The Effects of Dialysis Firm Consolidation on Patient Health[§]

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Abstract

Like much of healthcare, the U.S. dialysis industry has become increasingly consolidated. Yet, evidence on the mechanisms and magnitude of consolidation's impact on patient health remains limited. Using 30 years of administrative data covering more than 800 mergers and a stacked event-study design, we show that mergers trigger facility closures and short-term treatment disruptions that harm patients. Patients receive fewer dialysis sessions, and mortality rises by roughly 700 deaths per 100,000 patients in the merger year. Over time, patient health improves, with fewer hospitalizations, ICU days, and blood transfusion events, even as effects on laboratory biomarkers remain mixed. These merger effects are not driven by changes in market concentration, and hold across Medicare payment regimes with differing reimbursement structures and quality incentives. Mergers' health costs, through short-run mortality increases, outweigh their benefits, through long-run reductions in hospitalization. Yet, targeted antitrust safeguards against post-merger treatment disruptions have the potential to curtail harms without sacrificing gains.

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1 Introduction

The U.S. kidney dialysis industry has consolidated rapidly over the last three decades. In 2023, only two firms provided approximately 70% of dialysis treatments (Rosner et al. 2023). Although the industry treats over 500,000 patients with end-stage kidney disease (ESKD) annually and accounts for $45 billion in Medicare expenditures (NIH 2024), the effects of consolidation on health outcomes are not fully understood. Acquisitions of providers could reduce market competition and increase rent-seeking, thereby reducing the quality of patient care, especially in settings where patients have limited alternatives. On the other hand, mergers could improve efficiency and lead to standardization, thereby improving patient outcomes.

In this paper, we provide new evidence on both the mechanisms and magnitude of the health impacts of consolidation by examining market-wide effects arising from short-term disruptions and longer-term quality changes, across distinct Medicare payment regimes that differed in two respects—a shift from fee-for-service to bundled reimbursement, and the introduction of performance-based quality penalties. Our analysis draws on rich administrative panel data on dialysis facilities and patients spanning three decades (1991–2021). Using a stacked event-study design that appropriately accommodates mergers occurring in different years, and transparently captures the temporal evolution of effects, we find that mergers induce closures of merging facilities, and entry of non-merging facilities in exposed markets, which we define as counties. Patients are displaced from merging to non-merging facilities, where congestion rises. Consequently, patients experience short-term disruptions in care, receiving fewer dialysis sessions across the market. In the year of the merger, mortality increases by 4 percent, or roughly 700 deaths per 100,000 patients. Over the five years following a merger, we observe reductions in hospitalizations, ICU days, and inpatient blood transfusion events, suggesting overall long-run improvements in patient health. These

effects cannot be explained by differences in patient demographics or clinical risk profiles across facilities.

While inpatient health outcomes show clear improvement, the effects on laboratory biomarkers measured contemporaneously with treatment are mixed. Biomarkers monitored by Medicare for facility quality improve—consistent with targeted compliance—whereas others, particularly those associated with cardiovascular risk, deteriorate. Correspondingly, mortality from cardiovascular diseases increases, although overall long-run mortality remains unchanged. The observed variability in biomarker responses, despite robust improvements in inpatient outcomes, limits the extent to which these measures can be interpreted as reliable predictors of long-run health.

We provide three final policy-relevant insights. First, comparing merger effects before and after the introduction of the dual Medicare policy changes shows that these policies neither mitigated adverse outcomes, such as mortality, nor amplified beneficial ones, such as reductions in hospitalizations. Second, the effects of mergers do not appear to be driven by changes in market concentration. When we examine mergers separately by type of target and type of acquirer, we find substantial heterogeneity in health effects, even though market concentration generally increases. Third, we conduct a simple cost-benefit analysis comparing the *health* costs of mergers, via the short-run elevation in mortality, to the health benefits, via the long-run reductions in hospitalizations, finding that costs outweigh benefits. Yet, our results have a straightforward implication—antitrust stipulations aimed at minimizing post-merger treatment disruptions, and ensuring smooth patient transitions could preserve patient welfare while still allowing beneficial efficiencies to emerge.[1]

The outpatient dialysis industry has received relatively little attention in the literature on healthcare mergers, which has focused predominantly on consolidation in the inpatient hospital sector (Ho and Hamilton 2000; Mariani et al. 2022; Dafny 2009; Ferrier and Valdmanis 2004, among others). Yet, dialysis differs from hospitals in several important ways

[1]We discuss potential policy responses to reduce treatment disruptions in Section 7.3.

that shape how consolidation may affect patients and markets. First, dialysis patients are relatively homogeneous in their care needs, as treatment is a life-sustaining and standardized procedure delivered to individuals at the final stage of kidney failure. This contrasts with hospitals, which provide a wide range of services to a heterogeneous patient population receiving both emergent and elective care. Second, the high frequency of dialysis treatment imposes strong geographic constraints on patients, making them especially sensitive to local changes in facility access and quality. Hospital mergers, by contrast, tend to affect broader regional competition and referral patterns.[2] Third, dialysis prices are largely set by Medicare through fixed reimbursement rates, so competition occurs primarily along dimensions of quality and capacity rather than price.[3] The effects of mergers in dialysis are likely to be especially consequential for patients and to operate through channels that don't directly generalize from hospital markets, while offering distinctive and informative insights relevant to other settings, such as nursing homes (Hackmann 2019), characterized by local competition and regulated prices.

Our analysis is the first in the literature on dialysis firm consolidation to demonstrate the role of facility closures and patient turnover on health. Our estimates indicate that this short-term churn alone increases mortality by roughly 700 deaths per 100,000, which is nearly four times the 2023 crude death rate for cancer (National Vital Statistics Reports 2025). Prior research has documented merger-related mortality effects that occur independently of facility entry, exit, or patient displacement—for instance, among patients who remain at their facilities during mergers (Eliason et al. 2020), or within a balanced panel of facilities tracked continuously before and after mergers (Wollmann 2020). We highlight a novel, complementary mechanism—mergers increase mortality in the short run through patient displacement and transitional disruptions, a channel distinct from the longer-term structural

[2]Wollmann (2020) uses counties as market units for two reasons. First, United States Renal Data System data, also used in this study, show that the vast majority of patients receive dialysis within their own county. Second, Federal Trade Commission rulings on divestitures frequently define markets along county lines. By contrast, hospital or tertiary care markets are generally defined using Hospital Referral Regions (HRRs), which are larger than counties (approximately 300 HRRs versus over 3,000 counties) and based on patient referral patterns (Dartmouth Atlas of Health Care 2024).

[3]In comparison, research consistently finds that hospital consolidation drives up prices (Gaynor, Ho, and Town 2015).

effects on markets and facilities emphasized previously. Our findings align substantively with studies documenting the adverse health consequences of short-term care disruptions of other types, such as those caused by staffing shortages, in nursing care (Antwi and Bowblis 2018). Further, by incorporating facility and patient turnover across diverse merger types, our framework may account for the long-run health gains we observe, suggesting efficiency- or quality-enhancing effects rarely documented in prior work (Dafny and Lee 2015).

This paper also contributes to the literature on market-wide effects of consolidation in healthcare. While prior work in this line of research estimates market-level impacts (Dafny, Duggan, and Ramanarayanan 2012; Erickson et al. 2018, 2019), we distinguish outcomes for merging and non-merging facilities within the same market. This distinction allows us to draw sharper conclusions about the mechanisms driving market effects. Our findings indicate that mergers influence health outcomes not only at the merging facilities but also at other facilities in the market, suggesting that non-merging entities in affected areas may not provide valid controls for causal inference.

Finally, we provide the first examination of the differential effects of dialysis firm mergers occurring after the 2011 dual reform of the bundled Prospective Payment System (PPS), which shifted dialysis reimbursement from a fee-for-service schedule to a per-treatment bundled payment, and Medicare's End-Stage Renal Disease Quality Incentive Program (QIP), which tied reimbursement to the achievement of clinical and quality targets, finding that this pair of policies ultimately did not alter the health effects of mergers meaningfully.[4,5]

The rest of the paper proceeds as follows. Section 2 offers an introduction to dialysis and provides institutional background pertaining to the role of Medicare and antitrust enforcement in the dialysis industry. Section 3 provides a clinical overview of the health outcomes of interest and a rationale for their inclusion in the analysis. Section 4 describes the data sources, nuances of the study population and descriptive statistics. Section 5 outlines the em-

[4]Sloan et al. (2021) examine post-reform mergers and find that smaller chains and independent facilities were more likely to be acquired, but the health effects of these mergers remain unstudied.

[5]Wollmann (2020) analyzes health outcomes from a few years (2012-2017) covering this policy period but does not assess whether effects differ from periods before the policy.

pirical methods utilized. Section 6 presents the results. Section 7 discusses the implications of the results for policy. Section 8 concludes.

2 Background

2.1 Dialysis

End-Stage Kidney Disease (ESKD) is the final stage of Chronic Kidney Disease (CKD), occurring when the kidneys' ability to filter waste and excess fluids has been severely diminished. As of 2022, 815,896 individuals were living with ESKD (NIH 2024). Treatment options for ESKD include kidney transplants and dialysis. Although transplants can restore kidney function, they are feasible for only a limited subset of patients and frequently involve prolonged waiting periods. For the majority of ESKD patients, dialysis is the only long-term management option.

Hemodialysis is a medical procedure that involves taking the patient's blood out of their body, passing it through the dialysis machine (dialyzer) which cleans the blood, and returning the clean blood to the patient's body.[6] Hemodialysis is predominantly performed in outpatient facilities or centers, but some patients may opt for home hemodialysis after a sustained period of training.[7] Patients receive hemodialysis three to four times per week, with each session lasting between three and five hours. Because of this high treatment frequency, patients are sensitive to facility location.[8] One implication of the localized nature of hemodialysis care is that patients have limited ability to mitigate the negative effects of a dialysis merger by seeking treatment in another market. A second implication is that, when

[6] The dialysis machine contains a filter filled with a solution called dialysate, containing water and chemicals at levels tailored to a patient's needs. The patient's blood flows through the machine, separated from the dialysate by a semi-permeable membrane. Through diffusion, water and waste products like urea and creatinine, and minerals like calcium and potassium are exchanged across the membrane until optimal levels are achieved. The cleansed blood is then returned to the patient's body.

[7] Inpatient hemodialysis is typically reserved for patients undergoing other inpatient treatments.

[8] The median travel distance to a facility for patients receiving in-center hemodialysis is 5.7 miles (Prakash et al. 2014), with patients in rural counties traveling farther than those in metropolitan areas (Velázquez et al. 2022).

patients are able to switch facilities, awareness of merger effects across all facilities in the market becomes relevant for both patient choice and health outcomes.

Another form of dialysis is peritoneal dialysis which uses the patient's own abdominal lining (peritoneum) for blood filtration, and is generally performed at home. Together, home hemodialysis and peritoneal dialysis patients account for a small share of the ESKD population with in-center hemodialysis patients making up 58% of all ESKD patients and around 85% of those receiving dialysis (NIH 2024). Accordingly, we focus on in-center hemodialysis patients and assess how mergers influence the quality of care they receive.

2.2 The Role of Medicare

Prior to 1972, Medicare coverage was available only to individuals aged 65 and older or those with disabilities. The End Stage Renal Disease Program was established in 1972, extending Medicare coverage to Americans if they had ESKD, and were entitled to Medicare based on their work history, regardless of age. Since then, Medicare has played a dominant role in paying for dialysis coverage.

Medicare, including Medicare Advantage, covered 48% of dialysis patients in 2022 and nearly half of the patients receiving in-center dialysis (NIH 2024). Patients under 65 qualify for Medicare coverage after a 3-month waiting period. After this waiting period, there is a 30-month window during which Medicare acts as a secondary payer, with a patient's group health insurance providing primary coverage. After 30 months of treatment, Medicare becomes the primary payer, and the private plan becomes the secondary payer.[9] Medicare coverage based purely on ESKD ends 12 months after the patient stops dialysis treatments or 36 months after a kidney transplant (CMS 2023).[10]

[9] Although Medicare plays a central role in dialysis payment and policy, private insurance is particularly important during the 30-month coordination period when Medicare acts only as a secondary payer. During this period, private insurers primarily cover dialysis and, unlike Medicare, are free to negotiate prices, which are typically much higher than Medicare reimbursement rates. In 2017, one of the largest dialysis providers, DaVita, received approximately $250 per dialysis session from government insurance and about $1,040 per session from commercial insurance (Childers et al. 2019). As a result, facilities may favor privately insured patients and displace Medicare patients from merging to non-merging facilities to maximize revenue, a pattern consistent with evidence in Appendix Figure A.2.

[10] Individuals not entitled to work-based Medicare can receive dialysis coverage through Medicaid.

Medicare Part A covers most inpatient treatments for dialysis patients, as well as most costs associated with kidney transplants. Medicare Part B covers outpatient dialysis treatments and doctors' services, including dialysis at Medicare-certified facilities or at home, home dialysis training, home support services, immunosuppressant drugs, and laboratory tests. Patients do not pay a premium for Medicare Part A. Out-of-pocket costs include a monthly premium of about $175 for Medicare Part B, $240 per year in deductibles, and 20% coinsurance for the Medicare-approved amount for dialysis services (CMS 2023).[11]

In 2011, the Medicare End-Stage Renal Disease Program underwent a dual policy change. First, the bundled Prospective Payment System (PPS) was instituted, changing the way outpatient facilities were reimbursed for dialysis. Instead of receiving traditional fee-for-service payments for each individual component of care, facilities are paid a set amount per treatment under the bundled PPS. CMS determines a per-treatment base rate, which was $273.82 in 2025 (CMS 2024), and is adjusted for factors such as the patient case-mix, facility treatment volume, and nurse training, and augmented by add-ons for medically necessary outlier treatment costs.

At the same time, CMS introduced the Quality Incentive Program (QIP), under which facilities face reimbursement cuts of up to 2 percent if they fail to meet minimum quality performance standards.[12] A facility's quality performance score is based on two types of measures. The first type is clinical measures, where facilities are graded on meeting nationally established benchmarks, and the second is reporting measures, where facilities earn points simply for submitting the required data. Both the set of measures and their classification as clinical or reporting metrics have evolved over time. In 2021, the measures included *biomarkers* such as Kt/V dialysis adequacy, hypercalcemia, and ultrafiltration rate, *clinical endpoints* such as hospital readmissions, hospitalizations, blood transfusions, and bloodstream infections; and facility quality metrics, including vascular access type and patient engagement. Facilities report biomarkers and some quality metrics to CMS monthly via

[11] These are the Medicare cost figures for 2024.

[12] See Subpart H of 86 FR 73515.

the electronic system CROWNWeb, while CMS derives clinical endpoints from claims and other external data sources. Biomarkers and clinical endpoints, including the QIP measures described above, are central to this paper's analysis. We define these terms and provide a detailed account of included measures in Section 3.

2.3 Pre-Merger Review

For-profit firms are the predominant providers of dialysis among Medicare patients (Medicare Payment Advisory Commission 2021). Dialysis firms have undergone rapid consolidation over the last 30 years. In 2023, two large for-profit chains DaVita Inc. and Fresenius Medical Care provided 70% of dialysis treatments in the U.S. (Rosner et al. 2023).

Like all mergers, dialysis firm mergers are within the purview of the Hart-Scott-Rodino (HSR) Act of 1976 or the pre-merger notification program, in which proposed mergers exceeding a threshold in value must notify the Federal Trade Commission (FTC) and the Department of Justice (DOJ), before the merger can take place.[13] If a merger is within the scope of the pre-merger notification program due to its value, the merging parties file the proposed deal with both, the FTC and the DOJ, and only one agency is eventually assigned to the case. Once the merger is filed, the parties are subject to a 30-day waiting period during which the agency reviews the proposal to determine, based on data and documents they can solicit from the parties, whether the merger has potential for competitive harm. If the agency determines that the risk of competitive harm is low, the agency either terminates the waiting period early or allows it to expire, after which the parties are free to transact the deal. Alternatively, the agencies can ask for more information, imposing another waiting period, and begin an additional round of review. At the end of this round, the agency clears the deal, or clears it with caveats - primarily ordering the acquirer to divest facilities in some markets - that aim to mitigate competitive harm, or seeks to stop the entire transaction (Federal Trade Commission 2023).

[13] Prior work by Wollmann (2020) shows that mergers that escape reporting requirements are also harmful and that blocking these mergers can save lives.

It is challenging to determine which of the mergers considered in this paper, particularly the older ones, were subject to the pre-merger notification program and which, if any, facilities were divested. That chain-to-chain mergers were transacted after antitrust enforcement and with divestitures is a reasonable conclusion based on FTC rulings.[14] Nevertheless, in this paper, divested facilities are treated as *merging* facilities, since mergers are recorded solely using changes in ownership.

3 Outcomes: Clinical Background and Inclusion Rationale

In this section, we provide a clinical overview of each category of outcomes and, where required, a rationale for their inclusion in the analysis. We also briefly note how we construct each outcome, with details pertaining to variable construction provided in Appendix B.

3.1 Facility Entry and Closure

To our knowledge, this is the first paper to empirically study effects of mergers in the dialysis industry on entry and closures. The potential for firm entry and incumbent expansion is a policy-pertinent outcome as it is an important consideration in the FTC's assessment of whether a merger poses competitive harm in a market by changing the market concentration. In the context of health, the overall density of facilities in a market as well as differential behavior of merging and non-merging firms may have crucial consequences for patient welfare given that dialysis patients seek care locally. We measure the effect of mergers on market-level closures and entry, using the counts of total, merging, and non-merging facilities in the market as separate outcomes. We emphasize the distinction between firms and individual facilities. The CMS POS data provide information on a facility's chain ownership status but not on the specific owner, precluding analysis at the firm level.

[14] See, for example, the FTC's order in Fresenius' 2012 acquisition of Liberty: `https://tinyurl.com/yyz4j4a3`

3.2 Dialysis Sessions

Closures and entry are likely to affect the number of dialysis sessions delivered in the market both by imposing adjustment frictions on patients who have to move facilities and by changing the congestion of stations at existing facilities. A robust clinical literature links shorter dialysis duration to increased mortality (Hakim et al. 1994; Brunelli et al. 2010; Held et al. 1991; Béguin et al. 2021). Although dialysis hours are an important determinant of kidney health, previous studies of dialysis mergers have not, to my knowledge, investigated this outcome (Eliason et al. 2020; Wollmann 2020). We study the effects of mergers on dialysis sessions per patient at the facility level.

3.3 Facility Decisions

We distinguish between quality-of-care metrics such as congestion and dialysis sessions per patient that may arise, in part, from capacity constraints, and facility level decisions that are unlikely to be driven by such constraints. In this latter category, we consider three facility-driven metrics.

First, we consider the volume of staff in facilities. The worker categories we include are nurses, patient care technicians, and social workers. A dialysis patient's care team is led by a nephrologist, who sets the overall care plan and prescribes medications.[15] Day-to-day treatment is managed by nurses, including registered nurses, licensed practical nurses, and advanced practice nurses, who form the primary staffing input in outpatient dialysis clinics and typically receive specialized dialysis training. Patient care technicians prepare the dialysate, set up patients, monitor vital signs, and maintain equipment. Social workers play a key role in transplant eligibility; CMS requires a psychosocial evaluation by a social worker to process transplant candidacy. We further break down each staff category by full-

[15]The USRDS facility data do not provide information about nephrologist staffing.

time and part-time status, viewing any shifts in staffing volume from full-time to part-time as a potential reduction in quality.

Second, we study anemia drug utilization at facilities. Anemia is highly prevalent in individuals with kidney disease, primarily due to reduced renal synthesis of erythropoietin (EPO), the hormone responsible for initiating the production of red blood cells. During dialysis, anemia is treated with the prescription of erythropoietin stimulating agents (ESAs), which are artificial replacements of EPO, titrated to patient needs and administered intravenously. When anemia management is inadequate, patients typically need red blood cell transfusions to maintain red blood cell volume. CMS is sensitive to the relevance of anemia management in measuring facility quality and monitors it using a facility's standardized transfusion ratio, which captures the number of annual transfusion events for patients at a facility relative to the expected number given the facility's case-mix. In this analysis, we study facility processes relevant to anemia management, namely the share of patients prescribed an ESA, and the average monthly dosage of ESAs.[16] Commonly prescribed ESAs include Epoetin Alpha, Epoetin Beta, and Darbepoetin Alpha which differ slightly in molecular structure and absorption time, but not in treatment efficacy or safety if correctly dosed (Loughnan, Ali, and Abeygunasekara 2011). So, we don't consider the choice of the ESA agent as a quality decision but pool doses of all ESAs measured in units in the analysis of the monthly ESA dosage.[17]

Third, we consider the utilization of arteriovenous fistulas as the modality of vascular access. To receive dialysis, blood must flow in and out of the body at high rates. Central venous catheters are flexible tubes inserted into a large vein providing this high flow rate in the neck, chest, or groin and are used mainly for short-term or emergency dialysis because they carry the highest infection and complication risks. Two other vascular methods combine the property of arteries to provide the high-pressure flow needed for dialysis, and the property

[16] The effect of Medicare payment policy on the use of ESAs has been a focus of work by Eliason et al. (2022) and is not in itself a focus of this paper.

[17] Sometimes, ESAs are prescribed in mcg (micrograms) and sometimes in units. Since the orders of magnitude are different for these dosage types, we only consider ESAs prescriptions in units, regardless of the ESA agent.

of veins to be accessible and puncture easily. Of these, the first method is arteriovenous fistulas which are direct surgical connections between an artery and a vein, usually in the arm, that enlarge the vein and increase blood flow over time, making them the most durable access with the lowest infection risk. The second method is arteriovenous grafts which use a synthetic tube to connect an artery and a vein when a patient's own veins are too small or damaged for a fistula. Grafts last longer than catheters but still have higher rates of infection and clotting than fistulas. Because of their lowest infection risk, fistulas are considered the gold standard for dialysis access, but require outpatient surgery and maturation time. The Medicare QIP rewards facilities for maintaining a higher proportion of patients with fistula access. In this analysis, we examine each facility's share of patients using fistulas relative to other access types.

3.4 Biomarkers

In our analysis of health outcomes we distinguish between *biomarkers* and *clinical endpoints*. We borrow this categorization from Atkinson Jr et al. (2001) who define biomarkers as "[a] characteristic that is objectively measured and evaluated as an indicator of normal biological processes, pathogenic processes, or pharmacologic responses to a therapeutic intervention" and clinical endpoints as "[a] characteristic or variable that reflects how a patient feels, functions, or survives". In our setting, biomarkers are blood or other physiological measurements taken monthly in facilities, and clinical endpoints are hospitalization, days in the ICU, inpatient transfusions and mortality. Clinical endpoints have been the central focus of much of the literature on dialysis mergers, and are important indicators of care quality on their own, but provide limited clinical guidance without complementary analyses of biomarkers that could serve as more real-time targets. Moreover, correlations between biomarkers and endpoints provide insight into the effectiveness of the QIP, which incorporates a subset of biomarkers into its quality ratings, though their predictive validity remains contested (Vanholder, Glorieux, and Eloot 2015). Biomarker data are collected by facilities monthly for the

set of patients that are treated during one day in the month and submitted to the CMS via an online reporting system called CROWNWeb.

The first biomarker of interest to our analysis is dialysis adequacy, an indicator of how effectively the dialysis procedure cleans the blood, measured by the single-pool Kt/V. Kt/V captures the amount of urea or other solutes cleared from the body from before to after the dialysis session.[18] QIP guidelines specify a minimum target Kt/V of 1.2 and evaluate facilities based on the proportion of reported patient-month observations that meet or exceed this threshold. Accordingly, our outcome measure is the share of a facility's patients achieving a "good Kt/V," defined as Kt/V of at least 1.2 mg/dL.

The second biomarker of interest is hypercalcemia. Impaired kidney function in patients with ESKD compromises the body's ability to maintain electrolyte balance, often leading to abnormal blood calcium levels. Hypercalcemia, or elevated calcium levels, poses a significant cardiovascular risk, as excess calcium can disrupt the electrical impulses that regulate cardiac muscle function and contribute to vascular calcification. Accordingly, QIP tracks patients with serum calcium levels exceeding 10.2 mg/dL.[19] In this paper, we study the share of a facility's patients with hypercalcemia as defined by the QIP.

