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IMPACT OF RURAL AND URBAN HOSPITAL CLOSURES ON INPATIENT MORTALITY

Kritee Gujral
Anirban Basu

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ABSTRACT

This paper uses a difference-in-difference approach to examine the impact of California's hospital closures occurring from 1995-2011 on adjusted inpatient mortality for time-sensitive conditions: sepsis, stroke, asthma/chronic obstructive pulmonary disease (COPD) and acute myocardial infarction (AMI). Outcomes of admissions in hospital service areas (HSAs) with and without closure(s) are compared before and after the closure year. The paper focuses on: 1) the *differential* impacts of rural and urban closures, 2) the aggregate patient-level impact across several post-closure mechanisms, and 3) the effect on Medicare as well as non-Medicare patients. Results suggest that when treatment groups are not differentiated by hospital rurality, closures appear to have no measurable impact, i.e. there is no *general* impact of closures. However, estimating *differential* impacts shows that rural closures increase inpatient mortality by 0.78% points (an increase of 8.7%), whereas urban closures have no measurable impact. Subgroup analyses indicate the existence of a *general* impact for stroke and AMI patients (4.4% increase in inpatient mortality) and relatively worse impacts of rural closures for Medicaid patients and racial minorities (11.3% and 12.6%, respectively).

Kritee Gujral
The CHOICE Institute
University of Washington
1959 NE Pacific St
Box 357630
Seattle, WA 98195
kritee@uw.edu

Anirban Basu
The CHOICE Institute
Departments of Pharmacy,
Health Services, and Economics
University of Washington, Seattle
1959 NE Pacific St., Box - 357660
Seattle, WA 98195
and NBER
basua@uw.edu

1 Introduction

About 15% of U.S. hospitals have closed since 1990 and rates of hospital closure have increased over the last decade, with rural closures being of particularly high concern (Carroll, 2019). Common reasons cited for hospital (and emergency department (ED)) closures are weakened hospital finances resulting from low reimbursement rates and uncompensated care (Lindrooth et al., 2018; Hodgson et al., 2015). These problems become exacerbated in rural areas. As has been the case recently, economic downturns can lead to outmigration of rural residents to urban areas. Shrinking populations imply lower patient volumes at hospitals, which can prevent hospitals from achieving economies of scale. Moreover, populations left behind may be older, sicker, and uninsured or more reliant on Medicaid and Medicare, resulting in less favorable payer-mixes at hospitals, making them susceptible to closures.

Nonetheless, such market-driven hospital closures can have enormous negative implications for patient welfare. Hospital closures can severely fracture patient access to care (especially emergency care), leading to treatment delay and adverse health outcomes (Carroll, 2019; Wishner et al., 2016). Rural closures, in particular, have been known to increase travel times for patients (Troske and Davis, 2019; Hsia and Shen, 2011) and to cause outmigration of health care professionals (Manlove and Whitacre, 2017; Wishner et al., 2016; Germack et al., 2019), severely dismembering patient access to care and exacerbating social disparities in health outcomes (Song and Saghafian). On the other hand, given that closing hospitals may have had low volumes prior to closure, it is theoretically possible that closures ultimately do not result in adverse impact for patients if patients are rerouted and reallocated appropriately. It is also theoretically possible that closures result in more efficient reallocation of patients to neighboring hospitals, potentially improving patient outcomes and welfare. The net patient-level impact of closures, whether rural or urban, across several such post-closure mechanisms is not well-known, and remains an empirical question that this paper aims to answer.

Although previous studies have examined the reasons for (Lindrooth et al., 2018; Wishner et al., 2016; Friedman et al., 2016) and the financial and economic impact of hospital closures (Manlove and Whitacre, 2017; Hodgson et al., 2015), literature studying the effect of hospital closures on patient outcomes is relatively sparse and lacks sufficient consensus for guiding informed policy-making. Large-scale published studies have found no impact of hospital (or ED) closures on patient mortality (Joynt et al., 2015; Hsia et al., 2012; Rosenbach and Dayhoff, 1995), with some evidence of improved outcomes, i.e. a reduction in mortality, for heart attack patients (Joynt et al., 2015). Only one published county-level study (Buchmueller et al., 2006) thus far has shown adverse patient-level impact, i.e., an increase in deaths due to urban hospital closures. Additional studies examining the impact of hospital closures on patient outcomes are necessary given the accelerated rate of closures, especially rural, in recent years (GAO, 2018; Kaufman et al., 2016b). Such crucial efforts are underway; recent working papers (Song and Saghafian; Carroll, 2019) do find that hospital closures can increase mortality. Nonetheless, most of this emerging and existing work has focused on Medicare patients (Song and Saghafian; Carroll, 2019; Joynt et al., 2015; Rosenbach and Dayhoff, 1995). As post-closure health access barriers may be greater for the uninsured and Medicaid populations, there is a need for studies that also include these populations.[1] Furthermore, despite studying similar diagnostic conditions, results differ across the set of emerging and existing

[1] Liu et al. (2014) and Hsia et al. (2012) do study patient outcomes for these populations, but in the context of ED closures, and the impact of ED closures may be smaller and/or different compared to that of hospital closures. For instance, hospitals may have multiple EDs such that the effect of an ED closure could be smaller than that of a hospital closure. Further, the interruption of services due to the loss of a full hospital, which include potentially long-term losses of medical staff and other community resources, may be larger than that of an ED closure.

studies, of Medicare population studies, of California-specific studies (Liu et al., 2014; Hsia et al., 2012; Buchmueller et al., 2006), and of ED closure studies (Liu et al., 2014; Hsia et al., 2012). This warrants further discussion of the nuances, such as study design differences or population differences, that lead to differing results and implications. Additional research will provide more evidence on and help elucidate these nuances, thereby improving policy recommendations concerning closures.

This paper studies the impact of rural and urban hospital closures in California, 1995-2011, on inpatient mortality for time-sensitive conditions: sepsis, stroke, asthma/COPD, or acute myocardial infarction (AMI). Post-closure mechanisms that raise concerns (such as longer travel time or overcrowding at surrounding hospitals) are likely to result in larger adverse impacts for patients with time-sensitive conditions. These conditions also offer comparability with prior work (Carroll, 2019; Liu et al., 2014; Joynt et al., 2015; Hsia et al., 2012; Buchmueller et al., 2006). This paper's novel contribution is its focus on three nuances for analyzing the impact of closures: 1) the *differential* impact of rural and urban hospital closures, 2) controlling for unobserved time-invariant hospital characteristics, and 3) including Medicare as well as non-Medicare populations.

Previous patient-impact studies either examine rural closures while ignoring concurrent urban closures, which makes it difficult to disentangle the isolated effect of rural closures[2] or study the general effect by treating all hospital closures alike, without distinguishing between rural and urban hospitals. However, there are baseline differences between rural and urban health care markets such as rural patients experiencing larger transportation times, larger health care access barriers, bypassing nearby hospitals, and using hospitals for primary care needs, compared to urban patients (Lindrooth et al., 2018; Kaufman et al., 2016a; Premkumar et al., 2016; Wishner et al., 2016; Hsia and Shen, 2011). Additionally, there are post-closure differences; rural patients experience a 76% increase in ambulance transportation time post-closure in their zipcode, whereas urban patients do not experience any change in transportation time post-closure in their zipcode.[3] While urban patients have been shown to shift their usual source-of-care from hospitals to physicians' offices, which are viewed as more appropriate sources for primary care (Buchmueller et al., 2006), no such favorable substitution is observed for rural patients (Rosenbach and Dayhoff, 1995). There may also be larger declines in physician and health care staff availability in rural areas post-closure (Rosenbach and Dayhoff, 1995) compared to urban areas, exacerbating rural patients' baseline pattern of seeking primary care at hospitals. Given such baseline differences and post-closure differences, the overall effect of a rural hospital closure, which includes transportation delays and related spillover effects, is hypothesized to be larger than that of an urban closure. Theoretically possible post-closure improvements in patient outcomes may also differ depending on whether the closing hospital was rural or urban. Hence, this paper allows for differential impacts of rural and urban closures.

Finally, even with the usual generalizability limitations that accompany a state-specific study, California is an important state to study because it has historically been and continues to remain a state with high number of hospital closures (Nicholas C Petris Center, 2001; OIG, 2000; Troske and Davis, 2019). With early closures in the U.S. being concentrated in California and high data availability in the state, this study can offer insights, bifurcated by rural and urban closures, that move the needle forward for national research and policy efforts concerning closures, especially

[2]Neglecting to account for concurrent urban closures can confound estimates of rural closures (or vice versa). For instance, if urban closures decrease mortality, but are not controlled for when studying the impact of rural closures, the effect of rural closures may be overestimated. If urban closures increase mortality but are not controlled for, the impact of rural closures would be underestimated. This paper limits confounding of this sort by appropriately accounting for both types of closures.

[3]Miller et al. (2020) also find that rural hospital closures increased mean emergency medical services transport and total activation times in the year after a closure.

given that relatively more rural states that may be more vulnerable offer lower data access. To our best knowledge, this is the first paper drawing explicit focus to rural closures in California.

The paper is organized as follows: Section 2 outlines the methodology and data sources; Section 3 presents descriptive results, primary results estimating the general and differential impacts of closures, and subgroup analyses and robustness checks. Section 4 presents the concluding discussion.

2 Methodology

Using a difference-in-difference (DID) approach, two primary specifications were used for examining the impact of hospital closures on inpatient mortality:

- General impact of hospital closures
- Differential impacts of rural and urban closures

Section 2.1 outlines the definition of "affected patients" used, Section 2.2 details the estimation strategy, discussing the two above-mentioned specifications, and Section 2.3 describes the data.

2.1 Definition of "Affected Patients"

Several prior studies (Carroll, 2019; Hsia et al., 2012; Buchmueller et al., 2006) define "affected patients" in relation to the change in geographic distance to the nearest hospital that was experienced post-closure. While distance is an important factor determining patient choice, especially in rural areas, distance sensitivity can vary for different types of procedures (Premkumar et al., 2016) and other factors such as hospital size, technologies available, hospital quality may additionally influence hospital choice (Premkumar et al., 2016; Hodgson et al., 2015; Escarce and Kapur, 2009). Moreover, geodetic distances, driving distances, and driving times may sometimes suffer from measurement inaccuracies (Hsia et al., 2012). Given that prior California-specific studies (two of which use such metrics) (Hsia et al., 2012; Liu et al., 2014; Buchmueller et al., 2006) find differing implications for mortality (albeit due to other differences in study designs), this paper takes an approach for defining "affected patients."

Similar to Joynt et al. (2015) and Rosenbach and Dayhoff (1995), this paper uses Hospital Service Areas (HSAs), which represent local health care markets representing patients' past travel patterns, to define "affected patients." Patients are considered affected if their residential zip codes were part of HSA(s) that experienced a hospital closure. Appendix A.1 presents maps that help illustrate the concept of HSAs visually.

