) Epo	(2) Venofer	(3) Ferrlecit	(4) HGB	(5) HGB Good	(6) HGB Low	(7) HGB High	(8) URR Good	(9) Any Cause	(10) Sept.	(11) Cardiac Event
Post-Acquisition	0.796***	0.683***	-0.351***	0.00935**	-0.00986***	0.0400***	-0.0301**	0.0131*	0.00712***	0.00139*	0.000679*
	(0.0741)	(0.0809)	(0.0691)	(0.00287)	(0.00298)	(0.0103)	(0.00932)	(0.00521)	(0.00183)	(0.000617)	(0.000300)
Post-Acquisition × Has Competitor Within 1 Mile	-0.0536	-0.195*	0.130	0.00161	-0.00490	-0.00517	0.0101	0.0148*	-0.00320	-0.00219*	0.000189
	(0.0671)	(0.0902)	(0.0764)	(0.00323)	(0.00384)	(0.0116)	(0.0107)	(0.00663)	(0.00262)	(0.000881)	(0.000374)
Post-Acquisition	0.795***	0.688***	-0.354***	0.00951***	-0.00992***	0.0403***	-0.0304**	0.0132*	0.00705***	0.00132*	0.000689*
	(0.0742)	(0.0809)	(0.0691)	(0.00287)	(0.00300)	(0.0103)	(0.00935)	(0.00522)	(0.00182)	(0.000616)	(0.000300)
Post-Acquisition × 1 Competitor Within 1 Mile	-0.0372	-0.149	0.0826	0.00221	-0.00501	-0.00267	0.00768	0.0163*	-0.00335	-0.00270**	0.000152
	(0.0702)	(0.0964)	(0.0779)	(0.00334)	(0.00420)	(0.0114)	(0.0105)	(0.00680)	(0.00245)	(0.000931)	(0.000387)
Post-Acquisition × 2 Competitors Within 1 Mile	-0.142	-0.297*	0.273*	0.00319	-0.00609	-0.00744	0.0135	0.00926	-0.00430	-0.00185	0.000633
	(0.0951)	(0.118)	(0.106)	(0.00465)	(0.00561)	(0.0207)	(0.0194)	(0.0101)	(0.00432)	(0.00107)	(0.000541)
Post-Acquisition × 3+ Competitors Within 1 Mile	-0.00800	-0.587*	0.378	-0.00884	-0.000785	-0.0272	0.0280	0.0126	0.00115	0.00242	-0.000526
	(0.245)	(0.292)	(0.204)	(0.0110)	(0.00968)	(0.0238)	(0.0178)	(0.0196)	(0.0112)	(0.00268)	(0.00130)
Patient-Months	14,161,244	11,595,400	12,473,162	13,271,104	13,271,104	13,271,104	13,271,104	14,161,244	14,161,244	14,161,244	14,161,244
Units	log(UI)	log(mg)	log(mg)	log(g/dL)	pp	pp	pp	pp	pp	pp	pp
Pat. & Fac Controls	X	X	X	X	X	X	X	X	X	X	X
Year x Month FE	X	X	X	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Drug doses are winsorized at the 99th percentile. Each panel represents a separate specification. The top panel includes a dummy variable for having a competing facility within 1 mile. Potential competitors are defined as facilities owned by a different firm within 1 mile in the current time period. The bottom panel includes dummy variables for the number of competing facilities within 1 mile. Observations may vary due to availability of ZIP Code geocoding data. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