The third biomarker of interest is the ultrafiltration rate (UFR), which measures the rate at which fluid is removed from a patient's blood during a dialysis session.[20] A high UFR is undesirable because rapid fluid removal can cause complications such as hypotension, cramping, and cardiac stress. Conversely, a low UFR may also be harmful, increasing cardiovascular risks. Unlike Kt/V or hypercalcemia, UFR is a reporting measure in the QIP; facilities are required only to report the components used to calculate UFR, and the program

[18] Specifically, Kt/V is measured as a patient's urea clearance, multiplied by the duration of the dialysis treatment in minutes, divided by the volume of total body water. Another commonly used metric of dialysis adequacy is the Urea Reduction Ratio (URR). The URR is calculated as the percentage change in the blood urea nitrogen from before to after dialysis. Kt/V and URR are mathematically related.

[19] At the same time, hypocalcemia defined as serum calcium levels below 8.5 mg/dL (UCSF Hospitalist Handbook N.d.), is also undesirable as it increases muscle dysfunction and cardiovascular risks. I report the effects of mergers on hypocalcemia in Appendix Figure A.6, finding no merger-induced changes in this outcome.

[20] UFR (mL/kg/hr) $= \frac{\text{Net fluid removal (mL)}}{\text{Dialysis time (min)} \times \text{Post-dialysis weight (kg)}} \times 60$. Net fluid removal (mL) = [Pre-dialysis weight (kg) − Post-dialysis weight (kg)] × 1000.

does not specify optimal thresholds. Nonetheless, clinical evidence suggests that UFR values exceeding 10 mL/kg/hr are associated with higher cardiovascular risk (Flythe, Kimmel, and Brunelli 2011; Kim et al. 2018). In this analysis, we compute each patient's UFR using CROWNWeb data elements and examine how mergers affect the facility-level annual average UFR, which captures variation in both excessively high and low fluid removal rates.

We also examine three additional biomarkers that are collected by CMS through CROWNWeb but are not included in the QIP's portfolio of facility quality measures, namely hemoglobin, albumin, and iron saturation.[21] Specifically, we analyze the share of patients with low hemoglobin levels (<10 g/dL), the annual facility average of serum albumin (g/dL), and the annual facility average of pre-dialysis iron saturation (%). Among these, low hemoglobin and iron saturation are markers of anemia risk, while low albumin levels (<3.5 g/dL) indicate advanced kidney failure and are a known predictor of cardiovascular risk (Manolis et al. 2022). Both QIP and non-QIP biomarkers are available from 2012 onward, following the rollout of CROWNWeb, but they offer complementary insights. Effects of mergers on QIP biomarkers may capture policy interactions, that is, how mergers influence facilities' performance on measures explicitly monitored by the QIP. In contrast, effects on non-QIP biomarkers more directly reflect the intrinsic impact of mergers on patient care, as these outcomes are not subject to QIP reporting or incentives.

3.5 Clinical Endpoints

As outlined in Section 3.4, clinical endpoints in this analysis are hospitalization, ICU days, inpatient blood transfusions and mortality. Hospitalizations signal shifts from outpatient to inpatient care, serve as indicators of suboptimal outpatient quality, and also represent substantial ESKD-related costs that merit consideration in Medicare's assessment of the dialysis program. In the QIP, facilities are graded on the standardized hospitalization ratio, which is the ratio of total inpatient admissions for patients at a facility divided by the expected

[21] Hemoglobin was included as a QIP measure from 2012 to 2014 but was subsequently removed.

admissions given the case-mix. In this analysis, we consider a facility's hospitalizations per patient, measured from inpatient Medicare claims.

The second endpoint of interest is the frequency of blood transfusions, measured in the QIP by a standardized transfusion ratio and in this study by the number of inpatient events comprising red blood cell transfusions per patient. A higher per-patient blood transfusion rate is indicative of inadequate management of anemia. Third, we study the number of days in the ICU per patient, which is informative about the severity of hospitalizations.

The final clinical endpoint of interest is mortality from all causes as well as from various leading complications of kidney disease. We measure mortality as the facility's annual instantaneous mortality rate, which the ratio of total patients who die in any year to the total patients treated. Both mortality and the number of ICU days are not within the QIP's portfolio of facility-quality measures.

4 Data and Descriptive Statistics

4.1 Datasets

The primary data source for this paper is the United States Renal Data System (USRDS). This is a registry funded jointly by the Centers for Medicare and Medicaid Services (CMS) and the National Institute of Diabetes and Digestive and Kidney Diseases (NIDDK) of the National Institutes of Health (NIH). It includes longitudinal data on all outpatient dialysis facilities in the U.S., as well as all Medicare Part A and B claims beginning on the first day of dialysis for all Medicare Fee-for-Service (Original Medicare) patients, payer information, treatment history, laboratory measurements from CROWNWeb submitted in compliance with Medicare QIP requirements, and patients' information from the CMS Form 2728, which is required within 45 days of dialysis initiation. The USRDS is not a public dataset and can be accessed upon Institutional Review Board (IRB) review, proposal submission and a Data Use Agreement (DUA).

While the USRDS facility data provide information on many facility characteristics, it lacks detailed geographic and ownership information and is not by itself adequate to identify merger events. To identify merger events, we supplement the USRDS data with the publicly available CMS Provider of Services (POS) files. We obtain the POS files from the NBER data archive and utilize the facility data from the fourth quarter in each year. We identify merger events using facility ownership change dates, merger types using facility owner type (chain vs independent) details, and markets using county and other location information in the CMS POS. Further details about these procedures are described in Appendix B. We merge facility data from the USRDS with facility data from the CMS POS using Medicare facility identifiers.

Some analyses in this paper are at the market-level, where outcomes are constructed by aggregating facility-level outcomes up to the market-level. Other analyses are at the facility level where outcomes are either obtained directly from the USRDS facility data or by aggregating patient-level outcomes up to the facility-level. The algorithm we use to map patients to facilities is detailed in Appendix B.

Market-level covariates like county median income and population sizes by age, sex and race are from publicly available annual U.S. Census vintages. Data on the status of a CON law for renal care facilities are obtained from the American Health Planning Association.[22]

4.2 Study Population

The study period and population differs by outcome. In the USRDS data, a history of patients' treatment modalities and locations, and information about their death are available for all patients undergoing dialysis, regardless of whether they are covered by Medicare

[22] Under Certificate of Need (CON) laws, entities proposing the new provision of a health service in a market must prove the public need for the said service in the market and obtain a permit from the state. Every state covers different medical services under the CON umbrella and the existence of a dialysis CON in a state is relevant to this analysis since it restricts facility entry for reasons unrelated to mergers. AHPA data on the status of dialysis CON laws are available only for the years 2004, 2008, 2012, and 2016. Except for one state, no state changes its CON law status across these years. For the intervening years, the CON law status is extrapolated to cover the full sample period. If a state is not present in the AHPA data, its CON law status is assumed to be 0.

Fee-For-Service (FFS) or by other insurance. Therefore we can capture patient volume, congestion and mortality for all dialysis patients for the entire study period 1991-2021.

All biomarkers described in Section 3.4 are also available for patients regardless of insurance type. However, these outcomes are derived from CROWNWeb data submitted in accordance with QIP requirements and are consequently available for a facility's patients who are treated on the day of each month when the CROWNWeb measurements are taken. Additionally, these outcomes are only available from 2012 to 2021.

Some outcomes can only be evaluated for Medicare FFS patients since these are derived from Medicare claims. The number of dialysis sessions are determined from Medicare Part B claims available from 2002 to 2021, and therefore represent sessions delivered to Medicare FFS patients. Additionally hospitalizations, ICU days and transfusions are measured using Medicare Part A inpatient claims and are available for Medicare FFS patients for the full study period 1991 to 2021.

Facility entry and closure can be determined for all years from 1991 to 2021. With respect to facility decisions described in Section 3.3, information on staffing is available in the USRDS facility data from 2003 to 2021. Anemia drug utilization outcomes and fistula access are from CROWNWeb data and are available for Medicare and non-Medicare patients, if they were treated on the day of CROWNWeb measurements, from 2012 to 2021.

When the outcome is constructed on a per-patient basis, the relevant patient population is used as the denominator. For example, the mortality rate includes all patients treated at a facility as the denominator, hospitalizations per patient use Medicare FFS patients treated at a facility as the denominator, and the share of patients with hypercalcemia uses patients included in CROWNWeb measurements as the denominator.

In summary, the study population comprises patients with ESKD receiving in-center hemodialysis and, for certain outcomes, the subset covered by Medicare FFS. Patients receiving peritoneal or home dialysis are excluded from all outcomes, and those covered by Medicare Advantage, Medicaid, or private insurance are excluded from analyses that rely

on Medicare claims data. In-center hemodialysis accounts for approximately 85% of all dialysis patients, and roughly one-fourth of these patients are covered by Medicare FFS (NIH 2024). Because mergers are expected to most directly affect in-center dialysis infrastructure and care delivery, and because Medicare policies primarily influence firm revenues through reimbursements for Medicare FFS patients, this population is both analytically appropriate and policy-relevant.

4.3 Descriptive Statistics

Figure 1 shows the count of merger events in the study sample. For outcomes that are available during the entire study period 1991-2021, the earliest mergers we study took place in 1996 and the latest took place in 2018, allowing for five full pre-merger periods and at least four post-merger periods in the estimation of equation 1. Across all merger types, the analysis includes 856 merger events, of which 48% are chain-to-chain mergers, 21% are independent-to-chain mergers and 31% are independent to independent mergers. As consolidation increased over time, the proportion of all mergers that were chain-to-chain rose from 15% in 1996 to 58% in 2018.[23]

Table 1 presents summary statistics for the outcomes and covariates used in the market-level analyses. Treated markets tend to have more facilities than control markets, with the mean facility count rising from 1.79 across all markets to 2.94 within treated markets. They also have slightly more non-merging than merging facilities, averaging 1.93 and 1.01 respectively, and more facilities with chain ownership (1.65) than independent ownership (1.28). Across all markets the mean patient count is 76.1.

Table 2 presents summary statistics for the outcomes used in the facility-level analyses, including staffing, ESA prescriptions, fistula usage, and all health outcomes. The sample dialysis population performs comparably to the broader U.S. population on some health met-

[23]The sample of mergers does not capture the full universe of dialysis firm mergers between 1991 and 2021, as we restrict attention to merger events that are not preceded or followed by other mergers in the same market within a specified time window. See Section 5 for details on these sample restrictions.

rics, such as hypercalcemia, but fares substantially worse on others, including low hemoglobin and hospitalizations. Most notably, the all-cause death rate among dialysis patients is 0.177, or about 17,700 deaths per 100,000, and nearly 20 times the U.S. death rate of roughly 923 deaths per 100,000.[24] Table 3, which reports summary statistics for the covariates used in the facility-level analyses, shows that dialysis patients are disproportionately more likely to be Black and to be aged over 70.[25]

5 Empirical Methods

To estimate the effects of dialysis firm mergers on quality and health outcomes, we use a stacked event-study design implemented through a two-step estimation procedure. This approach enables us to examine the dynamic evolution of merger effects, which is particularly relevant in settings where short- and long-term impacts may differ.

The estimating equation in the first step is equation 1 below.

$$y_{i,e,d} = \sum_{\substack{k=-\mathcal{L}' \\ k\neq -1}}^{k=\mathcal{L}} \beta_e^{unadj} \ (\mathrm{D}_{i,d} \times \mathbb{1}\{e=k\}) \ + \ \mathrm{X'}_{m(i),e}\,\chi \ + \ \mathrm{Q'}_{f(i),e}\,\theta \ + \ \lambda_{i,d} \ + \ \Omega_{e,d} \ + \ \epsilon_{i,e,d} \quad (1)$$

To estimate this equation, we first construct a separate dataset for each sub-experiment d, where d denotes the calendar year in which a merger occurred. For each sub-experiment, we restrict the data to the periods between $\mathcal{L}'$ years before and $\mathcal{L}''$ years after year d, and include units that were treated in year d and units that were not treated in year d. We define event time $e = t - d$ as the difference between the calendar year of observation t and the merger year d. This indicates the time relative to the merger such that $e < 0$ refers to

[24] The mean share of a facility's patients with hypercalcemia in the sample is 2 percent, compared with 1–2 percent in the broader U.S. population as of 2022 (Cleveland Clinic 2022). The share of patients with low hemoglobin in the sample is 19 percent; by comparison, the prevalence of anemia, measured by low hemoglobin, in the broader population between 2021 and 2023 was 9.3 percent (Centers for Disease Control 2024*a*). In the U.S. population, average hospital admissions per person, which we calculate as total hospital admissions (American Hospital Association 2023) divided by the 2023 U.S. population, are about 0.1, whereas the corresponding number in the sample is 1.74. Similarly, the 2023 U.S. death rate is approximately 750 per 100,000 (Centers for Disease Control 2024*b*), while the death rate among dialysis patients in the sample is 17.7 percent (about 17,700 per 100,000), nearly 24 times higher.

[25] In 2023, Black individuals accounted for 14.4 percent of the U.S. population (Pew Research Center 2025), compared with 28 percent in the sample. In 2023, 17.8 percent of U.S. individuals were aged 65 and above (KFF 2025), while even without accounting for the five-year non-overlap the share aged over 70 in the sample is 36 percent.

years before the merger, $e = 0$ is the year of the merger, and $e > 0$ captures years after the merger. Next, we stack these sub-experiment-specific datasets on top of each other, aligning them around the merger year ($e = 0$). Note that some units may appear as controls in multiple sub-experiments. A more detailed description of our stacking procedure is provided in Appendix B.

In some analyses, the unit of observation is a market m , and the stacked event-study estimates outcomes for markets experiencing a merger relative to those without a merger. A market is typically a county, and three large counties (Cook County, IL, Los Angeles County, CA and San Diego County, CA) are further subdivided into markets following Wollmann (2020). In other analyses, the unit of observation is a facility f, and comparisons are made between facilities in markets with a merger and those in markets without a merger. A key feature of the approach in this paper is that all facilities in markets with a merger are considered treated, regardless of whether they were directly acquired. The analysis explicitly compares the trajectories of merging and non-merging facilities within the same market, allowing for the identification of important spillover effects of a merger.

$y_{i,e,d}$ represents the outcome for unit i at event time e in sub-experiment d. The treatment indicator $\mathrm{D}_{i,d}$ equals 1 if unit i is treated in sub-experiment d, and 0 otherwise. This is interacted with a set of event-time binary variables $\mathbb{1}\{e = k\}$ for values $k \in [-\mathcal{L}', \mathcal{L}'']$, excluding $e = -1$ as the reference period. The regression includes unit-by-sub-experiment fixed effects $\lambda_{i,d}$, which control for time-invariant unit characteristics (e.g., location) and event-time-by-sub-experiment fixed effects $\Omega_{e,d}$, which control for shocks that affect all units in a sub-experiment at a given event-time (e.g., national policy changes).

To account for time-varying differences across markets m, we include a vector of market-level covariates $\mathrm{X}_{m(i),e}$ in both the market-level and facility-level analyses. These are median household income, populations in 10-year age groups starting from 0 to 9 years and up to 80 years and older, male population, population in each of the racial groups Hispanic, non-

Hispanic White and non-Hispanic Black and an indicator for whether the market's state had a Certificate of Need law for dialysis facilities.[26,27]

In facility-level analyses, we additionally include facility-level covariates $Q_{f(i),e}$. All facility-level analyses include covariates for facility age, and variables capturing patient demographics, specifically the share of patients who are female, Non-Hispanic White, Non-Hispanic Black, Hispanic, and those in 10-year age groups ranging from 10–19 up to 80–89.

A key additional covariate included in facility-level analyses of biomarkers and clinical endpoints is the patient case-mix. Including this covariate accounts for patient selection, so that the estimated merger effects reflect changes due to the merger itself rather than differences in patient health profiles across facilities. Including this covariate also aligns the measurement of hospitalization and transfusion with the Medicare QIP procedure, which standardizes these outcomes to an expected value based on the facility's case-mix as described in Section 3.5. The challenge in constructing the patient case-mix from patients' observed health outcomes is that patient health profiles are endogenous to the merger. To address this challenge we construct a plausibly exogenous metric of the case-mix in the following way.

In each sub-experiment-specific dataset, before aggregating outcomes at the facility level or stacking data, we regress patients' contemporaneous hospitalizations on age, dialysis vintage (years on dialysis), sex, race, Hispanic origin, and urbanicity using only control-group patients. We then use these relationships to predict hospitalizations for all patients, including for patients in the treated group. The facility case-mix is constructed as predicted hospitalizations per patient, that is total predicted *exogenous* hospitalizations divided by total patients.

[26] Under Certificate of Need (CON) laws, entities proposing the new provision of a health service in a market must prove the public need for the said service in the market and obtain a permit from the state. Every state covers different medical services under the CON umbrella and the existence of a dialysis CON in a state is relevant to this analysis since it restricts facility entry for reasons unrelated to mergers.

[27] Market-level covariates are available at the county-level. Therefore, for almost all markets, the covariates describe the market uniquely but for the three large counties that were further split into smaller markets, covariate values are common within the county in which these markets lie.

The stacked event study approach addresses an important limitation of traditional event study designs in settings with staggered treatment timing, where different units experience treatment (e.g., mergers) in different years. Standard event studies can produce biased or misleading estimates under staggered treatment timing because they impose inappropriate weights, conflate heterogeneous treatment effects, and use contaminated control groups that are either already treated or eventually receive treatment themselves. The stacked event study, which has gained prominence in the recent applied literature (Cengiz et al. 2019, and others), resolves these issues by estimating treatment effects separately for each sub-experiment, aligning treatment events as if they occurred simultaneously across sub-experiments, and comparing treated units only to never-treated or not-yet-treated units during the same relative time window, producing credible estimates of dynamic treatment effects.

The key assumption in the identification of β_e^{unadj} is parallel trends; in the absence of a merger, outcomes for treated and control units within each sub-experiment would have evolved similarly over time. While this assumption cannot be tested directly, its plausibility can be assessed by comparing pre-merger trends for treated and control groups. If trends are similar in this window, it increases confidence in a causal interpretation of the post-merger effects, as any deviation from trends after the merger can be attributed to the merger itself.

In one primary outcome of interest namely the number of total facilities in the market, we find the existence of a pre-trend. Upon estimating equation 1, the coefficients on the dummy variables capturing the pre-merger periods -5 to -4 are negative and precise. This is visible in the top panel of Figure 2. For this outcome, in particular, the pre-trend suggests that mergers took place in markets with positive *growth* in the facility count. The pre-trend likely reflects an unobserved factor correlated with, but not fully captured by, facility growth, as controlling for the pre-merger growth rate does not eliminate it. Recent econometric literature has proposed several methods for addressing pre-trends driven by unobservables. This paper adopts a "linear trend extrapolation" approach described in Freyaldenhoven,

Hansen, and Shapiro (2019) and utilized in prior empirical research (Dobkin et al. 2018; Bhuller et al. 2013).

Following the first-step estimation in equation 1, we estimate a linear time trend using the pre-merger coefficients $\hat{\beta}_{-5}^{unadj}$, ..., $\hat{\beta}_{-1}^{unadj}$, as specified in equation 2.

$$\hat{\beta}_{e}^{unadj} = c \; + \; m\, e \; , \qquad e \in [-5, -1]. \tag{2}$$

Next, we extrapolate the estimated linear time trend to the post-merger periods as in equation 3.

$$\hat{\delta}_{e} = \hat{c} \; + \; \hat{m}\, e \; , \qquad e \in [-5, 5]. \tag{3}$$

We then construct adjusted coefficients using equation 4 as the difference between the prediction from the extrapolation and the unadjusted estimates.

$$\hat{\beta}_{e}^{adj} = \hat{\delta}_{e} \; - \; \hat{\beta}_{e}^{unadj}. \tag{4}$$

The adjusted coefficients $\hat{\beta}_{e}^{adj}$ are, therefore, net of the predicted counterfactual trend. In the main exhibits of this paper, we only present these adjusted estimates of merger effects. We highlight two key aspects of this procedure. First, as equation 4 shows, the predicted counterfactual trend is subtracted from all unadjusted coefficients, including those in the pre-period. Thus the adjusted coefficient associated with the pre-merger reference period -1 is not 0, as is typical in standard event study plots.[28]

Second, the causal interpretation of these adjusted estimates hinges on the validity of the extrapolated linear trend. One way to assess this is visually, by inspecting the linearity of the pre-trend after plotting the unadjusted coefficients, as in the top panel of Figure 2. A complementary approach is to examine the magnitude and precision of the adjusted pre-period coefficients. Magnitudes near zero indicate that the linear fit was appropriate, while narrow confidence intervals reflect a tight spread of the pre-merger coefficients around the fitted line. Both of these properties hold for a vast majority of outcomes. Moreover,

[28] The unadjusted coefficient associated with event-time -1 is 0, visible for example in the top panel of Figure 2.

for outcomes without a pre-merger trend, we still compute the primary estimates using this two-step procedure, trading potential bias in the post-merger coefficients for the additional imprecision introduced by the procedure.

The standard errors associated with the adjusted estimates represented in equation 4 are bootstrapped, and thus estimated non-parametrically.[29] The bootstrap procedure repeats the full two-step procedure multiple times, sampling unit–sub-experiments from the stacked dataset with replacement, and derives standard errors and confidence intervals for each period from the distribution of the resulting bootstrapped adjusted coefficients.

A final note returns to the definition of treatment. For each outcome, the primary estimate of interest captures the market-wide effect of a merger, comparing all units in treated markets with those in control markets. A secondary estimate assesses the distribution of this total effect across merging and non-merging facilities within treated markets. In this secondary analysis, the effect by merging-facility status represents a decomposition of the total effect, in which outcomes for a facility type in a treated market are compared with outcomes for *all* facilities in a control market. Accordingly, we evaluate point estimates relative to the baseline mean (measured at $e = -1$) of the outcome in treated markets when the outcome, such as the number of merging facilities, is undefined in control markets, or relative to the baseline mean across all markets when the outcome is well-defined everywhere.

6 Results

6.1 Facility Closures and Entry

Figure 2 presents the coefficients for the market-level effects on total facilities from equation 1 in the top panel and the coefficients from equation 4 in the bottom panel. The top panel provides a visual representation of the existing pre-trend, the line of best-fit through the

[29] Equation 1 clusters standard errors at the unit-sub-experiment level (e.g., facility-sub-experiment level in the facility-level analyses). These are the standard errors associated with unadjusted coefficients that are not presented in any exhibits except the top panel of Figure 2.

estimated pre-merger coefficients and its extrapolation to the post-merger periods. Consequently, it is straightforward to inspect that the adjusted coefficients displayed in the bottom panel are simply the unadjusted post-merger coefficients evaluated relative to the extrapolated line.[30] We find that mergers lead to an initial increase in the total number of facilities in a market, followed by a decline in subsequent years.

Next, we decompose the effect of mergers on the market-level facility count by the type of facility exposed to the merger. The top panel of Figure 3 shows the effect of mergers on the count of facilities in a market that went through the merger, finding that this quantity declines persistently in the post-merger period. By five years following the year of the merger, 0.4 fewer merging facilities remain in the market which is a 24% reduction relative to the baseline mean.

The bottom panel of Figure 3 shows a contrasting response among facilities that are exposed to a merger in the market, but are not themselves acquired. Mergers lead to an increase in non-merging facilities, which begins to manifest as early as the merger year, rises to nearly 0.2 additional facilities, and plateaus after three years.

Overall, these results demonstrate that mergers lead to closures of merging facilities and entry of non-merging facilities, resulting in a an overall null response in the market.[31] This result is novel both within the broader merger literature and in dialysis narrowly, and it carries key economic and methodological implications. Economically, it suggests that mergers may generate indirect effects extending beyond the entities directly involved. In the context of dialysis, mergers could influence patient access and outcomes across the broader market in heterogeneous ways. Methodologically, it indicates that non-merging entities in merger-affected markets may be unsuitable controls in causal analyses.[32]

[30] For all other outcomes, we focus the discussion of the analysis only on the final adjusted coefficients.

[31] The post-merger coefficients from the top and bottom panels of Figure A.1 sum to values that are close to, but not exactly equal to, the post-merger coefficients in the bottom panel of Figure 3. This discrepancy arises from the inclusion of market-level controls, which alter the partial correlation between each outcome and the treatment in distinct ways, as well as the additional step of adjusting coefficients relative to a fitted line, which is estimated separately for each outcome.

