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

[1] Insurer competition in network breadth involves competition on other dimensions such as wait times and extent of health care fragmentation. Insurance premiums are zero and copays, coinsurance rates, and maximum out-of-pocket amounts are indexed to the enrollee's monthly income but are standardized across insurers and hospitals.

[2] The congestion effect also exists in municipalities where SaludCoop operated but not its hospitals, as long as incumbent insurers did not have complete hospital network overlap with SaludCoop.

of the mortality effect is explained by individuals with chronic health conditions who see their treatments interrupted. Consistent with a congestion effect, we find not only that insurers drop providers from their networks, 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 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, which corresponds to adding 14 providers to the network in the average municipality, reduces mortality by 3.3 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 in-

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

surer 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 McConnell, 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 enrollees have access to the same national health insurance plan through private and public 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]

Importantly, enrollees 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 municipality of residence. At the end of every year, insurers are also compensated for their enrollees' health based on a coarse list of diagnoses, known as the High-Cost Account. 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. Selection incentives and hospital networks 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. 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. The government chose Cafesalud as the reassignment insurer because it had presence in almost the same

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

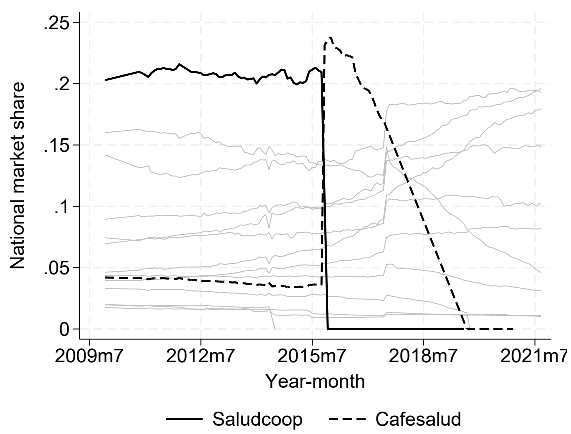
[5]See Decree 780 of 2016.

[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.

municipalities as SaludCoop did (see appendix figure 1).

SaludCoop's enrollees 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: National Market Share



Note: Figure shows monthly national market share per insurer from 2009 to 2021.

TABLE 1: Switching rate

	Cafesalud				Other insurers			
	2016	2017	2018	2019	2016	2017	2018	2019
SaludCoop 2015	0.76	0.53	0.00	0.00	0.24	0.47	1.00	1.00
Cafesalud 2015	0.82	0.59	0.00	0.00	0.18	0.41	1.00	1.00
Other insurers 2015	0.00	0.00	0.00	0.00	1.00	1.00	1.00	1.00

Note: Table reports the fraction of individuals who were enrolled with SaludCoop, Cafesalud, and other insurers in 2015, that move to Cafesalud or other insurers during 2016 to 2019.

Figure 1 shows the national market share per insurer in the contributory regime. We emphasize SaludCoop and Cafesalud in black, and the rest of insurers are illustrated in gray. SaludCoop (solid black line) covered an average of 20 percent of enrollees in the years prior to its termination. SaludCoop and Cafesalud participated in both the contributory and the subsidized regimes. Cafesalud had a national market share under 5 percent before the termination, 23 percent three years after the termination, and was itself terminated in 2019. We thus limit our analysis to the years 2012 to 2019.

Table 1 shows that 76 percent of individuals who were enrolled with SaludCoop during 2015 remained in Cafesalud for 2016, but 24 percent switched to other insurers in that year after the 90-day period. An additional 23 percent of SaludCoop's enrollees moved to other insurers during 2017, a potentially large influx of new enrollees to these incumbent insurers. Of those enrolled with Cafesalud during 2015, 82 percent were inertial in 2016, but 41 percent switched out in 2018 potentially as a preemptive response to Cafesalud's termination. Individuals enrolled with the rest of insurers in 2015 were fully inertial during the sample period. We explore congestion effects at these other insurers among fully inertial patients in the next section.

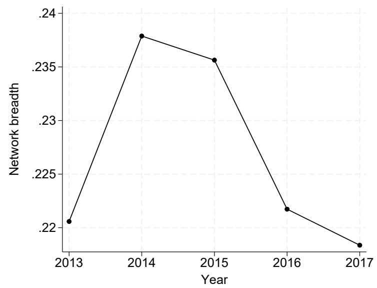
SaludCoop's termination forced substantial changes in the provision of health insurance and health care in Colombia. Fines and debts that resulted from this process continue to be paid to this day.[7] Not only did the termination reduce the number of insurers in each market, but also the country's hospital capacity. As part of the termination, SaludCoop was forced to sell the hospitals and clinics that it owned or was vertically integrated with. These hospitals were not allowed to operate until they were sold to other providers, which has not yet happened.

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 that covered SaludCoop hospitals. These insurers accounted for approximately 1.5

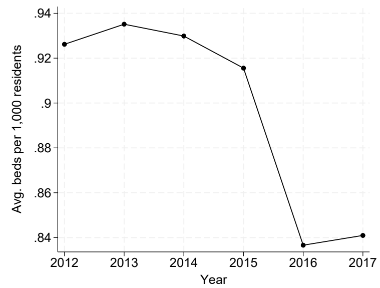
[7] See Resolution 252 of 2021 by the Ministry of Health.

Figure 2: Trends in Network Breadth and Hospital Beds

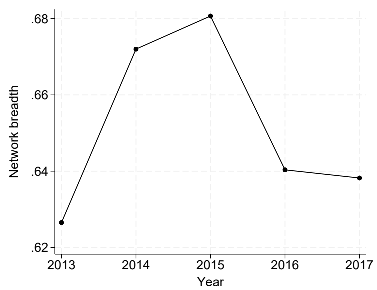
(a) Statal Networks



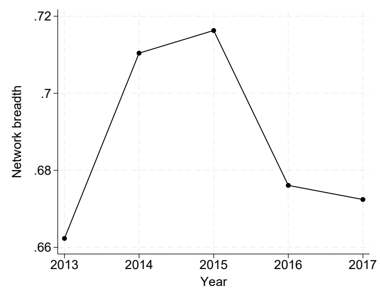
(b) Beds



(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.

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 its own hospitals.

We provide descriptive evidence of changes to hospital networks in figure 2. Using insurers' hospital network data obtained 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 2 percentage points 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 in a municipality decreased 10 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 table 2. The table presents a regression of the average Charlson index among switch-ins and among switch-outs on municipal network breadth. In column (1) we find that insurers with broader networks tend to enroll relatively sicker switchers compared to insurers with narrower networks. Similarly, findings in column (2) show that relatively healthy individuals tend to switch out of insurers with broader networks.

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.

Table 2: Evidence of Adverse Selection on Network Breadth

	Charlson	
	(1) Switch-ins	(2) Switch-outs
Municipal network breadth	0.009	-0.019
	(0.006)	(0.006)
Observations	11,189	11,179

Note: Table presents OLS regressions of average Charlson index among switch-ins in column (1) and switch-outs in column (2) on municipal network breadth. All specifications include municipality and year fixed effects. Standard errors in parenthesis are clustered at the municipality level.

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 SaludCoop's termination. 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.[8] 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 SaludCoop's 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 the insurer and the provider.

From the National Administrative Department of Statistics, we obtain individual level mortality and vital statistics 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 associated diagnosis, manner of death (fetal, violent, or natural), indicator for whether the individual died at the hospital or elsewhere, provider identifier, and insurer identifier.

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.

[8] 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).

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, hence mortality rates are measured relative to the population who is alive at the beginning of the year. We exclude fetal and maternal deaths from the analysis. Finally, we have data on insurers' hospital networks from 2012 to 2017 from the National Health Superintendency. This data reports overall hospital network inclusions but does not distinguish networks per health service.