Advantages of such HSA-based definition are:

- Inclusion of the transportation delay mechanism, which may occur due to increased distance, increased drive time, or increased ambulance time.
- Inclusion of multiple other mechanisms:
  - Immediate as well as delayed outcomes due to delayed or foregone care post-closures.
  - Effects on patients seeking care both within and outside their residential HSA.[4]

[4]This HSA-based definition of affected *patients* vs. HSA-based definition of affected *hospitals* overcomes a limitation noted in Liu et al. (2014): if affected patients switch HSAs post-closures, the effect on those patients would be missed if studying affected *hospitals*.

- Ripple effects, such as overcrowding and overburdening of hospitals, on surrounding communities within the HSA (similar concerns as those raised in Song and Saghafian; Hodgson et al. (2015); Liu et al. (2014)).[5]
- Potentially reduced quality at surrounding hospitals due to lower hospital market competition when prices are market-determined (Frakt, 2019).
- Increased efficiency at surrounding hospitals (Gaynor et al., 2015).

- Reduction in confounding of estimates due to inadequate control of simultaneous post-closure mechanisms that may occur when studying one specific post-closure mechanism without controlling for the others.

Naturally then, the primary disadvantage of such HSA-based definition of affected patients is its inability to distinguish between the various mechanisms of impact, limiting immediate policy implications from emerging. Nonetheless, it is important to estimate the *overall* impact of hospital closures, in case the adverse effects of closures are sparsely distributed across several mechanisms and are therefore being missed when studying one mechanism at a time when using a conservative measure of adverse effect such as mortality. Given that scholars are currently split on whether or not hospital closures have any adverse impact on patients, the paper furthers policy efforts by helping to reduce this divide.

2.2 Estimation Strategy

A "staggered" difference-in-difference (DID) approach, or DID with multiple time periods (Stevenson and Wolfers, 2006), is used for estimating the impact of closures. This study design is useful when the same policy is implemented in different regions non-concurrently, or staggeringly. It measures the effect of a policy, hospital closure in this case, which is constant across time, reasons of closure, and geographical units. We compare patient mortality in hospital service areas (HSAs) with and without closure(s), before and after the closure year. The estimate of interest is the difference between the pre- to post-treatment period change in outcomes for the treatment group and the pre-to post-treatment period change in outcomes for the control group. This methodology allows for the treatment and control groups to have different *levels* of mortality, but assumes that the treatment and control groups would have followed a similar mortality *trend* or *trajectory* in the absence of treatment. As the control group is never affected by closures, it is randomly assigned to treatment years for the purposes of the analysis.[6]

Two types of effects were estimated: 1) The general impact of closures and 2) the differential impacts of rural and urban closures.

2.2.1 General Impact of Closures

A general impact of hospital closures, in which no distinction is made between patients who experience a rural closure vs. those who experience an urban closure, is estimated first. We estimate

[5] Note that overcrowding and overburdening may lead to negative outcomes at hospitals *outside* the affected HSA. Such spillover effects from and on patients that travel outside their residential HSA to seek care post-closure would be included in the treatment effect. However, spillover effects that negatively affect patients of unaffected HSAs would be part of the control group effect. Thus, any adverse treatment effect due to spillovers would be conservatively estimated.

[6] We also run analyses without such random assignment of control groups to treatment year. This implies there is no $Post_t$ variable, as shown in Eq. 1. The results are very similar.

the following probability:

$$P(Y = 1|g, t) = F(\alpha_0 + \alpha_1(Treat_g) + \alpha_2(Post_t) + \alpha_3(Treat_g * Post_t) + \alpha_4 X_{igt} + H_i + T_t) \quad (1)$$

where F is a logit function and $0 \leq F \leq 1$. Y_{igt} is a binary variable indicating whether an admission resulted in death at the hospital. $Treat_g$ indicates whether or not the admitted patient's HSA was ever affected in the 1995-2011 period. $Post_t$ indicates whether an admission occurred prior to or after the closure. $Treat_g * Post_t$ indicates whether the admission is from an affected HSA *and* occurs in that HSA's post-closure period. X_{igt} are admission-level and hospital-level covariates that can impact inpatient mortality. H_i are hospital fixed effects and T_t are year fixed effects. α_3 is the parameter of interest.

2.2.2 Differential Impacts of Rural and Urban Closures

As mentioned earlier, this paper allows for potentially different effects of rural vs. urban hospital closures, which also provides adequate controlling of one type of closure while examining the impact of the other. A staggered DID approach with *two* treatment groups is used: 1) admissions affected due to a rural hospital closure, and 2) admissions affected due to an urban hospital closure.

We estimate the following:

$$\begin{aligned} P(Y = 1|g, t) = &F(\gamma_0 + \gamma_1(RuralTreat_g) + \gamma_2(RuralPost_t) + \gamma_3(RuralTreat_g * RuralPost_t) \\ &+\gamma_4(UrbanTreat_g) + \gamma_5(UrbanPost_t) + \gamma_6(UrbanTreat_g * UrbanPost_t) + \\ &\gamma_7 X_{igt} + H_i + T_t) \end{aligned} \quad (2)$$

Equation 2 above is similar to Equation 1, except in its treatment variables. $RuralTreat_g$ indicates whether or not the admitted patient's HSA was ever affected by a rural hospital closure in the 1995-2011 period. $RuralPost_t$ indicates whether the admission occurred prior to or after a rural closure. $RuralTreat_g * RuralPost_t$ indicates whether the admission is from such an HSA *and* occurs in that HSA's post-rural closure period. $UrbanTreat_g$ indicates whether or not the admitted patient's HSA was ever affected by an urban hospital closure in the 1995-2011 period. $UrbanPost_t$ indicates whether the admission occurred prior to or after an urban closure. $UrbanTreat_g * UrbanPost_t$ indicates whether the admission is from such an HSA *and* occurs in that HSA's post-urban closure period. γ_3 and γ_6 are the parameters of interest.

2.2.3 Exogeneity of Treatment

Concerns may arise that the impact of hospital closure is endogenous to hospital behavior or hospital quality (Carroll, 2019). That is, hospitals of lower quality or hospitals with worse behavior may be more likely to close. In such cases, HSAs affected by closure may exhibit worse outcomes due to worse hospitals in those areas, and not because of closure. To address such concerns, we exploit the variation in timings of and reasons for closures. This makes it highly unlikely that results are due to changes in hospital quality or behavior (unrelated to closures) that coincide perfectly with time of closure of various hospitals. Additionally, we include hospital fixed effects, which control for any time-invariant hospital-specific effects, including time-invariant hospital quality.[7]

[7]Note also that there is no evidence that affected HSAs or closing hospitals provide lower quality of care (even though patients may *perceive* certain rural hospitals to be of poor quality based on anecdotal evidence (Wishner et al., 2016; Escarce and Kapur, 2009)); instead, the bulk of the literature points to differences in financial health

Next, there may be concerns that hospital closures occur in HSAs where individual patients are relatively older, sicker, poorer, more likely to be racial minorities, and more likely to be uninsured, and that the outcomes observed are a result of these characteristics rather than due to closing of hospitals. Thus, to control for disease severity and poverty status, we include in our estimation patient characteristics such as age, Elixhauser co-morbidities (Stagg, 2015), admission source variables, insurance status and an indicator for whether an admission is from a rural or urban zip code are used. Lastly, it is important to note that the DID methodology allows for *level* differences in covariates across treatment and control groups and is sensitive to *trend* differences in *unobserved* covariates linked to increased treatment exposure. To that effect, we *observe* the relevant variables and show that trends for these variables do not differ between treatment and control groups prior to closure (see Appendix A.2), making it unlikely that trends of *unobserved* variables exhibit differential trends. However, if hospital closures were occurring due to unobserved trend changes in covariates, the results presented in this paper would not be interpreted causally. We also conduct analyses using event study specifications, which confirm a lack of pre-trends that re-affirm the validity of the DID methodology (See Appendix A.3).

2.3 Data

A list of closed hospitals in the 1995-2011 period was developed using OSHPD discharge data, 1995-2011, and confirming this list of potential closures against multiple other sources.[8] Patient characteristics and the outcome variable indicating inpatient mortality were obtained from OSHPD patient discharge data, 1995-2011, and hospital characteristics were obtained from corresponding OSHPD Hospital Financials Data. The Dartmouth Atlas of Health Care crosswalks was used for matching zipcodes to HSAs and USDA ERS RUCA codes 2004 were used for assigning rurality status to patient residential zipcodes and to hospitals.[9] Zipcode population density was obtained from the 2000 decennial census maintained by the U.S. Census Bureau. Annual county-level unemployment data were obtained from California Economic Development Departments "Labor Force Data."

2.3.1 Variables

The outcome variable, inpatient mortality, is a binary variable created using the "disposition [at the end of the hospital stay]" variable in OSHPD discharge data, which includes in-hospital death as one of the values. Patient characteristics include age, gender, race, diagnostic condition co-morbidities, admission source variables, insurance status, and the rurality status of patients residence. Hospital characteristics include hospital ownership type, number of medical staff, number of hospital beds, bed availability ratio, proportion of Medi-Cal patients, and hospital fixed effects. Area and market characteristics include county-level unemployment rate, zipcode-level population density from the year 2000, and market concentration/competition (calculated for each patient's HSA for hospitals that never closed) associated with each patient admission.

3 Results

Sections 3.1 and 3.2 present descriptive results, which are followed by estimates of the general impact of hospital closure and the differential impacts of rural and urban closures in 3.3 and

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and efficiency of treated vs. non-treated facilities (a point also noted by Carroll (2019)).

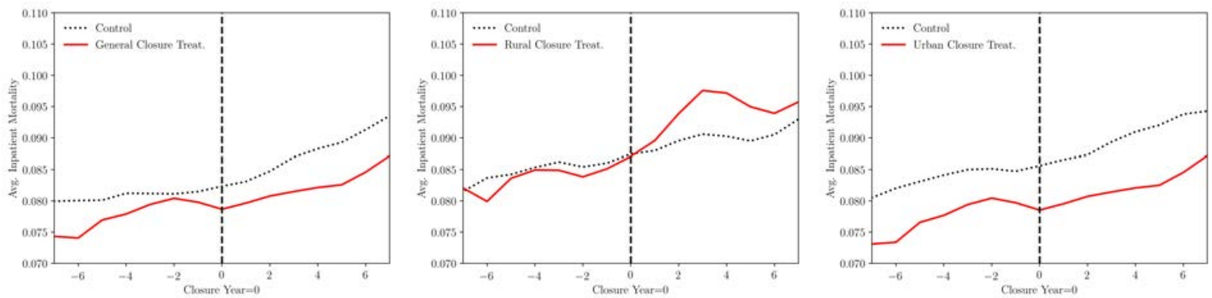
[8] Details regarding the development and verification of this list are provided in the Appendix A.4.

