The Role of Hospital Networks in Individual Mortality*

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Abstract

Narrow hospital networks have proliferated in health systems with managed care competition. In this paper, we investigate the causal effect of hospital network breadth on patient mortality. We leverage insurer terminations and subsequent hospital terminations for vertically integrated hospitals to identify this effect. We use data from the Colombian healthcare system where the largest health insurer and its hospitals were terminated by the end of 2015. Findings show that broad-network insurers reduce patient mortality because they include high-quality hospitals and can treat more health conditions. Our results suggest that in a setting without price competition, access to health care through a few insurers with broad networks is better for patient health than access to health care through many insurers with narrow networks.

Keywords: Mortality, Hospital networks, Health Insurance, Healthcare cost.

JEL codes: I10, I11, I13, I18.

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1 Introduction

A long-standing question in health policy is how to deliver universal health insurance coverage and how to guarantee appropriate access to health care. There are examples of countries that successfully deliver universal coverage through a single-payer system, such as Canada and the United Kingdom. Other countries do it through private insurers, such as Switzerland, and others through a combination of private and non-for-profit funds, like Germany. Although these health systems have different insurance market structures, they share a common feature: guaranteeing access to care through a complete network of hospitals. In this paper, we study the role that hospital network breadth has on access to care and health outcomes beyond insurance coverage. This is relevant for health systems where competition between private insurers has led to a proliferation of narrow-network plans (Dafny, Hendel, and Wilson, 2015).

There is a growing literature that studies the incentives behind insurers' decision to offer network breadth. For example, insurers may establish narrow networks to achieve a better bargaining position relative to hospitals (Ho and Lee, 2019; Ghili, 2022) or to avoid unprofitable enrollees (Serna, 2023; Shepard, 2022). However, there are fewer studies on the welfare and utilization effects of broad hospital networks (Atwood and Sasso, 2016), or more generally on whether there is a link between insurer or hospital market structure and patient health. In this paper, we estimate the causal effect of network breadth on mortality and distinguish the importance of insurance coverage vis-à-vis hospital coverage.

We study these questions in the context of the Colombian healthcare system. This system has near-universal coverage and provides access to a national health insurance plan through private insurers. Insurers compete mainly on the breadth of their hospital networks, but all other elements of the national plan are regulated by the government, including premiums and cost-sharing.[1] Importantly, drastic changes in this health system provide valuable exogenous variation for the purposes of our paper. In December 2015, the government terminated the largest health insurer in the country, called SaludCoop, and the hospitals which were vertically integrated with it. The termination induced exogenous shocks to consumers' choice set of insurers and hospitals, which we exploit to identify the causal effect of interest.

We have the universe of individual-level enrollment and mortality data from 2012 to 2019. We complement this data with health claims from half of the country enrolled in the contributory system and with data on insurers' hospital network inclusions for the sample period. We use this data to show descriptive evidence of the impacts of the termination. SaludCoop's enrollees were transferred to an incumbent insurer called Cafesalud, which had a 3 percent market share. These enrollees had to remain with Cafesalud for 90 days before they could switch. We see evidence that the rest of insurers responded to the termination by narrowing their networks potentially as a risk selection mechanism, or to avoid unprofitable switchers. The termination also reduced the country's hospital capacity, which generated a congestion effect at remaining insurers.

We explore this congestion effect at insurers other than SaludCoop and Cafesalud in a difference-in-differences event study framework, comparing enrollees in municipalities where SaludCoop (and its hospitals) operated versus those where it didn't, before and after the termination.[2] Our findings show that individual mortality increased 25 percent after the termination, an effect that is persistent over time. Most of the mortality effect is explained by individuals with chronic health conditions who

[1] Insurer competition in network breadth involves competition on other dimensions such as wait times and extent of health care fragmentation.

[2] The congestion effect also exists in municipalities where SaludCoop operated but not its hospitals, as long as the remaining insurers covered different hospitals from the ones SaludCoop used to cover.

see their treatments interrupted. Consistent with a congestion effect, we find not only that insurers drop hospital coverage, but also that each provider renders 10 to 40 more consultations after the termination.

To complement the findings about mortality and hospital networks, we study the impacts on different types of claims. Our results are in line with reduced access to care after the termination. We find that the average consumer made substantially fewer claims, without any impacts on health care cost. This suggests that claim prices increased after the termination and potentially that providers' gains in bargaining power overcompensated insurers' threats of exclusion. The decrease in utilization reinforces the idea that reductions in hospital coverage must have been substantial in order for each provider to experience a congestion effect even when individuals are making fewer claims. We find that the number of claims for services associated with the prevention or early detection of chronic conditions also decreased after the termination. These services include visits to the specialist, imaging, A1C tests for diabetics, and breast cancer screening for women.

In the last part of our paper, we quantify the causal effect of hospital network breadth on mortality and investigate the relative importance of insurance versus hospital coverage. This is a challenging exercise given that consumers choose their insurer and in-network hospital non-randomly. SaludCoop's termination gives us ideal quasi-experimental variation in insurer and hospital choice sets to identify this causal effect. Using an instrumental variables regression, we find that broad hospital networks reduce patient mortality. An interquartile-range increase in network breadth reduces mortality by nearly 8 per 1,000 individuals.

We show that while municipalities with the presence of SaludCoop saw an increase in mortality after the termination, those that had SaludCoop hospitals in addition to the insurer experienced a mortality effect that was 50 percent larger. These results suggest that even though insurance coverage matters for patient health, guaranteeing appropriate access to hospitals has stronger effects on health outcomes. Put differently, ensuring access to broad hospital networks even through a few insurers is better for patient health than having access to narrow hospital networks through many insurers. We find that broad-network insurers matter for health outcomes because they have greater variety of hospital specialities and are more likely to include high-quality hospitals compared to narrow-network insurers.[3]

Our paper contributes to the literature on the impact of insurance coverage on health and market outcomes. Several papers in this line of research focus on the effects of insurance expansions on utilization, costs, and health (e.g., Miller, Johnson, and Wherry, 2021; Wherry and Miller, 2016; Sommers, Baicker, and Epstein, 2012). Other papers look at the effects of insurer terminations on similar outcomes (e.g., Politzer, 2021). Abaluck, Caceres, Hull, and Starc (2021) show for example that insurers with low mortality rates reduce your likelihood of dying and this effect is causal. In addition to showing how insurer coverage affects mortality, our paper provides evidence on hospital network breadth as the mechanism by which the insurer can impact patient health.

This paper also contributes to the literature on insurer competition in hospital networks and its regulation. Some papers study the relation between hospital network breadth and premiums (Ho and Lee, 2017; Dafny, Hendel, Marone, and Ody, 2017; Dafny et al., 2015) and negotiated prices for health services (Ghili, 2022; Ho and Lee, 2019; Liebman, 2018; Ho, 2009). The work that analyzes regulation of insurer competition in the form of network adequacy rules is less abundant and most of it in the context of Medicaid Managed Care (e.g., Zhu, Polsky, Johnstone, and Mc-

[3]This finding goes in line with descriptive evidence of physician network inclusions in the medical literature (Yasaitis, Bekelman, and Polsky, 2017).

Connell, 2022; Zhu, Breslau, and McConnell, 2021). Yet several papers highlight the importance of out-of-network care for different market outcomes (Cooper, Scott Morton, and Shekita, 2020; Prager and Tilipman, 2020). By showing that broad hospital networks have a negative causal effect on individual mortality, our paper contributes to the debate about regulating insurer competition on networks to achieve broad hospital coverage.

The remainder of this paper is structured as follows. Section 2 describes the institutional background and SaludCoop's termination. Section 3 introduces our data. Section 4 presents our empirical strategy. Section 5 shows event study results on mortality, networks, and health claims. Section 6 presents our empirical approach and results on the causal effect of network breadth on mortality. Section 7 discusses mechanisms by which network breadth affects patient health. Section 8 concludes.

2 Institutional Background

We study the effect of hospital networks on patient mortality in the context of the Colombian statutory health care system. This system is divided into a contributory and a subsidized regime. The first covers the half of the population in the country who are formal workers and pay payroll taxes. The second is fully funded by the general budget. Nearly 99.6 percent of the population is covered by the system. Both contributory and subsidized insurance enrolees have access to the same national health insurance plan through private insurers. Almost every aspect of this plan is regulated by the government, except for hospital networks: insurers in Colombia can choose which hospitals to cover for each health service included in the national insurance plan.[4]

[4] For a more detailed description of the Colombian health care system see Serna (2023)

Importantly, enrolees pay zero insurance premiums. Instead, at the beginning of every year, insurers receive per-capita transfers from the government that are risk-adjusted for sex, age, and location. At the end of every year, insurers are also compensated for their enrollees' health based on a coarse list of diagnoses. After all risk-adjusted transfers are made substantial risk selection incentives remain. Serna (2023) shows that insurers respond to these incentives using their hospital networks. Hospital networks and selection incentives are determined in equilibrium as a result of insurer and hospital competition. Shocks to competition, such as insurer terminations, can therefore generate new network arrangements that may impact patient health.

The Colombian government can terminate insurers if they divert resources away from the health care system, have low enrollee satisfaction scores based on surveys conducted by the Ministry of Health, or cannot maintain their risk-based capital requirements.[5] In December 2015, the government terminated the largest health insurer in the country, SaludCoop, due to political considerations and engagement in illegal activities. SaludCoop had a national market share of 20 percent and participated in both the contributory and the subsidized regimes. Its board of directors diverted nearly one billion pesos to investments outside the health system, engaged in financial malpractice, and submitted false health claims to the government for reimbursement. The CEO and board of directors were fined 50 monthly minimum wages, prohibited to work in public office, and prohibited from participating in public auctions for at least 18 years.[6]

SaludCoop's enrollees were transferred to an incumbent insurer called Cafesalud, which also participated in the two health care regimes. At the time of the termination,

[5] See Decree 780 of 2016.

[6] A description of the termination process, fines, and investigation can be found in Resolution 002414 of 2015 and Bulletin 1103 of 2012 from the Procuraduría General de la Nación.

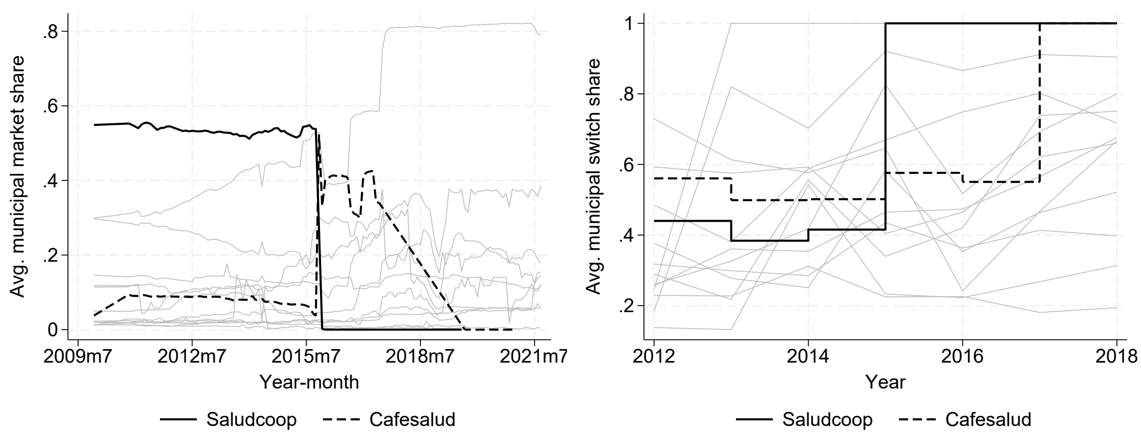
Cafesalud had a national market share of 3 percent. The government chose Cafesalud as the reassignment insurer because it had presence in almost the same municipalities as SaludCoop did (see appendix figure 1).

Enrollees to the terminated insurer had to remain in Cafesalud for a period of 90 days, from January to March 2016. After these 90 days, enrollees were allowed to switch their insurer. During the reassignment period, Cafesalud had to guarantee access to health care for SaludCoop's enrollees at the hospitals that SaludCoop used to cover in its network, in addition to the hospitals already in Cafesalud's network. To facilitate this transition, the government made a $70 million loan to Cafesalud.

Figure 1: Municipal Market Share and Switching Rate

(A) Municipal market shares

(B) Switching rate



Note: Panel (A) average municipal market share per insurer from 2009 to 2021. Panel (B) shows the fraction of $t-1$ enrollees who switched out of each insurer in year t.

Panel A of figure 1 shows the average municipal market share per insurer in the contributory regime. We emphasize SaludCoop and Cafesalud in black, and the rest of insurers are illustrated in gray. The figure shows that SaludCoop (solid black line) covered an average of 57 percent of enrollees in a municipality in the years prior to its termination. Cafesalud (dashed black line) had an average municipal market share of less than 2 percent before the termination, 40 percent three years after the termination, and was itself terminated in 2019. We thus limit our analysis to the years 2012 to 2019.[7]

Panel B of figure 1 shows the average municipal switching rate per insurer.[8] A switcher is an individual who either moves from the contributory to the subsidized regime or who switches insurers conditional on remaining within the same regime. In Colombia, there is no designated open enrollment period, hence consumers can switch their insurer at any moment throughout the year as long as they have been enrolled for at least 12 months with their incumbent insurer.

Consistent with the termination, all of SaludCoop's enrollees switched out of this insurer by 2016 (solid black line). The fact that SaludCoop's switching rate before the termination is stable over time suggests no preemption. That is, we do not see enrollees disproportionately switching out of SaludCoop before the termination, potentially responding to termination announcements made before December 2015. Before the termination, Cafesalud (dashed black line) had an average switching rate of approximately 57 percent, which increased to 80 percent by 2017. The jump in Cafesalud's switching rate suggests that around 20 percent of SaludCoop's enrollees switched out of Cafesalud after the 90-day period.