For our analysis, we compare mortality patterns across enrollees living in (treated) municipalities where SaludCoop operated at the time of the termination, against enrollees living in (control) municipalities where SaludCoop did not operate. 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. 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 operated during 2015 (treated group) against enrollees living in municipalities where SaludCoop did not operate (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 SaludCoop operated in municipality m the year of the termination, t^* is the year when SaludCoop is terminated (2016), x_{it} is a vector of (potentially time-varying) patient characteristics including sex, age, and dummies for each insurer and 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 2015. 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 with our estimates.

5 The Impact of Insurer Terminations

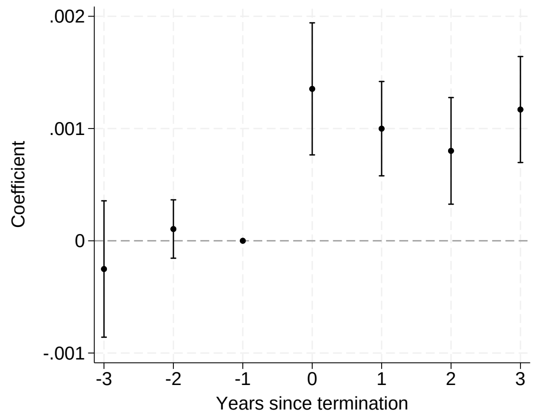
5.1 Individual Mortality

Figure 3 presents coefficients and 95 percent confidence intervals of our event study specification. We label this exercise the "congestion effect," because changes in mortality at insurers other than SaludCoop or Cafesalud can be potentially explained by their hospital networks becoming more congested with SaludCoop's enrollees. Appendix table 2 reports the associated coefficients and standard errors.[9]

Prior to the termination, individuals in municipalities where SaludCoop operated and those where it didn't had parallel mortality trends. The year of the termination, mortality increases 1.3 per 1,000 enrollees in treated municipalities, roughly a 25 percent increase over baseline. The magnitude of our estimate is in line with other studies on the effect of insurance coverage on mortality. For example, Miller et al. (2021) find that individuals in states that expand Medicaid experience a reduction

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

Figure 3: Congestion Effect



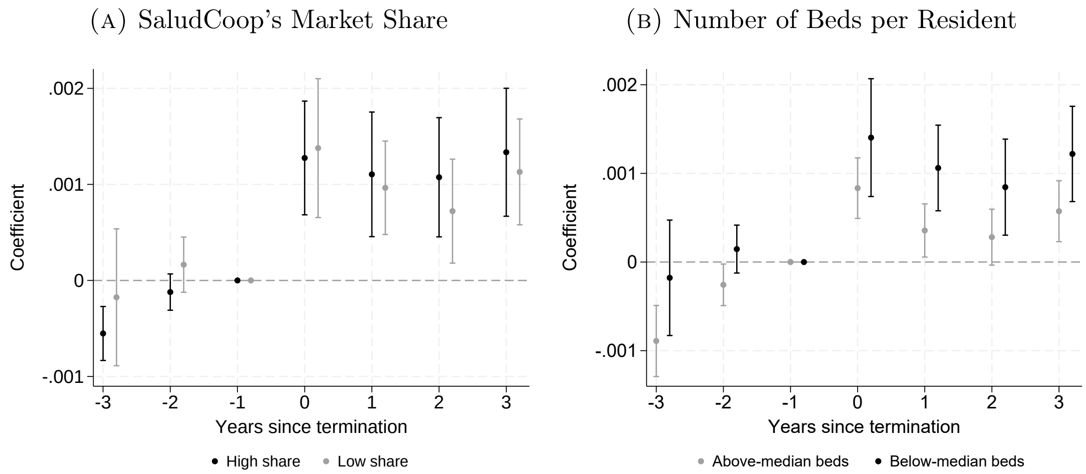
Note: Figure shows event study coefficients and 95 percent confidence intervals of enrollee mortality. Specification includes demographic controls, and municipality, year, and insurer fixed effects. Standard errors are clustered at the municipality level. Sample is restricted to individuals who do not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

of 11.9 percent in annual mortality three years after the expansion. Abaluck et al. (2021) estimate a 19 percent reduction in mortality from enrolling with a one-standard deviation higher-quality insurance plan in Medicare. And Card, Dobkin, and Maestas (2009) find that Medicare eligibility reduces 7-day in-hospital mortality by 20 percent.

Mortality effects in our setting are likely not driven by transitory disruptions in health care generated by the termination. We find that effects on mortality are persistent over time, suggesting that the Colombian health care system had not reached a new steady state by 2019. Three years after the termination, we estimate a mortality increase in treated municipalities equal to 0.8 per 1,000 enrollees, nearly 18 percent relative to baseline. These results are robust to excluding the largest cities, Bogotá and Medellín, as seen in appendix figure 6.

Our results are homogeneous across municipalities where SaludCoop had different market shares but heterogeneous depending on the number of hospital beds in the market. Panel A of figure 4 shows that in municipalities where SaludCoop had more than 50 percent market share, individual mortality increased 0.13 percentage points

Figure 4: Congestion Effect by Market Characteristics



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.

every year after the termination relative to control units. This effect is similar in size for those living in municipalities where SaludCoop had at most 50 percent market share. Although the homogeneity of results by size of SaludCoop seems at odds with a congestion effect, the relevant heterogeneity is on the degree of hospital network overlap between SaludCoop and the rest of insurers.

As a simple example, suppose that there are two insurers A and B that each cover the same three hospitals $\{x, y, z\}$. If insurer B is terminated, in-network hospitals at A will treat the same amount patients before and after the termination. In this case, we should not expect to see changes in mortality after the termination if insurers are only intermediaries between patients and hospitals. Instead, if insurer A covers hospitals $\{x, y\}$ and insurer B covers hospitals $\{y, z\}$, B's termination would result in $\{x, y\}$ treating not only their previous patients but also those who switch to A and were previously treated by z. This "congestion effect" at $\{x, y\}$ potentially reduces access to health care and worsens health outcomes.

To illustrate this heterogeneity by hospital capacity, panel B shows that treated municipalities with above-median number of beds per 1,000 residents during 2015 had a better buffer to deal with the new influx of patients. In these municipalities the mortality increase was 50 percent smaller than in treated municipalities with below-median number of beds. Mortality differences by total hospital capacity suggest that hospital networks determine patient health. We further explore this argument in section 7.

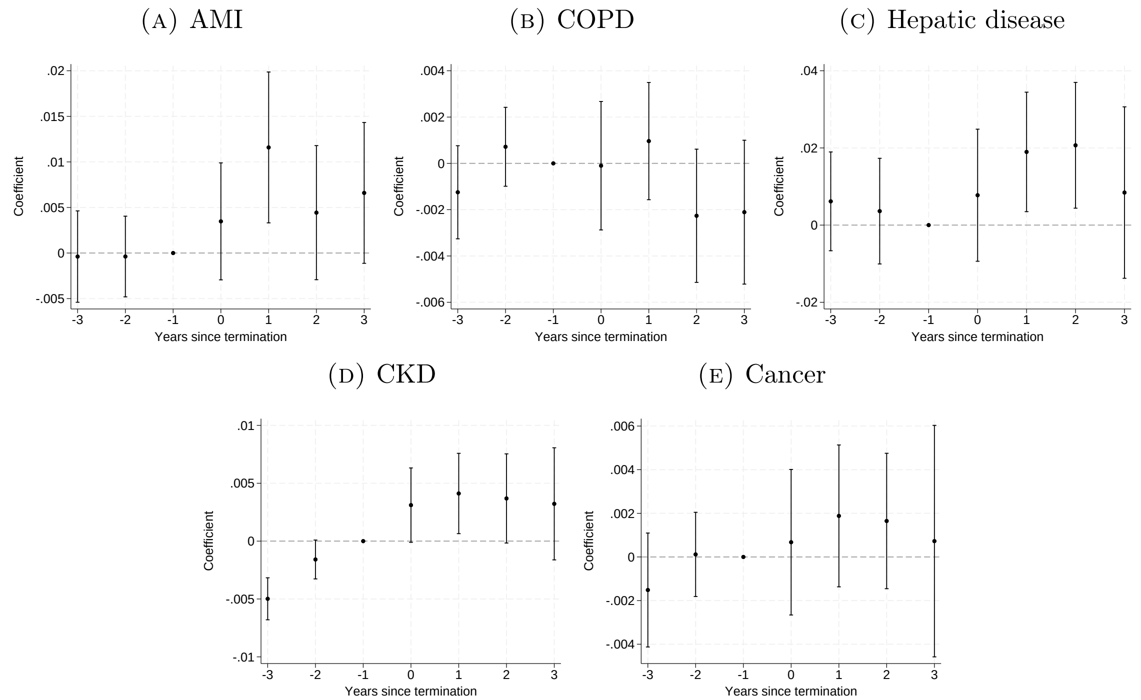
5.2 Mortality by Subgroups

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 a sudden interruption or disruption in care can be potentially fatal.