[9] Additional details regarding the development of the final analytic file are presented in Appendix A.5.

subgroup analyses and robustness checks in 3.4.

3.1 Common Trends

As mentioned earlier, the DID methodology assumes that treatment and control groups would have followed similar mortality trends in the absence of treatment. As shown in Figure 1, the treatment and control groups follow a roughly similar mortality trend prior to the year for all three types of treatment groups: general (i.e. all hospital closures without distinguishing whether rural or urban), rural closures, and urban closures. Furthermore, such visual evidence is useful for setting expectations for the potential impacts to be discovered in the estimation (Wing et al., 2018). As can be seen, while there appears to be no difference in mortality post-closure for the general and urban treatment (Figures 1a and 1c, respectively), rural closures appear to increase mortality (Figure 1b).



(a) General treatment (b) Rural closures treatment (c) Urban closures treatment

Figure 1: Average inpatient mortality (in proportions of admissions resulting in death) in treatment group vs. control group over time

3.2 Baseline Characteristics

In this section, we present baseline characteristics of control and treatment groups, which help demonstrate that control and treatment groups are well-balanced and further assure that control and treatment groups are unlikely to have differential trends for unobserved characteristics. Table 1 presents proportions (for dichotomous variables), means (for non-dichotomous variables) and the standardized differences[10] between treatment and control groups. Although there is no universally agreed upon criterion regarding what constitutes important imbalance between treatment and control groups, a standardized difference less than 0.1 is considered negligible (Austin, 2011). It can be seen that there are no meaningful differences between the proportions of patients presenting with AMI, stroke, and asthma/COPD in control vs. treatment groups for both rural closures treatment and urban closures treatment. Treatment areas are slightly less likely to have sepsis admissions. Differences between control and treatment groups are also negligible for most of the Elixhauser co-morbidities and for many of the remaining covariates.

We do note some expected differences across control and treatment groups. Rural closures treatment group is much more likely to include patients from rural zipcodes compared to the control and urban closures treatment group is much less likely to have patients from rural zipcodes compared to the control. Urban treatment group admissions are slightly more likely to be seen at hospitals that were investor hospitals and slightly less likely to be seen at district hospitals.

[10]Formulas used for calculations are provided in Austin (2011)

Rural closures treatment admissions are more likely to occur at hospitals with higher percentage of Medi-Cal days and with higher availability of beds, and more likely to be from areas with lower population density and higher unemployment, compared to control. Urban treatment group admissions exhibit roughly opposite patterns with respect to these latter variables, and additionally are more likely to occur at hospitals with a much larger number of beds. For the DID estimation to be valid, such differences between treatment and control groups must be stable over time and changes in treatment exposure should not be linked to changes in the distribution of covariates (Wing et al., 2018). Differences noted here are consistent with several previous studies examining hospital closures using different data sets from different time periods. As the distributions of the covariates appear consistent with expectations from prior work and as trends of key observed variables do not appear different (see Appendix A.2), there is sufficient confidence that differences in trends of *unobserved* variables are not driving the estimation results in this paper.

Table 1: Variable Proportions/Means & Standardized Differences, One Year Prior to Closure

	Ctrl	**Rural Trt.**	**Diff.**	**Urban Trt.**	**Diff.**
Time-sens. conditions					
AMI	0.238	0.274	0.081	0.255	0.038
Stroke	0.230	0.224	-0.010	0.243	0.030
Sepsis	0.227	0.169	-0.140*	0.154	-0.180*
Asthma/COPD	0.303	0.331	0.060	0.346	0.091
Elixhauser Co-morbs.					
Congestive Heart Failure	0.220	0.246	0.060	0.199	-0.050
Cardiac Arrhythmias	0.263	0.245	-0.040	0.234	-0.060
Valvular Disease	0.072	0.072	0.003	0.069	-0.000
Pulmonary Circulation Disorders	0.031	0.034	0.017	0.026	-0.030
Peripheral Vascular Disorders	0.067	0.060	-0.020	0.054	-0.050
Hypertension, Uncomplicated	0.428	0.418	-0.020	0.390	-0.070
Paralysis	0.093	0.104	0.035	0.110	0.055
Other Neurological Disorders	0.113	0.106	-0.020	0.106	-0.020
Chronic Pulmonary Disease	0.433	0.466	0.067	0.451	0.037
Diabetes, Uncomplicated	0.211	0.207	-0.000	0.186	-0.060
Diabetes, Complicated	0.062	0.056	-0.020	0.047	-0.060
Hypothyroidism	0.097	0.092	-0.010	0.071	-0.090
Renal Failure	0.104	0.077	-0.090	0.051	-0.190*
Liver Disease	0.032	0.028	-0.020	0.020	-0.070
Peptic Ulcer Disease	0.011	0.015	0.035	0.013	0.015
AIDS/HIV	0.001	0.000	-0.040	0.003	0.028
Lymphoma	0.008	0.006	-0.010	0.005	-0.020
Metastatic Cancer	0.019	0.017	-0.010	0.016	-0.020
Solid Tumor Without Metastasis	0.033	0.030	-0.010	0.028	-0.020
Rheumatoid Arthritis/Collagen Vascular	0.026	0.025	-0.000	0.023	-0.020
Coagulopathy	0.040	0.029	-0.050	0.028	-0.060
Obesity	0.079	0.066	-0.050	0.056	-0.090
Weight Loss	0.046	0.028	-0.090	0.027	-0.100
Fluid and Electrolytes Disorders	0.255	0.251	-0.010	0.226	-0.060
Blood Loss Anemia	0.009	0.009	-0.000	0.008	-0.000
Deficiency Anemia	0.025	0.019	-0.030	0.018	-0.040

Table 1 Continued: Proportions/Means & Standardized Differences

Alcohol Abuse	0.037	0.033	-0.020	0.031	-0.030
Drug Abuse	0.023	0.020	-0.010	0.019	-0.020
Psychoses	0.015	0.012	-0.020	0.013	-0.020
Depression	0.077	0.064	-0.050	0.049	-0.110*
Hypertension, Complicated	0.111	0.087	-0.080	0.073	-0.130*
Other Variables					
In-hospital death	0.087	0.093	0.021	0.082	-0.010
Age Category					
Under 1 year	0.008	0.009	0.007	0.014	0.049
1-17 years	0.033	0.032	-0.000	0.047	0.071
18-34 years	0.026	0.026	0.003	0.033	0.041
35-64 years	0.301	0.331	0.063	0.296	-0.010
65 years and over	0.629	0.600	-0.060	0.608	-0.040
Sex					
Male	0.477	0.472	-0.000	0.467	-0.010
Female	0.522	0.527	0.009	0.532	0.019
Race					
White	0.751	0.842	0.227*	0.716	-0.070
Black	0.050	0.031	-0.090	0.091	0.159*
Native American/Eskimo/Aleut	0.001	0.001	0.004	0.000	-0.010
Asian/Pacific Islander	0.054	0.014	-0.220*	0.049	-0.020
Other	0.048	0.039	-0.040	0.037	-0.050
Missing/Unknown	0.094	0.070	-0.080	0.104	0.032
Payer Category					
Medicare	0.606	0.608	0.003	0.554	-0.100
Medi-Cal	0.127	0.132	0.016	0.139	0.035
Private Payer	0.210	0.211	0.002	0.254	0.104*
Indigent	0.014	0.012	-0.020	0.018	0.029
Self-Pay	0.026	0.018	-0.050	0.021	-0.030
Missing/Unknown	0.013	0.016	0.019	0.011	-0.020
Admission source of patient					
Home	0.860	0.902	0.129*	0.880	0.058
Residential Care Facility	0.016	0.010	-0.040	0.011	-0.040
Ambulatory Surgery	0.004	0.003	-0.020	0.004	-0.000
Skilled Nursing/Intermediate Care	0.051	0.035	-0.070	0.039	-0.050
Acute/Inpatient Hospital Care	0.053	0.036	-0.080	0.054	0.006
Other (Inpatient) Hospital Care	0.005	0.003	-0.020	0.005	0.010
Prison/Jail	0.001	0.004	0.055	0.001	-0.00
Other	0.005	0.003	-0.030	0.001	-0.060
Admission source site					
This Hospital	0.012	0.010	-0.020	0.011	-0.000
Another Hospital	0.066	0.040	-0.110	0.063	-0.010
Not a Hospital	0.921	0.949	0.117*	0.925	0.015
Admission route					
This Hospital's ER	0.795	0.790	-0.010	0.760	-0.080
Not an ER or Another Hospital's ER	0.203	0.209	0.012	0.238	0.084

Table 1 Continued: Proportions/Means & Standardized Differences

Hospital Control Type					
City/County	0.031	0.048	0.087	0.055	0.117*
District	0.090	0.087	-0.010	0.048	-0.160*
Investor	0.125	0.137	0.035	0.204	0.214*
Non-Profit	0.750	0.726	-0.050	0.691	-0.130*
State	0.001	0.001	-0.010	0.000	-0.040
% Medi-Cal days	0.203	0.227	0.145*	0.188	-0.090
Medical Staff/1000	0.416	0.413	-0.000	0.420	0.011
Available Beds/ Licensed Beds	0.940	0.984	0.551*	0.912	-0.240
County-level Unemployment Rate	7.499	11.87	1.492*	7.360	-0.040
Zip-level Pop. Dens./Sq. Mi./10000	0.416	0.227	-0.530*	0.668	0.427*
HHI at open hosps. in an HSA	6.887	3.975	-1.000*	3.766	-1.020*
No. of Licensed Beds	302.3	292.1	-0.050	347.6	0.241*
N	168,787	16,840		70,090	

Asterisks indicate a standardized difference of magnitude greater than 0.1.
Baseline values for closures occurring in 1995 could not be observed in the data.

3.3 General and Differential Impacts

In this section, we present the main results estimating the impact of closures. As can be seen by the marginal effects presented in Table 2, when no distinction is made between HSAs affected due to rural closures vs. due to urban closures, there is no statistically significant impact of hospital closure on inpatient mortality, i.e. there is no discernible *general* impact of closures on inpatient mortality. In contrast, when examining the impacts of rural and urban closures differentially, it can be seen that rural hospital closures have a statistically significant impact; they increase inpatient mortality by 0.78% points (an increase of 8.7%) whereas urban closures have no measurable impact on inpatient mortality. These differential impacts are consistent with our hypotheses that rural closures are likely to have larger adverse patient-level impacts than urban closures. These results offer one possible explanation for a lack of published evidence on adverse patient-level impact of hospital closures. That is, combining rural and urban closures as one treatment group can mask harmful impact of certain types of closures.

