Harvesting the Toll: Rural Hospital Closures and the Health of American Farmers

By Mike Gibson *

Farmers are among the least healthy workers in the United States, across a range of health metrics. Increasingly, the rural hospitals they rely on for routine care are closing. This paper asks what losing a hospital does to farmers and to the farm economy. I exploit the staggered timing of 197 rural hospital closures and examine the effects of closure at the level of the hospital's market. Under this definition, counties that rely on a closing hospital are treated even if the hospital lay elsewhere. I separate these "failing" counties from "absorbing" counties, whose hospitals survive but take in the closed hospital's patients, and compare each group with isolated counties in the same state that are touched by neither. Using heterogeneity-robust difference-in-differences and synthetic difference-in-differences estimators, I find that closures reduce primary care physicians per resident in failing counties by about 5 percent, and the loss grows over time. Three consequences follow. The growth rate of agricultural establishments falls by about 4 percent. Farmers become 9.5 percentage points more likely to report worse health. All-cause mortality rises by 1.1 to 1.5 percent; death records do not identify occupation, so this increase cannot be attributed to farmers directly. County agricultural GDP, farm income, and total GDP do not change significantly, suggesting that the land of exiting farms stays in production and that the costs of closures fall on farm households rather than on aggregate output. Absorbing counties face a larger increase in ***potential*** *patients per hospital bed than failing counties but no increase in hospital utilization and no change in mortality. This points to distance and the loss of local care, rather than congestion at surviving hospitals, as the channel.*

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I. Introduction

A farmer's income depends directly on the capacity of his or her body to work, yet farmers are among the least healthy workers in the United States. Farmers exhibit higher rates of chronic disease than the general population, even after conditioning on demographic characteristics, including depression, anxiety, obesity, and respiratory disease (Beseran et al., 2022; Rudolphi et al., 2024). They die by suicide at roughly twice the rate of non-farmers, a higher rate than several groups commonly associated with "deaths of despair" (Beseran et al., 2022). According to data from the National Health Interview Survey, farmers are also more likely to die overall, and specifically from cancers, accidents, chronic respiratory conditions, and diseases of the heart and kidney. These gaps are neither new nor uniquely American: elevated rates of distress and suicide among farmers have been documented for at least two decades and across the United Kingdom, Canada, the Netherlands, Greece, Switzerland, Australia, and India (Fraser et al., 2005; Hounsome et al., 2012; Sanne, 2004; Demos et al., 2013; Berry et al., 2011; Ammann et al., 2025; Saju et al., 2024). If health is a productive input, why do the workers whose output depends most visibly on it appear to invest the least in it, and what does that cost agricultural production?

The question is becoming more pressing because the health care that farmers rely on is disappearing. Rural America is in a period of decline: populations are falling, growth is slowing, and communities are losing amenities such as schools and stores (Lichter and Johnson, 2025). Perhaps the most shocking loss is the loss of rural hospitals, more than 141 of which closed between 2010 and 2022 (Somanchi et al., 2024). Contrary to their popular image as emergency care providers, rural hospitals are a principal source of routine and preventative care: nearly all offer primary care, and most provide health screenings, immunizations, and community health education (Croll and Gale, 2024). A rural hospital closure therefore removes access to care from a population that is already at elevated health risk and already engaged with the health system less than other workers.

This paper asks whether reductions in rural health care access harm farmers and lower agricultural production. There are good reasons to expect that they would: Patients face a trade off between the cost to travel for care and the return they receive from care. A hospital closure raises the travel cost while reducing local medical capacity to provide care. Farmers have lower levels of health

before hospital closures, and tend to live in the areas where closures occur. As such, they might be the group most exposed to hospital closures.

To estimate these effects, I exploit the staggered timing of 197 rural hospital closures between 2010 and 2017. Using closures as identifying variation poses two challenges. First, hospitals do not close at random: closures reflect underlying economic conditions, and hospitals that close are likely unprofitable. Second, hospitals serve patients well beyond the county in which they are located, so comparing closure counties with non-closure counties misses spillovers within and between health care markets. I address the second challenge by defining treatment at the level of the Dartmouth Atlas Hospital Service Area (HSA), the area in which a plurality of Medicare patients use a given hospital, so that every county in a closing hospital's HSA is treated ("failing" counties). This choice of unit of treatment ensures I capture within-market spillovers. Closures also affect counties outside the failing HSA: neighbouring HSAs whose hospitals survive take in the closed hospital's patients. I treat these "absorbing" counties as a second, separately estimated group, and use as controls only "isolated" counties that neither lose a hospital nor absorb patients. The absorbing group captures spillovers between directly treated and nearby markets.

I address the first challenge by restricting the sample to rural counties and comparing treated counties only with isolated counties in the same state, which absorbs regional shocks such as state Medicaid expansions and regional economic cycles. I also show that across a range of economic, agricultural, and healthcare related outcomes, that the three groups have similar trends before closures occur. Because closures are staggered and their effects may grow over time, I use two heterogeneity-robust estimators: the difference-in-differences estimator of de Chaisemartin and D'Haultfoeuille (2023) and synthetic difference-in-differences (Arkhangelsky et al., 2021), estimated within state. I combine county-level data on physicians, hospitals, and agricultural and total output with individual-level data from the Current Population Survey.

I first show that hospital closures reduce access to care. In failing counties, primary care physicians per 1,000 residents fall by 4.5 to 5.3 percent, and the loss accumulates, from about 2 percent in the first year after closure to about 6 percent by the fourth. Pre-closure estimates are small and statistically insignificant. The decline appears throughout the hospital's market and not only in the county where the hospital was located. Fewer than one in ten treated counties loses its

nearest hospital, so the average distance to the nearest hospital rises by only about 2 percent (half a kilometer), yet physician supply falls across the market. Hospital employment in failing counties falls by about 3 percent, while ambulatory care and health care GDP do not change.

Second, closures slow the expansion in the number of farms. The growth rate in agricultural establishments in the Quarterly Census of Employment and Wages fall by about 4 percent under synthetic difference-in-differences (significant at 10 percent), and, like the loss of physicians, the decline builds over the years after closure, reaching about 5 percent by the fourth year. Agricultural employment grows by 2 to 3 percent less than in isolated, though not significantly. The reduction in growth of farms does not show up in county agricultural output: agricultural GDP, farm earnings, farm owner income, and agricultural wages change by small and statistically insignificant amounts, and the confidence intervals rule out declines in agricultural GDP larger than about 7 percent. One reading is that the farms that enter in isolated counties are small and contribute little to agricultural GDP.

Third, farmers report worse health. In the Current Population Survey, the share of farmers reporting good, fair, or poor health rather than very good or excellent health rises by 9.5 percentage points after closure (significant at 10 percent), and the effect grows with time since closure, mirroring the path of physician supply. Farmers are also 8.3 percentage points more likely to report that their health has worsened since their previous interview, although this estimate is imprecise. The deterioration is specific to farmers: the corresponding estimates for non-farmers are 1 to 2 percentage points and insignificant.

Finally, all-cause mortality rises. Log deaths per 100,000 residents increase by 1.1 percent under difference-in-differences (significant at 5 percent) and 1.5 percent under synthetic difference-in-differences (significant at 1 percent). The increase appears two to three years after closure. Under synthetic difference-in-differences the clearest age-specific increases are among adults aged 55 to 74, an age range that overlaps that of the typical farm operator, but the difference-in-differences estimates for these ages do not confirm them. Death records do not report occupation, so I cannot attribute the increase to farmers. The age pattern is suggestive, not conclusive, evidence that farm populations bear part of it. Beyond health care and agriculture, the local economy is largely unaffected: total GDP and non-farm income do not change, although non-farm proprietor income

falls by 3 to 4 percent, consistent with self-employed workers, like farmers, being more exposed to the loss of local care.

The absorbing counties help identify why closures matter. Their hospitals survive and their distance to care barely changes (about 0.5 percent), but they take in the closed hospital's patients: population per hospital bed in their HSA rises by 18 percent, more than the 12 to 14 percent increase in failing counties. Yet absorbing counties lose no physicians and no farm businesses, and their mortality does not rise. The hospitals that failing counties' patients are reassigned to offer slightly more services and score no worse on Hospital Compare quality measures. The losses in failing counties therefore appear to come from distance and the loss of local care, not from congestion or lower quality at the hospitals that remain.

These results differ from what earlier hospital-closure designs produce. A two-way fixed-effects regression of outcomes on an ever-treated indicator interacted with a post-2010 indicator finds that closures reduce agricultural GDP per capita by 0.6 to 1.0 thousand dollars and total GDP per capita by 1.8 to 3.4 thousand dollars. Both estimates fail pre-trend tests ($p \leq 0.002$), suggesting that closure areas were already on different paths before their hospitals closed. When the comparison is restricted to never-treated counties in the same state and the estimator accounts for staggered timing, these declines disappear. How treatment is defined matters as well. Treating only the county in which the hospital was located yields a first stage on distance more than three times larger than the market-level estimate, so studies that use county-level treatment and studies that use market-level treatment are estimating effects of different shocks.

Taken together, the results describe a closure that removes physicians from a rural market, after which farm growth slows, farmers report worse health, and mortality in the surrounding population rises, while county agricultural output holds steady. Neighboring markets that absorb the closed hospital's patients bear more patient pressure but none of these losses. Several explanations could reconcile fewer farms with stable output. First, county agricultural output may be resilient to the health of individual farmers, because land and capital remain in production when an operator cuts back or exits, by renting or selling to a neighbor or hiring custom operators. Second, four to nine years of post-closure data may be too short to observe losses in output.

This paper contributes to three literature. First, I extend the literature on access to care and

how that access contributes to health. Recent literature has shown that increased access to insurance **decreases** mortality. Using the Affordable Care Act expansion of 2014, researchers have found reductions in all-cause mortality of between 3-9.6% (Goldin, Lurie and McCubbin, 2019; Levy and Buchmueller, 2025; Borgschulte and Vogler, 2020; Miller, Johnson and Wherry, 2021). One paper found effects were concentrated in 20-45 year olds (Borgschulte and Vogler, 2020), while another finds the effects largest in the 45-54 age group, and no effects in older adults (Goldin, Lurie and McCubbin, 2019). The effects of the ACA expansion may differ on gender lines, although it is not entirely clear: one paper finds effects were larger for women than for men (Borgschulte and Vogler, 2020), while another finds the opposite (Miller, Johnson and Wherry, 2021). Effects were larger for White, non-Hispanic Americans (Miller and Wherry, 2017). A meta-analysis of other health insurance expansions found largest effects in children, particularly black children, and previously uninsured adults (Levy and Buchmueller, 2025). It appears that differences in compliers drives heterogeneous impacts across age, gender, and racial groups, and insurance expansion policies. Kowalski (2023) reconciles the differences in effects of similar expansions in Oregon and Massachusetts by showing that the compliers in Massachusetts—those who would only be insured if Massachusetts implemented the expansion—are more similar to Oregon never-takers. Ex ante, it is not clear how farmers might respond to a **decrease** in health access. Farmers are frequently uninsured in the United States, due in part to how many are self-employed, and have lower rates of health care utilization than non-farmers. A hospital closure differs from an insurance expansion: an expansion lowers the price of care for those who take up coverage, while a closure reduces the supply of care for everyone in the hospital's market. I show that the supply reduction is real and that it is followed by higher all-cause mortality, the mirror image of the mortality reductions found for insurance expansions.

Second, the paper contributes to the literature on the consequences of rural hospital closures. Existing estimates disagree: some studies find no effect of closures on mortality while others find increases, and some find that hospitalizations rise while others find no change (Joynt Maddox et al., 2015; Gujral and Basu, 2019; Khushalani et al., 2023). I argue that two features of the setting help explain these discrepancies. Much of the existing work predates heterogeneity-robust event-study estimators, and if the effects of closures grow over time, two-way fixed-effects regressions can place

negative weights on some treatment effects. In addition, studies that compare closure counties with non-closure counties ignore the fact that hospitals serve patients across a wider market. Defining treatment at the hospital service area and using heterogeneity-robust estimators, I show that both concerns are empirically relevant: effects on physician supply grow over time and appear in counties that never lose their nearest hospital. I also separate the counties that lose a hospital from the neighboring counties that absorb its patients, which would otherwise contaminate the control group, and show that the two groups respond differently. With this design, I find that closures raise all-cause mortality by 1.1 to 1.5 percent in failing counties. I also extend the outcomes studied in this literature beyond health and local economic activity to the number of farms, farmer health, and agricultural production.

Third, the paper contributes to work documenting the impacts of poor health on production. Previous literature shows that health conditions lower productivity (Beck et al., 2011; Bokma et al., 2017; Simon et al., 2008; Knudsen et al., 2012). These studies focus on how health conditions, particularly chronic conditions, increase the amount of time a worker misses work (absenteeism), or is present at work but distracted (presenteeism). The literature focuses on wage and salary work, often performed in office settings. Ex ante, one might expect poor health to impact farming differently: first, farmers face an "off-season" where the returns to labor are low. Secondly, many farms in the United States are operated by multiple operators: if one suffers an illness or injury, the other may be able to shoulder the load. I show that while hospital closures slow the expansion of new farms in failing counties, they do not lead to a reduction in agricultural GDP or farm owner income. These findings suggest that the production-decreasing effects of poor health observed in non-farm work do not directly translate to the agricultural sector.

The main results are robust in several directions. Pre-closure estimates show no differential trends in physician supply in either failing or absorbing counties, and the lower baseline physician supply of treated counties is a difference in levels that county fixed effects absorb rather than a difference in trends. I test three alternative specifications to both the difference in difference and synthetic difference in difference to restrict the comparison group; the results are robust for all outcomes.

These findings have implications for rural hospital policy. Closures reduce access to care, and they are followed by fewer new farms, worse farmer health, and higher mortality. Because the

counties that absorb patients do not see these losses, what a closure removes appears to be local access to care rather than regional hospital capacity. Arguments for preserving rural hospitals can therefore rest on their direct health benefits, including for farm households. Preserving hospitals alone is unlikely to reverse broader rural economic decline, however, because total GDP and non-farm income do not respond. The paper has several limitations. The Current Population Survey masks small counties, so farmer-level effects are estimated on a small and relatively populous subset of treated counties and are imprecise. Mortality data do not report occupation, so the increase in deaths cannot be assigned to farmers. The post-closure window may also be too short to capture slowly accumulating effects on production.

The remainder of the paper proceeds as follows. Section II provides background on farmer health, the role of rural hospitals, and the link between health and productivity. Section III describes the data, and Section IV presents the empirical strategy. Section V reports the results, including the first-stage effects on access to care, the effects on agricultural production, effects on agriculture and the general economy, dose response, and a comparison with designs used in earlier work. Section VI concludes and discusses future research.

II. Context

A. Farming, farmers, and farmer health

Despite decades of urbanization, agriculture remains an important component of the U.S. economy—particularly in rural areas–and occupies a substantial share of the country's land. In 2017, 1.19 billion acres, roughly 52.5% of total U.S. land area, were devoted to grazing land, cropland, or agricultural special uses. The specifics of agricultural land use varies widely across the country: cropland is concentrated primarily in the Midwest, while grassland, pasture, and range are concentrated in the south western region. Farming accounts for approximately 0.8% of U.S. GDP annually, generating $222.3 billion in 2023 (ERS, 2026). A large share of agricultural value comes from a relatively small number of crops. In 2024, corn accounted for 27% of agricultural cash receipts and soybeans for roughly 19%, followed by wheat at approximately 5%, and the production of these three crops is concentrated in the Northern Plains and the Corn Belt (ERS, 2026). Consolidation also means that land typically remains in production. Reports show that 37.3% of farms are run

by multiple operators, suggesting that declines in the health or number of farmers in a county may not lower agricultural production mechanically (Sherrick, 2017).

The structure of U.S. agricultural production has changed substantially since the 1800s. Today's agricultural sector has fewer farms, larger average farm sizes, and fewer agricultural workers, reflecting the development of labor-saving technologies such as pesticides and machinery. There are currently roughly 1.88 million farms in the United States, compared with a peak of nearly 7 million in the 1930s (Keller and Kassel, 2025). Although the total amount of land in farms has remained relatively stable since the 1850s at approximately 876 million acres, average farm size has increased from about 200 acres in the 1850s to nearly 500 acres today (Keller and Kassel, 2025). These changes reflect an economic transition that has favored larger operations: capital has become relatively cheaper while labor has become more expensive, increasing the minimum viable farm size and reducing the amount of labor required per farm (Sumner, 2014). The shift away from labor to capital may have contributed to population decline in many agricultural communities. More than 40% of rural counties reached their population peak in 1950, and the aggregate population of rural counties declined between 2010 and 2020 (Lichter and Johnson, 2025). Importantly for health outcomes, the decline in farms and farm workers can increase the geographic and social isolation experienced by farmers, which have been shown to increase the prevalence of mental health conditions such as depression (Luo, 2022).

The changes in the structure of agricultural production may play a role in the differences between farmers and the general public. As shown in Table A1, compared to non-farmers, farmers are more likely to be age 65 or older, more likely to be white, currently unmarried or widowed, and Hispanic. Farmers also have different socioeconomic characteristics, as shown in Table A2: they are more likely than non-farmers to have relatively low levels of education and less likely to hold bachelor's or postgraduate degrees. In terms of income, farmers are also more likely to have family incomes below $50,000. Consequently, they are more likely to have family incomes below the poverty threshold and, conditional on being above poverty, to fall into lower income-to-poverty-ratio categories. They are also more likely to receive Supplemental Security Income (SSI) and Electronic Benefits Transfer (EBT) benefits.

In addition to socio-economic differences, farmers have worse health than non-farmers. Studies

show that farmers in the United States had approximately twice the suicide rate of non-farmers between 2000 and 2020 (Beseran et al., 2022). Their suicide rate was also higher than that of several groups commonly identified with the broader "deaths of despair" literature, including non-college-educated, non-Hispanic white men; men over age 45; and rural Americans. Farmers also report substantially higher rates of anxiety and depression than the general public: as many as 71% report anxiety and 53% report depression, compared with 18% and 6.7% of the general population, respectively (Rudolphi et al., 2024). Evidence from work-related deaths in Georgia similarly finds that farmers were more likely than non-farmers to have current depressed mood or a current mental health problem identified as a circumstance surrounding their deaths (Lavender et al., 2016). However, the health gap between farmers and non-farmers is not unique to the United States. Studies from the United Kingdom, the Netherlands, Greece, Australia, Switzerland, and India similarly document elevated psychological distress, depression, anxiety, suicide, or related mental health risks among farmers relative to non-farmers or the general population (Hounsome et al., 2012; Sanne, 2004; Demos et al., 2013; Berry et al., 2011; Ammann et al., 2025; Saju et al., 2024).

Although the precise causes of the farmer health gap are not fully understood, three explanations are commonly emphasized: isolation, chemical exposure, and the financial and occupational stress inherent in farming. The decline in the number of farms and agricultural workers can leave farmers increasingly geographically and socially isolated. Isolation and loneliness have been shown to worsen anxiety and depression and increase suicidal ideation (Shankar, 2023; Prager, Tulman and Durst, 2018). Farmers rely heavily on fertilizers, pesticides, and herbicides. Farmers use approximately eight times as much pesticide as in 1948 (Keller and Kassel, 2025), and research links pesticide exposure to several health conditions, including mental distress and depression and, to a lesser extent, suicidal ideation (Zanchi, Marins and Zamoner, 2023; Antle and Pingali, 1994). Chemical inputs can also substitute for labor, potentially increasing the isolation of farm owners. Farmers face uncertainty from fluctuating output prices, trade policies, interest rates, insects, weeds, disease, and weather events such as floods (Fields and Rainey, 2025). Farmers also report limited control over important financial aspects of their businesses, including the prices they pay for inputs and receive for outputs. These financial pressures are particularly important because farming is both

an occupation and a source of personal identity. The typical American farm earns a profit only about one out of every three years, and slightly more than half of farm households reported negative income from farming operations in a 2018 report (Prager, Tulman and Durst, 2018). Farmers may internalize financial failure or reliance on government assistance as a failure of their identity as farmers, contributing to mental distress. Finally, because approximately 95% of U.S. farms are family-owned, farmers also face uncertainty surrounding farm succession and the transfer of their operations to the next generation (USDA, 2025; Baker and Bush, 2025).

Together, these differences between farmers and non-farmers make farmers a population for whom access to health care may have particularly important consequences. Farmers face unusually high physical and mental health risks while working in an occupation that is geographically concentrated, increasingly isolated, and dependent on the health and productivity of individual farm operators. The next section therefore considers the role of health in determining a worker's productivity.

B. Productivity and health

Health economics has shown that poor health can reduce worker productivity, creating an important economic consequence of the health problems described above. In this context, productivity is typically measured via labor supply: **absenteeism**, or time away from work because of a health condition, and **presenteeism**, or reduced productivity while remaining at work. This definition of productivity is different from how productivity is typically measured in agriculture, where it is used to determine which farms/farmers can produce the most output for the same inputs (or use the least inputs to produce the same output). In this paper, I focus on farm owner income as well as the value of products produced in the agricultural sector, which ties more closely into the health economic literature concept of "productivity".

There is ample evidence that negative health shocks reduce productivity. For example, research finds that depression increases both absenteeism and presenteeism. One study estimates that each additional point on the PHQ-9 depression scale is associated with a 1.65% decline in productivity (Beck et al., 2011). Other mental health conditions, including anxiety (Bokma et al., 2017), bipolar-II disorder (Simon et al., 2008)), and binge-eating disorder (Knudsen et al., 2012), have also been shown to reduce worker productivity. The burden of health shocks in most sectors is concentrated

in presenteeism because workers are generally absent relatively infrequently, but frequently work while sick. Maestas, Mullen and Rennane (2021) find that the modal worker was absent from work only once during the study period, while 69% reported working while sick. Workers who worked while sick estimated that their productivity was only 77% of its level when healthy (Maestas, Mullen and Rennane, 2021). The economic costs associated with these productivity losses can be substantial: estimates for depression place the yearly cost per U.S. worker at approximately $390 from absenteeism and $5,524 from presenteeism (Evans-Lacko and Knapp, 2016).

Evidence also shows that positive health shocks can improve productivity. Treating health conditions allows workers to supply more hours of labor, or devote more effort in the same amount of time. An elimination of parasitic flatworms in Burkina Faso farmers led to 7% increases in agricultural yield, while reducing the likelihood of heavy drinking among male agricultural workers in Kenya by 19 percentage points lead to a 32% increase in the annual harvest (Rinaldo et al., 2021; Murphy, 2026).

However, the standard approach to measuring the productivity effects of poor health may not translate directly to farmers. Much of the existing literature converts time lost to health conditions into a dollar value by multiplying hours or days lost by the worker's wage or salary. This approach is less straightforward for farmers because the majority of U.S. farms are family-owned businesses: family-owned farms account for approximately 94% of farms and 84% of agricultural production (USDA, 2025). Rather than receiving a conventional wage or salary, many farm owners receive the residual income generated by the farm after operating costs are paid. Consequently, the economic value of a farmer's time is not necessarily captured by an observed wage. Additionally, the value of an hour of labor *may* be similar across days, weeks, and months in many industries, but there is little reason to believe the same relationship holds in farming. Farming is a seasonal occupation: the return to an hour of labor is high during the planting and harvesting season, but might be lower during the "off-season" (i.e., winter) when crops cannot be grown.

