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The Impact of Hospital Mergers on Treatment Intensity and Health Outcomes

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The Impact of Hospital Mergers on Treatment Intensity and Health Outcomes

Abstract

Objective. To analyze the impact of hospital mergers on treatment intensity and health outcomes.

Data. Hospital inpatient data from California for 1990 through 2006, encompassing forty mergers.

Study Design. I used a geographic-based IV approach to determine the effect of a zip code's exposure to a merger. The merged facility's market share represents exposure, instrumented with combined pre-merge shares. Additional specifications include the Herfindahl index (HHI), instrumented with predicted change in HHI.

Results. The primary specification results indicate that mergers increase the use of bypass surgery and angioplasty by 3.7 percent and inpatient mortality by 1.7 percent above averages in the year 2000 for the average zip code. Isolating the competition mechanism mutes the treatment intensity result slightly, but more than doubles the merger exposure effect on inpatient mortality to an increase of 3.9 percent. The competition mechanism is associated with a sizeable increase in number of procedures.

Conclusions. Unlike previous studies, this analysis finds that hospital mergers are associated with increased treatment intensity and higher inpatient mortality rates among heart disease patients. Access to additional outcome measures such as 30-day mortality and readmission rates might shed additional light on whether the relationship between these outcomes is causal.

Key Words. Hospitals, mergers, quality

Introduction

Consolidation is a common response to financial and competitive pressures in many markets. The primary concern with consolidation is often financial: Will the merger increase prices? The healthcare sector involves additional complications. For example, both dimensions of demand for healthcare services are affected by third parties: treatments are prescribed by physicians while health insurance shields consumers from the full costs of care. Prices do not reflect their demand curve but are instead set by the insurer for public insurance plans or through negotiations between the insurer and providers in private insurance plans. Because of these factors, the implications of price changes in the healthcare sector on consumers may be more difficult to disentangle. Additionally, the quality of healthcare services is important for both survival and quality of life. Thus, the impact of consolidation in healthcare markets on non-price dimensions, such as quality, may be at least as important as its impact on price.

I define a hospital merger as the consolidation of two facilities into a single legal entity, wherein the license-relinquishing facility becomes a satellite campus of the license-retaining facility and hospital boards and physician and nursing staffs are unified. Despite the intensity of this transaction, the average rate of hospital mergers doubled from 12 per year in the mid-1980s to 24 per year in the early 1990s (Bazzoli et al. 2002). Pressures along both price and quantity dimensions may have contributed to this wave of mergers. Average Medicare hospital prices fell initially with the institution of the Prospective Payment System in 1984 and again in 1988 with a strengthening in the requirements for elevated payments (Coulam and Gaumer 1991). The advent of managed care in the 1990s reduced hospital prices within the private insurance market (Cutler, McClellan, and Newhouse. 2000; Dor, Grossman, and Koroukian 2004; Shen and Melnick 2006). This fall in average prices coincided with a reduction in demand for hospital services.

Vogt and Town (2006) suggest that technological advances created an excess capacity by shifting many inpatient procedures to an outpatient setting and by reducing length of stay for other procedures. By creating a single legal entity, a merger enables capacity reduction through consolidating services between two facilities without being subject to state regulations (Dranove and Lindrooth 2003).

Background

Hospital mergers have the potential to affect quality of care through several mechanisms. If a merger creates financial benefits, the consolidated hospital may reinvest these gains into quality improvement. Likewise, service consolidation may increase procedure volume and consequently enhance provision of care. However, reducing competition may decrease the incentive to improve quality to attract patients. Finally, the disruption caused by unifying two independent facilities may negatively affect quality, particularly in the immediate aftermath of the merger.

<u>Financial Impacts</u>. Hospital mergers may create financial gains by achieving economies of scale and by resulting in higher negotiated prices. The literature suggests that hospital mergers, particularly facility-level mergers, are capable of attaining substantial cost savings (Conner et al. 1997; Dranove and Lindrooth 2003). In addition, both anecdotal and empirical evidence suggests that a merged facility has additional leverage to negotiate larger reimbursements from private insurance companies (Devers et al. 2003; Capps, Dranove, and Satterthwaite 2003; Dafny 2009). Consequently, the combination of cost efficiencies and higher prices provides a merged facility with more resources. While these resources may not be

invested in quality improvements, they are unlikely to reduce quality. Thus, the financial impact of a merger on quality of service should be, at worst, neutral.

Volume-Outcome Relationship. Hospital mergers often involve service consolidation—at minimum, physician and nursing staffs are unified between the facilities. If the gained experience and shared expertise of the combined staffs improve outcomes, then mergers have the potential to improve outcomes and quality through this channel as well. Gaynor (2006) provides a review of the recent volume-outcome studies within the economics literature. Several studies find that outcomes improve when surgeons perform more of a particular procedure (Gaynor, Seider, and Vogt 2004; Gaynor, Seider, and Vogt 2005; Ho 2002; Gowrisankaran, Ho, and Town 2006). Overall, this literature suggests that the service consolidation generated by many mergers and the resulting increase in volume at service-retaining facilities (including non-merging facilities) are likely to improve surgical outcomes.

Competition and Quality. Several theories suggest that hospital quality should increase with competition. In general, the mechanism behind this relationship is that consumers face little difference in out-of-pocket expenditures across hospitals, leading hospitals to compete for patients on a quality dimension rather than a financial one. Several of these theories are reviewed in Gaynor (2006).

Most of the recent empirical research on hospital competition and quality is consistent with these theories. A seminal paper, Kessler and McClellan (2000), uses Medicare heart attack data and a patient choice model, and finds that hospitals facing more competition have a lower incidence of adverse health events. Several researchers have built upon this foundation and have largely corroborated the linkage between competition and quality (Kessler and Geppert 2005; Sari 2002; Gowrisankaran and Town 2003). To the extent that the competition-quality relationship is causal, these findings suggest that the effects of the reduction in competition associated with a merger would offset the effects of the financial and volume-outcome mechanisms.

Consolidation and Quality. To date, two papers investigate the direct impact of hospital mergers on measures of quality (Ho and Hamilton 2000; Capps 2005). These studies use similar methodology: Both analyze the difference in quality measures before and after a merger as compared to those for non-merging hospitals. Both study 10 or 11 mergers across a five-year time period. Neither finds mergers to have a significant effect on quality, though the relatively small number of mergers and short time period studied may have reduced the power of their analyses. One concern with this hospital-based approach is that comparing merging hospitals to non-merging hospitals requires assuming that non-merging hospitals do not respond to the neighboring merger. The evidence that non-merging hospitals raise prices in response to nearby mergers (Dafny 2009) suggests that they may respond in the quality dimension as well. Another concern with this approach is the potential change in patient composition. If perceived changes in quality affect post-merger patient choices, then this approach may yield biased estimates.

