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The Spillover Effects of Hospital Closures on the Efficiency and Quality of Other Hospitals

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Problem definition: The recent upward trend in the U.S. hospital closures can have important impacts on the healthcare sector by changing the operational efficiency and quality of care of the remaining hospitals. Understanding how hospital closures impact the way the remaining hospitals operate can allow policymakers to utilize more effective policy levers in order to mitigate the negative consequences of hospital closures. We investigate the impact of hospital closures on the surrounding hospitals' operational efficiency and quality, and study how such hospitals respond to the closure of their neighboring hospital.

Methodology/results: We analyze more than 14 million inpatient visits made during 8 years to over 3,000 hospitals in the U.S. (before and after various closures), and utilize longitudinal causal inference methods to evaluate the spillover effect of hospital closures on the nearby hospitals. We also conduct counterfactual analyses to evaluate policy interventions that could have been used by policymakers. Our results show that hospital closures have both positive and negative spillover effects. When a hospital closes, its nearby hospitals improve their operational efficiency. However, they do so via a speed-up response (i.e., by reducing their average service duration) instead of an effort to lower their average bed idle time, and this response occurs regardless of whether or not their baseline bed utilization rate is high. Importantly, however, this speed-up response negatively affects some aspects of quality of care, including the 30-day mortality rate. Finally, we find that the spillover effect of a hospital closure is highly heterogeneous: nearby hospitals that are more desirable (e.g., high-quality, urban, and teaching) tend to experience greater spillover effects on their operational efficiency. Such effect widens social disparities by increasing the efficiency gap between the more and less desirable hospitals.

Managerial Implications: Our analyses suggest two effective levers for policymakers that should be applied to specific hospitals: (a) bailing out from potential closures, and (b) eliminating the speed-up response. In addition to helping policymakers by identifying these levers and the specific hospitals to which they should be applied, our results help hospital administrators. Specifically, our findings allow them to better understand the consequences (or the absence) of various strategic responses to a neighboring hospital closure, thereby enabling them to adopt more suitable management strategies.

Key words: Hospital Closures; Healthcare Operations Management; Healthcare Quality; Hospital Efficiency

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1. Introduction

A substantial number of U.S. hospitals have closed in the past decade (Kaufman et al. 2016, Friedman et al. 2016, MedPAC 2017). Such closures have occurred widely across the nation (see Figure 1) and have affected a large number of people (MedPAC 2017). Since U.S. hospitals are facing multiple challenges, including decreasing demand for inpatient services, elevated financial

pressures, and uncertainty over the payment structure, the number of hospital closures is expected to rise (Kaufman et al. 2016, Bazzoli et al. 2014, Wertheim and Lynn 1993, Navigant 2019).

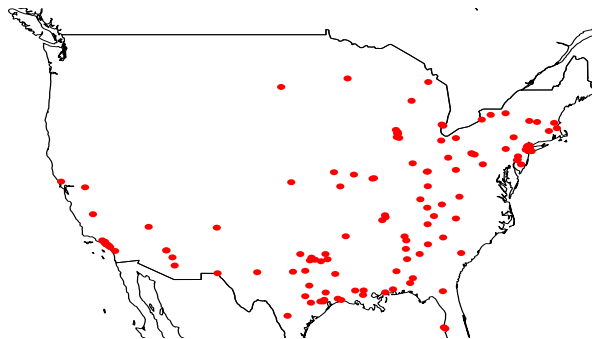
The increasing risk of hospital closures has fueled a debate on the need to implement policies that financially support, or “bail out,” hospitals that are on the verge of closure. Proponents of such policies argue that hospital closures can aggravate the already prevalent barriers to access (Hsia et al. 2012, Liu et al. 2014, Buchmueller et al. 2006), a concern that is recently become even more pronounced due to the need of accommodating a surge of COVID19 patients (Tribble 2020, Peter P. Reese 2020). Others point out that hospital closures are advantageous, and hence, should not be prevented: there are many inefficient or low-quality hospitals in the current system, and closing such hospitals can improve the healthcare system as a whole (Keeler and Ying 1996).

An essential but missing piece of information in this debate that can yield a better understanding of the implications of hospital closures is how they affect the efficiency and/or quality of care of the remaining hospitals. After a hospital closes, patients and payers have to rely on the remaining hospitals for care delivery. Therefore, hospital closures may alter patient demand and patient mix for the remaining hospitals, and the remaining hospitals may adjust their care delivery processes accordingly. However, this potential spillover effect of hospital closures on the remaining hospitals has been largely ignored in the literature. In this paper, we examine this potential spillover effect. Specifically, we study whether and how hospital closures impact (a) the operational efficiency, and (b) the quality of care in nearby hospitals, and shed light on their important societal implications.

Whether and how hospital closures impact the operational efficiency and the quality of care of nearby hospitals is largely unknown. This is partially due to the fact that this impact depends on whether the nearby hospitals behave strategically in response to such closures. Furthermore, such a strategic response might greatly differ among the remaining hospitals. For example, some hospitals might increase their capacity, especially if they face a spike in demand after a nearby hospital closes. Even if a remaining hospital does not increase capacity, it can accommodate the potential increase in demand with various other mechanisms (e.g., by increasing its service or bed utilization rate). These responses can have widely different impacts on the operational efficiency of the remaining hospitals, and hence, it is unclear how the overall operational efficiency in the healthcare system will be affected.

Similarly, the impact of hospital closures on the overall quality of care is unclear. On the one hand, hospital closures may improve the quality of the remaining hospitals. This can occur for at least two reasons. First, some of the remaining hospitals may put greater effort into improving their

Figure 1: Hospital Closures in the U.S. (2006–2014)



quality to be able to stay in the competition and prevent themselves from being closed. Second, due to learning-by-doing and related positive effects of volume on quality known as “volume-outcome effect” or “productivity spillover” (Ramanarayanan 2008, Chandra and Staiger 2007, Birkmeyer et al. 2002), the increase in patient volume may allow some of the remaining hospitals to improve their quality. On the other hand, hospital closures may reduce the quality of the remaining hospitals. This can happen, for example, if the remaining hospitals experience high congestions and care delivery delays due to the increase in patient demand, or if they decide to cut some of their value-adding treatment processes to accommodate the demand spike. Thus, in addition to studying the consequences of closures on the operational efficiency of the surrounding hospitals, we provide some evidence and insights into whether and how hospital closures affect the quality of care of the remaining hospitals. Finally, we study the heterogeneous impact of hospital closures and shed light on the type of hospitals that experience more significant positive and/or negative effects from the closure of a nearby hospital.

1.1. Framework

We focus on the U.S. short-term acute-care hospitals that serve patients with acute severe injury or episodes of illness. To investigate the impact of hospital closures on the operational efficiency of the nearby hospitals (our first goal), we define operational efficiency as a measure of how much output is produced per input. Specifically, we consider throughput per bed (i.e., the average number of patients served per bed per unit of time) as our measure of operational efficiency. We focus on beds as the main hospital resource, given that empty beds are the major contributors to the low operational efficiency of U.S. hospitals (Keeler and Ying 1996, Gaynor and Anderson 1995). Next, to identify the mechanism through which a change in operational efficiency might occur, we investigate changes in bed utilization and service duration separately. To study the impact of hospital closures on quality of care in the remaining hospitals (our second goal), we consider measures such as 30-day readmission rates and 30-day mortality rates, which are widely-accepted

measures for hospital care quality (Benbassat and Taragin 2000, Tourangeau et al. 2007). For both our first and second goals, we also identify the heterogeneous effects of closures by the market and hospital characteristics, and generate insights into variations in the closure effect that might elevate care disparities in the society. Finally, we perform counterfactual analyses to shed light on policies that, had been implemented by policymakers or hospital administrators, could yield significant societal gains.

1.2. Data and Empirical Challenges

There are several empirical challenges for studying the impact of hospital closures. First, although studies have examined rural hospital closures or closures in specific geographic areas (Kaufman et al. 2016, Lindrooth et al. 2003, Capps et al. 2010, Carroll 2019, Gujral and Basu 2019), no public data keeps track of all U.S. hospital closures. Thus, we independently identified closed hospitals through our own research and various validation steps, including direct phone call interviews when needed. Next, we used nationally representative, multi-year patient, hospital, and area level data to improve the generalizability of our findings. Specifically, we used the 20% random sample of Medicare Fee-for-Service (FFS) claims data, which cover the majority of the U.S. elderly population (those in ages 65 and older). These populations represent nearly 40 percent of all hospitalizations and nearly half of all health care dollars spent on inpatient care in the U.S. (Levit et al. 2007). We then linked the claims dataset to our hospital level data (from POS, Hospital Compare, and the Hospital Consumer Assessment of Healthcare Providers and Systems, or HCAPHS) and area level data (from Area Health Resource Files, or AHRF, and the Centers for Medicare & Medicaid Services (CMS) State/County/Plan Enrollment Data). Our final dataset, after combining these data sources (and cleaning them), includes over 14 million inpatient visits made during 8 observation years to 146 closed and 3,299 open hospitals (Table 1).

Another empirical challenge in studying the impact of hospital closures is that hospital closure can be correlated with both patient characteristics and market structure. Such endogeneity can bias the estimate of the hospital closure effect on the outcomes of our interest. To overcome this challenge, we use an extensive set of covariates, including patient level clinical and demographic information, as well as hospital and area level information such as provider supply, concentration, and the insurance market structure. As a main empirical strategy, we utilize the substantial geographic variation and the timing of U.S. hospital closures along with multilevel panel data to make use of difference-in-difference (DID) analysis with the hospital, area, and year fixed effects. The DID analysis enables us to use both cohort and time dimensions, and thereby, adjust for time-

Table 1: Overview of Our Final Data

Data	Description	Observations (N)	
FFS Medicare claims (20% random sample)	FFS claims submitted by Medicare providers to the CMS for reimbursement.	U.S. hospitals	3,299
Medicare Cost Report	Data collected from reports submitted by Medicare providers to the CMS. Contains provider information such as facility characteristics, utilization, cost and charges, and financial statement.	Closed hospitals	146
		Medicare patients	4,645,532
Medicare Provider of Service	Data collected through the CMS on characteristics of Medicare serving hospitals and other facilities.	Medicare inpatient visits	14,147,180
		Years	2008-2015 (8 years)
Hospital Compare	Hospital performance data collected by the CMS on the indicators of quality.		
HCAHPS Data	Patient’s perspectives on hospital care measured through a standardized instrument and data collection methodology and collected by the CMS.		
State/County/Plan Enrollment Data	Monthly enrollment information for Medicare program by geographic area.		
Area Health Resource Files	Data on healthcare resources and utilization, and population and socioeconomic characteristics by geographic area.		

invariant unobserved confounders. As we will describe, to perform our analyses, we use a matched sample that improves the comparability of the comparison groups. In addition to our primary analysis, we check the robustness of our findings by making use of various mechanisms, including an instrumental variable (IV) analysis. Our IV analysis utilizes the state level variation in the decision to expand Medicaid after the Affordable Care Act (ACA) to address the potential time-varying unobserved heterogeneity.

1.3. Main Findings

Our results show that hospital closures have both positive and negative average effects on the nearby hospitals, and hence, the healthcare sector as a whole. On the positive side, when a hospital closes, the nearby hospitals on average experience an increase in patient volume, which translates into an improvement in their operational efficiency (i.e., the number of patients treated per unit of capacity). Interestingly, however, we find that their bed utilization rates remain relatively constant, whereas their service duration significantly decreases. This implies that a speed-up behavior (i.e., improving efficiency via reducing service duration) rather than an effort to lower the average bed idle time is the primary mechanism through which nearby hospitals on average improve their operational efficiency. Because of such behavior, the gains in efficiency have negative implications on some (but not all) aspects of care quality. In particular, we find that when a hospital closes, the 30-day mortality rate of its nearby hospitals on average substantially increases. In addition to these average effects, we also observe that the effect of hospital closures is highly heterogeneous and largely depends on the neighbor hospitals’ characteristics. Specifically, we find that neighbor hospitals with more desirable characteristics (e.g., teaching, urban, or high-quality hospitals) expe-

rience greater spillover effects: the average effects discussed above is significantly more pronounced among such hospitals.

Taken together, our results suggest that although hospital closures are effective at improving the operational efficiency of the healthcare sector as a whole, there are unintended negative consequences. In particular, the improvement in operational efficiency—serving more patients per unit of capacity—is not due to more effective use of beds, but rather due to spending less time on each patient. Spending less time on each patient may not necessarily be undesirable if it eliminates non-value-adding procedures. However, our analyses show that it typically translates to a higher average 30-day mortality rate. This suggests that at least some of the value-adding procedures are eliminated as a result of the speed-up behavior, which is one of the concerning spillover effects of hospital closures we point out. Furthermore, our results on the heterogeneous effect of closures suggest that the current trend of hospital closures increase the efficiency gap between the hospitals that are either able to accommodate greater patient volume or are more likely to be demanded by patients and the rest, which raises concerns on aggravating social disparities.

Finally, our counterfactual analyses aimed at providing policy recommendations indicate that targeted versions of (a) bailouts, and (b) monitoring and regulations of service durations are effective policy levers. For example, bailing out the hospitals that have more desirable characteristics than their neighbors (e.g., high-quality, urban, and teaching hospitals) typically yields larger gains in operational efficiency than bailing out other hospitals. Focusing monitoring and regulation efforts on the hospitals with more desirable characteristics than their neighbors can also help policymakers to take advantage of the positive effects of hospital closures and mitigate their negative consequences. However, we conclude with a caveat that these targeted strategies typically come at a higher financial cost and can yield wider social disparities than their non-targeted counterparts.

1.4. Main Policy and Managerial Implications

The main policy and managerial implications of our study are two-fold. First, our results have important policy implications for the ongoing debate on the U.S. healthcare delivery system reform. Recently, some policymakers have argued against the payment mechanisms that support inefficient or financially unsustainable hospitals that rely on government subsidies such as the Medicaid Disproportionate Share Hospitals (DSH) payments for hospitals with high proportions of uncompensated care, or the Critical Access Hospital (CAH) status for hospitals with a high fixed cost. Yet, the limited evidence on the implications of hospital closures has impeded clear policymaking. We provide rigorous evidence using nationally representative data, and discuss actionable policy

suggestions. Second, our results help hospital administrators to better understand the implications of nearby hospitals' closures, and accordingly revise the management strategies that they may adopt in response to such closures.

2. Average Effects: Conceptual Framework and Hypotheses

We start our analyses by focusing on the average effect of hospital closures on their nearby hospitals' operational efficiency and quality of care. This allows us to better understand the overall impact of hospital closures on the healthcare system as a whole. We later perform heterogeneity analyses and study the specific types of hospitals that might be more significantly affected by such closures. Understanding such heterogeneity is especially useful in developing targeted policies that can be used by policymakers.