The nature of agricultural work creates additional reasons why conventional measures of absenteeism and presenteeism may be poorly suited to farmers. Farmers and farm workers face exceptionally high rates of occupational injury, increasing the likelihood of both temporary absences and longer periods away from work (Fife, 2018). Moreover, while previous research finds

that presenteeism generally generates greater productivity losses than absenteeism, this difference may be smaller in agriculture. In many occupations, workers can continue working while sick because their physical presence is not essential to the production process; this may be less feasible for agricultural workers facing physically demanding work or injuries.

C. Hospitals and their role in rural health

Farmers, by virtue of living in rural settings, tend to rely on hospitals. Hospitals play a large, central role in the provision of health in rural communities, particularly in the provision of routine, preventative care. A 2022 survey found that rural hospitals commonly provide community outreach and prevention services, including general community outreach (81.5% of rural hospitals), health fairs (79.2%), community health education (86.9%), health screenings (87.9%), and immunization programs (52.9%) (Croll and Gale, 2024). These services can prevent or identify health problems before they become emergencies, making hospitals an important component of the broader rural health-care system. Nearly 100% of rural hospitals offer routine care delivered by primary care physicians (Croll and Gale, 2024). Rural hospitals are also more likely than urban hospitals to provide services such as long-term care, home health services, and health fairs—services for which rural communities may have few or no alternative providers (Croll and Gale, 2024).

The importance of rural hospitals reflects the limited availability of alternative sources of care in rural communities. Rural communities face persistent shortages of primary care providers. Of the 1,977 counties classified as rural in 2023, only 166 were not designated as having a health professional shortage (Horstman and Shah, 2025). Limited access to primary care affects where rural residents ultimately seek treatment. Approximately 38% of adults in rural counties use hospital emergency departments for care that could have been provided by a primary care physician in a non-emergency setting if that option were available (Horstman and Shah, 2025). In theory, telehealth could provide a potential alternative to in-person care, but its use remains lower in rural areas. In 2023, 19% of rural adults reported receiving primary care through telehealth, compared with 29% nationally, and take-up was low (Horstman and Shah, 2025).

The limited availability of alternative providers makes rural hospitals especially important for farmers. Using the NHIS and shown in Figure **??**, I find that farmers are more likely than non-

farmers to receive both routine and non-routine care at a hospital. Part of this difference is that farmers are concentrated in rural areas and non-farmers are not: conditioning on rural status drives some of these effects towards zero. Farmers are also more likely to use the same facility for both routine and non-routine care. At the same time, farmers are more likely to experience delays in receiving medical care, particularly because they are unable to obtain an appointment. These patterns suggest that hospitals may play an especially important role in the health-care access of farmers, making changes in the availability of local hospitals particularly consequential for this population.

For farmers, these effects may be particularly important. Farmers already experience worse physical and mental health outcomes, face substantial occupational risks, and report difficulty obtaining care. At the same time, they are more likely than non-farmers to rely on hospitals for both routine and non-routine care. Whether these consequences are large enough to affect agricultural production is the question this paper examines.

III. Data

To determine if hospital closures result in changes in agricultural health and production, I first build a dataset of hospitals in the United States. I rely largely on the Dartmouth Atlas Hospital Research Data, which tracks hospitals from 2008—2017. I combine this with the Cecil G. Sheps Center for Health Services Research Rural Hospital closures data. I describe the details of how I combine these datasets in Section III.B. This information is combined with the dataset of hospital service areas (HSAs) crosswalked with zip codes, also sourced from the Dartmouth ATLAS project. The details of how HSAs are constructed is described in Section III.A.

I combine this information with the Area Health Resource Files published by the Health Resources and Services Administration. These files provide the counts of various medical professions and health facilities, measured at the county-year level. Each yearly release provides updated counts for several previous vintages (e.g., the 2001 release contains new information on 1999, as well as updated information on 1995—1998), so I use the final instance of each vintage rather than rely on the year of release. In particular, I use the count of practicing, patient-care focused primary care doctors to measure the count of primary care doctors in each county in each year, which excludes

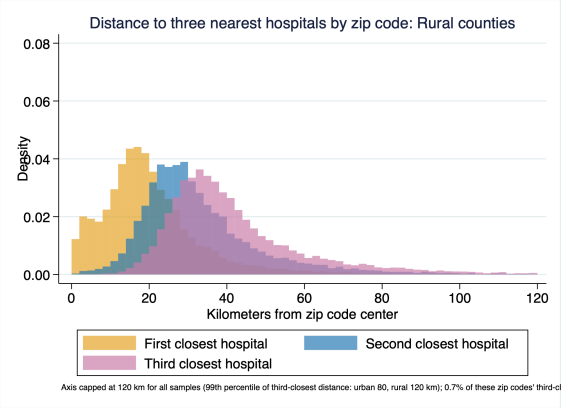
PCPs who either are not practicing, or devoted to research or administration.

For economic outcomes, I obtain county-year measures of agricultural production, land use, and yield from the USDA National Agricultural Statistical Service's QuickStats database, measured at the county-year level. These are aggregate variables and reflect the total amounts of money earned, land used, or production across all farms or farmers in the county. From the Bureau of Economic Analysis, and the Quarterly Census of Employment and Wages, I obtain county-year measures of employment, earnings, and GDP by sector. I use the Current Population Survey (CPS) to obtain farmer-county-year level information. The public information sourced from the CPS comes with the caveat that counties with fewer than 50,000 residents are masked, preventing me from including them in estimation. This limits my sample for detecting farmer-level effects.

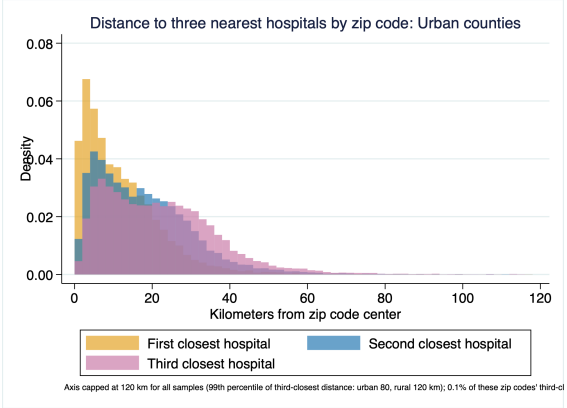
A. Hospital markets

I leverage the 2010 hospital service areas created by the Dartmouth Atlas project. HSAs are created from Center for Medicaid and Medicare Studies (CMS) patient records, and assign zip codes to the hospital in which the plurality of that zip's patients seek services. Most HSAs are defined by 1 hospital; but in some urban settings, HSAs can contain multiple. This is due to data limitations in the original creation of HSAs in the early 2000s, when hospitals were recorded in **towns**, not by **zip code**. The Dartmouth Atlas reports the HSA for each zip code, but not all hospitals the zip code sends patients to. For example, if zip code A sent 40% of patients to HSA 1, 30% to HSA 2, and 20% to HSA 3, I only observe that the zip code lies in HSA 1. As the bulk of my data is at the county level, I consider a county to be in a HSA if any zip code within the county is in the HSA and hold the county membership of zip codes at their 2010 level.

To understand how hospitals and services are allocated across space, I first focus on the distance from each zip code centroid to the three nearest hospitals, presented in Figure 1. For zip codes in rural counties (Panel 1a), the average distance to the closest hospital is roughly 20 kilometers; the second and third are roughly 32 and 38 kilometers respectively. The right tail of the distribution is large: for some zip codes in rural counties, the **closest** hospital is more than 60 kilometers away. In contrast, zip codes in rural counties on average have closer hospitals. Shown in Panel 1b, the average distance to a zip codes first, second, and third hospital are all under 10 kilometers away.



(a) Zip codes in rural counties

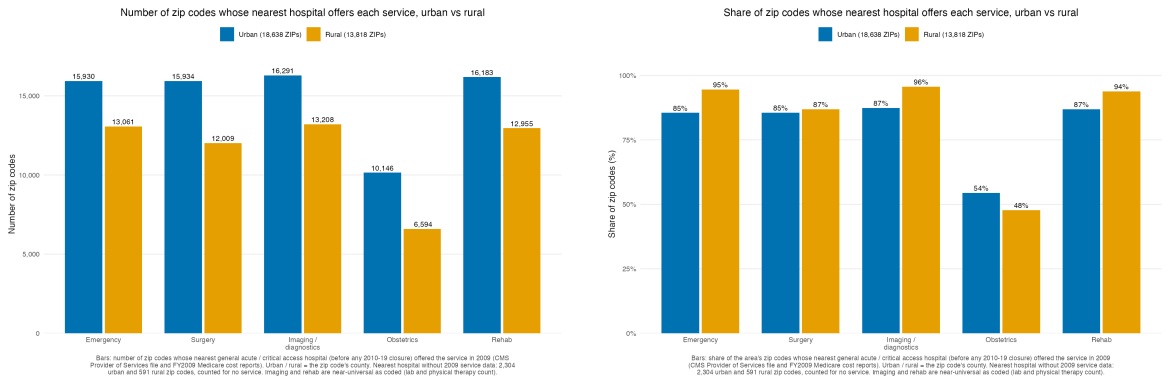


(b) Zip codes in urban counties

Figure 1. : Distance to three nearest hospitals

The difference in access to hospitals between rural and urban county zip codes extends beyond difference. I first focus on the number of services offered by hospitals. Reported by the CMS Provider of Care file, this metric reports whether the hospital provided broad categories of care: emergency (the hospital provides emergency services or has a dedicated emergency department), surgery (it has operating rooms, inpatient surgery, or an outpatient surgery unit) imaging/diagnostics (it offers diagnostic radiology, CT, MRI, nuclear medicine, PET, or a clinical lab), obstetrics (reported positive cost on the nursery or delivery & labor room line in its cost report) and rehabilitation (it offers physical, occupational or speech therapy, inpatient or outpatient rehab, or has a rehab unit). Shown in Panel 2a, larger numbers of urban zip codes' nearest hospitals offer all five types of services. Dividing the urban and rural counts of hospitals with service by the total number of urban and rural counties, shown in Panel 2b, shows that a greater *shares* of rural zip codes are connected to emergency, surgery, imaging and diagnostic services, and rehabilitation services. However, in both counts and share, less rural counties are connected to hospitals that offer obstetrics.

Rural zip codes are also connected to lower performing hospitals. The CMS requires hospitals to evaluate cases to determine if the process of care (POC) was followed. These scores report the percentage of cases where a provider gives the recommended, guideline-concordant care. For example, the heart attack POC score includes the percentage of heart attack patients who received clot-busting medications within 30 minutes of arrival. Shown in Figure 3, POC scores are lower in the hospitals connected to rural zips for heart attack processes, heart failure processes, pneumonia



(a) Count of zip codes with access

(b) Share of zip codes with access

Figure 2. : Services offered by nearest hospital

processes, and surgical processes; but higher for children's asthma processes. These scores are a rough measure of the quality of care provided at the hospital, but do not take into account patient outcomes or experiences–patients who receive the guideline-concordant care can still die, for instance. However, they measure the percent of the cases in which providers were doing "what they were supposed to".

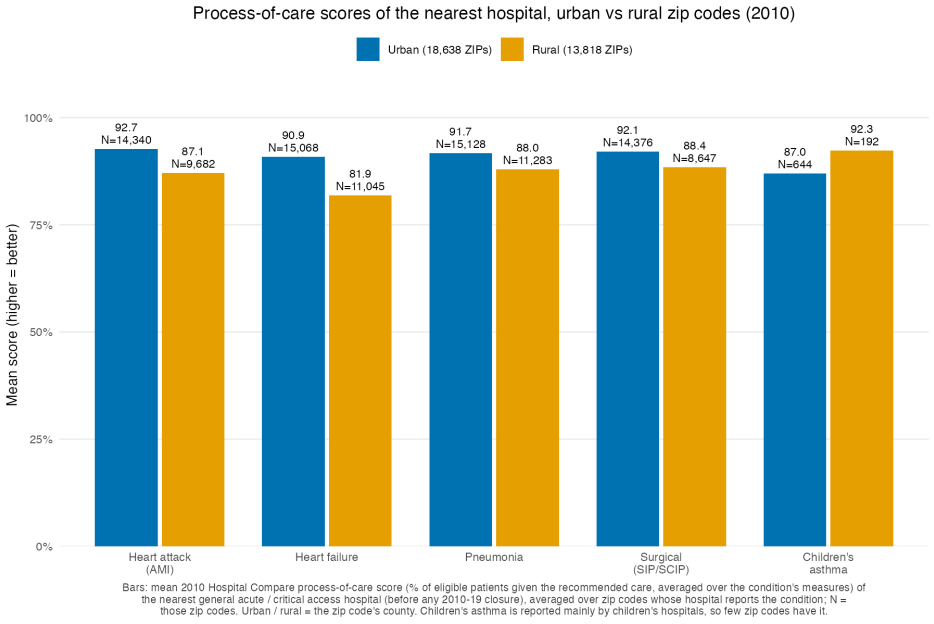


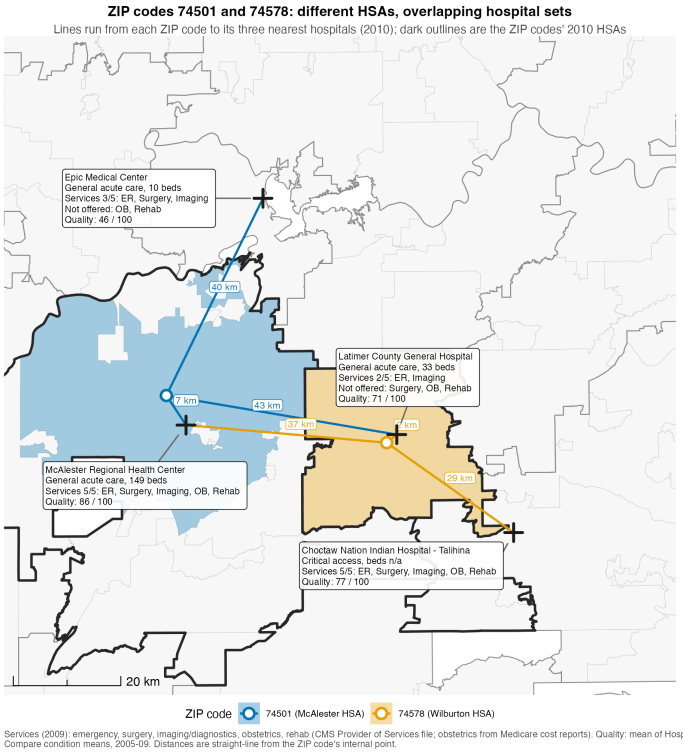
Figure 3. Process scores

The differences between rural and urban zip codes in hospital experiences presented so far beg the

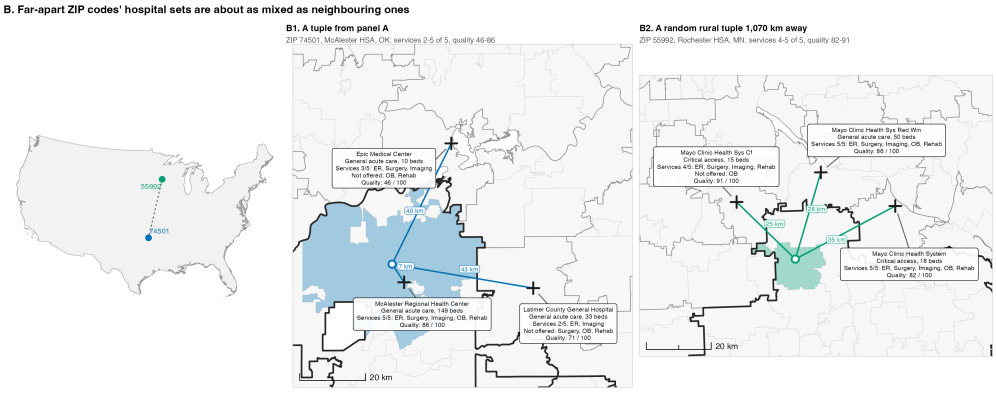
question: as rural zip codes are predominately serviced by lower-quality hospitals that offer worse services, does the closure of a rural hospital expose its HSA to a better hospital? Put differently, are the next-nearest hospitals a rural zip is closest to different than their HSA hospital? To determine the similarity of potential nearest hospitals a zip code is exposed to, I find the three nearest hospitals to each zip code, and call this a zip-hospital tuple (ZH). I then compare decompose the variance in hospital characteristics to within hospital variation in a ZH and between ZHs variation. Shown in Figure C2, the bulk of hospital characteristics have larger **within** ZH variance than between ZH variance. This implies that if a zip code's HSA hospital were to close, the next two nearest hospitals— who are potential absorbers of the HSA patients–are likely to offer different services, or the same services at a different level of quality, or both.

To illustrate this, I present two different ZH in Panel 4a. Zip 74501 (blue) and zip 74578 (orange) are neighboring zip codes, both of which contain a hospital. As such, these two hospitals constitute the nearest and second nearest hospital for each zip, correspondingly. Despite this overlap, the third hospital in each ZH is radically different. The blue zip code's third nearest has a lower quality score (46 compared to 86) and offers fewer services (no OB or rehab) than the blue's HSA hospital and second closest. The orange zip's third nearest hospital is actually of higher quality (77 compared to 71) and offers *more* services (Surgery, OB, and rehab) than orange's HSA hospital. In Panel 4b, I retain the blue ZH and compare it to a randomly selected ZH (zip 55992, in green). Despite how far apart the blue and green ZH are, the Malahanobis difference between blue and orange is larger than the Malahanobis distance blue and green (0.86 to 0.58). In combination, these findings suggest that hospital closures expose residents of its HSA to *different* types of hospitals, offering different care and at different quality levels.

Specifically, the closure of a resident's nearest hospital exposes them to hospitals that offer **both** better quality and more services. In Figures C3b and C3, I compare the quality or services offered at each zip's closest hospital in before and after closure, separately for closure and surviving zips. The surviving zips (in blue) do not experience changes in quality or service by definition, as their hospital has not changed. The closure zips (in orange), overwhelmingly move to hospitals that offer more services and at higher quality. In particular, a closure zip's new nearest hospital is more likely to offer obstetrics or surgery, and more likely to be in the highest rating for process of care.



(a) Neighboring Zip-Hospital tuples



(b) Distance Zip-Hospital tuples

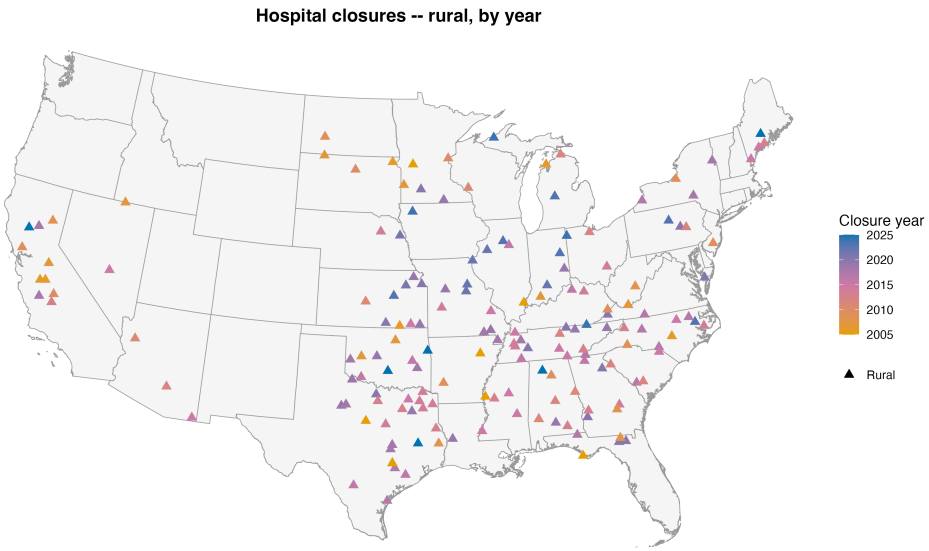
Figure 4. : Zip-hospital tuples that are close are just as different as those far apart

These findings suggest that patients in closing zips now compete for better or more types of care among surviving hospitals. These findings matter for the interpretation of results. If, for instance, I had found that when a zip's nearest hospital closed, it would be exposed to **worse** hospitals and its health declined, it would be difficult to determine if the results came from a loss of local access in care (i.e., fewer patients being treated), or from receiving worse care (i.e., no change in number

of treated patients, but patietns received worse care).

B. Hospital closures

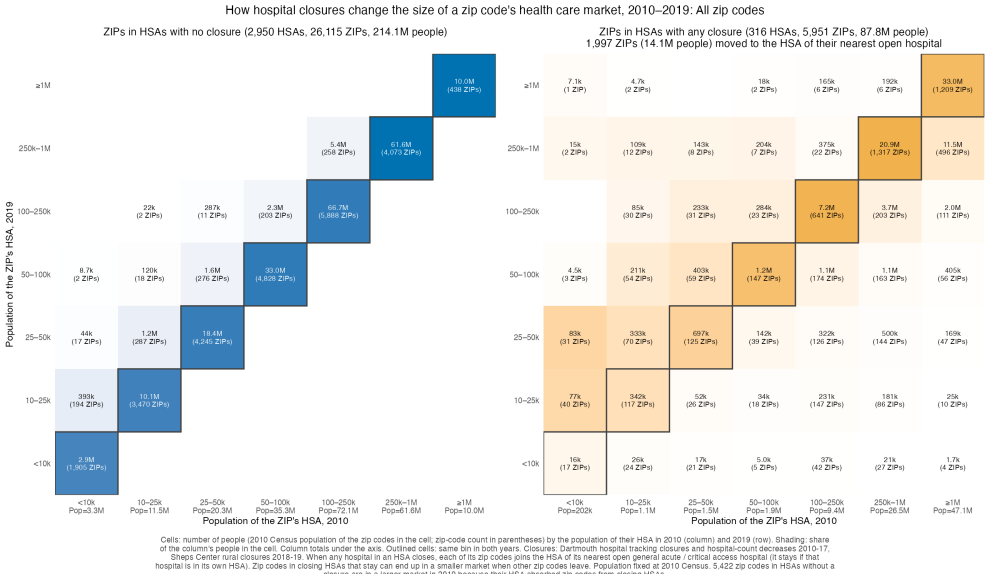
From the Hospital Research Data and the Rural Hospital Closure data, I end up with a list of hospital locations in the United States, as well as the dates of closures. When the sources conflict, I manually verify the hospitals status by searching local papers to confirm the hospital has closed (or if it is open but operating under another name). As a result, I end up with 224 closures, with 197 occurring in rural counties. A map of closures is presented in Figure **??**. *Rural* closures predominately occur in the eastern half of the United States & in California. In Section C.C1, I test if there are differences in county metrics by closure cohort, and can broadly reject that closures occur due to differences in county economic factors.