Table B.117: Acquisition Effects By Potential Competitors Within 5 Miles

	Drugs			Clinical Outcomes					Hospitalized		
	(1) Epo	(2) Venofer	(3) Ferrlecit	(4) HGB	(5) HGB Good	(6) HGB Low	(7) HGB High	(8) URR Good	(9) Any Cause	(10) Sept.	(11) Cardiac Event
Post-Acquisition	0.788***	0.631***	-0.284***	0.00717**	-0.00970**	0.0237**	-0.0140	0.0156**	0.00470*	0.000967	0.000349
	(0.0810)	(0.0882)	(0.0759)	(0.00270)	(0.00334)	(0.00833)	(0.00794)	(0.00564)	(0.00201)	(0.000736)	(0.000363)
Post-Acquisition ×	-0.0170	-0.0289	-0.0297	0.00427	-0.00291	0.0225*	-0.0196	0.00420	0.00200	-0.000545	0.000616
Has Competitor Within 5 Miles	(0.0646)	(0.0931)	(0.0803)	(0.00288)	(0.00341)	(0.0110)	(0.0103)	(0.00627)	(0.00233)	(0.000850)	(0.000381)
Post-Acquisition	0.804***	0.686***	-0.319***	0.00762**	-0.0102**	0.0253**	-0.0151	0.0156**	0.00449*	0.000894	0.000355
	(0.0810)	(0.0899)	(0.0775)	(0.00378)	(0.00349)	(0.00885)	(0.00843)	(0.00573)	(0.00204)	(0.000743)	(0.000368)
Post-Acquisition ×	0.0602	0.195	-0.172	0.00628	-0.00568	0.0288	-0.0231	0.00463	0.000817	-0.000940	0.000618
1 Competitor Within 5 Miles	(0.0760)	(0.103)	(0.0911)	(0.00378)	(0.00392)	(0.0155)	(0.0141)	(0.00669)	(0.00272)	(0.00102)	(0.000427)
Post-Acquisition ×	-0.0338	-0.106	-0.0437	0.00646	0.00129	0.0492**	-0.0505**	-0.00723	0.00507	0.000181	0.00103
2 Competitors Within 5 Miles	(0.0744)	(0.133)	(0.117)	(0.00389)	(0.00510)	(0.0165)	(0.0161)	(0.00941)	(0.00344)	(0.00119)	(0.000585)
Post-Acquisition ×	-0.107	-0.341**	0.178	0.000982	-0.000880	0.00547	-0.00459	0.00769	0.00240	-0.000309	0.000468
3+ Competitors Within 5 Miles	(0.0824)	(0.112)	(0.0960)	(0.00412)	(0.00491)	(0.0129)	(0.0119)	(0.00860)	(0.00314)	(0.00109)	(0.000456)
Patient-Months	14,161,244	11,595,400	12,473,162	13,271,104	13,271,104	13,271,104	13,271,104	14,161,244	14,161,244	14,161,244	14,161,244
Units	log(UI)	log(mg)	log(mg)	log(g/dL)	pp	pp	pp	pp	pp	pp	pp
Pat. & Fac Controls	X	X	X	X	X	X	X	X	X	X	X
Year x Month FE	X	X	X	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Drug doses are winsorized at the 99th percentile. Each panel represents a separate specification. The top panel includes a dummy variable for having a competing facility within 5 miles. Potential competitors are defined as facilities owned by a different firm within 5 miles in the current time period. The bottom panel includes dummy variables for the number of competing facilities within 5 miles. Observations may vary due to availability of ZIP Code geocoding data. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

Table B.118: Acquisition Effects By Potential Competitors Within 10 Miles

	Drugs			Clinical Outcomes					Hospitalized		
	(1) Epo	(2) Venofer	(3) Ferrlecit	(4) HGB	(5) HGB Good	(6) HGB Low	(7) HGB High	(8) URR Good	(9) Any Cause	(10) Sept.	(11) Cardiac Event
Post-Acquisition	0.797***	0.616***	-0.247**	0.00763**	-0.00973**	0.0245**	-0.0147*	0.0169**	0.00338	0.000665	0.0000835
	(0.0883)	(0.0919)	(0.0805)	(0.00281)	(0.00356)	(0.00808)	(0.00750)	(0.00548)	(0.00224)	(0.000855)	(0.000413)
Post-Acquisition ×	-0.0270	-0.00473	-0.0760	0.00315	-0.00253	0.0188	-0.0163	0.00203	0.00358	-0.0000670	0.000908*
Has Competitor Within 10 Miles	(0.0723)	(0.0997)	(0.0852)	(0.00280)	(0.00351)	(0.0104)	(0.00971)	(0.00620)	(0.00242)	(0.000930)	(0.000422)
Post-Acquisition	0.815***	0.668***	-0.278***	0.00801**	-0.00966*	0.0261**	-0.0164*	0.0160**	0.00341	0.000650	0.000122
	(0.0879)	(0.0936)	(0.0829)	(0.00310)	(0.00377)	(0.00893)	(0.00834)	(0.00559)	(0.00230)	(0.000863)	(0.000422)
Post-Acquisition ×	0.0536	0.208	-0.202*	0.00503	-0.00205	0.0266	-0.0245	-0.00234	0.00390	-0.000134	0.00110*
1 Competitor Within 10 Miles	(0.0890)	(0.113)	(0.0989)	(0.00426)	(0.00416)	(0.0172)	(0.0157)	(0.00702)	(0.00278)	(0.00105)	(0.000498)
Post-Acquisition ×	0.0372	-0.259	0.0577	0.00113	-0.00462	0.0148	-0.0101	0.00352	0.00133	-0.000144	0.000830
2 Competitors Within 10 Miles	(0.0929)	(0.152)	(0.128)	(0.00346)	(0.00544)	(0.0156)	(0.0156)	(0.0106)	(0.00369)	(0.00131)	(0.000595)
Post-Acquisition ×	-0.105	-0.163	0.0183	0.00194	-0.00254	0.0130	-0.0105	0.00534	0.00373	0.00000206	0.000769
3+ Competitors Within 10 Miles	(0.0832)	(0.122)	(0.103)	(0.00393)	(0.00488)	(0.0125)	(0.0115)	(0.00780)	(0.00306)	(0.00113)	(0.000486)
Patient-Months	14,161,244	11,595,400	12,473,162	13,271,104	13,271,104	13,271,104	13,271,104	14,161,244	14,161,244	14,161,244	14,161,244
Units	log(UI)	log(mg)	log(mg)	log(g/dL)	pp	pp	pp	pp	pp	pp	pp
Pat. & Fac Controls	X	X	X	X	X	X	X	X	X	X	X
Year x Month FE	X	X	X	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Drug doses are winsorized at the 99th percentile. Each panel represents a separate specification. The top panel includes a dummy variable for having a competing facility within 10 miles. Potential competitors are defined as facilities owned by a different firm within 10 miles in the current time period. The bottom panel includes dummy variables for the number of competing facilities within 10 miles. Observations may vary due to availability of ZIP Code geocoding data. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