2.1. Changes in Operational Efficiency

We first examine if the closure of a hospital results in an average improvement in the operational efficiency of its neighboring hospitals. Some studies show that hospital closures can result in increased demand at some of the remaining hospitals in the area (Lindrooth et al. 2003, Capps et al. 2010, Bazzoli et al. 2003). Yet, it is not clear whether the increase in demand is consistent nationally. More importantly, it is not clear whether this increase in demand translates to improved operational efficiency at nearby hospitals. Thus, we start by using our data to test the following:

HYPOTHESIS 1. Closure of a hospital increases the operational efficiency of the nearby hospitals.

Our first hypothesis will enable us to test whether or not the closures have an increasing overall impact on the operational efficiency of the neighboring hospitals. However, it will not generate a detailed understanding of why and how such a change might occur. For example, improvement in operational efficiency might stem from an increase in bed utilization rate (i.e., accommodating the increased demand by lowering bed idle times) and/or an increase in service rate (i.e., serving patients faster). In particular, conceptualizing a hospital as a general queueing system with s beds that play the role of servers, the throughput per bed λ/s —our measure of operational efficiency—can be expressed as $\lambda/s = \rho * \mu$, where ρ is the bed utilization, and μ is the service rate such that $1/\mu$ is the average service duration. This implies that if s remains constant—hospitals typically cannot change their number of beds in the short term for various reasons, including the lengthy regulatory processes such as Certificate of Need (CON)—an increase in throughput corresponds to either (a) an increase in ρ , (b) a decrease in $1/\mu$, or (c) both an increase in ρ and a decrease in $1/\mu$. Thus, to gain a deeper understanding of potential mechanisms through which operational

efficiency might be affected, we form two more hypotheses: one with respect to bed utilization (ρ) and one with respect to service duration ($1/\mu$). We next discuss each of these separately.

Bed Utilization. U.S. hospitals have a wide range in their existing bed utilization rate. Although this can be partially described by differences in average patient demand, there is a good level of discretion on a hospital to set its bed occupancy rate for reasons beyond current average demand (Joskow 1980, Green and Nguyen 2001). Similarly, post-closure, nearby hospitals may accommodate the increased demand by either increasing their bed utilization rate or keeping their utilization rate the same, and instead responding to the closure in a different way. To gain insights into the effect of hospital closure on the bed utilization rate, we examine the following:

HYPOTHESIS 2. Closure of a hospital increases the bed utilization rate of the nearby hospitals.

Service Duration. As an alternative strategy in response to increased patient demand, hospitals may decrease their service duration. Operations management literature suggests that individual servers can be strategic about the service duration under financial and non-financial motivations, and alter their behavior based on the characteristics of the queue (Berry Jaeker and Tucker 2017, Cachon and Zhang 2007, Debo et al. 2008, Hopp et al. 2007, Jouini et al. 2008, Tan and Netessine 2014, Oliva and Sterman 2001). In particular, the visibility of the queue length or the server occupancy rate can encourage a speed-up behavior by servers (Berry Jaeker and Tucker 2017, KC and Terwiesch 2012, Batt and Terwiesch 2012, Shunko et al. 2017). Beyond individual servers, various studies in healthcare operations have shown that increased workload can result in rushing in hospitals. However, these works have focused mainly on a single hospital setting, and more importantly, on specific units of the studied hospital such as ICU (KC and Terwiesch 2012) or emergency department (Batt and Terwiesch 2012). It is also possible that beyond a certain point of workload saturation, the servers may not implement a speed-up behavior as a response to increased workload (Berry Jaeker and Tucker 2017). In addition, the impact of increased workload within one hospital unit may not necessarily translate to the entire hospital, as the workload of one hospital unit can have spillovers on the efficiency of different units (Freeman et al. 2021). Furthermore, unlike the above-mentioned literature, we are interested in studying changes across over 3,000 hospitals as opposed to a single hospital or a single unit within a hospital. This is mainly because our goal is to provide insights into how hospital closures impact the healthcare system as a whole and shed light on effective policies that can be followed by policymakers. Thus, we test the following:

HYPOTHESIS 3. Closure of a hospital decreases the service duration of the nearby hospitals.

2.2. Changes in Quality of Care

As noted in Section 1, it is unclear whether and how a neighbor hospital's closure impacts the overall quality of care. To gain a deeper understanding in this regard, we examine the impact of hospital closure on the health outcomes of the neighbor hospitals' patients. We measure 30-day readmission rates as well as 30-day mortality rates, the two widely-used outcome measures for inpatient services (we additionally examined the patient experience when we further investigated the mechanisms of main findings; see Section 6.4 for further details). These enable us to test the following:

HYPOTHESIS 4. *Closure of a hospital changes the 30-day readmission rate of the nearby hospitals.*

HYPOTHESIS 5. *Closure of a hospital changes the 30-day mortality rate of the nearby hospitals.*

3. Related Studies and Contributions

Our work is relevant to the empirical literature on operations management that aims at understanding organizational level responses to demand spikes. Among the studies that empirically examine the impact of crowding on the organization's service (Song et al. 2015, Shunko et al. 2017, Tan and Netessine 2014, Freeman et al. 2021, Berry Jaeker and Tucker 2017, Powell et al. 2012), our study is particularly related to the work that examines how an increase in workload for healthcare providers can lead to rushing (KC and Terwiesch 2012, Batt and Terwiesch 2012). Several studies have also examined the impact of rushing on quality of care (KC and Terwiesch 2009, Kuntz et al. 2015, Weissman et al. 2007). Our work differs from this literature in that we (a) are interested in the effect of hospital closures, and (b) perform analyses across U.S. hospitals by using data of more than 3,000 hospitals as opposed to a single hospital (or a single unit of a hospital).

Another stream of literature has examined the impact of hospital closures on economic efficiency (and not operational efficiency or quality of care, which are our focus). Within this stream, Lindrooth et al. (2003), Capps et al. (2010) show that urban hospital closure improves the economic efficiency (i.e., costs per admission) of nearby hospitals through an increase in inpatient admissions, suggesting the existence of economies of scale. Yet, Hodgson et al. (2015) suggests that hospital closures do not generate economies of scale but merely shifts the high-cost patients to the remaining hospitals. A related body of literature has studied the effect of hospital closures on the patients who lose healthcare (Joynt et al. 2015, Buchmueller et al. 2006, Capps et al. 2010, Hsia et al. 2012, Liu et al. 2014, Carroll 2019, Gujral and Basu 2019), focusing on yet another dimension: access to care. Unlike these studies, we analyze the impact of hospital closures on a broader set

of patients—not just those who lose access—and aim at studying the spillover effect of hospital closure on the overall operational efficiency and quality of the care in the healthcare system.

4. Data and Study Sample

4.1. Data

We obtained patient, hospital, and area level information by linking various data sources for the years 2008-2015. Our hospital level information is from Medicare Cost Reports and POS data. The data are collected from hospitals that serve Medicare patients, which are essentially all U.S. hospitals (nearly all short-term acute care hospitals in the U.S., except military hospitals, participate in the Medicare program). In addition to healthcare use and costs, these data provide information on the facility and operational characteristics such as patient discharges (aggregated across all inpatient visits and by insurance types such as Medicare or Medicaid) and bed capacities. Two of our outcome variables, efficiency and utilization, were based on these data. For patient level information, we used panel data of a 20% random sample of FFS Medicare inpatient claims. Medicare inpatient claims provide information on all FFS inpatient services use, the types of procedures performed, and diagnosis through the International Classification of Diseases (ICD-9) and Healthcare Common Procedure Coding System (HCPCS). We benefitted from the Medicare Beneficiary Summary Files (BSF) to identify individual beneficiary’s sociodemographic information. Our three outcome variables (service duration, readmissions, and mortality) come from these data. We made use of the Hospital Compare data from the CMS for hospital quality, AHRF for the county level information on health services resources and demographic information, and CMS State/County/Plan Enrollment Data File for the insurance enrollment information.

Table 2 summarizes the data sources used to study each of the hypotheses. As can be seen, we made use of various hospital-level and patient-level data to address different hypotheses. The hospital-level data for efficiency and utilization is an aggregate measure that comes from all inpatient visits made by all FFS Medicare patients in the U.S., and the patient-level data for service duration, 30-day readmission and 30-day mortality are from the observations for individual FFS Medicare patients. Although Medicare patients account for a large proportion of inpatient stays (e.g., comprised 39% in 2014 (McDermott et al. 2019)), these patients might not provide a complete representation of the overall inpatient population. In our sensitivity analysis, hence, we conducted alternative analyses using datasets that include the entire U.S. population (both Medicare and non-Medicare patients), and show that our results are fairly consistent.

Table 2: Data Sources, Related Hypotheses, and Outcome Variables

Hypothesis	Outcome	Level	Definition	Source	Study population
1	Operational efficiency	Hospital	Total number of patients discharged per hospital bed per year.	Medicare Cost Report, POS	All FFS Medicare beneficiaries
2	Bed utilization	Hospital	Total bed days used out of available bed days per year.	Medicare Cost Report, POS	All FFS Medicare beneficiaries
3	Service duration	Patient	Days between admission and discharge date.	FFS Medicare inpatient claims	20% random sample of FFS Medicare beneficiaries
4	30-day readmission	Patient	A binary variable indicating the presence of another hospitalization within 30 days of discharge.	FFS Medicare inpatient claims	20% random sample of FFS Medicare beneficiaries
5	30-day mortality	Patient	A binary variable indicating the presence of death within 30 days of discharge.	FFS Medicare inpatient claims	20% random sample of FFS Medicare beneficiaries

4.2. Identifying Hospital Closures

We defined hospital closure as ceasing to deliver short-term general hospital services rather than the changes in the ownership or physical appearance of a hospital because we focus on the hospitals' responses to the workload changes. If a hospital remained in the same physical location but ceased to provide short-term acute care and converted to a different use such as an emergency department, rehabilitation facility, or long-term care facility, we regarded it as closure. However, absent such changes, if a hospital merely changed its name or ownership but stayed in the same physical location, we considered the hospital to be in operation.

To identify potential closures, we first used Medicare POS data. We regarded a hospital as potentially closed if it did not appear in the POS data after a specific year. Because virtually all short-term acute care hospitals in the United States, except military hospitals, participate in the Medicare program, discontinuation of data or claims submission to the CMS suggests it is highly likely that the hospital has experienced changes in operating and/or ownership status. Next, we identified a hospital as potentially closed if its number of hospitalizations from Medicare inpatient claims has dropped to zero. Among the list of potential closures that met either of our criteria, we then excluded the hospitals that are not short-term acute care hospitals. Finally, we systematically searched and validated each hospital's operating status through multiple external sources, including local news, state department documents, and a list of rural hospital closures from other research institutions (e.g., University of North Carolina Rural Health Research and Policy Analysis Center and Becker's Hospital Review). In a few cases where the evidence was not available or definitive, we conducted direct phone interviews with the hospital. We were eventually able to confirm the operating status of all hospitals on our list. Figure EC1 shows the detailed steps through which we determined hospital closures.

Table 3 shows the comparison of hospitals that were closed versus those that remained open during our study period. In general, the pre-closure characteristics of closed hospitals indicate clear signs of difficulty: compared to hospitals that did not close, closed hospitals are in more competitive markets (measured by Herfindahl-Hirschman Index). They are less likely to be teaching or nonprofit, more likely to be rural, and more likely to have low operational efficiency and/or quality of care. Their patients are slightly older, less likely to be White, and tend to be poorer and sicker. We note, however, that we eliminate the closed hospitals and their patients from our study sample, and focus on the spillover effects experienced by the hospitals that stay open throughout the study period (see Section 4.3 for more details).

4.3. Study Sample

Our study sample of hospitals included all the Medicare-participating U.S. hospitals that were in operations throughout our study period. Among our sample, we considered hospitals that are in the nearest distance from the closed hospital (calculated based on the distance between ZIP code) as “treated,” and the rest as “control.” We defined the nearby hospital this way because patients of closed hospitals are likely to choose their alternatives based on distance (Chandra and Staiger 2007, McNamara 1999). We also confirm this by analyzing our own data and finding that, among the patients of closed hospitals (i.e., those who have visited the closed hospital at least once throughout the study period), 71% have visited the hospitals in the nearest zip codes at least once during the study period. In our robustness checks, we use hospital referral regions (HRRs) instead of zip codes to identify nearby hospitals. In all analyses, we removed the closed hospitals from our study sample, because closed hospitals will have zero demand after closure, which can confound the spillover effect that we are examining. In our main analysis, we also removed the patients of closed hospitals. We did so by taking advantage of our longitudinal data to track each patients’ hospital visit history throughout the observation period. In our robustness checks (Section 7.4), we examined the impact of including the patients of the closed hospitals in the analysis.

For each hospital in the treatment group, we defined the index year as the year of a nearby hospital’s closure. We excluded the information from the index year to account for the noise during the transition period. We then compared outcomes pre- and post-closure. To be consistent, we limited our observations to three years post-closure since (a) we do not have enough observation years beyond that for the majority of closure index years, and (b) there are other contemporaneous changes in the market such as additional closures or entry of hospitals usually beyond three years. However, in our sensitivity analysis, we analyzed the effect of using other durations for post-closure

Table 3: Hospital and Patient Characteristics of the Open and Closed Hospitals

	Open	Closed		Open	Closed
Hospitals (n)	3,299	146	<i>Market characteristics</i>		
Inpatient visits (n)	14,044,827	102,353	MA Penetration (%)	17.0	13.9
Patients (n)	4,593,211	52,321	Herfindahl-Hirschman Index	1,451.4	1,317.0
<i>Hospital characteristics</i>			Population (n)	1,041,501	706,903
DSH (%)	80.5	78.3	Unemployment (%)	9.0	9.3
Teaching hospitals (%)	32.3	22.7	Poverty (%)	16.1	18.2
Ownership – nonprofit (%)	61.5	34.0	Under Age 65 (%)	83.0	82.5
Ownership – private (%)	22.6	10.9	Hosp (/10,000)	0.27	0.39
Ownership – public (%)	15.9	49.7	<i>Patient characteristics</i>		
Rural (%)	33.4	59.7	Avg age (years)	79.0	79.8
Discharges (n)	9,724.4	3,466.7	Sex – male (%)	42.1	37.3
Beds (n)	196.7	100.3	Race – White (%)	87.1	81.4
Operational efficiency	45.9	32.2	Race – Black (%)	8.5	13.2
Bed utilization	0.5	0.4	Dual-eligibles (%)	17.3	26.1
Patient experience (% good)	67.4	63.3	Comorbidity score	3.4	3.6
			Chronic conditions (n)	23.7	21.9

Note. The summary statistics include all observations from pre and post closure years for the open hospitals and all observations from pre closure years for the closed hospitals. DSH indicates Medicare Disproportionate Share Hospitals. MA indicates Medicare Advantage. Dual-eligibles indicate Medicare-Medicaid dual-eligibles. All differences in covariates between the two groups were statistically significant at p-value < 0.05.

observation years (Table EC18). Figure EC2 shows our timeline of hospital closures for index years as well as pre- and post-closure observations.