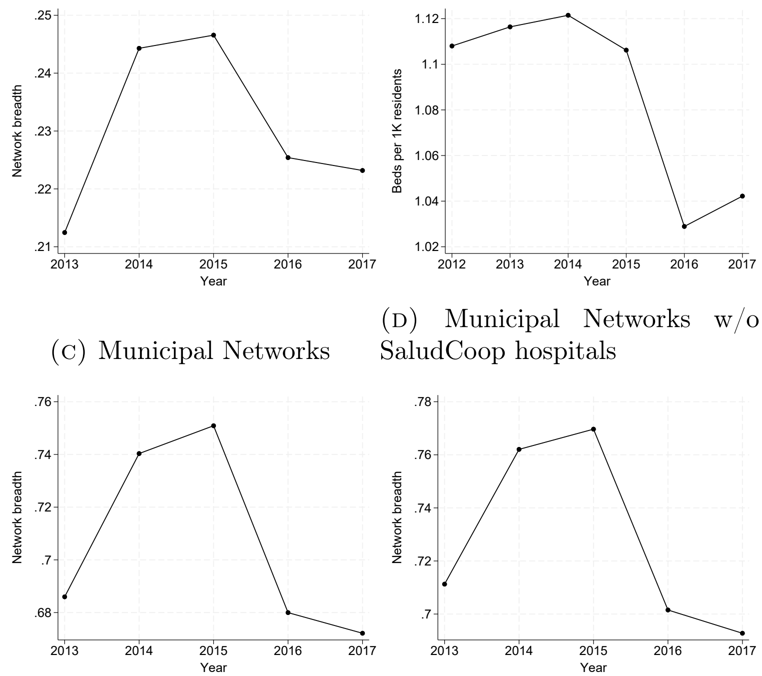
SaludCoop's termination forced substantial changes in the provision of health insurance and health care in Colombia. Fines and debts that resulted from this process continue to be paid to this day.[9] Not only did the termination reduce the number of insurers in each market, but also the country's hospital capacity. As part of the termination, SaludCoop was forced to sell the hospitals and clinics that it

[7]The figure also shows that insurer EPS037 (Nueva EPS) had an average market share of around 80 percent after 2018. In most municipalities in the Amazon and East regions of the country, Nueva EPS is either a monopoly or competes in a duopoly market.

[8]The switching rate in year t is calculated as the fraction of enrolees in year t that switch out of their insurer by year $t+1$.

[9]See Resolution 252 of 2021 by the Ministry of Health.

Figure 2: Trends in Network Breadth and Hospital Beds



(a) Statal Networks (b) Beds

(c) Municipal Networks (d) Municipal Networks w/o SaludCoop hospitals

Note: Panel (A) shows average statal network breadth. Panel (B) shows the average number of hospital beds per 1,000 residents across municipalities. Panel (C) shows average municipal network breadth. Panel (D) shows average municipal network breadth excluding municipalities where SaludCoop's hospitals operated.

owned or was vertically integrated with. These hospitals were not allowed to operate until they were sold to other providers, which generated disruptions in the provision of care.

In 2014, SaludCoop owned 38 hospitals and clinics, which accounted for 2,354 hospital beds. SaludCoop operated hospitals in 31 municipalities (out of 1,120 in the country) and in 12 of those there were insurers other than SaludCoop and Cafesalud which covered SaludCoop hospitals. These insurers accounted for approximately 1.5 million enrollees, for whom hospital access changed after the termination. Apart from the 31 municipalities where SaludCoop operated with hospitals, it also operated in 427 municipalities without hospitals.

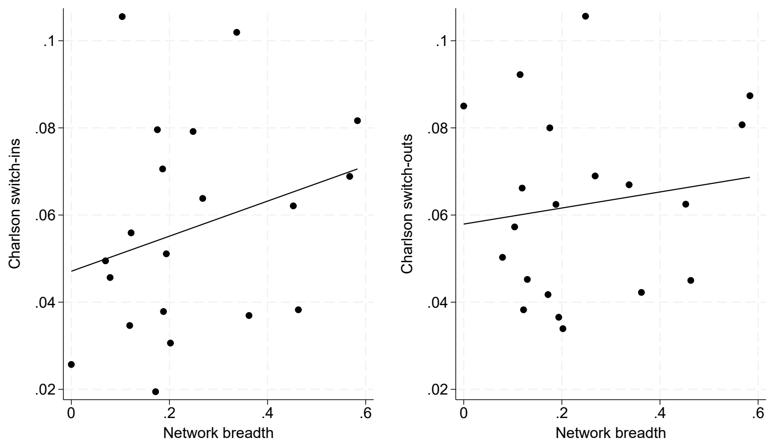
We provide descriptive evidence of these facts in figure 2. Using hospital network data from the National Health Superintendency, we create a measure of network breadth defined as the fraction of hospitals in a state or in a municipality that are covered by an insurer. Panel A of figure 2 shows that average network breadth falls 1 percentage point in 2016 relative to 2015, roughly a reduction of one hospital in the average network in a state. Reductions in network breadth after the termination are larger at the municipal level as seen in panel C. This is not a mechanical effect of SaludCoop's hospitals closing, since reductions in network breadth are of similar magnitude when we exclude municipalities where these hospitals operated in panel D. Panel B also shows that the average number of hospital beds per 1,000 residents decreased 7 percent from 2015 to 2016.

The trend in network breadth and the termination of vertically integrated hospitals raise several questions. For instance, why do networks respond immediately after the termination? Or why do we see network breadth decrease after the termination even in markets without SaludCoop hospitals? To answer the first question, we note that insurers and hospitals in Colombia negotiate service prices and network inclusions typically at the beginning of every calendar year, hence we can expect changes in networks to happen as soon as of the beginning of 2016.

To answer the second question, we rely on the finding in Serna (2023) and Shepard (2022) that insurers respond to adverse selection by providing narrow networks. If we see network breadth decrease after the termination, then it must be that most of SaludCoop's enrollees who switched out of Cafesalud where in worse health status than those who did not switch. As a result of a greater pool of sick "new enrollees", incumbent insurers may have responded by narrowing their networks. We find confirming evidence of this adverse selection argument in figure 3. The figure shows a positive correlation between average network breadth and average Charlson index among switch-ins in left-hand panel and among switch-outs in the right-hand panel.

This correlation is greater in magnitude for switch-ins consistent with adverse selection on network breadth.

Figure 3: Network Breadth and Switchers' Health Status



The purpose of our paper is to study the impact of hospital network breadth on patient health outcomes leveraging insurer and hospital terminations. The fact that SaludCoop's termination affects two dimensions of consumer choice allows us to quantify the relative importance of health insurers and hospitals for patient health. This is an important aspect of health systems that debate on how to deliver insurance coverage: is having fewer insurers with broad networks better than having many insurers with narrow networks?

Answering these questions is challenging given that insurer and hospital choices are characterized by adverse selection. Our main challenge has to do with identification: if we see mortality change after the termination in municipalities where SaludCoop (or its hospitals) operated, is it because (i) networks become narrower? (ii) enrollees who switch choose an insurer and subsequent in-network hospital non-randomly? or (iii) enrollees who switch are in worse health status to begin with? The next sections describe the data and main empirical approach that we use to identify the causal effect of interest and quantify the relative impacts of insurer and hospital coverage.

3 Data

We have a snapshot of enrollment data for every June from 2012 to 2019, which correspond to 4 years before and 4 years after the termination of SaludCoop. Our enrollment data comprises all enrollees to the contributory and the subsidized regimes, nearly the entire population in the country. Because we do not see enrollment every month, we assume that if an individual is enrolled with insurer A in June 2012, they remain with this insurer every month until June 2013 when we see the next enrollment snapshot.[10] The enrollment files have information on the individual's sex, age, municipality of residence, and insurer.

At the end of every year, insurers in the contributory and the subsidized regimes report all of their enrollees' health claims to the government. The government uses this data every year to update the risk-adjusted transfers and imposes several data quality filters to do so. We have claims data only for insurers in the contributory regime that pass these quality filters from 2012 to 2019. Although most insurers remain in our sample during the period of analysis (unless they are terminated), we do not have claims data for Cafesalud after the termination.

The claims data correspond only to enrollees in the contributory regime, which comprise approximately half of the population in the country. We do not have claims data for individuals in the subsidized regime. The claims data reports date in which the claim was filed, enrollee identifier, associated ICD-10 diagnosis code, provider that rendered the claim, insurer that reimbursed it, and negotiated service price between

[10]Conditional on staying within the same insurance regime and having continuous enrollment spells, the assumption that individuals remain enrolled with their insurer during the 12 months from June to June is consistent with the low switching rate reported in Serna (2023).

insurer and provider.

From the National Administrative Department of Statistics, we obtain individual level mortality from 2012 to 2019. Anonymous individual identifiers are the same across datasets, allowing us to merge mortality with enrollment and claims. The mortality data reports date of death, cause of death or diagnosis associated with the event, manner of death (fetal, violent, or natural), an indicator for whether the individual died at the hospital or elsewhere, and provider where the death occurred.

We construct our mortality outcome as an indicator for whether the individual died in each year from June to June, given that we observe enrollment in that month. The indicator takes the value of zero if the person is alive that year, and takes the value of one if they die that year. After the individual dies, they disappear from our data. We exclude violent deaths since they are not necessarily associated with the provision of health care, as well as fetal and maternal deaths. Finally, we have data on hospital networks from 2012 to 2017 from the National Health Superintendency. This data does not report hospital networks per service, but overall hospital network inclusions.

For our analysis, we compare mortality patterns across (treated) enrollees living in municipalities where SaludCoop was present at the time of the termination, against (control) enrollees living in municipalities where SaludCoop was not present. To guarantee that treated and control groups are similar before the termination, we restrict our data in several ways. These restrictions help control for differential adverse selection patterns across treatment status before the termination, similar to Politzer (2021).

First, we exclude individuals who are enrolled to SaludCoop or Cafesalud before SaludCoop's termination, so our results are reflective of changes in patient mortality at the rest of insurers. We label the effect at these other insurers as the "congestion effect." Insurers' hospital networks may become congested if they used to cover SaludCoop hospitals which are eventually terminated. Even in markets where SaludCoop operated but had no hospitals, we might expect a congestion effect as long as incumbent insurers cover different hospitals than the ones SaludCoop used to cover prior to its termination.

Second, we keep individuals with continuous enrollment spells, who did not switch their insurer during the sample period, and who did not move across municipalities before the termination. These restrictions limit selection on insurer choice that is endogenously caused by changes in insurer characteristics such as the breadth of their hospital network. Moreover, by requiring that individuals do not switch their insurer, we allow for them to have sufficient interaction with their insurer and its network of hospitals. This way any disruption of care such as those associated with an insurer termination would have stronger effects on patient health. Fourth, we drop individuals for whom we see enrollment data after they die. Appendix table 1 shows the number of observations that result after imposing each sample restriction.

4 Empirical Strategy

We start our analysis by estimating the effect of insurer terminations on mortality. We then quantify the impact of the termination on hospital networks. Finally, we connect the two analyses to get at the causal effect of network breadth on mortality. Our empirical strategy in the first part of the analysis consists of a difference-in-differences (*did*) event study design. We compare mortality between enrollees living in municipalities where SaludCoop had presence the year of the termination (treated group) against enrollees living in municipalities where SaludCoop did not have presence (control group), before and after the termination. The unit of treatment is therefore a municipality.

Our regression of interest is:

$$y_{imt} = \sum_{\substack{k=-3 \\ k \neq -1}}^{3} \beta_k 1\{t - t^* = k\} \times T_m + x'_{it}\lambda + \gamma_m + \gamma_t + \varepsilon_{imt} \tag{1}$$

Here y_{imt} is the outcome of individual i in municipality m in year t, T_m is an indicator for whether municipality m had presence of SaludCoop the year of the termination, t^* is the year when SaludCoop is terminated, x_{it} is a vector of (potentially time-varying) patient characteristics including dummies for their insurer, sex, age, and an indicator for being diagnosed with a chronic disease. Finally, γ_m and γ_t are municipality and year fixed effects, respectively.

Although SaludCoop's termination happened in December 2015, all (first-stage) effects on enrollment are observed starting in 2016 as seen in figure 1. The relative time indicators in equation (1) are thus constructed relative to 2016, and the omitted category is 2015. The coefficients β_k measure the average treatment effect on the treated in year k relative to the termination. Because the termination happens at the same time for all individuals in our treated group, we do not worry about staggered implementation. Finally, we cluster our standard errors at the municipality level.

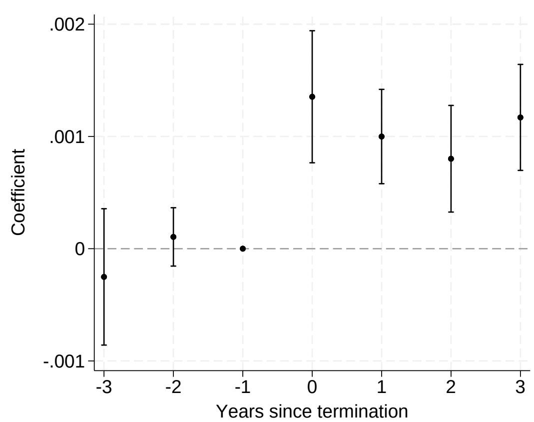
Identification of the dynamic treatment effect on the treated relies on treated and control groups being on similar mortality trends prior to the termination. Identification is threatened by consumers non-randomly sorting into insurers based on characteristics that are unobserved to us and that change within individuals and over time. Selection bias of this style would result in a violation of the classic parallel pre-trend assumption in *did* designs, which we can easily corroborate from our estimates.