The results above show that the effect of an acquisition on, say, how much EPO a provider gives its patients is not influenced by whether or how much the acquisition increased HHI, which suggests that the transference of firm strategy does not depend on market structure. In addition, perhaps the cleanest test of this involves monopoly markets. About 1/3 of all acquisitions happen in monopoly markets, where there is no change in market structure by definition (i.e., there is just one facility in the market before and after an acquisition). As the results in Table B.119 show, our baseline findings hold even when we restrict ourselves to just using these markets.

We also conside those markets (defined as HSAs) that are not "worrisome" according to the 2010 Horizontal Merger Guidelines. That is, we look only at acquisitions with a change in HHI < 100 or where the market is un-concentrated (HHI $<$1500). As we have mentioned before, most of the markets in which acquisitions occur do not experience a change in market structure because acquisitions represent de novo entry by the acquiring firm, and in those where HHI grows due to the acquisition, things were already fairly competitive. Thus, most of the identifying variation in our baseline results stems from markets that the DOJ and FTC would not classify as "worrisome." A related point is made by Wollmann (2019). The results when we restrict ourselves to looking only at these markets appear in Table B.1110. As the table shows, the results are very similar to our baseline results.

Table B.119: Acquisition Effects in HSAs With 1 Facility

	Drugs			Clinical Outcomes					Hospitalized		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Epo	Venofer	Ferrlecit	URR Good	HGB	HGB Good	HGB Low	HGB High	Any Cause	Sept.	Cardiac Event
Post-Acquisition	1.030***	0.645***	-0.374**	0.0146	0.0101*	-0.0238	-0.0145**	0.0383*	0.00826*	0.00120*	0.000677
	(0.119)	(0.155)	(0.126)	(0.00804)	(0.00486)	(0.0142)	(0.00534)	(0.0160)	(0.00323)	(0.000565)	(0.00112)
Observations	3,387,541	2,603,137	2,856,211	3,387,541	3,177,071	3,177,071	3,177,071	3,177,071	3,387,541	3,387,541	3,387,541
Dep. Var. Mean	7.392	1.319	0.594	0.881	2.451	0.524	0.092	0.385	0.138	0.007	0.030
Units	log(IU)	log(mg)	log(mg)	pp	log(g/dL)	pp	pp	pp	pp	pp	pp
Year x Month FE	X	X	X	X	X	X	X	X	X	X	X
Pat. & Fac. Controls	X	X	X	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample and where there is only 1 facility in the HSA. We drop observations within 6 months of the month of acquisition. Drug doses are winsorized at the 99th percentile. Each panel represents a separate specification. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