Analytical Approach

This paper builds upon the previous literature in a number of ways. I address many of the concerns detailed above with a geographic approach: I measure health outcomes by zip code. By exploiting the variation in exposure to a merger across zip codes, I analyze the average impact of a merger on all patients in the study population. Exposure is measured as the share of patients in the zip code who are discharged by the merged facility. While this method cannot disentangle the effects of changes in the merged facility from changes in non-merging facilities or patient sorting, it will capture the full market-level impact.

Forty mergers transpired throughout California from 1990 to 2005. This analysis utilizes inpatient discharge data through 2006 to study the impact of these mergers on inpatient mortality and treatment intensity among heart disease patients. I extend this analysis by isolating the competition mechanism from overall exposure to a merger. Finally, I replicate the hospital-based analysis with this larger panel of mergers to compare its results with those from the geographic approach.

Data and Methods

Data

California's Office of Statewide Health Planning and Development (OSHPD) provided the annual inpatient discharge data set for this project. The data includes patient-level data on demographic characteristics, payment, diagnoses, and procedures performed for every inpatient discharge. I limit my sample to discharges from general acute facilities because treatment at long-term care facilities likely consists of rehabilitative care after the patient has been discharged from a general acute facility.

I restrict my analysis to patients with heart disease, allowing me to construct relevant outcome variables and co-morbidity factors to use as control variables. Limiting my analysis to patients with a heart attack (acute myocardial infarction, AMI) might seem to be ideal because AMI requires immediate hospitalization, minimizing potential selection bias in admission decisions. However, there are too few zip codes with enough AMI discharges in all quarters. Instead, I include all individuals with a chronic or acute diagnosis of ischemic heart disease

(IHD)[1] and create a balanced panel of 697 zip codes with at least 15 IHD discharges in all 68 quarters. Ninety percent of these zip codes are exposed to a merger, containing residents who were discharged from a merged facility during the study period. Because IHD varies in severity, hospitals may have different decision rules for admitting a heart disease patient. This issue should not be problematic unless a merger induces a change in admission criteria. I demonstrate below that mergers do not appear to have an impact on the admission criteria for IHD patients.

Table 1 reports descriptive statistics for IHD patients and their hospital experiences over the 1990 to 2005 period. This subset of discharges is disproportionately male, older, and covered by Medicare. Additionally, the percentage of discharges covered by private insurance fell by 25 percent during the study period. These statistics suggest conflicting implications for cardiac quality provision. Hospitals had a smaller share of patients for whom they could negotiate higher prices to fund quality investments yet faced stronger competitive pressures to provide quality from the Medicare market. Dafny (2005), however, found evidence that hospitals optimize quality decisions across all services rather than considering each diagnostic category as a separate market in which to compete.

Illness severity and treatment intensity both increased over the study period. The prevalence of diabetes, hypertension, and heart failure rose substantially, as did the likelihood of receiving bypass surgery or angioplasty. Average charges more than tripled, though these represent list prices rather than transaction prices. Inpatient mortality and average length of stay fell by 30 percent. It is notable that the number of hospitals treating IHD patients fell by nearly 20 percent while average IHD patient volume rose by 40 percent. These changes were likely driven by both hospital closures and mergers over the time period.

Background on California Hospital Mergers

Forty hospital mergers occurred between 1990 and 2005. I used the appendix on consolidations that OSHPD provided with the inpatient discharge data and internet research to identify and confirm each merger. Of these mergers, 27 involved nonprofit hospitals, 10 involved for-profit hospitals, and 3 involved county or district hospitals. Mergers were more likely to take place in urban areas such as Los Angeles or San Francisco, particularly during the 1990s. Merging hospitals were often within close proximity: 25 pairs were located within five miles of each other, with nearly all of the remainder within 15 miles. (The one exception involved a rural hospital in danger of closing.) Nearly one-third of mergers occurred simultaneously with ownership consolidation, and 10 mergers transpired within five years of ownership consolidation. The remainder involved hospitals under the same ownership for over five years or since 1990.[2]

An annual financial dataset provided by OSHPD suggests that consolidating facilities may consolidate services as well (see Appendix Table 1). For example, merged hospitals average 397 beds, while average pre-merge parent hospitals and satellites average 264 and 168, respectively. Likewise, several diagnostic and other service offerings overlap. Roughly one-half of parents and one-quarter of satellites offer neonatal intensive care units, compared to 65 percent of merged hospitals. Two-thirds of merged facilities offered MRI services, compared to 54 percent of parents and 43 percent of satellites. This pattern suggests that hospitals may reduce overlapped service provision to reduce costs.

The effect of a merger on the local competitive landscape varies. The Eden Medical Center/San Leandro Hospital merger in 2004 had little impact on concentration because of limited overlap in the two facilities' markets. Despite individual shares above 10 percent in several zip codes, the greatest change in the Herfindahl index (HHI), or sum of squared market shares, was from 0.12 to 0.20. The St. Joseph Hospital-Eureka/General Hospital of Eureka merger in 2000, however, greatly increased local concentration. Several zip codes experienced HHI increases greater than 0.15, resulting in HHIs of 0.88 and market shares of 94 percent. The competitive impact of other mergers fell between these two examples.

Empirical Framework

Outcomes Analyzed. My analysis examines the impact of hospital mergers on treatment intensity, the likelihood of receiving treatment within one day, and inpatient mortality. The primary intensity measure is the percentage of patients receiving bypass surgery or angioplasty. Hospital mergers may increase use of these intensive heart surgeries if they become more accessible after the merger. Eighteen merging pairs offered cardiac surgery services, 2 with overlapping cardiac surgery capabilities and 16 without. Consolidation of all cardiology services for these 16 facilities would make these procedures more accessible to those who would have otherwise chosen the non-offering facility. But while a patient may be more likely to choose a hospital with these capabilities when she expects to need them, such as when she may be experiencing a heart attack, distance to the hospital may sometimes be a greater priority.

The number of procedures a patient experiences during a hospital stay is another measure of treatment intensity, and the likelihood of receiving treatment within one day could reflect quality in that delays may be associated with treatment by less-qualified physicians (Doyle et al. 2006), overcrowding, or understaffing. Inpatient mortality is the final outcome measure. While it is a low probability and an extreme adverse event, it is also the clearest measure of poor outcomes. Because discharge practices may affect inpatient mortality, I include average length of stay as a companion outcome.