In figure 5 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. These exercises are therefore conditional on patients who make claims in the contributory regime. We focus on the following conditions: Acute Myocardial Infarctions (AMI), Chronic Obstructive Pulmonary Disease (COPD), Hepatic diseases, Chronic Kidney Disease (CKD), and Cancer. Coefficients and standard errors are reported in appendix table 3.

In all cases, except for COPD, we see that mortality increases the year or two after

Figure 5: 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.

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 event. The rapid response of mortality rates to SaludCoop's termination is therefore explained by individuals with chronic diseases who see their healthcare treatments interrupted. Appendix figure 3 reinforces this finding by showing that mortality effects are only present among individuals who died of natural causes rather than among those who experienced a violent death. In appendix figures 4 and 5 we further examine the heterogeneity of our findings along age and sex.

5.3 Hospital Networks

Congestion effects after SaludCoop's termination are only possible (i) if insurers enroll more individuals, did not change their hospital networks, and did not have complete network overlap with SaludCoop, and (ii) if insurers drop hospitals from their networks even if their population of enrollees did not change. In this subsection, we analyze these congestion mechanisms more systematically.

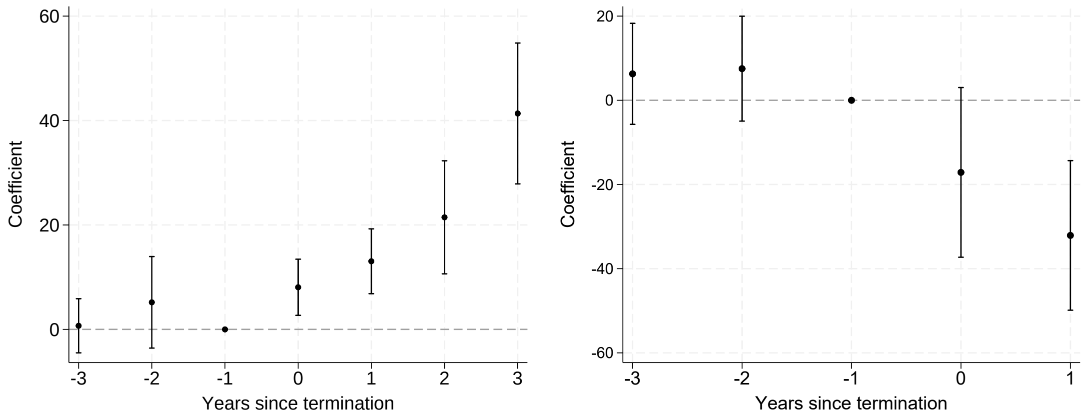
To provide evidence of the first mechanism, we use the claims data to construct the number of visits per provider. We collapse the claims data to the provider-insurer-year level, and compare municipalities where SaludCoop operated against those where it didn't using a similar event study specification as in equation (1).[10] 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. We have hospital network data for the period of 2013 to 2017.

Panel A of figure 6 shows that providers in treated municipalities had approximately 10 more visits or consultations the year of the termination relative to providers in control municipalities. This congestion effect at each provider worsens over time, as they saw nearly 40 more visits three years after the termination. In addition to each provider rendering more visits, insurers substantially narrowed their networks. Panel B shows that insurers in treated municipalities dropped around 17 providers per 1,000 enrollees the year of the termination, an effect that represents a 22 percent reduction relative to baseline.

[10] 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 6: Sources of Congestion

(A) Visits per provider (B) Providers per 1,000 enrollees



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 insurers having more than 0.05% market share in the municipality. Specification uses data at the insurer-market-year level and includes municipality and year fixed effects. We have hospital network data from 2013 to 2017, thus we exclude years 2 and 3 relative to the termination from panel B. In each specification, treatment is defined as municipalities where SaludCoop was present in 2015.

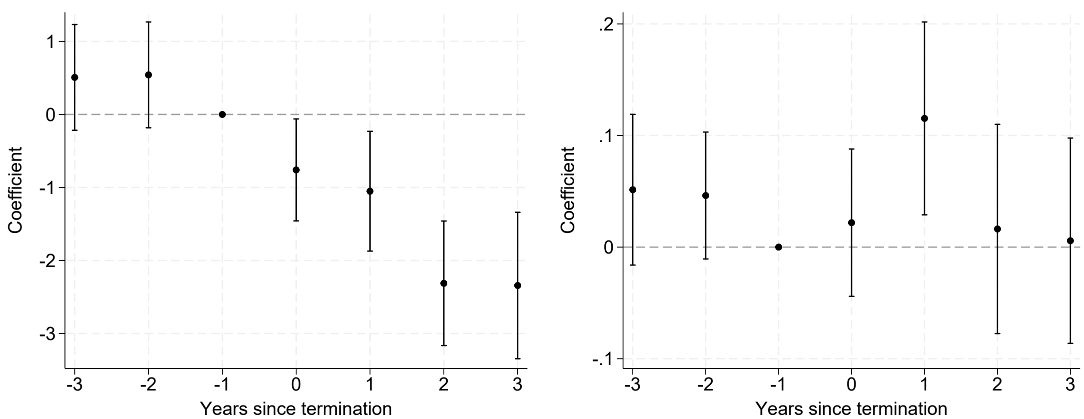
5.4 Health Claims

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 in health care 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 prices and health care costs is ambiguous. In this subsection we explore the impact of SaludCoop's termination on the cost and claims for several health services.

FIGURE 7: 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.

To conduct this 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.[11] This means that individual-level measures of utilization and costs will have several missing values. We circumvent this issue by aggregating our data to the municipality-year level, calculating averages across all individuals enrolled with the insurers that we observe.[12] Our analysis therefore will be indicative of changes in utilization and costs for the *average enrollee* in the contributory system.