5 The Impact of Insurer Terminations

5.1 Individual Mortality

Figure 4 presents coefficients and 95 percent confidence intervals of the event study that compares individual mortality in treated municipalities versus control municipalitites. We label this exercise the "congestion effect", because changes in mortality at insurers other than SaludCoop or Cafesalud are potentially explained by their hospital networks becoming more congested with SaludCoop's enrollees after the termination.[11]

FIGURE 4: Congestion Effect



Note: Figure shows event study coefficients and 95 percent confidence intervals of enrollee mortality. Specification includes demographic controls, and municipality, year, and insurer fixed effects. Standard errors are clustered at the municipality level. Sample is restricted to individuals who do not switch insurers. We exclude individuals enrolled with SaludCoop and Cafesalud. Treatment is defined as municipalities where SaludCoop was present in 2015.

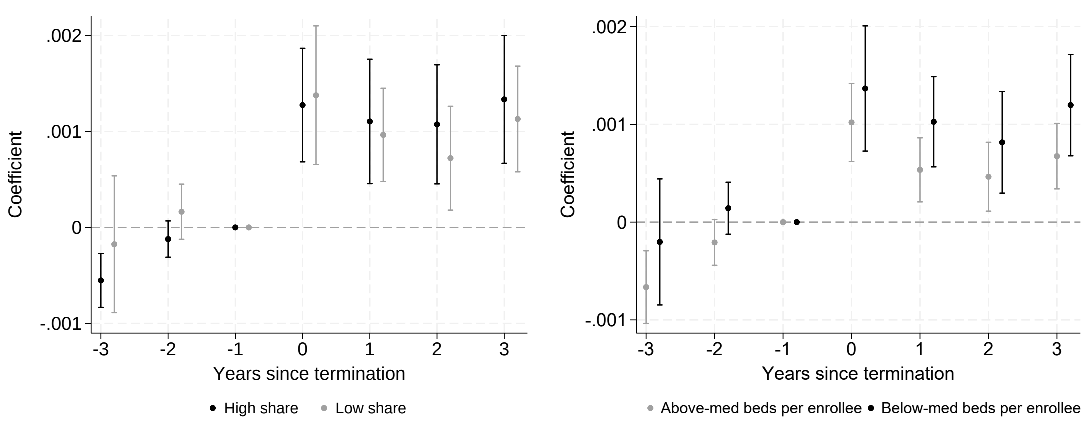
We find substantial congestion effects. Prior to the termination, individuals in municipalities where SaludCoop was present and those where it wasn't had similar mortality trends. The year of the termination mortality increases 1.3 per 1,000 in treated municipalities, roughly a 25 percent increase over baseline mortality in 2015. The mortality effects are likely not driven by transitory disruptions but they are

[11] Appendix B provides a description of what happened to SaludCoop's enrollees.

persistent over time. We find a positive mortality effect in 2019, suggesting that the Colombian health care system had not reached a new steady state by then. The magnitude of our estimates are in line with other studies on the effect of insurance coverage on mortality (Miller et al., 2021; Abaluck et al., 2021). Our results are robust to excluding the largest cities, Bogotá and Medellín, as seen in appendix figure 8. Appendix table 2 reports the associated coefficients and standard errors.

FIGURE 5: Congestion Effect by Market Characteristics

(A) SaludCoop's Market Share (B) Number of Beds per Resident



Note: Panel A shows event study coefficients and 95 percent confidence intervals of enrollee mortality conditional on treated municipalities where SaludCoop had more than 50 percent market share in black, and conditional on treated municipalities where it had at most 50 percent market share in gray. Panel B shows event study coefficients and 95 percent confidence intervals of enrollee mortality conditional on treated municipalities with above and below median total number of beds per resident during 2015 in black and gray, respectively. All specifications include municipality, insurer, and year fixed effects. Standard errors are clustered at the municipality level.

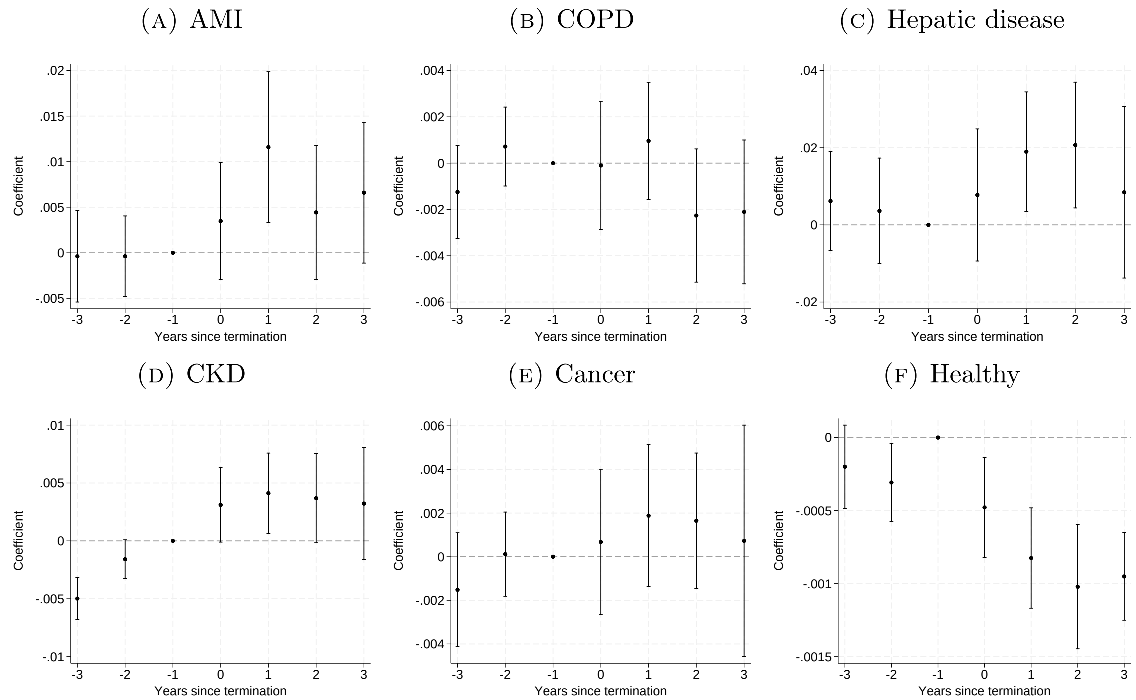
The congestion effect is similar across municipalities where SaludCoop had different market shares. Panel A of figure 5 shows that in municipalities where SaludCoop had more than 50 percent market share, individual mortality increased 0.13 percentage points every year after the termination relative to control units. This effect is similar in magnitude for those living in municipalities where SaludCoop had at most 50 percent market share. The congestion effect is also homogeneous across markets with above and below median total number of beds per resident in 2015, as seen in panel B of the figure. The homogeneous effects by size of SaludCoop suggest that changes in insurance coverage alone or which insurer provides it do not explain changes in mortality. Moreover, the homogeneous effects by total hospital capacity may reflect the fact that individuals do not have access to all the hospitals in a market but rather to those that their insurer covers. Hospital networks therefore may be an important determinant of patient health. We explore this argument later on.

5.2 Mortality by Subgroups

The fact that mortality increases significantly the year of the termination is unusual. Even though disruptions in health care happen immediately after forced switches such as those caused by insurer terminations (as shown in Politzer (2021)), we would have expected more delayed effects on patient mortality. To answer the question of why does mortality change immediately after the termination, we look at cause of death. We ask whether it is the case that individuals suffer from diseases where an interruption or disruption in care can be potentially fatal.

In figure 6 we estimate our event study specification conditional on individuals (treated and controls) who received a particular diagnosis at any point during the sample period. We obtain an individual's diagnoses using the ICD-10 codes that accompany their claims, which we also use to construct their Charlson index. These exercises are therefore conditional on patients who make claims in the contributory regime. We focus on conditions for which treatment interruptions may be fatal such as Acute Myocardial Infarctions (AMI), Chronic Obstructive Pulmonary Disease (COPD), hepatic diseases, Chronic Kidney Disease (CKD), and cancer. We also report results for the group of healthy individuals or those who had a Charlson index equal to zero every year. Coefficients and standard errors are reported in appendix table 3.

Figure 6: Congestion Effect by Diagnosis



Note: Figure shows event study coefficients and 95 percent confidence intervals conditional on patients who were diagnosed at any point during the sample period with Acute Myocardial Infarctions (AMI) in panel A, Chronic Obstructive Pulmonary Disease (COPD) in panel B, hepatic disease in panel C, Chronic Kidney Disease (CKD) in panel D, and cancer in panel E. Panel F uses the sample of individuals without diagnoses.

In all cases, except for COPD, we see that mortality increases the year or two after the termination. This effect is persistent over time for the group of enrollees with AMI, CKD, and Cancer, but falls to zero for those with hepatic diseases three years after the termination. For healthy individuals we find the opposite pattern in patient mortality. The year of the termination, healthy individuals in treated municipalities experience a significant decrease in mortality equal to 5 per 10,000. This reduction is more pronounced two years after the termination. The rapid response of mortality rates to SaludCoop's termination is therefore explained by individuals with chronic diseases who had their healthcare treatments interrupted.

5.3 Hospital Networks

The congestion effect on patient mortality after SaludCoop's termination could be explained by several mechanisms including: (i) insurers enrolling more individuals but covering the same number of hospitals in their networks, or (ii) insurers dropping hospitals from their networks after the termination. In this subsection, we analyze these mechanisms more systematically.

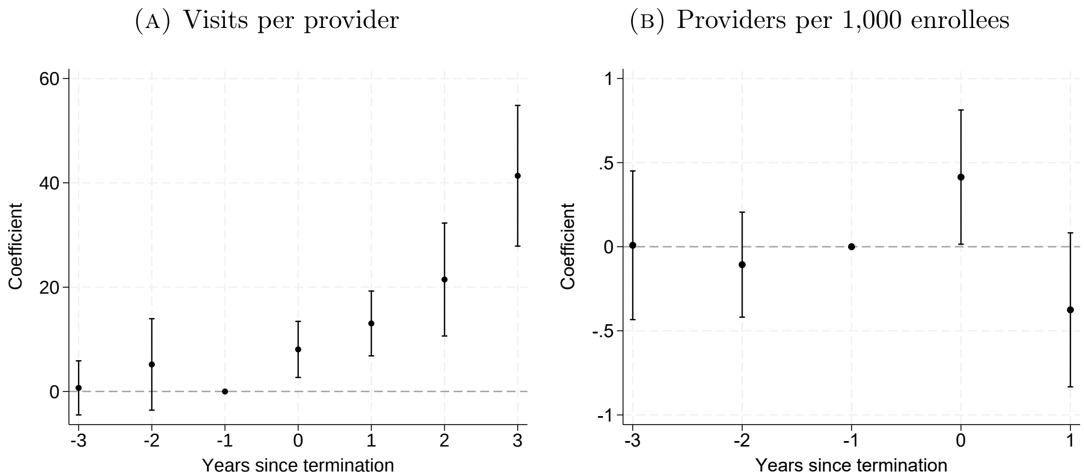
To provide evidence of the first mechanism, we use the claims data to construct the number of visits per provider. We observe this variable only for individuals enrolled in the contributory system. We collapse the claims-level data to the provider-insurer level, and compare municipalities where SaludCoop was present against those where it wasn't using a similar event study specification as in equation (1).[12] For the second mechanism, we use our network and enrollment data to construct the number of covered providers per 1,000 enrollees. An observation in this dataset corresponds to a combination of insurer, municipality, and year.

Figure 7 presents the results. Panel A shows that providers in municipalities where SaludCoop operated had approximately 10 more visits or consultations the year of the termination relative to providers in municipalities where SaludCoop did not operate. This congestion effect worsens over time, as providers in treated municipalities saw nearly 40 more visits than providers in control municipalities three years after the termination. The figure also shows that providers in treated and control municipalities had parallel trends in number of visits before the termination.

Panel B of the figure shows that insurers in municipalities where SaludCoop was present dropped around 0.4 providers per 1,000 enrollees one year after the termination. This effect represents roughly an 8 percent reduction relative to the average

[12] The majority of providers operate in a single municipality, since there are no large hospital systems in Colombia as there are in the US.

Figure 7: Sources of Congestion



Note: Panel (A) shows event study coefficients and 95 percent confidence intervals of number of visits per provider. Specification uses data at the provider-insurer-year level and includes municipality, insurer, provider, and year fixed effects. Standard errors are clustered at the municipality level. Panel (B) shows event study coefficients and 95 confidence intervals of providers per 1,000 enrollees conditional on having more than 100 enrollees throughout the sample period. Specification uses data at the insurer-market-year level and includes municipality and year fixed effects. In each specification, treatment is defined as municipalities where SaludCoop was present in 2015.

insurer in a control municipality during 2015. The figure also provides evidence of parallel pre-trends in coverage per enrollee across treated and control municipalities.

5.4 Health Claims

The reduction in the number of in-network providers is compatible with the idea that insurers engage in risk selection using their hospital networks. Leveraging strong insurer inertia among their current enrollees, incumbent insurers may drop provider coverage to potentially discourage enrollment from individuals previously enrolled to SaludCoop.

The bargaining literature suggests that insurers who were effective at narrowing their networks, would have negotiated lower prices with in-network providers. This is because providers' disagreement payoffs –defined as the profits they would enjoy from dropping an insurer– likely decreased after the termination. However, the congestion effect at each provider would also suggest that their bargaining power increased relative to insurers, which may lead to higher negotiated prices after the termination. Negotiated prices may also increase if each health claim is more severe due to delays in obtaining care. These arguments imply that the effect of insurer terminations on negotiated prices and health care costs is ambiguous. In this subsection we explore the impact of SaludCoop's termination on prices and claims for several health services.