Identification Issues. Identifying the causal impact of a merger within a hospital is difficult because a merger may cause patient reallocation, confounding the identification of outcome or treatment changes. I use the term "merged share" of the market to refer to the market share of the merged facility in a zip code exposed to a merger. Figure 1 demonstrates the varied responses to hospital mergers in two communities. Some mergers result in a widespread decline in merged shares over one year, as illustrated by the Eden Medical Center/San Leandro Hospital merger in 2004, and others prompt a more varied response, such as the 1999 Fresno Community Hospital/Valley Medical Center merger. This heterogeneity is unlikely to be entirely random, suggesting that patient composition changes could bias a hospital-based analysis.

Reallocation could stem from two sources. The closest facility offering a needed service might no longer be one of the merged facilities, or the public's perception of a merged facility's quality relative to other local hospitals could change. Sicker patients might be willing to travel farther for quality care, causing a negative correlation between perceived quality change and average patient health at a merged facility. While the first scenario is unlikely to bias hospital-based analyses, the latter could bias quality change estimates.

I analyze outcomes within geographic areas because the population within a defined geographic area is unlikely to change in average health status in response to a merger. Previous hospital merger research has used geographic areas such as counties or has incorporated overlapping fixed radius market definitions. Both of these methods have drawbacks. Counties are large enough that individuals on opposite ends of the market face a different set of nearby hospital choices, and individuals near a market border may also be closer to a hospital in another market. Fixed radius definitions are difficult to establish because hospitals of different sizes and service offerings have different geographic reaches.

Unlike counties or fixed radius definitions, zip codes are generally small enough that all individuals face a similar set of relevant hospital choices, and patient choices determine the relevant hospitals. Exposure to a merger is measured as the share of patients within a zip code discharged from a merged facility. This zip code-level analysis exploits the heterogeneity of exposure to a merger to measure its effect. Merger effects on quality or treatment practices should be more visible in zip codes with higher merged shares. And because mergers are unlikely to induce changes in residence, I can compare pre- and post-merger outcomes for plausibly similar populations.

The extent to which outside factors may affect both hospital mergers and outcomes represents a limitation to this approach. For example, local changes in the insurance market or physician/nurse labor markets could affect care outcomes and induce hospitals to merge. Because these types of factors are unobserved, I cannot include them in my analysis. A zip code-level analysis also encompasses not only the direct effect on patients using the merged facility but also the merger's ripple effects. These ripple effects include hospital responses to changes in the competitive landscape and patient responses to relative changes in quality. While I cannot disentangle the direct effect of a merger on the merged facility's quality from the indirect effects of patient reallocation and non-merging hospital changes, I can assess the merger's market-level impact.

<u>Instrumental Variables Strategy.</u> Relying on merged market shares is also subject to selection bias because shares respond to hospital choice. I use a solution similar to the approach used by Town et al. (2006) and predict post-merger shares with the sum of merging facility

shares from one year before the merger.[3] Even if merger discussions had begun one year before, it is unlikely that this information would be publicized, rendering patient choices unaffected by the impending merger. As described above, the baseline ordinary least squares (OLS) model is:

$$(1)\ Y_{zt} = \alpha_1 + \theta_{1z} + \phi_{1t} + \gamma_1 X_{zt} + \beta_1 merged\_share_{zt} + \varepsilon_{1zt}$$

The relationship between the endogenous explanatory variable and its instrument is modeled as:

$$(2)\ merged\_share_{zt} = \alpha_2 + \theta_{2z} + \phi_{2t} + \gamma_2 X_{zt} + \beta_2 pre\_share_{zt} + \varepsilon_{2zt}$$

The predicted exposure variable, pre_share_{zt}, equals zero before a merger has transpired and accumulates as mergers affect a zip code.[4] For example, in a zip code that experiences two mergers, pre_share_{zt} equals zero until the first merger is completed. Between the first merger and the second merger, pre_share_{zt} equals the combined market share of the first pair of merging facilities from one year before the merger was completed. After the second merger is completed, the combined market share of the second pair of merging facilities from one year before that merger was completed is added to pre_share_{zt}. Thus, the instrumental variable (IV) estimates will only use the variation in post-merge shares that comes from the expected impact of the merger ex-ante, alleviating the selection bias concern.

The outcomes described above are represented by Y_{zt} in equation (1). I control for several demographic characteristics such as average age and the percentage of zip code discharges in categories for race, gender, and insurance status. I include the percentage of discharges with co-morbidity diagnoses such as diabetes, heart failure, and pneumonia. These control variables are represented by X_{zt}. I also include quarter and zip code fixed effects (ϕ_t and θ_z respectively), and some specifications include hospital referral region (HRR)-specific trends to account for differing trends in local treatment and health patterns. Standard errors are clustered by zip code.

This model is identified on four main assumptions. First, while the specific hospitals that merge are endogenous, merger timing is not. As described above, completing a merger involves a complex negotiation process; thus, it is difficult to predict when a merger will be finalized, regardless of when negotiations begin. Second, the merger does not affect the composition of patients residing within a zip code. While available hospitals and their quality may affect housing choices, a merger is unlikely to induce a widespread decision to relocate. The third assumption is that pre-merge shares of discharges are not affected by the impending merger. As a robustness check, I performed the same analysis using shares from two years before the merger. The results from this analysis are very similar in magnitude and significance to those detailed later in this paper.

The fourth assumption is that merging hospitals do not change their admission criteria for IHD patients. I test this assumption by regressing the share of non-discretionary admissions on pre-merge shares. Non-discretionary admissions, by definition, should not be affected by a change in admission policies. Thus, their share of admissions would rise with a tightening of admission criteria and vice versa. I approximate non-discretionary admissions with AMI admissions and admissions through the ER. Results are mixed without HRR trends, as percent AMI yields a statistically significant coefficient of 0.023 and percent admitted through the ER yields a statistically insignificant coefficient of -0.007. However, controlling for local trends yields statistically insignificant coefficients of 0.008 and 0.011.[5] In aggregate, these results suggest that admission practices at merged facilities changed little, if at all, after a merger.