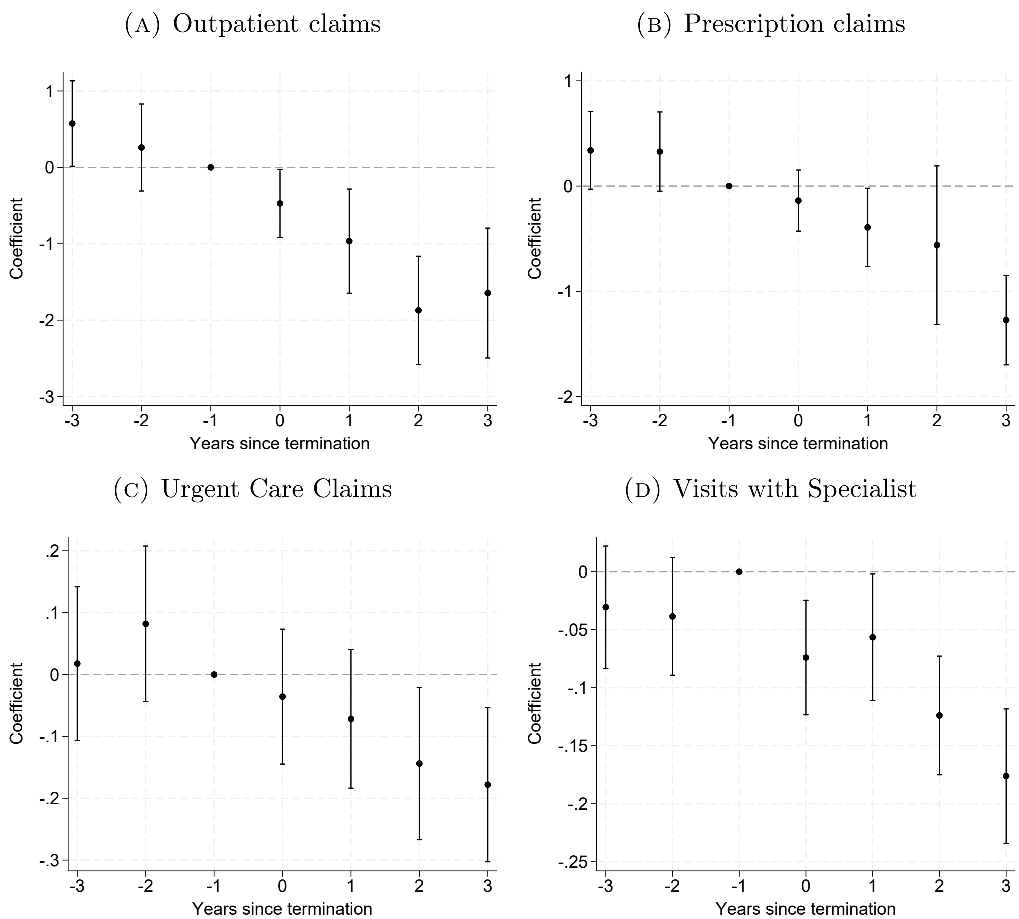
Panel A of figure 7 shows that individuals in treated and control municipalities

[11]Excluding SaludCoop and Cafesalud, out of the 10 remaining insurers we observe 6 for 7 years, 8 for 5 or more years, and 10 for 4 or more years.

[12]Results are robust to restricting our sample to individuals enrolled with the 6 insurers that we observe in the data every year.

had parallel utilization patterns in the pre-period. A year after the termination, the average enrollee in treated municipalities made roughly 1 fewer health claim than control units, an 8 percent decline relative to baseline. This reduction in the number of claims is much larger and equal to 2.5 claims 3 years after the termination. Although our estimates of changes in utilization are relatively large, they are within the range of other studies that analyze forced switches after insurer terminations. For example, Politzer (2021) finds a 9.2 percent reduction in visits to primary care physicians and a 9.8 percent increase in hospital admissions.

Figure 8: Impact of Congestion on Types of Claims



(A) Outpatient claims

(B) Prescription claims

(C) Urgent Care Claims

(D) Visits with Specialist

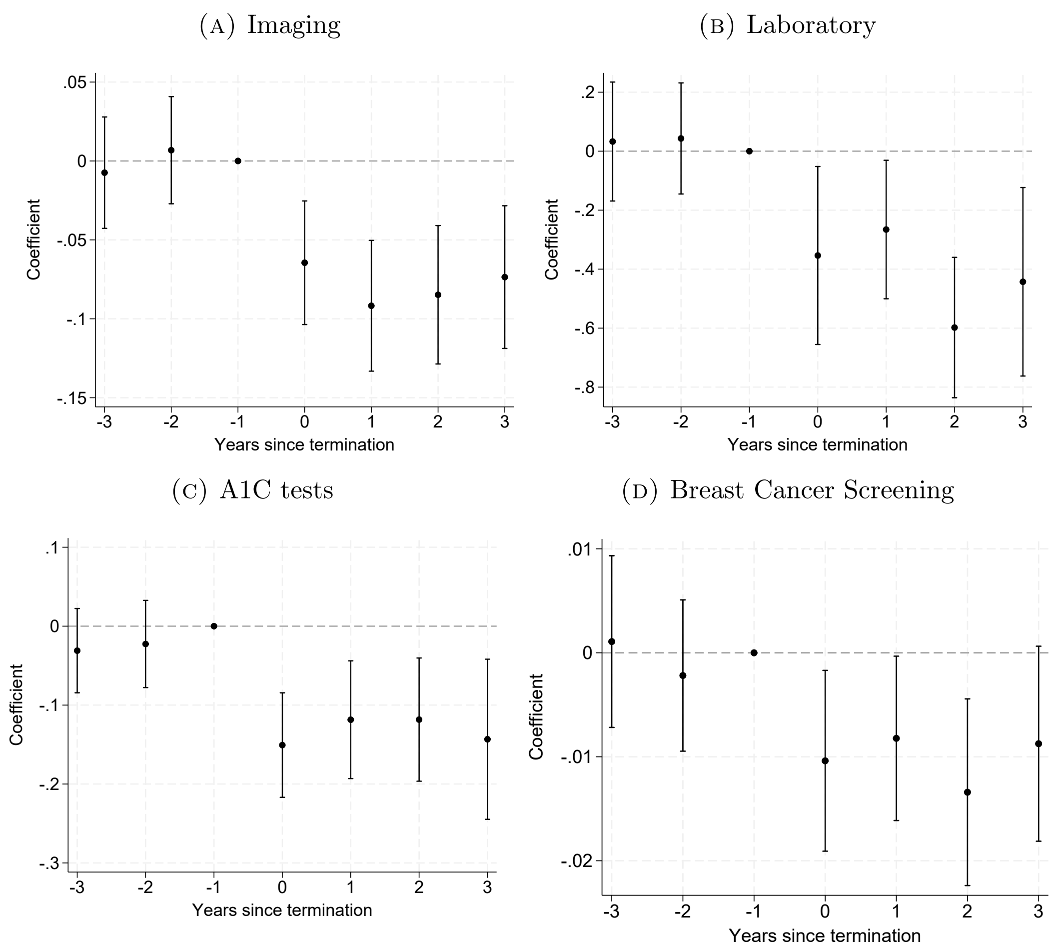
Note: Figure shows event study coefficients and 95 percent confidence intervals of outpatient claims in panel (A), prescription claims in panel (B), urgent care claims in panel (C), and visits with the specialist in panel (D). Specifications use individual level data from enrollees in the contributory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

Despite significant declines in utilization after 2015, the cost of the average enrollee did not change as seen in panel B of the figure. Together, these findings imply that the price per claim increased after the termination. Our results in figure 7 are not contrary to those reported in figure 6. 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 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 8 shows that the average consumer made 1.5 fewer outpatient claims a year after the termination. Likewise, in panels B and C we see that the average consumer filed 1.5 fewer prescription claims and 0.2 fewer urgent care claims around 2018. Finally, panel D shows that the average consumer in treated municipalities had 0.2 fewer visits to the specialist right after the termination. Importantly, average enrollees in treated and control municipalities had parallel utilization trends across these types of claims in the pre-period. Therefore, reductions in utilization after 2015 are suggestive of consumers in treated municipalities not receiving the type of care that they need.

Consistent with this argument, we find that utilization of health services needed for prevention or early detection of serious health conditions significantly decreased after the termination. Panels A and B of figure 9 show that the average consumer made 0.2 fewer imaging claims and received 1 fewer lab test in treated municipalities two years 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 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.

Figure 9: Impact of Congestion on Preventive and Diagnostic Aid



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.

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 networks 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 proliferation of narrow-network insurers in the United States and other countries, this exercise is important to understand the welfare effects of policies requiring insurers to meet certain network adequacy standards. Moreover, the quasi-experimental variation created by SaludCoop's termination provides an avenue to disentangle the relative importance of insurance coverage vis-à-vis hospital network breadth.