For our patient level analysis, we considered the study population to be the FFS Medicare beneficiaries who paid at least one visit to the hospitals in our study sample. To improve the comparability, we further restricted the patient population to those who were aged 65 or older, did not have a disability, and were entitled to Medicare due to age (although Medicare eligibility age is 65, Medicare also covers a small fraction of people under 65 with disabilities). Because patient's treatment status was based on the treatment status of the hospital they visited, a patient was allowed to be in both treatment and control groups if s/he made multiple visits to both treatment and control hospitals. We excluded transfers to or from another hospital, admissions for rehabilitation, and emergency department visits that did not result in inpatient admissions from the analysis. The methods we used for identifying these patients (e.g., through ICD-9 codes) are explained in the Online Appendix. As noted earlier and shown in Table 1, there were a total of 4,645,532 patients in our final sample with a total of 14,147,180 inpatient visits during our study period (i.e., across 8 observation years) made to 3,299 open and 146 closed hospitals. There were a total of 158 hospitals in the treatment group and 3,141 hospitals in the control group. Although there were 146 closures, we assigned 158 hospitals in the treatment group because some closed hospitals had more than one open neighbors in the nearest ZIP code.

4.4. Dependent Variables

Our dependent variables include three operational measures (operational efficiency, bed utilization, and service duration) and two quality measures (30-day readmission, and 30-day mortality). The definition of these variables, data sources used, and their corresponding hypotheses are shown in Table 2. For operational efficiency and bed utilization, we used the yearly average, because the monthly average can be subject to the seasonal and weekly variations in patient demand unrelated to the spillover effect. For service duration, we used data on length of stay, but excluded observations with values greater than 30 days, as our goal is to examine the impact of closures on short-term acute care. For measuring 30-day readmission, we considered inpatient claims that were within 30 days of a previous hospitalization’s discharge date. For measuring 30-day mortality, we obtained death information from the Medicare denominator files and calculated the time to death as the number of days between the index discharge date and the date of death. We linked a hospitalization to an incidence of 30-day mortality if death was present within 30 days of discharge.

4.5. Independent Variables

Table 4 shows the definition and data sources for the independent variables we used to control for potential confounders and examine effect heterogeneity. These variables can be classified into three categories: patient characteristics, hospital characteristics, and area characteristics.

Patient Characteristics. To control for patient heterogeneity, we included demographic characteristics such as age, gender, race, a reason for Medicare eligibility, and the Medicare-Medicaid dual-eligibility which is often used as a proxy for low-income status. We obtained the total number of chronic conditions a beneficiary had in the previous year from the chronic conditions segment of the Medicare BSF. We calculated patient comorbidities using the Charlson Comorbidity Index (Elixhauser Comorbidity classification) from the patient’s diagnosis history. The scores range from 0 (lowest severity) to 21 (highest severity) and capture the presence of 30 comorbidities (Elixhauser et al. 1998). Using these scores allows us to control for the variation in patient health. We categorized the admission into three types, i.e., emergent, urgent, and elective, according to the admission type variable on claims. We also divided the admissions into 15 clinical categories based on the primary diagnosis codes. These categories are further explained in the Online Appendix.

Hospital Characteristics. We obtained relevant hospital characteristics including the size, academic status, ownership, location, quality, and funding status. Academic status was identified by whether the hospital received any payment from the Graduate Medical Education (GME) program or Indirect Medical Education (IME) program, which pays hospitals for education and training. A

Table 4: Definition of Independent Variables and Data Sources

Variable	Description	Data source
<i>Patient characteristics</i>		
Age	Numeric, from 64 and up.	Medicare BSF
Gender	Binary, male or female.	Medicare BSF
Race	Factor, White, Black, Hispanic, Asian, or others.	Medicare BSF
Medicare entitlement	Factor, age, disability, or both.	Medicare BSF
Medicaid eligibility	Binary, dual or non-dual.	Medicare BSF
Chronic conditions	Numeric, from 0 to 27.	Medicare BSF
Comorbidity	Numeric, from 0 (least severe) to 21 (most severe).	Medicare inpatient, outpatient claims
Admission type	Factor, emergent, urgent, or elective.	Medicare inpatient claims
Diagnosis type	Factor, 15 clinically meaningful categories.	Medicare inpatient claims
<i>Hospital characteristics</i>		
Beds	Numeric, greater than 0	Medicare Cost Report, POS
Academic status	Binary, teaching or non-teaching hospital.	Medicare Cost Report, POS
Ownership	Factor, nonprofit, private, or public.	Medicare Cost Report, POS
Location	Binary, rural or urban.	Medicare Cost Report, POS
Quality	Numeric, from 1 (lowest) to 5 (highest).	Hospital Compare data
DSH	Binary, DSH or non-DSH.	Medicare Cost Report, POS
<i>Area characteristics</i>		
Managed care penetration	Numeric, from 0 (no penetration) to 1 (full penetration).	State/County/Plan Enrollment Data
Herfindahl-Hirschman Index	Numeric, greater than 0	Medicare inpatient claims, Medicare Cost Report
Provider supply	Numeric, number of providers per 10,000 persons.	AHRF
Unemployment rate	Numeric, from 0 (full employment) to 1 (full unemployment).	AHRF
Poverty rate	Numeric, from 0 (no poverty) to 1 (full poverty).	AHRF
Population under age 65	Numeric, from 0 (none under 65) to 1 (all under 65)	AHRF

hospital was defined as rural if its zip code based Rural-Urban Commuting Area code was greater than 4, or if it was designated as a Critical Access Hospital (CAH), following previous literature (see, e.g., Hart et al. (2005)). We also included an indicator for receiving payment for the Medicare DSH payments program, which funds hospitals that treat a greater proportion of needy patients. Hospital quality was measured from the publicly available Hospital Compare data provided by the CMS, which draws detailed information on hospital quality from multiple sources, including hospital submitted electronic health records, surveys, and Medicare claims data. The data include 57 quality measures across seven areas of quality and provide an overall rating as well as quality ratings on different dimensions of care. We used the overall rating, which can have values from one star (5.7% of the total hospitals) to five stars (7.36% of the total hospitals).

Area Characteristics. We included various market level factors that could influence the operations of hospitals. First, differences in care delivery and quality may exist between the areas with varying degrees of managed care and FFS insurance plan types (see, e.g., Miller and Luft (1997), Baicker et al. (2013), and the references therein). Thus, we included the yearly county level penetration rate of Medicare managed care plans by calculating the proportion of beneficiaries enrolled in any Medicare Advantage (i.e., Medicare’s managed care type plans, a type of health insurance plans that actively manages cost and quality) out of total Medicare beneficiaries each year. Second, to control for the changes in the degree of market competition, we constructed the Herfindahl-

Hirschman Indices (HHIs) for hospitals—a standard measure of concentration—for each market (defined as HRRs) per year. Third, the level of the provider supply may affect bed utilization and operational efficiency. Thus, we adjusted for the area level provider supply, such as the total number of primary care physicians and acute care hospitals per 10,000 persons from AHRF. Lastly, to adjust for any macro level socio-demographic factors, we controlled for the proportion of the population unemployed, in poverty, or aged 16 or older for each county from AHRF.

5. Main Empirical Analysis

5.1. Empirical Strategy Overview

Our main empirical strategy is a DID approach with hospital, market, and year fixed effects to examine the changes in hospital and patient outcomes before and after a hospital closure event. This approach allows for controlling observed and unobserved heterogeneity between the treatment and control group that is constant over time. If the parallel trend assumption is met, DID analysis can provide a causal interpretation of the treatment effect (see Section 5.2 for discussions and tests related to this assumption and other assumptions of the DID approach in our setting). We used a fixed effects model instead of a random effects model, because the hospital or market effects are likely correlated with the observed patient or hospital characteristics. We used hospital level instead of patient level fixed effect, since a large proportion of patients had only one hospital visit. We used a robust standard error clustered at hospital-year to account for the correlation of error terms.

We employed the following model for testing hospital level outcomes (hypotheses 1 and 2):

$$Y_{jt} = \alpha_1 \mathbf{X}_{jt} + \alpha_2 \mathbf{Z}_{jt} + \beta \text{POSTCLOSURE}_{jt} + \text{HOSPITAL}_j + \text{YEAR}_t + \epsilon_{jt}. \quad (1)$$

To test the patient level outcomes (hypotheses 3, 4, and 5), we utilized the following model:

$$Y_{ijt} = \alpha_1 \mathbf{X}_{it} + \alpha_2 \mathbf{Z}_{jt} + \beta \text{POSTCLOSURE}_{jt} + \text{HOSPITAL}_j + \text{AREA}_i + \text{YEAR}_t + \epsilon_{ijt}. \quad (2)$$

In both models (1) and (2), Y represents the outcome variables, POSTCLOSURE is a binary variable that indicates that the observation is made in the post-closure year for the treated group. In our robustness checks, we test the robustness of our results to temporal effects of hospital closures by introducing a different binary variable for each of the post-closure years (see Section 7.3). HOSPITAL , AREA , and YEAR represent the hospital, area, and year fixed effects. For the hospital-level model (Equation 1), there is no area fixed effect since hospitals do not change their locations. \mathbf{X} is a vector of patient characteristics, including age, gender, race, Medicare entitlement, Medicaid dual-eligibility, chronic conditions, comorbidity, admission type, and diagnosis type. \mathbf{Z}

is a vector of area characteristics, including managed care penetration (i.e., the proportion of patients who own managed care insurance plans per market), HHI, provider supply measures, and socioeconomic measures. ϵ is the error term. Indices i , j , and t represent a patient, a hospital, and a year, respectively. Bold notation is used to represent vectors.

To improve the comparability of our treatment and control groups, we made use of matching in our main analyses. Specifically, we first estimated the propensity score of being in the treatment and control groups using a logistic regression model where we employed hospital characteristics (size, academic status, ownership, location, funding, and quality) as matching criteria. We then utilized the nearest-neighbor matching method without replacement. The balance statistics of the 158 hospitals in the treatment group and 158 matched hospitals in the control group can be found in Table EC1.

5.2. Assumptions

The main assumption of our fixed effect DID model is that conditioned on the unobserved fixed differences by groups, each observation-specific error term is uncorrelated with the explanatory variables in all periods (i.e., strict exogeneity holds). We control for multiple dimensions of time-varying proxies for health, socio-economic status, and market characteristics such as insurance penetration and competition level in our analyses to address potential violations of the strict exogeneity assumption. However, there might still be two major threats to this assumption.

First, there can be a patient-level selection that is correlated with hospital closures. For example, sicker patients may have chosen a particular hospital and have contributed to its closure, and then their influx to neighbor hospital after closure could have contributed to increased adverse outcomes. Since our controls (e.g., our proxies for health) might have not fully captured this endogeneity, we also perform our analyses after eliminating the patients of closed hospitals from our sample in our robustness checks (see Section 7). Furthermore, we note that this endogenous selection process is rather static, because underlying clinical or socio-economic differences for healthcare are typically stable in the short term (see also Fiscella et al. (2000)). These give us confidence that our results are not affected by such a patient-level selection process.

Another critical assumption in our DID analyses is the parallel trend, which posits that the differences between the treatment and control groups are constant over time. The assumption is not formally testable, but we show that the pre-treatment outcomes do have a parallel trend (Figure EC3). To gain further confidence, we also test for the common trend between the treatment and the control group before the treatment year by including the interaction term of the treatment

variable with each pre-treatment year. We find that the pre-treatment trends are not significantly different between the two groups prior to the treatment year (Table EC4).

In our robustness checks (Section 7), we rerun our analyses by changing some of our other model specifications and testing the validity of some of our related assumptions. Finally, because we cannot completely verify the extent to which these unmeasurable aspects bias our results, we also use an instrumental variable (IV) approach as part of our robustness checks (see Section 7.2). This IV approach further mitigates the concerns mentioned above and gives us assurance about the validity of our results.

6. Results and Discussions

6.1. Summary Statistics

An average hospital in our data serves 11,330 patients per year, with an operational efficiency of 44.6 (patients per bed per year), and bed utilization of 56.7%. The average service duration, 30-day readmission rate, and 30-day mortality rate are 4.81 days, 16%, and 6%, respectively, which are consistent with the existing literature (see, e.g., Joynt et al. (2011), Bueno et al. (2010)). Compared to the hospitals in the control group, hospitals in the treatment group are more likely to be teaching, public, and urban (Table EC3). They are also more likely to be located in a competitive market, which is consistent with our results that hospitals in competitive markets are more likely to close. Patients in the treatment group are slightly older, less likely to be male or White race, more likely to be low-income, and sicker. We adjust these differences in hospital and patient characteristics in our DID analysis.

6.2. Average Effect

Table 5 and Figure 2 show the DID results of our main model for our hospital and patient level outcomes, respectively. The full results are presented in Table EC5 and Table EC6. Our results indicate that, on average, hospitals improve their operational efficiency after the closure of nearby hospitals by 0.8 additional discharges of FFS Medicare patient per bed (equivalent to 5.1% increase), and this change is statistically significant (hypothesis 1). To test if the efficiency gain is driven by an increase in patient volume as opposed to a change in hospitals' capacity, we separately examine the changes in volume and capacity. We see a substantial increase in patient volume (176.8 additional discharges of FFS Medicare patients per hospital per year with a standard deviation of 5.7), but no significant increase in the number of beds. While the bed utilization rate also increases by about 3.2% in the post-closure years, this increase is not statistically significant (hypothesis 2). The log length of stay of the remaining hospitals, however, decreases by a statistically significant amount of

2.3%, which translates to one out of every five patients being released a day earlier (hypothesis 3). Hospital closures do not impact the 30-day readmission rate of their nearby hospitals (hypothesis 4). The 30-day mortality rate of such hospitals, however, increases by a statistically significant amount of 3.8%, which translates to an additional 2.3 deaths per 1,000 patients per year (hypothesis 5).

Overall, we find evidence of overall efficiency improvement following a closure event as measured by the number of patients served per bed per unit of time. Although on average hospitals improve their efficiency, such an improvement is not due to an increase in bed utilization (lower bed idle times): a decrease in service duration—a speed-up behavior—is the main reason behind the efficiency improvement in the remaining hospitals. Importantly, indicating both a decrease in service duration and an increase in 30-day mortality rate, our results suggest that the remaining hospitals mainly respond to the increased demand caused by a nearby hospital closure by eliminating some value-added care steps.

The overall impact of hospital closures on the healthcare system suggests that policymakers should enact policies that can either prevent hospitals from being closed or can mitigate negative responses by the remaining hospitals after a closure occurs. Since the responses by the remaining hospitals might not be homogeneous, policies that target specific hospitals (or markets) might be more effective levers than other policies. Thus, as a precursor to our policy analyses (Section 8), we next study the heterogeneous effect of hospital closures among the remaining hospitals.