Results and Discussion

Table 2 reports the first-stage results between predicted and actual merged shares. Regression results for this table and all others are reported in pairs, without and with control variables included. Columns (1)-(2) and (5)-(6) demonstrate that the coefficient on pre-merge share is a precisely estimated 0.84 with or without trends. The large F-statistics suggest a strong first-stage relationship. Columns (3)-(4) and (7)-(8) report the results from interacting the predicted exposure variable with five timing variables; these specifications are included to show how merged facilities' share of discharges change with time and are not used in the IV estimation. Merged facilities appear to retain most of their market share several years after the merger transpires.

Table 3 reports the ordinary least squares (OLS) and IV regression results for each of the five dependent variables. The top two panels report the OLS results, while the bottom two panels report the IV results. Columns (1)-(2) of the first two panels suggest that hospital mergers increase use of intensive heart surgeries, regardless of trend inclusion. The average zip code exposed to a merger has a merged share of 0.204. The coefficient of 0.050 in column (2) of the second panel implies that intensive surgery utilization increased by 1.0 percentage points when the merged share increased from zero to its average after being exposed to a merger. This point estimate represents a 3.7 percent increase above the 2000 average of 27.3 percent. The year 2000 was selected for a reference point because it is near the middle of the study period and three-quarters of mergers were completed by the end of this year. Zip codes with larger exposure to a merger were affected proportionally to their share.

The next four columns in Table 3 list the coefficients for receiving primary treatment within one day and the average number of procedures. Both coefficients are statistically significant in the specifications without trends, yet their magnitudes fall substantially and the coefficients for treatment within one day are no longer statistically significant once trends are included. For the average zip code, the specifications with trends suggest that individuals are not more likely to receive treatment within one day and that they receive 2.0 percent more procedures.

The final two outcomes—inpatient mortality and average length of stay—are listed in the remaining columns. These outcomes are discussed together because hospital discharge practices could affect inpatient mortality rates. A decrease in inpatient mortality might be questionable coupled with a decrease in average length of stay if a hospital changed its discharge practices such that dying patients were more likely to be discharged to die at home. Indeed, the OLS results suggest a minimal and marginally statistically significant effect on inpatient mortality and average length of stay. The point estimates suggest a 1.1 percent increase in inpatient mortality and less than a 1 percent increase in average length of stay for the average zip code, though neither estimate is precise.

OLS results may be biased if individuals respond to a *perceived* quality change. The last two panels of Table 3 report the IV specification results. The IV specifications suggest a similar or slightly larger impact on treatment intensity. The coefficients for surgical utilization remain the same, though the coefficients for receiving treatment within one day and number of procedures are larger. For example, the coefficient of 0.392 in column (6) of panel 4 suggests that the average number of procedures rises by 2.9 percent for the mean zip code. Since surgery utilization increases 28 percent more than the number of procedures, the increase in surgeries is probably replacing less intensive procedures in some cases and adding to them in others.

The difference between OLS- and IV-estimated impacts on inpatient mortality is more striking. The coefficient on merged share increases by 50 percent to 0.003 without and with trends, though it is not quite significant at the 5 percent level in the latter. For the average zip code, this point estimate suggests an increase in inpatient mortality of 0.0006 percentage points, or a 1.7 percent increase above the 2000 average of 3.7 percent. The coefficients for average length of stay increase as well, and the specification with trends suggests that length of stay increases by 1.3 percent and may contribute to some of the increase in inpatient mortality if dying patients are less likely to be discharged to die at home.

As a specification check, I estimated a reduced form model in which the instrument, pre-merge share, was interacted with timing dummy variables for pre- and post-merger time periods. Appendix Table 2 suggests that the effect of a merger is not transitory, nor is there a change in the outcomes before the merger transpires. The one exception is for the likelihood of receiving treatment within one day. In these specifications, as in the OLS and IV specifications, the coefficients for the timing interacted share variables fall by half or more and lose statistical significance when trends are included.

Isolating the Competitive Effect

The main specification does not allow for attributing outcomes to any particular causal mechanism. To explore the effects of changes in the competitive environment on hospital outcomes, I add HHI as an explanatory variable, instrumented with predicted change in HHI. Predicted change in HHI is constructed by calculating the change in HHI from treating merging facilities as a single entity. Like the instrument for post-merger shares described above, predicted change in HHI is calculated using shares from one year before the merger. The strength of the instruments remains strong with this addition; the F-statistics are well above 10 for each of the relevant first-stage regressions, and their predictive power continues in the years following the merger (see Appendix Table 3).

The first two panels of Table 4 report the results from these IV specifications without and with trends. These results suggest that overall exposure drives the intensive surgery result, while increased concentration drives the increase in procedures. These specifications also suggest a much larger impact on number of procedures. The mean HHI in a zip code affected by a merger is 0.285, so the coefficient of 3.298 implies an increase of 0.9 procedures—a 34 percent increase.

The mortality results in these specifications are especially striking. The coefficient on merged share is a solidly statistically significant 0.007, while the coefficient on HHI is a statistically insignificant -0.02. Non-competitive mechanisms appear to drive mortality increases, while consolidation may reduce mortality. The merged share coefficient suggests a 3.9 percent increase in mortality. These specifications suggest that average length of stay is unaffected.

Hospital-Based Analysis

Thus far, the source of the differences between the inpatient mortality results reported above and those reported in the previous literature is unclear. I have asserted that the geographical approach enables me to avoid bias from patient composition changes and to capture the effect of a merger on non-merging hospitals. However, the larger data set in this study could allow for more precision outside of any methodological differences. To explore this issue, I follow the approach used by Ho and Hamilton (2000). The outcome variables are regressed on the same set of control variables, and a binary variable identifies discharges from a merged facility. Hospital fixed effects and trends replace zip code fixed effects and HRR trends.[6] While these are individual-level specifications, I label this approach "hospital-based" because results are driven by differences between merging and non-merging hospitals.

In this analysis, continuous variables such as length of stay are discharge-specific, and binary variables such as inpatient mortality are indicator variables. Following the Ho and Hamilton analysis, I also control for hospital volume. Computing limitations restrict me to a 5 percent sample of IHD discharges.

The last two panels of Table 4 report these results. Without hospital trends, merged facilities appear to increase treatment intensity and promptness relative to non-merging hospitals. However, the inclusion of trends substantially reduces the magnitude and statistical significance of these effects. In both cases, the mortality coefficients are small and statistically insignificant; thus, the longer time span and additional mergers do not yield similar results between the hospital-based and geographic approaches, particularly for the mortality results.