[32] Data limitations only allow for the distinction between a merging facility and other facilities, but preclude the assignment of granular firm identity to these facilities. Therefore it cannot be determined whether the entry of non-merging facilities is by the acquiring firm, which could open new facilities in addition to acquiring the target in the same market, or by other

A number of features relating to the evidence on facility closures and entry merits further discussion. First, are facility closures and entries driven by specific types of mergers? Figure 14 shows that mergers trigger closures of merging facilities and entry of non-merging facilities across three distinct merger types, each of which might be expected to have qualitatively different effects. This indicates the generality of our finding. Second, given barriers to entry, is the compensatory expansion by non-merging firms plausible? Startup costs to set up new dialysis clinics can be exorbitant, and entrants must obtain a Certificate of Need prior to opening a facility. Yet, the supply of dialysis facilities has been growing historically (Velázquez et al. 2022). Using anecdotal estimates of the startup costs of dialysis facilities and projected annual profits (Arnold, Carrie 2020), our back-of-the-envelope calculation suggests a conservative break-even horizon of six years.[33] Additionally, Appendix Figure A.1 shows that the entry of non-merging facilities is primarily driven by chain operators, who likely face lower expansion costs due to economies of scale, rather than by independent firms in the market. The entry patterns we observe suggest that profit opportunities are sufficiently large to incentivize new market participation, particularly by chain operators.

Third, why does non-merging facility entry, observed as early as the merger year, precede the exit of merging facilities? Several explanations are plausible. The first is data-related; because facility status is recorded only annually, a merging facility that closes earlier in a year and a non-merging facility that opens later in that same year will both appear as active in the data, making entry seem to precede exit even when the closure occurs first. A second explanation is behavioral. Non-merging firms may respond preemptively, anticipating quality declines or closures at merging facilities and positioning themselves to capture resulting competitive gains, possibly informed by prior market experiences.

non-merging firms. Further, it cannot be determined whether the entry of non-merging facilities is by new firm entrants or by incumbent firms.

[33] Arnold, Carrie (2020) reports that startup costs for a new facility are approximately $3 million. Annual revenues are also roughly $3 million, with an estimated net profit margin of 18%, implying an annual profit of slightly over $500,000. Distributing the full startup costs evenly over time, a facility would require approximately six years to cover initial costs and begin generating positive net earnings.

6.2 Patient Volume and Congestion

A natural question that arises from the analysis of facility closures is how mergers affect patient displacement. The top panel of Figure 4, presenting the effect of a merger on the market's patient volume, shows that the patient volume in the market remains unaffected by the merger. This evidence supports the view that patients largely remain local. Under the assumption that mergers do not affect the underlying incidence of kidney disease, there is no net patient entry or exit from the market. The middle and bottom panels show that mirroring facility closures, the patient volume at merging facilities declines, and similarly in accordance with facility entry, the patient volume at non-merging facilities increases. Appendix Figure A.2 shows that the decline in patient volume at merging facilities is driven primarily by an outflow of Medicare patients, which is mirrored by a corresponding increase at non-merging facilities, while patient volumes for other insurance types remain largely stable across both facility types.

Patient displacement may impact dialysis treatment through two distinct channels. First, displaced patients may encounter frictions in treatment receipt during the adjustment period. Second, all patients may be affected by equilibrium shifts at incumbent facilities, which, as a result of the merger, may operate under altered capacity constraints from absorbing or losing patients. To probe the second channel, we investigate whether mergers affect station congestion, measured as the ratio of patients to stations, finding in Figure 5 that mergers increase congestion steadily over the post-merger period. Congestion at merging facilities remains broadly stable, with a short-term dip in the merger year, consistent with the interpretation that some patients leave merging facilities prior to closure. The market-level increase in congestion is driven by non-merging facilities which see an increase of approximately 5% relative to the baseline mean of 3.64 patients per station.

6.3 Dialysis Sessions

Figure 6 presents the facility-level effects of mergers on the annual dialysis sessions per patient. It shows that mergers result in a decline of nearly 5 sessions per patient in all facilities in the year of the merger, with no effects on sessions per patient in the following year. This merger-year effect manifests at merging and non-merging facilities, with about 7 fewer sessions per patient at merging facilities and 3 fewer sessions per patient at non-merging facilities.

First, we provide an interpretation of the magnitude of this effect which corresponds to a reduction of one session every 2.5 months. Given that patients receive approximately 12 sessions a month, is this effect clinically meaningful? One clinical study suggests that skipping 1 or more dialysis sessions in a month was associated with a 25% higher risk of death (Leggat et al. 1998). In this study, we find that mergers lead to a 4% increase in mortality in the year of the merger.[34] This suggests, indirectly, that a reduction of approximately one session every 2.5 months is associated with a 4% increase in mortality. When evaluated alongside the evidence in Leggat et al. (1998), this pattern is consistent with missed sessions being distributed across the calendar months, as the observed increase in mortality is smaller than what would be expected if the 25% estimate applied linearly to non-consecutive months.[35] The observed reduction in treatment is, therefore, both statistically and clinically meaningful.

Next, we offer a speculative discussion of the timing and potential mechanisms driving the effect on dialysis sessions. Reductions in treatment occur only in the merger year, affecting both merging and non-merging facilities, albeit with different magnitudes. In merging facilities, operational disruptions during ownership transition such as slowed operations, renovations, or resource reallocation in anticipation of closure may temporarily reduce patient care. At non-merging facilities, smaller reductions may reflect transient congestion. The

[34] See Figure 12.

[35] If risk scaled linearly with treatment frequency, a risk of 25% for at least one missed session per month translates to a risk of 25/2.5 or 10% for at least one missed session per 2.5 months.

concentration of treatment declines in the merger year may reflect these transitional disruptions, with care stabilizing in subsequent years as facilities adjust and operations normalize, despite ongoing post-merger entry and exit.

6.4 Facility Decisions

The next set of results describes effects of mergers on facility decisions. First, we examine whether mergers affect facility staffing, namely the sum of nurses, patient care technicians and social workers. The top panel of Figure 7 shows that mergers result in a reduction in staff in the short-term, with approximately 0.3 fewer personnel in the year of, and the year following the merger. The timing of the response to staffing mirrors the timing of the decline in treatment, consistent with short-term operational disruptions.[36] Evidence in Appendix Figure A.4 suggests that mergers lead to market-level declines in employment with total dialysis staffing declining by 1 person, or 10% of the baseline mean.

Second, we consider whether mergers affect anemia drug utilization. The middle panel of Figure 7 shows that mergers result in a reduction in the share of patients with an erythropoietin stimulating agent (ESA) prescription. Appendix Figure A.5 reflects another dimension of merger-induced changes in ESA prescription practices, and shows a reduction in the average monthly dosage of ESAs. Our results, which show an unambiguous decrease in ESA prescriptions, contrast with the findings in Eliason et al. (2020) who find that that ESA dosage increases when independent firms are acquired by chains. However, we note that the study sample in Eliason et al. (2020) does not directly overlap with ours. Our analysis encompasses a much broader set of mergers, and their study examines mergers that occurred before the 2011 Medicare bundled payment reform. The timing of the reform is especially relevant for interpretation. Prior to 2011, dialysis facilities were reimbursed on a fee-for-service basis for ESA doses, whereas after the reform, ESAs were incorporated into the

[36] Appendix Figure A.3 shows, that reductions appear among nurses and technicians, with no impact on the employment of social workers.

larger dialysis bundle, fundamentally flipping the profit incentives for ESA prescriptions.[37] Our finding suggests that mergers may have enhanced firms' ability to extract per-treatment profits from reducing ESA use. Effective ESA dosing is crucial for anemia management, but neither high nor low doses alone reveal whether the dosing is appropriate. To assess its impact, we examine the need for blood transfusions, which we discuss in Section 6.6.

Finally, the bottom panel of Figure 7 suggests that mergers do not have any meaningful effects on the share of patients utilizing fistulas for vascular access.

6.5 Biomarkers

Figure 8 shows the effect of mergers on biomarkers. The figure reflects the analysis of outcomes measured starting in 2012 when Medicare began collecting detailed biomarker data. Panel (a) shows that the share of a facility's patients with recommended dialysis adequacy ($\geq$ 1.2 mg/dL) increases after a merger, though the effect is noisily measured. Panel (b) shows the effect of mergers on the share of patients with hypercalcemia (>10.2 mg/dL), finding that mergers significantly reduce this share. Appendix Figure A.6 shows that this is effect is not coupled with increases in hypocalcemia (<8.5 mg/dL), suggesting an unambiguous improvement in calcium levels among dialysis patients.

Next we consider patients' ultrafiltration rate (UFR), where both very high and very low UFRs are considered to affect patient health negatively. Panel (c) in Figure 8 shows that mergers lead to a sustained increase in the UFR in years following the merger. The magnitude of this increase reaches at most 0.3, which, when evaluated in relation to the baseline mean, would result in a UFR of at most 8 mL/kg/hr. This level remains below the thresholds associated with adverse endpoints in clinical literature (Flythe, Kimmel, and Brunelli 2011; Kim et al. 2018), yet represents an adverse outcome following a merger.

Probing further into non-QIP biomarkers, panel (d) reports merger effects on patients' hemoglobin levels. Mergers show no clear impact on the share of patients with hemoglobin

[37] See the report "How the Bundle Has Changed Clinical Care at Fresenius Kidney Care" (Fresenius Medical Care N.d.) from a large dialysis chain that provides anecdotal evidence for the relevance of this mechanism.

below 12 g/dL. With respect to albumin, panel (e) shows that by three years post-merger the mean level declines enough relative to the baseline mean of 3.66 g/dL to fall just below the recommended minimum of 3.5 g/dL. For iron saturation, the results in panel (f) indicate a decline from the baseline mean of 30.75% to about 25.75%. This level remains within the normal range for the average dialysis population (National Kidney Foundation 2025).[38]

Taken together, the evidence in Figure 8 indicates merger-induced improvements in biomarkers explicitly incentivized under the QIP, such as Kt/V and hypercalcemia. In contrast, mergers appear to have little effect, or even adverse effects, on biomarkers not included in the facility quality metrics. This implies that merger-induced quality gains reflect targeted QIP compliance rather than broader clinical improvements, and that expanding QIP measures could enhance merger-related effects.

6.6 Clinical Endpoints

In this section, we discuss the effects of mergers on clinical endpoints during the full 30-year horizon between 1991 and 2021. Figure 9 shows that, across facilities, hospitalizations decline modestly in the post-merger period, although the estimates are imprecise. The magnitude of the effect is similar and merging and non-merging facilities, even though precision of estimates is greater for the effect estimated at non-merging facilities.

To assess whether mergers affect the severity of hospitalizations, we examine their impact on ICU days associated with a hospitalization. Mergers result in reductions in ICU days per patient, both overall and at merging and non-merging facilities, as shown in Figure 10. We also investigate whether mergers influence inpatient blood transfusion events.[39] Figure 11 shows that mergers reduce transfusions per patient, with the decline concentrated at non-merging facilities. Our evidence suggests that the reductions in ESA prescriptions that we

[38] A 25.75% iron saturation falls below recommended standards for EPO-resistant patient populations (Fishbane et al. 1996). The reductions in iron saturation shown in panel (f) align with the declines in facilities' ESA dosages reported in Figure 7. While facilities often offset ESA reductions with increases in intravenous iron to manage anemia, the evidence on iron saturation suggests that such compensatory adjustments are not occurring here.

[39] We emphasize that our outcome is transfusion events, and not units of blood transfused.

showed in Figure 7 do not compromise health, as they are not linked to increases in severe, transfusion-requiring anemia.

Finally, we examine the effects of mergers on mortality. Figure 12 shows that mergers cause a sharp increase in the mortality rate in the year of the merger by about 0.7 percentage points and 4% of the baseline mean of a 17% mortality rate. For ease of interpretation, this represents an increase in the death rate among the dialysis population by 700 per 100,000 patients, which was just the full-population U.S. death rate in 2023 (Centers for Disease Control 2024*b*). The mortality effect in the merger year appears both at merging facilities where the magnitude is higher at a 1 percentage point increase and and non-merging facilities where the magnitude is lower at about a 0.3 percentage point increase.[40] By 1 year after the merger, the mortality effect subsides and stabilizes around 0.

Our short-term mortality effects mirror the immediate disruptions in treatment documented in Section 6.3, suggesting that market-level operational disruptions following a merger increase mortality. In contrast, analyses of hospitalization and related inpatient outcomes, as well those of some biomarkers, indicate long-run health gains from mergers, which, importantly, occur at both merging and non-merging firms. A plausible interpretation is that mergers elevate short-term mortality among high-risk patients, while survivors of this initial churn experience health improvements. The evidence further suggests that surviving firms gain operational efficiencies, with non-merging firms potentially benefiting from the larger scale resulting from patients shifting from closing merging facilities. Figure 13 decomposes mortality by cause, revealing long-run increases in cardiovascular and endocrine mortality following mergers, whereas infection-related mortality declines. This pattern indicates that long-run efficiencies may improve outcomes for some causes of death, though not for all-cause mortality.

[40] The preferred mortality effect estimate is the all-facility effect, rather than the estimates by facility type, since deaths are especially prone to attributional uncertainty when patients switch facilities.

6.7 The Role of Medicare's Quality Incentive Program

In 2011, Medicare implemented two major policy changes. First, the Quality Incentive Program (QIP) was introduced, requiring facilities to report biomarkers and other quality measures on a monthly basis, with penalties of up to 2% in reduced reimbursements for failure to meet standards. Second, Medicare rolled out bundled payments, providing a fixed payment per dialysis treatment episode rather than fee-for-service reimbursement. While some of our findings on facility-level decisions may reflect the effects of bundled payments, an important question is whether QIP amplifies the adverse health impacts of mergers or enhances their positive effects.

To evaluate this, we estimate merger effects on clinical endpoints separately for the periods 1991–2011 and 2012–2021. The presentation of results mirrors the event-study patterns presented earlier. We then compute the difference between estimates for the two policy periods and conduct a t-test to assess significance. Table 4 shows the differential effects of mergers before and after QIP implementation on hospitalization, ICU days, and transfusions. We find that QIP appears to eliminate improvements in these outcomes, with the effect reaching statistical significance for ICU days.

Table 5 shows that splitting the sample reduces the precision of merger-year mortality estimates; however, the effects remain positive both before and after QIP. Notably, post-QIP, mortality increases persist for a year following the merger, rendering the effect statistically higher than the corresponding pre-QIP estimate. Overall, QIP neither mitigates the adverse health effects of mergers, measured by all-cause mortality, nor amplifies their positive effects on hospitalization, ICU days, and transfusions.[41]

[41] Table 6, suggests that QIP appears to have a protective effect against mortality from endocrine conditions, with post-QIP mortality from these causes consistently lower than before.

6.8 Heterogeneous Effects of Mergers

While the analyses presented thus far shed light on the overall effects of mergers in dialysis, they may mask important heterogeneity relevant for more targeted antitrust enforcement or Medicare quality oversight. Next, we undertake the analysis of key outcomes separately for three merger subtypes.[42] The first subtype is mergers in which the target and acquirer are both chains (chain-to-chain). The second subtype is mergers in which the target is independent but the acquirer is a chain (independent-to-chain).[43] The third subtype is one in which both entities are independent firms (independent-to-independent).

Figure 14 presents heterogeneity in merger effects on facility closures and entry. Merger-induced closures are widespread, but the degree of offsetting entry by non-merging facilities varies substantially by merger type. Chain-to-chain mergers lead to net reductions in the number of facilities within a market, independent-to-chain mergers have no net effect, and independent-to-independent mergers yield a modest net increase that stabilizes within five years after the merger. Overall, the evidence suggests that independent-to-independent mergers are most conducive to new facility entry, while chain-to-chain mergers are least so.

Next, we examine heterogeneity in merger effects on clinical endpoints. Figures 15 and 16 show that long-term gains in hospitalizations, ICU days, and transfusion events are driven primarily by chain-to-chain and independent-to-chain consolidations. In contrast, independent-to-independent mergers are associated with increased ICU utilization, negligible changes in hospitalizations, and no discernible effects on transfusions. Notably, independent-to-independent mergers exhibit a pronounced long-term decline in all-cause mortality, a pattern that was obscured in the aggregated all-merger sample. Disaggregating by merger type reduces the precision of short-term mortality estimates in the merger year, precluding definitive conclusions about which mergers drive disruption-related mortality spikes.

[42] The analysis of mergers by type involves the estimation of equation 4 where the treated units are from markets experiencing a merger of the reference type, and the control units are from markets experiencing no merger.

[43] This is the merger type under consideration in (Eliason et al. 2020).

Our heterogeneity analysis also allows us to assess whether changes in market concentration may underlie the observed merger effects on health. We measure market concentration using facility-level patient shares in each year. This approach has limited direct antitrust relevance, as facility-level shares do not necessarily map onto firm-level shares. However, given incomplete information on firm identities, an issue shared with prior research in the dialysis sector (Eliason et al. 2020; Wollmann 2020), this remains the most feasible approach. Figure A.7 presents estimates from equation 4 showing the effect of mergers on market concentration.

We find that market concentration increases following mergers, both in the aggregate and across merger subtypes. The magnitude of this increase is broadly similar across merger types, although the rise in concentration is smallest when two independent facilities merge. Estimates are most precise for chain-to-chain mergers and less so for independent-to-chain and independent-to-independent mergers, suggesting greater heterogeneity across markets in the latter cases.

Taken together with the evidence on clinical endpoints, these findings suggest that changes in market competition are unlikely to be the primary mechanism driving health effects. While all merger types increase market concentration, the long-term effects of mergers vary substantially. This pattern implies that efficiency-enhancing mechanisms, rather than shifts in competitive pressure, may account for the observed health gains. These findings are in line with prior studies in the dialysis industry (Eliason et al. 2020; Cutler, Dafny, and Ody N.d.), which also report no systematic link between market concentration and patient outcomes.

7 Implications for Policy

7.1 Medicare

The discussion in Section 6.7 showed that Medicare's Quality Incentive Program (QIP) neither amplified the health benefits nor mitigated the health harms of dialysis mergers.[44] We now return to the effects of mergers on biomarkers, as a key policy question is whether the QIP targets the right biomarkers in its quality assessments, or, equivalently, whether its selected biomarkers are valid predictors of clinical outcomes.

In the post-QIP period, mergers reduce hypercalcemia and, correspondingly, mortality from endocrine and metabolic diseases. They also modestly improve Kt/V, alongside a reduction in mortality from genitourinary diseases. In contrast, mergers do not affect fistula use, which influences infection risk, and accordingly have little impact on infection-related mortality. Taken together, these patterns suggest moderate coherence between QIP-targeted biomarkers and downstream disease risks, implying that QIP's chosen measures capture some, but not all, clinically meaningful pathways. A natural policy implication is to expand QIP's scope to include additional biomarkers that more strongly predict other leading causes of death, thereby enhancing its potential to reduce long-term mortality.

Notably, mergers lower serum albumin levels and raise ultrafiltration rates (UFR) (Figure 8), both strong predictors of cardiovascular mortality (Manolis et al. 2022; Flythe, Kimmel, and Brunelli 2011). Yet, UFR remains only a reporting measure under QIP.[45] Elevating UFR from a reporting requirement to a performance-based quality metric could help mitigate merger-related increases in cardiovascular risk, as UFR is directly modifiable through clinical practice. By contrast, although albumin is also a powerful predictor of mortality, nephrologists generally regard it as largely beyond their direct control. Nonetheless, our

[44] We emphasize that our analysis cannot speak to the overall effectiveness of the QIP itself, but only to whether it altered the health effects of mergers.

[45] In QIP, a reporting measure earns a satisfactory score simply by being reported, regardless of its value.

results indicate potential clinical gains from closer monitoring of patients who experience declines in albumin following a merger, as these declines appear to coincide with increased cardiovascular risk.

7.2 Health Costs and Benefits

To evaluate the overall health effects of consolation, we weigh the *health-related* benefits of mergers, reflected in long-run reductions in hospitalizations, against their costs, measured by short-run increases in mortality, within five years of the merger. We emphasize that this exercise is intentionally limited to patient health outcomes and does not constitute a full welfare analysis. Other dimensions of patient welfare, such as prices, costs, firm profits, patients' travel distance and other metrics of access, are not incorporated directly, though they may influence health outcomes and thus are partially reflected in the health effects we estimate. The goal is to answer a narrower, evidence-based question: on balance, do mergers improve or worsen patient health over the span of 5 years following the merger? Traditional antitrust evaluations which typically focus on merger's effects on measures of competition, determined through market concentration, prices, and revenues, may tend to overlook or insufficiently capture the health consequences of consolidation; our contribution is to bring that dimension into view. Table 7 presents the estimates from our analysis, and a description is provided below.

We evaluate the health-related costs and benefits of mergers over a 5-year period. This horizon provides a natural endpoint for both, the event-study time frame and for our calculation of costs and benefits. In the context of ESKD, patient survival is often measured over 5 years from onset of kidney failure or from the initiation of dialysis, at which point survival typically declines by half (NIH 2024). For the purpose our analysis, this implies, conceptually, that for an average patient who begins dialysis just before a merger, the most significant health effects are likely to occur within a 5-year period.

We begin by estimating benefits using the long-run decline in hospitalizations. Specifically, we use the cumulative of market-wide coefficients over the 5-year post-merger horizon from the top panel of Figure 9, which shows that mergers reduce hospitalizations by 0.248 per patient over 5 years. To translate this effect into monetary terms, we use Medicare Part A spending data from the Kaiser Family Foundation (KFF). From KFF (Kaiser Family Foundation 2021*a*), we obtain the 2021 figures for total traditional Medicare Part A enrollees (35,337,335) and program payments per enrollee ($5,292), yielding aggregate Part A payments of $187,216 million. Because our hospitalization outcome data reflect Medicare Part A claims for traditional Medicare, these populations are directly comparable. We use 2021 data to reflect the most up-to-date value of benefits, although our regression estimates reflect effects of mergers spanning 1991–2021.

Program payments per enrollee do not directly capture spending per hospitalization, since each enrollee may experience multiple hospitalizations. To obtain this measure, we draw on a separate KFF report (Kaiser Family Foundation 2021*b*) that provides the total number of Part A discharges. Dividing aggregate payments by total discharges yields an estimated spending of $25,433 per hospitalization. Importantly, both the KFF and our own estimates are calculated over the full population of enrollees, including those with no hospitalizations, ensuring conceptual consistency.

Next, we estimate the aggregate reduction in hospitalizations. We begin with the number of unique merger-exposed markets in our 30-year sample (724), noting that roughly 105 of these involve repeated merger events. These 724 markets represent about 23% of all U.S. counties (724 of 3,143) (U.S. Census Bureau 2021). Applying this share to the 2021 total of in-center hemodialysis patients (474,468) (NIH 2024) implies that approximately 109,295 patients were plausibly exposed to mergers.[46]

Multiplying the per-patient reduction in hospitalizations (0.248) by the number of exposed patients (109,295) and the spending per hospitalization ($25,433) gives an aggregate

[46] We derive a similar number of merger-exposed patients from our data by aggregating all patients in exposed markets during the merger year across all events in our sample.

spending reduction of $689 million, which we interpret as the monetary value of the health benefits of mergers.

To estimate costs, we follow an approach similar to Clemens and Mahajan (2025), who value life years in the context of policy-driven mortality changes. The short-term increase in mortality is taken from event time 0 in Figure 12, which indicates a 0.7 percentage point rise in mortality following mergers. Applied to the merger-affected population, this implies roughly 765 additional deaths.

We convert deaths into years of life lost (YLL) under two distinct assumptions about remaining life expectancy. The first assumes that individuals in our sample would have the same remaining life expectancy as the general U.S. population. Based on the CDC's 2020 life tables (National Vital Statistics Reports 2022), individuals aged 68–69, the average age at death in our data (68.4 years), are expected to live an additional 16.3 years. The second assumption accounts for the possibility that individuals with ESKD experience lower survival probabilities than the average U.S. persons of the same age. Using 2021 estimates from the USRDS (NIH 2023), life expectancy for dialysis patients aged 65–69 is 4.3 years. Multiplying the estimated number of merger-related deaths by the YLLs implies total losses of 12,471 life-years under the assumption with higher life expectancies and 3,290 life-years under the assumption with lower life expectancies.