6.3. Heterogeneous Effect

To study the heterogeneous effect of hospital closures, we made use of the following model, where we included an interaction term between our DID variable and market or hospital characteristics of interests denoted by CHAR (and discussed next):

$$Y_{jt} = \alpha_1 \mathbf{X}_{jt} + \alpha_2 \mathbf{Z}_{jt} + \beta \text{POSTCLOSURE}_{jt} * \text{CHAR}_{jt} + \text{HOSPITAL}_j + \text{YEAR}_t + \epsilon_{jt}. \quad (3)$$

Heterogeneous Effect by Market Characteristics. We first examined whether the remaining hospitals in the more competitive markets (measured by HHI) or markets with greater resources (measured by the number of inpatient hospitals per population) experience a greater spillover effect. Table 6 shows that there is no significant difference in the spillover effect by either of these variables which represent the main market characteristics in our data set.

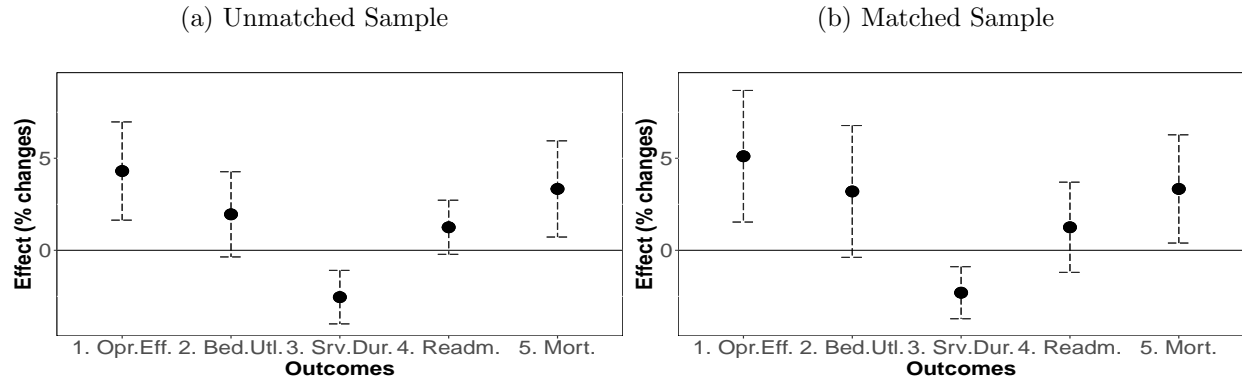
Heterogeneous Effect by Hospital Characteristics. The main concerning effect of closure we see is that some value-added steps are cut, as witnessed from the mortality increases post closure.

Table 5: Difference-in-Differences Estimates: Average Effect of Hospital Closure

	Hypothesis Outcomes	(1) Operational efficiency	(2) Bed utilization	(3) Service duration	(4) 30-day readmission	(5) 30-day mortality
Unmatched sample	Nearby hospital's closure	0.57* (0.18)	0.004 (0.003)	-0.51* (0.32)	0.002 (0.002)	0.002** (0.001)
	Number of observations	45,764	45,767	10,368,445	10,368,445	10,363,945
	Adjusted R^2	0.87	0.85	0.08	0.05	0.03
Matched sample	Nearby hospital's closure	0.77* (0.28)	0.007 (0.004)	-0.12** (0.036)	0.002 (0.002)	0.002** (0.001)
	Number of observations	4,163	4,163	1,623,079	1,623,932	1,623,079
	Adjusted R^2	0.87	0.86	0.08	0.05	0.03

Note. All models include hospital, area, and year fixed effects. Standard errors are in parentheses. Standard errors are robust and clustered at the hospital and the year levels. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

Figure 2: Difference-in-Differences Estimates: Average Effect of Hospital Closure



Note. All effects are scaled as changes in percentages. Each dot indicates the size of the DID coefficient. Grey lines depict the 95% confidence intervals around the coefficient of the DID variable. Standard errors are robust and clustered at the hospital and the year levels.

Can policymakers target specific hospitals to address this, or is this effect homogeneous? To answer this question, we next examined the heterogeneous spillover effect of closures based on neighbor hospitals' characteristics (DSH status, ownership, academic status, quality, location, and size). Our results are presented in Table 6 and show that, after a hospital closes, there is no noticeable heterogeneous effect on quality. However, we observe that after a hospital closure, the neighbor hospitals that are generally considered to be more desirable (e.g., teaching, high quality, and urban) experience a greater increase in their operational efficiency compared to the less desirable (e.g., non-teaching, low quality, and rural) hospitals. This suggests that the effect of hospital closures is not uniform across hospitals; it depends largely on hospital characteristics.

6.4. Mechanisms Behind the Main Findings

As noted in Section 6.2, our results indicate that the remaining hospitals react to a nearby hospital closure via a speed-up response instead of accommodating the increase in demand by increasing

Table 6: Difference-in-Differences Estimates: Heterogeneous Effect of Hospital Closure

		(1) Operational efficiency	(2) Bed utilization	(3) Service duration	(4) 30-day readmission	(5) 30-day mortality
Market characteristics	POSTCLOSURE*HHI	-0.0004 (0.001)	-0.00001 (0.00001)	0.00001 (0.00004)	-0.00001 (0.00001)	0.00001 (0.00001)
	POSTCLOSURE*Resource	-65,301.5 (35,281.8)	-394.9 (265.4)	-36.3 (1,198.4)	72.1 (106.6)	-45.8 (43.6)
Hospital characteristics	POSTCLOSURE*DSH	-1.6 (2.1)	-0.02 (0.02)	0.1 (0.06)	-0.004 (0.005)	-0.001 (0.001)
	POSTCLOSURE*Private	0.7 (2.0)	0.03 (0.002)	0.09 (0.08)	0.007 (0.007)	-0.007 (0.004)
	POSTCLOSURE*Teaching	3.2** (0.9)	0.03* (0.01)	-0.04 (0.08)	0.003 (0.003)	0.001 (0.002)
	POSTCLOSURE*Quality	1.1*** (0.2)	0.01* (0.004)	0.005 (0.004)	0.0001 (0.0002)	0.0001 (0.0001)
	POSTCLOSURE*Urban	3.6† (2.0)	0.04 (0.02)	0.08 (0.06)	-0.002 (0.005)	-0.0005 (0.003)
	POSTCLOSURE*Size	0.002 (0.003)	0.00003 (0.00002)	0.00004 (0.0002)	-0.00001 (0.00001)	0.00001 (0.00001)

Note. All models include hospital, area, and year fixed effects. Standard errors are in parentheses. Standard errors are robust and clustered at the hospital and the year levels. † $p < 0.1$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

their bed utilization rates. A driving force behind this might be that most hospitals are already running close to their maximum possible bed utilization rates. Furthermore, hospitals often reserve some slack in bed utilization in preparation for unexpected events (Joskow 1980), and they might want to keep such slack capacity intact. If these are the driving forces behind our observation on the speed-up response, then one would expect to see a different result for hospitals with a low baseline bed utilization rate. To test this, we examined whether hospitals with high versus low baseline utilization respond differently to neighbor's closures by estimating the DID model separately on the hospitals with above-median and below-median utilization rates (Table EC7). We find that neither type of hospital increase their bed utilization, while both provide similar speed-up responses. This suggests that even hospitals with a low utilization rate fail to reduce their bed idle items when a nearby hospital closes, and instead resort to shorter service durations to accommodate the demand increase. In Section 8, we discuss the implications of policy interventions that discourage such a speed-up behavior.

Recommending effective policy interventions also requires understanding the drivers behind the increase in mortality, and thus, we examined various potential pathways. First, changes in the patient mix might have driven an increase in mortality post-closure. For example, the mortality increase might have been due to a greater number of emergent patients choosing the neighbor hospitals, whereas a fewer number of patients are choosing further alternatives. We first examined whether there were differences in the increase in patient volume by admission type (e.g., emergent, urgent, and elective) and confirmed that it is not the case (Table EC8). We have already controlled

for the admission type, patient comorbidity, and chronic conditions in our main model as ways of risk adjustment (see also Section 7.4 for further robustness checks on the risk adjustments made). It is, however, still possible that our results are driven by an increase in mortality among emergent patients. To examine this possibility, we included the admission type as an interaction term, but did not find any difference in mortality changes among emergent patients compared to other types of patients (Table EC9). We also tested if there was any change beyond the admission type in the patient mix at the hospital level by measuring the proportion of patients with certain characteristics such as age, sex, race, and dual status, but did not find any significant change (Table EC10).

Second, after a closure event, the nearby hospitals may attract patients who are further away, which could increase the chance of mortality due to having longer travel distances, especially for emergent patients. We controlled for the distance between the patient and the hospital, and observed that the mortality still increases (Table EC18).

Third, as noted earlier, the speed-up behavior of hospitals could have resulted in an increase in mortality. To test this hypothesis, we performed a mediation analysis. Specifically, we modified our main model in Equation (2) to control for the service duration:

$$Y_{ijt} = \alpha_1 \mathbf{X}_{it} + \alpha_2 \mathbf{Z}_{jt} + \beta \text{POSTCLOSURE}_{j-1,t} + \gamma \text{SERV}_{ijt} + \text{HOSPITAL}_j + \text{AREA}_i + \text{YEAR}_t + \epsilon_{ijt}, \quad (4)$$

where $\text{POSTCLOSURE}_{j-1,t}$ is the lagged treatment variable and SERV_{ijt} is the service duration of each patient. The total effect of hospital closure on mortality that was estimated in Equations 1 and 2 can be decomposed into the indirect effect mediated by service duration during hospitalization and the direct effect of hospital closure, where the coefficient γ captures the direct effect of hospital closure on the mortality. We found that after controlling for the service duration, the hospital closure effect is no longer significant, whereas the coefficient γ is negative (Table EC11). This suggests that the reduction in service duration after hospital closure is at least one channel through which the increase in mortality after hospital closure occurs.

Based on our findings from the mediation analysis, we hypothesize that hospitals have reduced some value-added care in the process of discharging patients quicker, which could have resulted in an increase in mortality. To test this hypothesis, we examined various dimensions of patient experience during hospitalization. We used the HCAHPS data (see Table 1 for detail), and treated the patient's rating (1 for lowest and 10 for highest) as the primary patient experience measure. Because we do not observe individual level responses, we defined a hospital's overall rating as the total percentage of patients who gave "high" ratings (rating of 9 or 10). We also examined the secondary outcomes from each of the nine core questions (in HCAHPS survey) about patients'

hospital experiences. Our results do not indicate any statistically significant change in overall patient experience ratings after hospital closures (Table EC12). However, when the ratings for each of the nine domain for the patient experience is examined separately, staff explanation, pain control, and discharge information show a significant reduction in patient experience rating. The fact the care delivery steps that require staff engagement and time particularly show reductions in quality strengthens the claim that the nearby hospitals have likely cut some value-added care processes as part of their speed-up response.

Put together, our results suggest that the negative impact of closures on the nearby hospitals' mortality is not driven by changes in the patient mix or other patient related factors such as traveled distance. Instead, it is driven by the fact the nearby hospitals shorten their service duration, which most likely involves the elimination of some value-added care delivery procedures.

7. Robustness

7.1. Results for All U.S. Population

In our main analyses, we focused on providing insights by making use of data related to Medicare patients. To test the validity of our results for the entire population (both Medicare and non-Medicare patients), we performed some supplementary analysis. Specifically, to examine the effect of hospital closures on the operational outcomes, we made use of the Healthcare Cost and Utilization Project (HCUP) data, the longitudinal hospital care data in the United States. The two operational variables (operational efficiency and utilization) in this data set are defined at the hospital level, which is consistent with the definition we used in our main analysis. To measure the effect of hospital closures on the two outcome variables (30-day readmissions and 30-day mortality), we made use of the Hospital Compare data, which provides the risk-adjusted hospital-level outcomes. The detailed definitions and methods of our analyses are described in the Online Appendix.

Our main findings are fairly consistent between Medicare and non-Medicare patients (Table EC13). This gives us more confidence that FFS Medicare patients and the rest of the patients would have responded in similar ways. The reason we do not present our results for the entire U.S. population in our main analysis, however, is because we have more confidence in the Medicare data, given that it provides detailed individual level observations and patient information to adjust for the patient-level risk. Furthermore, we do not have access to measures related to service duration for the entire U.S. population. Nevertheless, using the data mentioned above, we are able to show that even when one considers both Medicare and Non-Medicare patients, the remaining hospitals

improve their operational efficiency post-closure without improving their bed utilization rates. This is consistent with the speed-up response we established earlier.

7.2. Instrumental Variable (IV) Analysis

As noted in Section 5.2, our fixed effects model may not fully address the time-varying unobservable confounders that can bias our results. Therefore, we incorporated an IV analysis in our DID design (i.e., instrumented difference-in-differences, or DDIV) by identifying an IV that can account for unmeasured confounders (De Chaisemartin and D’Haultfœuille 2017, Duflo 2001). Specifically, we made use of the state level variations in the decision to expand Medicaid as an instrument that influences the likelihood of hospital closures but is unlikely to be correlated with our outcome variables. The Affordable Care Act (ACA) originally intended to expand Medicaid coverage to low-income adults, but the provision was ruled coercive by the supreme court. Therefore, each state could choose to expand or not expand Medicaid, which created a variation in Medicaid eligibility by state. Evidence shows that the expansion is associated with improved hospital financial performance and a lower likelihood of hospital closure (Lindrooth et al. 2018, Blavin 2016, Blavin and Ramos 2021). Our results show that Medicaid expansion is a significant predictor of fewer closures of nearby hospitals and the impact of hospital closures on the nearby hospital’s operational efficiency and quality are consistent with the main results using the DID analysis. The model specifications and the first and second stage regression estimates are provided in the Online Appendix.

The key assumptions for our IV approach are: (1) the instrument does not affect the outcome except through treatment (exclusion restriction), and (2) the instrument is associated with the treatment variable. Available studies in the literature suggest that Medicaid expansion—our IV—is strongly correlated with our treatment variable (see, e.g., Lindrooth et al. (2018), Blavin (2016), Blavin and Ramos (2021)), and hence, assumption (2) holds. Our direct tests on the level of correlation between Medicare expansion and our treatment variable further confirm this (Table EC14). However, unlike assumption (2), we cannot directly test assumption (1). We argue that assumption (2) most likely holds for several reasons. First, our study population is Medicare patients, whose utilization and care-seeking behavior are unlikely to be affected by the Medicaid expansion since they already have insurance coverage. Second, we tested if Medicaid expansion is associated with changes in the hospital level outcomes among hospitals that are in the control group, but did not find such effect (Table EC15). Third, we examined the pre-expansion trends of the treatment group’s outcomes by expansion status, and found that the parallel trend assumption most likely holds (Table EC16). Lastly, there is consistent evidence that although Medicaid expansion is asso-

ciated with the changes in payer mix and financial margins of the hospitals, it does not impact their overall use or patterns of inpatient care (Pines et al. 2016, Freedman et al. 2017, Pickens et al. 2018).