Table B.1110: Effect of Acquisition in "Non-Worrisome" Markets

	Drugs			Clinical Outcomes					Hospitalized		
	(1) Epo	(2) Venofer	(3) Ferrlecit	(4) HGB	(5) HGB Good	(6) HGB Low	(7) HGB High	(8) URR Good	(9) Any Cause	(10) Sept.	(11) Cardiac Event
Post-Acquisition	0.798***	0.563***	-0.271**	0.00999*	-0.0124*	0.0343**	-0.0219*	0.0213**	0.00769**	0.00117	0.000901*
	(0.0836)	(0.118)	(0.0943)	(0.00415)	(0.00505)	(0.0112)	(0.0103)	(0.00717)	(0.00264)	(0.000972)	(0.000379)
Observations	13,277,095	10,779,274	11,621,592	12,461,781	12,461,781	12,461,781	12,461,781	13,277,095	13,277,095	13,277,095	13,277,095
Dep. Var. Mean	7.515	1.311	0.599	2.449	0.096	0.381	0.524	0.880	0.141	0.030	0.007
Units	log(IU)	log(mg)	log(mg)	log(g/dL)	pp	pp	pp	pp	pp	pp	pp
Year x Month FE	X	X	X	X	X	X	X	X	X	X	X
Pat. & Fac. Controls	X	X	X	X	X	X	X	X	X	X	X
Facility FE	X	X	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. Acquired facilities that are deemed potentially problematic by the Horizontal Merger Guidelines are dropped. We drop observations within 6 months of the month of acquisition. Drug doses are winsorized at the 99th percentile. HHI is calculated at the HSA level. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

B.12 Excluding Patient Characteristics

Here, we present results without patient covariates, which suggest that if selection on patient covariates is occurring, it is in favor of healthier patients and biases our results towards zero.

Table B.121: Robustness Check: Acquisition Effects Excluding Patient Controls

	Drugs & Payments				Clinical Outcomes					Hospitalized	
	(1) Epo	(2) Venofer	(3) Ferrlecit	(4) Payments	(5) URR Good	(6) HGB	(7) HGB Good	(8) HGB Low	(9) HGB High	(10) Any Cause	(11) Cardiac Event
Post-Acquisition	0.781***	0.582***	-0.272***	0.0670***	0.0201***	0.00876***	-0.0206*	-0.0105***	0.0312***	0.00623***	0.000747
	(0.0577)	(0.0727)	(0.0625)	(0.00597)	(0.00469)	(0.00256)	(0.00829)	(0.00291)	(0.00896)	(0.00165)	(0.000565)
Observations	14,161,244	11,595,400	12,473,162	14,161,243	14,161,244	13,271,104	13,271,104	13,271,104	13,271,104	14,161,244	14,161,244
Baseline Estimate	0.829	-0.303	-0.612	0.0665	0.0183	0.00992	-0.0266	-0.0116	0.0382	0.00599	0.000616
Units	log(IU)	log(mg)	log(mg)	log($)	pp	log(g/dL)	pp	pp	pp	pp	pp
Year x Month FE	X	X	X	X	X	X	X	X	X	X	X
Pat. & Fac. Controls											
Facility FE	X	X	X	X	X	X	X	X	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a patient-month. Sample includes hemodialysis patients who have complete covariates and are treated at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop observations within 6 months of the month of acquisition. Specifications include only time and facility fixed effects as controls. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

Table B.122: Robustness Check: Survival Acquisition Effects Excluding Patient Controls

	Survives for:		
	(1) 180 Days	(2) 365 Days	(3) 730 Days
Post-Acquisition	-0.00718	-0.00707	-0.00832
	(0.00368)	(0.00507)	(0.00706)
Observations	609,960	539,487	457,184
Baseline Estimate	-0.0107	-0.0127	-0.0174
Units	pp	pp	pp
Year FE	X	X	X
Pat. & Fac. Controls			
Facility FE	X	X	X

Notes. Facility-clustered standard errors in parentheses. An observation is a new dialysis patient. Sample includes new patients starting dialysis at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop any patients whose observation window overlaps the acquisition date. We only include those patients who remain at their original facility until death or the end of the observation window. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

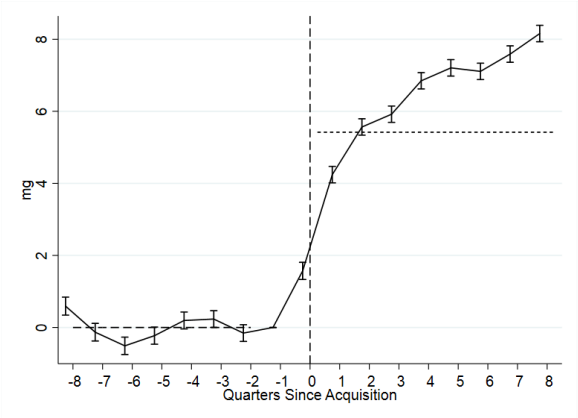
Table B.123: Robustness Check: Transplant Acquisition Effects Excluding Patient Controls

	Waitlisted or Transplanted Within:		
	(1) 180 Days	(2) 365 Days	(3) 730 Days
Post-Acquisition	-0.00229	-0.00355	-0.00415
	(0.00275)	(0.00449)	(0.00785)
Observations	690,391	610,955	498,056
Baseline Estimate	-0.00568	-0.0108	-0.0188
Units	pp	pp	pp
Year FE	X	X	X
Pat. & Fac. Controls			
Facility FE	X	X	X