From the list of closures, I identify the hospital service areas that have failed (i.e., lost their hospital) and use it to identify "failing" counties. The remaining counties are composed of HSAs whose hospital survived. These counties *might* serve as comparison group for the failing counties. However, these counties consist of two subsets of counties: some HSAs are located near to failing HSAs. If patients from the failed HSA seek care at neighboring hospitals, these patients join this

surviving HSA. Some HSAs are located far from failing HSAs, and so might not receive patients from the failing. This can be seen in the left hand side of Figure ??. In this figure, I calculate the population of each HSA by determining the population of its zip codes in 2010. I organize zip codes by the 2010 population level on the X-axis. I then look forward in time and identify all zip codes that will experience a closure. I then re-assign their population to the nearest zip code in a surviving HSA, and re-calculate the HSA population, shown on the Y-axis.

In the left hand side, two clear patterns emerge: many zips in surviving HSAs have the same population before- and after- hospital closures occur elsewhere. These zips can be seen in the dark blue diagonal. I consider these zips (and counties that compose them) "isolated". However, many zip codes have *absorbed* patients from the closed HSAs, resulting in a higher HSA population. These zips can be seen as the mass of zip codes that are on the off-diagonal. I consider these zips (and their counties) as "absorbing" HSAs. On the right hand side, two different patterns emerge: some zips move into a HSA with a larger population than their previous: these are zips above the diagonal line. A smaller number of zips move into HSAs with a smaller population than their previous HSA.



The shifts experienced by absorbing and failing zips have different economic meanings. Orange zips experience a hospital closure: their HSA hospital as closed. As shown in Figure 1a, this

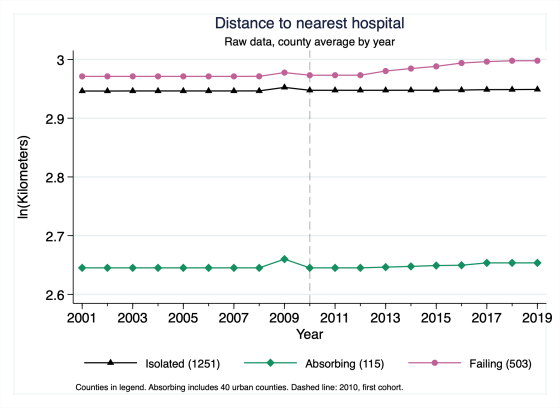
likely increases the distance that patients in these zips must travel to receive care by pushing these patients towards their second or third nearest hospital from their first. For orange zips above the diagonal, they move into a HSA with a higher total population, increasing the pressure for available resourses. Blue zips above the diagonal are also in a HSA whose population has increased due to hospitals failing in the orange zips. The increase in HSA population results in higher patient pressure for existing resources: there are now more potential patients for the given supply of care (at least in the short run). This change is an increase in pressure, absent the change in distance, as their hospital survives.

IV. Empirics

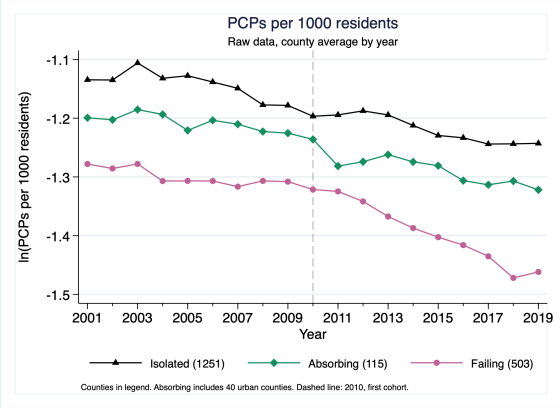
A natural starting point when seeking to understand how hospital closures impact areas is to compare ares with a closure to those who do not, before and after treatment. In other words, a difference-in-difference. However, as noted, closures create three distinct groups: counties experience a closure, counties i a surviving HSA and absorb patients from closed hospitals, and counties in a surviving HSA but do not absorb patients. The first two groups are exposed to a hospital shock, while the final group is not. As such, I consider this last group–the isolated counties–as the never-treated counties, and compare each of the exposed groups to the isolated counties.

I consider a county as experiencing a hospital closure starting from the first year in which a general acute-care or critical-access hospital closes in a hospital service area (HSA) that overlaps the county. I consider a county as absorbing starting from the first year a neighboring HSA closes and the county could receive new patients from the failed HSA. In this sense, the "absorbing" group is created when a hospital closure occurs. Both treatment indicators are absorbing: once a county experiences a closure (or absorbs from a closed county) it is considered closed (or absorbing) for the remainder of the sample period. The resulting sample contains 1,869 counties in 42 states: 503 failing counties, 115 absorbing counties, and 1,251 isolated counties.

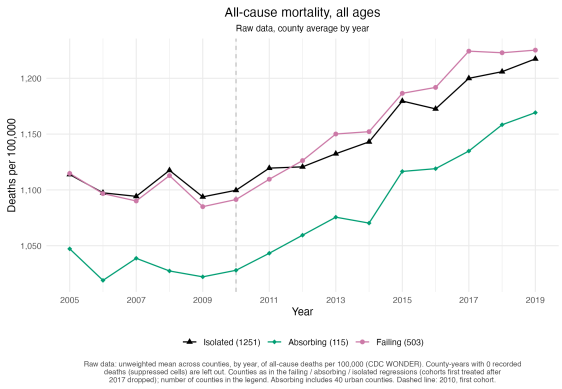
Crucially for identification, it must be the case that that failing, absorbing, and isolated counties have similar trends prior to closure and absorption. In Panels 5a, 5b and 5c of Figure 5, I show trends in the raw data of distance to nearest hospital, number of primary care doctors per population in thousands, and all-cause, all-age mortality, respectively. While there are clear level differences



(a) Distance to nearest hospital



(b) Ratio of PCPs to population in 1000s



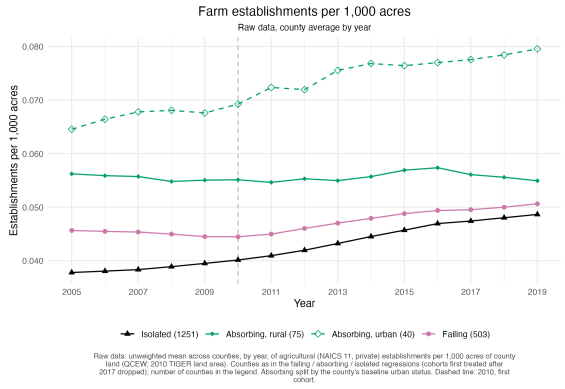
(c) All-cause, all-age mortality rate

Figure 5. Trends in raw data: health metrics

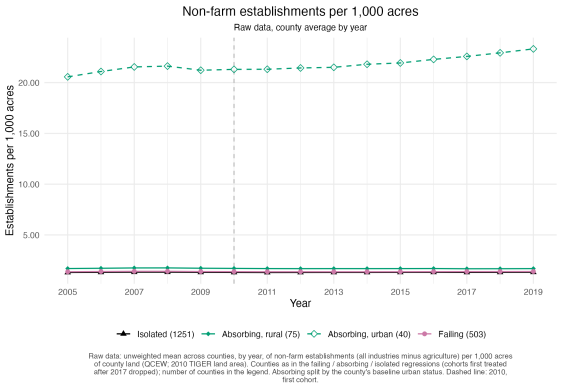
Notes:

between the three groups, each trends similarly prior to 2010 (date of first closure/absorption); and have different trends after the first closure happens. To provide context on the treatment effects, it is worth noting that all three groups were experiencing declines in the number of PCPs per residents, and increases in mortality. This latter finding, in particular, is part of an established trend of increased deaths in rural areas (Lichter and Johnson, 2025). Absorbing counties tend to have lower mortality, which likely reflects the fact that they tend to be on the large and developed side of rural counties. However, they do not offer relatively more PCPs for their residents, and see similar declines in the number of PCPs from 2001 to 2010.

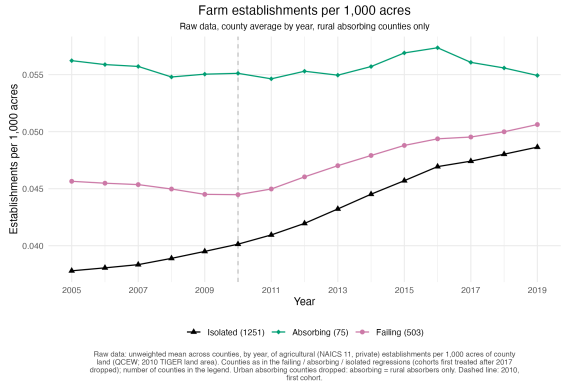
The similarity in trends extends beyond health-related outcomes. Shown in Figure 6, the three groups have similar pre-closure/absorption trends in number of non-farm-related businesses per acre



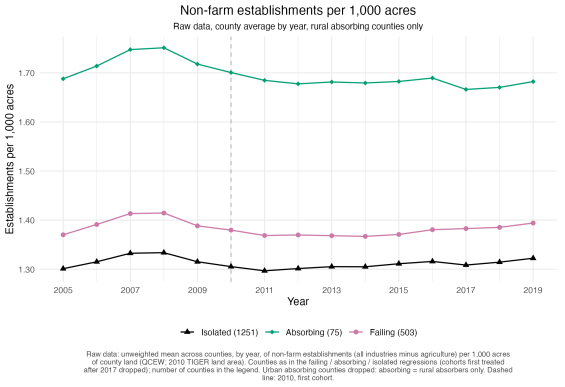
(a) Farms and farm establishments per acre, including urban absorbers



(b) Non-farm establishments per acre, including urban absorbers



(c) Farms and farm establishments per acre



(d) Non-farm establishments per acre

Figure 6. Trends in raw data: density of farm- and non-farm- establishments

Notes:

(Panels 6b and 6d), albeit with clear level differences. Absorbing counties have a higher-density of non-farm businesses per acre, even when omitting urban absorbers. The trends look slighlty less parallel in terms of farms per acre (Panels 6a and 6c). For context, it is clear that the bulk of the study period occurs *after* the 2008 recession: there are visible declines in the number of farm and non-farm establishments in 2008-2010, and increases (at east in farms) post 2010. This finding suggests the results should be considered as changes to areas recovering from an economic recession—i.e., a finding of slower GDP growth would indicate that treated areas are recovering slower than non-treated areas.

A. *Estimators*

Heterogeneity robust difference-in-difference

In light of the similarity of trends in the pre-closure/absorption period, my basic specification is an event-study difference-in-difference. With staggered treatment and effects that may vary across cohorts and over time, two-way fixed-effects regressions can put negative weight on some treatment effects. I therefore use the estimator of de Chaisemartin and D'Haultfoeuille (2023). For each treated county g with first treatment year F_g, the effect ℓ years after treatment compares the change in the outcome from $F_g - 1$ to $F_g - 1 + \ell$ with the same change among control counties. The estimand is

$$\delta_\ell^O = \frac{1}{N_\ell^O} \sum_{g:\, F_g - 1 + \ell \leq T} \Big[\big(Y^O_{g, F_g - 1 + \ell} - Y^O_{g, F_g - 1} \big) - \big(\bar{Y}^{\,c}_{s(g), F_g - 1 + \ell} - \bar{Y}^{\,c}_{s(g), F_g - 1} \big) \Big],$$

$$\text{for } O = \{closing, absorbing\},$$

where $\bar{Y}^{\,c}_{s(g),t}$ is the mean outcome among never-treated counties in county g's state $s(g)$ and N_ℓ is the number of treated counties observed ℓ years after treatment. The estimation is repeated separately by county type: first I compare closing to isolated; then absorbing to isolated. I impose some restrictions on the comparison group: only never-treated counties serve as controls, so already-absorbed/closed and not-yet-absorbed/closed counties never enter a comparison. This restriction ensures the comparison group remains the save over time. Second, comparisons are made within state, which is equivalent to allowing fully flexible state-specific time trends. I report $\ell = 1, \ldots, 4$ and the average of those four effects (the ATT). Placebo estimates for $\ell = 1, \ldots, 4$ years before treatment apply the same comparison to pre-treatment periods. Standard errors are clustered by county, and confidence intervals are 90 percent.

Synthetic difference-in-differences within state.

The second estimator is synthetic difference-in-differences (Arkhangelsky et al., 2021), in its event-study form for staggered adoption. SDID re-weights the never-treated counties so that their weighted average tracks the treated counties' pre-treatment path (unit weights), and re-weights

the pre-treatment years so that they resemble the post-treatment years for the controls (time weights). It then takes a weighted difference-in-differences. SDID is useful for outcomes that may have different pre-trends in the raw data, such as farm-related businesses per acre. To keep every comparison within state, as the DiD does, I estimate SDID separately in each state, with treated counties compared only to never-treated counties in the same state. Inference within a state uses a bootstrap that resamples counties (50 draws).

The state estimates are aggregated to a national estimate. The effect at event time ℓ weights each state by its number of treated counties observed at ℓ, and the ATT weights each state by its treated county-years. Because states are estimated on disjoint samples, the aggregate standard error is $\left(\sum_s w_s^2 \, \mathrm{se}_s^2\right)^{1/2}$, with weights normalized to sum to one. SDID requires a balanced panel with no missing outcomes and at least one pre-treatment period for every treated county. It also requires both treated and control units. I therefore use only states with at least two treated and two never-treated counties after these restrictions: 35 of the 42 states. The seven excluded states each have fewer than two treated counties.

B. Identifying assumptions

Both estimators identify the effect of closure/absorption on treated counties under a no-anticipation assumption: outcomes do not respond before the closure year. Hospitals often go through a period of financial distress before closing, so this assumption is not automatic. As shown in the raw trends graphs presented earlier, all three county types had similar trends on a variety of economic indicators, However, the placebo estimates test it directly. The DiD additionally assumes *parallel trends within state*: absent the closure, the outcomes of treated counties would have evolved like those of never-treated counties in the same state. Allowing arbitrary state-specific trends means that regional shocks, such as state Medicaid expansions, state-level hospital finance, or regional economic cycles, do not threaten identification. What remains is county-specific divergence within a state that coincides with closure timing.

SDID replaces parallel trends with a weaker condition. The treated counties' untreated outcomes need only be parallel to a *weighted* combination of in-state never-treated counties, with weights chosen to match the treated counties' pre-treatment trajectory. This is consistent with an interactive

fixed-effects model in which counties load differently on common shocks. SDID is therefore more robust when treated counties were on a different path before the closure, provided that difference is visible in the pre-period. Finally, both estimators require that control counties are not themselves affected by the closure (the stable unit treatment value assumption). This assumption could be violated if I did not separate out the absorbing counties, who are "indirectly" treated by absorbing the patients from the failing counties.

C. Cohorts and event-time contribution

Hospital closures occur between 2010 and 2019, meaning that counties enter treatment status in each year as well. Shown in Figure 7, the smallest increases in treatment coverage occur in years 2017, 2018, and 2019, while earlier cohort sizes were much larger. I only observe outcomes until 2019, partially due to not wanting to include data from Covid-19 years. As a result, the length at which I can observe the treatment effect is capped by each cohort's year of treatment relative to 2019: The 2019 cohort I observe only once as "treated" (in 2019), the 2018 cohort I observe twice as treated (2018 = event-time 1, 2019 = event-time 2), and so on.

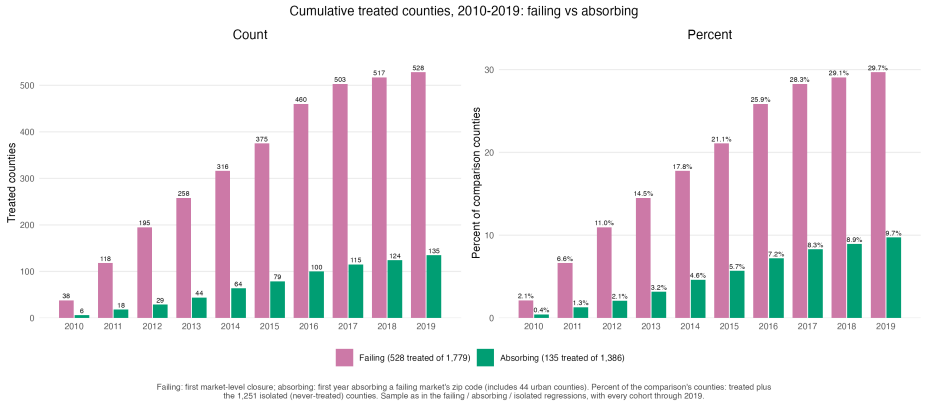


Figure 7. Count and share of closure cohorts across time

This staggered adoption means that the sample size being used to estimate event-time treatment effects declines in event-time. As shown in Figure 8, in event-time 1, I observe 100% of closing (absorbing) counties, declining to 7.2% (4.4%) in event-time 10 (which is composed of just the first cohort, treated in 2010). As treatment effects could be heterogeneous with respect to time, I limit my sample to cohorts 2010-2016, reducing the sample to 503 of the 529 closing counties (95%),

and 100 of the 135 absorbing counties (74%). With this restriction. I observe all cohorts for four event-time years post treatment, which eliminate changes in the treated composition from driving results (or lack there-of). As a robustness check, I estimate treatment effects for cohorts 2017-2019, located in Appendix D.

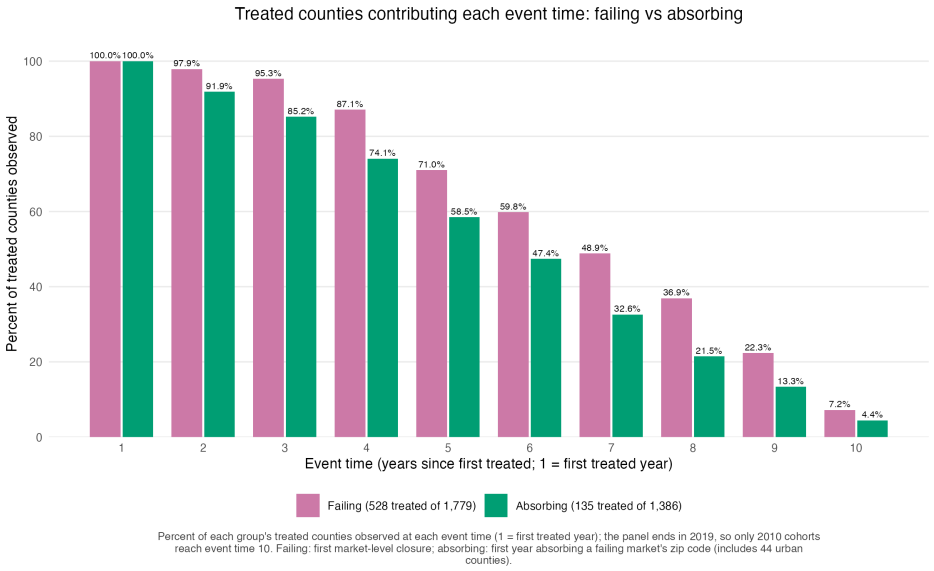


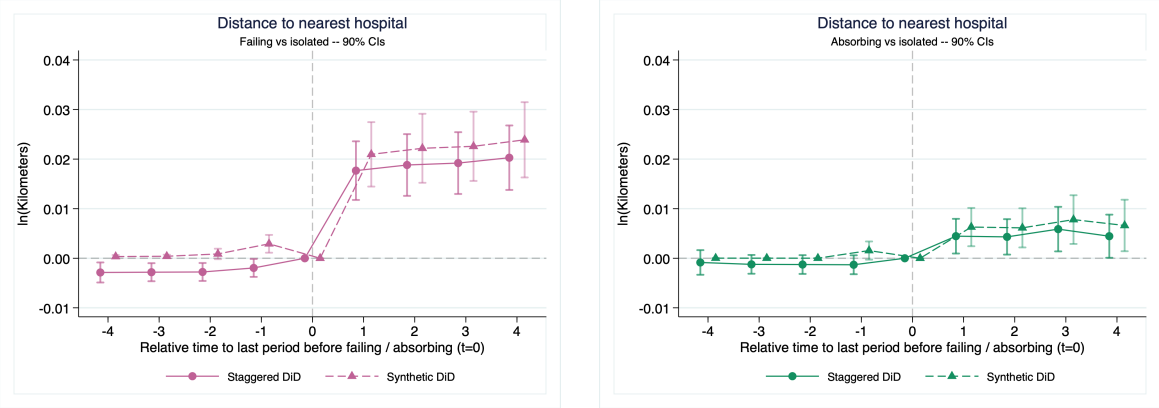
Figure 8. Event time years and treated sample

V. Results

A. Access to care

I first establish that hospital closures reduce access to care. As shown in Panel 9a of Figure 9 and Table B1, the average distance from a failing county's ZIP codes to the nearest open hospital rises by 1.9 percent under DiD and 2.3 percent under SDID (both significant at 1 percent). The effect is immediate and stable: it is 1.8 to 2.1 percent in every post-closure year under DiD. The DiD placebo estimates are statistically significant but an order of magnitude smaller (-0.2 to -0.3 percent). Distance rises much less, by 0.5 to 0.7 percent, in absorbing counties (Panel 9b, Table B2). This increase comes from the parts of absorbing counties that lie in a surviving HSA but were closer to the hospital that closed. The raw data in Panel **??** mirror these estimates: failing counties move from roughly 20 to 20.5 kilometers from their nearest hospital, absorbing counties

from roughly 14 to 14.07 kilometers, and isolated counties do not move. The average increase is small because most ZIP codes in a failing HSA were not closest to the hospital that closed. For the ZIP codes that were, the nearest hospital moves to their second or third nearest (Figure 1a).



(a) Closure to isolated

(b) Absorbing to isolated

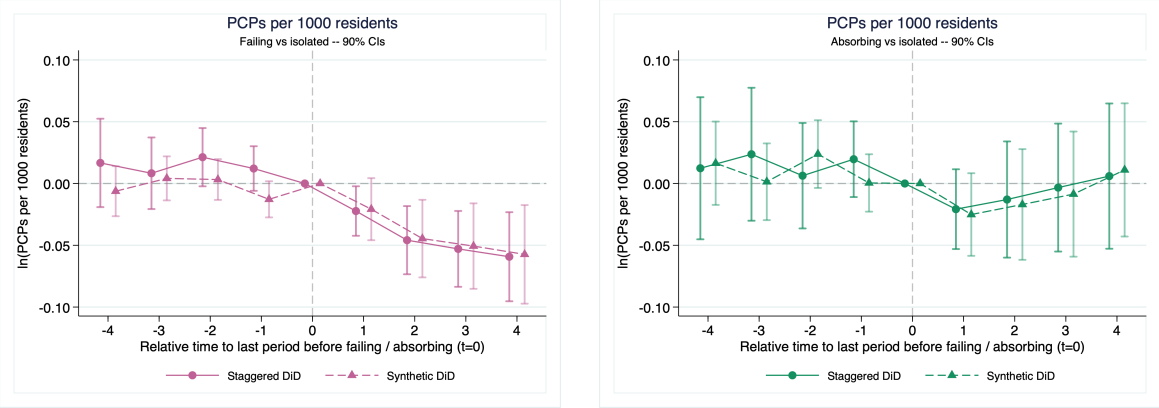
Figure 9. Distance to nearest hospital

It is not just distance that changes, but also the supply of primary care As shown in Table B1 and Panel 10a, primary care physicians per 1,000 residents fall by 4.5 percent (DiD) to 5.3 percent (SDID) in failing counties, both significant at 1 percent. The loss accumulates: it is about 2 percent in the first year after closure and grows to about 6 percent by the fourth. None of the eight placebo estimates are statistically significant. This difference is not mechanical: many primary care doctors operate outside of hospitals, and, the hospital itself is located in only one of the counties in the hospital service area. The fact that I find declines in primary care doctors **across** the HSA after its hospital closes is evidence of the role hospitals play in providing primary care and serving as an aggregator of health care in the HSA. Absorbing counties, by contrast, do not lose physicians: their ATTs are -0.8 and 0.05 percent and insignificant (Panel 10b, Table B2). This finding suggests that PCPs who leave failing counties do not relocate in absorbing counties.