Discussion

The results detailed above suggest that hospital mergers are associated with greater treatment intensity, both in the type of treatment performed and in the number of procedures patients received during a hospital stay. The competition mechanism appears to drive the increase in procedures, while general merger exposure appears to drive the increased utilization of intensive surgeries. Mergers are also associated with increased inpatient mortality, particularly when the competition channel is isolated. Because the empirical methodology does not isolate other channels through which mergers might affect outcomes, I can only speculate on the mechanisms in play. Merger-induced price increases may lead to infrastructure expansion and, consequently, increased treatment intensity. However, mergers also allow for cost cutting in the form of infrastructure consolidation, requiring some patients to travel farther for care. Additional travel time, even in urban and hospital-dense areas, has been found to increase mortality from heart attacks (Buchmueller et al. 2006). These results differ from those found in the previous literature in that I find statistically significant increases in inpatient mortality. The results in the last section of this paper suggest that the larger data set is not driving the difference in results. I believe the difference is, instead, driven by some combination of not comparing merging facilities to affected non-merging facilities and estimating models that are less susceptible to bias from changes in patient composition.

It is difficult to discern from the results listed above whether the increases in treatment intensity enhance or reduce welfare. Increased provision of heart surgeries may improve the length or quality of life for some individuals, and the volume-outcome literature suggests that outcomes may improve with the additional surgical volume. However, any improvements must be weighed against the additional cost of providing the surgeries. Additionally, previous research has found a negative relationship between more intensive treatment and quality of care for heart attack patients (Fisher et al. 2003). Furthermore, it is possible that increases in treatment intensity are related to increases in mortality. Access to readmission rates and 30-day mortality rates may shed further light on this question, though it would remain difficult to weigh improvements in quality against the costs of more intensive treatments. This question requires further investigation.

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[1] I further restrict the sample to IHD discharges with a cardiac major diagnosis code; I assume that a non-cardiac admission with IHD includes those for whom heart disease is merely a complicating factor and for those who develop an AMI in response to complications from another disease or procedure. Both groups are likely to be distinctly different from those included in this sample.

[2] Eight mergers were completed before 1995 without an ownership change after 1990. Because my data begin in 1990, I cannot track ownership changes for earlier years.

[3] The earliest possible quarter is used for the four mergers that were completed less than one year into the study period.

[4] All share variables are based on heart disease diagnoses. Using shares of all discharges yields similar results.

[5] Regressions using the share of population discharged (overall and aged 65+) as the dependent variable yield similar results.

[6] The same fixed effects were used for the two hospitals in a merging pair because the data do not distinguish between the two hospitals after the merger.

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Table 1: Descriptive Statistics for All Ischemic Heart Disease Diagnoses for Selected Years

Year	1990	1995	2000	2005
% With the Following Demographic Characteristics:				
Male	58.2	58.4	57.2	58.4
Black	6.6	6.7	6.5	7.2
Hisp	9.0	11.4	11.5	14.5
Ages 60-74	43.6	41.7	36.9	35.1
Ages 75+	31.1	34.7	40.8	41.6
% With the Following Expected Payer:				
Medicare	57.2	58.3	61.7	62.9
Private	30.5	28.9	25.8	22.9
Average Number of Procedures	2.9	2.8	2.8	3.0
Average Length of Stay (days)	6.2	5.1	4.6	4.4
Average Charges ($)	17395	25431	42557	66389
Average Charges (1990$)	17395	21810	32301	44430
Received Bypass Surgery or Angioplasty	22.4	25.8	27.3	27.9
% Treat Within 1 Day	53.8	52.7	47.3	47.9
% With the Following Co-morbidity Diagnoses:				
Heart Attack	25.5	25.9	26.4	24.2
Pneumonia	2.0	2.6	3.3	4.7
Diabetes	21.2	27.0	32.8	38.4
Heart Failure	25.8	31.2	32.6	36.8
Hypertension	34.2	49.4	60.3	72.0
% With the Following Admission & Discharge Characteristics:				
Admitted from (own) ER	55.2	53.6	59.3	61.6
Discharged Home	73.0	68.5	67.1	67.1
Die	4.6	4.0	3.7	3.2
Average # Ischemic Discharges	1122	1303	1523	1555
Average # Ischemic Discharges (unweighted)	508	588	695	713
# Hospitals	461	448	408	382
# Mergers Completed (Cumulative)	4	14	31	40

All averages are weighted by number of discharges unless otherwise specified. The data sample includes all discharges with a major diagnostic category of “circulatory system, diseases & disorders” (5) and any diagnostic code of “Ischemic Heart Disease” (ICD-9 codes 410-414) in relevant hospitals. Numbers of hospitals and mergers are as of the end of the calendar year.

Table 2: First-Stage Relationship between Merged Share and Pre-Merge Share

	without hospital referral region trends				with hospital referral region trends			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pre-Merge Share	0.839	0.838			0.834	0.835		
	(0.020)***	(0.020)***			(0.021)***	(0.021)***		
Pre-Merge Share * Year 1 (post)			0.823	0.822			0.819	0.818
			(0.022)***	(0.021)***			(0.022)***	(0.022)***
Pre-Merge Share * Year 2 (post)			0.807	0.805			0.802	0.801
			(0.023)***	(0.022)***			(0.024)***	(0.023)***
Pre-Merge Share * Year 3 (post)			0.817	0.815			0.812	0.811
			(0.022)***	(0.022)***			(0.023)***	(0.023)***
Pre-Merge Share * Year 4 (post)			0.807	0.806			0.803	0.802
			(0.021)***	(0.021)***			(0.022)***	(0.022)***
Pre-Merge Share * Year 5+ (post)			0.794	0.793			0.781	0.782
			(0.023)***	(0.023)***			(0.025)***	(0.025)***
R-squared	0.92	0.92	0.92	0.92	0.93	0.93	0.92	0.92
F statistic for instrument(s)	1730.85	1761.39	323.86	331.03	1575.2	1600.73	296.07	301.49
Control Variables		X		X		X		X
Observations	47396	47396	47396	47396	47396	47396	47396	47396

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Zip codes are only included if they have 15 or more discharges in every quarter. All regressions include quarter and zip code fixed effects and are weighted by the number of heart disease discharges. Standard errors are clustered by zip code. Control variables include the percentage of discharges with demographic characteristics such as race, gender, and age categories; with expected payer such as Medicare and private insurance; with co-morbidities such as hypertension, diabetes, and heart failure; admission through the ER; and a constant.