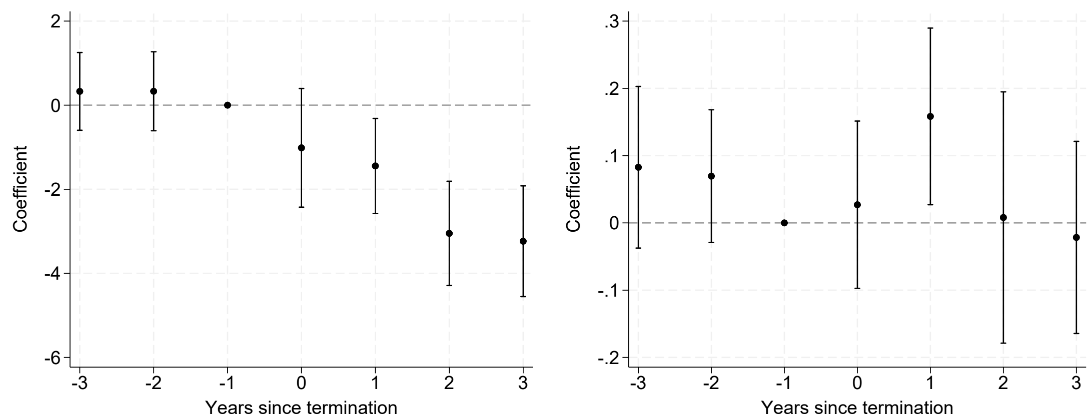
To conduct our analysis we use the claims data. Because the Ministry of Health imposes several data quality filters before releasing the data, we do not observe all insurers every year. In fact, only 5 out of the 16 private insurers that participated in the contributory regime during 2015 are observed throughout the sample period. This means that individual-level measures of utilization and costs will have substantial measurement error. To circumvent this issue we aggregate our data to the municipality-year by calculating averages across all individuals who reside in each municipality. Our analysis will therefore be indicative of changes in utilization and costs for the average enrollee in the contributory system after SaludCoop's termination.

Panel A of figure 8 shows that individuals in treated and control municipalities had parallel utilization patterns prior to the termination. A year after the termination, the average enrollee in treated municipalities made roughly 2 fewer health claims than control units. This reduction in the number of claims is much larger 3 years after the termination. Although utilization decreased significantly after 2015, the cost of the average enrollee did not change as seen in panel B of the figure. These findings imply that the price per claim increased after the termination.

In appendix figure 7 we estimate event studies on the price of CT scans and MRIs for which the average severity of a claim likely does not change after the termination. We find no changes in the price of these imaging services, suggesting that overall price increases can be explained by provider gains in relative bargaining power and

Figure 8: Impact of Congestion on Prices

(A) Claims (B) Cost



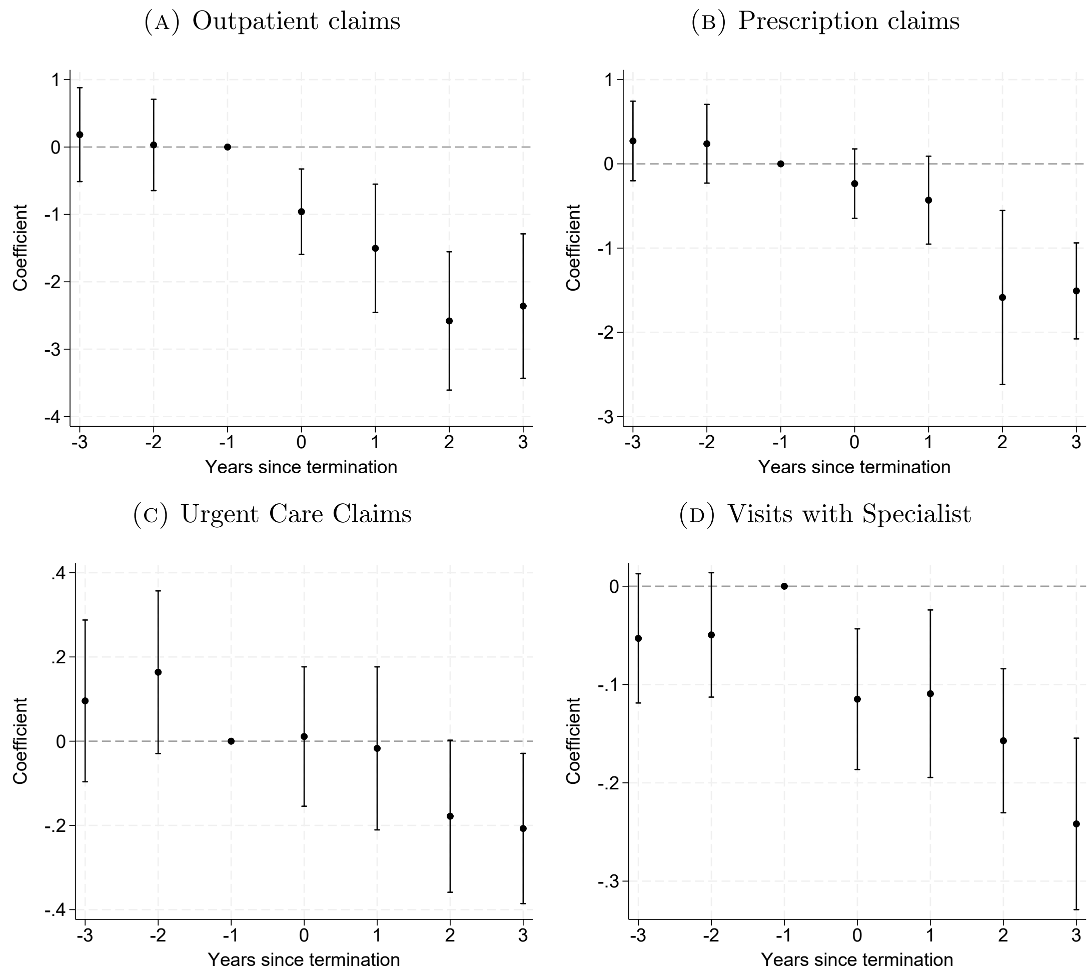
Note: Figure shows event study coefficients and 95 percent confidence intervals of annual number of claims in panel (A) and annual health care cost in millions of pesos in panel (B). Specifications use individual level data from enrollees in the contributory system aggregated or averaged to the municipality-year level. Treatment is defined as municipalities where SaludCoop was present in 2015.

by changes in the severity of claims.

Our results in figure 8 are not at odds with those reported in figure 7. In fact, they reinforce the importance of narrow networks in generating a congestion effect. The reduction in the number of covered providers in each municipality must be substantial in order to explain why each provider renders more visits even when the total number of claims is falling.

The reduction in utilization happens across different types of claims. Panel A of figure 9 shows that a year after the termination the average consumer in treated municipalities made 2 fewer outpatient claims, which explains the decline in total number of claims presented above. Likewise, panels B and C display substantial reductions in the number of prescription and urgent care claims between two and three years after the termination. We see that the average consumer filed 1.5 fewer prescription claims and 0.2 fewer urgent care claims around 2017. Finally, panel D shows that the average consumer in treated municipalities saw 0.25 fewer visits to the specialist right after the termination. Importantly, enrollees in treated and control municipalities had similar utilization trends across these types of claims prior to the termination. Reductions in utilization after 2015 may therefore be suggestive of consumers not receiving the type of care they need.

FIGURE 9: Impact of Congestion on Types of Claims

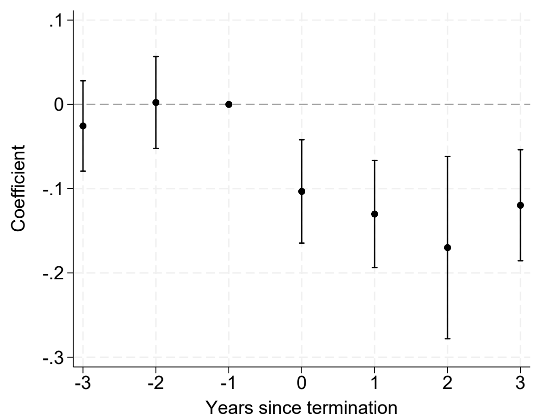


Note: Figure shows event study coefficients and 95 percent confidence intervals of outpatient claims in panel (A), prescription claims in panel (B), urgent care claims in panel (C), and visits with the specialist in panel (D). Specifications use individual level data from enrollees in the contributory system aggregated or averaged to the municipality-year level. Treatment is defined as municipalities where SaludCoop was present in 2015.

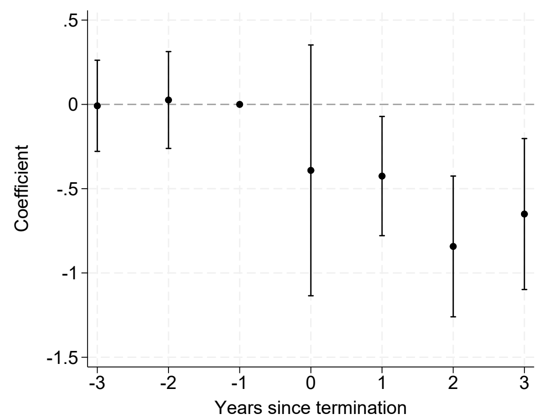
Consistent with this argument, we find that utilization of health services used for prevention or early detection of serious health conditions also decreased in treated municipalities relative to controls after the termination. Not only did treated and control units have statistically similar utilization patterns for these health services before the termination, but panels A and B of figure 10 show that the average consumer in treated municipalities made 0.2 fewer imaging claims and received 1 fewer lab test two years after the termination.

FIGURE 10: Impact of Congestion on Preventive and Diagnostic Aid

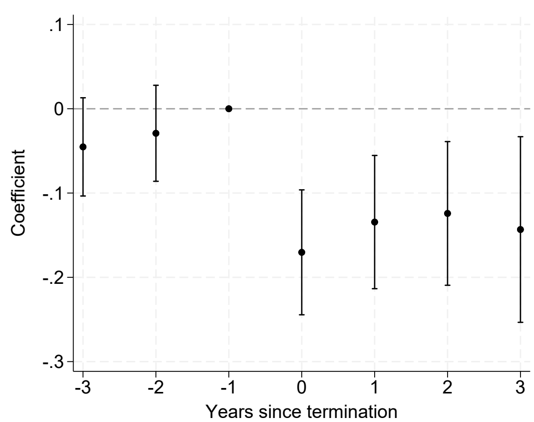
(A) Imaging



(B) Laboratory



(C) A1C tests



(D) Breast Cancer Screening

Note: Figure shows event study coefficients and 95 percent confidence intervals of imaging claims in panel (A), laboratory tests in panel (B), A1C blood tests in panel (C), and breast cancer screening in panel (D). Specifications use individual level data from enrollees in the contributory system aggregated or averaged to the municipality-year level. Treatment is defined as municipalities where SaludCoop was present in 2015.

Findings in the bottom panels of figure 10 also support the argument that prevention measures worsened after the termination. In panel C we find that the average diabetic in treated municipalities received 0.2 fewer A1C lab tests every year after the termination, a service that is required for adequate diabetes management. Additionally, panel D shows that the average woman in treated municipalities experienced a reduction of 1.5 percentage points in the likelihood of claiming services related to breast cancer screening, such as mammograms and breast magnetic resonance imaging.

6 The Causal Effect of Hospital Network Breadth

So far we have shown that SaludCoop's termination caused a significant increase in mortality and a significant reduction in hospital network breadth in municipalities where it operated relative to those where it didn't. In this section we link these two findings to estimate the causal effect of hospital network breadth on patient mortality. With concerns about access to care and recent proliferation of narrow-network insurers, it is important to understand the effects of hospital networks on patient health. More broadly, the quasi-experimental variation created by SaludCoop's termination provides an avenue to study the relative importance of insurance coverage vis-à-vis hospital network breadth.

Identifying the effect of network breadth on mortality is a challenging exercise because differences in mortality can be explained by individuals non-randomly selecting their insurer and their in-network hospital. For example, if patients have strong preferences for a high-quality hospital and this hospital is more likely to be covered under a broad network insurer, failure to account for hospital choice would yield an estimate of the effect of network breadth on mortality that is biased toward zero. Moreover, if unobservably healthy patients disproportionately enroll with narrow network insurers, then we would predict that narrow network plans reduce patient mortality when in fact these plans had a healthier population of enrollees to begin with.

To see how the bias from variation in hospital quality arises, consider a simple model of hospital choice where individual i's indirect utility from choosing hospital h in the network of insurer j in market m is:

$$u_{ijhm} = \xi_{hm} + \varepsilon_{ijhm}$$

Here ξ_{hm} captures hospital h's average quality and ε_{ijhm} is a preference shock assumed to follow a type-I extreme value distribution. Given the distribution of the preference shock, individual i's value for insurer j's network of hospitals G_{jm} is:

$$w_{ijm} = \log\Big(\sum_{h\in G_{jm}} \exp(\xi_{hm})\Big)$$

Let $|G_m|$ be the total number of hospitals in the market and $|G_{jm}|$ the number of hospitals in insurer j's network. The measure of network value derived from a hospital choice model relates to our measure of network breadth as follows:

$$w_{ijm} = \log\Big(\sum_{h\in G_{jm}} \exp(\xi_{hm})\Big) \geq \log\Big(\frac{1}{|G_m|}\sum_{h\in G_{jm}} \exp(\xi_{hm})\Big) \geq \frac{1}{|G_m|}\sum_{h\in G_{jm}} \log(\exp(\xi_{hm}))$$

$$= \frac{1}{|G_m|}\sum_{h\in G_{jm}} \xi_{hm} = \frac{|G_{jm}|}{|G_m|}\sum_{h\in G_{jm}} \frac{1}{|G_{jm}|}\xi_{hm} = \bar{\xi}_{jm}H_{jm}$$

where the second inequality uses Jensen's inequality and $\bar{\xi}_{jm} = |G_{jm}|^{-1}\sum_{h\in G_{jm}} \xi_{hm}$ is the average quality of the hospitals in insurer j's network. Ideally, the regression we want to estimate is

$$y_{imt} = \alpha\bar{\xi}_{jmt}H_{j(i)mt} + \gamma_i + \gamma_t + \epsilon_{imt}, \tag{2}$$

from which we know $\overline{\xi}_{jmt} H_{j(i)mt}$ is a downward measure of w_{ijm}, and γ_i and γ_t are individual and year fixed effects, respectively. Estimating equation (2) using OLS would thus yield $\hat{\alpha}$ that is biased towards zero due to a classical measurement error problem in the explanatory variable.[13]

In addition to the bias arising from hospital choice, $\overline{\xi}_{jt} H_{j(i)mt}$ need not be uncorrelated with ϵ_{imt} due to insurer choice. We can write equation (2) more generally as

$$y_{imt} = \alpha \sum_j \overline{\xi}_{jmt} H_{j(i)mt} D_{ijmt} + \gamma_i + \gamma_t + \epsilon_{imt},$$

where D_{ijmt} is an indicator variable for individual i choosing insurer j in year t. This formulation makes explicit the second endogeneity problem since $cov(D_{ijmt}, \epsilon_{imt}) \neq 0$, but is infeasible to estimate given that it would require one instrument for every insurer and hospital. Instead, with equation 2 we require only one instrument for network breadth. This is similar to the formulation in Abaluck et al. (2021) who use one forecast coefficient to estimate the causal effect on mortality from enrolling with a particular health plan.