We acknowledge that our IV analysis is not perfect, as it is applicable only to the hospitals whose behaviors are influenced by the IV. In addition, the instrument generates a variation at the state level, so within-state unobservable differences between the treatment and the control groups may still remain. Despite these, our IV approach provides us with a useful additional robustness check mechanism, and gives us further confidence about the validity of our main findings.

7.3. Temporal Effects

We further examined the robustness of our results by studying the temporal trend of closure effects (i.e., by separately measuring the effect for each observation year) that go beyond what we examined from our main model (e.g., three years post-closure). We employed the following model for testing hospital level outcomes (hypotheses 1 and 2):

$$Y_{jt} = \alpha_1 \mathbf{X}_{jt} + \alpha_2 \mathbf{Z}_{jt} + \text{HOSPITAL}_j + \text{YEAR}_t + \beta_1 \text{POSTYEAR1}_{jt} + \beta_2 \text{POSTYEAR2}_{jt} + \beta_3 \text{POSTYEAR3}_{jt} + \beta_4 \text{POSTYEAR4}_{jt} + \beta_5 \text{POSTYEAR5}_{jt} + \epsilon_{jt}. \quad (5)$$

To test patient level outcomes (hypotheses 3, 4, and 5), we utilized the following model:

$$Y_{ijt} = \alpha_1 \mathbf{X}_{it} + \alpha_2 \mathbf{Z}_{jt} + \text{HOSPITAL}_j + \text{AREA}_i + \text{YEAR}_t + \beta_1 \text{POSTYEAR1}_{jt} + \beta_2 \text{POSTYEAR2}_{jt} + \beta_3 \text{POSTYEAR3}_{jt} + \beta_4 \text{POSTYEAR4}_{jt} + \beta_5 \text{POSTYEAR5}_{jt} + \epsilon_{ijt}. \quad (6)$$

In both models (1) and (2), POSTYEAR1, ..., POSTYEAR5 are binary variables that indicate that the observation is made in each of the post-closure years for the treated group. All other variables are the same as our main equations.

Our results (Table EC17) show that the effects of hospital closures discussed earlier persist over time, though their significance typically decreases. This diminishing effect could be due to other contemporaneous market or policy changes that mask the closure effect. For example, many markets have experienced either an opening or an additional closure of hospitals within three years of closure. Alternatively, the diminishing effect could suggest that hospitals get accustomed to the increased demand over time. Nevertheless, the fact that we observe the same directional effects for every post-closure year as those in our main analyses gives us further confidence that our results are robust during our entire study period.

7.4. Other Robustness Checks

To gain further confidence in the validity of our results, we also performed various other robustness checks. Here, we first describe them and then discuss our related findings.

Risk Adjustment for Mortality. Although in our main analysis, we do risk-adjust for the patient’s underlying health conditions (e.g., by including the comorbidity and chronic conditions), it is possible that there are some other changes in the patient mix among neighboring hospitals that may drive the increase in mortality. For example, the affected hospitals may prioritize severe patients in admission given the increased demand, thus inadvertently increasing their mortality. To examine such a possibility, we examined the summary statistics and the tests of the sample mean before and after treatment for the variables that factor into the mortality rate (Table EC2). There is a slight change in age, gender, and HMO penetration, but the majority of the variables (11 out of 14), especially the ones related to patient health, do not change statistically significantly after their neighbors’ closures. In addition, as noted earlier, we used a DID model to test whether there is a significant change in patient characteristics such as age, sex, race, dual status, and admission type by examining the changes in the proportion of patients with certain characteristics per hospital before and after closure (Table EC10).

Treatment Variable. Our definition of the treatment group in our main analysis has a limitation in that patients can still visit other hospitals that are not in the nearest distance. Thus, we repeated our analyses using an alternative area-based definition of the treatment variable. Specifically, we assigned all hospitals located in the same healthcare market that experienced at least one closure to the treatment group, where the market was defined as an HRR.

Patients of Closed Hospitals. Because the patients of closed hospitals tend to be poorer and sicker (Table 3), and the patients of closed hospitals experience both the direct effect of losing access in addition to the potential spillover effect at nearby hospitals, we have removed them from our main analysis. To test the robustness of our findings, we included the patients of closed hospitals from the study population and re-estimated the closure effect.

Travel Distance. The changes in the travel distance as a result of closure may have contributed to the increased mortality. Thus, we repeated our analyses after including the distance between each patient and hospitals calculated based on their zip codes.

Fixed Observation Years. We look up to three years before and after the closure for hospitals in the treatment group, but some hospitals that experience closures toward the beginning or end of the observation period have missing years. To address the possibility that missing observations in these years are not at random, we limited the analysis to only the hospitals that have full observation years before and after closure.

Placebo Test. We randomly assigned hospital closures to some hospitals and patients in the control group and re-estimated the model with the placebo-treated group. Specifically, we randomly assigned the same number of closures as the original data to hospitals, and assigned up to three years post the closure year to the treatment group. We repeated the test 100 times to create 100 placebo sets, re-ran our analyses, and estimated effects and the 95% confidence intervals of hospital closure for each placebo test. Figure EC4 shows that only 5 out of 100 tests, 0 out of 100 tests, and 4 out of 100 tests for the outcomes operational efficiency, service duration, and 30-day mortality, respectively, have resulted in a statistically significant coefficient for the hospital closure.

Overall, these results of the robustness checks (Table EC18) indicate that our main findings are fairly robust: hospital closures increase the operational efficiency of nearby hospitals, but have negative consequences on some aspects of quality of care, especially the mortality rate. When the market based treatment definition was used, service duration decreased marginally (p-value < 0.1). This is likely because HRR encompasses a much wider area than a zip code, and more hospitals (especially the ones that are further away from the closed hospitals, and thus, are less likely to be chosen by the patients of closed hospitals) are included in the treatment group. When the patients of closed hospitals were included, our main effects were still consistent. The results of the placebo test showed no significant closure effect on all outcomes. Furthermore, the estimates of the rest of the outcomes were all consistent with our primary results. Finally, the fact that including the patients of closed hospitals and adding the changes in distance does not affect the spillover effect strengthens the interpretation of the speed-up behavior as the driver of adverse patient outcomes.

8. Policy Recommendations and Implications

Our results suggest that there are at least two policy levers that could be utilized by policymakers and hospital administrators to harness the positive spillover effect of hospital closures and/or mitigate their negative consequences. First, the fact that the effect of hospital closure depends on the neighbor hospital's characteristics indicates that bailing out hospitals that have specific characteristics can have a strong impact. Second, policies that can eliminate the speed-up behavior (e.g., appropriate monitoring and regulations against reductions in service durations) post a nearby hospital closure can be beneficial. In order to provide clear policy recommendations based on our results, we now perform various counterfactual analyses and examine the effectiveness of these policy levers (had they been utilized).

8.1. Policy Lever 1: Selective Hospital Closures and Bailouts

For policymakers, knowing “which” hospitals they should close or bail out under considerations of efficiency and quality can be highly informative. For example, there has been an ongoing debate on cutting the Medicaid DSH payment program or reforming the CAH status for financial support—two programs that, roughly speaking, try to bail out specific hospitals and prevent them from potential closures. To assist policymakers, we examined the hypothetical scenarios of bailout by calculating the benefits of bailing the hospitals with certain types of characteristics among all hospitals that had originally closed in our data ($N = 153$).

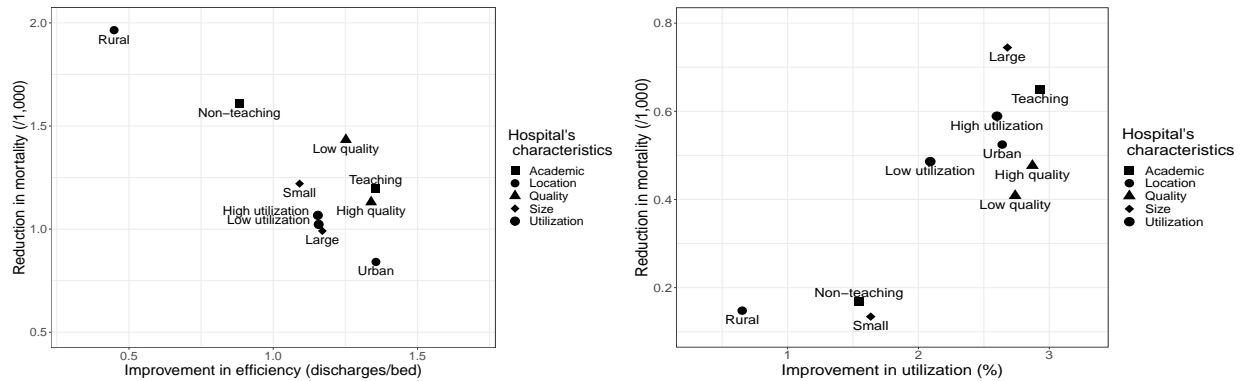
Figure 3(a) shows the average increases in operational efficiency and reduction in 30-day mortality under different scenarios compared to the case of hospital closures we originally observe in the data. Overall, our results indicate that there is no dominant strategy that can improve both efficiency and patient outcome. There is also wide heterogeneity in the magnitude of policy effect, which suggests that choosing specific types of hospitals to bail out can make a substantial difference in overall impact. Because our analysis suggests that the heterogeneity effect on mortality was not statistically significant, if greater weight is placed on improving efficiency, our results recommend bailing out hospitals that have more desirable (e.g., teaching, urban, high quality, and large) characteristics rather than those with less desirable (e.g., non-teaching, rural, low quality, and small) ones. However, these strategies can widen social disparities by accelerating the rural hospital closures.

8.2. Policy Lever 2: Selective Elimination of Speed-up Behavior

Our empirical findings suggest that the hospitals that speed up do so by reducing the service duration while keeping the bed utilization rate constant. Because limited data exist on interventions that can slow down the service to conserve value-added care (Fonarow et al. 2011, Meretoja et al. 2012), we focused on estimating the maximum achievable benefits from eliminating the speed-up behavior. We examined the hypothetical scenario when hospitals respond to the increase in patient demand by increasing their bed utilization rate instead of changing their service duration. By making use of our empirical model that shows how the changes in service duration affect the 30-day mortality, we also estimate the reduction in mortality as a result of eliminating the speed-up behavior.

Using our main model, we first predicted the changes in service duration in the absence of the speed-up behavior and translated it into the number of additional patient days for each hospital by multiplying the changes in service duration with the annual patient volume. Using the predicted

Figure 3: Counterfactual Results Under (a) Policy Lever 1: Selective Hospital Closures and Bailouts (Left) and (b) Policy Lever 2: Selective Elimination of Speed-up Behavior (Right)



bed days and service duration, we then re-estimated the impact of the closure on bed utilization and mortality. Our results show that without the speed-up behavior, a hospital's closure will, on average, increase its neighbors' bed utilization by 2.2%, and reduce their 30-day mortality rate by 0.08% such that instead of the observed 3.3% increase in the 30-day mortality in our main analysis, there is only a $3.3 - 0.08 = 3.22\%$ increase in mortality.

Next, we considered the cases where only certain types of hospitals (based on the hospital characteristics) are targeted to eliminate their speed-up behavior. Figure 3(b) shows the potential average gains in bed utilization and the reduction in mortality via policy interventions aimed at eliminating the speed-up behavior (e.g., monitoring and regulating service durations) compared to the status quo where hospitals speed up their services. We observe that in implementing policy lever 2, targeting hospitals that have more desirable (e.g., teaching, urban, high quality, and large) characteristics than the less desirable ones is a dominant strategy. This means that policymakers can focus their monitoring and regularization efforts of service durations on these types of markets or hospitals so as to gain the best results. However, these strategies again can widen social disparities by improving the outcomes of already more desirable hospitals.

9. Conclusion

We examined how an exit of a hospital from a market affects the remaining hospitals' operational efficiency and quality. Our results indicate that in response to the increase in patient demand, nearby hospitals improve their operational efficiency. However, this improvement in operational efficiency is not due to better utilization of resources but is instead due to a speed-up behavior as a response to the increase in demand. This speed-up behavior allows the remaining hospitals to serve more patients with their current level of resources. There is, however, an important negative consequence on some aspects of quality of care, especially an increase in the 30-day mortality rate.

Furthermore, the spillover effect of hospital closures is heterogeneous and is stronger when nearby hospitals have more desirable characteristics (e.g., teaching, urban, or high-quality hospitals).

Our empirical findings and counterfactual analyses suggest that targeted versions of some policies can be effective in harnessing the positive impacts of hospital closures and mitigating their negative consequences. For example, we find that (a) bailing out hospitals that have more desirable characteristics than their neighbors, and (b) reducing the speed-up behavior of the hospitals that have more desirable characteristics than their neighbors could be effective policies. Our results can be helpful for the current policy debates on rural hospital closures by showing that the targeted policy interventions that invest in hospitals with more desirable characteristics (e.g., teaching, urban, high quality, and large) can be effective. It should be noted, however, that our study has focused on understanding the spillover effect of closures (and thus these policies) in the dimensions of efficiency and quality, but policymakers should also consider other dimensions such as cost and equity. For example, while our study has not focused on the cost dimension, our estimates of the cost based on the cost of increased length of stay from previous studies (Bartel et al. 2014, Taheri et al. 2000) suggest that the average intervention cost for large hospitals can be up to five times greater than that of small hospitals (Figure EC5). Thus, it is likely that our policy recommendations are more costly than some other potential options.

In addition to policymakers, our findings can also help hospital administrators to adopt suitable strategies in response to a neighboring hospital closure. From hospitals' perspectives, the surge of patient demand as a result of a nearby hospital's closure may present an opportunity for improving profit margins. In particular, because Medicare pays for inpatient services mainly based on diagnosis-related groups (DRGs) that classify patients of similar clinical characteristics and costs rather than the length of stay, the strategy of speeding up to treat more patients might maximize the hospital's revenue in the short-term. Our results, however, point out that such a strategy can adversely affect their hospitals' long-term sustainability. In light of the recent payment reforms that emphasize the role of hospital quality outcomes (The Centers for Medicare & Medicaid Services 2017) and the growing role for hospitals' quality outcomes on patients' choice (Saghafian and Hopp 2019, 2020), hospital administrators should be aware that deterioration in key quality measures as a result of speed-up can result in a loss of patient share for their hospital.

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Appendix

EC1. Independent Variables

Transfer patients: Transfer patients were identified as those with the source of inpatient admission code “transfer from a different facility,” “transfer from ER,” or “transfer from the same facility.” Admissions for rehabilitation were identified from the presence of ICD-9 codes indicating care involving the use of rehabilitation procedures: V570, V571, V5721, V573, V5781, V5789, V579, and 462.