We then assign a monetary value to each YLL using the value of a statistical life year (VSLY). To do this, we divide the value of a statistical life (VSL) provided by U.S. Department of Health and Human Services (2021) by the average U.S. life expectancy to obtain a low, central, and high VSLY estimates of $130,000, $278,000, and $423,000, respectively (corresponding to VSLs of $5.3 million, $11.4 million, and $17.4 million). Multiplying these by the aggregate YLLs yields total costs ranging from $428 million (low VSL, low life expectancy) to $5.3 billion (high VSL, high life expectancy). Our preferred estimate—using the central VSL and the general-population life expectancy—implies nearly $3.5 billion in costs. For context, this is roughly one-twelfth of total Medicare spending on dialysis patients.

Under alternative assumptions regarding YLL and the VSL, the lower bound of costs is $428 million and the upper bound approaches $5.3 billion.

Comparing these figures, the mortality-related costs exceed the $689 million in hospitalization-related benefits across nearly all cost estimates, except under the most conservative assumption for the value of a YLL. It is worth noting that our hospitalization spending estimates are based on Medicare fee-for-service data, whereas the exposure estimates encompass all dialysis patients. Accordingly, an adjustment to the valuation of benefits to reflect private insurance reimbursement rates may be warranted. Private insurers paid, on average, a little more than twice the Medicare rate for comparable services in 2020 (Whaley et al. 2022).[47] Inflating all hospitalization benefits by a factor of two would therefore provide an upper bound on potential benefits—since in practice, only a minority of dialysis patients are covered by private insurance. Even so, this adjustment would raise aggregate benefits to roughly $1.378 billion, which remain below mortality-related costs for all but two scenarios—those using the low life expectancy and the low or central VSLY assumptions. Hence, even under reasonable adjustments for non-Medicare spending, the evidence consistently indicates that merger-related health costs outweigh their benefits, absent efforts to mitigate short-term mortality effects.

7.3 Antitrust Regulation

The body of our results yields several implications for antitrust regulation. First, the cost–benefit analysis discussed in Section 7.2 indicates that, under the current policy environment, mergers are not justified based on their health costs. However, observed mortality increases appear transitory, arising primarily during the merger year rather than reflecting lasting declines in care quality. Regulatory interventions that specifically target post-merger disruptions could likely mitigate these short-term mortality effects, potentially tipping the

[47] Whaley et al. (2022) compare private insurance and Medicare prices for individual services within a claim and therefore do not directly report prices per hospitalization. We make the simplifying assumption that the ratio of service prices extends to overall hospitalization costs—an assumption that would hold if privately insured and Medicare patients receive comparable bundles of services during hospitalizations for similar health events.

cost–benefit balance in favor of positive net effects from consolidation. This suggests a relatively simple policy solution that focuses on reducing treatment disruptions, rather than relying solely on preventing mergers or facility closures.

While a detailed evaluation of specific policy solutions is beyond the scope of this paper, existing antitrust and CMS tools offer several feasible approaches. For example, pre-merger notification could require disclosure of the capacity and geographic distribution of nearby non-merging facilities, helping ensure patients retain reasonable access if a facility closes. Approval could be tied to enforceable post-merger conditions—such as reporting treatment access data for all pre-merger patients, establishing transition teams to reassign patients, or coordinating with nearby facilities—implemented through consent decrees. Patient outcomes could then be monitored using CMS claims data, with reimbursement penalties linked to merger-year outcomes and ongoing Medicare approval contingent on compliance. Such a framework illustrates how antitrust enforcement can be combined with payment oversight to protect patient outcomes beyond traditional structural remedies.

Second, Section 6.8 highlights substantial heterogeneity in the effects of dialysis mergers on facility entry and health outcomes. Chain-to-chain and independent-to-chain mergers produce long-term reductions in hospitalizations, ICU days, and transfusions, whereas independent-to-independent mergers appear to raise hospitalization and ICU days, but generate notable long-term decreases in all-cause mortality. Breaking down effects by merger type reduces the precision of short-term mortality estimates, making it difficult to identify which mergers contribute to temporary mortality spikes during the merger year. Moreover, consistent with Wollmann (2020), who documents welfare gains from reviewing smaller mergers typically overlooked by regulators, we find that independent-to-independent consolidations that may fall below reporting thresholds can still increase concentration and can produce a range of both beneficial and adverse effects that warrant attention.

A third takeaway, like the second, highlights that antitrust evaluations may need to consider factors beyond simple measures of market concentration. We document overall long-run

health benefits of dialysis mergers that have received limited emphasis in both the academic literature and broader policy discourse. These benefits occur alongside facility closures—which may coincide with efficiency gains—and increases in market concentration, reinforcing the value of considering long-run patient outcomes in conjunction with conventional measures of market competition in antitrust evaluations.

8 Conclusion

In this paper, we examine the effects of mergers among dialysis firms on patient health outcomes. We find that mergers lead to the closure of merging facilities and the entry of non-merging facilities into exposed markets, resulting in patient displacement and greater congestion at remaining stations. In the merger year, treatment volume falls sharply across the market, with patients missing about five sessions annually. Facility-level mortality rises by roughly 700 deaths per 100,000 patients, consistent with treatment disruptions.

Over the longer term, however, patient health outcomes improve. By five years post-merger, patients experience fewer hospitalizations, ICU days, and blood transfusions, consistent with the exit of lower-quality firms, the entry of higher-quality firms and efficiency gains among surviving providers. A cost–benefit assessment of mergers' health effects suggests that, under a range of assumptions, costs from merger-year mortality increases outweigh the gains from subsequent declines in hospitalization. At the same time, the results point to a tractable policy response—antitrust remedies that mitigate short-run treatment disruptions could reduce mortality while preserving longer-run health improvements, generating net positive health effects of mergers.

While long-run effects on several inpatient health outcomes are unambiguously positive, analyses of biomarkers measured contemporaneously with treatment at dialysis facilities yield more mixed evidence. Some laboratory measures—particularly those used in Medicare's Quality Incentive Program (QIP) to assess facility performance—improve, whereas

others, especially markers associated with cardiovascular risk, deteriorate. Consistent with this pattern, cardiovascular mortality rises but is offset by reductions in mortality from other causes, leaving overall long-run mortality unchanged. Taken together, the mixed biomarker responses alongside improvements in hospitalization outcomes limit our ability to draw precise conclusions about the predictive validity of these biomarkers.

Two sets of supplementary analyses provide additional policy-relevant insights for the regulation of quality and merger activity in dialysis markets. First, we compare the health effects of mergers before and after the implementation of a pair of Medicare policy changes that tied reimbursement to quality performance and moved facilities from fee-for-service reimbursement to a bundled, per-treatment payment system. We find little evidence of differential merger effects across most outcomes. Although this analysis does not identify the direct effects of these policies on health, it suggests that they neither mitigated the adverse short-run consequences of mergers nor amplified their longer-run benefits.

Second, we find little evidence that the health effects of mergers are systematically related to changes in market concentration. Estimates by merger subtype reveal substantial heterogeneity in health impacts alongside broadly similar increases in concentration. This pattern suggests that reliance on concentration measures alone may be insufficient for evaluating dialysis mergers and highlights the importance of accounting for longer-run health effects in antitrust enforcement decisions.

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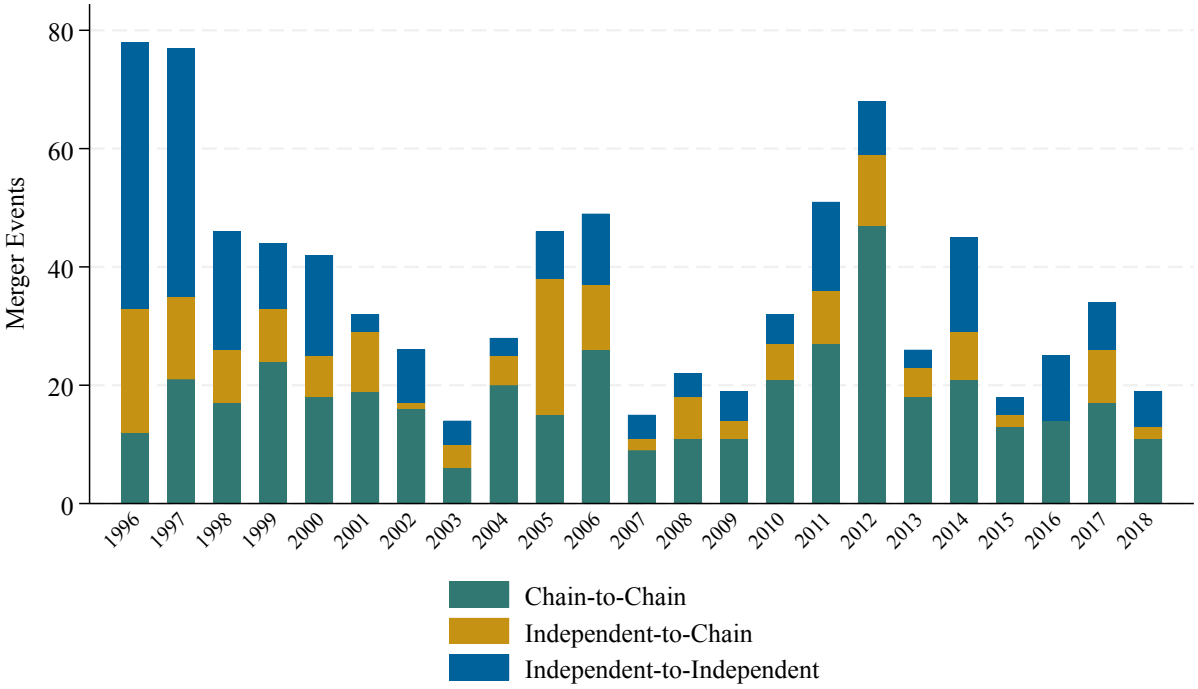
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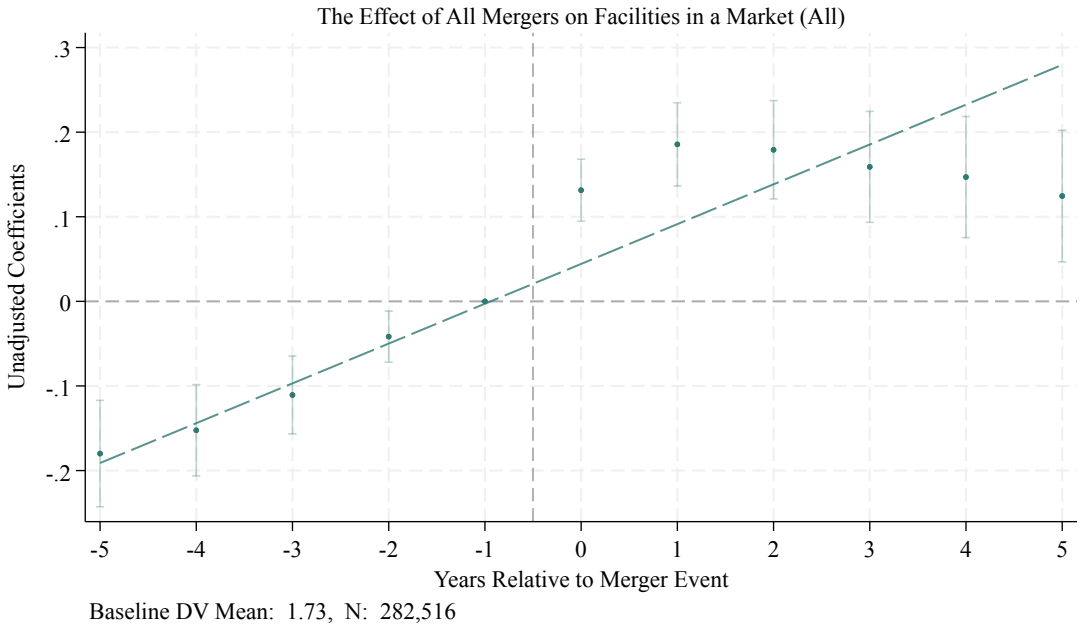
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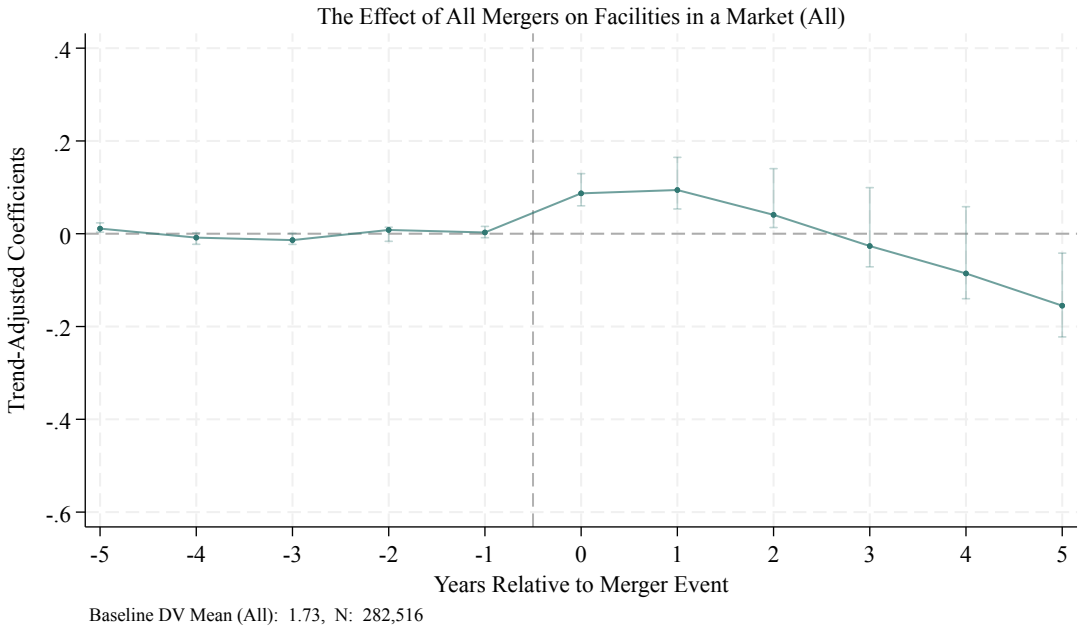
Figure 1: Number of Merger Events in the Study Sample



Notes: This figure displays the merger events considered in the analysis of the net and heterogeneous effects of mergers. The sample of mergers results from applying the market-exclusion criteria described in Section 5 and Appendix B to the set of all mergers.

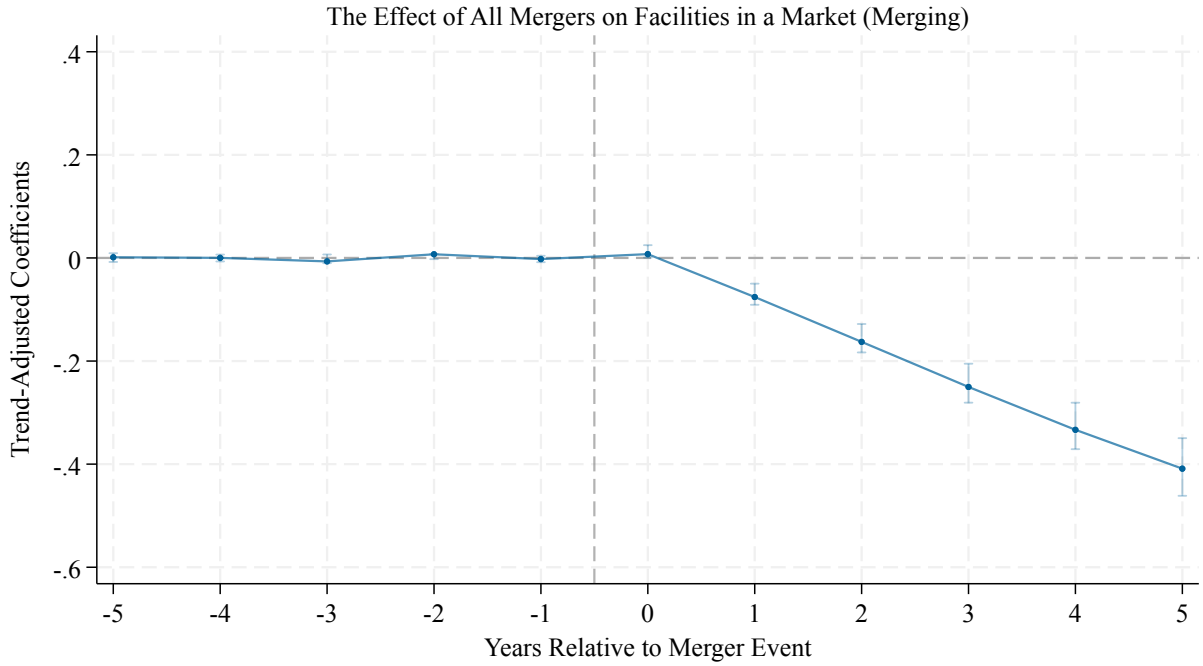
Figure 2: The Effect of All Mergers on the Count of Facilities in a Market



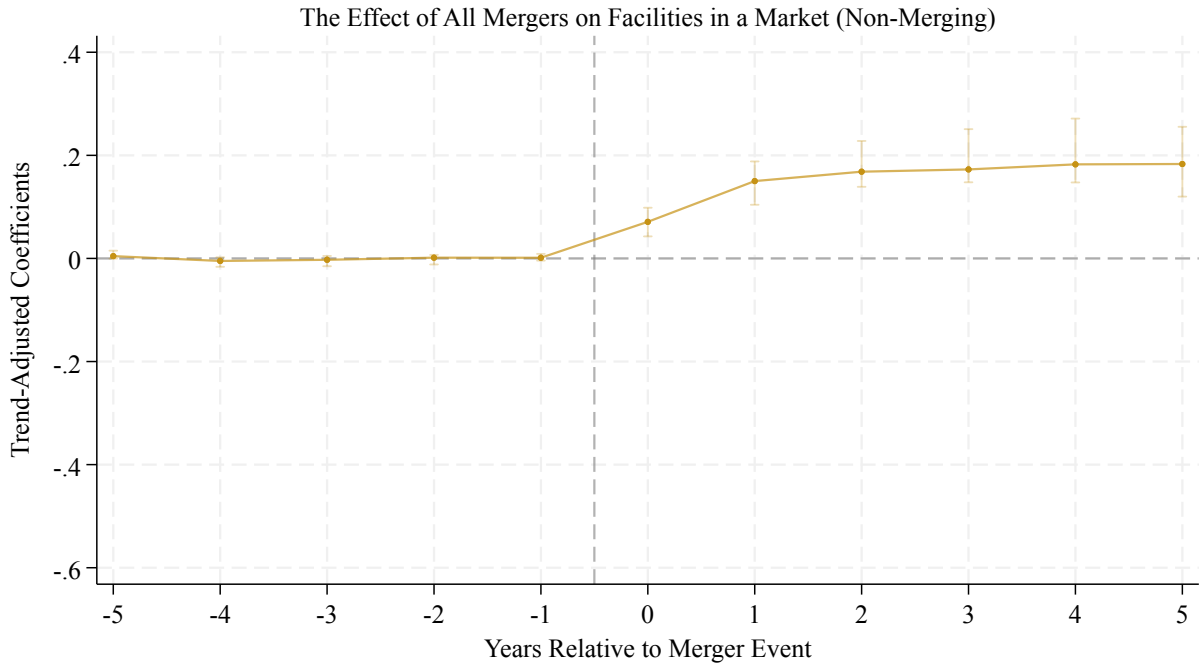


Notes: The top panel in this figure presents unadjusted coefficients from equation 1 estimated at the market-level, representing the effect of a merger on the number of all facilities in the market. The green dashed line depicts the line of best fit through the coefficients associated with event times -5 to -1, extrapolated to all future event times. 95% confidence intervals around the estimates are shown and are calculated using standard errors clustered at the market-sub-experiment level. The bottom panel in this figure presents trend-adjusted coefficients from equation 4 representing the effect of a merger on the number of all facilities in the market. The vertical dashed line represents the merger event. 95% confidence intervals around the estimates are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in both panels include mergers taking place between 1996 and 2018, and incorporate market-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all (treated and control) markets.

Figure 3: The Effect of All Mergers on Merging and Non-Merging Facilities



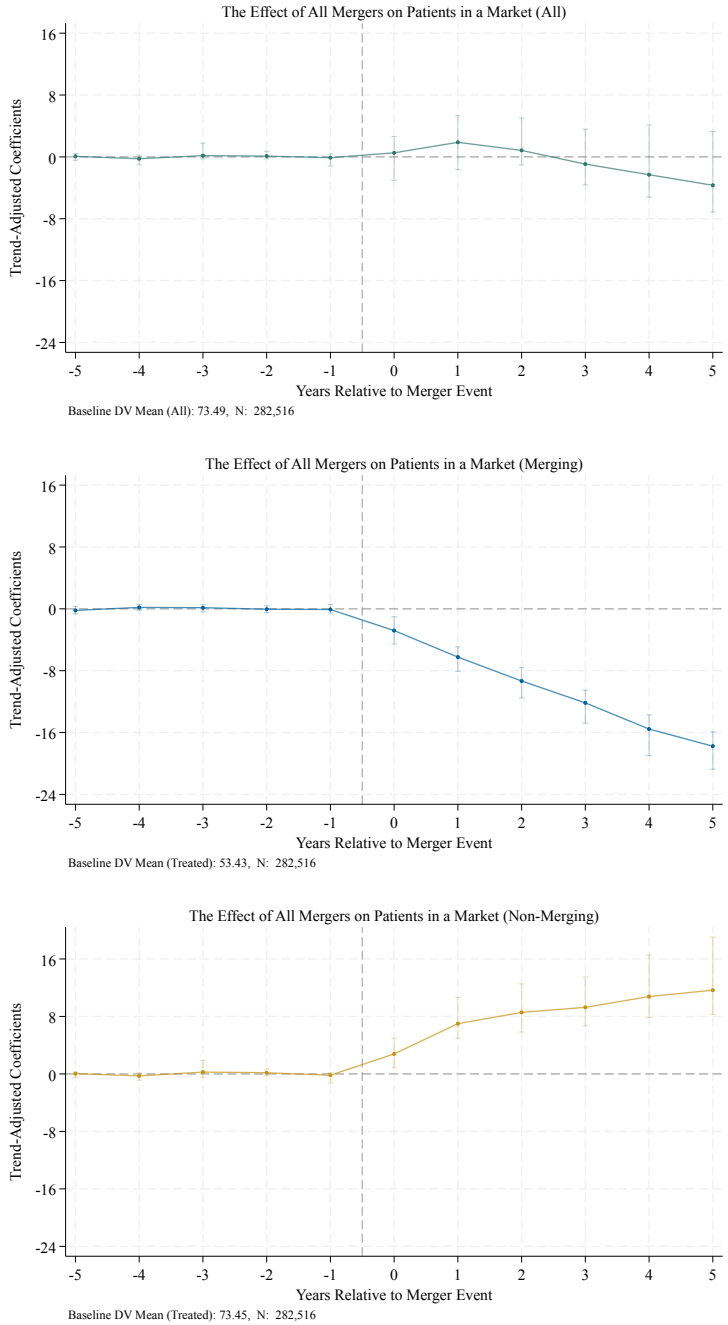
Baseline DV Mean (Treated): 1.03, N: 282,516



Baseline DV Mean (Treated): 1.77, N: 282,516

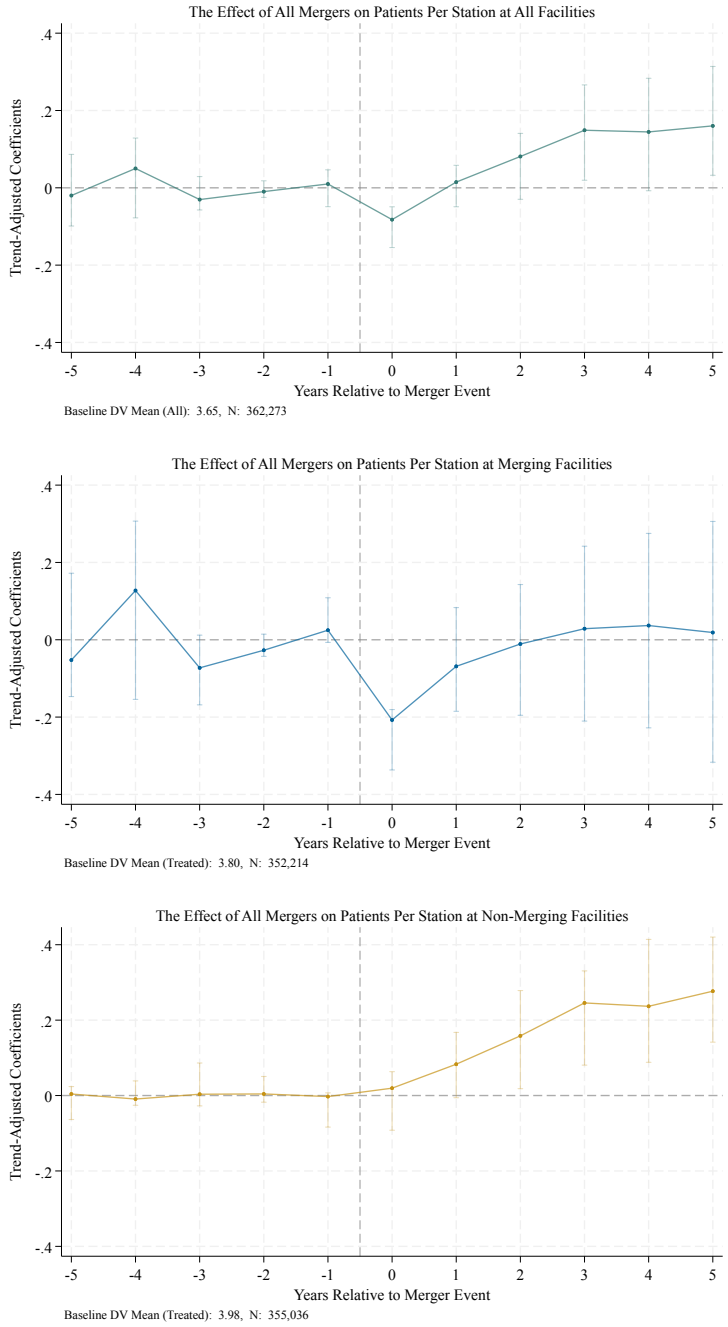
Notes: The top and bottom panels in this figure present coefficients from equation 4 estimated at the market-level, representing the effect of a merger on the number of merging facilities and non-merging facilities in the market, respectively. The vertical dashed line represents the merger event. Mergers taking place between 1996 and 2018 are included. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in both panels include market-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across treated markets.