Failing counties start with about 0.26 primary care physicians per 1,000 residents. A 5 percent decline is therefore about 0.013 physicians per 1,000 residents, or 1.3 physicians per 100,000. For a treated county of typical size (about 25,000 residents), that is roughly one physician lost for every three counties. The loss is small in absolute terms but large relative to the thin supply of these

counties. Primary care is particularly important for long-term health, because it gives doctors the chance to diagnose and treat chronic conditions before they become acute.

Closures change more than distance and physician supply. They also change which hospital is nearest, and how many patients each remaining hospital must serve. I return to these changes in Section V.C, where I use the contrast between failing and absorbing counties to separate the role of distance from the role of patient pressure.



(a) Closure to isolated

(b) Absorbing to isolated

Figure 10. Primary care provider to population (in thousands) ratio

B. Hospital closures and agriculture

As discussed earlier, farmers may be especially exposed to a loss of local care. I therefore look first at the size of the farm sector (establishments, employment and output), and then at the health of farmers and of the rural population around them. Throughout, the comparisons are failing versus isolated and absorbing versus isolated counties, as in Section V.A.

Agricultural production

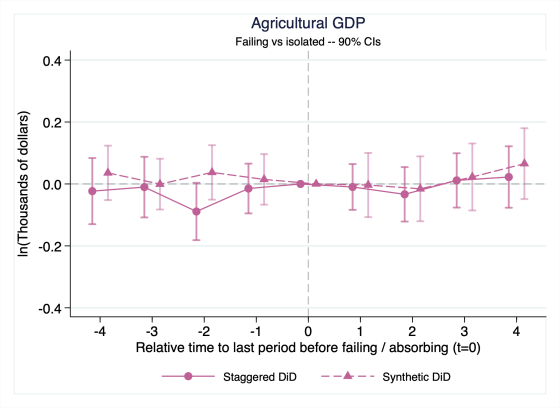
Closures reduce the number of farm businesses but not agricultural output. Table B3 reports the ATTs for failing counties. Under SDID, the growth rate in agricultural establishments in the Quarterly Census of Employment and Wages falls by 4.0 percent (significant at 10 percent, Figure 13). As with physicians, the decline builds over time: from 1.3 percent in the first year after closure to 4.9 percent in the fourth. The pre-closure estimates are small and insignificant.

The DiD estimate has the same sign and shape but is smaller: −1.0 percent on average and −2.4 percent by the fourth year, neither significant. Agricultural employment falls by 2 to 3 percent, but the estimates are imprecise and rest on the 126 failing counties whose employment cells are not suppressed (Figure 14).

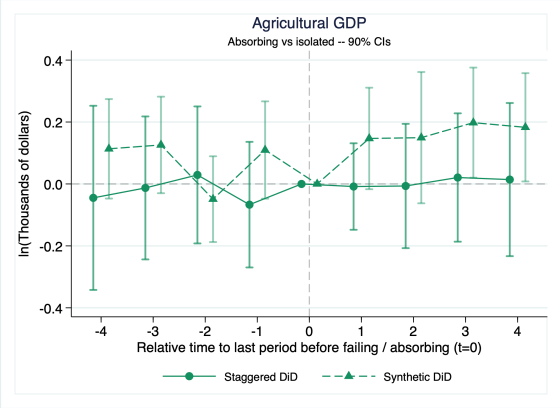
The change in the growth rate of farm businesses does not show up in county agricultural output. Agricultural GDP changes by −0.3 percent under DiD and +4.9 percent under SDID, neither significant (Figure 11). The DiD's 90 percent confidence interval rules out declines larger than about 7 percent. Farm income behaves the same way (Figure 12). The share of counties with positive farm owner income changes by −2.1 and +0.2 percentage points, and log farm owner income among counties with positive owner income by +0.6 and −2.3 percent, all insignificant. Agricultural wages per establishment and per employee, and farm proprietor income, do not change significantly either. Fewer farms with unchanged output suggests that the new farms opening in the isolated counties are small and that their land stays in production through neighboring operations, as the high rate of multi-operator farms would lead us to expect (Sherrick, 2017).

Absorbing counties show no change in the farm sector (Table B4). Establishments, employment, farm income and farm proprietor income all have small and insignificant ATTs. The one exception, a 17 percent increase in agricultural GDP under SDID (significant at 10 percent), rests on 51 counties in 14 states. Its placebo estimates are as large as the post-treatment effects (+11 to +13 percent in three of the four pre-periods), so I do not read it as an effect of absorbing.

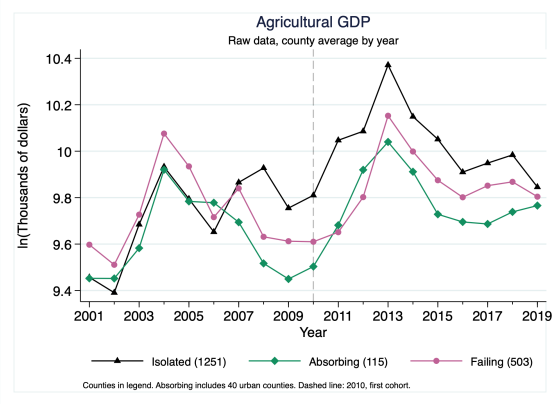
The broader local economy is largely unaffected (Tables B5 and B6). In failing counties, total GDP changes by +0.2 percent under DiD and −1.9 percent under SDID (significant at 10 percent only under SDID), and non-farm income does not change. The exception is non-farm proprietor income, which falls by 3.1 to 4.1 percent (significant at 1 percent under both estimators). Like farmers, the self-employed carry their own health costs and cannot easily replace their own labor, so this is consistent with the farm results. Absorbing counties show no change in any of these outcomes. Taken as a whole, the results suggest while hospital closures may change where new farms and farm businesses open, they do not change the total amount of agricultural goods and services produced, nor the earnings of the farmers operating the farms.



(a) Closure to isolated



(b) Absorbing to isolated

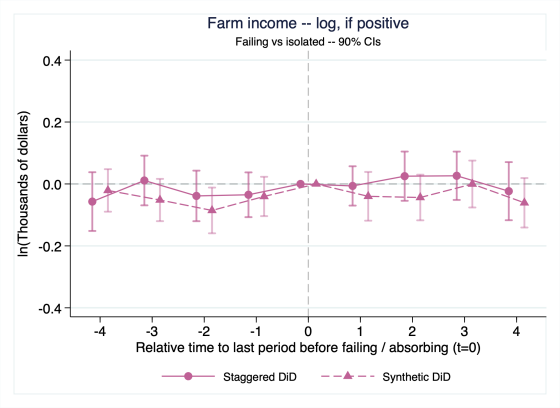


(c) Raw trends

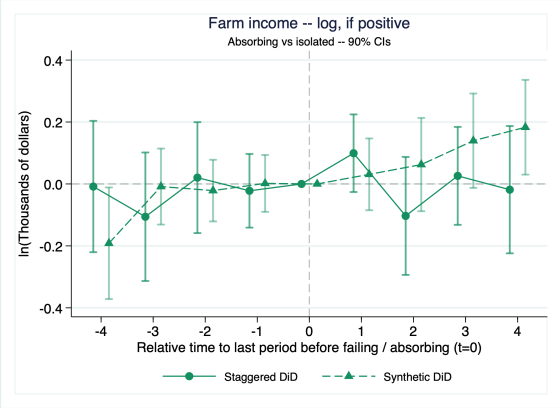
Figure 11. GDP produced in agricultural sector

Farmer and rural health

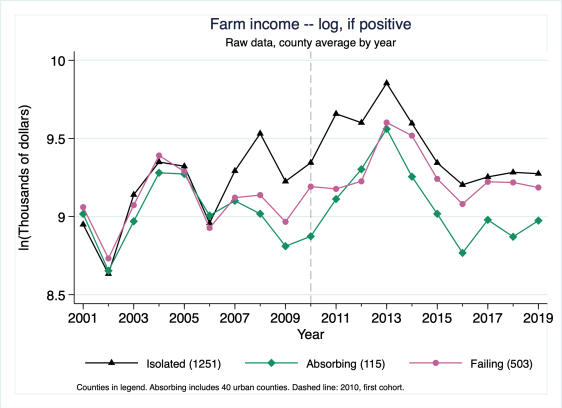
Despite little to no evidence for the impact of hospital closures on farm production, there is at least some evidence that they change farmer health. All-cause mortality rises in failing counties: Log deaths per 100,000 residents increase by 1.1 percent under DiD (significant at 5 percent) and 1.5 percent under SDID (significant at 1 percent; Table B7, Figure 15). The increase appears with a delay: it is under half a percent in the first year after closure, about 1 percent in the second, and peaks at about 2.4 percent in the third, matching the gradual loss of physicians. The placebo estimates are small and insignificant under both estimators. By age, the clearest increases are among adults aged 55 to 64 and 65 to 74, whose log(deaths per 100,000 + 1) rise by 28 and 15 percent under SDID (both significant at 1 percent). These age-specific estimates are fragile,



(a) Closure to isolated



(b) Absorbing to isolated

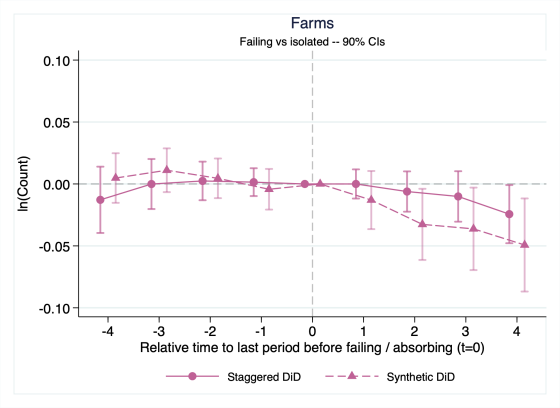


(c) Raw trends

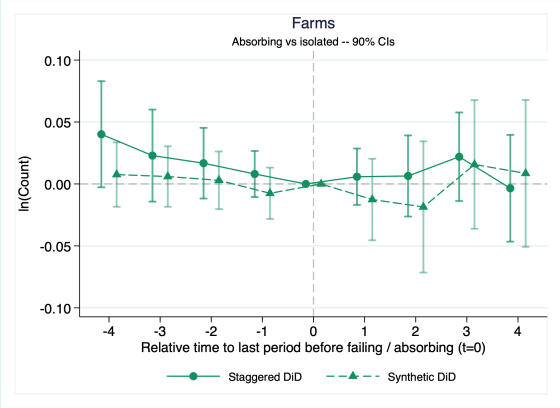
Figure 12. Farm owner income

however. The DiD estimates for the same ages are negative and insignificant. Many county-age cells have no deaths in some years, so changes in the transformed measure largely reflect cells moving between zero and positive deaths. The age-specific estimates in logs, which drop zero-death cells, are mostly small and insignificant. The exceptions are a 2.1 percent increase among those aged 85 and over (DiD only) and a large but imprecise estimate for ages 35 to 44 that rests on 14 counties. Death records do not report occupation, so I cannot attribute the mortality increase to farmers. The ages involved, however, overlap with those of the typical farm operator. In contrast, absorbing counties do not experience higher mortality (Table B8).

Farmers themselves report worse health. Farmers in failing counties become 9.5 percentage points more likely to report good, fair or poor health rather than very good or excellent health (DiD, significant at 10 percent). Farmers are also 8.3 percentage points more likely to say their health

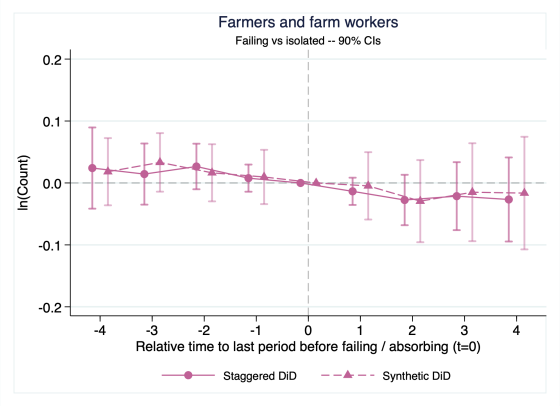


(a) Closure to isolated

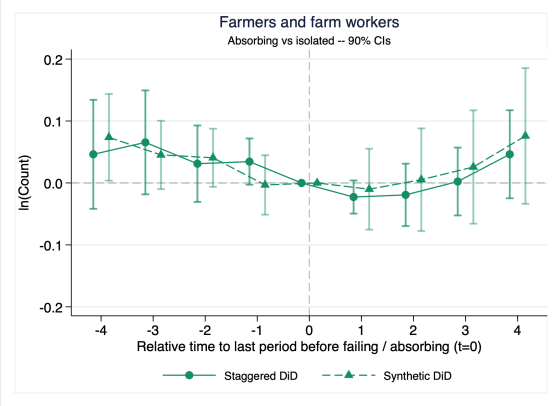


(b) Absorbing to isolated

Figure 13. Number of farms and farm businesses



(a) Closure to isolated



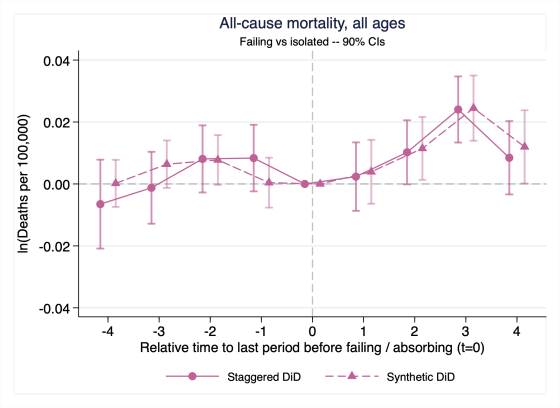
(b) Absorbing to isolated

Figure 14. Number of farm and farm business employees

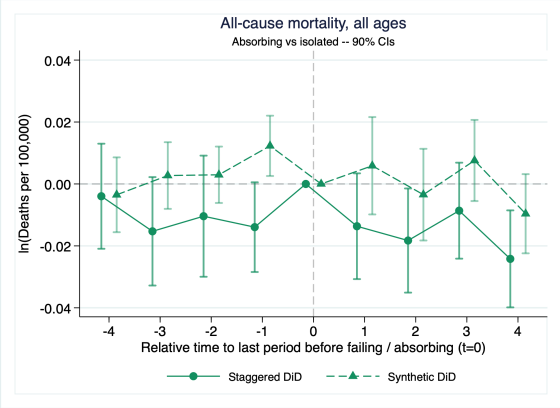
has worsened since their previous interview, although this estimate is imprecise. The deterioration is specific to farmers: the corresponding estimates for all workers and for non-farmers are 0 to 2 percentage points, and only one of them is significant (a 2.0-point increase in worsened health for all workers). Farmers do not reduce employment, hours or labor force participation (Table B11).

C. Mechanisms: Competition and distance

The failing and absorbing groups let me separate two channels through which a closure can reduce care. Failing counties experience two shocks: their patients travel further, and they join surviving HSAs that now serve more patients. Absorbing counties experience mainly the second: their own



(a) Closure to isolated



(b) Absorbing to isolated

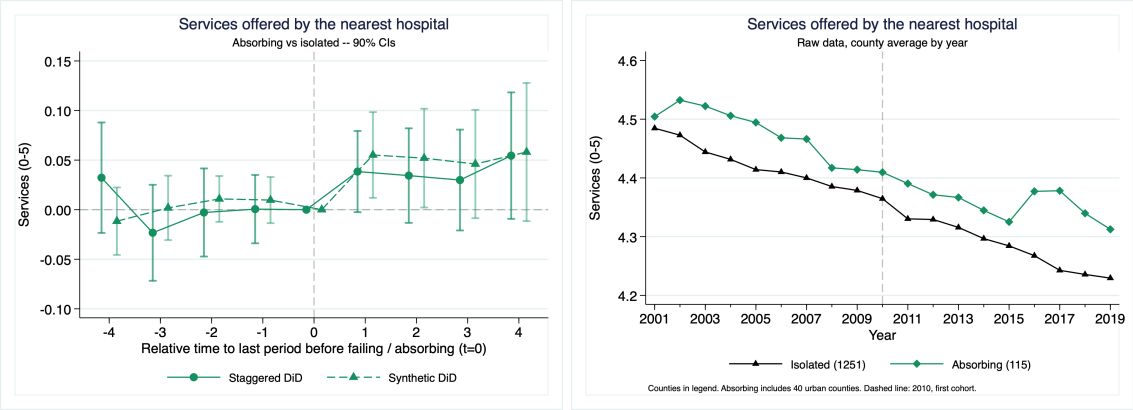
Figure 15. All-cause, all-age mortality rate

hospital survives and distance barely changes, but more patients compete for its beds and staff. If the effects in failing counties came from congestion at the surviving hospitals, absorbing counties should show them too.

In short, they do not. Patient pressure rise more in absorbing counties than in failing counties (Figure 19 and Table B16). This means surviving hospitals face more patients per available bed. In absorbing counties, the population of the HSA rises by about 8 percent and the HSA's population per hospital bed by 18 percent under both estimators, with flat pre-trends. Yet absorbing counties lose no physicians, no farm businesses, and see no rise in mortality. The losses in failing counties therefore appear to come from the loss of local care and the added distance to the next hospital, not from more patients per bed in the hospitals that remain. These findings suggest that patients have preferences for their local hospital or doctor; when they lose access to thier preferred provider, they do not seek out new care. In fact, the construction of the HSA itself reveals patient preferences for hospitals that are *lower quality* and offer *fewer services*. There was nothing preventing the patients from attending these better hospitals *prior* to the nearer hospital closing. In light of this preference, perhaps it is unsurprising that they do not substitute towards the better hospitals after the nearest closes.

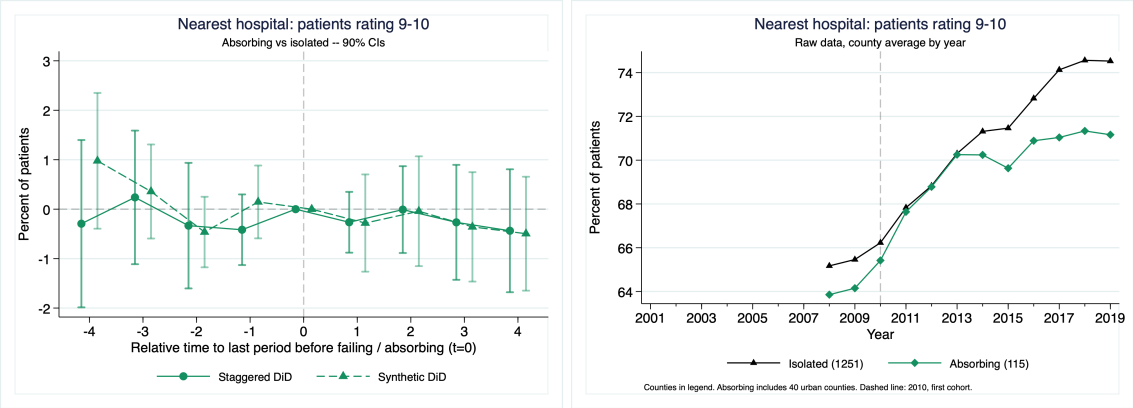
Despite increases in the patient pool & patient pressure, absorbing hospitals do not see increases in utilization, as shown in Figure 20 (and Tables B15 and B16). Hospitals in absorbing counties have higher rates of occupancy, measured by the average share of beds utilized per-day, shown in

Panel 20b. However, as shown in Panel 20a, absorbing counties do not experience large increases in occupancy rates. There might be an increase of 1 percentage point, significant at the 5% level, in the immediate year post hospital closure. This effect drifts back towards zero starting in period 2. This suggests that the patients who lived in failing counties do not actually switch hospitals–that total utilization falls, rather than the now-hospital-less patients of failing counties substituting to a surviving hospital. This effect explains why mortality increases in failing counties, but not in absorbing: the difference is driven by those in failing counties avoiding care, rather than facing increased competition for care in the surviving hospitals.



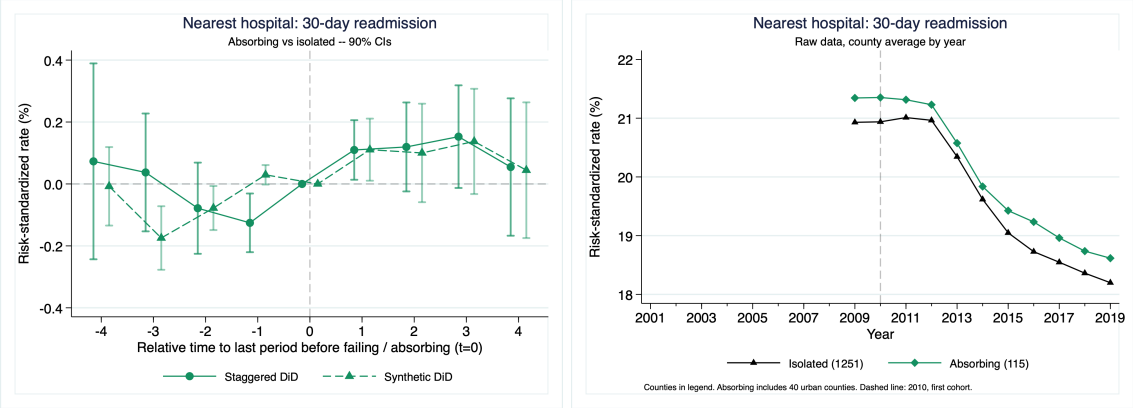
(a) Absorbing to isolated (b) Raw trends: Number of services

Figure 16. Access to services



(a) Absorbing to isolated (b) Raw trends

Figure 17. Patients reporting highest satisfaction



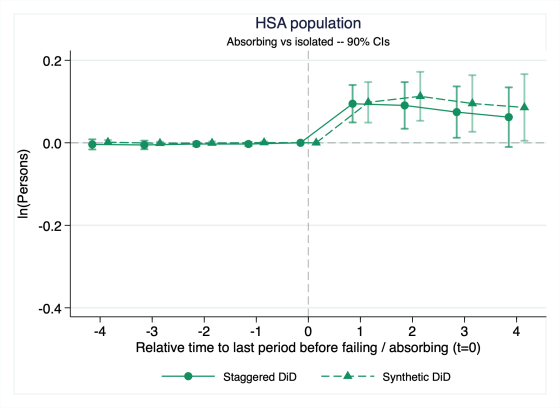
(a) Absorbing to isolated (b) Raw trends

Figure 18. Hospital level readmission rate

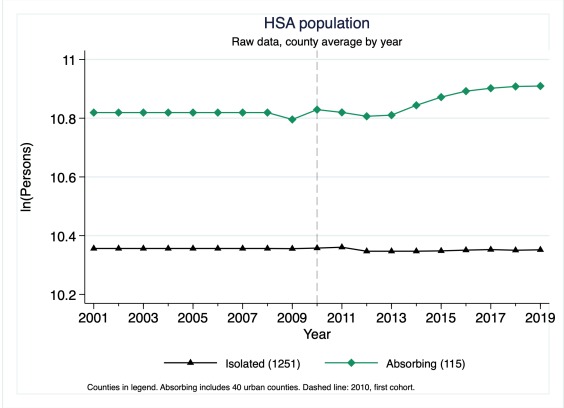
D. Comparison to earlier papers

In contrast to earlier papers, the unit of treatment I evaluate is the market: a county is treated once a hospital in its Dartmouth hospital service area closes. Most of these counties did not lose a hospital themselves. Tables 1 and 2 check the results against two alternatives utilized by earlier papers in the hospital closure literature. The first is direct treatment, where a hospital closed in one of the county's own ZIP codes (129 treated counties). This estimation strategy ignores the spatial spillovers between counties in the same hospital service area, as well as the presence of "absorbing counties" that border the market-treated counties. Every directly treated county is also market-treated, and never earlier. The second is a simple two-way fixed effects (TWFE) regression on ever treated $\times$ post-2010. This estimation strategy ignores the heterogeneity of treatment effects with respect to time, and means that observed effects could be driven by the negative weights issue. Each panel reports the average pre-closure and post-closure coefficient and joint tests that all pre- or all post-closure coefficients are zero.