Table 3: Regression Results

OLS without hospital referral region trends:	% Received Intensive Treatment		% Treated Within One Day		Average # Procedures		% Die in Hospital		Average Length of Stay	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Merged Share	0.048	0.043	0.084	0.077	0.522	0.452	0.005	0.002	0.230	0.054
	(0.010)***	(0.009)***	(0.018)***	(0.018)***	(0.122)***	(0.122)***	(0.001)***	(0.001)*	(0.145)	(0.102)
R-squared	0.6	0.68	0.48	0.52	0.48	0.52	0.11	0.16	0.69	0.69
OLS with hospital referral region trends:	% Received Intensive Treatment		% Treated Within One Day		Average # Procedures		% Die in Hospital		Average Length of Stay	
Merged Share	0.048	0.050	0.013	0.016	0.285	0.276	0.003	0.002	0.277	0.221
	(0.008)***	(0.008)***	(0.021)	(0.022)	(0.120)**	(0.121)**	(0.002)*	(0.001)	(0.126)**	(0.119)*
R-squared	0.62	0.69	0.52	0.55	0.53	0.56	0.11	0.16	0.69	0.7
IV without hospital referral region trends:	% Received Intensive Treatment		% Treated Within One Day		Average # Procedures		% Die in Hospital		Average Length of Stay	
Merged Share	0.043	0.041	0.109	0.106	0.593	0.533	0.007	0.003	0.330	0.195
	(0.009)***	(0.008)***	(0.019)***	(0.018)***	(0.122)***	(0.119)***	(0.002)***	(0.001)**	(0.155)**	(0.144)
R-squared	0.6	0.68	0.48	0.52	0.48	0.52	0.11	0.16	0.69	0.69
IV with hospital referral region trends:	% Received Intensive Treatment		% Treated Within One Day		Average # Procedures		% Die in Hospital		Average Length of Stay	
Merged Share	0.045	0.050	0.029	0.035	0.395	0.392	0.005	0.003	0.414	0.300
	(0.010)***	(0.008)***	(0.022)	(0.022)	(0.125)***	(0.124)***	(0.002)***	(0.002)*	(0.124)***	(0.116)***
R-squared	0.62	0.69	0.52	0.55	0.53	0.56	0.11	0.16	0.69	0.7
Control Variables		X		X		X		X		X
Observations	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Zip codes are only included if they have 15 or more discharges in every quarter. All regressions include quarter and zip code fixed effects and are weighted by the number of heart disease discharges. Standard errors are clustered by zip code. Control variables include the percentage of discharges with demographic characteristics such as race, gender, and age categories; with expected payer such as Medicare and private insurance; with co-morbidities such as hypertension, diabetes, and heart failure; admission through the ER; and a constant.

Table 4: Alternate Specifications

IV with HHI; without Hospital Referral Region Time Trends:

	%Received Intensive Treatment		% Treated Within One Day		Average # Procedures		% Die in Hospital		Average Length of Stay	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Merged Share	0.037	0.023	0.086	0.065	0.332	0.167	0.009	0.006	0.322	0.198
	(0.012)***	(0.010)**	(0.029)***	(0.028)**	(0.171)*	(0.169)	(0.003)***	(0.002)***	(0.225)	(0.206)
HHI	0.045	0.136	0.163	0.317	1.853	2.772	-0.013	-0.020	0.055	-0.024
	(0.077)	(0.070)*	(0.179)	(0.200)	(0.925)**	(1.054)***	(0.015)	(0.015)	(1.025)	(0.951)
R-squared	0.60	0.68	0.47	0.49	0.46	0.49	0.11	0.16	0.69	0.69

IV with HHI; with Hospital Referral Region Time Trends:

	%Received Intensive Treatment		% Treated Within One Day		Average # Procedures		% Die in Hospital		Average Length of Stay	
Merged Share	0.056	0.040	-0.052	-0.067	-0.106	-0.207	0.005	0.007	0.396	0.321
	(0.015)***	(0.013)***	(0.033)	(0.034)*	(0.185)	(0.190)	(0.003)	(0.003)**	(0.238)*	(0.218)
HHI	-0.061	0.053	0.438	0.563	2.703	3.298	0.000	-0.020	0.096	-0.118
	(0.079)	(0.068)	(0.171)**	(0.186)***	(0.786)***	(0.843)***	(0.016)	(0.016)	(1.039)	(0.983)
R-squared	0.61	0.70	0.47	0.49	0.49	0.52	0.11	0.16	0.69	0.70
Observations	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396

Hospital-Level Without Hospital-Specific Time Trends:

	%Received Intensive Treatment		% Treated Within One Day		Average # Procedures		% Die in Hospital		Average Length of Stay	
Merged Share	0.058	0.059	0.032	0.029	0.378	0.371	-0.001	-0.001	0.250	0.265
	(0.024)**	(0.020)***	(0.015)**	(0.015)*	(0.191)**	(0.189)*	(0.003)	(0.003)	(0.185)	(0.171)
R-squared	0.22	0.33	0.04	0.08	0.16	0.23	0.01	0.06	0.09	0.12

Hospital-Level With Hospital-Specific Time Trends:

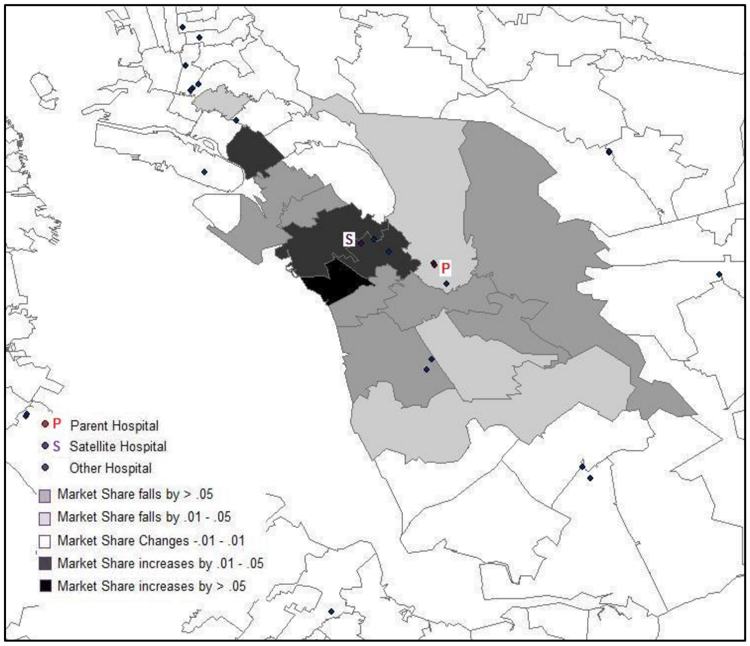
	%Received Intensive Treatment		% Treated Within One Day		Average # Procedures		% Die in Hospital		Average Length of Stay	
Merged Share	0.016	0.021	0.004	0.006	0.087	0.122	0.000	0.000	0.094	0.186
	(0.021)	(0.019)	(0.014)	(0.014)	(0.157)	(0.153)	(0.003)	(0.003)	(0.148)	(0.138)
R-squared	0.23	0.34	0.05	0.09	0.18	0.25	0.01	0.06	0.14	0.16
Observations	203139	203139	146765	146765	203139	203139	203139	203139	200296	200296
Control Variables		X		X		X		X		X