To overcome the two biases arising from non-random selection into insurers and hospitals, we leverage insurer terminations and subsequent hospital terminations for vertically integrated hospitals. At the time of its termination, SaludCoop owned 39 hospitals and clinics across the country, which were also forced to shut down operations after 2015. We showed in the descriptive section a substantial decline in the number of hospital beds in the country, which was entirely driven by SaludCoop-owned hospital beds. While the government stipulated that SaludCoop's hospitals had to be sold to other providers to reduce the potential for interruptions in healthcare, in practice this did not happen until several years after the termination.

[13] Ericson and Starc (2015) provide further discussion on how to measure the breadth of insurance networks.

Based on this evidence, we use one instrument for every source of endogeneity and their interaction to identify the causal effect of interest. We use lagged network breadth, $\overline{\xi}_{jm,t-1}H_{j(i)m,t-1}$, and the treatment indicator as instruments for insurer choice; and an indicator for whether the insurer covered SaludCoop hospitals in 2014, P_{jm}, as an instrument for hospital choice. Intuitively, the treatment indicator and lagged network breadth capture exogenous changes network breadth generated by changes in consumers' choice set of insurers. The indicator for whether insurer j covered SaludCoop hospitals captures exogenous changes in network breadth that are generated by changes in consumers' choice set of hospitals conditional on the insurer. Because our sample includes individuals who did not switch their insurer over the years, we can further consider within-person changes in network breadth to be exogenous to their enrollment decision.

The treatment indicator and the indicator for inclusion of SaludCoop hospitals are both relevant instruments for their respective endogenous choice variables. As discussed in section 2, SaludCoop operated in 458 out of the 1,120 municipalities in the country during 2014. Municipalities with SaludCoop presence accounted for 96 percent of all enrollees in the Colombian health insurance system. In terms of hospital choice sets, our data shows that in markets with SaludCoop hospitals, between one and three other insurers covered these hospitals. SaludCoop hospitals accounted on average for 34 percent of all hospital admissions at insurers that included these hospitals in their networks.

We calculate hospital quality, ξ_{hm}, using data from hospital readmissions in the years prior to SaludCoop's termination. We construct readmissions from the claims data as those that occur within 30 days of one another. We use the resulting individual-admission level dataset to estimate the following regression:

$$b_{it} = x_i'\beta + \xi_{t(h)} + \mu_{it}$$

where b_{it} is an indicator for individual i's visit t resulting in a readmission and x_i is a vector of patient demographic characteristics. To account for statistical noise, we apply an empirical Bayes shrinkage procedure to our estimated hospital fixed effects $\hat{\xi}_h$ (Morris, 1983). Our hospital fixed effects are invariant over time and insurers. However, to the extent that different insurers cover different hospitals and change their network coverage over time, the average quality of in-network hospitals $\overline{\xi}_{jmt}$ will vary across insurers, markets, and years. Appendix figure 9 presents the distribution of hospital fixed effects.

Formally, our first-stage regression is:

$$\begin{aligned}\overline{\xi}_{jmt}H_{j(i)mt} =& \delta_1\overline{\xi}_{jm,t-1}H_{j(i)m,t-1} + \delta_2 T_m + \delta_3 P_{jm} + \delta_4\overline{\xi}_{jm,t-1}H_{j(i)m,t-1} \times T_m \\ &+ \delta_5\overline{\xi}_{jm,t-1}H_{j(i)m,t-1} \times P_{jm} + \nu_{j(i)mt}\end{aligned}$$

We then estimate equation (2) using 2-step GMM and cluster our standard errors at the individual level. In the second stage we also include lagged network breadth so that identification comes only from termination indicators. The estimation sample consists of individuals enrolled in the contributory system from 2013 to 2017 since this corresponds to our network data from the National Health Superintendency. Table 1 presents results of estimating equation (2) using OLS, table 2 presents results using only lagged network breadth and the treatment indicator as instruments, finally table 3 presents results of our main specification with the triple interaction. In each table, columns (1) and (3) use network breadth at the state level, while columns (2) and

(4) use network breadth at the municipality level. Furthermore, columns (1) and (2) use an unweighted measure of network breadth, while columns (3) and (4) weight network breadth with the average quality of in-network providers. Appendix tables 9 and 10 provide first-stage results.

The main takeaway from the different specifications is that broad hospital networks significantly reduce patient mortality. In column (2) of table 1 we find that increasing network breadth from the first to the third quartile of the distribution –which corresponds roughly to adding 13 providers on average– reduces mortality by 1.6 per 1,000.[14] Focusing on column (4) where we weight municipal network breadth with the average quality of in-network hospitals, we find that a similar effect. An interquartile-range increase in quality-weighted network breadth –which corresponds roughly to adding 1.6 providers of above-average quality– reduces mortality by 1.4 per 1,000.[15]

Table 1: OLS Regression of Mortality on Network Breadth

	State (1)	Muni (2)	Qual-State (3)	Qual-Muni (4)
Network breadth	-0.0077 (0.0002)	-0.0064 (0.0002)	-0.1278 (0.0031)	-0.094 (0.0019)
IQ range network breadth	[0.29, 0.50]	[0.28, 0.48]	[-0.01, 0.01]	[-0.01, 0.005]
Individuals x Years	38,378,892		38,378,892	
Individuals	8,506,350		8,506,350	

Note: Table reports coefficients and standard errors in parenthesis of an OLS regression of individual mortality on network breadth. Column (1) uses network breadth at the state level. Column (2) uses network breadth at the municipality level. Column (3) uses network breadth at the state level interacted with average in-network provider quality. Column (4) uses network breadth at the municipality level interacted with average in-network provider quality. All specifications include individual and year fixed effects. Standard errors are clustered at the individual level. Interquartile range of network breadth reported in brackets.

[14] In small municipalities, increasing network breadth from the 25th to the 75th percentile means moving towards a complete network.

[15] We obtain this number by dividing the 25th (-0.014) and 75th (0.0045) percentiles by the average municipal network breadth (0.411), taking the difference between the two resulting numbers ($= 0.046$), and dividing by the average provider quality conditional on being above the full sample average ($= 0.046/0.029$).

Table 2: IV-1 Regression of Mortality on Network Breadth

	State	Muni	Qual-State	Qual-Muni
	(1)	(2)	(3)	(4)
Network breadth	-0.0881	-0.0980	-0.5372	-0.5311
	(0.0080)	(0.0040)	(0.0233)	(0.0275)
IQ range network breadth	[0.29, 0.50]	[0.28, 0.48]	[-0.01, 0.01]	[-0.01, 0.005]
F statistic	11,744	13,165	31,967	11,082
Individuals x Years	29,191,518		29,191,518	
Individuals	7,825,326		7,825,326	

Note: Table reports instrumental variables regression of individual mortality on network breadth. Column (1) uses network breadth at the state level. Column (2) uses network breadth at the municipality level. Column (3) uses network breadth at the state level interacted with average in-network provider quality. Column (4) uses network breadth at the municipality level interacted with average in-network provider quality. In each column the instruments are lagged network breadth, treatment indicator, and their interaction. All specifications include individual and year fixed effects. Standard errors in parenthesis are clustered at the individual level. Interquartile range of network breadth in reported in brackets.

In table 1 selection of sicker individuals into broad-network insurers biases the mortality effect towards zero. Column (4) of table 2, where we instrument for insurer choice, shows that an interquartile-range increase in quality-weighted municipal network breadth reduces mortality by 8 per 1,000, which is 6 times larger than the corresponding estimate in table 1. The effect of our raw measure of network breadth also increases in magnitude in column (2) after correcting for the bias induced by selection into insurers. We find that the raw measure of network breadth has a greater impact on mortality than the quality-weighted measure, which suggests that a simple count of the number of in-network hospitals may be picking up aspects of hospital quality.

Hospital choice is a meaningful source of selection bias as seen in table 3. In this table we instrument for hospital choice in addition to insurer choice. Estimates in columns (1) and (2) are similar in magnitude to the corresponding estimates in table 2, which corroborates our intuition on the direction of the bias. In particular, column (4) shows that an interquartile range increase in quality-weighted municipal network breadth reduces mortality by 7 per 1,000. Because SaludCoop hospitals were of relatively high-quality, further accounting for hospital selection in columns (3) and (4) actually reduces the magnitude of our estimates relative to table 2.

Table 3: IV-2 Regression of Mortality on Network Breadth

	State (1)	Muni (2)	Qual-State (3)	Qual-Muni (4)
Network breadth	-0.1067	-0.0980	-0.4844	-0.4677
	(0.0059)	(0.0035)	(0.0197)	(0.0212)
IQ range network breadth	[0.29, 0.50]	[0.28, 0.48]	[-0.01, 0.01]	[-0.01, 0.005]
F statistic	12,784	12,781	33,810	12,897
Individuals x Years	29,191,518		29,191,518	
Individuals	7,825,326		7,825,326	

Note: Table reports instrumental variables regression of individual mortality on network breadth. Column (1) uses network breadth at the state level. Column (2) uses network breadth at the municipality level. Column (3) uses network breadth at the state level interacted with average in-network provider quality. Column (4) uses network breadth at the municipality level interacted with average in-network provider quality. In each column the instruments are an indicator for insurers that covered SaludCoop hospitals in 2014, lagged network breadth, treatment indicator, and their interactions. All specifications include individual and year fixed effects. Standard errors in parenthesis clustered at the individual level. Interquartile range of network breadth in reported in brackets.

Our results in this section speak to the importance of hospital network breadth for patient health. While having access to insurance coverage is important, previous research has shown that it may not be the main driver of changes in health. Finkelstein, Gentzkow, and Williams (2021) show for example that, conditional on Medicare coverage, individuals who move to locations with higher life expectancy experience themselves an improvement in health. This finding is in contrast to the literature that finds large mortality effects of insurance expansions such as Miller et al. (2021) in the context of Medicaid. Our results harmonize these two stories by providing evidence that having access to a broad network of hospitals through an insurer improves patient's health, and not just having insurance coverage with potentially narrow hospital networks.

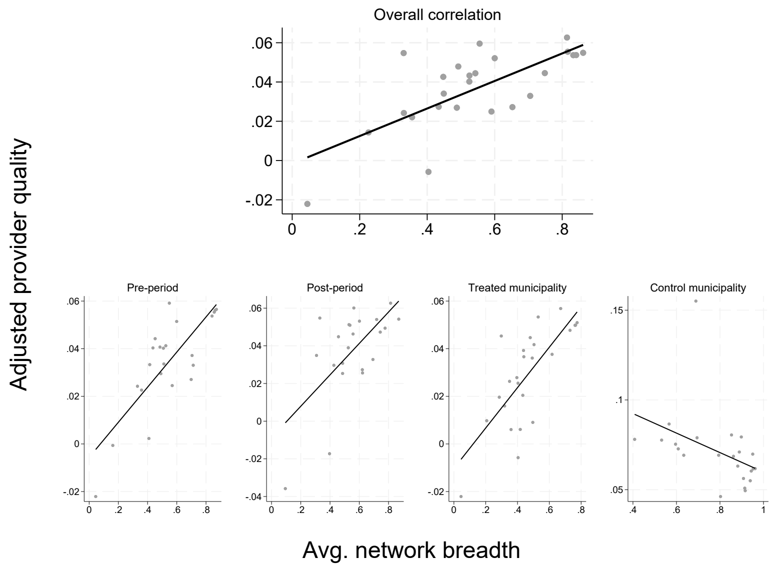
Robustness checks. To verify the validity of our instruments we conduct several placebo or falsification tests in appendix tables 11 and 12. We use as outcome variables an indicator for individual-level deaths by suicide and number of fetal deaths per 1,000 enrollees. To the extent that deaths by suicide and fetal deaths are not determined by the breadth of insurers' hospital networks, we do not expect our instrument to be correlated with these outcomes. We find that our instruments for the quality-weighted network breadth do not explain changes in these types of deaths. In appendix table 13 we also present the reduced-form estimates of the effect of network breadth on individual mortality, which go in line with our causal estimates.

7 Hospital Networks and Access to Care

The previous section showed that hospital network breadth has a negative causal effect on patient mortality, that is, individuals enrolled with broad-network insurers have lower mortality rates. Although these results suggest that access to hospitals has greater impacts on patient health than access to insurance, they still beg the question of what are the mechanisms by which network breadth affects health outcomes. In this section we explore different explanations for why network breadth matters.