Categories of diagnosis for inpatient visit: We divided the admissions into 15 clinical categories based on the primary diagnosis codes. These categories include: infections and parasitic diseases, neoplasms, endocrine, nutritional and metabolic diseases, and immunity disorders, diseases of the blood and blood-forming organs, mental disorders, diseases of the nervous system and sense organs, diseases of the circulatory system, diseases of the respiratory system, diseases of the digestive system, diseases of the genitourinary system, diseases of the skin and subcutaneous tissue, diseases of the musculoskeletal system and connective tissue, congenital anomalies, symptoms, signs, and ill-defined conditions, and injury and poisoning (Organization et al. 1988).

EC2. Analysis for All U.S. Population

EC3. Instrumental Variable (IV) Analysis

As noted in Section 5.2, our fixed effects model may not fully address the time-varying unobservable confounders that can bias our results. Therefore, we incorporated an IV analysis in our DID design (i.e., instrumented difference-in-differences, or DDIV) by identifying an IV that can account for unmeasured confounders (Duflo 2001). Specifically, we made use of the state level variations in the decision to expand Medicaid as an instrument that influences the likelihood of hospital closures but is unlikely to be correlated with our outcome variables. The Affordable Care Act (ACA) originally intended to expand Medicaid coverage to low-income adults, but the provision was ruled coercive by the supreme court. Therefore, each state could choose to expand or not expand Medicaid, which created a variation in Medicaid eligibility by state. Evidence shows that the expansion is associated with improved hospital financial performance and a lower likelihood of hospital closure (Lindrooth et al. 2018, Blavin 2016). Using these facts, we specified our first-stage equation as:

$$\text{POSTCLOSURE}_{jt} = \delta_1 \mathbf{X}_{jt} + \delta_2 \mathbf{Z}_{jt} + \mu \text{MEDICAID}_{jt} + \text{HOSPITAL}_j + \text{YEAR}_t + \nu_{jt}, \quad (7)$$

where MEDICAID_{jt} denotes whether hospital j ’s state expanded Medicaid in year t and POSTCLOSURE_{jt} indicates whether the neighbor hospital of hospital j is closed in year t . Our second-stage equation for hospital level outcomes is

$$Y_{jt} = \alpha_1^{IV} \mathbf{X}_{jt} + \alpha_2^{IV} \mathbf{Z}_{jt} + \beta^{IV} \widehat{\text{POSTCLOSURE}}_{jt} + \text{HOSPITAL}_j + \text{YEAR}_t + \epsilon_{jt}^{IV}, \quad (8)$$

and for patient level outcomes is

$$Y_{ijt} = \alpha_1^{IV} \mathbf{X}_{it} + \alpha_2^{IV} \mathbf{Z}_{jt} + \beta^{IV} \widehat{\text{POSTCLOSURE}}_{jt} + \text{HOSPITAL}_j + \text{AREA}_i + \text{YEAR}_t + \epsilon_{ijt}^{IV}, \quad (9)$$

where $\widehat{\text{POSTCLOSURE}}$ is the estimated value from the first-stage equation (7), and β^{IV} is the impact of hospital closures on outcome variables adjusting for the selection using the instrument.

The key assumptions for our IV approach are: (1) the instrument does not affect the outcome except through treatment (exclusion restriction), and (2) the instrument is associated with the treatment variable (Hudson et al. 2017). Available studies in the literature suggest that Medicaid expansion—our IV—is strongly correlated with our treatment variable (see, e.g., Lindrooth et al. (2018)), and hence, assumption (2) holds. Our direct tests on the level of correlation between Medicare expansion and our treatment variable further confirm this (see Table EC14).

However, unlike assumption (2), we cannot directly test assumption (1). Several studies suggest that although Medicaid expansion is associated with the changes in payer mix and financial margins of the hospitals, it does not impact their overall use or patterns of inpatient care (Pines et al. 2016, Freedman et al. 2017, Pickens et al. 2018). Nevertheless, to gain further confidence, we tested if Medicaid expansion is associated with changes in the hospital level outcomes among hospitals that are in the control group. Our results suggest that assumption (1) most likely holds for hospital level outcomes (see Table EC15). For patient level outcomes, we note that because our study population is Medicare beneficiaries who have already had insurance coverage, the expansion of Medicaid is unlikely to affect their care patterns.

In addition to the two above-mentioned assumptions required for an IV approach, the DDIV approach that we employ requires two more assumptions: (3) in the absence of treatment, the difference between the treatment and control group is constant over time, and hence, shows a parallel trend, and (4) the effect of the instrument is monotone (Hudson et al. 2017). Assumption (4) is well-satisfied, since once a hospital closes, it will stay closed throughout the study period by our definition of a closure. To verify the parallel trend assumption in (3), we examined the pre-expansion trends of the treatment group’s outcomes by expansion status. Our results indicate that the parallel trend assumption most likely holds. However, it should be noted that our IV estimate is applicable only to the hospitals whose behaviors are influenced by the IV. In addition, the instrument generates a variation at the state level, so within-state unobservable differences between the treatment and the control groups may still remain. Despite these, our IV analysis helps to validate the findings from our primary analysis and provides us with a useful additional robustness check mechanism.

Table EC14 shows the result of the first stage and second stage regression estimates (Equations (8)-(9)). The first column shows that Medicaid expansion is a significant predictor of fewer closures of nearby hospitals. The subsequent columns show the impact of hospital closures on the nearby hospital’s operational efficiency and quality. The fact that the results of our IV analysis and the DID analysis are consistent gives us confidence that our results are fairly robust.

EC4. Cost Estimation for Policy Levers

For selective bailout policy, we calculated the bailout cost as the DSH savings that incur as a result of the closures by calculating the average DSH payment made to each hospital type in 2015. For selective elimination of speed-up policy, we estimated the cost of such interventions based on the cost of increased length of stay from previous studies (Bartel et al. 2014, Taheri et al. 2000). Specifically, the annual per hospital cost of eliminating the speed-up behavior was calculated as the product of the inflation-adjusted per-patient cost of the stay for an additional day (\$611 per patient in 2015, see Taheri et al. (2000)), the magnitude of the speed-up behavior, and the annual patient volume per hospital.

EC5. Tables and Figures

Table EC1: Summary of Hospital and Patient Characteristics, Matched Sample, (a) Hospital Characteristics Only (Top) and (b) Both Hospital and Patient Characteristics (Bottom)

	Treat	Control	P-value
<i>Hospital characteristics only</i>			
Total (n)	158	158	
Observation years	3,229	2,367	
DSH (%)	82.9	80.7	0.04
Teaching hospitals (%)	46.9	45.8	0.45
Ownership-Nonprofit (%)	61.6	60.2	0.25
Ownership-Private (%)	18.1	17.6	
Ownership-Public (%)	20.3	22.2	
Rural (%)			< 0.001
Avg Star Rating	2.8	3.0	< 0.001
Avg Discharges	12,079.7	12,526.1	0.19
Avg Beds	246.9	254.3	0.22
<i>Hospital and patient characteristics</i>			
Total (n)	149	149	
Observation years	3,140	1,982	
DSH (%)	84.1	59.1	< 0.001
Teaching hospitals (%)	47.8	31.8	< 0.001
Ownership-Nonprofit (%)	61.8	61.0	0.18
Ownership-Private (%)	17.3	16.0	
Ownership-Public (%)	20.9	23.0	
Rural (%)			< 0.001
Avg Star Rating	2.7	3.2	< 0.001
Avg Discharges	12,315.4	6,273.0	< 0.001
Avg Beds	251.4	139.0	< 0.001

Table EC2: Summary of Treatment Group Hospital Characteristics Before and After Closure

	Before	After	P-value
Avg Age	78.9	79.2	0.043
Avg Male (%)	0.4	0.4	0.035
Avg White (%)	0.8	0.8	0.33
Avg Black (%)	0.1	0.1	0.51
Avg Duals (%)	0.3	0.3	0.91
Avg Emergent (%)	0.6	0.6	0.37
Avg Urgent (%)	0.2	0.2	0.83
Avg Elective (%)	0.2	0.2	0.32
Avg HMO pen (%)	0.2	0.2	< 0.001
Avg HHI	1,246.8	1,203.0	0.4
Avg Pop (n)	1,332,492.0	1,530,215.0	0.094
Avg Unemp (%)	9.3	9.2	0.68
Avg Poverty (%)	17.4	17.0	0.11
Avg Under 65 (%)	84.0	84.0	0.88

Table EC3: Hospital, Market, and Patient Characteristics in the Treatment and Control Groups

	Treatment	Control
Hospitals (n)	158	3,141
Hospitals (hospital-year)	1,009	30,656
Patients (n)	175,694	4,537,801
Inpatient visits (n)	418,175	13,626,652
<i>Hospital characteristics</i>		
DSH (%)	82.1	80.4
Teaching hospitals (%)	46.2	31.9
Ownership – nonprofit (%)	62.1	61.5
Ownership – private (%)	18.3	22.8
Ownership – public (%)	19.5	15.8
Rural (%)	24.4	33.7
Avg Star rating	2.8	3.0
Avg patient experience (% good)	66.0	67.4
Avg discharges (n)	11422.8	11238.4
Avg discharges, Medicare (n)	3442.6	3564.6
Avg beds (n)	235.2	227.3
Avg operational efficiency	44.4	44.8
Avg operational efficiency, Medicare	14.7	15.4
Avg bed utilization	0.56	0.57
Avg bed utilization, Medicare (%)	21.1	22.7
<i>Market characteristics</i>		
Avg MA penetration rate (%)	19.4	16.9
Avg Herfindahl-Hirschman Index	1,186.2	1,460.4
Avg total population (n)	1,520,613	1,024,876
Avg unemployment rate (%)	9.2	9.0
Avg poverty rate (%)	17.0	16.1
Avg under age 65 (%)	83.8	83.0
Avg hospitals (/10,000)	0.26	0.27
<i>Patient demographics</i>		
Avg age (years)	79.25	78.97
Sex – male (%)	41.34	42.10
Race – White (%)	82.49	87.20
Race – Black (%)	11.65	8.36
Race – Asian (%)	1.98	1.33
Race – Hispanic (%)	1.98	1.71
Dual-eligibles (%)	18.93	17.24
Avg comorbidity score	3.62	3.36
Avg chronic conditions (n)	24.02	23.71
<i>Patient clinical</i>		
Admissions – circulatory (%)	20.02	19.64
Admissions – digestive (%)	5.20	5.44
Admissions – endocrine (%)	2.64	2.29
Admissions – infectious (%)	46.66	46.14
Admissions – injury (%)	6.67	6.92
Admissions – musculoskeletal (%)	6.35	7.42
Admissions – nervous (%)	1.27	1.05
Admissions – respiratory (%)	5.19	5.40
Avg service duration (days)	5.14	4.80
Avg readmission (%)	17.44	16.03
Avg mortality (%)	5.96	6.38

Note. The summary statistics include all observations from pre and post closure years. DSH indicates Disproportionate Share Hospital. MA indicates Medicare Advantage. Dual-eligibles indicate Medicare-Medicaid dual eligible beneficiaries. For hospital level variables, all differences in covariates between the two groups were statistically significant at p-value < 0.001, except DSH (p-value 0.21), avg. bed utilization (p-value 0.10), avg. unemployment (p-value 0.07), and avg. hospital (p-value 0.29). For patient level variables, all differences in covariates between the two groups were statistically significant at p-value < 0.001.

Table EC4: Results of Pre-Treatment Parallel Test

	<i>Dependent variable:</i>	
	Efficiency	Utilization
	(1)	(2)
PRECLOSURE3	−0.426 (0.229)	−0.005 (0.004)
PRECLOSURE2	−0.734 (0.434)	−0.010 (0.008)
PRECLOSURE1	−0.159 (0.352)	−0.004 (0.006)
POSTCLOSURE1	0.601† (0.303)	0.004 (0.005)
POSTCLOSURE2	0.855† (0.417)	0.006 (0.005)
POSTCLOSURE3	0.913† (0.478)	0.008 (0.006)
Observations	45,764	45,767
R ²	0.887	0.872
Adjusted R ²	0.871	0.852
Residual Std. Error	2.780 (df = 39839)	0.054 (df = 39841)
<i>Note:</i>	†p<0.1; *p<0.05; **p<0.01; ***p<0.001	

Table EC5: Full Difference-In-Differences Results Using Unmatched Sample

	<i>Dependent variable:</i>			<i>Dependent variable:</i>		
	medicare_efficiency	medicare_utilization		Service duration	30-day readmission	30-day mortality
	(1)	(2)		(3)	(4)	(5)
age	-0.029 (0.016)	0.001* (0.0002)	race_black	0.212*** (0.016)	0.005*** (0.001)	-0.006*** (0.0004)
sex_prop_male	-0.122 (0.136)	-0.009* (0.003)	race_other	-0.050* (0.018)	-0.009*** (0.002)	-0.005*** (0.001)
race_prop_black	-0.808 (0.411)	-0.009 (0.010)	race_aapi	-0.086** (0.021)	-0.011*** (0.002)	-0.009*** (0.001)
race_prop_hispanic	-0.431 (0.521)	-0.006 (0.013)	race_hispanic	-0.194*** (0.011)	-0.008*** (0.002)	-0.011*** (0.001)
buyin_months	-0.082 (0.250)	0.001 (0.005)	race_native	0.052 (0.027)	-0.00005 (0.004)	0.001 (0.002)
type_adm_urgent	1.157 (0.751)	0.029 (0.017)	sex_female	0.013 (0.015)	-0.002 (0.001)	-0.012*** (0.0002)
type_adm_elective	1.463 (0.784)	0.039 (0.017)	age_75to84	0.234*** (0.013)	0.005** (0.001)	0.017*** (0.0002)
type_adm_emergent	1.556 (0.801)	0.048* (0.017)	age_85above	0.272*** (0.023)	0.003* (0.001)	0.056*** (0.0004)
prop_hmo	-4.845** (1.273)	-0.061* (0.020)	ind_dual	0.207*** (0.014)	0.013*** (0.001)	0.003*** (0.0003)
hhi	-0.001** (0.0002)	-0.00001* (0.00000)	type_adm_emergent	1.210*** (0.082)	0.071*** (0.004)	0.001** (0.001)
population	-0.00000 (0.00000)	-0.000 (0.000)	type_adm_urgent	1.374*** (0.092)	0.043*** (0.004)	-0.007*** (0.001)
unemployment	-0.186 (0.212)	-0.004 (0.007)	type_adm_elective	0.562*** (0.104)	0.042*** (0.005)	-0.035*** (0.001)
poverty	-0.014 (0.050)	0.0004 (0.001)	type_adm_newborn	1.317** (0.377)	0.039 (0.018)	-0.015 (0.013)
under_65	0.177 (0.098)	0.002 (0.002)	type_adm_trauma	1.690*** (0.096)	0.045*** (0.004)	-0.001 (0.002)
resources	(0.000)	(0.000)	type_adm_others	0.936*** (0.143)	0.049*** (0.007)	-0.007* (0.003)
POSTCLOSURE	0.574* (0.182)	0.004 (0.003)	crec	-0.089** (0.024)	0.0005 (0.004)	-0.001 (0.002)
			esrd	0.418*** (0.048)	0.047*** (0.002)	0.003*** (0.001)
			chronic_conditions	-0.002 (0.001)	-0.00003 (0.0001)	0.0001*** (0.00001)
			comorbidity	0.244*** (0.008)	0.024*** (0.001)	0.005*** (0.00005)
			population	-0.00000*** (0.000)	0.000 (0.000)	-0.000 (0.000)
			unemployment	0.016*** (0.001)	-0.001*** (0.0002)	0.0003** (0.0001)
			poverty	-0.002** (0.001)	0.0004*** (0.0001)	0.0001* (0.00004)
			under_65	-0.0001 (0.0005)	0.0003** (0.0001)	-0.0001* (0.00005)
			resources	1,544.631*** (127.925)	-33.725* (10.397)	15.376* (6.836)
			rural	0.126*** (0.007)	-0.011*** (0.001)	0.001** (0.0004)
			hhi	0.00001 (0.00001)	-0.00000 (0.00000)	0.00000** (0.00000)
			hmo	-0.179*** (0.028)	0.019*** (0.002)	0.012*** (0.002)
			POSTCLOSURE	-0.047* (0.019)	0.002 (0.002)	0.002** (0.001)
Observations	45,764	45,767	Observations	10,368,445	10,368,445	10,363,945
R ²	0.887	0.872	R ²	0.083	0.047	0.027
Adjusted R ²	0.871	0.852	Adjusted R ²	0.083	0.047	0.027
Residual Std. Error	2.780 (df = 39844)	0.054 (df = 39846)	Res. Std. Error	3.949 (df = 10365409)	0.360 (df = 10365409)	0.243 (df = 10360613)