Figure 4: The Effect of All Mergers on Patient Volume



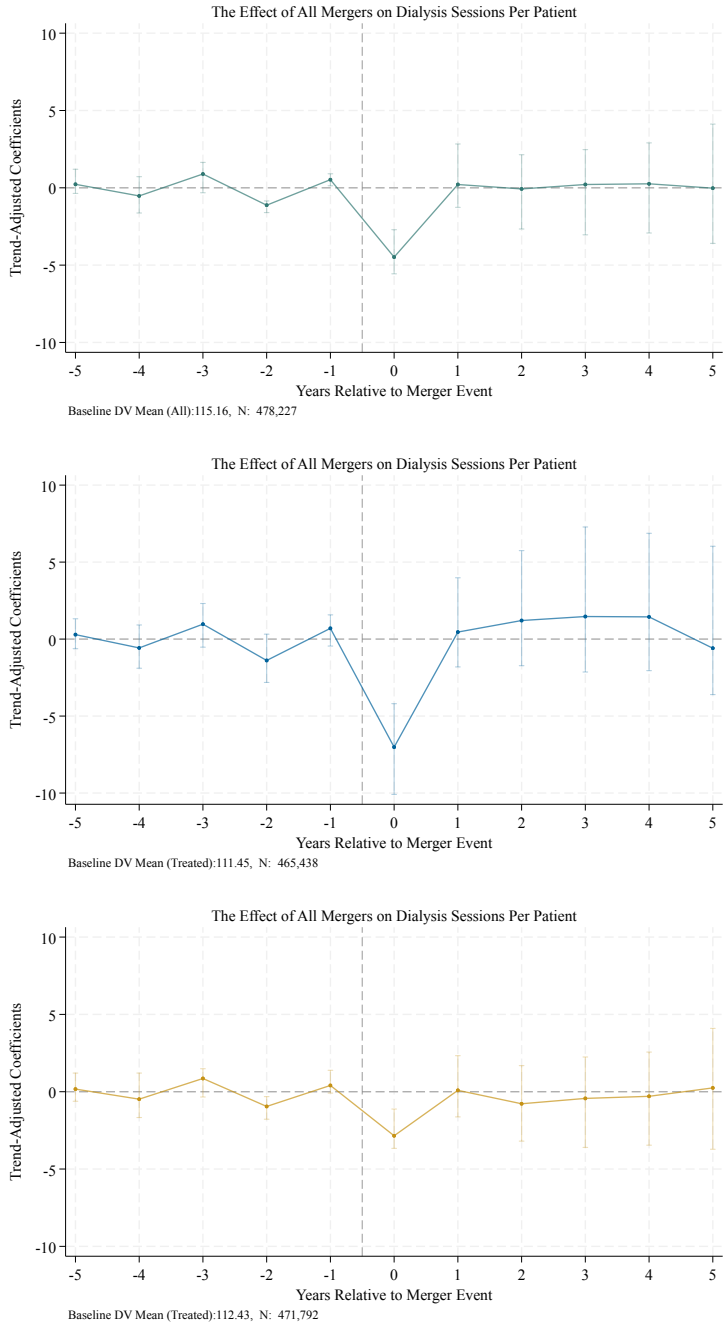
Notes: This figure presents coefficients from equation 4 estimated at the market-level, representing the effect of a merger on the number of patients in the market. The vertical dashed line represents the merger event. The outcome represents all patients, regardless of insurance type. Mergers taking place between 1996 and 2018 are included. The top panel shows effects for all facilities, the middle for merging facilities, and the bottom for non-merging facilities. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all markets (treated and control) in the top panel, and across treated markets in the middle and bottom panels.

Figure 5: The Effect of All Mergers on Congestion (Patients Per Station)



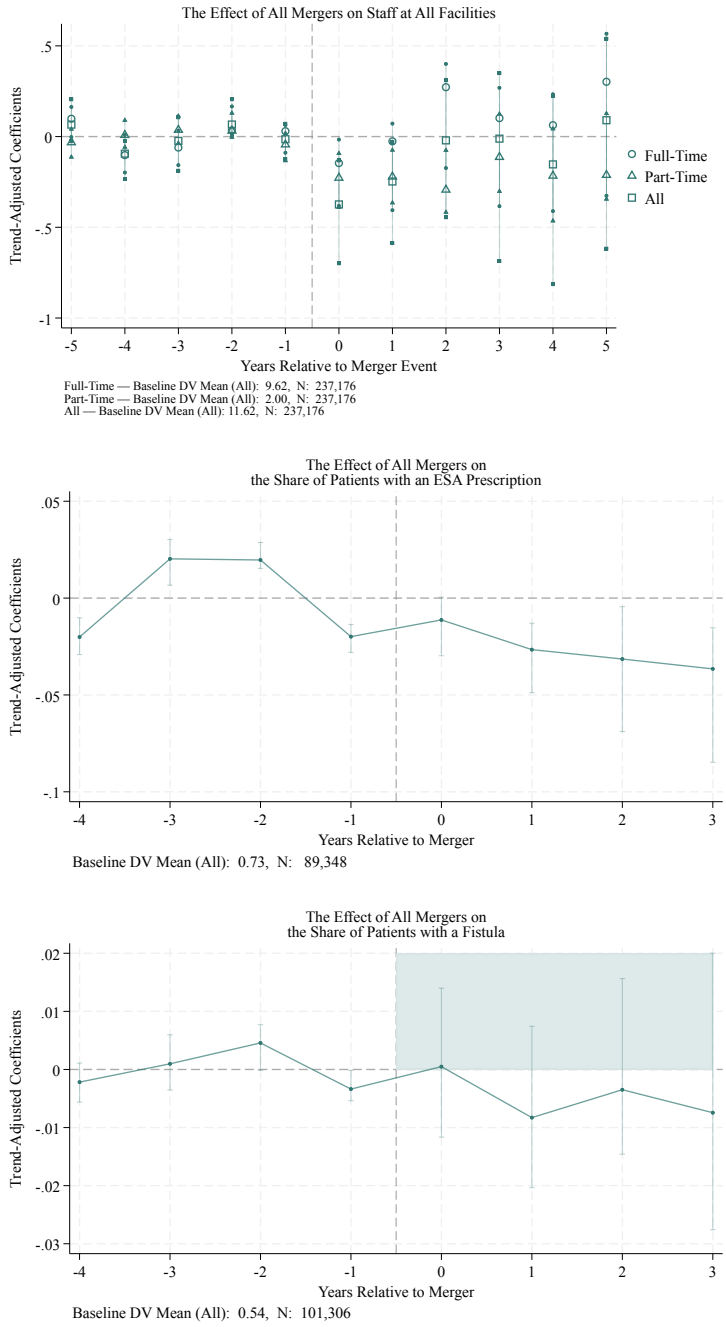
Notes: This figure presents coefficients from equation 4 estimated at the facility-level, representing the effect of a merger on congestion, measured by the number of patients per station, in the facility. The vertical dashed line represents the merger event. The outcome represents all patients, regardless of insurance type. Mergers taking place between 1996 and 2018 are included. The top panel shows effects for all facilities, the middle for merging facilities, and the bottom for non-merging facilities. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all markets (treated and control) in the top panel, and across treated markets in the middle and bottom panels.

Figure 6: The Effect of All Mergers on Treatment (Dialysis Sessions Per Patient)



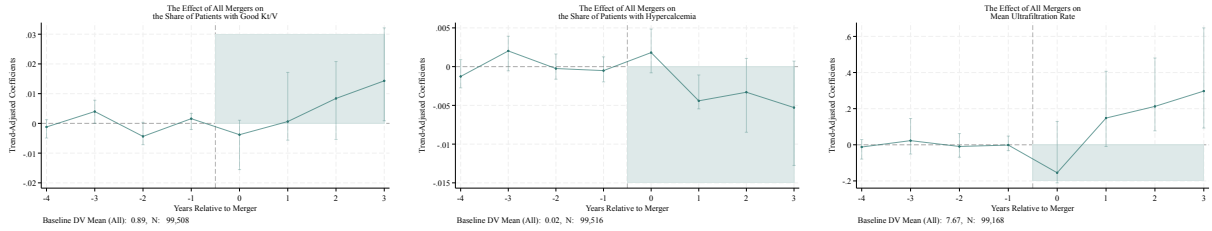
Notes: This figure presents coefficients from equation 4 estimated at the facility-level, representing the effect of a merger on the number of annual dialysis sessions per patient in the facility. The vertical dashed line represents the merger event. The outcome represents Medicare Fee-For-Service patients. Mergers taking place between 2006 and 2018 are included. The top panel shows effects for all facilities, the middle for merging facilities, and the bottom for non-merging facilities. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level and facility-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all markets (treated and control) in the top panel, and across treated markets in the middle and bottom panels.

Figure 7: The Effect of All Mergers on Facility Decisions

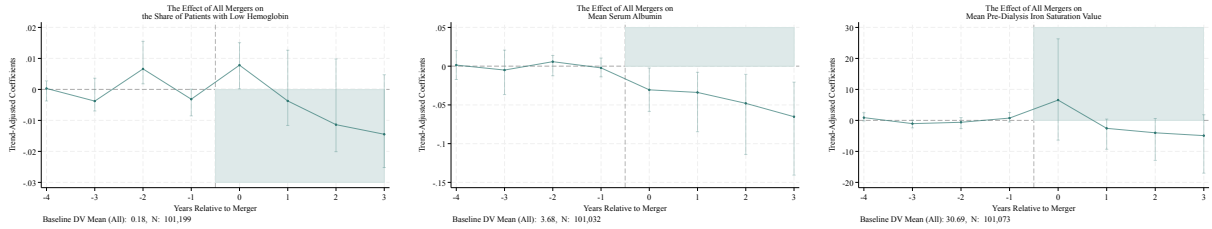


Notes: This figure presents coefficients from equation 4 estimated at the facility-level, representing the effect of a merger on facility decisions at all facilities. The vertical dashed line represents the merger event. The outcome in the top panel is staff comprising nurses, technicians, and social workers. Mergers taking place between 2008 and 2018 are included for the analysis in the top panel. The outcome in the middle panel is the share of patients in a facility with a prescription for an ESA (a drug to manage anemia). The outcome in the bottom panel is the share of patients in a facility utilizing a fistula for vascular access. Green shading indicates the direction of clinically beneficial merger effects. In the middle and bottom panels, mergers taking place between 2015 and 2018 are included. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. The regression specifications used to produce the coefficients in the top panel include market-level controls, while those in the middle and bottom panels include both market- and facility-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all (treated and control) markets.

Figure 8: The Effect of All Mergers on Biomarkers



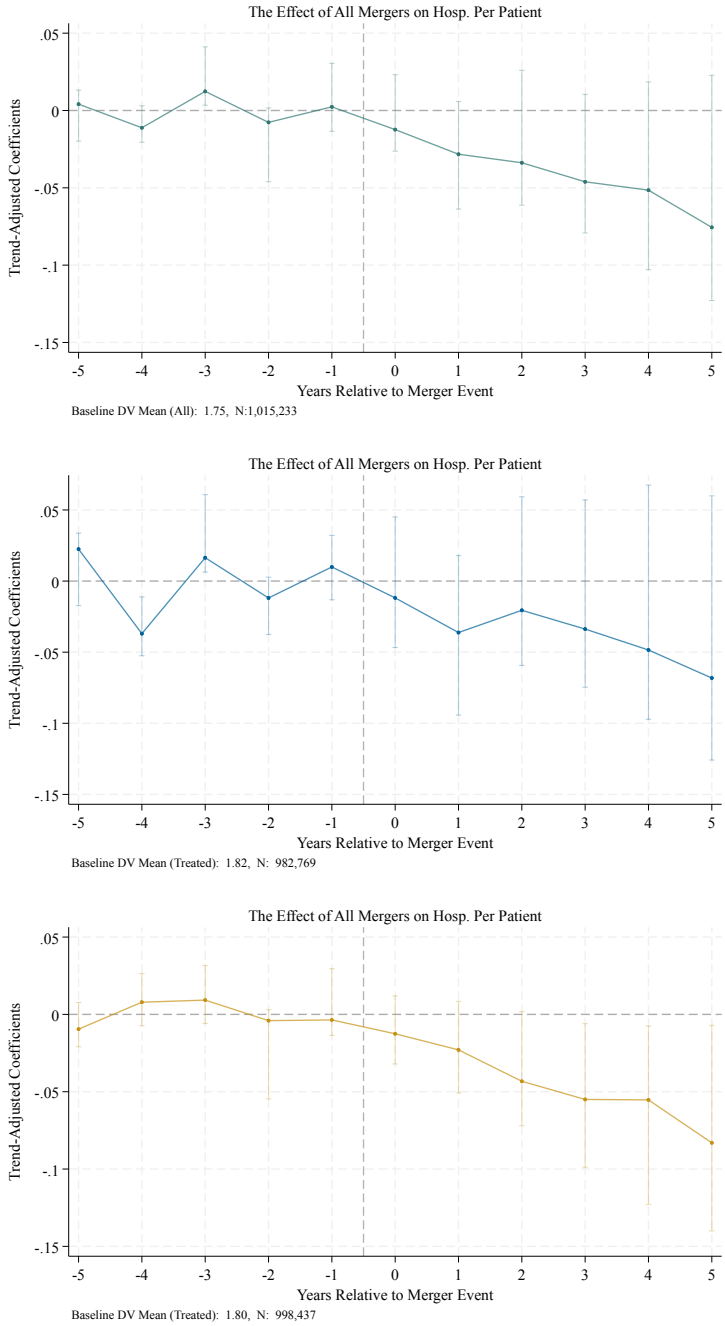
(a) Good Kt/V ($\geq$1.2 mg/dL) **(b)** Hypercalcemia ($\geq$ 10.2 mg/dL) **(c)** Ultrafiltration Rate (mL/kg/hr)



(d) Low Hemoglobin (<10 g/dL) **(e)** Albumin (g/dL) **(f)** Iron Saturation (%)

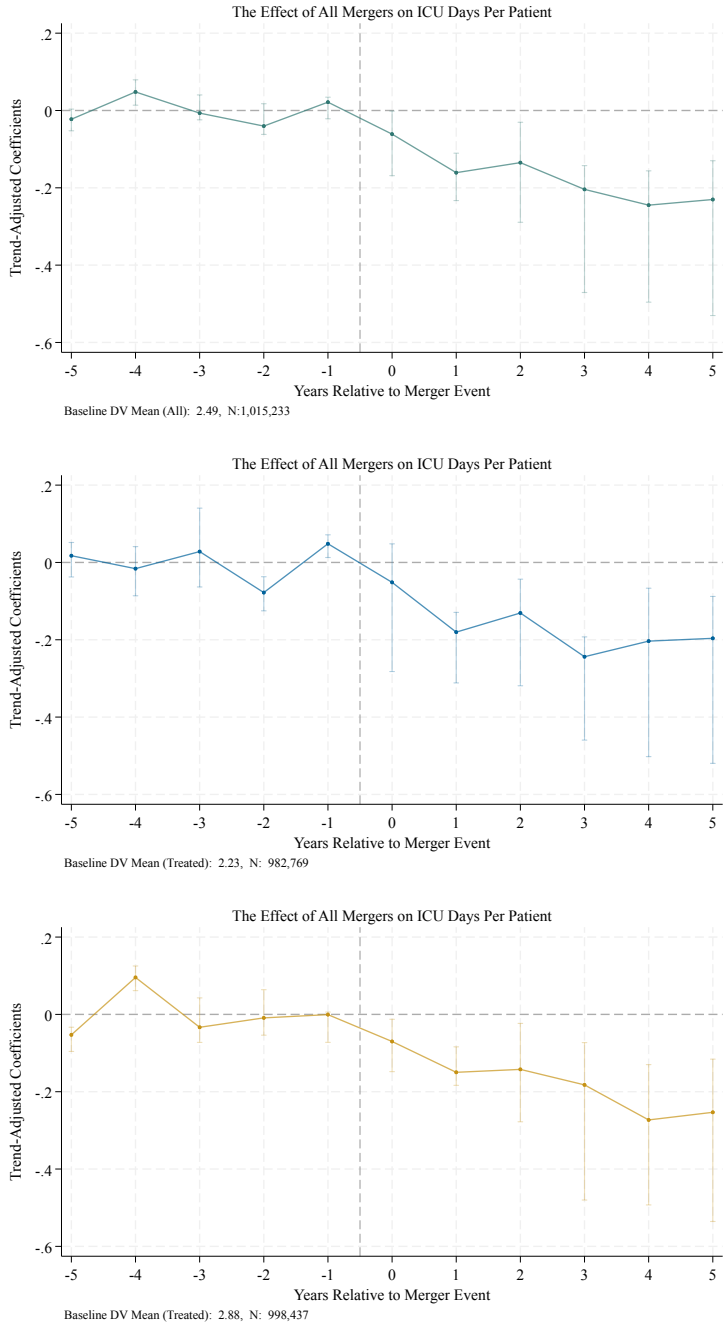
Notes: This figure presents coefficients from equation 4 estimated at the facility-level, representing the effect of a merger biomarkers. The vertical dashed line represents the merger event. The outcome in panel (a) is the share of patients with good Kt/V ($\geq$ 1.2 mg/dL). The outcome in panel (b) is the share of patients with hypercalcemia defined as serum calcium levels exceeded 10.2 mg/dL. The outcome in panel (c) is the mean ultrafiltration rate (mL/kg/hr). The outcome in panel (d) is the share of patients with low hemoglobin (<10g/dL). The outcome in panel (e) is the mean serum albumin (g/dL). The outcome in panel (f) is the mean iron saturation (%). Green shading indicates the direction of clinically beneficial merger effects. The outcome represents all patients, regardless of insurance type, included in CROWNWeb reports. Mergers taking place between 2015 and 2018 are included. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level and facility-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all (treated and control) markets).

Figure 9: The Effect of All Mergers on Hospitalizations Per Patient (Facility-Level)



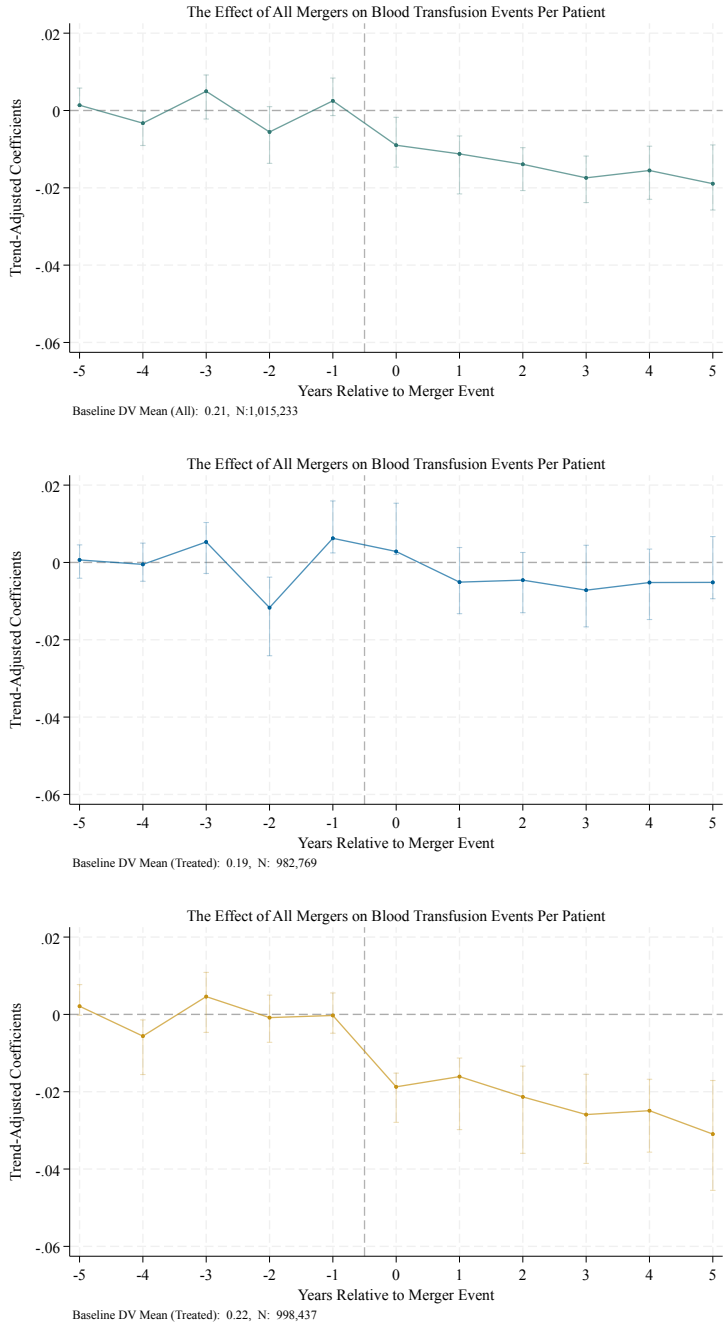
Notes: This figure presents coefficients from equation 4 estimated at the facility-level, representing the effect of a merger on hospitalizations per patient at the facility. The vertical dashed line represents the merger event. The outcome represents Medicare Fee-For-Service patients. Mergers taking place between 1996 and 2018 are included. The top panel shows effects for all facilities, the middle for merging facilities, and the bottom for non-merging facilities. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level and facility-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all markets (treated and control) in the top panel, and across treated markets in the middle and bottom panels.