We start with a mechanism that was made clear by our discussion of the bias arising from hospital choice: the correlation between network breadth and average quality of in-network hospitals. Figure 11 shows a scatter plot of average network breadth in the horizontal axis and our measure of average in-network hospital quality in the vertical axis. Each dot in this figure is an insurer and the black line represents a linear fit. The figure shows that insurers with broad networks tend to include higher-quality hospitals compared to narrow-network insurers. This positive correlation holds along several dimensions of our data such as in the pre- and post-periods as well among treated municipalities. Control municipalities are typically small and have

FIGURE 11: Network Breadth and In-Network Provider Quality



Note: Figure shows a scatter plot of average municipal network breadth against average quality of in-network providers in the gray dots. The black solid line corresponds to a linear fit. The top panel uses the full sample of insurer-municipality level network data. The bottom panels present scatter plots conditional on the pre-termination period, the post-termination period, treated municipalities, and control municipalities.

a few low-quality providers.

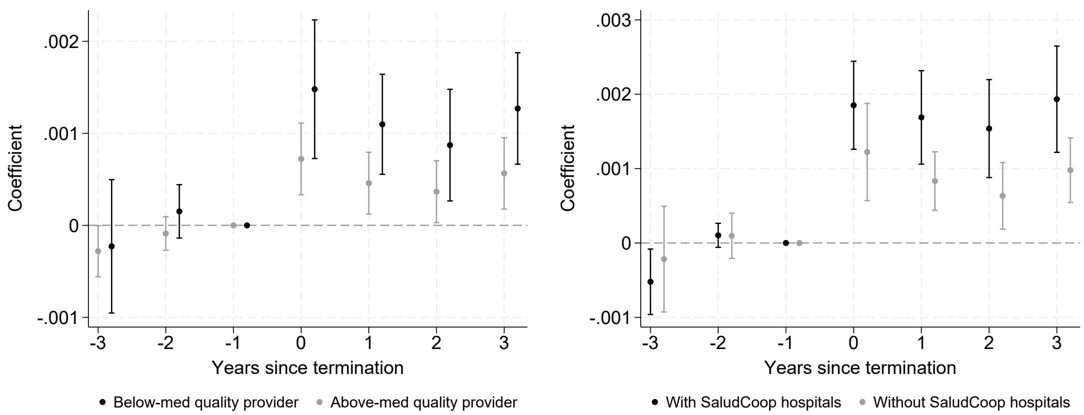
Figure 11 implies that mortality effects may differ across network breadth depending on which hospitals insurers include in their networks. In panel A of figure 12 we explore this dimension of heterogeneity by estimating our event study specification conditional on treated municipalities where the average in-network hospital in 2015 had below- or above-median quality in the black and gray dots, respectively.[16] Results show that this type of heterogeneity can explain our main mortality effects. In treated municipalities with relatively low-quality in-network hospitals, mortality increases 1.5 per 1,000 the year after SaludCoop's termination. Instead, treated municipalities with relatively high-quality hospitals saw an increase in mortality of only

[16]We merge our measure of hospital quality to the hospital network data for 2015. To calculate the average quality of in-network hospitals per municipality, we first average the merged data to the insurer-municipality level, and then to the municipality level. The median quality is calculated in the resulting cross-section of municipalities.

0.7 per 1,000.

Figure 12: Heterogeneity in Mortality Effects by Type of Hospital

(a) Provider quality (b) SaludCoop hospitals



Note: Figure presents event study coefficients and 95 percent confidence intervals of individual mortality. Treatment is defined as municipalities where SaludCoop was present in 2015. In panel (A), we compare treated municipalities with above- and below-median average hospital quality against control municipalities in the black and gray dots, respectively. In panel (B), we compare treated municipalities where SaludCoop owned and did not own hospitals against control municipalities in the black and gray dots, respectively.

To provide further evidence of the importance of having access to broad hospital networks, we estimate our event study conditional on treated municipalities with and without SaludCoop hospitals in panel B of figure 12. Our hypothesis is that municipalities where hospital coverage decreased mechanically because SaludCoop hospitals were terminated, saw larger mortality effects. Indeed, we estimate an increase in mortality that is 50 percent larger in municipalities with SaludCoop hospitals than in those without SaludCoop hospitals the year after the termination. However, the mortality effect in the latter is not zero because incumbent insurers dropped other providers from their networks and because they did not have complete network overlap with SaludCoop. These results suggest unsurprisingly that insurance coverage is important to access health care, but notably that conditional on insurance coverage, having appropriate access to hospitals is more important for patient health.

We move now to investigating the suitability of broad hospital networks for treating patients of different health conditions. We regress different characteristics of the networks, such as which types of services they cover, on network breadth. An observation in these regressions is an insurer-municipality-year. Table 4 shows that broad-network insurers are more suitable for patient health along several dimensions. We find that broad networks tend to provide a greater number of health services. A one percentage point increase in municipal network breadth is associated to a 10 percentage point and an 8 percentage point increase in the likelihood of covering dialysis and chemotherapy providers, respectively. Broad-network insurers tend to cover larger hospitals as measured by the number of beds, which suggests they may be better able to deal with congestion effects after insurer terminations.

Table 4: Network Breadth Mechanisms

Mechanism	coef	se
Total number of services	0.182	(0.017)
Dialysis	0.101	(0.019)
Cardiology	0.162	(0.021)
Chemo/Radiotherapy	0.081	(0.016)
Neurology	0.128	(0.018)
Beds	42.61	(10.39)

Note: Table presents OLS regressions of the outcome in the row on municipal network breadth. The data is at the insurer-municipality-year level. All specifications include municipality and year fixed effects. Standard errors in parenthesis are clustered at the municipality level.

8 Conclusion

Narrow-network insurers have proliferated in health systems with managed care competition, yet the literature that studies the impacts of hospital network breadth on patient health is scarce. We fill this gap in the literature in two ways: first, we quantify the causal effect of hospital network breadth on patient mortality, and second we decompose the relative importance of insurance coverage vis-à-vis hospital coverage. We use data from the Colombian health care system where the largest health insurer and its hospitals were terminated by government during 2015. The termination provides valuable exogenous variation in insurer and hospital choice sets for consumers.

Using an event study framework we find that individual mortality increases by nearly 25 percent and that hospital networks become much narrower after the termination. We link these two findings in an instrumental variables regression to show that hospital network breadth, defined as the fraction of hospitals in a market covered by an insurer, has a negative causal effect on individual mortality. That is, an interquartile-range increase in network breadth reduces mortality by 8 in every 1,000.

To decompose the relative importance of insurer and hospital coverage, we compare changes in mortality between markets that had the terminated insurer but not its hospitals against markets that had both of them. Our findings indicate that mortality is 50 percent larger in the latter than in the former. This suggests that having access to broad hospital networks, even if through a few insurers, is better for patient health than access to narrow hospital networks through many insurers. Our paper more broadly addresses the question of which mechanisms can guarantee appropriate access to health care to consumers of different health status. This is a common concern across health systems with or without universal insurance coverage.

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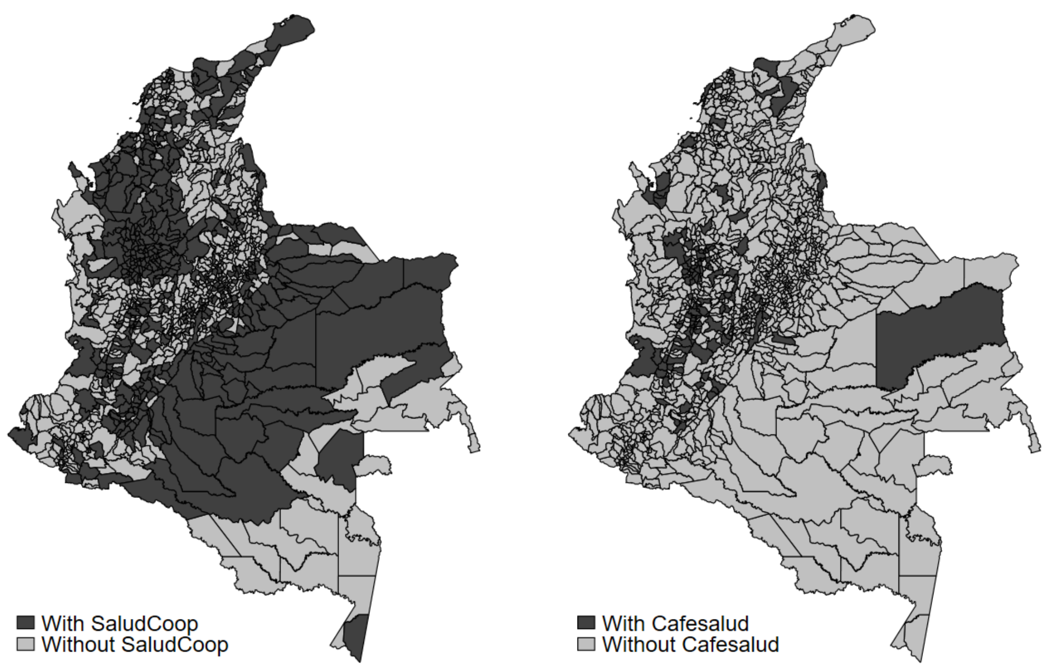
Appendix A Descriptives

Appendix Table 1: Sample restrictions

Sample restriction	Observations
Full sample	66,498,109
Continuous enrollment	47,910,916
No insurer switching before termination + enrollment after death	40,883,417
No moving across municipalities before termination	23,501,299
Exclude SaludCoop and Cafesalud	23,264,825

Note: Table reports the number of individuals left in our sample after imposing each sample restriction.

Appendix Figure 1: Municipal Presence of SaludCoop and Cafesalud

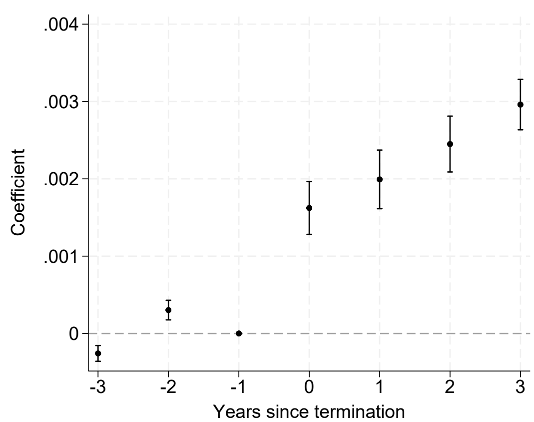


Note: The left panel shows a map of municipalities where SaludCoop was present in 2015 and the right panel shows the municipalities where Cafesalud was present in 2015 in dark gray.

Appendix B What Happened to SaludCoop's enrollees?

In this appendix we investigate changes in mortality among individuals who were enrolled with SaludCoop prior to its termination. We restrict our data to individuals who never switched out of SaludCoop prior to the termination or prior to their death, whichever happens first. But we do not restrict switching patterns after the termination. We use an interrupted time analysis to compare mortality every year of our data relative to 2015, which is our excluded year. Our specification includes municipality fixed effects. We do not pursue a difference-in-differences specification because no other group of enrollees has their incumbent insurer terminated, thus there is no appropriate control group.

Appendix Figure 2: Interrupted time series of mortality for SaludCoop



Note: Figure presents interrupted time series coefficients and 95 percent confidence intervals of individual mortality conditional on consumers who were enrolled with SaludCoop prior to its termination. Specification includes municipality fixed effects.

Appendix figure 2 presents the results. The figure plots the coefficients and 95 percent confidence intervals associated with each year dummy. We find that there is no systematic trend in individual mortality prior to the termination. The coefficient for year 2013 relative to 2015 is negative, while the one for year 2014 is positive. Instead, we find a substantial increase in individual mortality after the termination. In 2016 mortality increases by 1.5 per 1,000 individuals or 26 percent relative to baseline. This effect grows over time to 3 per 1,000 individuals by the end of our sample period.

Appendix C Event Study Coefficients

Appendix Table 2: Congestion Effect

	Congestion effect (1)	High market share (2)	Low market share (3)	High munic. beds (4)	Low munic. beds (5)
t-3	-0.0003	-0.0006	-0.0002	-0.0007	-0.0002
	(0.0003)	(0.0001)	(0.0004)	(0.0002)	(0.0003)
t-2	0.0001	-0.0001	0.0002	-0.0002	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
t-1	(ref)	(ref)	(ref)	(ref)	(ref)
t+0	0.0014	0.0013	0.0014	0.0010	0.0014
	(0.0003)	(0.0003)	(0.0004)	(0.0002)	(0.0003)
t+1	0.0010	0.0011	0.0010	0.0005	0.0010
	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0002)
t+2	0.0008	0.0011	0.0007	0.0005	0.0008
	(0.0002)	(0.0003)	(0.0003)	(0.0002)	(0.0003)
t+3	0.0012	0.0013	0.0011	0.0007	0.0012
	(0.0002)	(0.0003)	(0.0003)	(0.0002)	(0.0003)
Individuals x Year	124,796,233	39,333,141	105,057,295	31,549,891	116,289,332
Individuals	23,264,825	7,576,077	19,596,834	6,087,884	21,738,255

Note: Table reports coefficients and standard errors in parenthesis of a regression of individual mortality on time indicators relative to SaludCoop's termination. Specifications include demographic controls, and insurer and municipality fixed effects. Standard errors are clustered at the municipality level. Column (1) uses the main analysis sample. Columns (2) and (3) use the subsample of treated municipalities where SaludCoop had at least and at most 50 percent market share, respectively. Columns (4) and (5) use the subsample of treated municipalities with above- and below-median number of beds per resident, respectively.