Note. *p<0.05; **p<0.01; ***p<0.001. DSH indicates Disproportionate Share Hospitals. Dual-eligibles indicate Medicare-Medicaid dual eligibles. All differences in covariates between the two groups were statistically significant at p-value < 0.05.

Table EC6: Full Difference-In-Differences Results Using Matched Sample

	<i>Dependent variable:</i>			<i>Dependent variable:</i>		
	medicare_efficiency	medicare_utilization		Service duration	30-day readmission	30-day mortality
	(1)	(2)		(3)	(4)	(5)
age	-0.061 (0.061)	-0.0003 (0.001)	race_black	0.223*** (0.040)	0.005* (0.002)	-0.006*** (0.001)
sex_prop_male	-0.201 (0.893)	-0.020 (0.014)	race_other	-0.016 (0.046)	-0.013* (0.005)	-0.005** (0.002)
race_prop_black	-3.075* (1.127)	-0.089 (0.049)	race_aapi	-0.190** (0.063)	-0.014*** (0.002)	-0.006* (0.003)
race_prop_hispanic	-0.732 (1.889)	-0.065 (0.053)	race_hispanic	-0.220*** (0.049)	-0.002 (0.003)	-0.011*** (0.002)
buyin_months	-2.148 (1.413)	-0.047 (0.022)	race_native	0.151 (0.084)	0.004 (0.009)	0.011* (0.005)
type_adm_urgent	-0.170 (2.602)	-0.033 (0.035)	sex_female	-0.002 (0.009)	-0.0002 (0.001)	-0.012*** (0.001)
type_adm_elective	0.011 (2.662)	-0.028 (0.036)	age_75to84	0.221*** (0.013)	0.003 (0.002)	0.018*** (0.001)
type_adm_emergent	-0.679 (2.665)	-0.037 (0.038)	age_85above	0.251*** (0.031)	0.002 (0.002)	0.054*** (0.001)
prop_hmo	-4.544* (1.966)	-0.046 (0.028)	ind_dual	0.128** (0.041)	0.013*** (0.002)	0.001 (0.001)
hhi	0.0003 (0.002)	0.00001 (0.00001)	type_adm_urgent	0.218*** (0.045)	-0.031*** (0.003)	-0.008*** (0.001)
population	0.00002*** (0.00000)	0.00000*** (0.00000)	type_adm_elective	-0.790*** (0.068)	-0.029*** (0.002)	-0.036*** (0.001)
unemployment	7.023*** (0.500)	0.093*** (0.007)	type_adm_newborn	0.064 (0.870)	-0.063 (0.051)	-0.004 (0.035)
poverty	1.580*** (0.145)	0.018*** (0.002)	type_adm_trauma	0.411** (0.138)	-0.028** (0.008)	-0.003 (0.003)
under_65	-5.784*** (0.445)	-0.073*** (0.007)	type_adm_others	-0.577** (0.215)	-0.030 (0.014)	-0.019*** (0.006)
resources	0.001 (0.000)	0.001 (0.000)	crec	-0.148* (0.073)	-0.003 (0.007)	-0.004 (0.004)
POSTCLOSURE	0.771* (0.275)	0.007 (0.004)	esrd	0.442*** (0.042)	0.042*** (0.003)	0.0003 (0.002)
			chronic_conditions	-0.004*** (0.001)	-0.00001 (0.0001)	0.0001* (0.00003)
			comorbidity	0.242*** (0.005)	0.024*** (0.001)	0.005*** (0.0001)
			population	0.000 (0.000)	0.000 (0.000)	-0.000** (0.000)
			unemployment	0.009 (0.009)	-0.002* (0.001)	-0.0005 (0.0004)
			poverty	0.002 (0.003)	0.0003 (0.0002)	0.0003** (0.0001)
			under_65	0.003 (0.004)	0.0002 (0.0002)	-0.0002 (0.0002)
			resources	584.460 (476.608)	18.889 (39.630)	19.727 (18.534)
			rural	0.132*** (0.037)	-0.009** (0.002)	0.001 (0.001)
			hhi	-0.0001 (0.00004)	-0.00000 (0.00000)	0.00000* (0.00000)
			hmo	-1.405*** (0.128)	0.024** (0.006)	0.008 (0.004)
			POSTCLOSURE	-0.119** (0.036)	0.002 (0.002)	0.002** (0.001)
Observations	4,163	4,163	Observations	1,623,079	1,623,932	1,623,079
R ²	0.872	0.873	R ²	0.081	0.049	0.027
Adjusted R ²	0.858	0.858	Adjusted R ²	0.081	0.049	0.026
Residual Std. Error	2.732 (df = 3726)	0.041 (df = 3726)	Res. Std. Error	4.152 (df = 1622406)	0.366 (df = 1623555)	0.242 (df = 1622406)

Note. *p<0.05; **p<0.01; ***p<0.001. DSH indicates Disproportionate Share Hospitals. Dual-eligibles indicate Medicare-Medicaid dual eligibles. All differences in covariates between the two groups were statistically significant at p-value < 0.05.

Table EC7: Difference-in-Differences Estimates: Heterogeneous Effect by Baseline Utilization

	<i>Dependent variable:</i>			
	medicare_efficiency		medicare_utilization	
	(1)	(2)	(3)	(4)
POSTCLOSURE	0.999* (0.368)	0.971* (0.368)	0.008 (0.004)	0.007 (0.005)
Observations	34,993	36,876	34,996	36,878
R ²	0.850	0.866	0.808	0.829
Adjusted R ²	0.828	0.847	0.779	0.805
Residual Std. Error	2.909 (df = 30367)	2.909 (df = 32292)	0.053 (df = 30369)	0.059 (df = 32294)

Note:

*p<0.05; **p<0.01; ***p<0.001

Note. Baseline utilization for each hospital was defined as the utilization for the first year of observation. Hospitals were then categorized into high and low baseline utilization groups depending on whether their baseline utilization is greater than or less than the median baseline utilization (i.e., 0.45 for the overall utilization and 0.20 for the Medicare patient utilization).

Table EC8: Difference-in-Differences Estimates: Effect of Hospital Closure on Admission Types

	<i>Dependent variable:</i>		
	type_adm_emergent	type_adm_urgent	type_adm_elective
	(1)	(2)	(3)
POSTCLOSURE	-0.001 (0.006)	-0.006 (0.008)	0.007 (0.009)
Observations	45,773	45,773	45,773
R ²	0.885	0.864	0.801
Adjusted R ²	0.868	0.843	0.772
Residual Std. Error (df = 39852)	0.123	0.140	0.149

Note:

*p<0.05; **p<0.01; ***p<0.001

Note. We first measured the admission type rate for each hospital-year as the patient volume of each admission type (e.g., emergent, urgent, and elective) out of all patient volume. We then examined the changes in the admission type rate as a function of neighbor hospitals' closure, using the same model as our main DID model.

Table EC9: Difference-in-Differences Estimates: Heterogeneous Effect of Hospital Closure on Mortality by Admission Types

	<i>Dependent variable:</i>
	30-day mortality
type_adm_urgent	-0.008*** (0.001)
type_adm_elective	-0.036*** (0.001)
POSTCLOSURE	0.002** (0.001)
type_adm_urgent:POSTCLOSURE	-0.001 (0.002)
type_adm_elective:POSTCLOSURE	0.001 (0.002)
Observations	1,614,332
R ²	0.027
Adjusted R ²	0.026
Residual Std. Error	0.242 (df = 1613660)

Note:

*p<0.1; **p<0.05; ***p<0.01

Table EC10: Difference-in-Differences Estimates: Changes in Patient Mix

	<i>Dependent variable:</i>			
	age	sex	race	dual
	(1)	(2)	(3)	(4)
POSTCLOSURE	0.095 (0.086)	0.0004 (0.006)	0.006 (0.006)	-0.004 (0.004)
Observations	4,163	4,163	4,163	4,163
R ²	0.805	0.440	0.956	0.915
Adjusted R ²	0.783	0.375	0.951	0.905
Residual Std. Error	1.127 (df = 3727)	0.070 (df = 3727)	0.047 (df = 3728)	0.056 (df = 3727)
<i>Note:</i>			*p<0.05; **p<0.01; ***p<0.001	

Table EC11: Results of Mediation Analysis

	<i>Dependent variable:</i>	
	readmissions	mortality
	(1)	(2)
SERV	-0.0001 (0.0001)	-0.0002* (0.0001)
LAGGED_POSTCLOSURE	0.001 (0.001)	0.002 (0.002)
Observations	1,623,932	1,623,079
R ²	0.049	0.027
Adjusted R ²	0.049	0.027
Residual Std. Error	0.366 (df = 1623555)	0.242 (df = 1622406)
<i>Note:</i>		
*p<0.05; **p<0.01; ***p<0.001		

Table EC12: Difference-in-Differences Estimates: Average Effect of Hospital Closure on Patient Experience

	<i>Dependent variable:</i>				
	overall	doctor communicate	nurse communicate	quick help	staff explain
	(1)	(2)	(3)	(4)	(5)
POSTCLOSURE	-0.508 (0.320)	-0.340 (0.218)	-0.486 (0.257)	-0.443 (0.337)	-0.682* (0.331)
Observations	24,997	24,996	24,997	24,994	24,979
R ²	0.842	0.807	0.823	0.852	0.728
Adjusted R ²	0.811	0.769	0.789	0.823	0.675
Residual Std. Error	4.134 (df = 20914)	2.649 (df = 20914)	2.980 (df = 20914)	4.006 (df = 20911)	4.122 (df = 20902)
<i>Note:</i>					
*p<0.05; **p<0.01; ***p<0.001					
	<i>Dependent variable:</i>				
	pain control	area quiet	room clean	discharge info	recommend
	(1)	(2)	(3)	(4)	(5)
POSTCLOSURE	-0.611* (0.278)	-0.282 (0.380)	-0.487 (0.299)	-0.670** (0.226)	-0.620 (0.349)
Observations	24,988	24,997	24,996	24,992	24,997
R ²	0.686	0.876	0.806	0.772	0.856
Adjusted R ²	0.624	0.851	0.768	0.727	0.827
Residual Std. Error	3.626 (df = 20908)	4.110 (df = 20914)	3.884 (df = 20914)	2.765 (df = 20910)	4.194 (df = 20914)
<i>Note:</i>					
*p<0.05; **p<0.01; ***p<0.001					

Table EC13: Results for All U.S. Population

	Hypothesis Outcomes	(1) Operational efficiency	(2) Bed utilization	(4) 30-day readmission	(5) 30-day mortality
Unmatched sample	Nearby hospital's closure	1.63* (0.64)	0.009 (0.006)	0.002 (0.002)	0.002** (0.001)
	Number of observations	45,773	45,773	10,368,445	10,363,945
	Adjusted R^2	0.87	0.83	0.05	0.03

Note. All models include hospital, area, and year fixed effects. Standard errors are in parentheses. Standard errors are robust and clustered at the hospital and the year levels. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

Table EC14: Instrumental Variable Estimates

	First stage Post-closure	Second stage (1) Operational efficiency	(2) Bed utilization	(3) Service duration	(4) Readmission	(5) Mortality
State Medicaid expansion	-0.013** (0.004)					
Nearby hospital's closure		0.94** (0.36)	0.01 (0.0049)	-0.13*** (0.010)	-2.1 (1.63)	-0.007 (0.012)
Adjusted R^2	0.51	0.76	0.35	0.08	0.35	0.05
Number of observations	24,440	35,315	35,315	9,830,849	35,315	9,830,849
Weak identification (F-stat)	91.62					

Note. The F-statistics test for identifying the weak instrument is based on (Stock and Yogo 2002). The rule of thumb suggests that a first stage F-statistic below 10 indicates the presence of weak instruments. All models include hospital, area, and year fixed effects. Standard errors are in parentheses. Standard errors are robust and clustered at the hospital and the year levels. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

Table EC15: IV Robustness Check: Impact of Medicaid Expansion Among Control Group

	Dependent variable:	
	discharges (1)	efficiency (2)
POSTCLOSURE	-219.059 (101.722)	-0.598 (0.334)
Observations	41,797	41,797
R^2	0.981	0.886
Adjusted R^2	0.978	0.869
Residual Std. Error (df = 36307)	1,325.745	7.303

Note: * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$

Note. Our study population here are the hospitals in the control group, i.e., those who are not the neighbors of closed hospitals. We then ran a new DID analysis where hospitals in the Medicaid expansion states were assigned to the new treatment group, and those in the non-expansion states were assigned to the new control group. Our results show that in the absence of hospital closures, the hospitals do not experience any significant changes in patient volume and efficiency after Medicaid expansion.