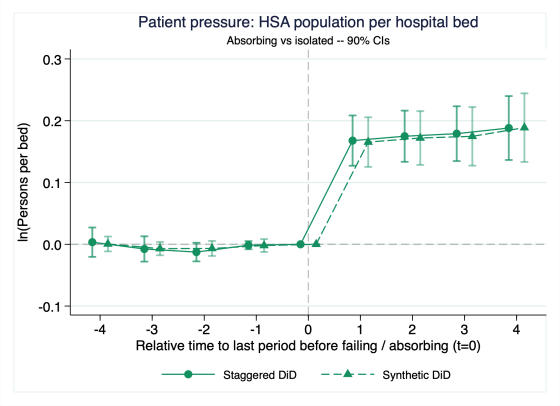
The results show why earlier papers conflicted with each other. The first stage is much stronger under direct treatment. Distance to the nearest hospital rises by 1.32 km in the DiD, more than three times the market-level estimate of 0.39 km, and by 0.89 km in the TWFE. PCPs per 1,000 residents fall by 0.025, against 0.017 for market treatment. The direct-treatment placebos are clean for both outcomes (joint p between 0.27 and 0.72). The market-level DiD placebos for distance are



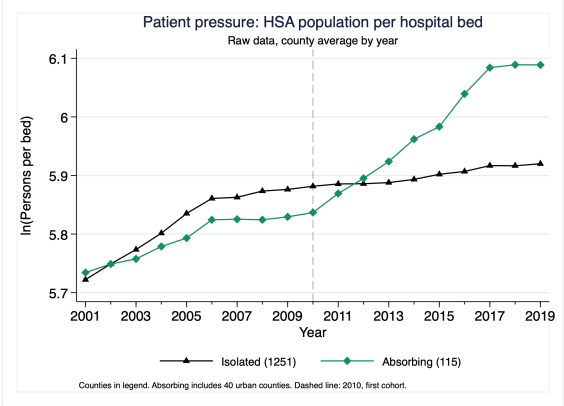
(a) Hospital service area population



(b) Raw trends: HSA population



(c) Pressure: HSA pop per bed

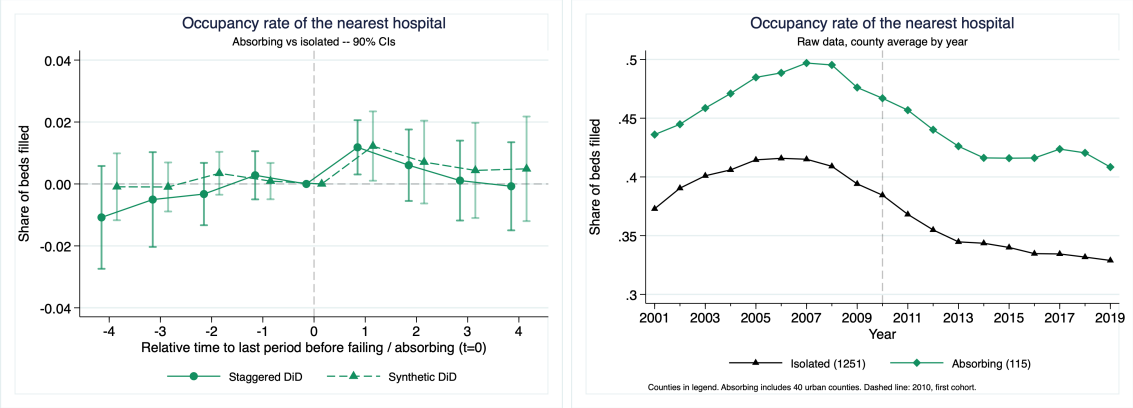


(d) Raw trends: HSA pop per bed

Figure 19. Patient population and pressure

small (−0.06 km on average) but jointly reject. With direct treatment, the PCP shortage effect is significant only in the TWFE (6.4 percentage points); the DiD estimate is 3.1 points and not significant.

Indirect-only counties (388 counties that are market-treated but never directly treated) are never-switchers under direct treatment, so they enter the control group even though their market lost a hospital. Dropping them (rows 2b and 4) leaves the first stage essentially unchanged: distance moves from 1.32 to 1.33 km and PCPs per 1,000 from −0.025 to −0.026. Contaminated controls do not drive the direct-treatment results, and the GDP estimates barely move either. No GDP outcome has a significant average effect in any DiD specification. Under direct treatment the joint tests on the four post effects reject for total GDP per capita ($p = 0.01$) and health care GDP per capita ($p = 0.03$). The averages are small, though, and of opposite signs: +0.7 and −0.07 thousand



(a) Absorbing vs isolated (b) Raw trends

Figure 20. Occupancy rate

dollars.

TWFE, by contrast, finds declines in total GDP per capita (-3.4 thousand dollars with direct treatment, significant at 1 percent; -1.8 with market treatment, significant at 10 percent) and in agricultural GDP per capita (-1.0 and -0.6, both significant at 5 percent or better). Both outcomes fail the TWFE pre-trend test in all three TWFE specifications (joint $p \leq 0.002$), with positive average pre-closure coefficients. These TWFE estimates therefore reflect differential trends between ever-treated and never-treated counties rather than effects of closure. The DiD compares within states, using never-treated counties as controls, and finds none of them. The TWFE decline in health care GDP per capita with market treatment (-0.15, $p < 0.01$) passes the joint pre-trend test but is not matched by any DiD.

Taken together, these results provide additional evidence why the definition of "treated" matters. Hospital service areas are constructed by observing where patients seek care. The fact that they span counties suggests that the impact of hospital closures *also* spans counties. Ignoring this spatial spillover would violate the SUTVA assumption. My second treatment status, the absorbing counties, would also violate SUTVA if they were included in the never-treated group. As they border the failing counties, they *could* absorb patients from the failed hospitals. I show earlier that in fact, they do not appear to absorb patients.

Table 1—Access to care under alternative treatment definitions and TWFE

	Pre-closure		Post-closure		
	Average	Joint p	Average	Joint p	Treated / control
Distance to nearest hospital (km)					
Main DiD, market treatment	-0.06^{*}	<0.001	0.39^{***}	<0.001	502 / 1,340
	(0.03)		(0.08)		
(1) TWFE, ever market-treated	−0.03	0.494	0.30^{***}	–	502 / 1,340
	(0.03)		(0.07)		
(2) TWFE, ever directly treated	−0.10	0.266	0.89^{***}	–	129 / 1,728
	(0.09)		(0.22)		
(2b) = (2), no indirect-only counties	−0.10	0.357	0.91^{***}	–	129 / 1,340
	(0.09)		(0.22)		
(3) DiD, direct treatment	−0.10	0.589	1.32^{***}	<0.001	129 / 1,728
	(0.11)		(0.26)		
(4) = (3), no indirect-only counties	−0.12	0.716	1.33^{***}	<0.001	129 / 1,340
	(0.11)		(0.26)		
PCPs per 1,000 residents					
Main DiD, market treatment	0.0031	0.867	-0.0165^{***}	0.004	500 / 1,332
	(0.0046)		(0.0050)		
(1) TWFE, ever market-treated	−0.0036	0.005	-0.0126^{**}	–	500 / 1,332
	(0.0066)		(0.0063)		
(2) TWFE, ever directly treated	0.0098	0.404	-0.0284^{**}	–	129 / 1,718
	(0.0102)		(0.0114)		
(2b) = (2), no indirect-only counties	0.0081	0.374	-0.0294^{**}	–	129 / 1,332
	(0.0103)		(0.0116)		
(3) DiD, direct treatment	0.0044	0.672	-0.0250^{***}	0.007	129 / 1,718
	(0.0075)		(0.0073)		
(4) = (3), no indirect-only counties	0.0069	0.589	-0.0264^{***}	0.004	129 / 1,332
	(0.0076)		(0.0074)		
PCP shortage (share)					
Main DiD, market treatment	−0.0136	0.840	0.0260^{*}	0.210	500 / 1,332
	(0.0154)		(0.0146)		
(1) TWFE, ever market-treated	0.0085	0.030	0.0307^{**}	–	500 / 1,332
	(0.0182)		(0.0155)		
(2) TWFE, ever directly treated	0.0214	0.549	0.0638^{**}	–	129 / 1,718
	(0.0326)		(0.0284)		
(2b) = (2), no indirect-only counties	0.0220	0.650	0.0670^{**}	–	129 / 1,332
	(0.0329)		(0.0286)		
(3) DiD, direct treatment	−0.0353	0.148	0.0306	0.474	129 / 1,718
	(0.0285)		(0.0276)		
(4) = (3), no indirect-only counties	−0.0378	0.104	0.0342	0.385	129 / 1,332
	(0.0283)		(0.0280)		

Source: 46_PaperCheck.do. Main DiD: 41's `did_multiplegt_dyn` on market treatment. (1)–(2b): county and year fixed effects, ever treated × post (year > 2010), standard errors clustered by county; the pre-closure average is from an event-study version of the same regression (ever treated × year, base year 2010) and averages the 2001–2009 coefficients, and the post-closure column is the ever treated × post coefficient. (3)–(4): 41's DiD with direct treatment (a hospital closed in one of the county's own ZIP codes) in place of market treatment; averages are the simple means of the four placebos and of the four effects. Indirect-only counties: market-treated but never directly treated (388). Joint p: all pre (post) coefficients zero; for TWFE some years have identical outcomes, and redundant restrictions are dropped; there is no joint post test for TWFE, whose post column is a single coefficient. Stars refer to the average. Treated / control: counties with the outcome observed; each specification applies 41's 2017 cohort cap to its own treatment variable.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table 2—GDP under alternative treatment definitions and TWFE

	Pre-closure		Post-closure		
	Average	Joint p	Average	Joint p	Treated / control
GDP per capita ($000)					
Main DiD, market treatment	−0.79	0.702	0.49	0.261	500 / 1,332
	(0.83)		(0.47)		
(1) TWFE, ever market-treated	0.36	0.002	−1.77*	–	500 / 1,332
	(0.60)		(1.00)		
(2) TWFE, ever directly treated	1.34	<0.001	−3.38***	–	129 / 1,718
	(1.31)		(1.01)		
(2b) = (2), no indirect-only counties	1.38	<0.001	−3.66***	–	129 / 1,332
	(1.31)		(1.02)		
(3) DiD, direct treatment	1.45	0.080	0.73	0.012	129 / 1,718
	(1.06)		(0.49)		
(4) = (3), no indirect-only counties	1.03	0.129	0.69	0.015	129 / 1,332
	(1.08)		(0.53)		
Health care GDP per capita ($000)					
Main DiD, market treatment	0.026	0.624	−0.026	0.752	134 / 365
	(0.022)		(0.023)		
(1) TWFE, ever market-treated	0.072**	0.560	−0.146***	–	134 / 365
	(0.032)		(0.052)		
(2) TWFE, ever directly treated	−0.026	0.810	−0.080	–	33 / 467
	(0.060)		(0.096)		
(2b) = (2), no indirect-only counties	−0.005	0.755	−0.115	–	33 / 365
	(0.061)		(0.097)		
(3) DiD, direct treatment	0.052	0.519	−0.073	0.026	33 / 467
	(0.039)		(0.059)		
(4) = (3), no indirect-only counties	0.056	0.673	−0.075	0.025	33 / 365
	(0.042)		(0.061)		
Agricultural GDP per capita ($000)					
Main DiD, market treatment	0.14	0.729	−0.06	0.070	254 / 565
	(0.17)		(0.14)		
(1) TWFE, ever market-treated	0.25	0.002	−0.64**	–	254 / 565
	(0.20)		(0.26)		
(2) TWFE, ever directly treated	0.39*	<0.001	−1.05***	–	74 / 750
	(0.21)		(0.28)		
(2b) = (2), no indirect-only counties	0.45**	<0.001	−1.15***	–	74 / 565
	(0.22)		(0.29)		
(3) DiD, direct treatment	0.31*	0.207	0.26	0.277	74 / 750
	(0.17)		(0.18)		
(4) = (3), no indirect-only counties	0.29	0.163	0.25	0.454	74 / 565
	(0.18)		(0.17)		

Source: 46_PaperCheck.do. Main DiD: 41's `did_multiplegt_dyn` on market treatment. (1)–(2b): county and year fixed effects, ever treated × post (year > 2010), standard errors clustered by county; the pre-closure average is from an event-study version of the same regression (ever treated × year, base year 2010) and averages the 2001–2009 coefficients, and the post-closure column is the ever treated × post coefficient. (3)–(4): 41's DiD with direct treatment (a hospital closed in one of the county's own ZIP codes) in place of market treatment; averages are the simple means of the four placebos and of the four effects. Indirect-only counties: market-treated but never directly treated (388). Joint p: all pre (post) coefficients zero; for TWFE some years have identical outcomes, and redundant restrictions are dropped; there is no joint post test for TWFE, whose post column is a single coefficient. Stars refer to the average. Treated / control: counties with the outcome observed; each specification applies 41's 2017 cohort cap to its own treatment variable. GDP outcomes use only counties never missing the outcome in a published year between 2005 and 2019, and are top-coded at the 99th percentile of county-years.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

VI. Conclusion

This paper studies what happens to farmers and to the farm economy when a rural hospital closes. Defining treatment at the level of the hospital's market, separating the counties that lose a hospital from the neighboring counties that absorb its patients, and comparing each group with isolated counties in the same state, I find that closures reduce primary care physicians per resident in failing counties by about 5 percent, with losses that grow over the years after closure. Three consequences follow. The number of agricultural establishments falls by about 4 percent. Farmers become 9.5 percentage points more likely to report good, fair, or poor health rather than very good or excellent health, an effect that grows over time and does not appear among non-farmers. And all-cause mortality rises by 1.1 to 1.5 percent, with the clearest, though fragile, increases among adults aged 55 to 74. Death records do not identify occupation, so I cannot attribute the increase in mortality to farmers. It shows that the loss of care has real health consequences in the communities where farmers live, and its age profile overlaps that of farm operators. Absorbing counties, which could take in the closed hospital's patients but keep their own hospital, see a larger rise in patients per bed and none of these losses. The harm therefore comes from distance and the loss of local care rather than from congestion at the hospitals that remain.

These losses do not, within the study period, reduce county agricultural output. Agricultural GDP, farm income, and agricultural wages are unchanged, and the confidence intervals rule out large declines. Fewer farms combined with stable output suggests that the land and capital of exiting operations remain in production, so that the costs of closures for agriculture fall on farm households rather than on aggregate output.

Closures also do not drive broader rural decline. Total GDP and non-farm income in treated counties are unchanged, although non-farm proprietor income falls by 3 to 4 percent, consistent with self-employed workers, like farmers, being especially exposed to the loss of local care. Preserving rural hospitals by itself is therefore unlikely to slow rural decline, but the case for preserving them rests on their effects on health, which this paper finds are substantial.

Finally, the findings of this paper suggest several directions for future research. Firstly, it would be important to determine which aspects of either farmers as a population, or farming as a profession, drive the differences in health observed in farmers. I show that farmers are less engaged with the

health care system as a whole even *prior* to a hospital closing (specifically, they are less insured, and less likely to have been to a medical appointment than non-farmers). It is possible that this lack of engagement with medical services is reflective of farmer believes or assumptions about the return to medical services. In future work I plan to measure these beliefs and assumptions, and if they are inaccurate, test if informing farmers of the return to medical services can increase farmer take up of medical services. I also plan to explore what aspects of the profession—such as exposure to climate risk, the delay between the purchase of inputs and sale of outputs—contribute to low-baseline health of farmers.

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Table A1—Summary statistics: Demographics

Metric	Non-farmer Mean (SD) (1)	Farmer Mean (SD) (2)	Difference Coeff(SE) (3)
Age 18-49	0.577	0.566	0.012
	(0.494)	(0.496)	(0.008)
Age 50+	0.423	0.434	-0.012
	(0.494)	(0.496)	(0.008)
Male	0.446	0.709	0.262
	(0.497)	(0.454)	(0.008)***
All others	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Married	0.470	0.563	0.073
	(0.499)	(0.496)	(0.008)***
Widowed	0.100	0.091	-0.017
	(0.300)	(0.288)	(0.005)***
White	0.818	0.908	0.042
	(0.386)	(0.290)	(0.006)***
All other racial categories	0.182	0.092	-0.042
	(0.386)	(0.290)	(0.006)***
Hispanic	0.102	0.232	0.120
	(0.302)	(0.422)	(0.004)***

Appendix A: Summary statistics

Table A2—Summary statistics: Education, Employment, and Income

Metric	Non-farmer Mean (SD) (1)	Farmer Mean (SD) (2)	Difference Coeff(SE) (3)
Less than bachelor's degree	0.739	0.878	0.084
	(0.439)	(0.328)	(0.007)***
Bachelor's degree or higher	0.261	0.122	-0.084
	(0.439)	(0.328)	(0.007)***
Employed	0.622	0.770	0.171
	(0.485)	(0.421)	(0.008)***
Not employed	0.033	0.013	-0.017
	(0.178)	(0.114)	(0.003)***
Not in labor force	0.345	0.217	-0.154
	(0.476)	(0.412)	(0.008)***
Below poverty threshold	0.137	0.212	0.055
	(0.344)	(0.409)	(0.007)***
Family income less than 50k USD annually	0.556	0.716	0.091
	(0.497)	(0.451)	(0.017)***
Family income 50k USD or more annually	0.444	0.284	-0.091
	(0.497)	(0.451)	(0.017)***
Recieved welfare in calendar year	0.012	0.007	-0.007
	(0.110)	(0.081)	(0.002)***
Any government income program	0.069	0.063	-0.014
	(0.253)	(0.243)	(0.004)***

Table A3—Summary statistics: Health Care Access

Metric	Non-farmer Mean (SD) (1)	Farmer Mean (SD) (2)	Difference Coeff(SE) (3)
Has usual place for medical care	0.860	0.766	-0.093
	(0.347)	(0.424)	(0.006)***
Sick care treated at hospital	0.026	0.024	0.001
	(0.160)	(0.152)	(0.003)
Attends same place for routine and sick care	0.944	0.936	-0.008
	(0.229)	(0.245)	(0.005)*
Routine care treated at hospital	0.036	0.025	-0.006
	(0.186)	(0.155)	(0.006)
Delayed care due to any of above	0.185	0.161	-0.027
	(0.388)	(0.367)	(0.007)***
Needed but could not afford mental health care	0.025	0.016	-0.009
	(0.156)	(0.125)	(0.003)***
Has health insurance coverage from any source	0.850	0.721	-0.117
	(0.358)	(0.449)	(0.006)***

Table A4—Summary statistics: Disability

Metric	Non-farmer Mean (SD) (1)	Farmer Mean (SD) (2)	Difference Coeff(SE) (3)
Self-reported health status: very good or better	0.608	0.581	-0.002
	(0.488)	(0.494)	(0.008)
Self-reported health status: good or worse	0.392	0.419	0.002
	(0.488)	(0.494)	(0.008)
BMI class: overweight or obese	0.596	0.651	0.032
	(0.491)	(0.477)	(0.009)***
Bed disability days in last 12 mon: Any	0.365	0.273	-0.085
	(0.481)	(0.445)	(0.008)***
Work loss days in last 12 mon: Any	0.457	0.283	-0.164
	(0.498)	(0.451)	(0.010)***
Can not work due to health problem	0.084	0.064	-0.029
	(0.277)	(0.245)	(0.005)***

Table A5—Summary statistics: Mental Health

Metric	Non-farmer Mean (SD) (1)	Farmer Mean (SD) (2)	Difference Coeff(SE) (3)
Drinking status: Binge drinker	0.081	0.097	0.009
	(0.273)	(0.296)	(0.005)*
Drinking status: Light drinker	0.556	0.494	-0.033
	(0.497)	(0.500)	(0.008)***
Drinking status: Non-drinker	0.391	0.432	0.018
	(0.488)	(0.495)	(0.008)**

Table A6—Summary statistics: Mortality

Metric	Non-farmer Mean (SD) (1)	Farmer Mean (SD) (2)	Difference Coeff(SE) (3)
Respondent has died during surveying period	0.188	0.200	-0.009
	(0.391)	(0.400)	(0.007)
Diseases of the heart	0.257	0.273	0.013
	(0.437)	(0.446)	(0.017)
Cancers (all)	0.226	0.230	0.007
	(0.419)	(0.421)	(0.016)
Chronic lower respiratory diseases	0.062	0.058	-0.009
	(0.241)	(0.233)	(0.009)
Accidents (unintent. injuries)	0.036	0.058	0.022
	(0.186)	(0.233)	(0.007)***
Alzheimer's disease	0.037	0.046	0.010
	(0.189)	(0.210)	(0.007)
Diabetes	0.031	0.027	-0.005
	(0.173)	(0.163)	(0.007)
Kidney and related diseases	0.019	0.022	0.003
	(0.135)	(0.146)	(0.005)
All other causes	0.258	0.218	-0.036
	(0.437)	(0.413)	(0.017)**

Table A7—Hospital-level differences

Metric	Never-closed	Ever-closed	Difference
(1)	(2)	(3)	(4)
Hospital-level metrics			
Owned by proprietary	0.223	0.351	0.128
	(0.417)	(0.478)	[-4.632]***
Owned by non-profit	0.541	0.368	-0.173
	(0.498)	(0.483)	[5.295]***
Owned by federal government	0.010	0.004	-0.006
	(0.098)	(0.064)	[0.875]
Owned by state government	0.016	0.021	0.004
	(0.127)	(0.143)	[-0.502]
Owned by local government	0.086	0.054	-0.033
	(0.281)	(0.226)	[1.779]*
Owned by district government	0.066	0.062	-0.004
	(0.249)	(0.242)	[0.266]
Owned by any other organization	0.057	0.140	0.084
	(0.231)	(0.348)	[-5.360]***
Trauma level I	0.409	0.058	-0.351
	(0.492)	(0.234)	[11.036]***
Trauma level II	0.174	0.017	-0.158
	(0.379)	(0.128)	[6.450]***
Trauma level III	0.103	0.012	-0.091
	(0.304)	(0.111)	[4.633]***
Trauma level IV	0.189	0.041	-0.148
	(0.391)	(0.199)	[5.828]***
Trauma level V	0.003	0.004	0.001
	(0.058)	(0.064)	[-0.200]
Trauma level other	0.588	0.938	0.350
	(0.492)	(0.242)	[-10.994]***
Childrens hospital	0.000	0.000	0.000
	(0.000)	(0.000)	[.]
Critical access hospital	0.241	0.095	-0.146
	(0.427)	(0.294)	[5.237]***
General acute care hospital	0.759	0.905	0.146
	(0.427)	(0.294)	[-5.237]***
Longterm care hospital	0.000	0.000	0.000
	(0.000)	(0.000)	[.]
Military hospital	0.000	0.000	0.000
	(0.000)	(0.000)	[.]
Psychiatric hospital	0.000	0.000	0.000
	(0.000)	(0.000)	[.]
Rehabilitation hospital	0.000	0.000	0.000
	(0.000)	(0.000)	[.]
Special hospital	0.000	0.000	0.000
	(0.000)	(0.000)	[.]
Womens hospital	0.000	0.000	0.000
	(0.000)	(0.000)	[.]
Chronic disease hospital	0.000	0.000	0.000
	(0.000)	(0.000)	[.]
In a hospital system	0.010	0.037	0.027
	(0.098)	(0.190)	[-4.017]***
Hospital has helipad	0.703	0.430	-0.273
	(0.457)	(0.496)	[9.040]***
Number of beds at hospital	168.419	85.089	-83.330
	(204.978)	(103.577)	[5.914]***
Distance	14.418	9.033	-5.385
	(19.332)	(21.640)	[4.209]***

Notes: Table A7 compares medical facilities that close to those that do not close, between 2000—2019.