Appendix Table 3: Congestion Effect

	AMI (1)	COPD (2)	Hepatic (3)	CKD (4)	Cancer (5)	Healthy (6)
t-3	-0.0004	-0.0012	0.0062	-0.0050	-0.0015	-0.0002
	(0.0026)	(0.0010)	(0.0065)	(0.0009)	(0.0013)	(0.0001)
t-2	-0.0004	0.0007	0.0036	-0.0016	0.0001	-0.0003
	(0.0023)	(0.0009)	(0.0070)	(0.0009)	(0.0010)	(0.0001)
t-1	(ref)	(ref)	(ref)	(ref)	(ref)	(ref)
	0.0035	-0.0001	0.0077	0.0031	0.0007	-0.0005
t+0	(0.0033)	(0.0014)	(0.0087)	(0.0016)	(0.0017)	(0.0002)
	0.0116	0.0010	0.0190	0.0041	0.0019	-0.0008
t+1	(0.0042)	(0.0013)	(0.0079)	(0.0018)	(0.0017)	(0.0002)
	0.0044	-0.0023	0.0207	0.0037	0.0016	-0.0010
t+2	(0.0037)	(0.0015)	(0.0083)	(0.0020)	(0.0016)	(0.0002)
	0.0066	-0.0021	0.0084	0.0032	0.0007	-0.0010
t+3	(0.0039)	(0.0016)	(0.0113)	(0.0025)	(0.0027)	(0.0002)
Individuals x Year	790,332	5,557,071	165,295	3,360,793	3,831,570	38,135,689
Individuals	122,734	934,156	26,547	555,886	615,738	6,984,296

Note: Table reports coefficients and standard errors in parenthesis of a regression of individual mortality on time indicators relative to SaludCoop's termination. Specification includes demographic controls, and insurer and municipality fixed effects. Standard errors are clustered at the municipality level. Results use the subsample of individuals who were diagnosed at any point during the sample period with Acute Myocardial Infarctions (AMI) in column (2), Chronic Obstructive Pulmonary Disease (COPD) in column (2), hepatic diseases in column (3), Chronic Kidney Disease (CKD) in column (4), and cancer in column (5). Column (6) uses the subsample of individuals without diagnoses.

Appendix D Robustness Checks

Appendix E First-Stage Regressions

Appendix Table 4: Sources of Congestion

	Visits per provider (1)	Providers per enrollee (2)
t-3	0.6907	0.0086
	(2.6427)	(0.2252)
t-2	5.1827	-0.1067
	(4.4660)	(0.1590)
t-1	(ref)	(ref)
t+0	8.0661	0.4139
	(2.7401)	(0.2032)
t+1	13.0451	-0.3752
	(3.1637)	(0.2331)
t+2	21.4644	—
	(5.5171)	
t+3	41.3548	—
	(6.8737)	
Observations	7,444,963	7,805

Note: Table reports coefficients and standard errors in parenthesis of visits per provider in column (1) and providers per 1,000 enrollees in column (2) on time indicators relative to SaludCoop's termination. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level.

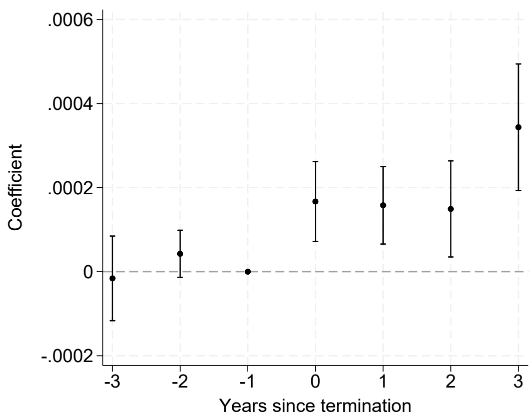
Appendix Table 5: Impact of Congestion on Prices

	Claims (1)	Cost (2)
t-3	0.3280	0.0827
	(0.4713)	(0.0612)
t-2	0.3309	0.0695
	(0.4788)	(0.0503)
t-1	(ref)	(ref)
t+0	-1.0144	0.0270
	(0.7192)	(0.0634)
t+1	-1.4450	0.1583
	(0.5756)	(0.0669)
t+2	-3.0495	0.0080
	(0.6320)	(0.0952)
t+3	-3.2363	-0.0216
	(0.6711)	(0.0728)
Observations	7,685	7,685

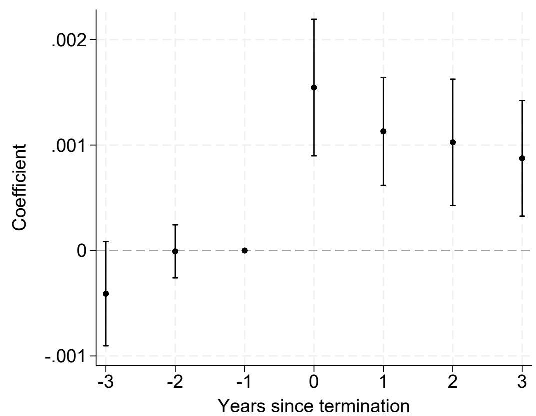
Note: Table reports coefficients and standard errors in parenthesis of total claims (column 1) and total cost (column 2) for the average enrollee in each municipality on time indicators relative to SaludCoop's termination. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level.

Appendix Figure 3: Heterogeneity in Mortality Effects by Age Group

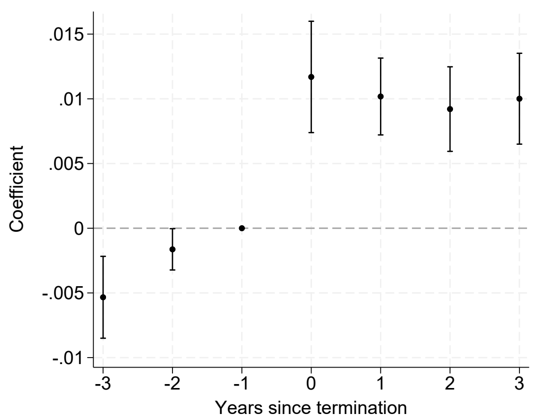
(a) Age less than 35



(b) Age 35-69



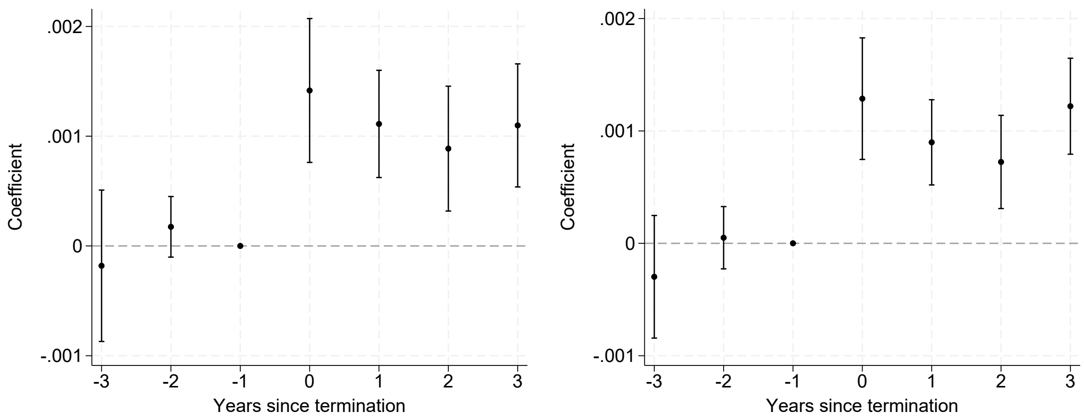
(c) Age more than 69



Note: Figure presents event study coefficients and 95 percent confidence intervals of individual mortality. Treatment is defined as municipalities where SaludCoop was present in 2015. Panel (A) conditions on individuals aged less than 35 at the start of the sample period. Panel (B) conditions on individuals aged 35 to 69 at the start of the sample period, and panel (C) conditions on individuals aged more than 69 at the start of the sample period.

Appendix Figure 4: Heterogeneity in Mortality Effects by Sex

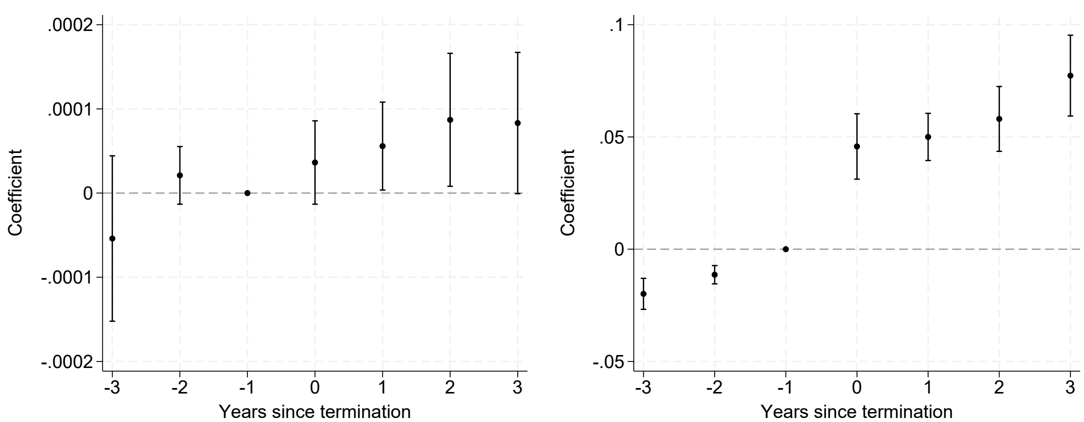
(A) Male (B) Female



Note: Figure presents event study coefficients and 95 percent confidence intervals of individual mortality. Treatment is defined as municipalities where SaludCoop was present in 2015. Panel (A) conditions on males and panel (B) conditions on females.

Appendix Figure 5: Heterogeneity in Mortality Effects by Manner of Death

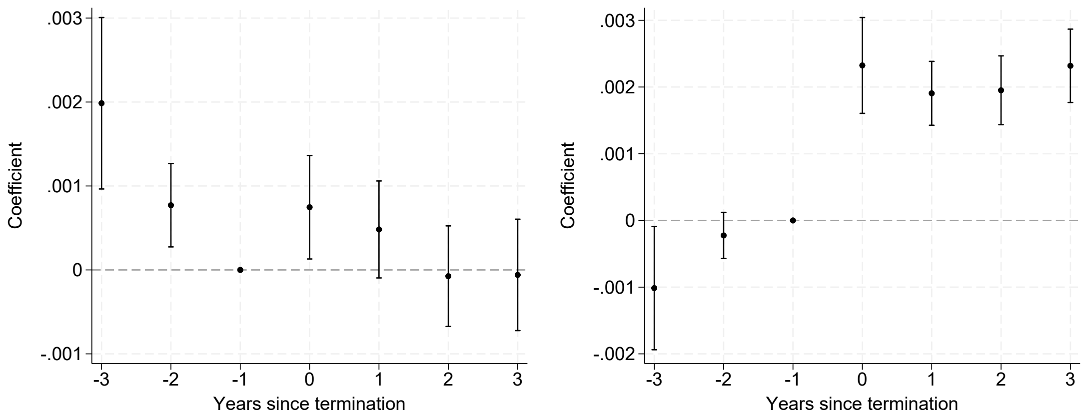
(A) Violent (B) Natural



Note: Figure presents event study coefficients and 95 percent confidence intervals of individual mortality. Treatment is defined as municipalities where SaludCoop was present in 2015. Panel (A) uses individuals who are either alive during the sample period or have a violent cause of death. Panel (A) uses individuals who are either alive during the sample period or have a natural cause of death.

Appendix Figure 6: Heterogeneity in Mortality Effects by System

(a) Contributory (b) Subsidized



Note: Figure presents event study coefficients and 95 percent confidence intervals of individual mortality. Treatment is defined as municipalities where SaludCoop was present in 2015. Panel (A) uses individuals who never switch out of the contributory system during the sample period. Panel (A) uses individuals who never switch out of the subsidized system during the sample period.

Appendix Table 6: Congestion Effect by Health Care Setting

	Outpatient claims	Prescription claims	Urgent care claims	Visits with specialist
	(1)	(2)	(3)	(4)
t-3	0.1835	0.2718	0.0957	-0.0530
	(0.3551)	(0.2408)	(0.0979)	(0.0335)
t-2	0.0307	0.2388	0.1638	-0.0495
	(0.3455)	(0.2380)	(0.0984)	(0.0322)
t-1	(ref)	(ref)	(ref)	(ref)
t+0	-0.9597	-0.2345	0.0110	-0.1149
	(0.3229)	(0.2099)	(0.0843)	(0.0365)
t+1	-1.5029	-0.4309	-0.0171	-0.1094
	(0.4852)	(0.2660)	(0.0986)	(0.0434)
t+2	-2.5809	-1.5860	-0.1782	-0.1572
	(0.5231)	(0.5261)	(0.0920)	(0.0373)
t+3	-2.3606	-1.5860	-0.2073	-0.2418
	(0.5462)	(0.5261)	(0.0909)	(0.0445)
Observations	7,685	7,685	7,685	7,685

Note: Table reports coefficients and standard errors in parenthesis of total outpatient claims (column 1), prescription claims (column 2), urgent care claims (column 3), and visits with the specialist (column 4) for the average enrollee in each municipality on time indicators relative to SaludCoop's termination. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level.