Figure 10: The Effect of All Mergers on ICU Days Per Patient (Facility-Level)



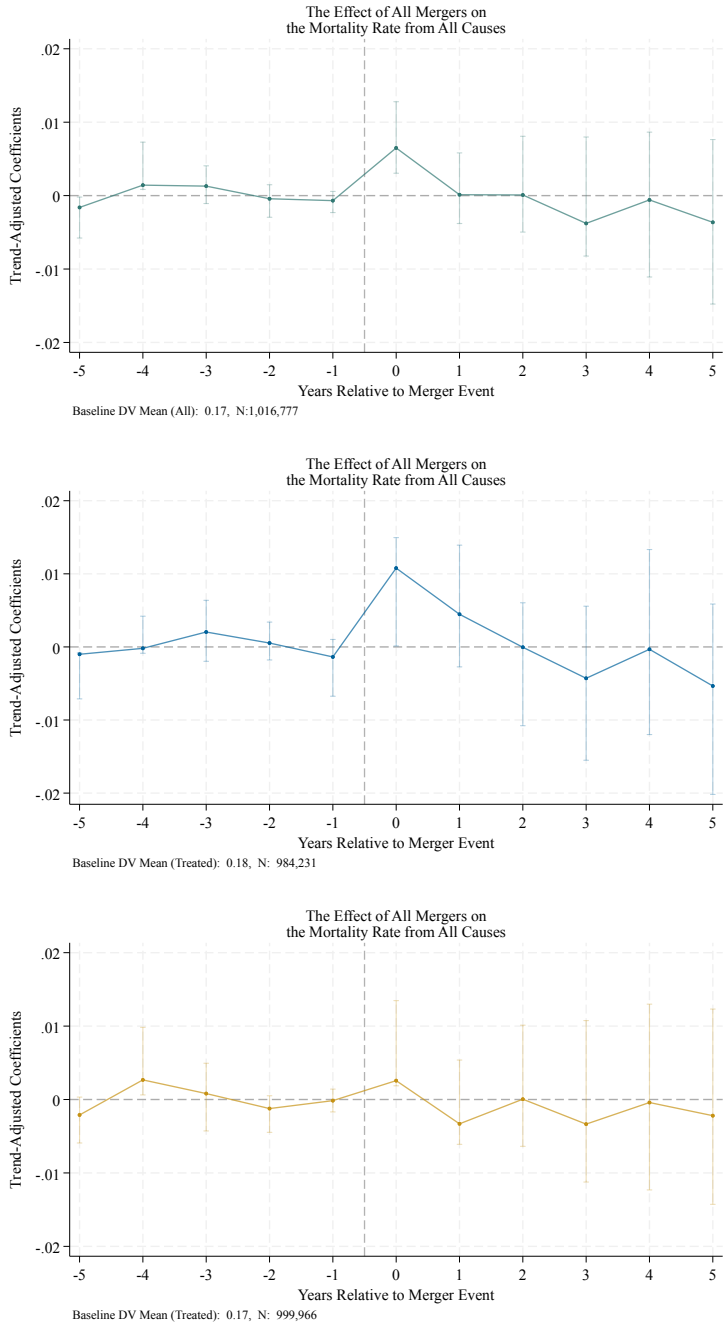
Notes: This figure presents coefficients from equation 4 estimated at the facility-level, representing the effect of a merger on ICU days per patient at the facility. The vertical dashed line represents the merger event. The outcome represents Medicare Fee-For-Service patients. Mergers taking place between 1996 and 2018 are included. The top panel shows effects for all facilities, the middle for merging facilities, and the bottom for non-merging facilities. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level and facility-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all markets (treated and control) in the top panel, and across treated markets in the middle and bottom panels.

Figure 11: The Effect of All Mergers on Blood Transfusions Per Patient (Facility-Level)



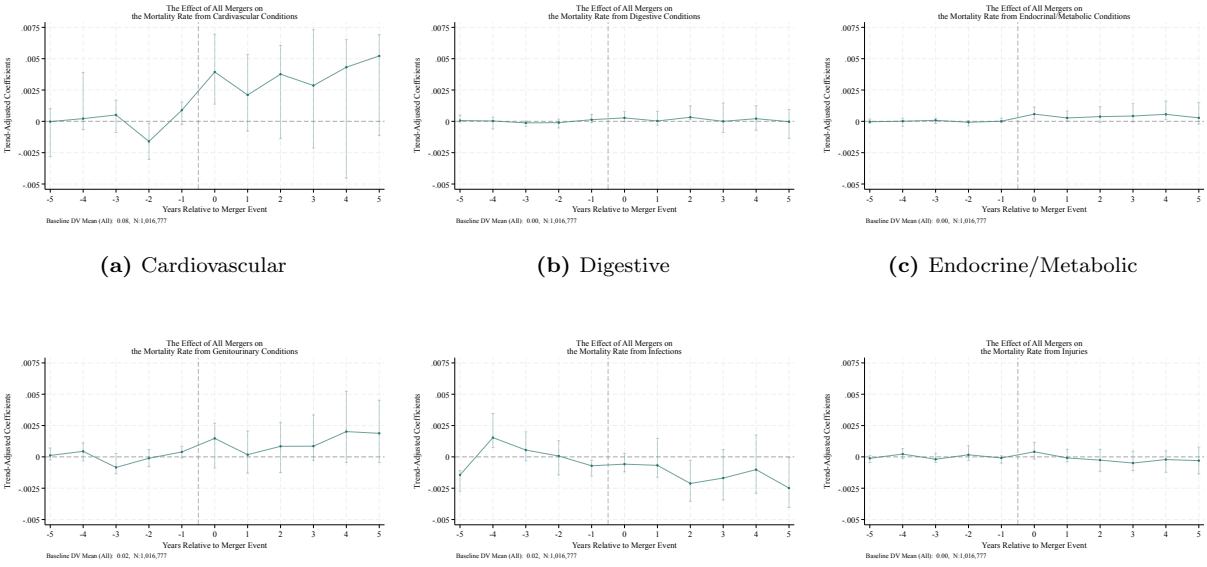
Notes: This figure presents coefficients from equation 4 estimated at the facility-level, representing the effect of a merger on blood transfusion events per patient at the facility. The vertical dashed line represents the merger event. The outcome represents Medicare Fee-For-Service patients. Mergers taking place between 1996 and 2018 are included. The top panel shows effects for all facilities, the middle for merging facilities, and the bottom for non-merging facilities. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level and facility-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all markets (treated and control) in the top panel, and across treated markets in the middle and bottom panels.

Figure 12: The Effect of All Mergers on Mortality (Facility-Level)



Notes: This figure presents coefficients from equation 4 estimated at the facility-level, representing the effect of a merger on all-cause deaths per patient. The vertical dashed line represents the merger event. The outcome represents all patients, regardless of insurance type. Mergers taking place between 1996 and 2018 are included. The top panel shows effects for all facilities, the middle for merging facilities, and the bottom for non-merging facilities. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level and facility-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all markets (treated and control) in the top panel, and across treated markets in the middle and bottom panels.

Figure 13: The Effect of All Mergers on Mortality by Cause (Facility-Level)

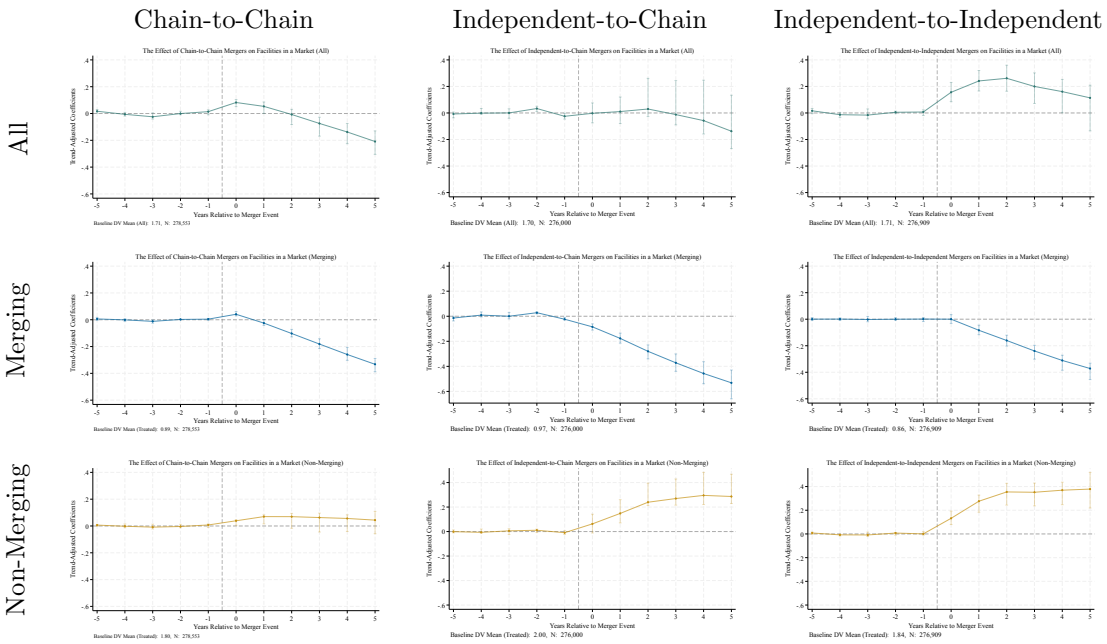


(a) Cardiovascular **(b)** Digestive **(c)** Endocrine/Metabolic

(d) Genitourinary **(e)** Infections **(f)** Injuries

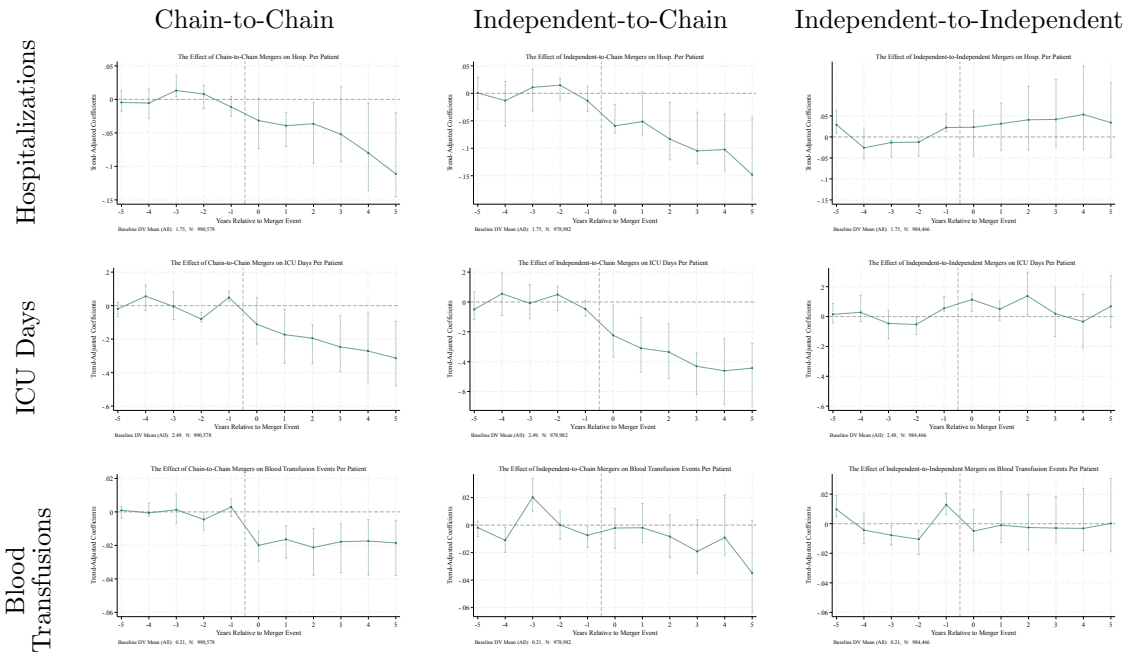
Notes: This figure presents coefficients from equation 4 estimated at the facility-level, representing the effect of a merger on mortality by cause per patient. The vertical dashed line represents the merger event. The outcome represents all patients, regardless of insurance type. Mergers taking place between 1996 and 2018 are included. Each panel presents the effect on a different cause of mortality. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level and facility-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all (treated and control) markets.

Figure 14: Heterogeneity in the Effect of Mergers on Facility Closure and Entry by Merger Type



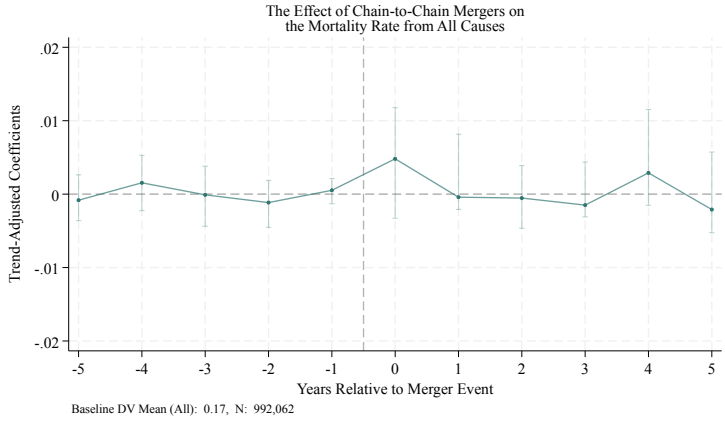
Notes: This figure presents coefficients from equation 4 estimated at the market-level, representing heterogeneity the effect of a merger of the number of all, merging and non-merging facilities in the market, by merger type. The vertical dashed line represents the merger event. The left column includes mergers where the target and acquirer are both chains. The middle column includes mergers where the target is an independent facility and the acquirer is a chain. The right column includes mergers where the target and acquirer are both independent. Mergers taking place between 1996 and 2018 are included. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all markets (treated and control) in the top panel, and across treated markets in the middle and bottom panels.

Figure 15: Heterogeneity in the Effect of Mergers on Hospitalizations, ICU Days and Blood Transfusions

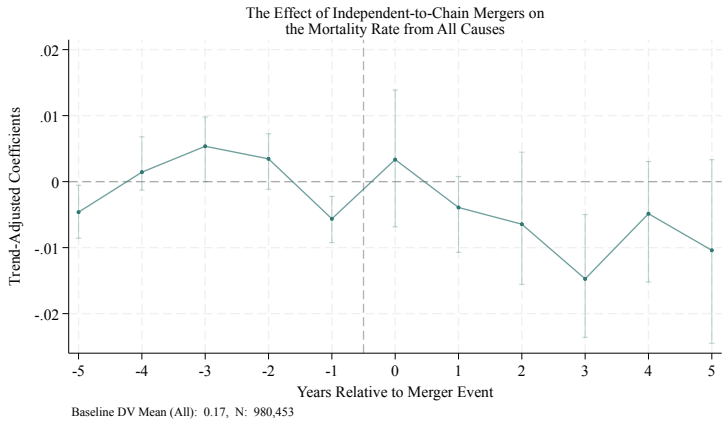


Notes: This figure presents coefficients from equation 4 estimated at the facility-level, representing heterogeneity by merger type in the effect of a merger on hospitalizations per patient, ICU days per patient and blood transfusion events per patient at all facilities in the market. The vertical dashed line represents the merger event. The left column includes mergers where the target and acquirer are both chains. The middle column includes mergers where the target is an independent facility and the acquirer is a chain. The right column includes mergers where the target and acquirer are both independent. The outcome represents Medicare Fee-For-Service patients. Mergers taking place between 1991 and 2018 are included. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market- and facility-level controls. The baseline dependent variable mean represents the mean of the outcome in the treated markets at event time -1. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all (treated and control) markets.

Figure 16: Heterogeneity in The Effect of Mergers on All-Cause Mortality



(a) Chain-to-Chain



(b) Independent-to-Chain



(c) Independent-to-Independent

Notes: This figure presents coefficients from equation 4 estimated at the facility-level, representing heterogeneity by merger type in the effect of a merger on all-cause deaths per patient at all facilities in the market. The vertical dashed line represents the merger event. The top panel includes mergers where the target and acquirer are both chains. The middle panel includes mergers where the target is an independent facility and the acquirer is a chain. The bottom panel includes mergers where the target and acquirer are both independent. The outcome represents all patients, regardless of insurance type. Mergers taking place between 1996 and 2018 are included. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market- and facility-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all (treated and control) markets.

Tables

Table 1: Summary Statistics for Market-Level Outcomes and Covariates

Outcomes		
Variable	Mean	SD
All Facilities	1.79	2.18
All Facilities (Treated Markets)	2.94	4.17
Merging Facilities (Treated Markets)	1.01	0.51
Non-Merging Facilities (Treated Markets)	1.93	4.05
Chains (Treated Markets)	1.65	2.95
Independent (Treated Markets)	1.28	2.03
Patients	76.10	132.21

Covariates		
Variable	Mean	SD
Median Income	42,228	13,078
Pop. Ages 0-9	10,675	30,961
Pop. Ages 10-19	11,596	31,559
Pop. Ages 20-29	11,336	35,647
Pop. Ages 30-39	11,280	35,882
Pop. Ages 40-49	11,660	32,985
Pop. Ages 50-59	10,669	28,606
Pop. Ages 60-69	7,963	20,381
Pop. Ages 70-79	5,113	13,050
Pop. Ages 80 +	3,052	8,162
Pop. Female	42,416	121,309
Pop. Hispanic	9,110	61,064
Pop. Non-Hispanic Black	9,242	53,912
Pop. Non-Hispanic White	60,677	120,970
Dialysis CON Law	0.24	0.43

Notes: This table presents summary statistics for market-level outcomes and covariates across treated and control markets in the stacked dataset. In this dateset, some control markets appear multiple times.

Table 2: Summary Statistics for Facility-Level Outcomes

Variable	Mean	SD	Variable	Mean	SD
Patients Per Station	3.65	2.90	Death Rate	0.177	0.087
Staff (FT & PT)	11.48	7.33	Death Rate - Cardiovascular	0.074	0.062
Nurses	5.10	3.91	Death Rate - Genitourinary	0.019	0.034
Technicians	5.30	4.35	Death Rate - Endocrine / Metabolic	0.001	0.008
Social Workers	1.07	0.55	Death Rate - Digestive	0.003	0.009
Share With ESA Prescription	0.72	0.20	Death Rate - Injuries	0.003	0.009
Mean ESA Dose (Units)	52,430	419,292	Death Rate - Infections	0.017	0.027
Share With Fistula	0.54	0.15			
			Hosp. Per Patient	1.74	0.62
Share With Good Kt/V	0.89	0.15	ICU Days Per Patient	2.50	2.08
Share With Hypocalcemia	0.10	0.07	Sessions Per Patient	114.97	31.54
Share With Hypercalcemia	0.02	0.03	Transfusions Per Patient	0.19	0.18
Mean UFR	7.56	4.98			
Share With Low Hemoglobin	0.19	0.12	All patients	70.24	47.55
Mean Iron Saturation	30.54	108.89	Patients with Medicare	53.75	37.21
Mean Serum Albumin	3.72	0.48			

Notes: This table presents summary statistics for facility-level outcomes across treated and control facilities in the stacked dataset. In this dateset, some control facilities appear multiple times.

Table 3: Summary Statistics for Facility-Level Covariates

Variable	Mean	SD
Facility Age	11.78	9.16
Case-Mix (Predicted Hospitalizations Per Patient)	1.76	0.09
Share Female Patients	0.45	0.10
Share White Patients	0.67	0.29
Share Black Patients	0.28	0.29
Share Hispanic Patients	0.07	0.15
Share Ages 10 to 19	0.00	0.03
Share Ages 20 to 29	0.02	0.03
Share Ages 30 to 39	0.05	0.04
Share Ages 40 to 49	0.10	0.06
Share Ages 50 to 59	0.18	0.08
Share Ages 60 to 69	0.26	0.08
Share Ages 70 to 79	0.24	0.09
Share Ages 80 to 89	0.12	0.08
Share Medicare Patients	0.88	0.14

Notes: This table presents summary statistics for facility-level covariates across treated and control facilities in the stacked dataset. In this dateset, some control facilities appear multiple times.

Table 4: The Effect of Mergers on Hospitalizations, ICU Days, and Transfusions Before 2011 vs. After 2012

	Hospitalizations Per Patient				ICU Days Per Patient				Transfusions Per Patient			
	1991-2011	2012-2021	Difference	T-Test Pval	1991-2011	2012-2021	Difference	T-Test Pval	1991-2011	2012-2021	Difference	T-Test Pval
Merger Year	-0.0052	0.0028	0.0080	0.8266	-0.1092**	0.0577	0.1669	0.0782	-0.0072	0.0082	0.0154	0.2502
	(0.0274)	(0.0240)			(0.0501)	(0.0804)			(0.0076)	(0.0110)		
1 Year Post	-0.0384	0.0176	0.0560	0.2621	-0.1868***	-0.0007	0.1862	0.0287	-0.0095	0.0067	0.0162	0.3137
	(0.0382)	(0.0322)			(0.0582)	(0.0621)			(0.0061)	(0.0148)		
2 Years Post	-0.0356	-0.0138	0.0218	0.6850	-0.1516*	-0.0224	0.1292	0.3894	-0.0111	-0.0069	0.0042	0.8134
	(0.0387)	(0.0374)			(0.0790)	(0.1276)			(0.0082)	(0.0158)		
3 Years Post	-0.0426	-0.0372	0.0054	0.9306	-0.2344**	0.0055	0.2398	0.2206	-0.0150*	-0.0096	0.0054	0.7783
	(0.0439)	(0.0433)			(0.1020)	(0.1671)			(0.0087)	(0.0172)		
4 Years Post	-0.0544	0.0101	0.0646	0.3406	-0.3127***	0.1228	0.4355	0.0406	-0.0195**	0.0038	0.0233	0.2073
	(0.0517)	(0.0438)			(0.1185)	(0.1767)			(0.0096)	(0.0158)		
5 Years Post	-0.1043	0.0029	0.1072	0.1836	-0.2908**	0.1022	0.3930	0.1121	-0.0211**	0.0015	0.0226	0.4148
	(0.0644)	(0.0485)			(0.1407)	(0.2034)			(0.0097)	(0.0259)		
DV Mean	1.9639	1.5085			2.1495	3.0863			0.1633	0.2197		
N	535271	216353			535271	216353			535271	216353		

Notes: This table presents estimates from equation 4 of the effect of mergers on hospitalization, ICU days and blood transfusion events. For each outcome, we estimate the effects separately for the years 1991-2011 (pre-QIP), or for mergers between 1996 and 2018, and the years 2012-2021 (post-QIP), or for mergers between 2015 and 2018. For each event-time, the difference between the pre- and post-QIP coefficients is computed, and the statistical significance of this difference is evaluated using a t-test. The outcomes represent patients with Medicare Fee-For-Service. Standard errors are bootstrapped. Regression specifications utilized to produce the coefficients include market- and facility-level controls. The baseline dependent variable mean represents the mean of the outcome in all (treated and control) markets at event time -1.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5: The Effect of Mergers on All-Cause Mortality Before 2011 vs. After 2012

	1991-2011	2012-2021	Difference	T-Test Pval
Merger Year	0.0052	0.0078	0.0026	0.7468
	(0.0047)	(0.0067)		
1 Year Post	-0.0045	0.0092**	0.0137	0.0317
	(0.0049)	(0.0040)		
2 Years Post	-0.0026	-0.0011	0.0015	0.8577
	(0.0062)	(0.0054)		
3 Years Post	-0.0071	0.0004	0.0075	0.4345
	(0.0082)	(0.0049)		
4 Years Post	-0.0031	0.0066	0.0096	0.3644
	(0.0078)	(0.0073)		
5 Years Post	-0.0078	0.0085	0.0163	0.1805
	(0.0104)	(0.0063)		
DV Mean	0.1791	0.1636		
N	536062	216715		

Notes: This table presents estimates from equation 4 of the effect of mergers on all-cause deaths per patient. We estimate the effects separately for the years 1991-2011 (pre-QIP), or for mergers between 1996 and 2018, and the years 2012-2021 (post-QIP), or for mergers between 2015 and 2018. For each event-time, the difference between the pre- and post-QIP coefficients is computed, and the statistical significance of this difference is evaluated using a t-test. The outcomes represent all patients, regardless of insurance type. Standard errors are bootstrapped. Regression specifications utilized to produce the coefficients include market- and facility-level controls. The baseline dependent variable mean represents the mean of the outcome in all (treated and control) markets at event time -1.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 6: The Effect of Mergers on Mortality by Cause Before 2011 vs. After 2012

	Cardiovascular Conditions				Digestive Conditions				Endocrine Conditions			
	1991-2011	2012-2021	Difference	T-Test Pval	1991-2011	2012-2021	Difference	T-Test Pval	1991-2011	2012-2021	Difference	T-Test Pval
Merger Year	0.0069*	-0.0011	-0.0080	0.1079	0.0005	0.0007	0.0001	0.8382	0.0010**	-0.0002	-0.0012	0.0332
	(0.0038)	(0.0033)			(0.0005)	(0.0004)			(0.0004)	(0.0004)		
1 Year Post	0.0037	0.0034	-0.0003	0.9471	0.0000	0.0010	0.0010	0.2604	0.0007	-0.0006	-0.0013	0.0304
	(0.0035)	(0.0033)			(0.0004)	(0.0008)			(0.0005)	(0.0004)		
2 Years Post	0.0074**	0.0026	-0.0048	0.3537	0.0007	0.0008	0.0001	0.9336	0.0006	-0.0007	-0.0013	0.0871
	(0.0034)	(0.0038)			(0.0007)	(0.0009)			(0.0005)	(0.0006)		
3 Years Post	0.0074	0.0004	-0.0070	0.1867	0.0005	0.0007	0.0002	0.8481	0.0007	-0.0005	-0.0012	0.3038
	(0.0049)	(0.0021)			(0.0009)	(0.0007)			(0.0009)	(0.0008)		
4 Years Post	0.0098	0.0072	-0.0026	0.7393	0.0007	0.0013	0.0006	0.6229	0.0012	-0.0011*	-0.0023	0.0241
	(0.0064)	(0.0045)			(0.0010)	(0.0008)			(0.0008)	(0.0006)		
5 YearsPost	0.0110*	0.0049	-0.0061	0.4174	0.0006	0.0005	-0.0002	0.9023	0.0006	-0.0009	-0.0015	0.2146
	(0.0058)	(0.0047)			(0.0009)	(0.0009)			(0.0009)	(0.0007)		
DV Mean	0.0902215	0.0580639			0.0044971	0.0022833			0.003144	0.0031408		
N	536062	216715			536062	216715			536062	216715		