Table 2: Results: General & Differential Impacts of Closures

	General Impact	**Differential Impacts**	
Variable	M.E. (Std. Error)	Variable	M.E. (Std. Error)
Treat*Post	0.0016	RuralTreat*RuralPost	0.0078**
	(0.0015)		(0.0025)
		UrbanTreat*UrbanPost	-0.0014
			(0.0014)
Treat	-0.0002	Rural Treat	-0.0087**
	(0.0009)		(0.0021)
		Urban Treat	-0.0006
			(0.0011)
N	3,049,009	N	3,049,009

Results are reported in proportions.
Standard errors are reported in parentheses.
Errors are clustered by patient HSA.
* $p<0.10$, * $p<0.05$
Average inpatient mortality rate for the control group is 8.9%. An increase of 0.0078 or .78% percentage points translates to an 8.7% increase in inpatient mortality.

3.4 Subgroup Analyses & Robustness Checks

We now present subgroup analyses that provide robustness checks on the results presented and shed additional insight into the potential mechanisms underlying the observed results. A results summary is presented in Table 3 and details are presented in Appendix A.6. Table 3 shows that when the analysis sample is restricted to patients from urban zipcodes, the adverse impact of rural closures persists, albeit slightly smaller at 7.6%, compared to the overall effect of 8.7%. This suggests that the effects observed are not driven from rural zipcodes alone, confirming that differential unobserved trends of rural zipcodes are not driving the results observed, and indicating that rural closures affect neighboring urban zipcodes. Next, concerns may arise that rural closure treatment areas follow differential trends than control areas, which include HSAs that have no rural hospital. To address this concern, we restrict the sample to patients from HSAs that had at least one rural hospital, and find that the effect of rural closures persists; rural closures increase mortality by 8.8% for this subgroup.

As can be seen in Table 3, rural closures increase inpatient mortality for Medicare patients by 7.3% and for Medicaid patients by 11.3%. This larger adverse effect for Medicaid patients compared to that for Medicare patients is consistent with the hypothesis that access issues are likely greater for the non-Medicare population, leading to worse outcomes for these populations. Rural closures increase mortality for White patients by 7.4% and for Non-White patients by 12.6%, despite the fact that rural closure treatment group has a higher proportion of White patients, compared to the control group. Again, this suggests that racial minorities, who may face larger health access barriers, may be disproportionately affected by hospital closures. These results emphasize concerns that hospital closures can exacerbate existing health care access issues for vulnerable populations and potentially worsen health outcome disparities.

Owing to smaller sample sizes when examining subgroups by condition, we group together conditions that are relatively most time-sensitive.[11] Combining AMI and stroke admissions, we

[11] Summarized results for each diagnostic condition are also presented in Table 3, with related details presented in

find both a general impact on mortality, an increase of 4.4%, and a differential impact of rural closures, an increase of 10%, and no measurable impact of urban closures. When analyzing sepsis, stroke, and AMI patients (and excluding the least time-sensitive condition of asthma/COPD), we find that rural closures increase mortality by 10% (larger than the overall increase of 8.7% for all conditions). When studying asthma/COPD, we find no impact of hospital closures, general or differential. This null result for asthma/COPD is consistent with prior studies of hospital (and ED) closures (Buchmueller et al., 2006; Liu et al., 2014) and suggests that relatively more time-sensitive conditions are likely to exhibit larger adverse patient-level effects (Buchmueller et al., 2006). We do find a somewhat surprising result for sepsis patients; while rural closures increase mortality by 10.6%, as is consistent with Mohr et al. (2017), urban closures *decrease* mortality, i.e. improve patient outcomes, by 4.4%. Additional research mapping the effect of closures across various diagnostic categories and research examining potentially positive, i.e. patient outcome-improving, mechanisms post-closures will be helpful for further understanding.

Next, we examine the impact of hospital closures on length of stay (LOS) as LOS can provide an additional signal of quality of care and can shed light on burdened utilization. We find that rural closures increase LOS by 5.2% or 6.6 hours. Although the link between LOS and quality of care can be debated,[12] increases in LOSs can contribute to significant overcrowding (Henneman et al., 2010). As shown in Table 3, when examining various subgroups, the adverse mortality impact of rural closures is always accompanied by an increase in LOS. As is the case with rural closures' impact on mortality, the impact of rural closures on LOS is larger for racial minorities, compared to Whites and larger for Medicaid populations, compared to Medicare populations. This pattern of alignment between rural closures' impact on LOS and on mortality suggests potentially decreased quality of care post-closures or increased complications underlying the observed increase in mortality, while also suggesting hospital overcrowding due to longer LOSs.

Table 9 of Appendix A.6.

[12] While shorter LOSs can indicate poor quality of care if hospitals respond to financial pressures by reducing service durations and discharge patients earlier than appropriate, longer LOSs can indicate poor quality of care or of ED crowding that can result in complications that lead to longer LOSs. Despite the recent trend of shortening LOSs, recent body of evidence favors the link between increased LOS and poor quality/mortality (Tran et al., 2018; McRae et al., 2017; Chaou et al., 2016; Pines et al., 2010; Thomas et al., 1997).

Table 3: Summary of Robustness Checks

	Treat*Post	**RTreat*RPost**	**UTreat*UPost**
Recall: Impact on inpatient mortality			
Sepsis, Stroke, AMI & Asthma/COPD	No Impact	+8.7%	No Impact
Restricting sample by diag. condition			
Stroke &AMI	+4.4%	+7.0%	No Impact
Sepsis, Stroke, &AMI	No Impact	+10.0%	No Impact
Sepsis	No Impact	+10.6%	−4.4%
Stroke	No Impact	+6.1%	No Impact
AMI	+6.1%	No Impact	+4.7%
Asthma/COPD	No Impact	No Impact	No Impact
Restricting sample by subpopulation			
Urban patients	No Impact	+7.6%	No Impact
Patients in HSAs with $\geq$ 1 rural hospital[13]	No Impact	+8.8%	No Impact
Medicare patients	No Impact	+7.3%	No Impact
Medicaid patients	No Impact	+11.3%	No Impact
White patients	No Impact	+7.4%	No Impact
Non-White patients	No Impact	+12.6%	No Impact
Recall: Impact on length of stay			
Sepsis, Stroke, AMI & Asthma/COPD	No Impact	+5.1% or 7 hrs	No Impact
Restricting sample by diag. condition			
Stroke &AMI	No Impact	+4.6% or 6 hrs	No Impact
Sepsis, Stroke, &AMI	No Impact	+6.7% or 10 hrs	No Impact
Sepsis	No Impact	+9.4% or 18 hrs	No Impact
Stroke	No Impact	+7.2% or 10 hrs	No Impact
AMI	+6.1%	No Impact	No Impact
Asthma/COPD	No Impact	No Impact	No Impact
Restricting sample by subpopulation			
Urban patients	No Impact	+5.3%	No Impact
Patients in HSAs with $\geq$ 1 rural hospital[14]	No Impact	+7.1%	No Impact
Medicare patients	No Impact	+4.2%	No Impact
Medicaid patients	No Impact	+9.5%	No Impact
White patients	No Impact	+3.9%	No Impact
Non-White patients	No Impact	+10.2%	No Impact

Reported numerical results are all statistically significant at the 5% level except for stroke only and AMI only subgroups, which are statistically significant at the 10% level. Additional details corresponding to the summarized results are available in Tables 9 and 10 of Appendix A.6.

[13] Due to a smaller sample, this analysis could only be conducted on model that excluded hospital fixed effects and did not include random assignment of treatment date to the control group.

[14] Due to a smaller sample, this analysis could only be conducted on model that excluded hospital fixed effects and did not include random assignment of treatment date to the control group.

4 Discussion

To summarize, the paper finds no measurable impact of hospital closures when all closures are considered alike, i.e. no distinction is made between a rural vs. urban hospital closure. When examining the *differential* impact of hospital closures, rural closures increase inpatient mortality by 8.7%, whereas urban closures have no measurable impact.

The lack of a measurable *general* impact of hospital closures is a finding consistent with Joynt et al. (2015); Hsia et al. (2012); Rosenbach and Dayhoff (1995), which also do not find any measurable impact of hospital (or ED) closures on mortality.As noted in Hsia et al. (2012), a majority of patients in California experience a less than 10 minutes increase in driving time and a median increase of 0.8 miles post-ED closures. If changes in distance and driving times post-*hospital* closures are similar, then detecting a *general* impact of closures on mortality through this particular mechanism would be difficult. As mortality may capture only relatively worse effects, smaller effects of closures may be better captured using measures such as readmissions or morbidity (Hsia et al., 2012). Additionally, our analysis does not study out-of-hospital mortality, implying that patients who may have died before generating a hospital admission would be missed (similar to (Liu et al., 2014; Hsia et al., 2012)). As such, the results of this paper are conservative estimates of the impact of hospital closures on patient outcomes. Furthermore, recall that the methodology used allows for theoretically possible mechanisms, such as improved efficiency and quality at surrounding hospitals, which can improve patient outcomes and could offset some of the adverse effects of closures, leading to an overall null result.

The estimated *differential* impacts of rural and urban closures are consistent with our hypothesis that rural closures likely have larger adverse impacts on patients than urban closures. That is, given that there are baseline differences and post-closure differences between rural and urban health care markets, it is reasonable that the overall effect of a rural hospital closure, which includes transportation delays and related spillover effects, differs from that of urban closures. Our results emphasize that combining rural and urban closures as one treatment group can mask the detrimental impact of rural closures.

Additional subgroup analyses presented in the paper suggest that time-sensitivity of the diagnostic conditions likely influences the presence of adverse patient outcomes. These analyses also show that the adverse impact of rural closures is larger for Medicaid populations and racial minorities, suggesting that existing health access issues experienced by these subgroups might worsen post-closures. This is consistent with expectations emphasized in prior hospital and ED closure literature that post-closures health outcomes may be disproportionately worse for lower-income and other vulnerable subgroups that are most affected by health access barriers (Song and Saghafian; Hsia and Shen, 2011; Buchmueller et al., 2006). We also find evidence of spillover effects; when the analysis sample is restricted to urban patients, we find that rural closures increase mortality by 7.6%, i.e. rural closures increase mortality also for patients that reside in *urban* zipcodes. Additionally, rural closures increase length of stay for emergency conditions by 5.2% or about 7 hours, and this can cause significant overcrowding (Henneman et al., 2010). Substantial spillover effects of hospital (and ED) closures are also noted in Song and Saghafian; Liu et al. (2014).