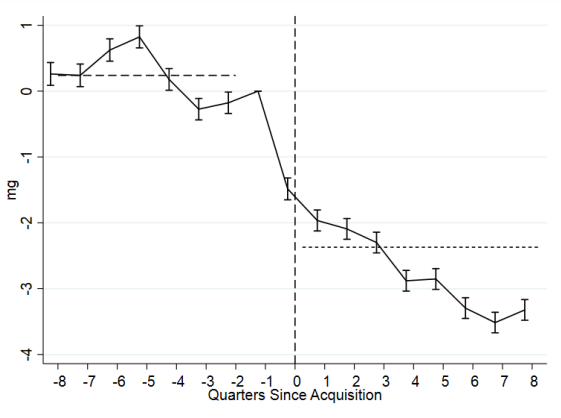
*Notes.*Facility-clustered standard errors in parentheses. An observation is a new dialysis patient. Sample includes new patients starting dialysis at facilities involved in an independent-to-chain acquisition or that are independent or owned by the same chain for the entirety of our sample. We drop any patients whose observation window overlaps the acquisition date. We only include those patients who remain at their original facility until death or the end of the observation window. *, ** and *** indicate significance at the 5%, 1% and 0.1% level, respectively.

B.13 Event Studies

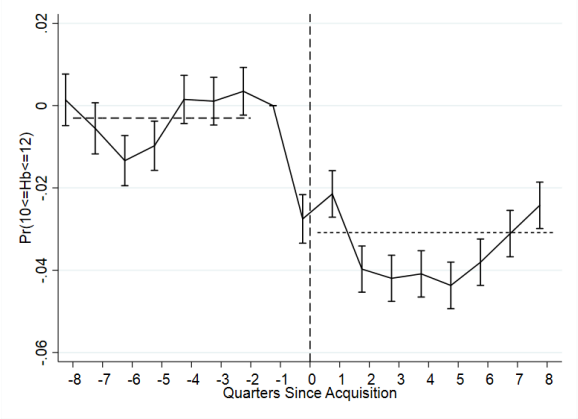
In this appendix, we present other event studies of the dependent variables analyzed in chapter 3.3 but for which we did not include figures. For monthly plots, months outside the 48 month window are included in the regression but not shown. Observations are binned by quarter to reduce noise. Observations within 6 months of acquisition are included. Horizontal lines indicate mean of pre- and post- acquisition dummy variables, respectively. For annual plots, years outside the 8 year window are included in the regression but not shown. Error bars are 95 percent confidence intervals.