	Genitourinary Conditions				Infections				Injuries			
	1991-2011	2012-2021	Difference	T-Test Pval	1991-2011	2012-2021	Difference	T-Test Pval	1991-2011	2012-2021	Difference	T-Test Pval
Merger Year	0.0004	0.0005	0.0001	0.9705	-0.0021**	-0.0020	0.0001	0.9567	0.0003	0.0010	0.0006	0.5189
	(0.0011)	(0.0034)			(0.0009)	(0.0014)			(0.0004)	(0.0009)		
1 Year After Merger	-0.0002	-0.0038*	-0.0036	0.1106	-0.0021	-0.0014	0.0007	0.7564	0.0000	0.0001	0.0002	0.8427
	(0.0009)	(0.0021)			(0.0014)	(0.0017)			(0.0004)	(0.0008)		
2 Years After Merger	-0.0001	-0.0047*	-0.0046	0.0968	-0.0042**	-0.0014	0.0028	0.3336	-0.0003	0.0004	0.0007	0.3648
	(0.0010)	(0.0026)			(0.0020)	(0.0022)			(0.0006)	(0.0004)		
3 Years After Merger	0.0000	-0.0033	-0.0033	0.3093	-0.0044*	-0.0011	0.0033	0.3923	0.0000	-0.0006	-0.0006	0.4678
	(0.0012)	(0.0030)			(0.0026)	(0.0028)			(0.0005)	(0.0006)		
4 Years After Merger	0.0007	-0.0033	-0.0041	0.2988	-0.0033	-0.0013	0.0020	0.6918	-0.0002	0.0002	0.0004	0.7221
	(0.0018)	(0.0035)			(0.0026)	(0.0042)			(0.0007)	(0.0007)		
5 Years After Merger	0.0011	-0.0053	-0.0064	0.1177	-0.0052*	-0.0006	0.0046	0.3625	-0.0003	0.0012	0.0015	0.3003
	(0.0019)	(0.0036)			(0.0030)	(0.0040)			(0.0010)	(0.0011)		
Baseline DV Mean	0.0032	0.0249			0.0218	0.0149			0.0032	0.0024		
N	536062	216715			536062	216715			536062	216715		

Notes: This table presents estimates from equation 4 of the effect of mergers on deaths per patient by cause of death. For each outcome, we estimate the effects separately for the years 1991-2011 (pre-QIP), or for mergers between 1996 and 2018, and the years 2012-2021 (post-QIP), or for mergers between 2015 and 2018. For each event-time, the difference between the pre- and post-QIP coefficients is computed, and the statistical significance of this difference is evaluated using a t-test. The outcomes represent all patients, regardless of insurance type. Regression specifications utilized to produce the coefficients include market- and facility-level controls. The baseline dependent variable mean represents the mean of the outcome in all (treated and control) markets at event time -1.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

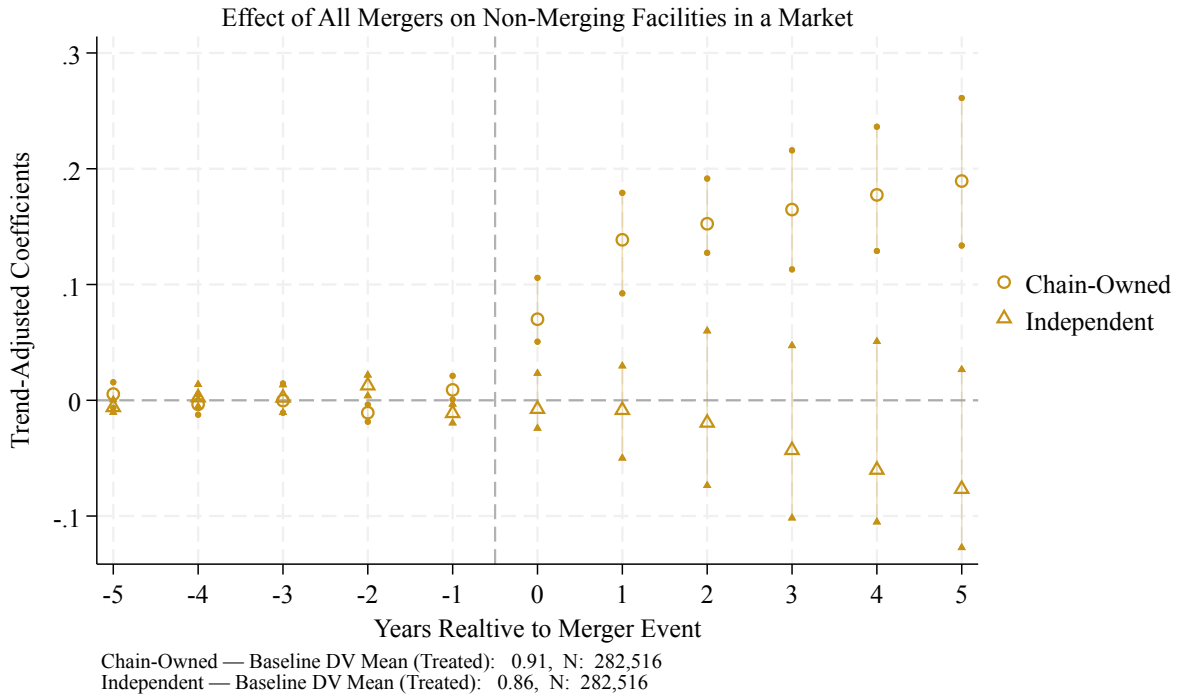
Table 7: Estimates of the Health Costs and Benefits of Dialysis Firm Mergers

	Object	Value	Source
	BENEFITS		
(1)	Total Traditional Medicare Part A Enrollees	35,377,335	KFF
(2)	Medicare Part A Program Payments Per Traditional Medicare Enrollee, $	5292	KFF
(3)	Total Traditional Medicare Part A Payments, $millions	187,216	(1) × (2)
(4)	Total Discharges	7,361,070	KFF
(5)	Total Traditional Medicare Part A Payments Per Discharge, $	25,433	(3)/(4)
(6)	Reductions in hospitalizations per person	0.248	Figure 9
(7)	Reductions in hospitalization spending per person , $	1922	(5) × (6)
(8)	Total persons exposed to mergers	109,295	See (20) to (24)
(9)	Reductions in aggregate spending, $ millions	**689**	
	COSTS		
(10)	Increase in deaths per patient	0.007	Figure 12
(11)	Increase in aggregate deaths	765	(8) × (10)
(12)	Mean age at death, years	68.4	USRDS data, all years
	Value of life years lost; Assumption 1		
(13)	Average U.S. life expectancy, years (68-69)	16.3	CDC Life Tables
(14)	Aggregate life years lost, years	12471	(11) × (13)
(15)	Value of life years lost, $ millions (low VSLY)	1621	$130 thousand × (14)
(16)	Value of life years lost, $ millions (central VSLY)	**3467**	$278 thousand × (14)
(17)	Value of life years lost, $ millions (high VSLY)	5275	$423 thousand × (14)
	Value of life years lost; Assumption 2		
(14)	Life expectancy within dialysis population, years (65-69)	4.3	USRDS 2023 Report, Table 6.1
(16)	Aggregate life years lost, years	3290	(11) times (14)
(17)	Value of life years lost, $ millions (low VSLY)	428	$130 thousand × (16)
(18)	Value of life years lost, $ millions (central VSLY)	**915**	$278 thousand × (16)
(19)	Value of life years lost, $ millions (high VSLY)	1392	$423 thousand × (16)
	Total persons exposed to mergers		
(20)	Unique treated markets	724	Internal estimate
(21)	US Counties	3143	U.S. Census
(22)	Share counties treated	0.23	(24) / (25)
(23)	In-center dialysis patients	474,468	USRDS 2024 Report, Figure 1.8
(24)	Patients exposed to mergers	109,295	(25) × (26)

Notes: This table reports our calculations estimating the health costs and benefits of dialysis firm mergers. Data sources for each input into the calculation are described in detail in Section 7.2.

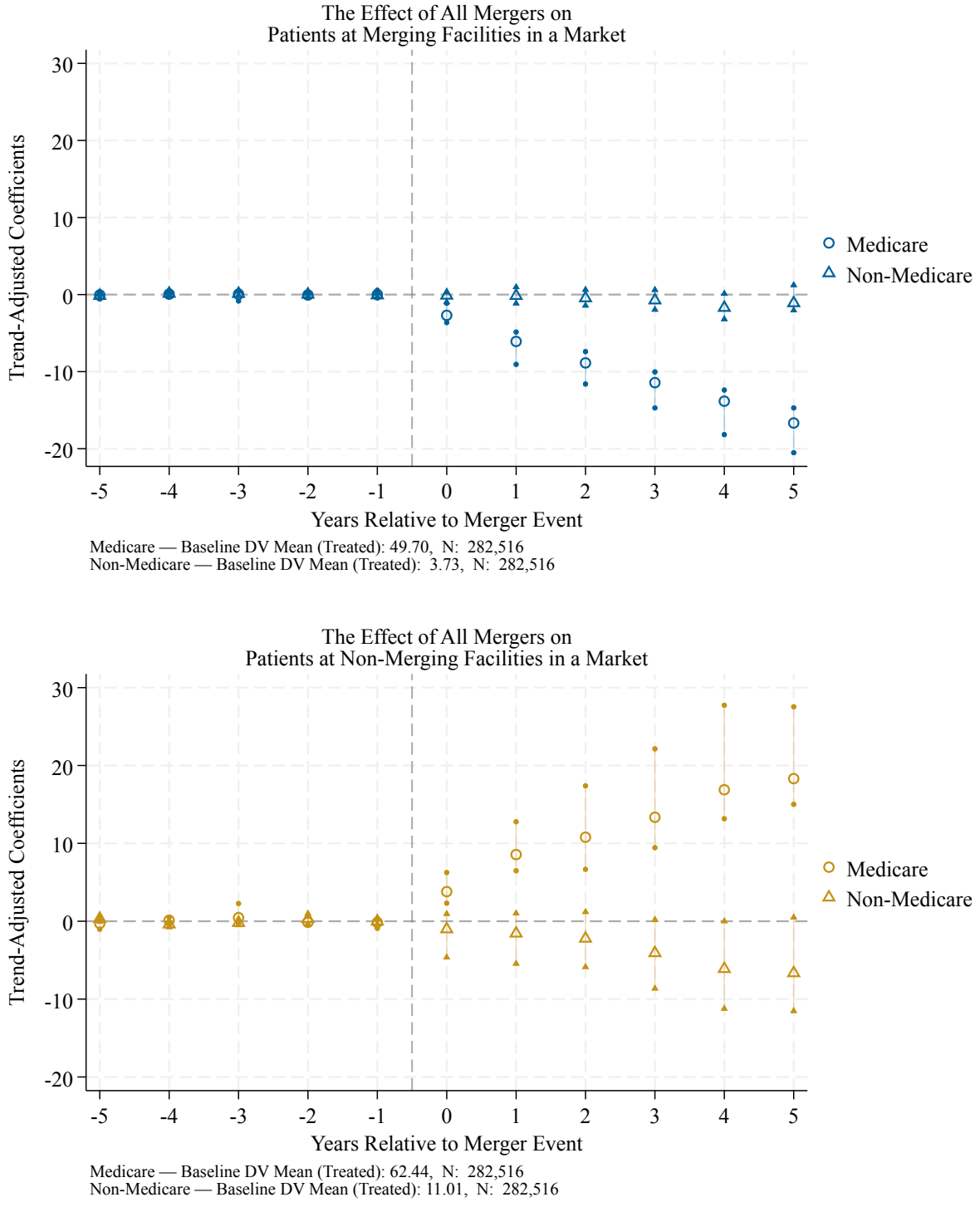
A Appendix Figures

Figure A.1: The Effect of All Mergers on Non-Merging Facilities By Owner Type



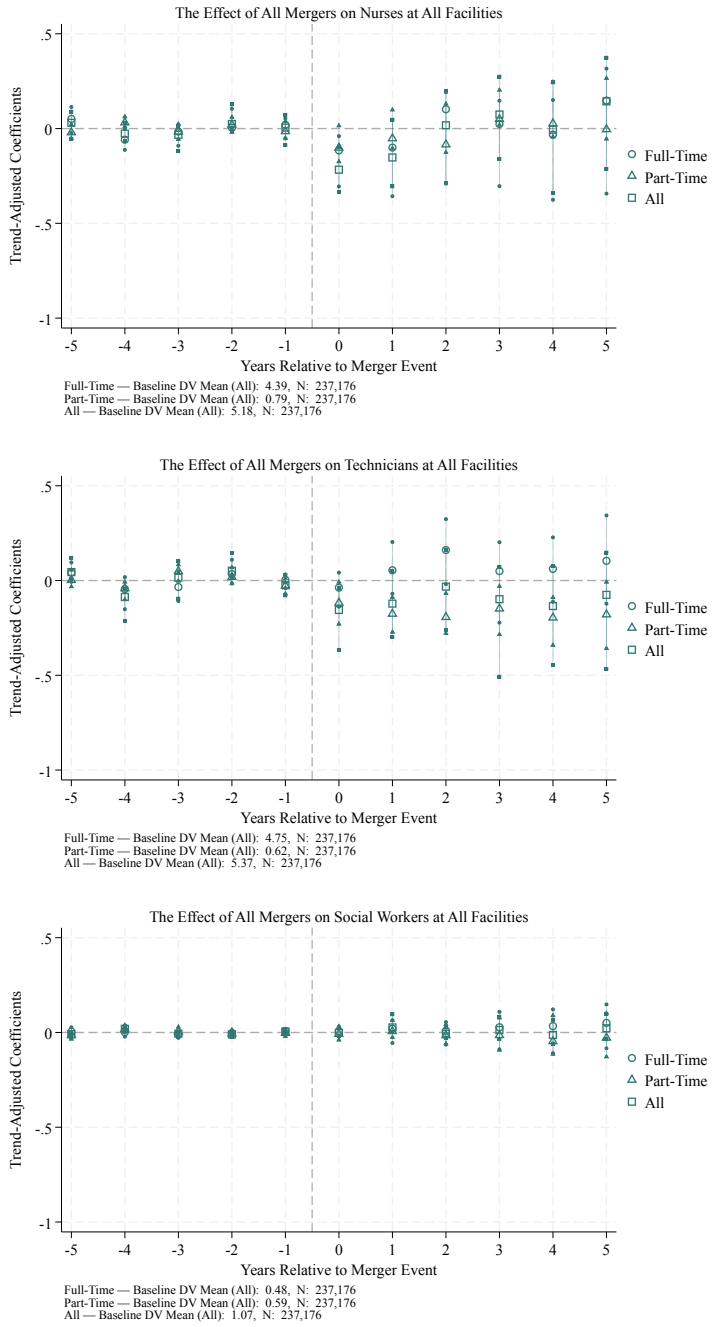
Notes: This figure present coefficients from equation 4 estimated at the market-level, representing the effect of a merger on the number of chain-owned and independent non-merging facilities in the market, respectively. The vertical dashed line represents the merger event. Mergers taking place between 1996 and 2018 are included. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. The regression specification utilized includes market-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across treated markets.

Figure A.2: The Effect of All Mergers on Patient Volume at Merging and Non-Merging Facilities by Insurance Type



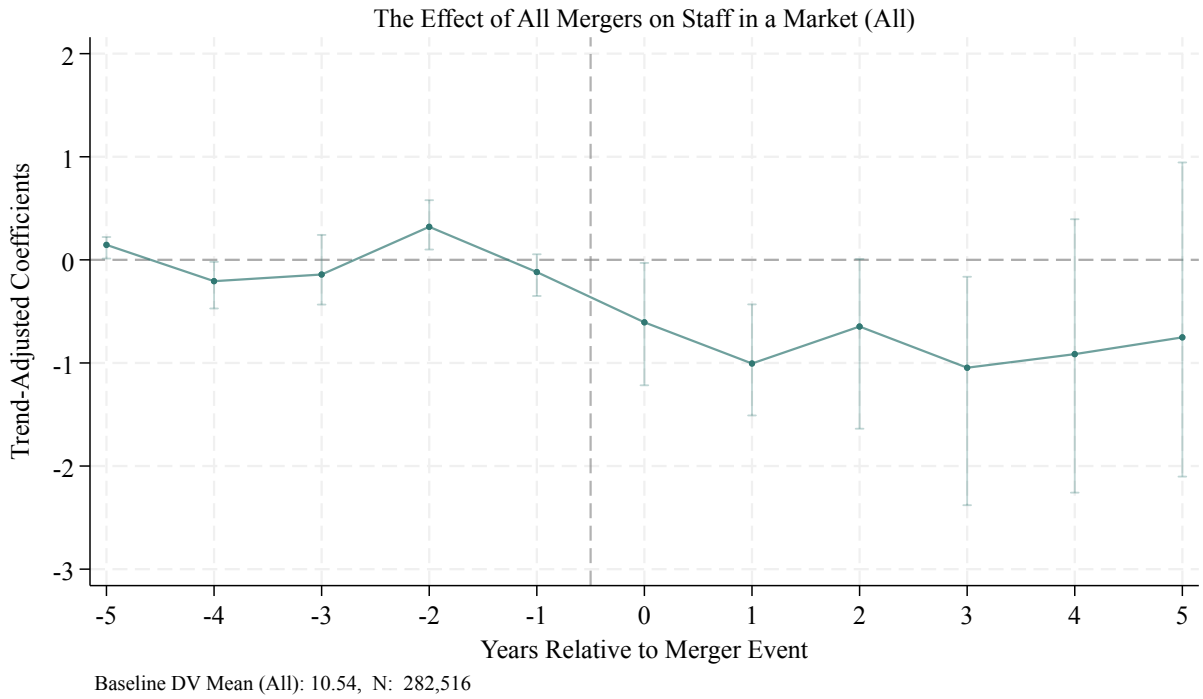
Notes: This figure present coefficients from equation 4 estimated at the market-level, representing the effect of a merger on the number of patients in a market by insurance type. The vertical dashed line represents the merger event. The top panel represents effects at merging facilities, and the bottom panel represents effects at non-merging facilities. Mergers taking place between 1996 and 2018 are included. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. The Regression specification utilized includes market-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across treated markets.

Figure A.3: The Effect of All Mergers on Nurses, Technicians and Social Workers



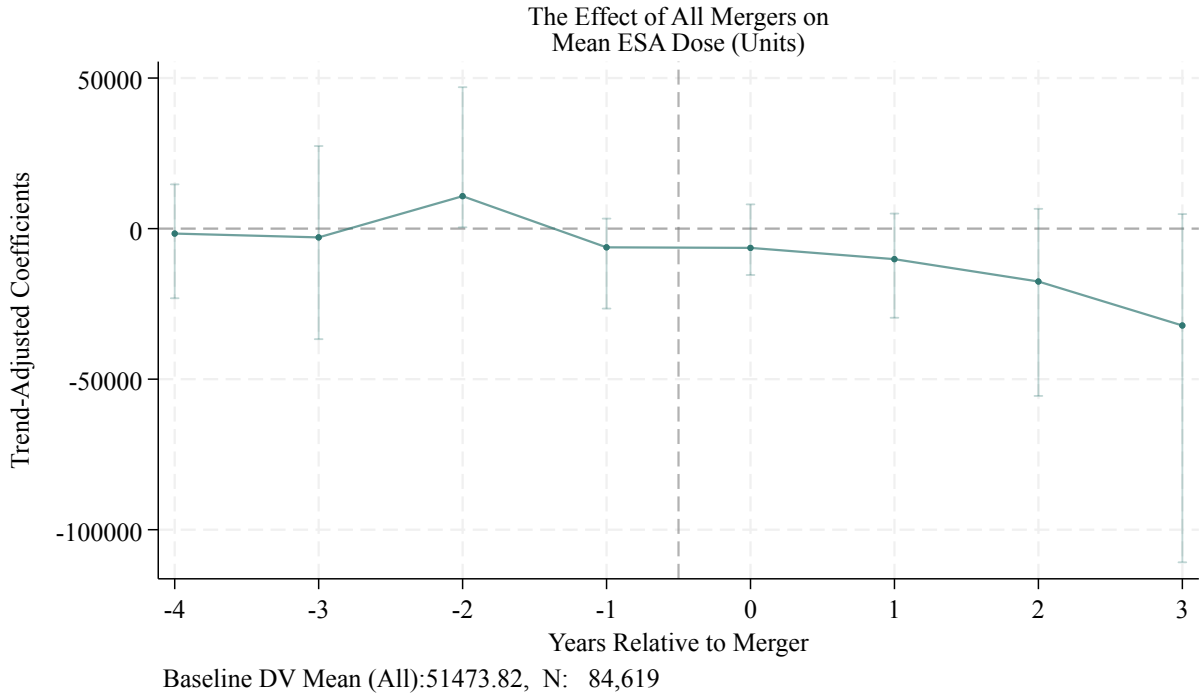
Notes: This figure presents coefficients from equation 4 estimated at the facility-level, representing the effect of a merger on staffing categories at all facilities. The vertical dashed line represents the merger event. The top panel shows effects for nurses, the middle for technicians and the bottom for social workers. Separate estimates are provided for all, full-time, and part-time staff. Mergers taking place between 2008 and 2018 are included. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all (treated and control) markets.

Figure A.4: The Effect of All Mergers on Market-Level Staffing



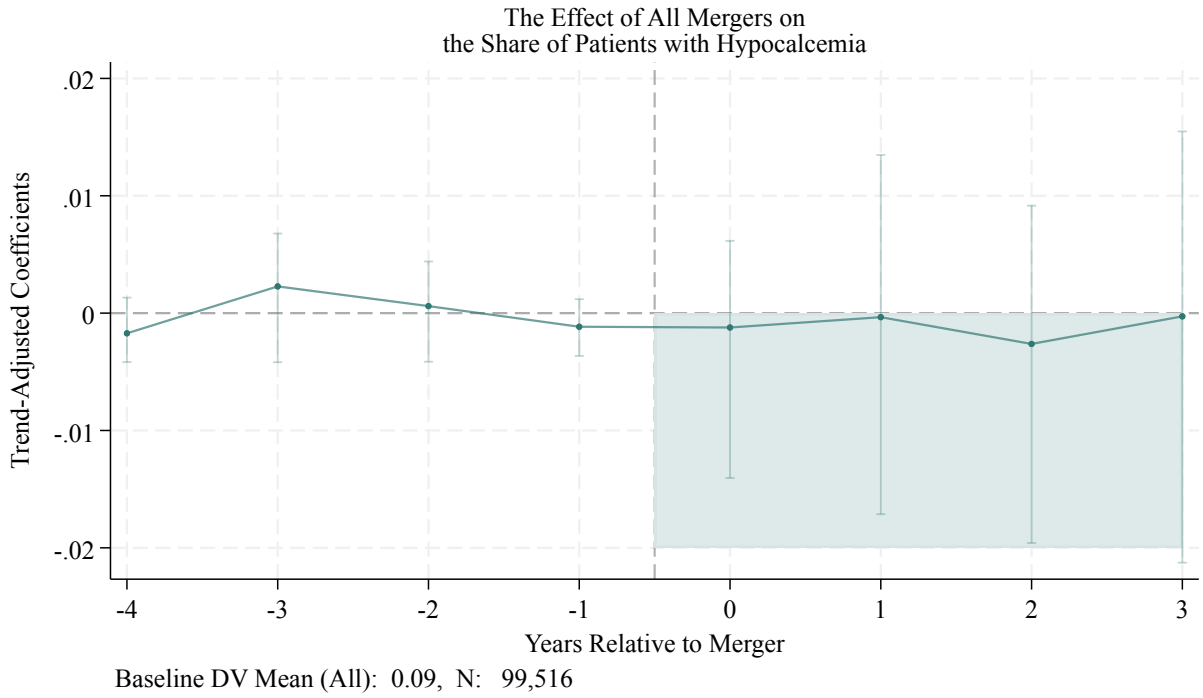
Notes: This figure presents coefficients from equation 4 estimated at the market-level, representing the effect of a merger on total staffing volume. The vertical dashed line represents the merger event. Mergers taking place between 2008 and 2018 are included. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all (treated and control) markets.