As mentioned earlier, although this paper's approach captures multiple mechanisms through which hospital closures can impact patient mortality and estimates *net* impacts of closures across these mechanisms, it is unable to distinguish between them. Further research is needed for understanding each individual mechanism, keeping in mind that adverse effects on mortality may be sparsely distributed across these channels. Using an intermediate measure of adverse patient outcomes, such as readmission or co-morbidity instead of mortality, may be particularly helpful for studies attempting to isolate the impact of a given mechanism. Additional research tracing the impact of closures across all diagnostic categories will also shed light on important post-closure mechanisms. Nevertheless, this paper contributes to the emerging concord in very recent studies (Carroll, 2019; Song and Saghafian; Liu et al., 2014) that hospital (and ED) closures do lead to increased patient mortality which may be distributed across various channels. This helps complement and extend earlier understandings of the effect of closures on patients. These results warrant serious consideration by hospital administrators and policymakers. While policy instruments that tackle root causes for closures appear less straightforward, there is a need to pay closer attention to rural closures, to ensure emergency transportation and health care accessibility post-closures, especially for vulnerable populations, and to manage increased and potentially differential health-care demands post-closures at surrounding hospitals, particularly if there are simultaneous reductions in the long-term supply of health care professionals.

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A APPENDICES

A.1 Appendix: Map: Zip-Level Rurality, Closures, and HSA Boundaries

The maps in Figure 2 below show the classificaiton of zipcodes into rural and urban (in colors grey and tan, respectively), zipcodes where rural and urban closures occurred (in colors orange and purple, respectively), and the boundaries for affected HSAs (in dark brown outline). The zoomed in version of the map shows that within an HSA, there are both rural and urban zipcodes surrounding a rural hospital closure. Thus, note that a rural hospital closure can affect both rural *and urban* zipcodes.

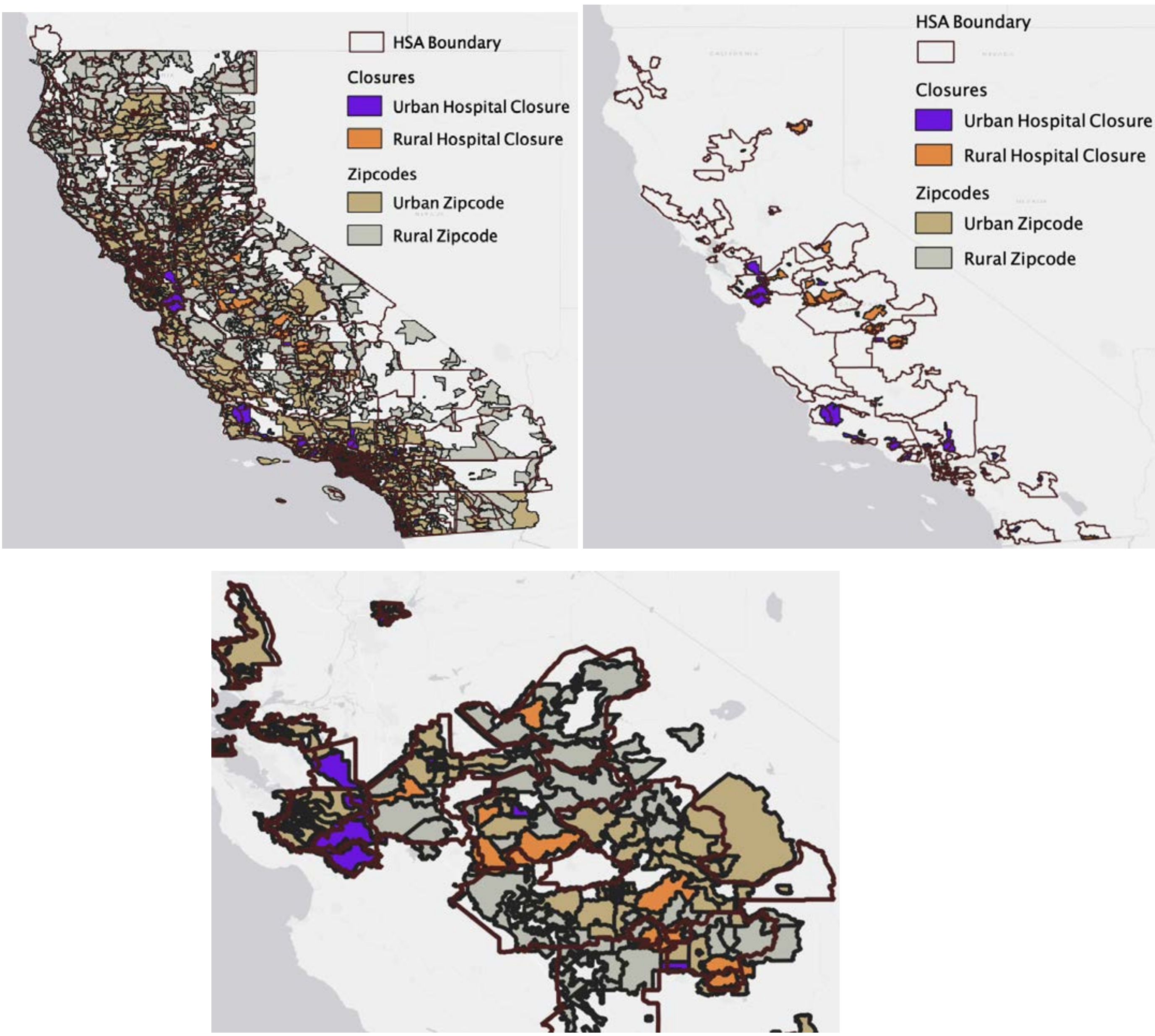
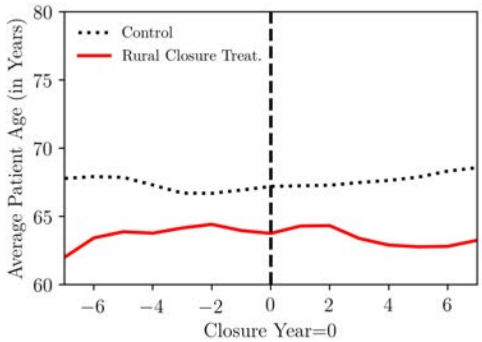


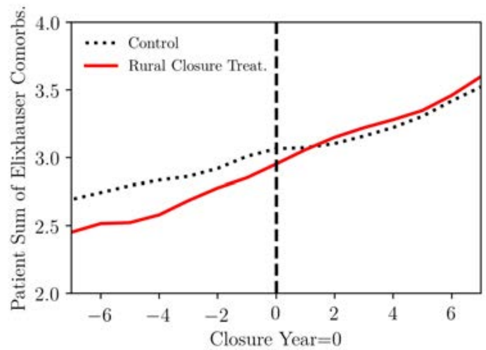
Figure 2: Zip-Level Rurality, Closures, and HSA Boundaries

A.2 Appendix: Control vs. Rural Treatment Trends for Relevant Observed Variables

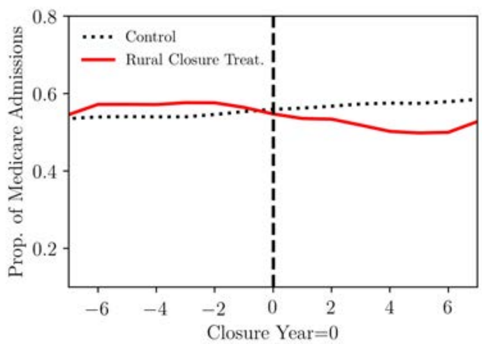
The DID methodology is sensitive to unobserved differential trends across treatment and control groups that may be linked to increased treatment exposure. Note, we *observe* relevant variables, i.e. variables that may be linked to increased treatment exposure, and we present trends comparing the rural treatment group and the control group for these variables. As can be seen in Figure 3, trends for rural treatment and control appear roughly similar prior to closure. This provides confidence that our estimation results are unlikely to be driven by differential trends of *unobserved* variables.



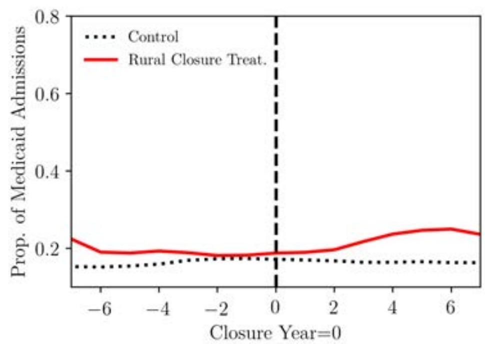
(a) Age of admissions



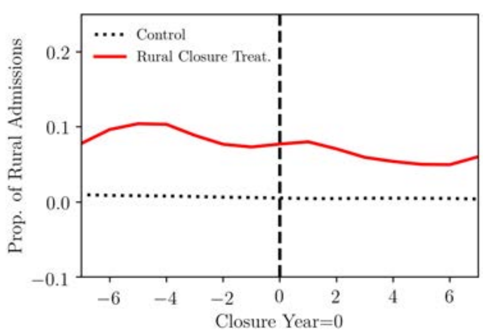
(b) Patient Sum of Elix. Co-morbs.



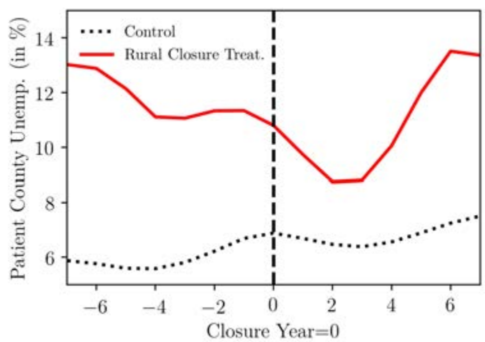
(c) Proportion of Medicare admissions



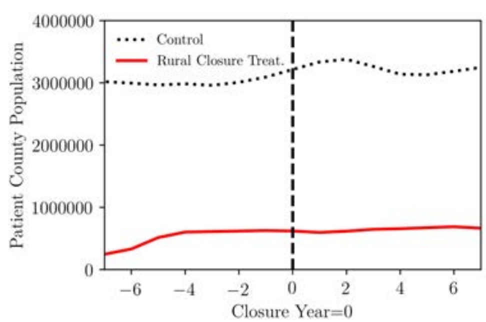
(d) Proportion of Medicaid admissions



(e) Proportion of rural admissions



(f) Patient county unemployment rate



(g) Patient county population

Figure 3: Trends for Relevant Observed Covariates, Rural Treatment vs. Control

A.3 Appendix: Event Study Specifications

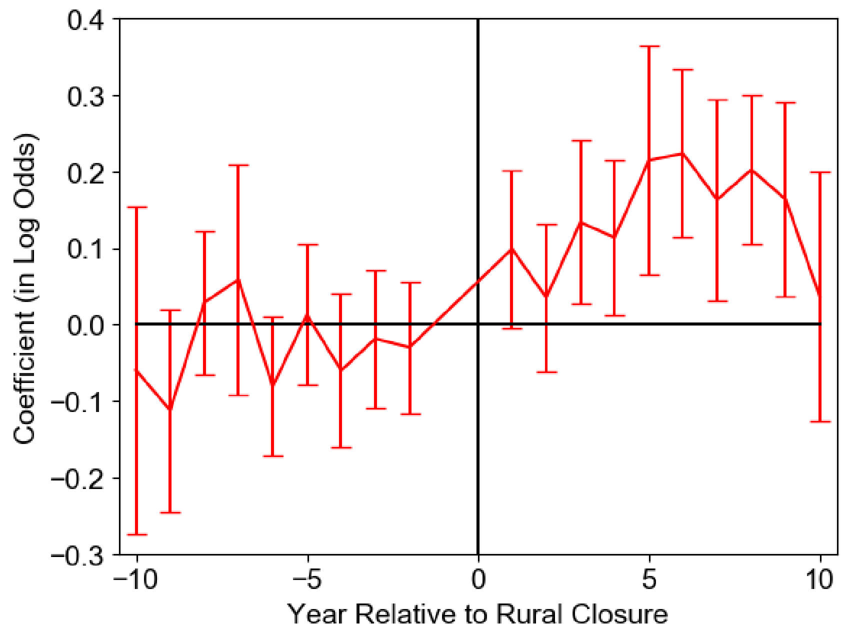
We additionally run event study specifications to visually test for pre-trends. Given that our empirical approach includes *two* treatment groups, we run two separate event study specifications; one for rural closures treatment and another for urban closures treatment, as shown in Equations 3 and 4:

$$\begin{aligned} P(Y=1|g,t) = &F(\sum_{m=-10}^{10} \beta_m (RuralTreat_g * \mathbb{1}(RuralPostYear_t = m)) + \beta_{21}(RuralTreat_g) \\ &+ \beta_{22}(UrbanTreat_g) + \beta_{23}(UrbanPost_t) + \beta_{24}(UrbanTreat_g * UrbanPost_t) \\ &+ \beta_{25}X_{igt} + H_i + T_t + \beta_0) \end{aligned} \tag{3}$$

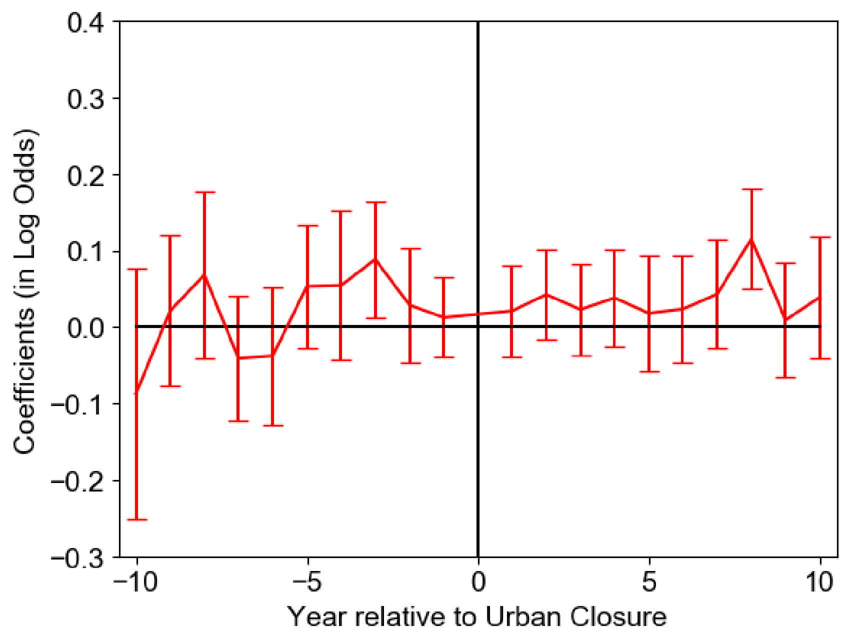
$$\begin{aligned} P(Y=1|g,t) = &F(\sum_{n=-10}^{10} \eta_n (UrbanTreat_g * \mathbb{1}(UrbanPostYear_t = n)) + \eta_1(UrbanTreat_g) \\ &+ \eta_{22}(RuralTreat_g) + \eta_{23}(RuralPost_t) + \eta_{24}(RuralTreat_g * RuralPost_t) \\ &+ \eta_{25}X_{igt} + H_i + T_t + \eta_0) \end{aligned} \tag{4}$$

where $\mathbb{1}(RuralPostYear = m)$ indicates whether or not an admission occurred in m years relative to a rural closure and similarly, $\mathbb{1}(UrbanPostYear = n)$ indicates whether or not an admission occurred in n years relative to an urban closure. β_m and η_n are the parameters of interest, estimating the impacts of rural closures in each m^{th} year relative to rural closure and estimating the impacts of urban closures in each n^{th} year relative to urban closure, respectively. Two separate event study specifications are run because if an admission occurred in an HSA that experienced a rural closure, but did not experience an urban closure, there is no appropriate value to use for the relative year to urban closure, or n. Thus, Equation 3 provides event study estimates only for patients that were affected by rural closures (while controlling for the effects of urban closures, i.e. $\beta_{22} - \beta_{24}$, for those patients), and Equation 4 provides event study estimates for patients that were affected by urban closures (while controlling for the effects of rural closures, i.e. $\eta_{22} - \eta_{24}$, for those patients).

Figure 4 below presents coefficients from the logit models, i.e. Equations 3, and 4 and shows a lack of pre-trends for both rural closures estimates and urban closures estimates. It also shows an increase in mortality due to rural closures and no measurable impact of urban closures, confirming the findings of the DID analysis presented in the main text of this paper. As there may have been an anticipatory effect in the year just prior to closure, this potential anticipatory effect, although statistically insignificant, has been removed from Figure 4a for the purposes of showing pre-trends.



(a) Rural Closure Event Study



(b) Urban Closure Event Study

Figure 4: Event Study Specification Graphs

A.4 Appendix: Closed Hospitals Data

Recent working papers have pointed to the lack of a centralized list of hospital closures and some data issues in identifying closures (Song and Saghafian; Carroll, 2019), and thus, in this section, we provide a detailed description of the the development and verification of the list of closed hospitals used for our analysis.

OSHPD patient discharge data from 1995 to 2011 is used for identifying hospital closures with a process comparable to that used in Song and Saghafian. A hospital is defined as "closed" if its general acute care admissions dropped to 10 or less in a given year (and remained so in subsequent years) from 1995 to 2011.[15] "Closure year" was defined as the year *prior* to the year in which hospital admissions dropped to 10 or less.[16]

To distinguish appropriately between closed hospitals and hospitals that may exhibit 10 or fewer admissions due to a name/hospital identifier change (owing to a merger or ownership change not resulting in closure), additional sources were consulted for confirming closures. Sources consulted include government reports (OIG, 1995, 1996, 1997, 1998, 1999), closed hospitals list from the LA Times (LA Times Data Desk, 2019), and academic publications and reports (Hodgson et al., 2015; Buchmueller et al., 2006; Nicholas C Petris Center, 2001).[17,18] After the confirmation process, a list of 139 potential closures identified using admissions data was reduced to a list of 99 hospital closures.[19] Seven of the 99 hospitals, which closed and re-opened in the same year and in the same zip code, were excluded from the list of closed hospitals. The resulting 92 hospitals are the confirmed closures used for this analysis.[20]

Next, using 2004 data from United States Department of Agriculture (USDA) Economic Research Service (ERS), hospitals were assigned a rural-urban commuting area (RUCA) code based on the hospital's zip code. RUCA codes were used for classifying hospitals as rural or urban (consistent with Holmes et al. (2017)). Rural classifications were further confirmed using other sources where hospital closures were reported and using additional internet search.[21] The 92 hospitals were thus classified as follows: 16 rural and 76 urban.

Some discrepancies across other sources of hospital closure lists are worth mentioning here. Hospitals appearing in one data source for a given time period were sometimes entirely excluded from other sources reporting closures in the same period. Additionally, there were some discrepancies regarding the exact year of closure, where the margin of error was typically a year, but in a few cases, this error margin ranged from 3 to 9 years. Because of these discrepancies, the present method for identifying closures has been explicitly outlined, and the resulting list is compared to the primary sources consulted.

[15] Additional sources mentioned in the next paragraph are used for identifying closures in 2011, as admissions data for 2012 were not available and thus a potential drop in admissions in 2012 cannot be identified.

[16] Alternate analyses using a modified definition of "closure year" were also conducted. That is, instead of using the year *prior*, "closure year" was defined as the very year in which a hospital's admissions dropped to 10 or less, and results of this analysis were very similar to the results presented in the paper.

[17] Closed hospitals data, not used in the published version of Hodgson et al. (2015), was also obtained from the authors.

[18] Numerous other sources, such as non-profit organization reports (CHCF, 2015; HASC, 2010) and news articles announcing individual hospital closures (or suggesting lack thereof), were also used, but are not all listed here. A complete list of these sources can be provided upon request.

[19] Note that this confirmation process may have resulted in excluding real but under-publicized closures, and thus remains a conservative list of closures. Nevertheless, as will be discussed later in this section, this paper ensures inclusion of the largest number of closures in California, and potential exclusion of a few such hospitals is unlikely to be a problem.

[20] A complete list of these closures is provided in supplemental materials.

[21] Only one hospital, which was classified as urban using RUCA codes, was updated to being "rural" based on other reporting.

Table 4 compares the number of closures in each year with those reported in four of the primary sources consulted. As can be seen, there are discrepancies in number of closures per year across the sources consulted. Nevertheless, the closure list by year is largely consistent with that of the LA times. It also picks up the largest or second largest number of hospital closures in most years. However, as closure year definitions across sources vary slightly, lists by year appear less compatible than comparisons of exact hospitals in each list. Thus, Table 5 compares the actual hospitals in the closure list to each of the four primary sources consulted, disregarding the year of closure for this exercise. 77%-91% of hospital closures reported by other sources are included in this analysis. The remaining 29 excluded hospitals (i.e. 9%-13% of unmatched hospitals implicit in Table 5) are listed in Table 6 alongside the reason for their exclusion.

Direct comparisons of California's closed hospitals, which are part of national studies, were not possible, as California-specific hospital counts or names were not reported in Song and Saghafian; Carroll (2019), and the study period does not overlap with that in Troske and Davis (2019). Nevertheless, a map provided in Joynt et al. (2015) appears to suggest a similar number of closures in California from 2003-2011.

Note that the methodology used in this paper for identifying closures through admissions data picks up most missed closures across the various lists, providing a more reconciled and complete list of closures in California from 1995 to 2011. An added advantage of this approach is the reliance on year-to-year comparisons of admissions data vs. on mere year-to-year comparisons of hospital listings (which do not include admissions information). The latter approach may be more susceptible to data reporting issues, as it is easier to wrongly include or exclude one hospital identifier in a given year than it is to wrongly include or exclude several admissions for a given hospital and year.