Identifying the effect of network breadth on mortality is a difficult 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 that is biased towards zero. Also, 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 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:

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

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

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

In practice, the regression that is feasible to estimate is:

$$y_{imt} = \alpha\overline{\xi}_{jmt}H_{j(i)mt} + x'_{it}\beta + \gamma_{mt} + \epsilon_{imt}, \tag{2}$$

where y_{imt} is observed mortality, x_{it} are exogenous potentially time-varying characteristics (such as age and sex) and γ_{mt} is a municipality-by-year fixed effect. Estimating equation (2) via OLS would yield $\hat{\alpha}$ that is biased towards zero due to a classical measurement error in the explanatory variable: $\overline{\xi}_{jmt}H_{j(i)mt}$ is a downward measure of w_{ijm}.[13]

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

$$y_{imt} = \alpha\sum_{j}\overline{\xi}_{jmt}H_{j(i)mt}D_{ijmt} + x'_{it}\beta + \gamma_{mt} + \epsilon_{imt}, \tag{3}$$

where D_{ijmt} is an indicator variable for individual i choosing insurer j in market m

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

and year t. This formulation makes explicit the second endogeneity problem since $cov(D_{ijmt}, \epsilon_{imt}) \neq 0$ due for example to unobserved changes in individual health status. Estimation of (3) is likely infeasible and under-powered because it would require one instrument for every insurer and hospital. Instead, equation (2) identifies the *average effect* of network breadth on the outcome of interest requiring only one instrument. 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 construct our main independent variable and later on our instrument, we first calculate hospital quality, ξ_{hm}, using hospital readmissions data for the entire sample period. Readmissions are defined as those that occur within 30 days of one another. Using the claims data we derive a patient-admission level dataset to estimate the following regression:

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

b_{it} is an indicator for individual i's visit t *not* resulting in a readmission and x_i is a vector of characteristics including sex, and dummies for age group (0-24, 25-44, 45-64, 65+), insurer, and year. To account for statistical noise, we apply an empirical Bayes shrinkage procedure to our estimated hospital fixed effects $\hat{\xi}_h$, following Morris (1983). We shrink our estimated hospital fixed effects toward their municipality-level mean.[14] These fixed effects are invariant over time and insurers. However, to the extent that different insurers cover different hospitals and change their network inclusions over time, the average quality of in-network hospitals $\overline{\xi}_{jmt}$ will vary across insurers, markets, and years in our final specification. Appendix figure 7 presents the distribution of the Bayes-adjusted hospital fixed effects.

[14] We use the `ebayes` and `fese_fast` codes in Chandra, Finkelstein, Sacarny, and Syverson (2016) and Nichols (2008).

To overcome the two biases arising from non-random selection into insurers and hospitals, we leverage exogenous changes in network breadth generated by SaludCoop's termination. Our instrument is the interaction between the treatment indicator T_m, a post-termination period indicator P_t, and network breadth in 2015 $\overline{\xi}_{jm,2015}H_{j(i)m,2015}$. To see the intuition for our instrument, consider table 3 below. Insurer A is in a treated municipality T and insurer B is in a control municipality C. Cells highlighted in orange represent changes in network breadth within a market that are endogenous or that insurers make in response to their competitors. Cells highlighted in blue represent exogenous changes in network breadth explained by SaludCoop's termination, and cells in purple contain both plausibly exogenous and endogenous variation in network breadth. To isolate the potentially exogenous variation in blue and purple from the one in orange, we need to simulate our event study specification interacting the treatment indicator with the post-period indicator. Then, because only changes relative to 2015 can be explained by the termination, we further interact with baseline network breadth in 2015.

TABLE 3: Instrument Example

Market	Insurer	2013	2014	2015	2016	2017	2018	2019
T:	A	0.5	0.5	0.5	0.3	0.3	0.2	0.2
C:	B	0.3	0.3	0.2	0.2	0.2	0.1	0.1

Our instrument is relevant for several reasons discussed in section 2. SaludCoop operated in 458 out of the 1,120 municipalities in the country during 2014. Municipalities with presence of SaludCoop 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, at least three other insurers covered these hospitals as well. SaludCoop hospitals accounted on average for 34 percent of all hospital admissions at insurers that included these hospitals in their networks.

Formally, our first-stage regression is:

$$\overline{\xi}_{jmt}H_{j(i)mt} = \delta_1\Big(T_m \times P_t \times \overline{\xi}_{jm,2015}H_{j(i)m,2015}\Big) + x'_{it}\delta_2 + \gamma_{mt} + \nu_{j(i)mt}$$

We then estimate equation (2) using 2SLS and clustering our standard errors at the municipality level. The estimation sample consists of individuals enrolled in the contributory system from 2013 to 2017 since this corresponds to the period for which we have hospital network data. Table 4 presents the results of estimating equation (2) via OLS and table 5 presents the results of our instrumental variable specification. In each table, columns (1) and (2) use municipal network breadth and columns (3) and (4) use municipal network breadth weighted by hospital quality. Furthermore, columns (2) and (4) include demographic controls. Appendix table 9 provides first-stage results.

The main takeaway from the different specifications is that broad hospital networks significantly reduce patient mortality. In column (1) of table 4 we find that increasing network breadth from the first to the third quartile of the distribution, which corresponds roughly to adding 14 providers to the network in the average municipality, reduces mortality by 2.3 per 1,000.[15] Adding demographic controls in column (2) reduces the magnitude of our estimate because these variables capture unobserved patient health that is correlated with their choice of insurer conditional on network breadth. In column (3) we find that the same reduction in mortality equal to 2.3 per 1,000 can be achieved by adding only 5 providers of above-average quality to the network in the average municipality. This corresponds to an interquartile range

[15]We obtain the number of providers by taking the difference between the 75th and the 25th percentiles and multiplying by the average number of providers in a municipality in the full sample.

increase in quality-weighted network breadth.[16]

Table 4: OLS Regression of Mortality on Municipal Network Breadth

	(1)	(2)	(3)	(4)
	Raw		Quality-adjusted	
Network breadth	-0.0110	-0.0028	-0.0132	-0.0032
	(0.0020)	(0.0012)	(0.0024)	(0.0015)
Demographic controls	No	Yes	No	Yes
IQ range network breadth	[0.288, 0.497]		[0.235, 0.412]	
Individuals x Years	38,651,482		38,651,482	

Note: Table reports coefficients and standard errors in parenthesis of an OLS regression of individual mortality on municipal network breadth. Columns (1) and (2) use municipal network breadth. Columns (3) and (4) use municipal network breadth weighted by the average in-network provider quality. Columns (2) and (4) include demographic controls (sex and age). All specifications include municipality-by-year fixed effects. Standard errors are clustered at the municipality level. Interquartile range of network breadth reported in brackets.

Selection of sicker individuals into broad-network insurers biases the mortality effect towards zero in table 4. When we instrument for insurer and hospital choice in table 5, we find larger effects consistent with our intuition on the direction of the bias. Results in column (1) show that an interquartile-range increase in municipal network breadth reduces mortality by 3.3 per 1,000, which is 1.4 times larger than the corresponding estimate in table 4. Similarly, in column (3) we find that increasing quality-weighted network breadth from the first to the third quartile reduces mortality by 3.4 per 1,000. The fact that our raw measure of network breadth has smaller impact on mortality than our quality-weighted measure suggests that a simple count of the number of in-network hospitals may be underestimating aspects of hospital quality and therefore that it is important to account for this measurement error.

Our results in this section speak to the relevance 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. Finkel-

[16]We obtain the number of providers by taking the difference between the 75th and the 25th percentiles and multiplying by the average number of providers with above-average quality in a municipality in the full sample.