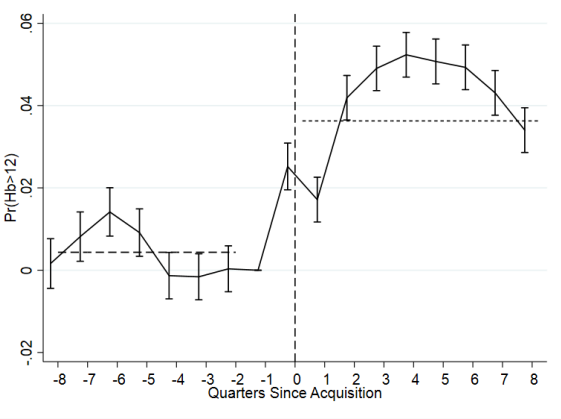
Venofer



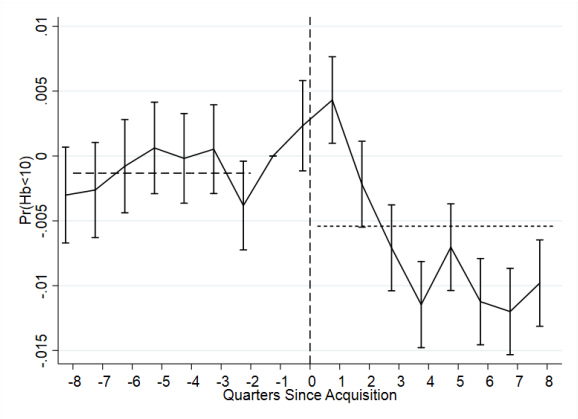
Ferrlecit



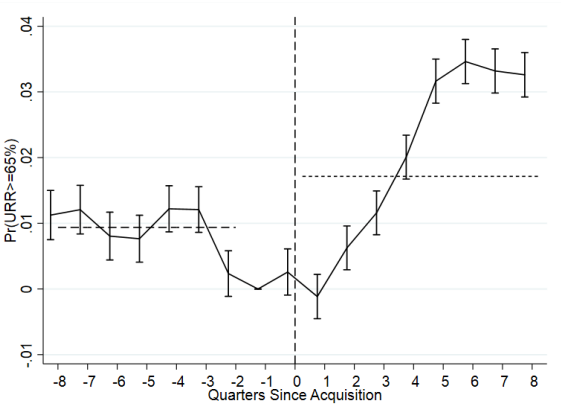
Good Hemoglobin



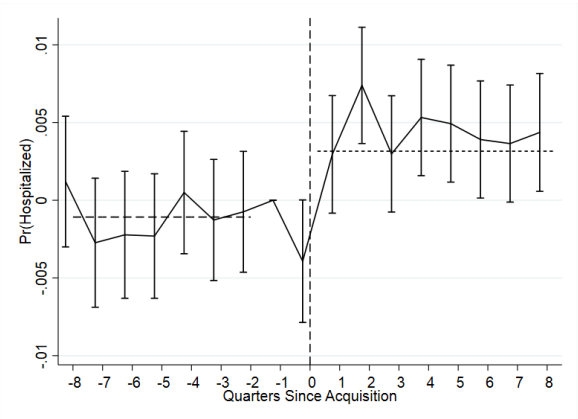
High Hemoglobin



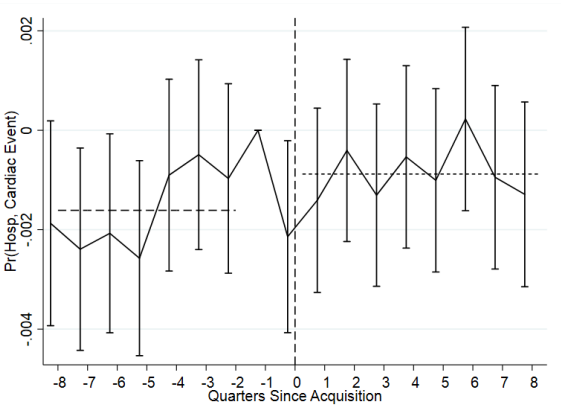
Low Hemoglobin



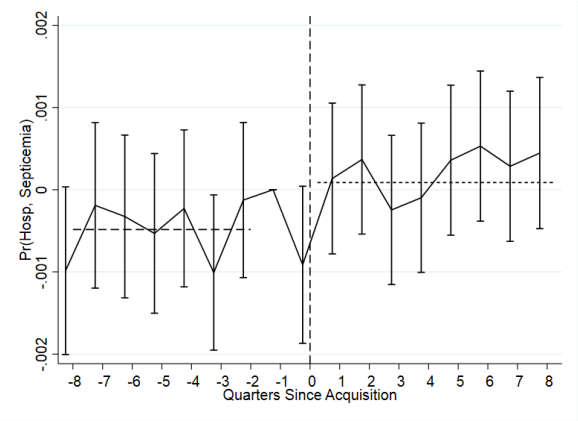
Good URR



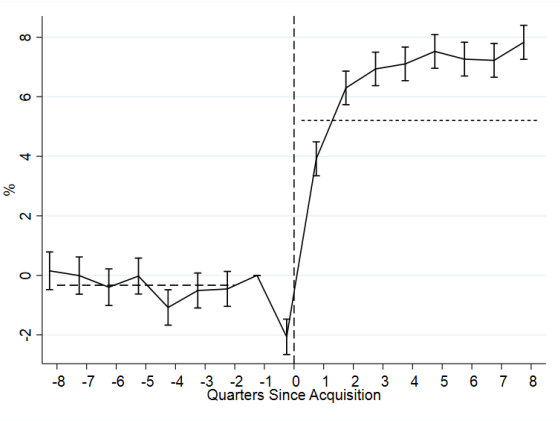
Any Cause Hospitalization



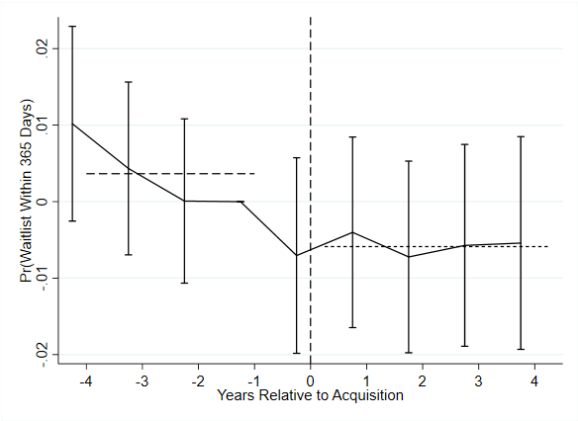
Cardiac Hospitalization



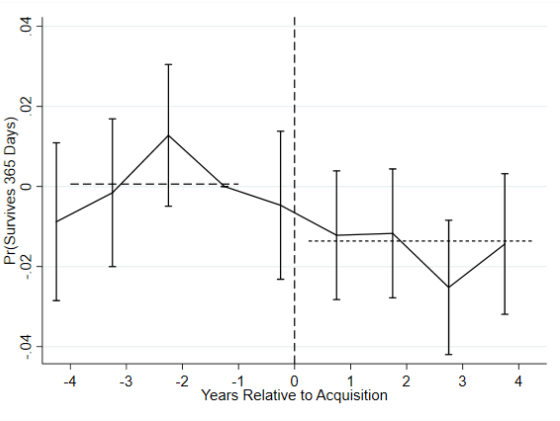
Septicemia Hospitalization



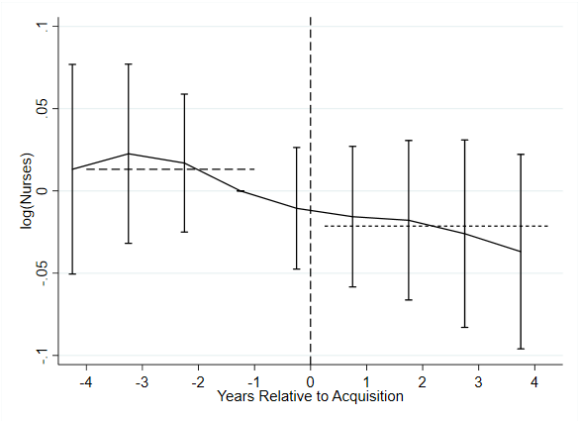
Payments



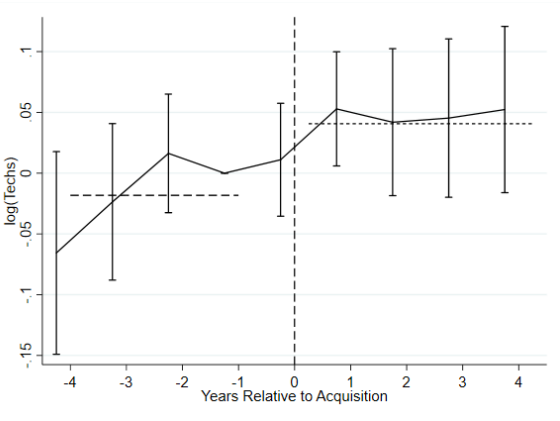
1 Year Waitlisted



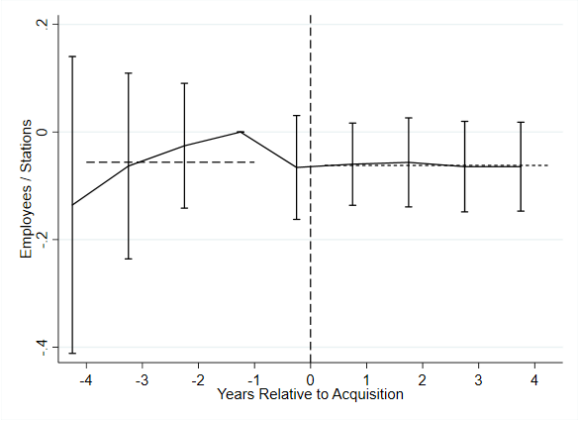
1 Year Survival



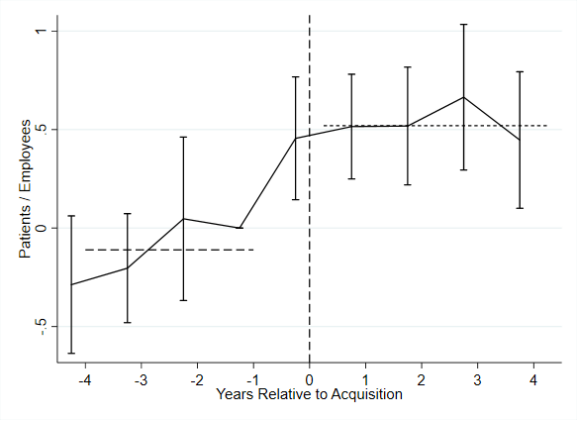
Nurses



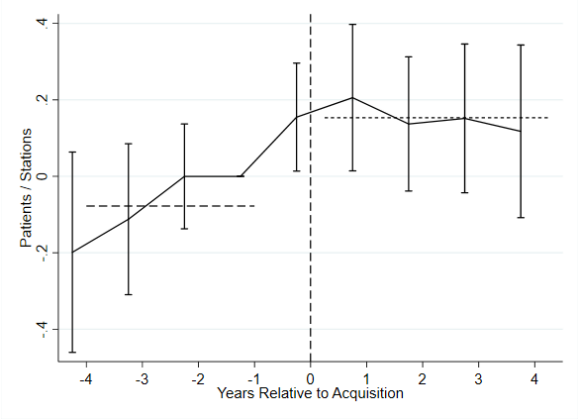
Technicians



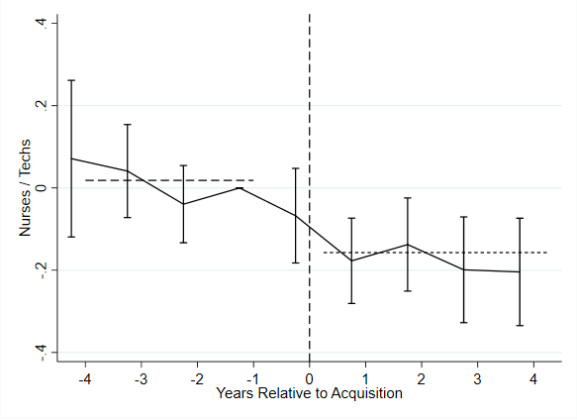
Employees-Per-Station



Patients-Per-Employee



Patients-Per-Station



Nurses-Per-Technician

B.14 Identifying Changes of Ownership and Acquisition Dates

We construct our acquisition data in two steps. First, we identify the set of facilities that undergo an acquisition using the Annual Facility Survey from the USRDS data. These data identify the chain affiliation of each facility on an annual basis. In most cases, we can track the same facility across years using their Medicare ID and observe changes in chain affiliation. However, sometimes the Medicare ID changes after an acquisition. To identify these cases, we match facilities with the same location that have different chain affiliations in consecutive years and interpret this as a change in ownership.