Table EC16: IV Robustness Check: Pre-Trend Test

	<i>Dependent variable:</i>				
	efficiency (1)	utilization (2)	service duration (3)	readmissions (4)	mortality (5)
PRE_EXPANSION1	0.867 (1.585)	-0.0001 (0.011)	-0.043 (0.084)	0.00055 (0.0073)	0.00088 (0.0014)
PRE_EXPANSION2	1.068 (0.962)	0.004 (0.010)	-0.048 (0.063)	0.00022 (0.00099)	0.00073 (0.0077)
PRE_EXPANSION3	-0.420 (0.772)	-0.006 (0.009)	0.059 (0.082)	0.0019 (0.0083)	0.00071 (0.012)
Observations	1,694	1,694	541,026	541,026	541,026
R ²	0.803	0.891	0.08	0.05	0.03
Adjusted R ²	0.781	0.879	0.08	0.05	0.03

Note: *p<0.05; **p<0.01; ***p<0.001

Note. We examined, among the hospitals in our treatment group (i.e., neighbors of closed hospitals), whether the hospitals in the Medicaid expansion states and the non-expansion states show different trends before the expansion year. Our results show that there is no statistically significant different trend between the two groups.

Table EC17: Results of Temporal Effect

	<i>Dependent variable:</i>				
	efficiency (1)	utilization (2)	service duration (3)	readmissions (4)	mortality (5)
POSTCLOSURE1	0.819* (0.286)	0.005 (0.004)	-0.670* (0.31)	0.0009 (0.001)	0.0024* (0.001)
POSTCLOSURE2	1.082* (0.376)	0.007 (0.004)	-0.356* (0.17)	0.0016 (0.001)	0.0023* (0.001)
POSTCLOSURE3	1.148* (0.445)	0.009 (0.006)	-0.458* (0.23)	0.0019 (0.002)	0.0017 (0.001)
POSTCLOSURE4	0.519 (0.380)	-0.021 (0.004)	-0.024 (0.024)	0.0001 (0.002)	0.0007 (0.002)
POSTCLOSURE5	0.243 (0.490)	-0.073 (0.007)	0.034 (0.040)	0.0001 (0.003)	0.0004 (0.002)
Observations	45,764	45,767	10,368,445	10,368,445	10,368,445
R ²	0.89	0.87	0.08	0.06	0.04
Adjusted R ²	0.87	0.85	0.07	0.05	0.03

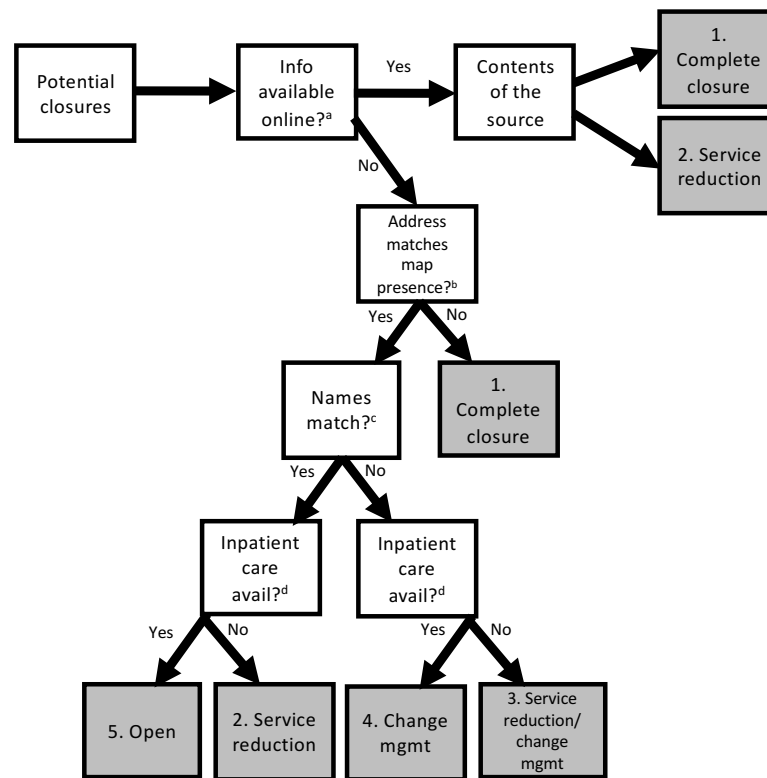
Note: *p<0.05; **p<0.01; ***p<0.001

Table EC18: Robustness Checks Results

Hypothesis Outcomes		(1) Operational efficiency	(2) Bed utilization	(3) Service duration	(4) Readmission	(5) Mortality
Risk adjustment for mortality	Nearby hospital's closure	2.84** (1.01)	0.01 (0.01)	-0.11** (0.04)	0.001 (0.004)	0.002* (0.001)
	Adjusted R^2	0.75	0.85	0.08	0.04	0.03
	Number of observations	25,813	25,813	13,539,638	13,539,638	13,539,638
Treatment variable – HRR defined	Nearby hospital's closure	1.61*** (0.47)	0.007 (0.005)	-0.006† (0.004)	0.002 (0.001)	0.002* (0.001)
	Adjusted R^2	0.79	0.88	0.10	0.04	0.04
	Number of observations	16,601	16,601	3,103,077	3,103,077	3,103,077
Include patients of – closed hospitals	Nearby hospital's closure	NA ¹	NA ¹	-0.13** (0.04)	0.003 (0.004)	0.001† (0.001)
	Adjusted R^2	NA	NA	0.08	0.04	0.03
	Number of observations	NA	NA	13,398,447	13,398,447	13,398,447
Travel distance	Nearby hospital's closure	1.85* (0.82)	0.014 (0.007)	-0.18*** (0.041)	-0.0003 (0.005)	0.003* (0.001)
	Adjusted R^2	0.78	0.86	0.08	0.04	0.03
	Number of observations	28,757	28,757	10,905,790	10,905,790	10,905,790
Fixed observation – years	Nearby hospital's closure	1.87* (0.94)	0.011 (0.010)	-0.012* (0.005)	-0.0003 (0.001)	0.001* (0.0005)
	Adjusted R^2	0.78	0.86	0.08	0.04	0.03
	Number of observations	28,212	28,212	10,711,050	10,711,050	10,711,050

Note. All models include hospital, area, and year fixed effects. Standard errors are in parentheses. Standard errors are robust and clustered at the hospital and the year levels. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$; †A marginal increase at p -value < 0.10 .
¹Removing the patients of closed hospitals only affects the patient-level analyses. ²Including the hospital level data only affects the hospital-level analyses.

Figure EC1: The General Procedure for Identifying Hospital Closures



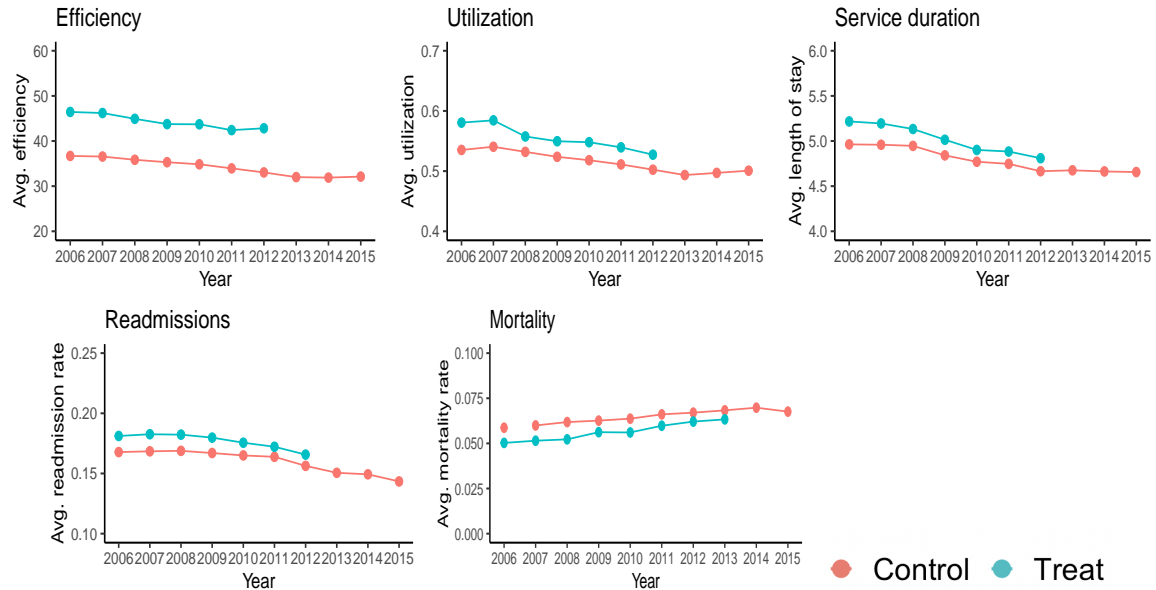
Notes.

- Google searched "Name" + "Closure/merger" for information on closures from local news, Yelp, or Wikipedia.
- Examined the physical presence through google map streetview and the date.
- Do names of the physical building and the original source match?
- Checked the hospital's website to see if inpatient care is available. If the information is not available online, called the hospital.

Figure EC2: Timeline of Hospital Closures

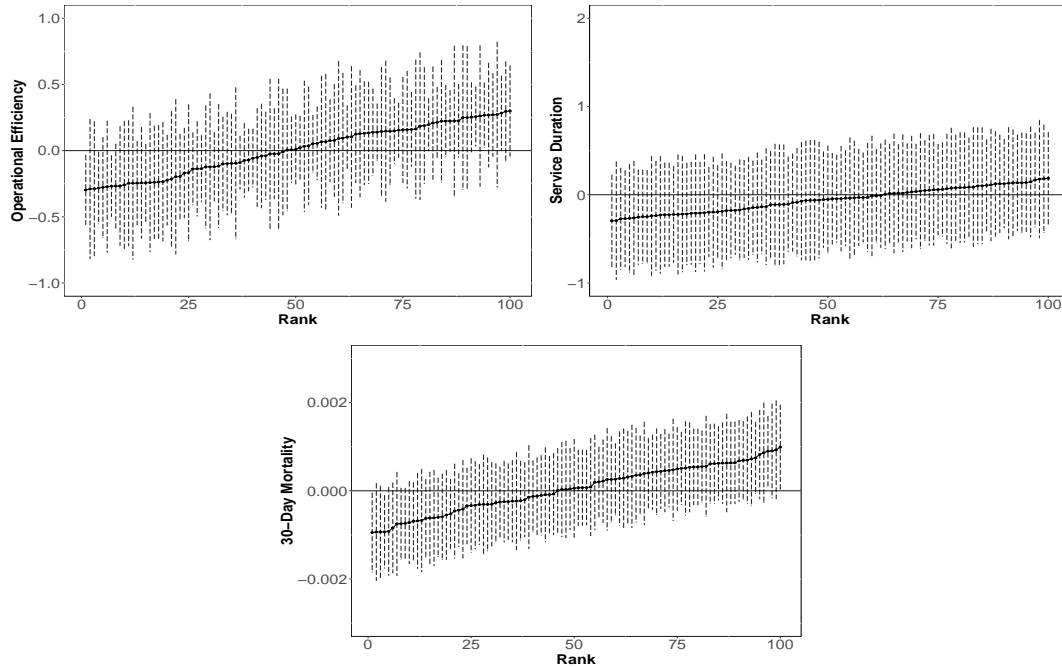
Year	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	2015
2006 closure	Pre-closure year	Closure year	Post-closure years			Observations removed					
2007 closure	Pre-closure years		Closure year	Post-closure years			Observations removed				
2008 closure	Pre-closure years			Closure year	Post-closure years			Observations removed			
2009 closure	Obs. removed	Pre-closure years			Closure year	Post-closure years			Observations removed		
2010 closure	Observations removed		Pre-closure years			Closure year	Post-closure years			Observations removed	
2011 closure	Observations removed			Pre-closure years			Closure year	Post-closure years			Obs. removed
2012 closure	Observations removed				Pre-closure years			Closure year	Post-closure years		
2013 closure	Observations removed					Pre-closure years			Closure year	Post-closure years	
2014 closure	Observations removed						Pre-closure years			Closure year	Post-closure year

Figure EC3: Trends in Outcomes for Treatment and Control Groups



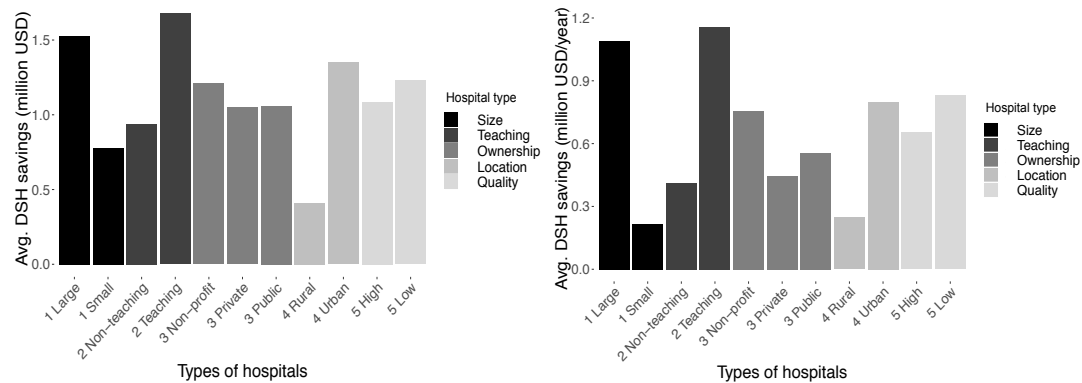
Note. For the treatment group, pre-closure outcomes are calculated as the average among all hospitals in the treatment group and pre-closure observation years. We do not present the post-closure trend for the treatment group, because the closure years vary for different hospitals. The hospitals with the latest closure year is 2012, which was to ensure at least three years of post-closure observation. Thus, there are no outcomes presented for the treatment group post 2012. In general, we see parallel trends between the treatment group and the control group before the cohort-specific closure year.

Figure EC4: Placebo Tests of Treatment Effect on (a) Operational Efficiency (Top Left) (b) Service Duration (Top Right) and (c) 30-Day Mortality (Bottom)



Note. Actual treatments were randomly assigned to hospitals 100 times. Each point and whiskers represents an OLS regression with fixed effects and full controls, using our original data.

Figure EC5: Estimated Intervention Costs



Note. For selective bailout policy, we calculated the bailout cost as the DSH savings that incur as a result of the closures by calculating the average DSH payment made to each hospital type in 2015. For selective elimination of speed-up policy, we estimated the cost of such interventions based on the cost of increased length of stay from previous studies (Bartel et al. 2014, Taheri et al. 2000). Specifically, the annual per hospital cost of eliminating the speed-up behavior was calculated as the product of the inflation-adjusted per-patient cost of the stay for an additional day (\$611 per patient in 2015, see Taheri et al. (2000)), the magnitude of the speed-up behavior, and the annual patient volume per hospital.