Table A8—County-level differences

Metric	(1) No hospital	(2) Has hopsital, never closed	(3) Has hospital, ever closed	(4) Diff. (2-1)	(5) Diff. (3-1)	(6) Diff. (3-2)
	Average county-level metrics, 2000-2010					
GDP from agriculture, million USD	620.251	108.755	96.938	-511.496	-523.313	-11.817
	(3362.011)	(337.916)	(280.383)	[40.973]***	[9.660]***	[2.137]**
GDP from all industries, million USD	45439.564	39193.680	47257.967	-6245.884	1818.404	8064.288
	(2.70e+05)	(96270.118)	(1.12e+05)	[5.157]***	[-0.443]	[-5.505]***
GDP from healhtcare, million USD	5749.552	3221.234	3812.527	-2528.318	-1937.025	591.293
	(25349.309)	(6409.356)	(7304.024)	[21.465]***	[4.439]***	[-5.310]***
Total farm owner income, million USD	220.413	47.590	41.358	-172.822	-179.055	-6.233
	(1421.474)	(139.676)	(120.472)	[36.397]***	[8.531]***	[2.973]***
Total farm worker income, million USD	150.522	26.143	22.214	-124.378	-128.307	-3.929
	(1026.159)	(90.337)	(74.605)	[36.525]***	[8.471]***	[2.902]***
Total per capita income, 100 thousand USD	0.343	0.388	0.373	0.045	0.030	-0.015
	(0.121)	(0.136)	(0.129)	[-36.367]***	[-14.404]***	[7.336]***
Total income, million USD	38429.431	30427.231	34355.790	-8002.200	-4073.641	3928.558
	(2.28e+05)	(72096.649)	(81299.048)	[8.260]***	[1.185]	[-3.587]***
Share of total GDP from ag	0.091	0.037	0.030	-0.054	-0.061	-0.007
	(0.127)	(0.078)	(0.056)	[58.689]***	[28.482]***	[5.528]***
Population, 100000 persons	9.102	6.655	7.465	-2.447	-1.637	0.810
	(52.728)	(15.254)	(17.037)	[11.321]***	[2.068]**	[-3.497]***
Growth rate in Ag GDP	0.016	0.010	-0.002	-0.006	-0.018	-0.012
	(0.713)	(0.663)	(0.732)	[0.756]	[1.244]	[1.053]
Growth rate in total GDP	0.035	0.037	0.035	0.002	0.000	-0.002
	(0.131)	(0.071)	(0.064)	[-2.488]**	[-0.045]	[1.661]*
Growth rate in health care GDP	0.047	0.049	0.047	0.002	-0.000	-0.002
	(0.173)	(0.053)	(0.055)	[-1.855]*	[0.107]	[2.146]**
Growth rate in farm owner income	0.003	-0.003	-0.015	-0.006	-0.018	-0.012
	(0.814)	(0.678)	(0.748)	[0.811]	[1.151]	[1.021]
Growth rate in farm worker income	0.010	0.010	-0.030	-0.000	-0.040	-0.040
	(0.995)	(0.977)	(1.005)	[0.019]	[1.811]*	[2.086]**
Growth rate in income per capita	0.035	0.032	0.032	-0.003	-0.003	-0.000
	(0.073)	(0.043)	(0.038)	[5.875]***	[2.531]**	[0.384]
Growth rate in total income	0.035	0.038	0.037	0.003	0.002	-0.001
	(0.072)	(0.044)	(0.039)	[-5.416]***	[-1.744]*	[0.747]
Growth rate in ag share of GDP	-0.017	-0.027	-0.037	-0.010	-0.020	-0.010
	(0.675)	(0.649)	(0.718)	[1.334]	[1.433]	[0.869]
Growth rate in population	0.000	0.006	0.005	0.005	0.005	-0.000
	(0.019)	(0.015)	(0.017)	[-34.143]***	[-15.092]***	[1.064]

Notes: Table A8 compares *counties* do not have either a critical access or general acute care hospital closure (column 1), to counties that do have one of the two and that facility does not close (column 2) and to counties that have either type of facility and it closes (column 3).

Appendix B: Results Tables

Table B1—Access to care: event study. Failing vs isolated counties

	ln distance to nearest hospital		ln PCPs per 1,000 residents		PCP shortage (share)	
	DiD	SDID	DiD	SDID	DiD	SDID
$t = -4$	−0.0029**	0.0003*	0.0167	−0.0062	−0.0359	−0.0061
	(0.0012)	(0.0002)	(0.0217)	(0.0123)	(0.0237)	(0.0128)
$t = -3$	−0.0028**	0.0004**	0.0083	0.0041	−0.0328	0.0020
	(0.0011)	(0.0002)	(0.0176)	(0.0108)	(0.0201)	(0.0122)
$t = -2$	−0.0028**	0.0009	0.0213	0.0032	−0.0240	0.0021
	(0.0011)	(0.0006)	(0.0143)	(0.0100)	(0.0172)	(0.0117)
$t = -1$	−0.0020*	0.0029***	0.0121	−0.0128	−0.0192	0.0220*
	(0.0011)	(0.0011)	(0.0110)	(0.0089)	(0.0137)	(0.0131)
$t = +1$	0.0177***	0.0210***	−0.0223*	−0.0208	−0.0007	0.0151
	(0.0036)	(0.0040)	(0.0122)	(0.0153)	(0.0127)	(0.0154)
$t = +2$	0.0188***	0.0222***	−0.0459***	−0.0446**	0.0179	0.0368**
	(0.0038)	(0.0042)	(0.0168)	(0.0191)	(0.0166)	(0.0180)
$t = +3$	0.0192***	0.0226***	−0.0529***	−0.0506**	0.0270	0.0471**
	(0.0038)	(0.0042)	(0.0187)	(0.0210)	(0.0173)	(0.0194)
$t = +4$	0.0203***	0.0239***	−0.0593***	−0.0574**	0.0392*	0.0606***
	(0.0040)	(0.0046)	(0.0219)	(0.0243)	(0.0204)	(0.0201)
ATT	0.0190***	0.0228***	−0.0446***	−0.0534***	0.0205	0.0532***
	(0.0036)	(0.0042)	(0.0154)	(0.0199)	(0.0141)	(0.0164)
Treated counties	502		437		500	
SDID states		35		35		35

Source: 41_CompareFailAbsorbIsolated.do, failing vs isolated. Treated: failing counties (market-level closure), from the first closure year. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. DiD: `did_multiplegt_dyn` with state-specific nonparametric trends and never-switchers as controls. SDID: `sdid_event` estimated within each state and aggregated by switchers; standard errors from 50 bootstrap draws per state. $t = 0$ is the last year before treatment and is normalized to zero. Distance and PCPs per 1,000 in logs (county-years with zero PCPs are missing); PCP shortage is a 0/1 indicator.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B2—Access to care: event study. Absorbing vs isolated counties

	ln distance to nearest hospital		ln PCPs per 1,000 residents		PCP shortage (share)	
	DiD	SDID	DiD	SDID	DiD	SDID
$t = -4$	−0.0008	0.0000	0.0123	0.0164	0.0323	−0.0188
	(0.0015)	(0.0000)	(0.0350)	(0.0205)	(0.0354)	(0.0216)
$t = -3$	−0.0012	0.0000	0.0237	0.0014	−0.0188	0.0177
	(0.0012)	(0.0000)	(0.0327)	(0.0189)	(0.0326)	(0.0197)
$t = -2$	−0.0013	0.0000	0.0063	0.0238	0.0206	0.0155
	(0.0012)	(0.0000)	(0.0260)	(0.0167)	(0.0221)	(0.0193)
$t = -1$	−0.0013	0.0015	0.0197	0.0004	0.0115	0.0157
	(0.0012)	(0.0011)	(0.0186)	(0.0141)	(0.0138)	(0.0185)
$t = +1$	0.0045**	0.0063***	−0.0208	−0.0251	−0.0141	0.0062
	(0.0021)	(0.0023)	(0.0196)	(0.0203)	(0.0219)	(0.0256)
$t = +2$	0.0043**	0.0061**	−0.0130	−0.0170	−0.0039	0.0075
	(0.0022)	(0.0024)	(0.0286)	(0.0272)	(0.0327)	(0.0298)
$t = +3$	0.0059**	0.0078***	−0.0033	−0.0086	0.0122	0.0152
	(0.0027)	(0.0030)	(0.0315)	(0.0308)	(0.0341)	(0.0318)
$t = +4$	0.0044*	0.0066**	0.0060	0.0110	0.0312	0.0293
	(0.0026)	(0.0032)	(0.0357)	(0.0328)	(0.0334)	(0.0354)
ATT	0.0048**	0.0065***	−0.0083	0.0005	0.0055	0.0133
	(0.0022)	(0.0025)	(0.0255)	(0.0274)	(0.0245)	(0.0256)
Treated counties	114		109		111	
SDID states		25		23		25

Source: 41_CompareFailAbsorbIsolated.do, absorbing vs isolated. Treated: absorbing counties, from the first year they absorb a failing market's zip code; includes urban absorbing counties. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. DiD: `did_multiplegt_dyn` with state-specific nonparametric trends and never-switchers as controls. SDID: `sdid_event` estimated within each state and aggregated by switchers; standard errors from 50 bootstrap draws per state. $t = 0$ is the last year before treatment and is normalized to zero. Distance and PCPs per 1,000 in logs (county-years with zero PCPs are missing); PCP shortage is a 0/1 indicator.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B3—Agricultural outcomes. Failing vs isolated counties

	DiD ATT	SDID ATT	Treated counties	SDID states
Agricultural output and employment				
ln agricultural GDP	−0.0030	0.0486	250	28
	(0.0433)	(0.0547)		
ln ag. establishments	−0.0098	−0.0403*	468	29
	(0.0097)	(0.0208)		
ln ag. employees	−0.0222	−0.0326	126	18
	(0.0257)	(0.0469)		
Ag. wages per establishment ($)	−4,054	−8,773	126	18
	(4,943)	(9,993)		
Ag. wages per employee ($)	482	−669	126	18
	(361)	(588)		
Farm income				
Positive (share)	−0.0214	0.0019	500	35
	(0.0138)	(0.0160)		
ln, if positive	0.0062	−0.0229	381	31
	(0.0384)	(0.0426)		
Level, if ≤ 0 ($000)	24	−16	45	2
	(459)	(712)		
Farm proprietor income				
Positive (share)	−0.0133	−0.0204	500	35
	(0.0156)	(0.0179)		
ln, if positive	0.0103	0.0603	316	23
	(0.0479)	(0.0716)		
Level, if ≤ 0 ($000)	−372	−62	97	2
	(383)	(455)		

Source: 41_CompareFailAbsorbIsolated.do, failing vs isolated. Treated: failing counties (market-level closure), from the first closure year. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. ATT is the average of the four post-treatment effects. Agricultural GDP, establishments and employees in logs (levels ≤ 0 missing); ag. wages per establishment and per employee in dollars. QCEW outcomes are private-sector NAICS 11, with BLS-suppressed employee and wage cells missing. Farm income and farm proprietor income (BEA) split by sign as in Table **??**. –: not estimated (too few treated counties with the outcome). Treated counties: switchers at $t = +1$.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B4—Agricultural outcomes. Absorbing vs isolated counties

	DiD ATT	SDID ATT	Treated counties	SDID states
Agricultural output and employment				
ln agricultural GDP	0.005	0.170*	51	14
	(0.097)	(0.096)		
ln ag. establishments	0.0080	0.0045	107	15
	(0.0179)	(0.0276)		
ln ag. employees	0.0000	0.0652	36	8
	(0.0258)	(0.0537)		
Ag. wages per establishment ($)	14,752	21,431	36	8
	(10,614)	(28,752)		
Ag. wages per employee ($)	196	47	36	8
	(587)	(964)		
Farm income				
Positive (share)	0.0048	0.0105	111	25
	(0.0275)	(0.0253)		
ln, if positive	0.0027	0.0907	87	12
	(0.0851)	(0.0820)		
Level, if ≤ 0 ($000)	−4,248	–	13	
	(3,199)			
Farm proprietor income				
Positive (share)	−0.0122	−0.0052	111	25
	(0.0286)	(0.0304)		
ln, if positive	−0.098	0.018	76	4
	(0.092)	(0.184)		
Level, if ≤ 0 ($000)	−588	–	24	
	(1,996)			

Source: 41_CompareFailAbsorbIsolated.do, absorbing vs isolated. Treated: absorbing counties, from the first year they absorb a failing market's zip code; includes urban absorbing counties. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. ATT is the average of the four post-treatment effects. Agricultural GDP, establishments and employees in logs (levels ≤ 0 missing); ag. wages per establishment and per employee in dollars. QCEW outcomes are private-sector NAICS 11, with BLS-suppressed employee and wage cells missing. Farm income and farm proprietor income (BEA) split by sign as in Table **??**. –: not estimated (too few treated counties with the outcome). Treated counties: switchers at $t = +1$.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B5—The broader local economy. Failing vs isolated counties

	DiD ATT	SDID ATT	Treated counties	SDID states
ln GDP	0.0023	−0.0193*	500	35
	(0.0071)	(0.0117)		
ln non-farm income	−0.0019	−0.0061	500	35
	(0.0028)	(0.0041)		
Non-farm proprietor income > 0 (share)	−0.0013	−0.0023*	500	35
	(0.0019)	(0.0013)		
ln non-farm proprietor income, if > 0	−0.0305***	−0.0409***	499	35
	(0.0116)	(0.0158)		

Source: 41_CompareFailAbsorbIsolated.do, failing vs isolated. Treated: failing counties (market-level closure), from the first closure year. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. Total GDP and non-farm income (BEA) in logs; non-farm proprietor income split by sign as in Table **??**, level row omitted. ATT is the average of the four post-treatment effects. Treated counties: switchers at $t = +1$.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B6—The broader local economy. Absorbing vs isolated counties

	DiD ATT	SDID ATT	Treated counties	SDID states
ln GDP	−0.0002	−0.0078	111	25
	(0.0099)	(0.0147)		
ln non-farm income	−0.0049	−0.0052	111	25
	(0.0051)	(0.0054)		
Non-farm proprietor income > 0 (share)	−0.0002	0.0027	111	25
	(0.0002)	(0.0020)		
ln non-farm proprietor income, if > 0	0.0010	0.0085	111	25
	(0.0217)	(0.0242)		

Source: 41_CompareFailAbsorbIsolated.do, absorbing vs isolated. Treated: absorbing counties, from the first year they absorb a failing market's zip code; includes urban absorbing counties. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. Total GDP and non-farm income (BEA) in logs; non-farm proprietor income split by sign as in Table **??**, level row omitted. ATT is the average of the four post-treatment effects. Treated counties: switchers at $t = +1$.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B7—All-cause mortality by age. Failing vs isolated counties

	DiD ATT	SDID ATT	Treated counties	SDID states
ln deaths per 100,000 (zero-death county-years missing)				
All ages	0.0113**	0.0146***	492	35
	(0.0052)	(0.0052)		
Ages 35–44	0.425**	−0.218	14	1
	(0.170)	(0.140)		
Ages 45–54	0.0469	−0.0386	135	17
	(0.0358)	(0.0307)		
Ages 55–64	0.0125	0.0071	264	28
	(0.0155)	(0.0144)		
Ages 65–74	−0.0150	0.0024	337	32
	(0.0129)	(0.0095)		
Ages 75–84	0.0036	0.0089	390	34
	(0.0103)	(0.0076)		
Ages 85+	0.0208**	0.0133	403	34
	(0.0102)	(0.0083)		
ln(deaths per 100,000 + 1)				
All ages	−0.0248	0.0214	503	35
	(0.0249)	(0.0195)		
Ages 15–24	−0.0022	−0.0719***	503	35
	(0.0297)	(0.0224)		
Ages 25–34	−0.0123	0.0388	503	35
	(0.0435)	(0.0305)		
Ages 35–44	0.0183	−0.0188	503	35
	(0.0627)	(0.0453)		
Ages 45–54	0.063	−0.104	503	35
	(0.088)	(0.066)		
Ages 55–64	−0.052	0.281***	503	35
	(0.084)	(0.066)		
Ages 65–74	−0.035	0.147***	503	35
	(0.074)	(0.057)		
Ages 75–84	−0.157*	0.042	503	35
	(0.081)	(0.055)		
Ages 85+	0.0688	0.0778	496	35
	(0.0792)	(0.0699)		

Source: 41_CompareFailAbsorbIsolated.do, failing vs isolated. Treated: failing counties (market-level closure), from the first closure year. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. CDC WONDER all-cause deaths per 100,000 residents; WONDER suppresses counts of 1–9 deaths, which are set to zero. Age-group rates add the two sex cells, which are suppressed far more often than the all-ages count, so the strict-log age rows rest on fewer counties and some cannot be estimated (not shown). ATT is the average of the four post-treatment effects. Treated counties: DiD switchers at $t = +1$.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B8—All-cause mortality by age. Absorbing vs isolated counties

	DiD ATT	SDID ATT	Treated counties	SDID states
ln deaths per 100,000 (zero-death county-years missing)				
All ages	−0.0159**	0.0006	114	25
	(0.0074)	(0.0062)		
Ages 15–24	−0.160	–	2	
	(0.173)			
Ages 25–34	0.036	–	4	
	(0.188)			
Ages 35–44	0.181*	−0.078	14	1
	(0.098)	(0.135)		
Ages 45–54	0.0437	−0.0549*	55	11
	(0.0402)	(0.0303)		
Ages 55–64	−0.0030	−0.0493***	79	15
	(0.0263)	(0.0163)		
Ages 65–74	−0.0363**	−0.0350**	91	19
	(0.0173)	(0.0140)		
Ages 75–84	−0.0302*	−0.0230*	102	22
	(0.0154)	(0.0124)		
Ages 85+	−0.0240	−0.0039	104	24
	(0.0166)	(0.0137)		
ln(deaths per 100,000 + 1)				
All ages	−0.0204*	−0.0087	114	25
	(0.0108)	(0.0083)		
Ages 15–24	0.0576	−0.0499	114	25
	(0.0718)	(0.0450)		
Ages 25–34	0.022	0.102	114	25
	(0.099)	(0.072)		
Ages 35–44	0.055	−0.070	114	25
	(0.157)	(0.093)		
Ages 45–54	0.045	−0.081	114	25
	(0.168)	(0.111)		
Ages 55–64	0.073	0.199*	114	25
	(0.096)	(0.116)		
Ages 65–74	−0.129	0.151	114	25
	(0.115)	(0.095)		
Ages 75–84	0.0456	−0.0026	114	25
	(0.0958)	(0.0999)		
Ages 85+	0.000	−0.021	114	25
	(0.159)	(0.098)		

Source: 41_CompareFailAbsorbIsolated.do, absorbing vs isolated. Treated: absorbing counties, from the first year they absorb a failing market's zip code; includes urban absorbing counties. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. CDC WONDER all-cause deaths per 100,000 residents; WONDER suppresses counts of 1–9 deaths, which are set to zero. Age-group rates add the two sex cells, which are suppressed far more often than the all-ages count, so the strict-log age rows rest on fewer counties and some cannot be estimated (not shown). ATT is the average of the four post-treatment effects. Treated counties: DiD switchers at $t = +1$.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B9—All-cause mortality, all ages: event study. Failing vs isolated counties

	ln deaths per 100,000		ln(deaths per 100,000 + 1)	
	DiD	SDID	DiD	SDID
$t = -4$	−0.0065	0.0002	−0.0622	0.0004
	(0.0087)	(0.0046)	(0.0394)	(0.0129)
$t = -3$	−0.0012	0.0064	−0.0518	0.0253
	(0.0071)	(0.0047)	(0.0320)	(0.0184)
$t = -2$	0.0081	0.0077	−0.0168	−0.0058
	(0.0066)	(0.0049)	(0.0301)	(0.0176)
$t = -1$	0.0084	0.0004	−0.0523	0.0438
	(0.0065)	(0.0049)	(0.0355)	(0.0272)
$t = +1$	0.0024	0.0039	−0.0643**	−0.0164
	(0.0067)	(0.0063)	(0.0291)	(0.0323)
$t = +2$	0.0102	0.0115*	−0.0128	0.0358*
	(0.0063)	(0.0062)	(0.0268)	(0.0212)
$t = +3$	0.0240***	0.0245***	−0.0031	0.0401
	(0.0065)	(0.0064)	(0.0293)	(0.0262)
$t = +4$	0.0085	0.0120*	−0.0186	0.0339
	(0.0072)	(0.0072)	(0.0405)	(0.0250)
ATT	0.0113**	0.0146***	−0.0248	0.0214
	(0.0052)	(0.0052)	(0.0249)	(0.0195)
Treated counties	492		503	
SDID states		35		35