Figure A.5: The Effect of All Mergers on the Average Monthly ESA Dosage (Units)



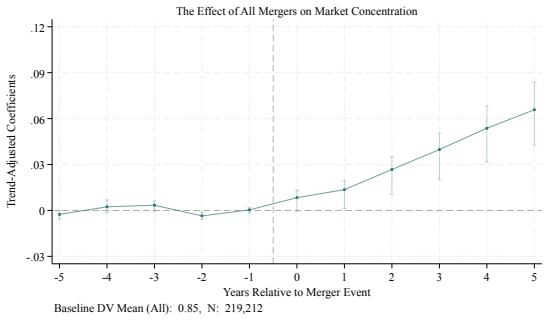
Notes: This figure presents coefficients from equation 4 estimated at the facility-level, representing the effect of a merger on the average monthly ESA dose measured in units. The vertical dashed line represents the merger event. The outcome represents all patients, regardless of insurance type, included in CROWNWeb reports. Mergers taking place between 2015 and 2018 are included. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level and facility-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all (treated and control) markets.

Figure A.6: The Effect of All Mergers on the Share of Patients with Hypocalcemia

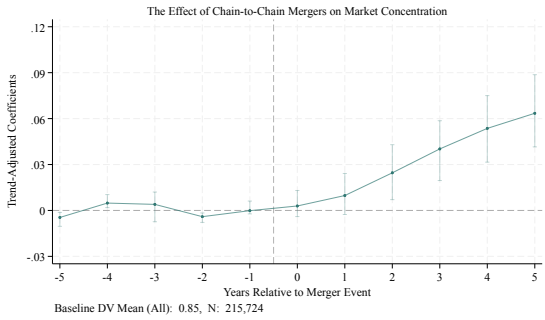


Notes: This figure presents coefficients from equation 4 estimated at the facility-level, representing the effect of a merger on the facility's share of patients with hypocalcemia measured as mean serum calcium < 8.5 mg/dL. Green shading indicates the direction of clinically beneficial merger effects. The vertical dashed line represents the merger event. The outcome represents all patients, regardless of insurance type, included in CROWNWeb reports. Mergers taking place between 2015 and 2018 are included. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level and facility-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all (treated and control) markets.

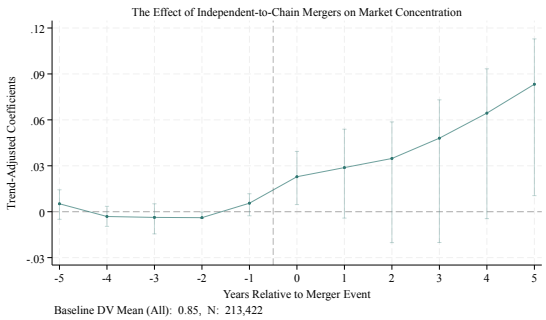
Figure A.7: The Effect of Mergers on Market Concentration



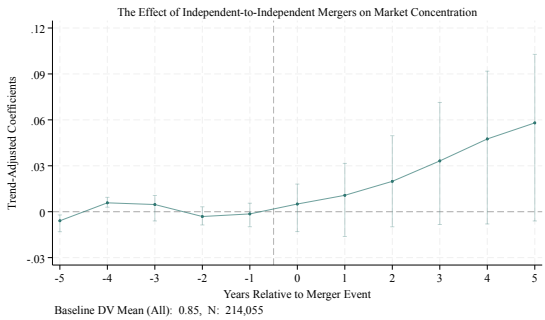
(a) All Mergers



(b) Chain-to-Chain Mergers



(c) Independent-to-Chain Mergers



(d) Independent-to-Independent Mergers

Notes: This figure presents coefficients from equation 4 estimated at the market-level, representing the effect of a merger on the market-concentration. The vertical dashed line represents the merger event. Market concentration is estimated using facility-level, and not firm-level, patient shares. Mergers taking place between 1996 and 2018 are included. Panel (a) presents the estimated effects for all mergers, panel (b) presents the estimated effects for mergers where the target and acquirer are both chains, panel (c) presents the estimated effects for mergers where the target is an independent facility and the acquirer is a chain, and panel (d) presents the estimates effects for mergers where the target and acquirer are both independent facilities. 95% confidence intervals around the estimates are shown and are calculated using bootstrapped standard errors. Regression specifications utilized to produce the coefficients in all panels include market-level controls. The baseline dependent variable mean reflects the mean of the outcome at event time -1, calculated across all (treated and control) markets.

B Data Appendix

B.1 Determining Mergers and Markets

We obtain the Centers for Medicare and Medicaid Services (CMS) Provider of Service (POS) Files for the fourth quarter of each year from 1991 to 2021 from the NBER.[48] In cleaning the data, we assign each facility the modal state and county codes across its observations (or the lowest code if there are several modal values) when it appears in more than one state or county due to data errors. When a state and county code is available for a provider in some years, but missing in others, we fill out the missing values using the available information in other years. When the state and county code is missing in all years, we identify the state and county manually by web-searching the address. To address cases where the same address shows multiple provider numbers over the years, we identify situations where changes appear to reflect the same facility rather than distinct entities, and make appropriate corrections.[49]

The POS indicates a facility's chain ownership status. Missing values for this variable are assumed to be zero. For facilities with a missing year within a sequence, the panel is completed by carrying backward the values for chain ownership, state, and county from the next available non-missing year. We emphasize that we do not construct a balanced panel of facilities from 1991 to 2021; years preceding a facility's first appearance or following its last appearance are treated as the facility not existing, which is crucial for the analysis of entry and closures.

The POS file provides dates of facility ownership changes. We extract the year from this variable and tag each facility-year as a merger year when applicable. For facilities with multiple ownership changes, we use the most recent change, allowing identification of multiple mergers within a facility. We classify merger type using chain ownership information from the year prior to the merger for the target and from two years after the merger for the acquirer.[50]

Markets are defined at the county level. We divide three large counties, namely Cook County (IL), Los Angeles County (CA) and San Diego County (CA), into smaller markets. This classification of markets is adopted from the approach by Wollmann (2020).[51]

[48] NBER hosts the cleaned POS files here: `https://www.nber.org/research/data/provider-services-files`.

[49] Specifically, we compare the number of provider IDs at each state–county–address across years and within each year, and we merge IDs only when there is never more than one facility at an address within a year, so as not to combine genuinely separate providers. When the algorithm flags an apparent duplication of codes, we replace the provider number with the number recorded for that provider in the most recent year of the data.

[50] we use the value from two years after the merger, rather than the year of or immediately after, to account for delays in updates. Because my algorithm restricts treated units to those without merger activity for five years following the merger, the two-year post-merger value is unlikely to reflect subsequent mergers.

[51] Cook County (IL) is split into three markets where the first includes Evanston, the second includes Orland Park, and the third includes Elk Grove Village. San Diego County (CA) is split into two markets where the first consists of Encinitas, Escondido, Oceanside, San Marcos and Vista, and the second includes the rest of San Diego. Los Angeles County (CA) is subdivided into five markets where the first comprises Bellflower, Cerritos, Compton, Cudahy, Downey, Hawaiian Gardens, Huntington Park, Lakewood, Long Beach, Lynwood, Norwalk, Paramount, Santa Fe Springs, South Gate, Whittier, Rancho Dominguez, and San Pedro. The second comprises Alhambra, Arcadia, Azusa, Burbank, El Monte, Encino, Glendale, Glendora, Mission Hills, Monrovia, Montebello, Monterey Park, North Hollywood, Pacoima, Panorama City, Pasadena, San Gabriel, Sherman Oaks, South El Monte, Sun Valley, Temple City, Van Nuys, Boyle Heights. The third comprises Beverly Hills, Carson, Culver City, Gardena, Harbor City, Hawthorne, Inglewood Lomita, Redondo Beach, Santa Monica, Torrance. The fourth consists

B.2 Creating the Stacked Dataset

To estimate equation 1, we first construct a separate dataset for each sub-experiment d, where d denotes the calendar year in which a merger occurred. For each sub-experiment, we restrict the data to the periods between $\mathcal{L}'$ years before and $\mathcal{L}''$ years after year d, and include units that were treated in year d and units that were not treated in year d. In this paper, $\mathcal{L}'$ and $\mathcal{L}''$ are both 5 years for outcomes available over a long time horizon. For CROWNWeb outcomes which are available from 2012 onwards, $\mathcal{L}'$ is 4 and $\mathcal{L}''$ is 3.

In each sub-experiment d, treated units belong to markets that experience a merger in year d but no mergers in the 10 years prior to d or in the 5 years following d. This ensures that any post-merger effects observed during the 5 years after the merger can be attributed to the merger in year d and are not confounded by other consecutive mergers. Similarly, the 10-year pre-event window ensures not only that no merger occurs in the unit's market in the 5 years preceding the event of interest, but also that any effects from earlier mergers have had at most five years to dissipate. In all analyses, control units belong to markets that were not exposed to any merger in year d, for 10 years prior to year d and for 5 year after d. The procedure for identifying treated and control markets in the analysis of merger types (e.g., chain-to-chain mergers) is analogous, except that treated markets are those experiencing the reference-type merger in the year of interest but no merger of any type in the surrounding windows.

For outcomes measured from 1991 to 2021, we study mergers occurring between 1996 and 2018, allowing for complete data on all five pre-merger periods and at least four post-merger periods for each unit. Further, for mergers occurring between 2001 and 2016, we construct treated and control markets using the procedure described above, imposing restrictions on merger activity in the market during the 10 years prior to and 5 years after the merger. For mergers from 1991 to 2000, we allow for a shorter clean pre-merger period, calculated as the number of years between 1991 and the reference year (e.g., a 9-year clean pre-event window for mergers in 2000). Similarly, for mergers in 2017 and 2018, we allow for a shorter clean post-merger period, determined by the number of years between the reference year and 2021 (e.g., a 3-year clean post-event window for mergers in 2018). For outcomes measured from 2012 to 2021, the estimating equation includes fewer pre- and post-merger periods, but the restrictions on merger activity in markets in years surrounding the merger remain the same as previously described.

We define event time e as the calendar year minus the merger year d, with event time 0 corresponding to the merger year. This creates a sub-experiment specific dataset where the sub-experiment is indexed by the merger year. We then append the sub-experiment–specific datasets to create a stacked dataset aligned on event time. Markets may appear multiple times in the stacked dataset as they can serve as controls for more than one merger, and the same market may have multiple eligible mergers, separated sufficiently in time to be included as treated observations in the stacked dataset.

of Baldwin Park, Canoga Park, Canyon Country, Covina, Granada Hills, Hacienda Heights, La Puente, Lancaster, Newhall, Northridge, Palmdale, Pomona, San Dimas, San Fernando, Sylmar, Tarzana, Valencia, West Covina, Woodland Hills, City Of Industry, Irwindale, Santa Clarita, West Hills. The fifth includes the rest of Los Angeles County.

B.3 Market-Level Covariates

We compile a comprehensive set of county-level controls for inclusion in all regressions. For almost all markets, the covariates describe the market uniquely but for three large counties that were further split into smaller markets, covariate values are common within the county in which these markets lie. County median income is drawn from the Census SAIPE program. Because county estimates are missing for some early years (1991, 1992, 1994, 1996), we impute the 1993 value for 1991–1994 and otherwise carry forward the most recent available year. County population totals by age, sex, and race are compiled from the Census' July intercensal estimates. Dialysis certificate-of-need (CON) laws by state are taken from the AHPA matrix, available for 2004, 2008, 2012, and 2016. Only Missouri changed status during this period. We code CON law status for missing states as 0, use the 2004 value for all years up to 2007, and carry forward the last available value thereafter.

B.4 Variable Construction in the Analysis of Closures, Patient Volume, Congestion and Staffing

The USRDS facility data provide information on total dialysis patients and stations at each facility, the number of patients with Medicare (pending or approved), and staff counts by category and by full- or part-time status. These data are first merged with a crosswalk of USRDS and CMS facility IDs to enable linkage with the CMS POS data. The resulting market-level dataset retains all POS facilities. Facilities present in POS but absent from USRDS contribute to market-level counts of facilities but not to totals for staff, patients, or stations.

Each facility is classified as "merging" or "non-merging" within a sub-experiment if it is located in a treated market and is the facility undergoing a merger in the year indexed by the sub-experiment. In analyses of general-equilibrium effects, both categories are treated. The same classification is applied to staff, patients, and stations in treated markets. Facility-level outcomes are then used directly in the analyses of staffing and station congestion.

We next construct a balanced panel covering the full study period from 1991 to 2021. For any years in which a market is not observed, either before its first appearance or after its last, outcome variables are set to zero. This yields a consistent panel of all U.S. markets that contained at least one facility at some point between 1991 and 2021. We then aggregate the numbers of facilities, stations, staff, and patients at the market level, both overall and by merging status. These aggregates serve as the outcomes in the market-level analyses.[52] By definition, control markets contain no merging facilities. Consequently, their outcomes are identical across categories (e.g., total facilities equals total non-merging facilities).

B.5 Patient Insurance Information

For each patient and treatment spell, the USRDS provides payer history. We construct a patient–year indicator for whether Medicare was the primary payer. In each calendar year, we record a patient's modal payer, preferring the latest spell in any year when there is a

[52] Market-level outcomes are used in Figures 2, 3, A.4, 4, A.2, 14, and A.1.

tie. We then record whether the patient's modal primary payer was Medicare FFS. Patients for whom Medicare was not the primary payer are excluded whenever claims data are used to define outcomes (e.g., hospitalizations) or to assign modal facilities, ensuring that counts accurately reflect Medicare-covered care.

B.6 Linking Patients to Facilities

In the analysis of ESA prescription, fistula usage and all biomarkers and clinical endpoints, we utilize patient level data and aggregate it up to the facility level. To enable this, we first create unique annual linkages between patients and facilities. Following is the linkage algorithm we employ.

1. **Identify patient-facility pairs using outpatient claim revenue center codes (2002-2021)**.
    - Extract calendar year from the claim beginning dates. All claims end and begin in the same calendar year so this algorithm would work similarly when using claim end dates.
    - Count the number of dialysis sessions in each claim period for a patient and record the facility associated with the claim. Utilize the HCPCS revenue center code 90999 to identify dialysis.
    - Retain the patient-facility pair where the patient received the maximal dialysis sessions during a year. If there is a tie, prefer the facility with the later claim end date. If still tied prefer facility with earlier claim start date. If still tied, pick randomly (this happens rarely).
    - Merge with patient insurance information and keep only patients for whom Medicare FFS is the primary payer, since any information from claims is most accurate only for these patients.
2. **Identify patient-facility pairs using pre-cleaned outpatient claims (2011-2021)**.
    - In addition to raw outpatient claims, the USRDS also provides pre-cleaned claims files for years 2011 to 2021.
    - Like before, retain the patient-facility pair where the patient received the maximal dialysis sessions during a year. If there is a tie, prefer observations where the treatment modality was in-center hemodialysis.
    - Merge with patient insurance information and keep only patients for whom Medicare FFS was the primary payer.
3. **Identify patient-facility pairs using treatment spells (1991-2021)**.
    - For all patients, USRDS provides a detailed treatment history including precise dates of change in treatment modality and the facility where treatment was incurred during each such period.

- Retain patients receiving hemodialysis.
- For each treatment spell, calculate the number of treatment days in each calendar year covered by the spell and record the facility where the patient received treatment.
- For each patient-year keep the facility with the maximum treatment days in any calendar year.

4. **Create the final patient-facility linkage using the following heirarchy**.
    - Begin with the linkages from Step 3.
    - When the facility ID is missing, utilize the linkage determined in Step 1.
    - When the facility ID is missing, utilize the linkage determined in Step 2.

This produces the complete annual patient-facility linkages which include all patients on dialysis, regardless of insurance type.

B.7 Construction of Patient-Level Outcomes

B.7.1 Dialysis Sessions (2002-2021)

The enumeration of total annual dialysis sessions by patients mirrors step 1 in Section B.6 above. We retain all patient-year-facility records.

B.7.2 CROWNWeb Outcomes (2012-2021)

We derive most biomarkers, ESA prescriptions and fistula use, from CROWNWeb data. CROWNWeb (now called the End Stage Renal Disease Quality Reporting System, or EQRS) is an electronic reporting system through which dialysis facilities submit patient information and lab results to comply with CMS coverage requirements and, in particular, the QIP. These data are reported monthly and include both Medicare and non-Medicare patients who receive treatment on the day of data collection.

For these patients, we compute the annual modal ESA receipt status and vascular access type at each facility where the patient receives treatment. We then construct a variable indicating whether the annual modal vascular access type was a fistula. Next, for each patient-year-facility, we calculate the mean values of single-pool Kt/V, corrected serum calcium, ESA dosage (if reported in units), pre-dialysis weight, post-dialysis weight, dialysis session length (minutes), serum ferritin, serum hemoglobin, iron saturation, serum albumin, and serum phosphorus.

We create dummy variables for quality thresholds, namely good Kt/V (mean $\geq$ 1.2), hypercalcemia (mean serum calcium $>$ 10.2 mg/dL), hypocalcemia (mean serum calcium $<$ 8.5 mg/dL), high hemoglobin (mean $>$ 12 g/dL), and low hemoglobin (mean $<$ 10 g/dL). These thresholds are derived from Medicare QIP measures (Kt/V, hypercalcemia) or the clinical literature. We also calculate each patient's mean annual ultrafiltration rate.

B.7.3 Mortality (1991-2021)

The USRDS provides a patient-level overview file contain demographic, death, transplant and high-level treatment information for each dialysis patient. The date of death of each patient provides information for the year of death. The file also reports a patient's primary cause of death through a set of USRDS' own codes. We assign these codes to broad disease categories as follows.

- **Cardiovascular:** Atherosclerotic heart disease, cardiomyopathy, cardiac arrhythmia, cardiac arrest (cause unknown), valvular heart disease, pulmonary edema due to exogenous fluid, congestive heart failure, pulmonary embolus, cerebro-vascular accident including intracranial hemorrhage, ischemic brain damage/anoxic encephalopathy, mesenteric infarction/ischemic bowel, air embolism.
- **Genitourinary (non-infectious only):** Withdrawal from dialysis/uremia.
- **Endocrine/Metabolic:** Hyperkalemia, hypokalemia, hypernatremia, hyponatremia, acidosis, adrenal insufficiency, hypothyroidism, hyperthyroidism, hypoglycemia, hyperglycemia, diabetic coma.
- **Digestive (not including Hepatitis):** Gastro-intestinal hemorrhage, pancreatitis, hemorrhage from transplant site, hemorrhage from surgery, liver-drug toxicity, cirrhosis, polycystic liver disease, liver failure (cause unknown other), perforation of peptic ulcer, perforation of bowel.
- **Injury/Poisoning:** Suicide, accidental (treatment related), accidental (not treatment related), accident related to treatment, accident unrelated to treatment, drug overdose (street drugs), drug overdose (other), complications of surgery, hemorrhage from vascular access, hemorrhage from dialysis circuit, hemorrhage from ruptured vascular aneurysm, other hemorrhage.
- **Infections (all septicemia + viral/bacterial/fungal/COVID-19):** Pulmonary infection, septicemia, viral hepatitis, infection (other), septicemia due to internal vascular access, septicemia due to vascular access catheter, septicemia due to peritonitis, septicemia due to peripheral vascular disease (gangrene), peritoneal access infectious complication, peritonitis (complication of peritoneal dialysis), central nervous system infection (brain abscess, meningitis, encephalitis, etc.), pulmonary infection (bacterial, fungal, other), viral infection (CMV), viral infection (other), tuberculosis, AIDS, cardiac infection (endocarditis), pulmonary infection (pneumonia, influenza), abdominal infection (peritonitis not complication of PD, perforated bowel, diverticular disease, gallbladder), COVID-19, COVID-19 unconfirmed, genitourinary infection (urinary tract infection, pyelonephritis, renal abscess), Hepatitis B, other viral hepatitis, Hepatitis C.

Upon merging with the patient panel data described in detail in Section B.8, the year corresponding to a patient's death is assigned a value of 1 for the death indicator, and all prior years are assigned a value of 0. By definition, the patient is excluded from the sample in all subsequent years.

B.7.4 Hospitalization, ICU Days, Transfusions (1991-2021)

Hospitalizations are identified from Medicare Part A claims. For each patient-year-facility, the number of hospitalizations equals the total count of unique claims in that year. Each claim also reports days spent in the ICU, which are summed to construct the ICU-days variable. Medicare claims include up to 25 ICD-9 or ICD-10 procedure codes. A red blood cell transfusion is flagged if any of these codes indicate transfusion of packed cells, of nonautologous red blood cells, of nonautologous frozen red cells, or of nonautologous whole blood into a central or peripheral vein.

B.8 Constructing Facility-Level Health Outcomes

We construct patient-level variables available for (we) all dialysis patients, (ii) all dialysis patients with CROWNWeb data, and (iii) patients with Medicare FFS, and then aggregate these outcomes to the facility level.

We begin with the unique patient–year–facility combinations generated by the algorithm described in Section B.6. At this step, we calculate each facility's annual counts of all patients. Next, we merge in death data using patient IDs and CROWNWeb data using patient ID, year, and facility ID so that each merged record retains its patient–facility linkage.[53] For each facility–year, we sum total deaths (all-cause and by cause), sum the values of dummy variables (e.g., fistula use, ESA prescription, hypercalcemia, good Kt/V), and compute means of continuous variables (e.g., ESA dose, ultrafiltration rate, serum albumin). For each facility–year, we also count the number of patients with non-missing data for each CROWNWeb outcome and take the maximum of these counts as the facility's total CROWNWeb patient count for that year.

Collapsing to the facility–year level yields annual averages for continuous outcomes. For dummy variables, we calculate facility-level shares by dividing the total count for that outcome (e.g., deaths, patients with a fistula, patients with good Kt/V) by the relevant patient denominator. For mortality, the denominator is total facility patients; for CROWNWeb outcomes, it is the facility's CROWNWeb patient count.

In a separate step, we construct facility-level health outcomes for Medicare FFS patients only. As before, we start from the unique patient–year–facility combinations described in Section B.6. We merge in patient insurance information from Section B.5 and retain only observations where Medicare FFS is the primary payer. We then merge in dialysis session counts and URR from Part B claims. If a patient–year appears multiple times in Part B, the merge retains the observation matching the patient's unique facility linkage. If a Part B claim has no matching patient–facility link, we retain the record but treat session and URR values as missing.

We next merge in hospitalization, ICU, and transfusion data at the patient–year level. Because these Part A data are not facility-linked, we attribute hospitalizations to the patient's unique facility by construction. If a patient–year does not merge, we assume the patient had no Part A claims (and therefore no hospitalizations) and set all hospitalization-related variables to zero. Finally, all patients in the resulting dataset form the denominator

[53] This means that a patient's information associated with their non-modal facility is dropped.

for hospitalization, ICU-days, and transfusions per patient, while only patients with non-missing session information form the denominator for sessions per patient and for the share of patients with good URR.

B.9 Facility-Level Covariates

The USRDS patient-level overview file provides demographic information. During the construction of the facility-level health outcome data, we merge these demographics using the unique patient ID. We create indicator variables for sex, two race categories, Hispanic origin, and 10-year age groups. Before collapsing to the facility level, we sum these indicators so that, in the all-patient dataset, we capture the total number of patients in each demographic category and, in the Medicare-only dataset, we capture the total number of Medicare FFS patients in each demographic category. In the collapsed dataset, we then calculate the share of the relevant patient population in each demographic category, which we use as covariates.

Separately, from the USRDS facility data we compute the share of patients with Medicare pending or approved status and include this measure among the facility-level covariates. Finally, using the CMS POS data, we identify each facility's first year of operation and calculate its age by subtracting this year from the calendar year of observation. We also use facility age as a covariate.