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

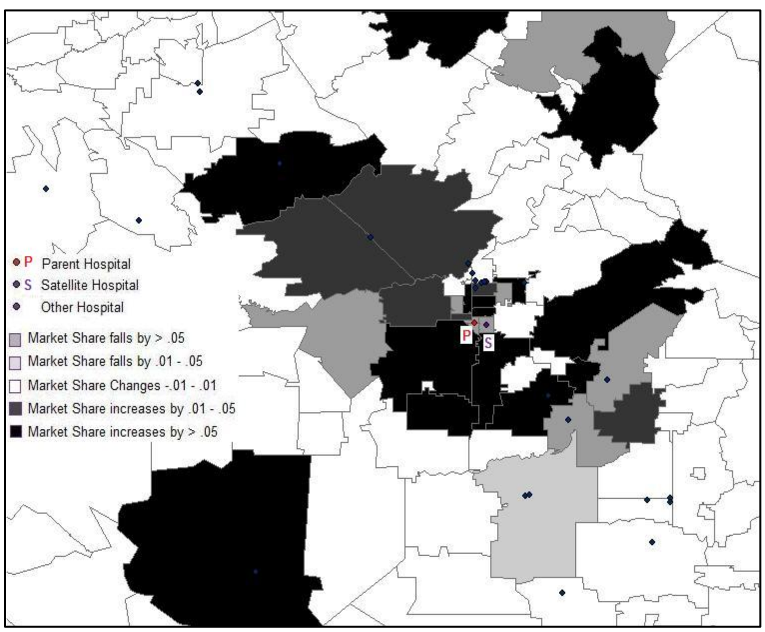
All regressions include quarter and zip code or hospital fixed effects as applicable. Because satellite facilities cannot be tracked separately from the parent post-merger, facility pairs that merge in future are treated as a single facility for fixed effect purposes. Standard errors are clustered by zip code or hospital as applicable. Control variables include the percentage of discharges with demographic characteristics such as race, gender, and age categories; with expected payer such as Medicare and private insurance; with co-morbidities such as hypertension, diabetes, and heart failure; admission through the ER; and a constant.

Figure 1: Changes in Merged Share of the Market Over One Year

A: Merger of Eden Medical Center and San Leandro Hospital in San Leandro



B: Merger of Fresno Community Hospital and Valley Medical Center in Fresno



Note: Merged share of the market is the market share of the merged facility in a zip code exposed to a merger. Only zip codes with at least 15 heart disease discharges in the merging quarter are shaded.

Appendix Table 1: Comparing Merging and Non-Merging Hospitals - An Overview

	Parent Hospital Year Before Merge	Satellite Hospital Year Before Merge	Merged Hospital Year Afer Merge	Non-Merging Hospitals (1995)
Beds:				
Licensed	264	168	397	207
Available	232	145	339	185
Delivery Room	78.4%	48.6%	91.9%	54.6%
NICU	51.4%	27.0%	64.9%	29.1%
Surgery Services:				
Neurological	78.4%	54.1%	83.8%	44.8%
Heart	33.3%	13.9%	41.7%	28.1%
Open Heart	32.4%	8.1%	37.8%	22.6%
Organ Transplant	13.5%	10.8%	21.6%	7.9%
MRI	54.1%	43.2%	67.6%	37.7%
Emergency Room	89.2%	81.1%	91.9%	65.6%

* Note that 3 mergers are excluded because pre-merger data was unavailable for the satellite facility.

Appendix Table 2: Regressions on Timing Interacted Pre-Merge Share

Without Hospital Referral Region Code-Specific Time Trends:	%Received Intensive Treatment		% Treated Within One Day		Average # Procedures		% Die in Hospital		Average Length of Stay	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pre-Merge Share * Year 1 (pre)	-0.002	0.000	0.046	0.045	0.166	0.139	0.004	0.002	-0.166	-0.136
	(0.007)	(0.006)	(0.014)***	(0.012)***	(0.111)	(0.100)	(0.002)*	(0.002)	(0.106)	(0.108)
Pre-Merge Share * Year 1 (post)	0.030	0.032	0.047	0.048	0.190	0.166	0.006	0.003	-0.055	-0.035
	(0.007)***	(0.006)***	(0.016)***	(0.014)***	(0.095)**	(0.090)*	(0.002)**	(0.002)	(0.113)	(0.110)
Pre-Merge Share * Year 2 (post)	0.032	0.030	0.058	0.055	0.305	0.278	0.003	0.003	-0.028	-0.003
	(0.008)***	(0.006)***	(0.018)***	(0.017)***	(0.118)***	(0.111)**	(0.002)	(0.002)	(0.127)	(0.113)
Pre-Merge Share * Year 3 (post)	0.029	0.031	0.069	0.070	0.364	0.349	0.006	0.004	0.059	0.036
	(0.008)***	(0.007)***	(0.017)***	(0.017)***	(0.124)***	(0.118)***	(0.002)***	(0.002)*	(0.123)	(0.115)
Pre-Merge Share * Year 4 (post)	0.039	0.037	0.083	0.082	0.551	0.495	0.007	0.003	0.253	0.144
	(0.009)***	(0.008)***	(0.018)***	(0.017)***	(0.113)***	(0.107)***	(0.002)***	(0.002)	(0.141)*	(0.137)
Pre-Merge Share * Year 5+ (post)	0.042	0.037	0.139	0.133	0.780	0.688	0.006	0.002	0.501	0.297
	(0.011)***	(0.010)***	(0.018)***	(0.017)***	(0.119)***	(0.118)***	(0.002)***	(0.002)	(0.188)***	(0.177)*