Source: 41_CompareFailAbsorbIsolated.do, failing vs isolated. Treated: failing counties (market-level closure), from the first closure year. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. CDC WONDER all-cause deaths per 100,000 residents; WONDER suppresses counts of 1–9 deaths, which are set to zero. ln: county-years with zero recorded deaths are missing; ln(rate + 1) keeps them. $t = 0$ is the last year before treatment.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B10—All-cause mortality, all ages: event study. Absorbing vs isolated counties

	ln deaths per 100,000		ln(deaths per 100,000 + 1)	
	DiD	SDID	DiD	SDID
$t = -4$	−0.0040	−0.0035	0.0080	−0.0038
	(0.0103)	(0.0073)	(0.0125)	(0.0080)
$t = -3$	−0.0153	0.0027	−0.0067	0.0004
	(0.0106)	(0.0065)	(0.0125)	(0.0071)
$t = -2$	−0.0104	0.0030	−0.0122	−0.0008
	(0.0119)	(0.0055)	(0.0132)	(0.0073)
$t = -1$	−0.0139	0.0123**	−0.0139	0.0109
	(0.0088)	(0.0059)	(0.0107)	(0.0071)
$t = +1$	−0.0137	0.0059	−0.0225	−0.0075
	(0.0104)	(0.0096)	(0.0147)	(0.0122)
$t = +2$	−0.0183*	−0.0035	−0.0205	−0.0107
	(0.0102)	(0.0090)	(0.0151)	(0.0117)
$t = +3$	−0.0086	0.0076	−0.0056	0.0072
	(0.0094)	(0.0080)	(0.0117)	(0.0097)
$t = +4$	−0.0242**	−0.0096	−0.0349**	−0.0302**
	(0.0095)	(0.0078)	(0.0155)	(0.0135)
ATT	−0.0159**	0.0006	−0.0204*	−0.0087
	(0.0074)	(0.0062)	(0.0108)	(0.0083)
Treated counties	114		114	
SDID states		25		25

Source: 41_CompareFailAbsorbIsolated.do, absorbing vs isolated. Treated: absorbing counties, from the first year they absorb a failing market's zip code; includes urban absorbing counties. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. CDC WONDER all-cause deaths per 100,000 residents; WONDER suppresses counts of 1–9 deaths, which are set to zero. ln: county-years with zero recorded deaths are missing; ln(rate + 1) keeps them. $t = 0$ is the last year before treatment.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B11—Effect of hospital closure on adults in the CPS: DiD

	All adults	Non-farmers	Farmers
Employed	0.0031	0.0035	0.0122
	(0.0087)	(0.0089)	(0.0474)
In labor force	−0.0020	−0.0016	0.0049
	(0.0090)	(0.0093)	(0.0166)
Hours worked last week	0.13	0.13	1.49
	(0.24)	(0.24)	(1.94)
Worked full time (35+ hours)	0.0040	0.0062	−0.0151
	(0.0096)	(0.0097)	(0.0544)
Absent from work, illness	0.0008	0.0007	0.0040
	(0.0009)	(0.0009)	(0.0031)
Part time, illness	0.0019	0.0020	−0.0430*
	(0.0017)	(0.0017)	(0.0243)
Left last job, illness	−0.0007	−0.0007	−0.0165
	(0.0029)	(0.0030)	(0.0124)
Disability prevents work	0.0004	−0.0000	−0.0029
	(0.0062)	(0.0061)	(0.0213)
Health good or worse	0.0122	0.0102	0.0952*
	(0.0113)	(0.0114)	(0.0511)
Health worse than last interview	0.0201*	0.0176	0.0834
	(0.0119)	(0.0122)	(0.0719)
Moved out of county	0.0034	0.0034	0.0083
	(0.0040)	(0.0041)	(0.0232)
Moved within county	0.0080	0.0078	−0.0137
	(0.0065)	(0.0064)	(0.0346)
Moved between counties in state	−0.0016	−0.0015	−0.0097
	(0.0032)	(0.0033)	(0.0205)
Moved between states	0.0052**	0.0053**	0.0054
	(0.0026)	(0.0026)	(0.0105)
Farm income negative	–	–	−0.0051
			(0.0079)
Farm income if non-negative ($)	–	–	−3,142
			(4,423)
Treated counties (employed)	135	135	74

Source: 17_CPS_MasterMerge.do. March CPS-ASEC, 2001–2019, adults 16+. Outcomes are county-year means weighted by `asecwt`, by group; farmers are persons in farm industries (IND1950 105, 116, 126). All CPS-identified counties, including metropolitan ones; treatment from the closure file before the urban restriction. Estimators as in 41. Health good or worse: self-reported health good, fair or poor rather than very good or excellent. Health worse than last interview: self-reported health lower than at the person's previous CPS interview. Farm income rows: farmers only.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B12—Effect of hospital closure on self-reported health in the CPS

	All adults		Non-farmers		Farmers	
	DiD	SDID	DiD	SDID	DiD	SDID
Health very good or excellent	−0.0122	0.0006	−0.0102	0.0003	−0.0952*	−0.0975
	(0.0113)	(0.0140)	(0.0114)	(0.0149)	(0.0511)	(0.0679)
Health good or better	0.0025	0.0043	0.0030	0.0062	−0.0401	−0.0442
	(0.0077)	(0.0072)	(0.0077)	(0.0084)	(0.0261)	(0.0470)
Health fair or better	0.0023	0.0062	0.0027	0.0062	−0.0100	−0.0030
	(0.0035)	(0.0048)	(0.0035)	(0.0047)	(0.0128)	(0.0190)
Health very good or worse	0.0097	−0.0067	0.0095	−0.0072	−0.0541	0.0582
	(0.0091)	(0.0115)	(0.0092)	(0.0115)	(0.0488)	(0.0948)
Health good or worse	0.0122	−0.0006	0.0102	−0.0003	0.0952*	0.0975
	(0.0113)	(0.0146)	(0.0114)	(0.0144)	(0.0511)	(0.0711)
Health fair or poor	−0.0025	−0.0043	−0.0030	−0.0062	0.0401	0.0442
	(0.0077)	(0.0076)	(0.0077)	(0.0087)	(0.0261)	(0.0462)
Health worse than last interview	0.0201*	0.0018	0.0176	0.0030	0.0834	–
	(0.0119)	(0.0107)	(0.0122)	(0.0120)	(0.0719)	
Treated counties / states	135	8	135	8	74	2

Source: 17_CPS_MasterMerge.do. March CPS-ASEC, 2001–2019, adults 16+; county-year shares weighted by `asecwt`, by group (farmers: IND1950 105, 116, 126). All CPS-identified counties, including metropolitan ones; market treatment from the closure file before the urban restriction; estimators as in 41. Each outcome is a 0/1 indicator for self-reported health in the named range (excellent, very good, good, fair, poor), so rows 1–3 and 4–6 are complements in reverse order (e.g. very good or excellent = 1− good or worse); health worse than last interview: self-reported health lower than at the person's previous CPS interview. ATT is the average of the four post-closure effects. Last row: DiD treated counties and SDID states for health good or worse.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B13—The nearest hospital's services and quality. Failing vs isolated counties

	DiD ATT	SDID ATT	Treated counties	SDID states
Services				
Services offered (0–5)	0.0621***	0.0688***	479	35
	(0.0137)	(0.0200)		
Quality				
Process of care (%)	0.760*	0.881*	354	35
	(0.422)	(0.486)		
30-day mortality (%)	−0.0092	−0.0074	425	35
	(0.0473)	(0.0524)		
30-day readmission (%)	−0.0344	−0.0251	426	35
	(0.0519)	(0.0622)		
Patients rating 9–10 (%)	−0.632*	−0.586	269	33
	(0.327)	(0.382)		
Definitely recommend (%)	−0.541	−0.449	269	33
	(0.332)	(0.446)		

Source: 41_CompareFailAbsorbIsolated.do (CMP_SET hs), failing vs isolated. Treated: failing counties (market-level closure), from the first closure year. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. County means over the county's zip codes of the nearest open general acute or critical access hospital's value that year (91_CountyNearestHospPanel.py): the open set loses each closed hospital from its closure year, so the nearest hospital changes only through closures. Services: how many of emergency, surgery, imaging/diagnostics, obstetrics and rehab the hospital's cost report shows (0–5). Quality: Hospital Compare, release year; process of care 2005–2016 (higher is better), risk-standardized 30-day mortality 2008–2019 and readmission 2009–2019 (AMI, heart failure, pneumonia; lower is better), HCAHPS 2008–2019. Zip codes whose hospital reports no score are left out of the county mean. Levels. ATT is the average of the four post-treatment effects. –: not estimated. Treated counties: switchers at $t = +1$.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B14—The nearest hospital's services and quality. Absorbing vs isolated counties

	DiD ATT	SDID ATT	Treated counties	SDID states
Services				
Services offered (0–5)	0.0388	0.0368	110	25
	(0.0276)	(0.0323)		
Quality				
Process of care (%)	0.26	1.09**	81	20
	(0.44)	(0.46)		
30-day mortality (%)	0.0273	0.0890	99	23
	(0.0844)	(0.0846)		
30-day readmission (%)	0.111	0.114	99	23
	(0.079)	(0.096)		
Patients rating 9–10 (%)	−0.239	−0.765	74	18
	(0.508)	(0.638)		
Definitely recommend (%)	−0.59	−1.07	74	18
	(0.63)	(0.80)		

Source: 41_CompareFailAbsorbIsolated.do (CMP_SET hs), absorbing vs isolated. Treated: absorbing counties, from the first year they absorb a failing market's zip code; includes urban absorbing counties. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. County means over the county's zip codes of the nearest open general acute or critical access hospital's value that year (91_CountyNearestHospPanel.py): the open set loses each closed hospital from its closure year, so the nearest hospital changes only through closures. Services: how many of emergency, surgery, imaging/diagnostics, obstetrics and rehab the hospital's cost report shows (0–5). Quality: Hospital Compare, release year; process of care 2005–2016 (higher is better), risk-standardized 30-day mortality 2008–2019 and readmission 2009–2019 (AMI, heart failure, pneumonia; lower is better), HCAHPS 2008–2019. Zip codes whose hospital reports no score are left out of the county mean. Levels. ATT is the average of the four post-treatment effects. –: not estimated. Treated counties: switchers at $t = +1$.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B15—HSA population and patient pressure. Failing vs isolated counties

	DiD ATT	SDID ATT	Treated counties	SDID states
Population and beds (logs)				
ln HSA population	−0.193***	−0.229***	502	35
	(0.027)	(0.038)		
ln HSA population per hospital bed	0.121***	0.141***	497	35
	(0.013)	(0.018)		
Occupancy (share of beds filled)				
Occupancy rate of the nearest hospital	0.0191***	0.0151***	479	35
	(0.0040)	(0.0052)		
Occupancy rate of the HSA's hospitals	0.0041	0.0021	495	35
	(0.0040)	(0.0053)		

Source: 41_CompareFailAbsorbIsolated.do (CMP_SET hs, occ), failing vs isolated. Treated: failing counties (market-level closure), from the first closure year. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. County means over the county's zip codes of the population of the zip code's current HSA (zip codes are reassigned to the HSA of their nearest open hospital after a closure in their HSA; 62_HSAReassignment2005to2019.py, 2010 Census population, so HSA population changes only through reassignment) and of HSA population per bed in the HSA's open hospitals that year (CMS Provider of Services beds, else cost-report beds), in logs. Occupancy (CMP_SET occ): inpatient days over bed days from the cost reports, for the zip code's nearest open hospital and for the HSA's open hospitals pooled, county means over zip codes, in levels; hospital-years with occupancy above 1 are missing. ATT is the average of the four post-treatment effects. –: not estimated. Treated counties: switchers at $t = +1$.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B16—HSA population and patient pressure. Absorbing vs isolated counties

	DiD ATT	SDID ATT	Treated counties	SDID states
Population and beds (logs)				
ln HSA population	0.0811**	0.0835*	114	25
	(0.0338)	(0.0432)		
ln HSA population per hospital bed	0.177***	0.184***	114	25
	(0.026)	(0.029)		
Occupancy (share of beds filled)				
Occupancy rate of the nearest hospital	0.0047	0.0062	110	25
	(0.0061)	(0.0086)		
Occupancy rate of the HSA's hospitals	−0.0026	−0.0008	113	25
	(0.0069)	(0.0088)		

Source: 41_CompareFailAbsorbIsolated.do (CMP_SET hs, occ), absorbing vs isolated. Treated: absorbing counties, from the first year they absorb a failing market's zip code; includes urban absorbing counties. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. County means over the county's zip codes of the population of the zip code's current HSA (zip codes are reassigned to the HSA of their nearest open hospital after a closure in their HSA; 62_HSAReassignment2005to2019.py, 2010 Census population, so HSA population changes only through reassignment) and of HSA population per bed in the HSA's open hospitals that year (CMS Provider of Services beds, else cost-report beds), in logs. Occupancy (CMP_SET occ): inpatient days over bed days from the cost reports, for the zip code's nearest open hospital and for the HSA's open hospitals pooled, county means over zip codes, in levels; hospital-years with occupancy above 1 are missing. ATT is the average of the four post-treatment effects. –: not estimated. Treated counties: switchers at $t = +1$.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B17—Ambulatory care and the health care sector. Failing vs isolated counties

	DiD ATT	SDID ATT	Treated counties	SDID states
ln ambulatory care wages	−0.0019	−0.0023	271	31
	(0.0156)	(0.0183)		
ln ambulatory care employees	0.0029	0.0009	271	31
	(0.0129)	(0.0168)		
ln health care GDP	0.0054	0.0051	126	23
	(0.0170)	(0.0312)		

Source: 41_CompareFailAbsorbIsolated.do (CMP_SET hs), failing vs isolated. Treated: failing counties (market-level closure), from the first closure year. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. Private-sector QCEW ambulatory health care (NAICS 621) total wages and employees, BLS-suppressed cells missing; BEA health care GDP. Logs. Hospital (NAICS 622) employment and wages come from the cost reports instead (hospital employment table), since QCEW suppresses them in nearly every rural county. ATT is the average of the four post-treatment effects. –: not estimated. Treated counties: switchers at $t = +1$.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B18—Ambulatory care and the health care sector. Absorbing vs isolated counties

	DiD ATT	SDID ATT	Treated counties	SDID states
ln ambulatory care wages	0.0239	0.0474*	88	19
	(0.0180)	(0.0261)		
ln ambulatory care employees	0.0106	0.0331	88	19
	(0.0163)	(0.0249)		
ln health care GDP	0.0033	0.0269	43	8
	(0.0197)	(0.0295)		

Source: 41_CompareFailAbsorbIsolated.do (CMP_SET hs), absorbing vs isolated. Treated: absorbing counties, from the first year they absorb a failing market's zip code; includes urban absorbing counties. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. Private-sector QCEW ambulatory health care (NAICS 621) total wages and employees, BLS-suppressed cells missing; BEA health care GDP. Logs. Hospital (NAICS 622) employment and wages come from the cost reports instead (hospital employment table), since QCEW suppresses them in nearly every rural county. ATT is the average of the four post-treatment effects. –: not estimated. Treated counties: switchers at $t = +1$.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B19—Hospital employment and salaries. Failing vs isolated counties

	DiD ATT	SDID ATT	Treated counties	SDID states
Hospital FTE employees	−7.31*	−7.20	502	35
	(4.38)	(5.00)		
Hospital salaries ($ million)	−0.377	−0.600	502	35
	(0.290)	(0.399)		

Source: 41_CompareFailAbsorbIsolated.do (CMP_SET hs4), failing vs isolated. Treated: failing counties (market-level closure), from the first closure year. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. Medicare cost reports (92_CountyHospStaffing.py), summed over the open general acute and critical access hospitals located in the county: FTE employees on payroll (Worksheet S-3 Part I) and total salaries in millions of nominal dollars (Worksheet A). A closed hospital counts 0 from its closure year, and a county with no open hospital is 0. Values outside one third to three times the hospital's own median are treated as entry errors, and gaps are interpolated within the hospital. Levels. ATT is the average of the four post-treatment effects. –: not estimated. Treated counties: switchers at $t = +1$.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

Table B20—Hospital employment and salaries. Absorbing vs isolated counties

	DiD ATT	SDID ATT	Treated counties	SDID states
Hospital FTE employees	70**	110**	114	25
	(29)	(49)		
Hospital salaries ($ million)	8.7***	13.9***	114	25
	(3.1)	(5.4)		

Source: 41_CompareFailAbsorbIsolated.do (CMP_SET hs4), absorbing vs isolated. Treated: absorbing counties, from the first year they absorb a failing market's zip code; includes urban absorbing counties. Controls: isolated counties (no market-level closure and never absorbing a failing market's zip code). Sample, estimators and missing-data rules as in 41; cohorts first treated after 2017 dropped. Medicare cost reports (92_CountyHospStaffing.py), summed over the open general acute and critical access hospitals located in the county: FTE employees on payroll (Worksheet S-3 Part I) and total salaries in millions of nominal dollars (Worksheet A). A closed hospital counts 0 from its closure year, and a county with no open hospital is 0. Values outside one third to three times the hospital's own median are treated as entry errors, and gaps are interpolated within the hospital. Levels. ATT is the average of the four post-treatment effects. –: not estimated. Treated counties: switchers at $t = +1$.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

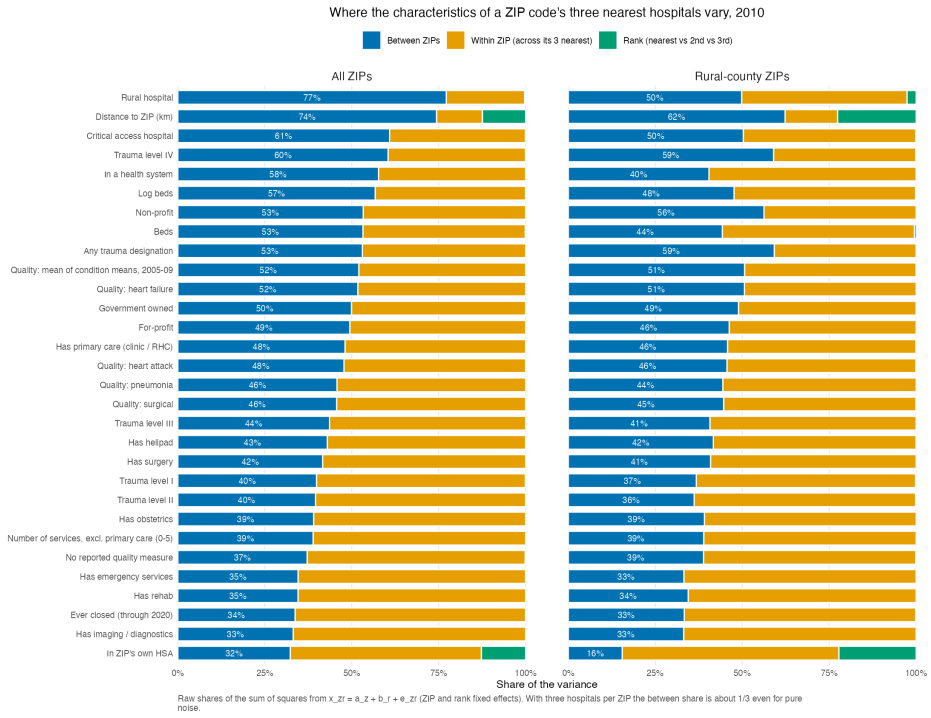


Figure C1. : Raw regression

First, I take each zip code and identify the three nearest hospitals. I then run regressions on the characteristics of the three hospitals, explained by zip code fixed effect and the "rank" of the hospital (as distance depends mechanically on rank. You can see in the figures that rank contributes basically nothing else to any other variance).

The raw ("regression") figure plots the r^2 from regressing each characteristic on ZIP fixed effects (plus rank). The other figure plots the ICC, the share of the true variance that lies between ZIP codes. Note that mechanically, the raw r^2 starts at about one third of variance explained by between-variation: The ZIP fixed effect is the mean of that ZIP's three hospitals. Even if hospitals were assigned to ZIP codes at random, the mean of three draws still varies from ZIP to ZIP. So the fixed effects soak up about a third of the variance even when there is no real ZIP effect:

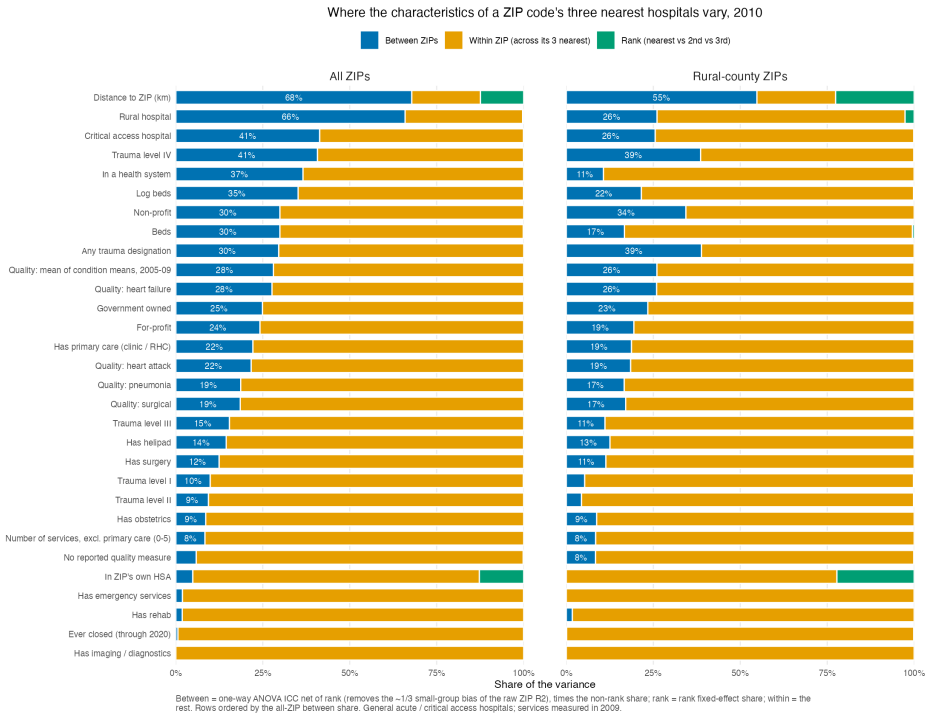


Figure C2. : ICC Corrected

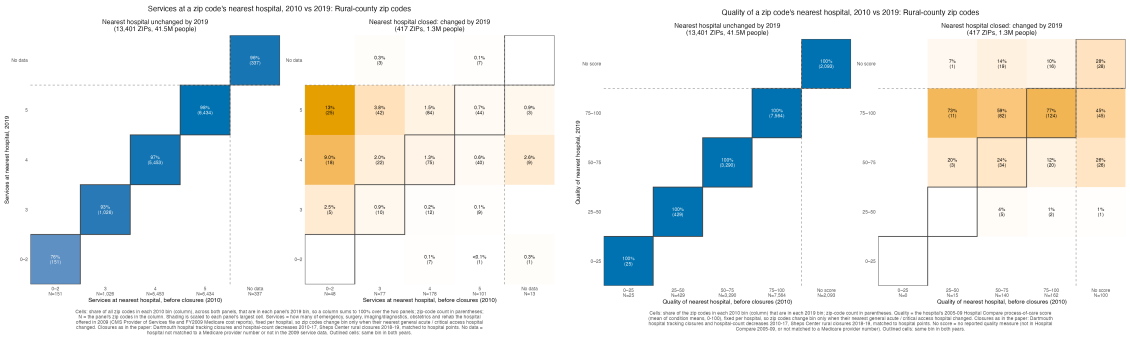
$E[r^2] \approx (G-1)/(N-1) \approx 1/3$ with 3 hospitals per ZIP (G = number of ZIP codes, N = 3G)

The ICC graph corrects this by subtracting the chance part:

$$ICC = (MSB - MSW)/(MSB + 2MSW)$$

Here MSB is the between-ZIP mean square and MSW the within-ZIP mean square. If ZIP codes don't differ, $MSB \approx MSW$ and the $ICC \approx 0$.