Table 5: IV Regression of Mortality on Municipal Network Breadth

	(1)	(2)	(3)	(4)
	Raw		Quality-adjusted	
Network breadth	-0.0159	-0.0059	-0.0193	-0.0069
	(0.0036)	(0.0023)	(0.0042)	(0.0026)
Demographic controls	No	Yes	No	Yes
F statistic	246.72	232.72	260.44	246.16
IQ range network breadth	[0.288, 0.497]		[0.235, 0.412]	
Individuals x Years	38,651,482		38,651,482	

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 municipality level. Interquartile range of network breadth in reported in brackets.

stein, 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 instrument we conduct several placebo or falsification tests in appendix tables 10 to 12. We use as outcome variables an indicator for violent deaths, deaths by suicide, and number of fetal deaths per 1,000 enrollees. To the extent that these types of 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 in fact zero correlation between our instrument and these types of deaths. In appendix table 13 we also present reduced-form estimates of our main specification.

7 Hospital Networks and Access to Care

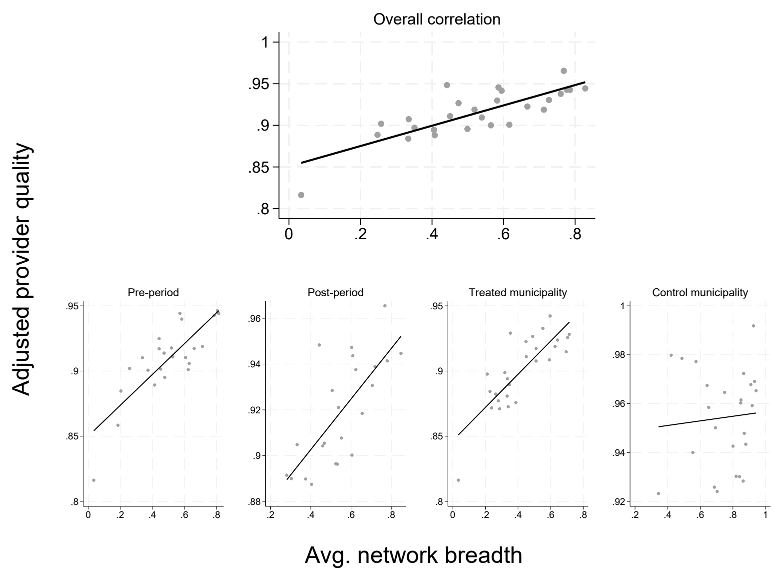
The previous section showed that hospital network breadth has a negative causal effect on patient mortality, that is, individuals enrolled with broad-network insurers have lower mortality rates. Although these results suggest that access to hospitals has greater impacts on patient health than access 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 evident from the discussion of the bias arising from hospital choice: the correlation between network breadth and hospital quality. Figure 10 shows a scatter plot of average municipal network breadth in the horizontal axis and average quality of in-network hospitals 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 depicted in the bottoms panels of the figure, hence it is not affected by changes in market structure such as those created by insurer terminations.

Figure 10 implies that mortality effects may differ across network breadth depending on which hospitals insurers include in their networks. In panel A of figure 11 we explore this dimension of heterogeneity by estimating our main 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.[17] Results show that this type of heterogeneity can explain our main mortality effects. In treated municipalities with relatively low-quality in-network hospitals, mortality

[17] 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.

Figure 10: Network Breadth and In-Network Provider Quality



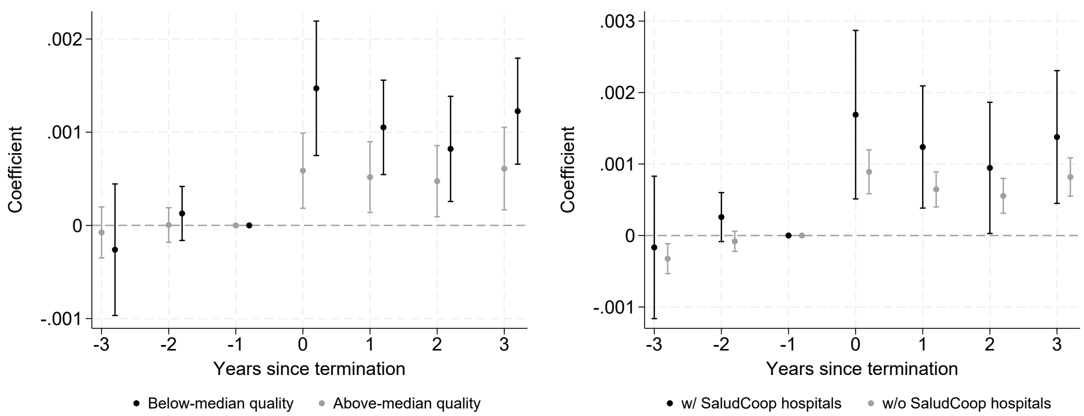
Note: Figure shows a scatter plot of average municipal network breadth against average quality of in-network providers in the gray dots. The black solid line corresponds to a linear fit. Each dot represents an insurer. Top panel presents correlation in the full sample. Bottom panels present correlations conditional on the pre-termination period, post-termination period, treated municipalities, and control municipalities.

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 roughly half as large.

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 11. 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 over-

Figure 11: 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.

lap 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 municipal network breadth. An observation in these regressions is an insurer-municipality-year. Table 6 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 6 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 6: Network Breadth Mechanisms

Mechanism	coef	se
Total number of services	0.198	(0.015)
Dialysis	0.106	(0.017)
Cardiology	0.170	(0.019)
Chemo/Radiotherapy	0.060	(0.012)
Neurology	0.115	(0.015)
Beds	34.89	(8.190)

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 in December 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 increased nearly 25 percent and that hospital networks became 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 that are covered by an insurer, has a negative causal effect on individual mortality. That is, an interquartile-range increase in network breadth, which corresponds to adding roughly 14 providers to the network, reduces mortality by 3.3 per 1,000 enrollees.

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 and without universal insurance coverage.

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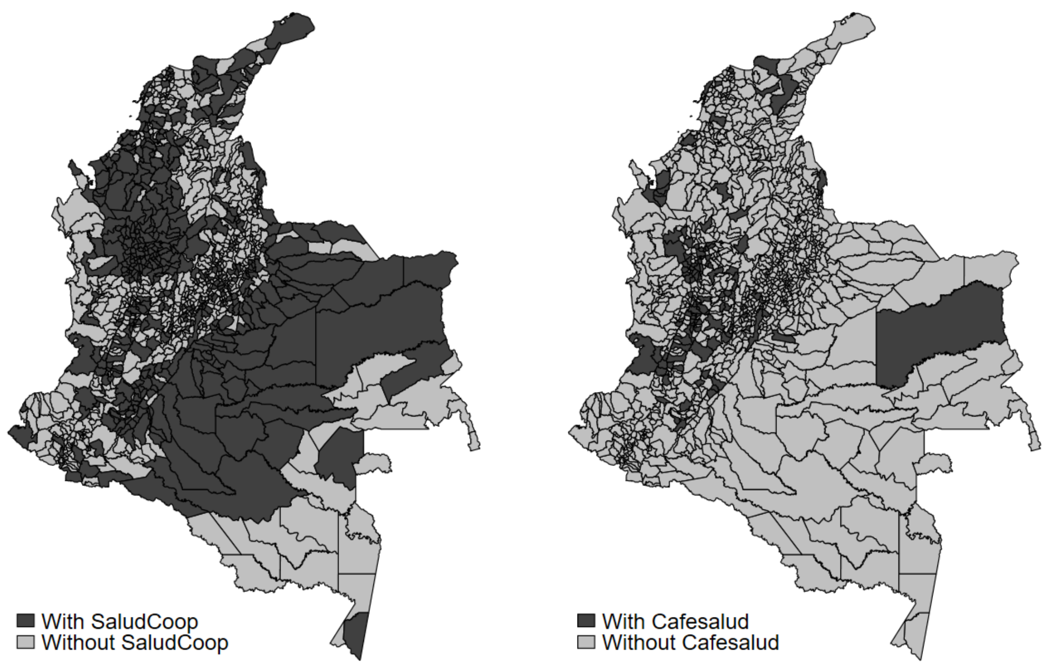
Appendix A Descriptives