A few factors contributing to "closure year" discrepancies are also noted here. When a hospital retains its license, but has ceased to admit new general acute care patients, reported closure years may be later than years in which access to care was actually interrupted (also pointed out by Nicholas C Petris Center (2001)). When parts of the hospital remain open (e.g. ambulatory care or EDs), or when hospitals convert to entities providing outpatient or specialty care, official closure years reported may be later or different than years in which general acute care access was interrupted. Definitions of "closure year" should thus be explicitly noted in literature, and appropriate definitions must be adopted depending on the purpose of analysis. The "closure year" definition in this paper is well-aligned with the paper's intention to capture the very first instances of interruptions in general acute care. In cases of closure year discrepancies, closure year in this paper is always earlier than in other reports. Thus, if a hospital stopped providing general acute care services, but was still providing some or other aspects of care, the reduction or interruption in care due to closure, i.e."treatment", would be smaller than if all services were completely shut off. This implies that the paper's estimates are conservative (similar to Troske and Davis (2019)).

Table 4: No. of Hospital Closures by Year

Year	No. of Closures	Compare to other sources			
		Petris.org[22]	OIG reports[23]	LA times[24]	Hodgson[25]
1995	6	1	2	-	-
1996	9	3	7	-	-
1997	13	3	6	-	-
1998	10	7	10	14	1
1999	9	3	5	6	0
2000	8	6	7	12	0
2001	1	-	-	4	8
2002	3	-	-	2	6
2003	8	-	-	10	8
2004	9	-	-	6	7
2005	3	-	-	4	4
2006	1	-	-	1	2
2007	5	-	-	2	5
2008	1	-	-	1	6
2009	4	-	-	-	8
2010	2	-	-	-	3
2011	0	-	-	-	2
Total	92	23	37	62	60

The second column from the left indicates the number of closures in our analysis by year. The columns to the right of it indicate the number of closures identified by year in other sources. As neither of the other sources report closures for the entire period between 1995-2011, dashes are used to indicate missing values. Note that in some cases, a closed hospital identified by a source may occur in the following or preceding year for a different source.

Table 5: % Closed hospitals matched with other data sources

Other Sources	No. of Closures	No. Matched	% Matched
Petris Center report, 1995-2000	23	21	91%
OIG reports, 1995-2000	37	33	89%
Buchmueller[26] (LA county only), 1997-2003	15	13	87%
LA times, 1998-2008	62	53	85%
Hodgson, 1995-2011	60	46	77%

This table reports the number and percentage of closed hospitals identified by other sources that also occur in our closed hospitals list. Note that the year of closure may differ across the matched hospitals in some cases.

[22] Nicholas C Petris Center (2001)
[23] OIG (1995, 1996, 1997, 1998, 1999, 2000)
[24] LA Times Data Desk (2019)
[25] Hodgson et al. (2015)
[26] Buchmueller et al. (2006)

Table 6: Closures in other sources that were excluded

	Hospital Name	Year	Source	Reason for exclusion
1	Tustin Hospital	1996	OIG reports	Re-opened within a year under a different name, with same hospital ID.
2	Lakeside Hospital	1996	OIG reports	Name not found in OSHPD data.
3	Watsonville Comm. Hospital	1998	OIG reports	Re-opened within a year.
4	Suncrest Hospital, Orange Co.	1998	Buchmueller	Name not found in OSHPD data & not confirmed in internet search.
5	Los Medanos Comm. Healthcare Dist.	1998	LA times	Name not found in OSHPD data. Possibly closed before 1995.
6	SHC Specialty Hospital	1998	LA times	Name not found in OSHPD data. May exist in closures list under a different name.
7	Kaiser Foundation, Norwalk	1999	Buchmueller	Name not found in OSHPD data & not confirmed in internet search.
8	Saint Louise Mental Health Ctr	1999	LA times	Name not found in OSHPD data & not confirmed in internet search.
9	Kaiser Foundation, El Cajon	2000	Petris.org, LA times	Name not found in OSHPD data. Possibly closed before 1995.
10	Long Beach Community Med. Ctr.	2000	Petris.org, OIG reports	Re-opened within a year under a different name, with same hospital ID.
11	Hoopa Community Hospital	2000	LA times	Name not found in OSHPD data & not confirmed in internet search.
12	Kaiser Permanente, SF French Camp.	2000	LA times	Name not found in OSHPD data & not confirmed in internet search.
13	Anaheim Memorial Medical Ctr.	2001	LA times	Name not found in OSHPD data & not confirmed in internet search.
14	Ukiah Valley Medical Ctr.	2001	Hodgson	No general acute care admissions matched the hospital ID & year not confirmed in internet search.
15	Rehabilitation Institute, Santa Barbara	2001	Hodgson	No general acute care admissions matched the hospital ID & not confirmed in internet search.
16	Fremont Hospital Behavioral Health	2001	Hodgson	No general acute care admissions matched the hospital ID & not confirmed in internet search.
17	Mission Comm. Hospital, San Fernando	2002	Hodgson	Admissions in OSHPD data never dropped to 10 or below.
18	East Valley Pavilion	2002	Hodgson	No general acute care admissions matched the hospital ID & not confirmed in internet search.
19	Lassen Comm. Hospital	2003	LA times	Re-opened within a year in the same zip code.
20	Hazel Hawkins Convalescent Hospital	2003	Hodgson	No general acute care admissions matched the hospital ID & not confirmed in internet search.
21	Mercycare, Mercy General Hospital	2004	Hodgson	No general acute care admissions matched the hospital ID & not confirmed in internet search.
22	Monrovia Community Hospital	2005	LA times	Re-opened within 2 years under a different name, with same hospital ID.
23	Little Co of Mary Transitional Care Ctr	2005	Hodgson	No general acute care admissions matched the hospital ID & not confirmed in internet search.
24	Baywood Court (Eden Medical Ctr.)	2008	Hodgson	No general acute care admissions matched the hospital ID & not confirmed in internet search.
25	Kaiser Fnd Hosp, Carson	2008	Hodgson	No general acute care admissions matched the hospital ID & not confirmed in internet search.
26	Simi Valley Hospital, Heywood	2008	Hodgson	No general acute care admissions matched the hospital ID & not confirmed in internet search.
27	Agnews State Hospital	2009	Hodgson	No general acute care admissions matched the hospital ID & not confirmed in internet search.
28	Palm Drive Nursing & Rehab. Ctr.	2009	Hodgson	No general acute care admissions matched the hospital ID & year not confirmed in internet search.
29	Laurel Grove Hospital	2009	Hodgson	No general acute care admissions matched the hospital ID & year not confirmed in internet search.

This table lists the 29 hospitals considered closed by other sources, which did not meet the criteria of closure for our analysis. The reasons for excluding these hospitals from the closure list are presented in the right-most column.

Table 7 below presents variable proportions (for dichotomous variables) and means (for non-dichotomous variables) for rural and urban closed hospitals, one year prior to closure.

Table 7: Proportions/Means, Closed Hospitals, One Year Prior to Closure: Rural vs. Urban

Variable	**Rural Hosps.**	**Urban Hosps.**
Time-sensitive conditions		
AMI	0.119	0.215
Stroke	0.106	0.217
Sepsis	0.138	0.183
Asthma/COPD	0.635	0.383
Elixhauser Co-morbidities		
Congestive Heart Failure	0.230	0.187
Cardiac Arrhythmias	0.138	0.208
Valvular Disease	0.019	0.051
Pulmonary Circulation Disorders	0.019	0.022
Peripheral Vascular Disorders	0.023	0.041
Hypertension, Uncomplicated	0.309	0.355
Paralysis	0.044	0.086
Other Neurological Disorders	0.051	0.115
Chronic Pulmonary Disease	0.703	0.481
Diabetes, Uncomplicated	0.191	0.202
Diabetes, Complicated	0.026	0.049
Hypothyroidism	0.052	0.069
Renal Failure	0.047	0.060
Liver Disease	0.012	0.021
Peptic Ulcer Disease	0.014	0.011
AIDS/HIV	0.000	0.003
Lymphoma	0.004	0.003
Metastatic Cancer	0.006	0.013
Solid Tumor Without Metastasis	0.034	0.025
Rheumatoid Arthritis/Collagen Vascular	0.017	0.017
Coagulopathy	0.008	0.023
Obesity	0.045	0.055
Weight Loss	0.025	0.034
Fluid and Electrolytes Disorders	0.166	0.242
Blood Loss Anemia	0.010	0.007
Deficiency Anemia	0.022	0.022
Alcohol Abuse	0.022	0.037
Drug Abuse	0.023	0.024
Psychoses	0.006	0.023
Depression	0.067	0.049
Hypertension, Complicated	0.035	0.086
Other Variables		
In-hospital death	0.061	0.074
Age Category		
Under 1 year	0.001	0.004
1-17 years	0.052	0.020

Table 7 Continued: Closed Hospitals: Rural vs. Urban

18-34 years	0.051	0.032
35-64 years	0.302	0.314
65 years and over	0.591	0.628
Sex		
Male	0.379	0.433
Female	0.620	0.566
Race		
White	0.590	0.533
Black	0.008	0.076
Native American/Eskimo/Aleut	0.006	0.000
Asian/Pacific Islander	0.003	0.023
Other	0.065	0.030
Missing/Unknown	0.326	0.335
Payer Category		
Medicare	0.588	0.552
Medi-Cal	0.249	0.148
Private Payer	0.095	0.226
Indigent	0.013	0.024
Self-Pay	0.035	0.037
Missing/Unknown	0.016	0.009
Admission source of patient		
Home	0.899	0.841
Residential Care Facility	0.010	0.014
Ambulatory Surgery	0.002	0.001
Skilled Nursing/Intermediate Care	0.071	0.081
Acute/Inpatient Hospital Care	0.006	0.039
Other (Inpatient) Hospital Care	0.000	0.006
Prison/Jail	0.002	0.003
Other	0.006	0.001
Missing/Unknown	0.000	0.010
Admission source site		
This Hospital	0.050	0.031
Another Hospital	0.011	0.059
Not a Hospital	0.937	0.898
Missing/Unknown	0.000	0.010
Admission route		
This Hospital's ER	0.705	0.690
Not an ER or Another Hospital's ER	0.294	0.298
Missing/Unknown	0.000	0.010
Rural Admission	0.812	0.032
Hospital Control Type		
City/County	0.062	0.096
District	0.500	0.000
Investor	0.125	0.532
Non-Profit	0.250	0.370
State	0.062	0.000

Table 7 Continued: Closed Hospitals: Rural vs. Urban

% Medi-Cal days	12.45	6.655
Medical Staff	0.056	0.644
Available Beds/ Licensed Beds	3.311	3.037
County-level Unemployment Rate	0.532	0.246
Zip-level Pop. Dens./Sq. Mi./10000	0.040	0.252
HHI at open hosps. in an HSA	0.926	0.918
No. of Licensed Beds	94.06	158.2
N	16	60

The table presents proportions for dichotomous variables and means for non-dichotomous variables.
As discharge data was missing for some closed hospitals, information for those closed hospitals is not reported here.