The results, shown in Figure **??**, show that almost all variation is explained by *within* zip code-hospital tuple. The hospitals within a Z-H tuple are more different than Z-H tuples. This indicates that within the tuple, there is substantial variation in services/characteristics/quality (as shown below).



(a) Services at nearest hospital (b) Quality at nearest hospital

Figure C3. Services and quality at nearest hospital

C1. Cohorts of hospital closures

Tables C1 and C2 compare each treatment cohort, in the last year before its first closure, with never-treated counties in the same year. Treated counties are slightly larger than never-treated counties (24,700 vs.21,600 residents) and start with fewer primary care physicians: 0.27 vs. 0.33 PCPs per 1,000 residents, and 62 vs. 52 percent in a PCP shortage. Both gaps are significant at 1 percent. The PCP gap comes from the 2011–2014 and 2017 cohorts; the 2010, 2015 and 2016 cohorts look like never-treated counties. Treated and never-treated counties do not differ in distance to the nearest hospital (21.9 vs. 21.5 km) or in median GDP per capita (32 vs. 30 thousand dollars). The lower physician supply in treated counties before closure is a level difference, which the county fixed effects in every specification absorb; the placebo estimates in the Section V.A test whether it is also a difference in trends, and rejects that they are different.

Table C1—Baseline characteristics by cohort: failing vs never-treated (isolated) counties

	Failing cohort (first market-level closure)								All	Never treated
	2010	2011	2012	2013	2014	2015	2016	2017	treated	(isolated)
Baseline year	2009	2010	2011	2012	2013	2014	2015	2016	$t_0 - 1$	2009
Counties	38	80	77	61	57	59	85	43	500	1,241
Population	28,716	21,357	29,651	26,741	25,416	20,328	23,429	22,971	24,683	21,261
	(19,935)	(20,006)	(26,788)	(20,275)	(20,406)	(18,450)	(20,423)	(22,918)	(21,504)	(19,703)
Diff. from never treated	[7,455**]	[42]	[8,338***]	[5,445**]	[4,131]	[−939]	[2,172]	[1,725]	[3,405***]	
GDP per capita ($000)	33.7	34.8	54.5	47.5	30.2	47.9	40.3	47.2	42.3	35.7
	(18.8)	(25.7)	(54.5)	(55.4)	(14.2)	(36.5)	(22.7)	(39.4)	(37.6)	(26.3)
Median	28.2	28.7	35.0	30.0	26.9	35.0	34.5	36.9	31.9	30.4
Diff. from never treated	[−2.0]	[−3.4]	[12.8**]	[4.9]	[−15.4***]	[1.3]	[−4.3*]	[3.4]	[−0.2]	
Distance to nearest hospital (km)	20.5	24.6	22.6	19.6	21.8	21.6	20.5	23.5	21.9	21.8
	(7.2)	(14.4)	(12.1)	(9.1)	(11.8)	(13.2)	(8.8)	(15.3)	(11.8)	(12.5)
Diff. from never treated	[−1.2]	[2.9*]	[0.9]	[−2.0*]	[0.1]	[−0.1]	[−1.2]	[1.9]	[0.2]	
PCPs per 1,000 residents	0.321	0.251	0.268	0.234	0.241	0.317	0.311	0.241	0.273	0.327
	(0.179)	(0.173)	(0.186)	(0.158)	(0.186)	(0.224)	(0.260)	(0.193)	(0.202)	(0.284)
Diff. from never treated	[−0.006]	[−0.069***]	[−0.047**]	[−0.086***]	[−0.076***]	[0.005]	[0.000]	[−0.070**]	[−0.043***]	
PCP shortage (share)	0.526	0.575	0.649	0.689	0.684	0.525	0.588	0.698	0.616	0.527
	(0.506)	(0.497)	(0.480)	(0.467)	(0.469)	(0.504)	(0.495)	(0.465)	(0.487)	(0.499)
Diff. from never treated	[−0.001]	[0.048]	[0.108*]	[0.145**]	[0.143**]	[−0.031]	[0.029]	[0.138*]	[0.071***]	

Source: 49_CohortBaselineStats.do. Means with standard deviations in parentheses, each cohort in its own baseline year, the last year before its first treated year ($t = 0$ in the event studies). Never-treated counties are the isolated counties (no market-level closure and never absorbing a failing market's zip code), in 2009. Differences in brackets compare each cohort with never-treated (isolated) counties in the same year (robust standard errors; none for a one-county cohort); All treated stacks the cohorts with year fixed effects, standard errors clustered by county. PCP rows use 2008 where the baseline is 2009, because the AHRF PCP count is missing in 2009. GDP per capita is top-coded at the 99th percentile of county-years; the median is also shown. Counties: with population data; other rows can differ by a few counties. Sample as in 41_CompareFailAbsorbIsolated.do.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C2—Baseline characteristics by cohort: absorbing vs never-treated (isolated) counties

	Absorbing cohort (first year absorbing a failing market's zip code)								All	Never treated
	2010	2011	2012	2013	2014	2015	2016	2017	treated	(isolated)
Baseline year	2009	2010	2011	2012	2013	2014	2015	2016	$t_0 - 1$	2009
Counties	6	11	11	15	18	15	21	15	112	1,241
Population	190,277	148,780	50,835	108,573	20,865	145,431	95,952	62,093	93,477	21,261
	(371,030)	(169,295)	(48,371)	(90,066)	(11,046)	(303,845)	(263,662)	(101,938)	(194,656)	(19,703)
Diff. from never treated	[169,016]	[127,465***]	[29,521**]	[87,277***]	[−420]	[124,164]	[74,696]	[40,846]	[72,266***]	
GDP per capita ($000)	40.3	40.0	37.1	34.4	36.6	36.2	58.0	44.2	41.9	35.4
	(25.8)	(25.3)	(22.1)	(9.3)	(15.8)	(14.0)	(50.9)	(37.4)	(30.3)	(23.9)
Median	28.8	34.1	29.9	31.9	32.2	33.3	38.1	35.1	33.5	30.4
Diff. from never treated	[4.9]	[2.2]	[−4.2]	[−7.8***]	[−8.4**]	[−9.8***]	[13.7]	[0.7]	[−1.0]	
Distance to nearest hospital (km)	12.5	14.6	16.6	12.5	19.4	15.0	16.5	16.3	15.9	21.8
	(5.8)	(7.2)	(9.1)	(4.0)	(11.8)	(10.5)	(7.0)	(7.0)	(8.5)	(12.5)
Diff. from never treated	[−9.3***]	[−7.1***]	[−5.0*]	[−9.1***]	[−2.3]	[−6.7**]	[−5.2***]	[−5.4***]	[−5.8***]	
PCPs per 1,000 residents	0.231	0.344	0.428	0.248	0.275	0.360	0.354	0.345	0.326	0.327
	(0.107)	(0.193)	(0.357)	(0.128)	(0.156)	(0.132)	(0.274)	(0.218)	(0.214)	(0.284)
Diff. from never treated	[−0.096**]	[0.024]	[0.114]	[−0.071**]	[−0.042]	[0.048]	[0.043]	[0.034]	[0.011]	
PCP shortage (share)	0.833	0.364	0.545	0.600	0.556	0.333	0.524	0.467	0.509	0.527
	(0.408)	(0.505)	(0.522)	(0.507)	(0.511)	(0.488)	(0.512)	(0.516)	(0.502)	(0.499)
Diff. from never treated	[0.306**]	[−0.163]	[0.004]	[0.056]	[0.014]	[−0.223*]	[−0.035]	[−0.093]	[−0.038]	

Source: 49_CohortBaselineStats.do. Means with standard deviations in parentheses, each cohort in its own baseline year, the last year before its first treated year ($t = 0$ in the event studies). Never-treated counties are the isolated counties (no market-level closure and never absorbing a failing market's zip code), in 2009. Differences in brackets compare each cohort with never-treated (isolated) counties in the same year (robust standard errors; none for a one-county cohort); All treated stacks the cohorts with year fixed effects, standard errors clustered by county. PCP rows use 2008 where the baseline is 2009, because the AHRF PCP count is missing in 2009. GDP per capita is top-coded at the 99th percentile of county-years; the median is also shown. Counties: with population data; other rows can differ by a few counties. Sample as in 41_CompareFailAbsorbIsolated.do, which keeps urban absorbing counties.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix D: Sensitivity Analysis

D1. Later cohorts

Table D1—Late cohorts (first treated 2017–2019): event study. Failing vs isolated counties

	ln distance		ln PCPs per 1,000		ln mortality		ln ag. GDP	
	DiD	SDID	DiD	SDID	DiD	SDID	DiD	SDID
$t = -4$	–	0.0001	–	0.0483	–	0.0078	–	0.115
		(0.0001)		(0.0364)		(0.0102)		(0.139)
$t = -3$	−0.0005	0.0012	0.0264	−0.0004	0.0322	0.0067	0.177	0.034
	(0.0007)	(0.0007)	(0.0641)	(0.0281)	(0.0290)	(0.0101)	(0.187)	(0.145)
$t = -2$	0.0004*	0.0011	0.0448	0.0032	0.0184	−0.0057	−0.150	0.185*
	(0.0002)	(0.0007)	(0.0319)	(0.0285)	(0.0232)	(0.0137)	(0.175)	(0.112)
$t = -1$	0.0002*	0.0011	0.0122	−0.0128	0.0061	−0.0003	−0.021	0.163
	(0.0001)	(0.0007)	(0.0336)	(0.0238)	(0.0192)	(0.0128)	(0.098)	(0.130)
$t = +1$	0.0849***	0.0916***	−0.0095	−0.0311	0.0168	0.0058	−0.018	0.086
	(0.0292)	(0.0301)	(0.0181)	(0.0333)	(0.0150)	(0.0126)	(0.104)	(0.149)
$t = +2$	0.0438**	0.0479**	−0.0276	−0.0665	0.0182	0.0231	−0.022	0.115
	(0.0196)	(0.0223)	(0.0319)	(0.0494)	(0.0172)	(0.0151)	(0.105)	(0.159)
$t = +3$	0.0293	0.0312	0.0261	−0.0375	0.0207	0.0062	0.195	0.144
	(0.0183)	(0.0227)	(0.0497)	(0.0627)	(0.0242)	(0.0184)	(0.152)	(0.182)
ATT	0.0568***	0.0617***	−0.0066	−0.0446	0.0183	0.0118	0.040	0.112
	(0.0195)	(0.0217)	(0.0255)	(0.0412)	(0.0154)	(0.0113)	(0.105)	(0.149)
Treated counties	67		57		67		36	
SDID states		17		13		17		8

Source: 41_CompareFailAbsorbIsolated.do with CMP_COHORT late, failing vs isolated. Treated: failing counties (market-level closure), from the first closure year. Only cohorts first treated 2017–2019 are kept; controls are the same isolated counties as in the main comparison. Sample, estimators and missing-data rules otherwise as in Table B1. The 2017 cohort is observed for three post-treatment years, so the event study stops at $t = +3$ and ATT is the average of the three post-treatment effects. $t = 0$ is the last year before treatment and is normalized to zero; `did_multiplegt_dyn` allows no more placebos than effects, so the DiD has no $t = -4$ (–). Distance, PCPs per 1,000, all-cause deaths per 100,000 and agricultural GDP in logs. Treated counties: DiD switchers at $t = +1$.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

D2. Alternative specifications

I re-estimate the main specification six ways, in three pairs. Each pair changes one feature of the design, and within each pair one version uses the DiD and the other SDID.

1. Population weights (1a, 1b).

The main estimates weight counties equally. Specification 1a adds population weights to the DiD, where each county's weight is its share of its HSA's 2001 population. The SDID estimator does not accept observation weights. In 1b, therefore, each state's SDID estimate is unweighted,

Table D2—Late cohorts (first treated 2017–2019): event study. Absorbing vs isolated counties

	ln distance		ln PCPs per 1,000		ln mortality		ln ag. GDP	
	DiD	SDID	DiD	SDID	DiD	SDID	DiD	SDID
$t = -4$	–	0.0001	–	−0.017	–	0.0045	–	0.494
		(0.0028)		(0.032)		(0.0114)		(0.331)
$t = -3$	0.0002	0.0001	0.116	−0.025	0.0191	−0.0039	−0.068	0.360
	(0.0002)	(0.0028)	(0.075)	(0.033)	(0.0416)	(0.0148)	(0.479)	(0.279)
$t = -2$	0.0002	0.0001	0.024	0.027	−0.0122	0.0079	0.092	−0.217
	(0.0003)	(0.0028)	(0.061)	(0.026)	(0.0396)	(0.0121)	(0.295)	(0.296)
$t = -1$	0.0001	−0.0000	0.035	−0.008	0.0012	−0.0025	−0.014	0.235
	(0.0001)	(0.0028)	(0.028)	(0.017)	(0.0198)	(0.0128)	(0.262)	(0.448)
$t = +1$	0.0056	0.0072	−0.024	−0.037	0.0228	0.0173	−0.036	0.284
	(0.0050)	(0.0066)	(0.020)	(0.033)	(0.0202)	(0.0174)	(0.180)	(0.412)
$t = +2$	0.0076	0.0096	−0.085	−0.103	0.0040	0.0160	−0.065	0.445
	(0.0073)	(0.0088)	(0.058)	(0.067)	(0.0235)	(0.0203)	(0.260)	(0.403)
$t = +3$	0.0124	0.0155***	−0.090	−0.142	−0.0060	0.0070	0.010	0.652
	(0.0114)	(0.0030)	(0.109)	(0.102)	(0.0344)	(0.0140)	(0.430)	(0.476)
ATT	0.0076	0.0097	−0.057	−0.080	0.0109	0.0147	−0.033	0.447
	(0.0070)	(0.0062)	(0.046)	(0.052)	(0.0211)	(0.0132)	(0.264)	(0.421)
Treated counties	35		35		35		12	
SDID states		9		9		9		2

Source: 41_CompareFailAbsorbIsolated.do with CMP_COHORT late, absorbing vs isolated. Treated: absorbing counties, from the first year they absorb a failing market's zip code; includes urban absorbing counties. Only cohorts first treated 2017–2019 are kept; controls are the same isolated counties as in the main comparison. Sample, estimators and missing-data rules otherwise as in Table B2. The 2017 cohort is observed for three post-treatment years, so the event study stops at $t = +3$ and ATT is the average of the three post-treatment effects. $t = 0$ is the last year before treatment and is normalized to zero; `did_multiplegt_dyn` allows no more placebos than effects, so the DiD has no $t = -4$ (–). Distance, PCPs per 1,000, all-cause deaths per 100,000 and agricultural GDP in logs. Treated counties: DiD switchers at $t = +1$.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; standard errors in parentheses.

and the weights enter when states are aggregated: each state is weighted by its number of treated counties times the mean population weight of those counties.

2. Lasso-selected donor pool (2a, 2b).

The main specifications use every never-treated county as a potential control. Here the control group is restricted to never-treated counties that resemble treated counties before 2010. A cross-validated logistic lasso (Belloni, Chernozhukov and Hansen, 2014; Ahrens, Hansen and Schaffer, 2020) selects predictors of ever being treated from a broad set of county characteristics. The candidates are economic, agricultural, health-care supply, access, and rurality measures, each averaged over the 2001–2009 baseline. The penalty is the largest λ within one standard error of the cross-validation minimum, which selects 32 predictors. A post-lasso logit on those predictors gives each county a propensity score. Each treated county then keeps its five nearest never-treated counties on the logit of that score, and the pool is the union of those neighbors: 831 of the 1,268 never-treated counties. Specification 2a runs the DiD on this pool without state-specific trends, so the lasso rather than the state boundary defines the comparison. Specification 2b runs a single national SDID on the same pool, with a bootstrap clustered by state.

3. Lasso donors within state (3a, 3b).

Specification 3 combines the two ideas: comparisons stay within state, and within each state only lasso-matched controls are used. Each treated county keeps its five nearest never-treated counties *in its own state* on the same propensity score, leaving 757 never-treated counties. The lasso is not re-estimated by state, because most states have too few treated counties to fit one. Specification 3a runs the DiD state by state and 3b runs SDID state by state. Both are aggregated across states in the same way as the main SDID estimates.

Table D3—Sensitivity of the average effect (ATT). Failing vs isolated

	Weighted		Lasso		Lasso by state	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
	DiD	SDID	DiD	SDID	DiD	SDID
Distance to nearest hospital	0.0185	0.0200	0.0195	0.0221	0.0198	0.0238
	(0.0034)	(0.0038)	(0.0042)	(0.0053)	(0.0037)	(0.0039)
PCPs per 1000 residents	-0.0164	-0.0615	-0.0321	-0.0242	-0.0421	-0.0577
	(0.0152)	(0.0190)	(0.0132)	(0.0173)	(0.0157)	(0.0196)
Agricultural GDP	-0.0080	0.0294	0.0007	-0.0195	0.0039	0.0377
	(0.0555)	(0.0558)	(0.0412)	(0.0584)	(0.0454)	(0.0598)
Farm income – log, if positive	-0.0097	-0.0584	0.0756	-0.0239	0.0085	0.0091
	(0.0552)	(0.0413)	(0.0376)	(0.0472)	(0.0406)	(0.0407)
Farms	0.0005	-0.0447	-0.0180	-0.0456	-0.0100	-0.0277
	(0.0122)	(0.0205)	(0.0092)	(0.0170)	(0.0100)	(0.0239)
Farmers and farm workers	-0.0088	-0.0240	-0.0142	-0.0315	-0.0099	-0.0322
	(0.0280)	(0.0462)	(0.0214)	(0.0303)	(0.0267)	(0.0438)
All-cause mortality, all ages	0.0118	0.0151	0.0136	0.0164	0.0096	0.0127
	(0.0055)	(0.0049)	(0.0048)	(0.0067)	(0.0054)	(0.0048)

Failing vs isolated counties. 1a: did_multiplegt_dyn with state trends and weight(pop_weight). 1b: sdid_event (sdid) by state; states aggregated by switchers times the mean pop_weight of their treated counties. 2a: did_multiplegt_dyn without state trends; isolated controls restricted to the comparison's lasso donor pool. 2b: one national sdid_event on the same pool; bootstrap clustered by state. 3a and 3b: did_multiplegt_dyn and sdid_event state by state; isolated controls restricted to the 5 nearest in-state counties on the lasso propensity score; states aggregated by switchers. Outcomes with suppressed or missing years: the DiD columns use only counties not missing the outcome in any published year from 2005 to 2019. 90% intervals throughout; standard errors in parentheses.

Table D4—Sensitivity of the average effect (ATT). Absorbing vs isolated

	Weighted		Lasso		Lasso by state	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
	DiD	SDID	DiD	SDID	DiD	SDID
Distance to nearest hospital	0.0023	0.0062	0.0055	0.0062	0.0057	0.0075
	(0.0014)	(0.0023)	(0.0021)	(0.0024)	(0.0022)	(0.0023)
PCPs per 1000 residents	-0.0230	0.0021	0.0101	0.0075	-0.0102	0.0009
	(0.0231)	(0.0298)	(0.0226)	(0.0163)	(0.0269)	(0.0281)
Agricultural GDP	0.1460	0.2377	0.0343	0.0262	-0.0410	0.0646
	(0.1302)	(0.1329)	(0.1025)	(0.0882)	(0.1095)	(0.1285)
Farm income – log, if positive	0.1718	0.1375	-0.0483	-0.0229	0.0171	0.0630
	(0.1414)	(0.0875)	(0.0933)	(0.1067)	(0.0912)	(0.0930)
Farms	0.0127	0.0111	-0.0037	-0.0121	0.0180	0.0111
	(0.0138)	(0.0250)	(0.0185)	(0.0251)	(0.0207)	(0.0398)
Farmers and farm workers	0.0098	0.0456	-0.0149	-0.0006	-0.0381	-0.0025
	(0.0283)	(0.0578)	(0.0318)	(0.0434)	(0.0390)	(0.0800)
All-cause mortality, all ages	-0.0177	-0.0010	-0.0106	0.0050	-0.0182	-0.0012
	(0.0082)	(0.0079)	(0.0075)	(0.0053)	(0.0084)	(0.0079)
Services offered by the nearest hospital	0.0099	0.0474	0.0490	0.0354	0.0414	0.0114
	(0.0176)	(0.0333)	(0.0238)	(0.0308)	(0.0300)	(0.0368)
Nearest hospital: patients rating 9-10	0.030	-0.735	-0.481	-1.296	-0.024	-0.814
	(0.436)	(0.617)	(0.552)	(0.818)	(0.582)	(0.677)
Nearest hospital: 30-day readmission	0.1467	0.1013	0.0887	0.0674	0.1408	0.1901
	(0.0988)	(0.1000)	(0.0786)	(0.0977)	(0.0858)	(0.1107)
HSA population	0.0695	0.0857	0.0774	0.0445	0.0818	0.0822
	(0.0272)	(0.0471)	(0.0318)	(0.0387)	(0.0355)	(0.0404)
Patient pressure: HSA population per hospital bed	0.1282	0.1953	0.1880	0.1992	0.1940	0.2048
	(0.0253)	(0.0314)	(0.0245)	(0.0227)	(0.0267)	(0.0305)
Occupancy rate of the nearest hospital	0.0097	0.0100	0.0062	0.0114	0.0045	0.0095
	(0.0075)	(0.0098)	(0.0063)	(0.0088)	(0.0065)	(0.0093)

Absorbing vs isolated counties (urban absorbers included). 1a: did_multiplegt_dyn with state trends and weight(pop_weight). 1b: sdid_event (sdid) by state; states aggregated by switchers times the mean pop_weight of their treated counties. 2a: did_multiplegt_dyn without state trends; isolated controls restricted to the comparison's lasso donor pool. 2b: one national sdid_event on the same pool; bootstrap clustered by state. 3a and 3b: did_multiplegt_dyn and sdid_event state by state; isolated controls restricted to the 5 nearest in-state counties on the lasso propensity score; states aggregated by switchers. Outcomes with suppressed or missing years: the DiD columns use only counties not missing the outcome in any published year from 2005 to 2019. 90% intervals throughout; standard errors in parentheses.