Appendix Table 7: Congestion Effect by Type of Claim

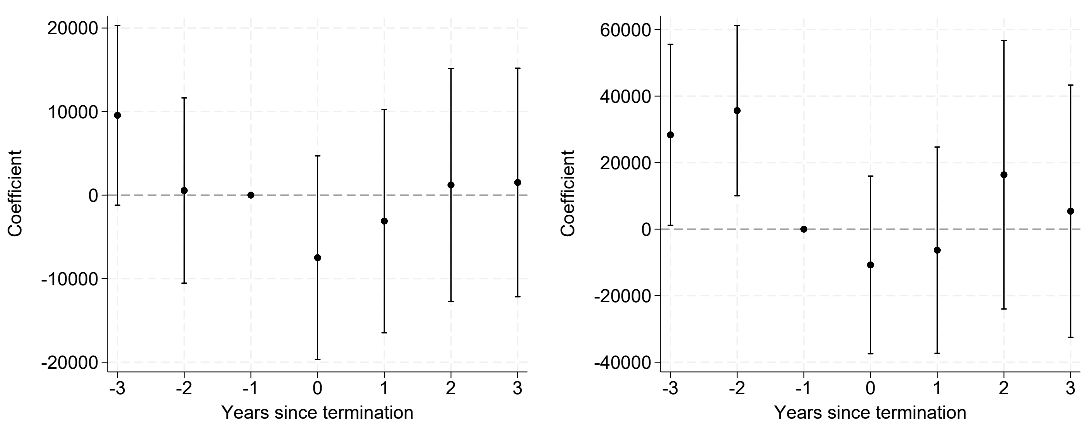
	Imaging (1)	Laboratory (2)	A1C tests (3)	Breast Cancer Screening (4)
t-3	-0.0255	-0.0083	-0.0452	-0.0038
	(0.0273)	(0.1378)	(0.0297)	(0.0056)
t-2	0.0023	0.0260	-0.0291	-0.0038
	(0.0278)	(0.1464)	(0.0290)	(0.0054)
t-1	(ref)	(ref)	(ref)	(ref)
t+0	-0.1033	-0.3914	-0.1704	-0.0150
	(0.0312)	(0.3791)	(0.0378)	(0.0061)
t+1	-0.1301	-0.4252	-0.1345	-0.0150
	(0.0324)	(0.1802)	(0.0403)	(0.0057)
t+2	-0.1699	-0.8425	-0.1242	-0.0157
	(0.0551)	(0.2128)	(0.0434)	(0.0058)
t+3	-0.1197	-0.6504	-0.1433	-0.0120
	(0.0336)	(0.2282)	(0.0561)	(0.0061)
Observations	7,685	7,685	6,593	7,608

Note: Table reports coefficients and standard errors in parenthesis of total imaging claims (column 1), laboratory claims (column 2), A1C tests (column 3), and breast cancer screenings (column 4) for the average enrollee in each municipality on time indicators relative to SaludCoop's termination. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level.

Appendix Figure 7: Congestion Effect on the Price of Imaging Services

(A) CT scans

(B) MRIs



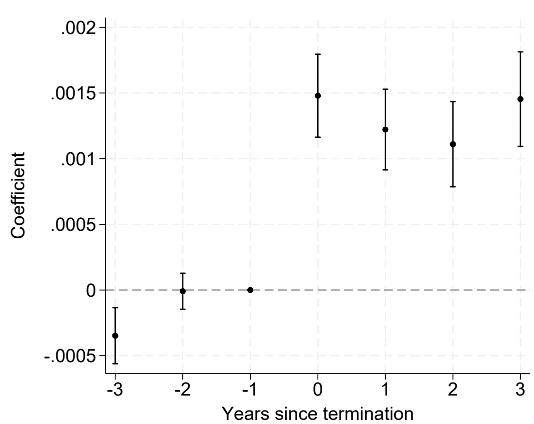
Note: Figure presents event study coefficients and 95 percent confidence intervals of the price of CT scans in pesos in panel (A) and of the price of Magnetic Resonance Imaging tests in pesos in panel (B). Specifications use individual level data from the contributory system averaged or aggregated to the municipality-year level. Treatment is defined as municipalities where SaludCoop was present in 2015.

Appendix Table 8: Congestion Effect by Type of Hospital

	Below-median prov. quality (1)	Above-median prov. quality (2)	w/ SaludCoop hospitals (3)	w/o SaludCoop hospitals (4)	Above-median beds per enroll. (5)	Below-median beds per enroll. (6)
t-3	-0.0002	-0.0003	-0.0005	-0.0002	-0.0007	-0.0002
	(0.0004)	(0.0001)	(0.0002)	(0.0004)	(0.0002)	(0.0003)
t-2	0.0002	-0.0001	0.0001	0.0001	-0.0002	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)
t-1	(ref)	(ref)	(ref)	(ref)	(ref)	(ref)
t+0	0.0015	0.0007	0.0019	0.0012	0.0010	0.0014
	(0.0004)	(0.0002)	(0.0003)	(0.0003)	(0.0002)	(0.0003)
t+1	0.0011	0.0005	0.0017	0.0008	0.0005	0.0010
	(0.0003)	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0002)
t+2	0.0009	0.0004	0.0015	0.0006	0.0005	0.0008
	(0.0003)	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0003)
t+3	0.0013	0.0006	0.0019	0.0010	0.0007	0.0012
	(0.0003)	(0.0002)	(0.0004)	(0.0002)	(0.0002)	(0.0003)
Individuals x Year	106,284,600	39,503,203	39,686,408	106,101,395	31,549,891	116,289,332
Individuals	19,956,124	7,707,111	7,929,078	20,003,119	6,087,884	21,738,255

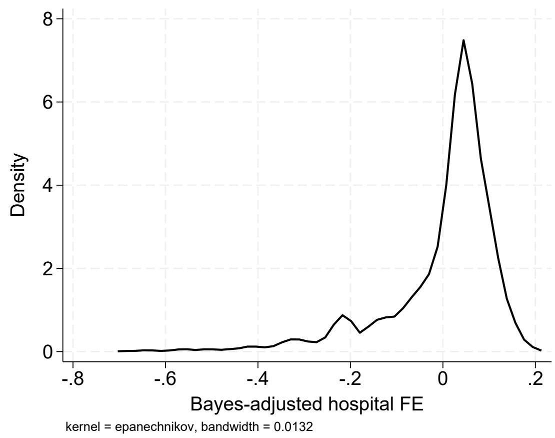
Note: Table reports coefficients and standard errors in parenthesis of a regression of individual mortality on time indicators relative to SaludCoop's termination. Specifications include demographic controls, and insurer and municipality fixed effects. Standard errors are clustered at the municipality level. Columns (1) and (2) use the subsample of treated municipalities with below- and above-median quality of the average in-network hospital, respectively. Columns (3) and (4) use the subsample of treated municipalities with and without presence of SaludCoop hospitals, respectively. Columns (5) and (6) use the subsample of treated municipalities with above- and below-median number of bed per 1,000 enrollees, respectively.

Appendix Figure 8: Congestion Effect Excluding Bogotá and Medellín



Note: Figure shows event study coefficients and 95 percent confidence intervals of enrollee mortality. Specification includes demographic controls, and municipality, year, and insurer fixed effects. Standard errors are clustered at the municipality level. Sample is restricted to individuals who do not switch insurers. We exclude individuals enrolled with SaludCoop and Cafesalud. We also exclude the largest cities, Bogotá and Medellín. Treatment is defined as municipalities where SaludCoop was present in 2015.

Appendix Figure 9: Distribution of Bayes-Adjusted Hospital Fixed Effects



Appendix Table 9: First-Stage Regression of Network Breadth - IV1

	State	Muni	Qual-State	Qual-Muni
Lag network breadth	-0.0449	0.2430	-0.2863	-0.0080
	(0.0020)	(0.0028)	(0.0015)	(0.0036)
Treated	-0.2133	-0.1509	-0.0112	-0.0436
	(0.0014)	(0.0031)	(0.0001)	(0.0003)
Lag network breadth x Treated	0.2899	-0.1649	0.4178	0.0561
	(0.0021)	(0.0028)	(0.0017)	(0.0039)
F statistic	11,744	13,165	31,967	11,082
Individuals x Years	29,191,518		29,191,518	
Individuals	7,825,326		7,825,326	

Note: Table reports first-stage regression of network breadth on lagged network breadth, treatment indicator, and their interaction. Specifications include individual and year fixed effects. Column (1) uses network breadth at the state level. Column (2) uses network breadth at the municipality level. Column (3) uses network breadth at the state level interacted with average in-network provider quality. Column (4) uses network breadth at the municipality level interacted with average in-network provider quality. All specifications include individual and year fixed effects. Standard errors are clustered at the individual level.

Appendix Table 10: First-Stage Regression of Network Breadth - IV2

	State	Muni	Qual-State	Qual-Muni
Lag network breadth	-0.0441	0.2448	-0.2837	-0.0049
	(0.0020)	(0.0028)	(0.0016)	(0.0036)
Treated	-0.2169	-0.1504	-0.0103	-0.0420
	(0.0014)	(0.0031)	(0.0001)	(0.0003)
SaludCoop hosp	0.0289	-0.0028	-0.0182	-0.0255
	(0.0007)	(0.0010)	(0.0001)	(0.0002)
Lag network breadth x Treated	0.2948	-0.1581	0.4233	0.0530
	(0.0021)	(0.0029)	(0.0017)	(0.0039)
Lag network breadth x SaludCoop hosp	-0.1690	-0.1685	-0.1919	-0.0825
	(0.0011)	(0.0012)	(0.0016)	(0.0030)
F statistic	12,784	12,781	33,810	12,897
Individuals x Years	29,191,518		29,191,518	
Individuals	7,825,326		7,825,326	

Note: Table reports first-stage regression of network breadth on lagged network breadth, treatment indicator, indicator for insurers that covered SaludCoop hospitals in 2012, and their interaction. Specifications include individual and year fixed effects. Column (1) uses network breadth at the state level. Column (2) uses network breadth at the municipality level. Column (3) uses network breadth at the state level interacted with average in-network provider quality. Column (4) uses network breadth at the municipality level interacted with average in-network provider quality. All specifications include individual and year fixed effects. Standard errors are clustered at the individual level.

Appendix Table 11: Non-Linear Placebo Test on Fetal Deaths per 1,000 Enrollees

	State (1)	Muni (2)	Qual-State (3)	Qual-Muni (4)
Lag network breadth	-0.7062	-1.0498	-2.9705	-22.835
	(2.2067)	(0.6470)	(20.764)	(16.897)
Treated	—	—	—	—
	—	—	—	—
SaludCoop hosp	-19.729	-20.719	-12.952	-12.977
	(5.7764)	(6.2214)	(5.9050)	(5.3562)
Lag network breadth x Treated	-21.235	-9.0839	-184.24	4.4591
	(4.6462)	(2.1088)	(79.032)	(25.474)
Lag network breadth x SaludCoop hosp	17.448	22.281	447.06	198.43
	(16.107)	(10.1340)	(302.10)	(129.50)
Observations	13,015		13,015	

Note: Table reports OLS reduced-form regressions of number of fetal deaths per 1,000 enrollees on lagged network breadth, an indicator for whether the insurer covered SaludCoop hospitals, the treatment indicator, and their interactions. An observation is a municipality-insurer-year. Column (1) uses network breadth at the state level. Column (2) uses network breadth at the municipality level. Column (3) uses network breadth at the state level interacted with average in-network provider quality. Column (4) uses network breadth at the municipality level interacted with average in-network provider quality. In each column the instruments are lagged network breadth, treatment indicator, and their interaction. All specifications include individual and year fixed effects. Standard errors in parenthesis are clustered at the individual level.

Appendix Table 12: Non-Linear Placebo Test on Deaths by Suicide

	State (1)	Muni (2)	Qual-State (3)	Qual-Muni (4)
Lag network breadth	-0.0001	-0.00004	-0.0001	0.0001
	(0.0001)	(0.0001)	(0.0004)	(0.0005)
Treated	-0.0001	-0.0001	-0.00004	-0.00004
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
SaludCoop hosp	-0.00001	-0.00004	-0.0001	-0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Lag network breadth x Treated	0.0002	0.0001	0.0007	-0.0001
	(0.0001)	(0.0001)	(0.0005)	(0.0005)
Lag network breadth x SaludCoop hosp	-0.0002	-0.0001	0.0013	0.0012
	(0.0001)	(0.0001)	(0.0009)	(0.0008)
Individuals x Years	29,033,810		29,033,810	
Individuals	7,789,931		7,789,931	

Note: Table reports OLS reduced-form regressions of an indicator for death by suicide on lagged network breadth, an indicator for whether the insurer covered SaludCoop hospitals, the treatment indicator, and their interactions. Column (1) uses network breadth at the state level. Column (2) uses network breadth at the municipality level. Column (3) uses network breadth at the state level interacted with average in-network provider quality. Column (4) uses network breadth at the municipality level interacted with average in-network provider quality. In each column the instruments are lagged network breadth, treatment indicator, and their interaction. All specifications include individual and year fixed effects. Standard errors in parenthesis are clustered at the individual level.

Appendix Table 13: Reduced-Form Estimates of Lagged Network Breadth on Mortality

	State	Muni	Qual-State	Qual-Muni
Lag network breadth	-0.0187	-0.02060	-0.0273	-0.1226
	(0.0017)	(0.0010)	(0.0062)	(0.0067)
Treated	0.0171	0.0097	0.02420	0.0199
	(0.0016)	(0.0014)	(0.0012)	(0.0012)
SaludCoop hosp	-0.0012	0.0016	0.0067	0.0076
	(0.0007)	(0.0007)	(0.0007)	(0.0007)
Lag network breadth x Treated	0.0117	0.0214	-0.1265	0.1130
	(0.0018)	(0.0011)	(0.0085)	(0.0074)
Lag network breadth x SaludCoop hosp	0.0221	0.0146	0.1055	0.0783
	(0.0011)	(0.0010)	(0.0174)	(0.0112)
Individuals x Years	29,191,518		29,191,518	
Individuals	7,825,326		7,825,326	

Note: Table reports regression of individual mortality on lagged network breadth, treatment indicator, indicator for insurers that covered SaludCoop hospitals in 2014, and their interactions. Column (1) uses network breadth at the state level. Column (2) uses network breadth at the municipality level. Column (3) uses network breadth at the state level interacted with average in-network provider quality. Column (4) uses network breadth at the municipality level interacted with average in-network provider quality. All specifications include individual and year fixed effects. Standard errors in parenthesis clustered at the individual level.