With Hospital Referral Region Code-Specific Time Trends:	%Received Intensive Treatment		% Treated Within One Day		Average # Procedures		% Die in Hospital		Average Length of Stay	
Pre-Merge Share * Year 1 (pre)	-0.002	0.005	0.016	0.022	0.100	0.116	0.003	0.002	-0.083	-0.075
	(0.007)	(0.006)	(0.015)	(0.014)	(0.109)	(0.102)	(0.002)	(0.002)	(0.111)	(0.110)
Pre-Merge Share * Year 1 (post)	0.028	0.035	0.012	0.019	0.102	0.120	0.004	0.002	0.039	0.053
	(0.008)***	(0.007)***	(0.017)	(0.016)	(0.099)	(0.095)	(0.002)*	(0.002)	(0.120)	(0.114)
Pre-Merge Share * Year 2 (post)	0.032	0.035	0.014	0.019	0.202	0.217	0.002	0.002	0.087	0.111
	(0.009)***	(0.007)***	(0.020)	(0.019)	(0.118)*	(0.114)*	(0.002)	(0.002)	(0.129)	(0.118)
Pre-Merge Share * Year 3 (post)	0.030	0.038	0.015	0.023	0.252	0.282	0.005	0.003	0.179	0.155
	(0.009)***	(0.008)***	(0.020)	(0.020)	(0.124)**	(0.122)**	(0.003)*	(0.002)	(0.117)	(0.107)
Pre-Merge Share * Year 4 (post)	0.040	0.045	0.021	0.028	0.419	0.413	0.005	0.003	0.385	0.272
	(0.011)***	(0.009)***	(0.021)	(0.020)	(0.117)***	(0.112)***	(0.002)**	(0.002)	(0.137)***	(0.135)**
Pre-Merge Share * Year 5+ (post)	0.050	0.052	0.052	0.055	0.607	0.579	0.004	0.002	0.590	0.387
	(0.011)***	(0.010)***	(0.023)**	(0.024)**	(0.127)***	(0.129)***	(0.002)**	(0.002)	(0.146)***	(0.142)***
Control Variables		X		X		X		X		X

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Zip codes are only included if they have 15 or more discharges in every quarter. All regressions include quarter and zip code fixed effects and are weighted by the number of heart disease discharges. Standard errors are clustered by zip code. Control variables include the percentage of discharges with demographic characteristics such as race, gender, and age categories; with expected payer such as Medicare and private insurance; with co-morbidities such as hypertension, diabetes, and heart failure; admission through the ER, and a constant.

Appendix Table 3: First-Stage Relationship between HHI, Merged Share and Predicted Change in HHI, Pre-Merge Share

	without hospital referral region trends								with hospital referral region trends							
	HHI		Merged Share		HHI		Merged Share		HHI		Merged Share		HHI		Merged Share	
	(1)	(2)	(3)	(4)	(5)	(6)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Pre-Merge Share	0.012	0.008	0.844	0.841					0.046	0.051	0.836	0.836				
	(0.021)	(0.019)	(0.025)***	(0.025)***					(0.021)**	(0.019)***	(0.026)***	(0.026)***				
Predicted Change in HHI	0.720	0.690	-0.035	-0.021					0.668	0.623	-0.009	-0.010				
	(0.151)***	(0.146)***	(0.153)	(0.152)					(0.137)***	(0.130)***	(0.135)	(0.134)				
Pre-Merge Share * Year 1 (post)					0.016	0.001	0.804	0.800					0.040	0.031	0.792	0.790
					(0.020)	(0.018)	(0.030)***	(0.029)***					(0.020)**	(0.018)*	(0.030)***	(0.030)***
Pre-Merge Share * Year 2 (post)					0.024	0.019	0.791	0.786					0.053	0.055	0.778	0.777
					(0.020)	(0.018)	(0.031)***	(0.031)***					(0.020)***	(0.018)***	(0.032)***	(0.032)***
Pre-Merge Share * Year 3 (post)					0.034	0.016	0.792	0.787					0.058	0.051	0.779	0.778
					(0.025)	(0.022)	(0.029)***	(0.029)***					(0.024)**	(0.021)**	(0.029)***	(0.029)***
Pre-Merge Share * Year 4 (post)					0.005	-0.001	0.794	0.790					0.030	0.036	0.782	0.782
					(0.021)	(0.019)	(0.030)***	(0.030)***					(0.021)	(0.020)*	(0.031)***	(0.031)***
Pre-Merge Share * Year 5+ (post)					-0.006	-0.001	0.775	0.773					0.023	0.039	0.758	0.759
					(0.020)	(0.019)	(0.027)***	(0.027)***					(0.022)	(0.022)*	(0.030)***	(0.030)***
Predicted Change in HHI * Year 1 (post)					0.782	0.779	0.140	0.163					0.756	0.745	0.188	0.193
					(0.151)***	(0.147)***	(0.152)	(0.153)					(0.144)***	(0.135)***	(0.144)	(0.144)
Predicted Change in HHI * Year 2 (post)					0.707	0.659	0.119	0.142					0.669	0.610	0.162	0.166
					(0.157)***	(0.152)***	(0.166)	(0.166)					(0.151)***	(0.140)***	(0.156)	(0.155)
Predicted Change in HHI * Year 3 (post)					0.738	0.756	0.171	0.193					0.721	0.715	0.207	0.211
					(0.167)***	(0.164)***	(0.169)	(0.169)					(0.153)***	(0.146)***	(0.153)	(0.154)
Predicted Change in HHI * Year 4 (post)					0.693	0.671	0.096	0.114					0.681	0.631	0.131	0.129
					(0.156)***	(0.155)***	(0.171)	(0.171)					(0.145)***	(0.140)***	(0.156)	(0.156)
Predicted Change in HHI * Year 5+ (post)					0.740	0.693	0.129	0.135					0.689	0.619	0.140	0.138
					(0.162)***	(0.155)***	(0.168)	(0.166)					(0.143)***	(0.135)***	(0.145)	(0.144)
R-squared	0.830	0.840	0.920	0.920	0.830	0.840	0.920	0.920	0.840	0.850	0.930	0.930	0.840	0.850	0.920	0.920
F statistic for instrument(s)	16.31	16.28	969.86	987.99	5.05	5.6	180.86	185.98	21.65	24.32	852.92	863.93	6.1	7.14	160.74	163.72
Control Variables		X		X		X		X		X		X		X		X
Observations	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396	47396

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Zip codes are only included if they have 15 or more discharges in every quarter. All regressions include quarter and zip code fixed effects and are weighted by the number of heart disease discharges. Standard errors are clustered by zip code. Control variables include the percentage of discharges with demographic characteristics such as race, gender, and age categories; with expected payer such as Medicare and private insurance; with co-morbidities such as hypertension, diabetes, and heart failure; admission through the ER; and a constant.