A.5 Appendix: Data Used for Constructing the Analytic File

The final admission-level analytic file was assembled using publicly available OSHPD patient discharge data, 1995-2011, for admissions with 5-digit residential zip codes,[27] OSHPD Hospital Financials Data, Dartmouth Atlas of Health Care crosswalks, USDA ERS RUCA codes 2004, and closed hospitals list. Table 8 presents the ICD-9 diagnostic codes used for identifying the time-sensitive conditions analyzed: sepsis, stroke, asthma or COPD, and AMI.

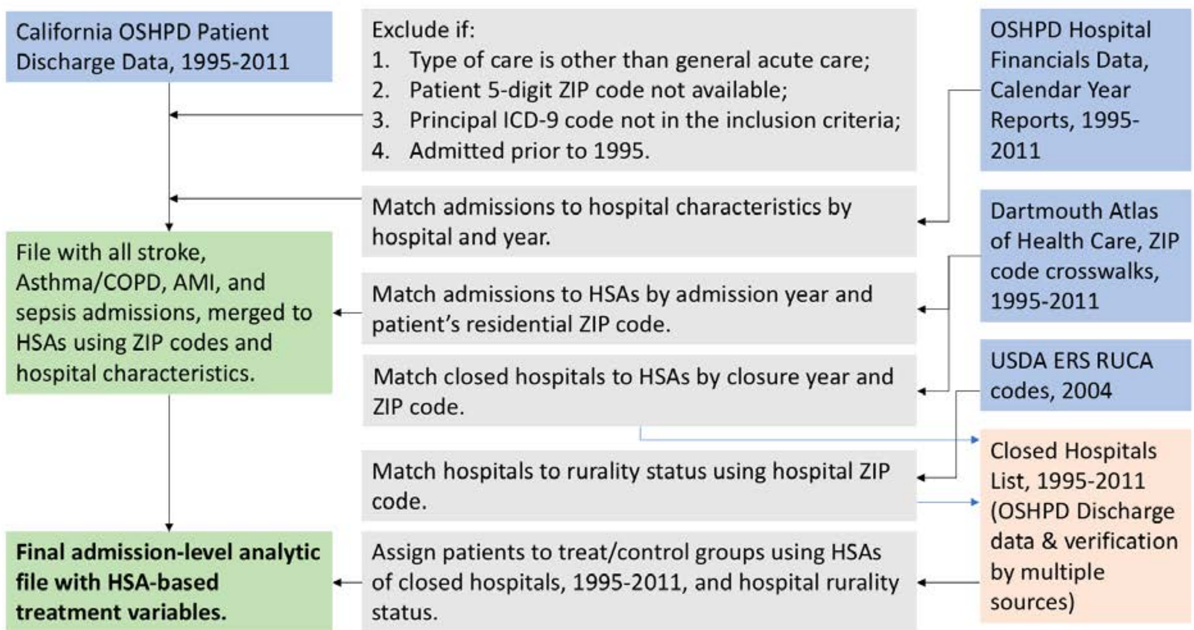


Figure 5: Data flow used to create primary analytic files

Table 8: ICD-9 codes used for patient conditions from the Clinical Classifications Software from Agency for Healthcare Quality and Research (AHRQ) Hsia et al. (2012)

Sepsis	003.1, 020.2, 022.3, 036.2, 038.0, 038.1, 038.10, 038.11, 038.19, 038.2, 038.3, 038.40, 038.41, 038.42, 038.43, 038.44, 038.49, 038.8,038.9, 054.5, 449, 790.7
Stroke	430, 431, 432.0, 432.1, 432.9, 433.01, 433.11, 433.21, 433.31, 433.81, 433.91, 434.0, 434.00, 434.01, 434.1, 434.10, 434.11, 434.9, 434.90, 434.91, 436
Asthma/COPD	493.00, 493.01, 493.02, 493.10, 493.11, 493.12, 493.20, 493.21, 493.22, 493.81, 493.82, 493.90, 493.91, 493.92, 490, 491.0,491.1, 491.2, 491.20, 491.21, 491.22, 491.8, 491.9, 492.0, 492.8, 494.0, 494, 494.1, 496
AMI	410.0, 410.00, 410.01, 410.02, 410.1, 410.10, 410.11, 410.12, 410.2, 410.20, 410.21, 410.22, 410.3, 410.30, 410.31, 410.32, 410.4, 410.40, 410.41, 410.42, 410.5, 410.50, 410.51, 410.52, 410.6, 410.60, 410.61, 410.62, 410.7, 410.70, 410.71, 410.72, 410.8, 410.80, 410.81, 410.82, 410.9,410.90, 410.91, 410.92

[27] Patient admissions whose zip codes were masked or unavailable were dropped (which includes homeless individuals and foreigners). Figure 5 presents a flow chart further describing the construction of the analytic file. Additionally, admissions at hospitals

A.6 Appendix: Details of Subgroup Analyses and Robustness Checks

This section provides additional details on subgroup analyses and robustness checks mentioned in Section 3.4. Table 9 provides marginal effects and standard errors for subgroup analyses conducted for each separate diagnostic condition. Table 10 presents marginal effects and standard errors for the remaining subgroup analyses.

Table 9: Subgroup Analyses - Differential Impacts of Rural and Urban Closures

Variable	**Sepsis**	**Stroke**	**As./CO.**	**AMI**	Variable	**Sepsis**	**Stroke**	**As./CO.**	**AMI**
Treat*Post	-0.0024	0.0030	0.0004	0.0049**	RTreat*RPost	0.0187**	0.0064*	0.0005	0.0033
	(0.0039)	(0.0008)	(0.0033)	(0.0020)		(0.0024)	(0.0039)	(0.0013)	(0.0025)
					UTreat*UPost	-0.0078**	0.0009	-0.0005	0.0038*
						(0.0034)	(0.0029)	(0.0008)	(0.0020)
Treat	-0.0010	-0.0012	-0.0007	0.0019	Rural Treat	-0.0210**	-0.0143**	-0.0033**	-0.0005
	(0.0018)	(0.0019)	(0.0005)	(0.0043)		(0.0050)	(0.0041)	(0.0012)	(0.0033)
					Urban Treat	0.0004*	-0.0013**	-0.0007	-0.0009
						(0.0023)	(0.0021)	(0.0005)	(0.0014)
N	717,138	690,292	946,152	693,337	N	717,138	690,292	946,152	693,337

Standard errors are reported in parentheses.
Errors were clustered by patient HSA.
* p<0.10, ** p<0.05
Average inpatient mortality rate for the control group with time-sensitive conditions studied are as follows: 17.6% for sepsis patients; 10.5% for stroke patients; 8.05% for AMI patients; & 1.7% for asthma/COPD patients.

Table 10: Robustness Checks: General & Differential Impacts of Closures

Stroke & AMI			
General Impact		Differential Impacts	
Variable	M.E. (Std. Error)	Variable	M.E. (Std. Error)
Treat*Post	0.0041*	RTreat*RPost	0.0065**
	(0.0019)		(0.0023)
		UTreat*UPost	0.0019
			(0.0021)
Treat	0.0006	Rural Treat	-0.0053**
	(0.0012)		(0.0029)
		Urban Treat	-0.0003
			(0.0014)
N	1,383,675	N	1,383,675
Sepsis, Stroke, & AMI			
General Impact		Differential Impacts	
Variable	M.E. (Std. Error)	Variable	M.E. (Std. Error)
Treat*Post	0.0025	RTreat*RPost	0.0122**
	(0.0021)		(0.0034)
		UTreat*UPost	-0.0013
			(0.0020)
Treat	0.0006	Rural Treat	-0.0096**
	(0.0012)		(0.0031)
		Urban Treat	0.0000

Table 10 Continued: Robustness Checks: General & Differential Impacts

			(0.0016)
N	2,100,858	N	2,100,858
Urban patients			
	General Impact	Differential Impacts	
Variable	M.E. (Std. Error)	Variable	M.E. (Std. Error)
Treat*Post	0.0011	RTreat*RPost	0.0068**
	(0.0017)		(0.0030)
		UTreat*UPost	-0.0013
			(0.0015)
Treat	0.0001	Rural Treat	-0.0075**
	(0.0009)		(0.0034)
		Urban Treat	-0.0006
			(0.0012)
		N	2,830,678
Medicare patients			
	General Impact	Differential Impacts	
Variable	M.E. (Std. Error)	Variable	M.E. (Std. Error)
Treat*Post	0.0025	RTreat*RPost	0.0079**
	(0.0021)		(0.0030)
		UTreat*UPost	-0.0007
			(0.0020)
Treat	0.0004	Rural Treat	-0.0107**
	(0.0012)		(0.0033)
		Urban Treat	-0.0003
			(0.0014)
N	1,807,165	N	1,807,165
Medicaid patients			
	General Impact	Differential Impacts	
Variable	M.E. (Std. Error)	Variable	M.E. (Std. Error)
Treat*Post	-0.0004	RTreat*RPost	0.0074**
	(0.0024)		(0.0030)
		UTreat*UPost	-0.0016
			(0.0023)
Treat	-0.0015	Rural Treat	-0.0035**
	(0.0013)		(0.0025)
		Urban Treat	-0.0007
			0.0017
N	436,931	N	436,931
White patients			
	General Impact	Differential Impacts	
Variable	M.E. (Std. Error)	Variable	M.E. (Std. Error)
Treat*Post	0.0018	RTreat*RPost	0.0067**
	(0.0018)		(0.0025)
		UTreat*UPost	-0.0007
			(0.0016)

Table 10 Continued: Robustness Checks: General & Differential Impacts

Treat	-0.0003	Rural Treat	-0.0090**
	(0.0009)		(0.0023)
		Urban Treat	-0.0006
			0.0012
N	2,126,579	N	2,126,579
Non-White patients			
	General Impact	Differential Impacts	
Variable	M.E. (Std. Error)	Variable	M.E. (Std. Error)
Treat*Post	0.0020	RTreat*RPost	0.0109**
	(0.0021)		(0.0029)
		UTreat*UPost	-0.0025
			(0.0017)
Treat	0.0001	Rural Treat	-0.0098**
	(0.0012)		(0.0030)
		Urban Treat	-0.0007
			(0.0017)
N	921,108	N	921,108

Results are reported in proportions.
Standard errors are reported in parentheses.
Errors are clustered by patient HSA.
* p<0.10, ** p<0.05
Average inpatient mortality rates for the control group by subgroups are as follows: 9.2% for stroke and AMI patients; 12.1% for sepsis, stroke, and AMI patients; 8.9% for urban patients; 10.8% for Medicare patients; 6.5% for Medicaid patients; 9.0% for White patients; and 8.7% for Non-White patients.