Appendix Table 1: Sample restrictions

Sample restriction	Observations
Full sample	66,498,109
Continuous enrollment	47,910,916
No insurer switching + No 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

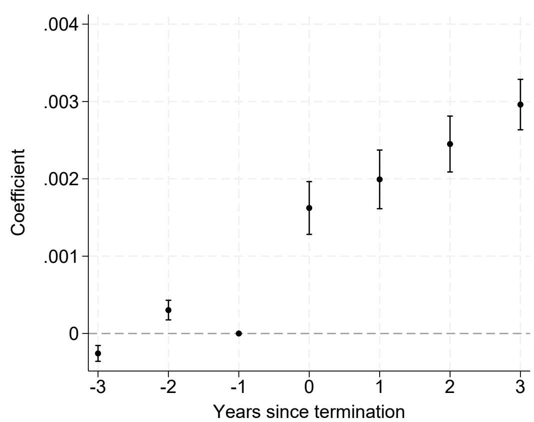


Note: The left panel shows a map of municipalities where SaludCoop was present in 2015 and the right panel shows the municipalities where Cafesalud was present in 2015 in dark gray.

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



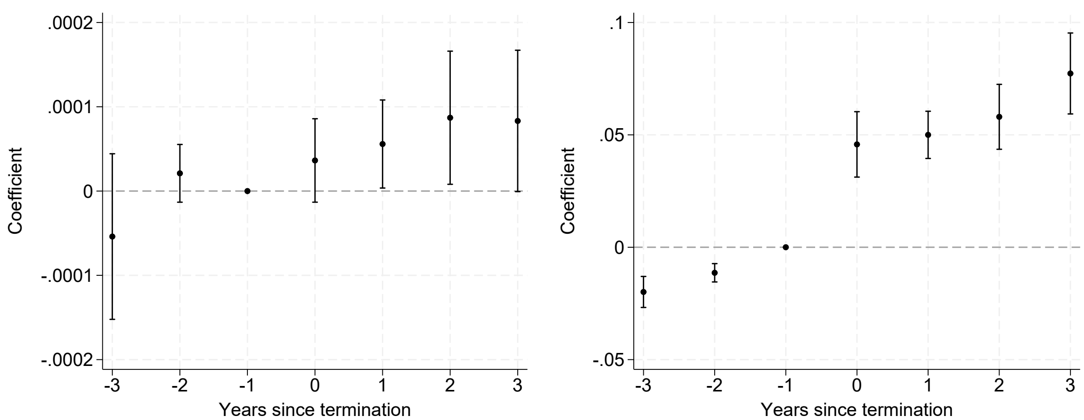
Note: Figure presents interrupted time series coefficients and 95 percent confidence intervals of individual mortality conditional on consumers who were enrolled with SaludCoop prior to its termination. Specification includes municipality fixed effects.

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 Figure 3: 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.

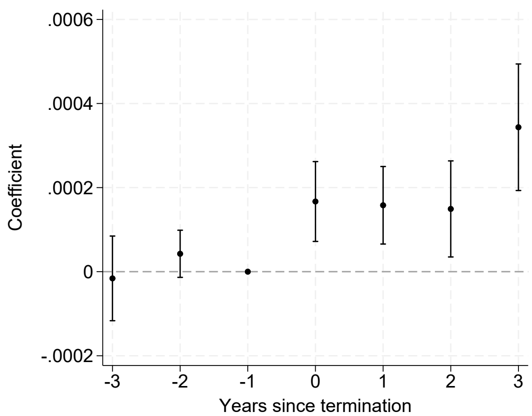
Appendix Table 2: Congestion Effect

	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

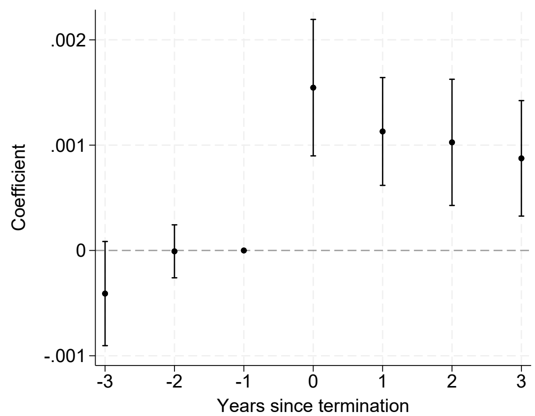
Note: Table reports coefficients and standard errors in parenthesis of a regression of individual mortality on time indicators relative to SaludCoop's termination. Specifications include demographic controls, and insurer and municipality fixed effects. Standard errors are clustered at the municipality 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 Figure 4: Heterogeneity in Mortality Effects by Age Group

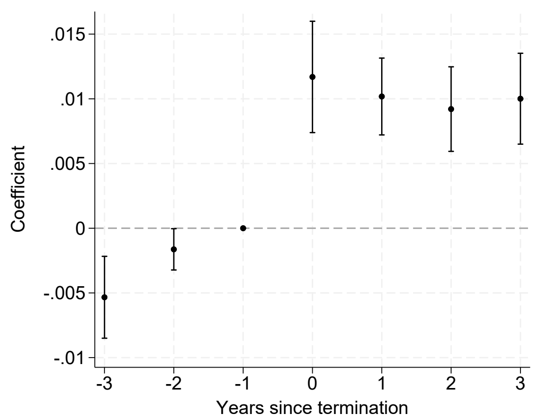
(a) Age less than 35



(b) Age 35-69



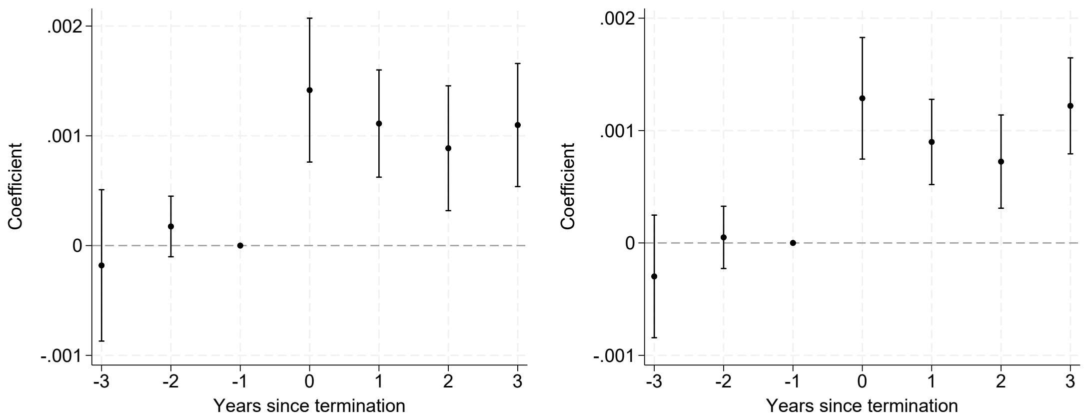
(c) Age more than 69



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 5: 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 Table 3: Congestion Effect

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

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).