Second, after identifying the set of facilities that changed ownership, we use a variety of sources, all published by CMS, to establish the precise date the change occurred. To do so, we assign each facility the acquisition date that is highest in the following hierarchy:

1. Change of Ownership date in Provider of Service File (PoS)
2. Certification date in PoS
3. Certification date in cost reports (HCRIS)
4. Report filing date in cost report if multiple reports are filed for one year.

The use of certification dates, in addition to reported change of ownership dates, is motivated by CMS documentation. Data from the PoS come from CMS registration form 855-A. This form states that certification as a new provider is required if the

provider is: "Undergoing a change of ownership where the new owner will not be accepting assignment of the Medicare assets and liabilities of the seller/former owner." Additionally, filing regulations for cost reports state (CMS-Pub. 15-2-1): "A provider (including a provider that changes ownership) is considered to be a new provider upon its entry into the program if it enters the program at the inception of or during its initial business year. ... If the provider enters the program at the same time that it begins operations, the initial cost reporting period will begin with the effective date of participation."

Below, we present a table with the number of acquisition dates matched in each step. Of the 1,236 acquisitions identified in the data, we are able to match precise acquisition dates to 1,088. After restricting to facilities where we observe patients, we end up with a sample of 1,026 acquisitions.

Source for Acquisition Date	Count
PoS Change of Ownership Date	761
PoS Certification Date	299
HCRIS Certification Date	24
HCRIS Report Dates	4
Total Number of Acquisitions Identified in Facility Survey	1,236
Number Matched to Precise Acquisition Date	1,088

Notes. Each observation is an acquisition event. Counts in the first panel only include new matches. For example, if a facility matches both the PoS Change of Ownership date and PoS Certification date, it will be counted in the first row but not the second.

These dates are very difficult to validate from sources outside of CMS, as acquisitions are not (usually) required to be reported to antitrust authorities. That said, we check these dates for consistency across all of our sources and find that more than 80% of acquisitions match multiple criteria and there are no instances of a facility having multiple conflicting matched dates.

B.15 Standardization

Table B.151 shows the R^2 from estimating the regression,

$$EPO_{ijt} = \alpha_y X_{ijt} + \gamma_j + \epsilon_{ijt}, \tag{B.3}$$

separately for pre- and post-acquisition facilities. That is, we regress EPO on all patient covariates included in equation (3.1) but allow each year to have a separate coefficient.

Table B.151: Standardization

	(1) Pre-Acquisition	(2) Post-Acquisition
N	1,483,917	1,960,286
R^2	0.109	0.176

Notes. An observation is a patient-month. R^2 from equation (B.3). Covariates included are the same as equation (3.1).

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