Appendix Table 4: Sources of Congestion

	Visits per provider (1)	Providers per enrollee (2)
t-3	0.6907	6.286
	(2.6427)	(6.1150)
t-2	5.1827	7.5148
	(4.4660)	(6.3506)
t-1	(ref)	(ref)
t+0	8.0661	-17.124
	(2.7401)	(10.274)
t+1	13.045	-32.100
	(3.1637)	(9.0562)
t+2	21.464	—
	(5.5171)	
t+3	41.355	—
	(6.8737)	
Observations	7,444,963	20,264

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	0.5074	0.0514
	(0.3691)	(0.0344)
t-2	0.5420	0.0462
	(0.3692)	(0.0290)
t-1	(ref)	(ref)
t+0	-0.7596	0.0219
	(0.3559)	(0.0336)
t+1	-1.0513	0.1153
	(0.4179)	(0.0440)
t+2	-2.3121	0.0163
	(0.4345)	(0.0478)
t+3	-2.3423	0.0057
	(0.5110)	(0.0469)
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 Table 6: Congestion Effect by Health Care Setting

	Outpatient claims (1)	Prescription claims (2)	Urgent care claims (3)	Visits with specialist (4)
t-3	0.5736	0.3392	0.0176	-0.0306
	(0.2850)	(0.1879)	(0.0633)	(0.0269)
t-2	0.2600	0.3279	0.0819	-0.0385
	(0.2904)	(0.1918)	(0.0641)	(0.0258)
t-1	(ref)	(ref)	(ref)	(ref)
t+0	-0.4726	-0.1380	-0.0357	-0.0740
	(0.2278)	(0.1480)	(0.0556)	(0.0251)
t+1	-0.9657	-0.3925	-0.0717	-0.0565
	(0.3476)	(0.1897)	(0.0571)	(0.0278)
t+2	-1.8716	-0.5620	-0.1439	-0.1239
	(0.3610)	(0.3840)	(0.0627)	(0.0261)
t+3	-1.6452	-1.2740	-0.1780	-0.1762
	(0.4334)	(0.2163)	(0.0635)	(0.0295)
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	-0.0074	0.0327	-0.0310	0.0011
	(0.0180)	(0.1028)	(0.0272)	(0.0042)
t-2	0.0068	0.0431	-0.0226	-0.0022
	(0.0173)	(0.0960)	(0.0281)	(0.0037)
t-1	(ref)	(ref)	(ref)	(ref)
t+0	-0.0645	-0.3537	-0.1507	-0.0104
	(0.0200)	(0.1538)	(0.0337)	(0.0044)
t+1	-0.0917	-0.2657	-0.1185	-0.0082
	(0.0211)	(0.1197)	(0.0380)	(0.0040)
t+2	-0.0848	-0.5981	-0.1184	-0.0134
	(0.0223)	(0.1213)	(0.0398)	(0.0046)
t+3	-0.0736	-0.4429	-0.1433	-0.0087
	(0.0230)	(0.1629)	(0.0517)	(0.0048)
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.

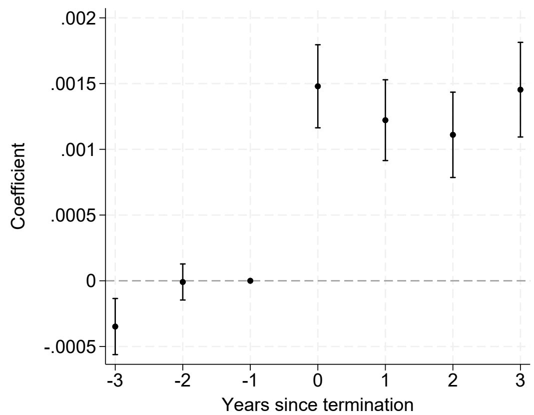
Appendix Table 8: Congestion Effect by Type of Hospital

	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 D Robustness Checks

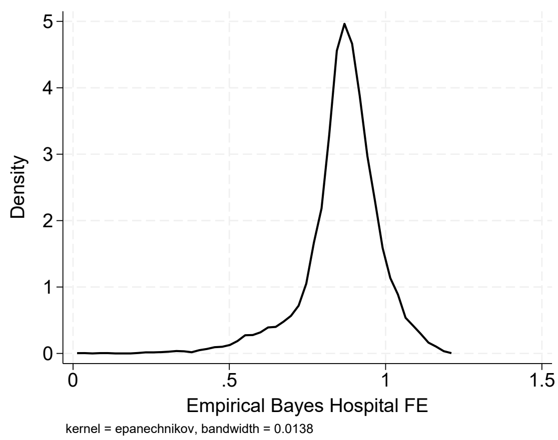
Appendix Figure 6: Congestion Effect Excluding Bogotá and Medellín



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 E First-Stage Regressions

Appendix Figure 7: Distribution of Bayes-Adjusted Hospital Fixed Effects



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

	(1)	(2)	(3)	(4)
	Raw		Quality-adjusted	
Instrument	0.6963	0.6866	0.6897	0.6795
	(0.0443)	(0.0450)	(0.0427)	(0.0433)
Demographic controls	No	Yes	No	Yes
F statistic	246.72	232.72	260.44	246.16
IQ range network breadth	[0.288, 0.497]		[0.235, 0.412]	
Individuals x Years	38,651,482		38,651,482	

Note: Columns (1) and (2) report first-stage regression results of municipal network breadth on the interaction between treatment indicator, post-termination period indicator, and municipal network breadth in 2015. Columns (3) and (4) report first-stage regression results of quality-weighted municipal network breadth on the interaction between treatment indicator, post-termination period indicator, and quality-weighted municipal network breadth in 2015. Columns (2) and (4) include demographic controls (sex and age). All specifications include municipality-by-year fixed effects. Standard errors are clustered at the municipality level.

Appendix Table 10: Placebo Test on Violent Deaths

	(1)	(2)	(3)	(4)
	Raw		Quality-adjusted	
Instrument	-0.00008	-0.00006	-0.00011	-0.00007
	(0.00005)	(0.00004)	(0.00006)	(0.00005)
Demographic controls	No	Yes	No	Yes
Individuals x Years	38,471,979		38,471,979	

Note: Table reports OLS reduced-form regressions of an indicator for violent deaths on our instrument. Columns (1) and (2) use our instrument for municipal network breadth. Columns (3) and (4) use our instrument for municipal network breadth weighted by the average in-network provider quality. Columns (2) and (4) include demographic controls (sex and age). All specifications include municipality-by-year fixed effects. Standard errors are clustered at the municipality level.

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

	(1) Raw	(2) Quality-adjusted
Instrument	-9.7926	-10.026
	(5.8549)	(5.933)
Municipalities x Years	7,005	7,005

Note: Table reports OLS reduced-form regressions of fetal deaths per 1,000 enrollees on our instrument. Column (1) uses our instrument for municipal network breadth. Column (2) uses our instrument for municipal network breadth weighted by the average in-network provider quality. Columns (2) includes demographic controls (sex and age). All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level.

Appendix Table 12: Placebo Test on Deaths by Suicide

	(1)	(2)	(3)	(4)
	Raw		Quality-adjusted	
Instrument	5.4E-06	7.9E-06	5.1E-06	8.3E-06
	(7.3E-06)	(7.7E-06)	(8.8E-06)	(9.3E-06)
Demographic controls	No	Yes	No	Yes
Individuals x Years	38,468,907		38,468,907	

Note: Table reports OLS reduced-form regressions of an indicator for deaths by suicide on our instrument. Columns (1) and (2) use our instrument for municipal network breadth. Columns (3) and (4) use our instrument for municipal network breadth weighted by the average in-network provider quality. Columns (2) and (4) include demographic controls (sex and age). All specifications include municipality-by-year fixed effects. Standard errors are clustered at the municipality level.

Appendix Table 13: Reduced-Form Estimates

	(1)	(2)	(3)	(4)
	Raw		Quality-adjusted	
Instrument	-0.0111	-0.0040	-0.0133	-0.0047
	(0.0026)	(0.0016)	(0.0031)	(0.0019)
Demographic controls	No	Yes	No	Yes
Individuals x Years	38,651,482		38,651,482	

Note: Columns (1) and (2) report reduced-form estimates of individual mortality on the interaction between treatment indicator, post-termination period indicator, and municipal network breadth in 2015. Columns (3) and (4) report reduced-form estimates of individual mortality on the interaction between treatment indicator, post-termination period indicator, and quality-weighted municipal network breadth in 2015. All specifications include municipality-by-year fixed effects. Standard errors in parenthesis clustered at the municipality level.