The Role of Provider Networks in Individual Mortality*

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Abstract

Insurers with narrow provider networks have proliferated in health systems with managed care. We investigate the causal effect of provider network breadth on mortality leveraging the termination of the largest health insurer in Colombia. The termination caused a substantial reduction in network breadth among incumbent insurers and a 22% increase in mortality. We estimate that this mortality increase is generated by the reduction in network breadth because narrow networks are more likely to experience congestion and interruptions in care. Results imply that policies requiring minimum network coverage are needed to maintain patient health.

Keywords: Mortality, Provider networks, Health Insurance, Health Care Cost.

JEL codes: I10, I11, I13, I18.

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1 Introduction

Health insurers fulfill the role of pooling the financial risk associated with their enrollees' medical costs to protect them against unexpected medical bills. Within this remit, a key issue has been the design of the financial features of insurance contracts (premiums, co-payments, deductibles, etc.) to deal with the market failures of moral hazard and adverse selection.[1] Conditional on these financial features, health insurance was traditionally mostly a homogeneous good. However, with the increased popularity of managed care, insurers have taken an active role in influencing the care that individuals receive, which has lead to insurers differentiating across non-financial features (Glied, 2000; Glazer and McGuire, 2000).

As expected, consumers have preferences over the non-financial features of their health plans, such as which benefits are covered, how service utilization is monitored, whether referrals are required to see specialists, etc. One feature that has gained recent interest among researchers and policymakers is provider network breadth, which encompasses the density and quality of providers covered by the health plan (Dafny et al., 2017, 2015b; Gruber and McKnight, 2016).

Existing research has studied the effect of provider network breadth on insurance premiums (e.g., Polsky et al., 2016), hospital prices (e.g., Ho and Lee, 2019), and health care utilization and spending (e.g., Wallace, 2023), but the consequences of narrow networks on health outcomes are largely unexplored. We aim to fill this gap in the literature leveraging the ideal context of the Colombian health care system. This system has near-universal insurance coverage and provides access to a national health insurance plan through private insurers. Similar to Medicaid Managed Care in the U.S., insurers compete only on the breadth of their provider networks, while all other elements of the insurance contract are

[1] The literature is extensive and includes theoretical and empirical contributions such as Rothschild and Stiglitz (1976); Ellis (1986); Cutler and Zeckhauser (2000); Vera-Hernández (2003); Einav et al. (2010); Aron-Dine et al. (2015); Kowalski (2015); Handel and Kolstad (2015); Brot-Goldberg et al. (2017); Einav and Finkelstein (2018a,b).

regulated, including premiums, cost-sharing, services, and benefits.[2]

Our empirical strategy leverages the termination in December 2015 of the largest health insurer in the country, called SaludCoop, and the 38 hospitals that were vertically integrated with it. The government terminated SaludCoop because it diverted nearly 1.3% of total healthcare spending to investments outside the healthcare system and its board of directors engaged in illegal activities. SaludCoop covered 20% of enrollees in the country, who were transferred to an incumbent insurer called Cafesalud. Before the termination, Cafesalud had less than 5% market share. SaludCoop's enrollees had to remain with Cafesalud for 90 days before they could switch. We use this exogenous shock to consumers' choice set of insurers and providers to quantify the causal effect of provider network breadth (defined as the fraction of providers in a municipality that are covered by an insurer) among incumbent insurers on patient mortality (excluding SaludCoop and Cafesalud).

We have the universe of individual-level insurer choices and vital statistics from 2013 to 2019, health claims from half of the country's population who pay payroll taxes in the same period, and data on insurers' network of covered providers between 2013 and 2017. We study effects at incumbent insurers in a difference-in-differences framework, comparing non-SaludCoop enrollees' outcomes in municipalities where SaludCoop (and its hospitals) operated versus municipalities where it did not operate, before and after the termination.

First, we find that incumbent insurers in treated municipalities reduced provider network breadth around 10% relative to baseline. The reduction in provider coverage is consistent with incumbent insurers engaging in strategic behavior to discourage enrollment from potentially unprofitable switchers. Then, we estimate a persistent 22% increase in individual mortality after the termination among patients who never switched their insurer. Most of the impact on mortality comes from individuals with chronic health conditions who see their healthcare treatments interrupted.

Can the reduction in provider network breadth cause the mortality increase at incumbent

[2] 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.

insurers? SaludCoop's termination gives us ideal quasi-experimental variation in insurer and provider choice sets to identify this causal effect. We start by showing that mortality increased substantially among consumers who had most of their pre-period claims at providers that were dropped from the network after the termination. Then, we use an instrumental variables approach to directly relate provider network breadth to mortality. We regress individual mortality on provider network breadth using SaludCoop's termination as an instrument. Our estimates show that broad provider networks significantly reduce patient mortality. A lower interquartile-range increase in provider network breadth, which corresponds roughly to adding 9 provider organizations to the network in the average municipality, reduces mortality by 0.8 per 1,000 individuals. We show that our results are robust to excluding markets where SaludCoop operated with its hospitals and to imposing other sample restrictions that prevent potential violations of the exclusion restriction.

Given the importance of provider network breadth for patient health, our findings suggest that policies targeting network design or insurer competition on provider networks can have downstream effects on mortality. Findings also indicate that broad network coverage can improve continuity of care for patients and make insurers more resilient to shocks to competition.

Contributions and relation to the literature. This paper contributes to the growing literature analyzing the causal effects of narrow provider networks in managed care health systems, which has focused on outcomes like utilization, spending, and premiums (e.g., Wallace, 2023; Shepard, 2022), and relates more broadly to the study of how health insurance affects health outcomes (e.g., Conti and Ginja, 2023; Abaluck et al., 2021; Miller et al., 2021; Bauernschuster et al., 2020; Wherry and Miller, 2016; Sommers et al., 2014; Baicker et al., 2013; Finkelstein et al., 2012). We show that in managed care systems provider network breadth is the mechanism through which insurance coverage impacts patient mortality.

Our paper is also related to the literature analyzing interruptions in health care due to involuntary patient switches of insurer or provider (e.g., Bonilla et al., 2024; Chamorro et al., 2024; Sabety, 2023; Politzer, 2021; Barnett et al., 2017; Lavarreda et al., 2008). We contribute to this literature by studying an insurer termination that was politically motivated and unrelated to its overall performance and by providing general equilibrium estimates of changes in health and market outcomes. We document how incumbent insurers react strategically to a competitor's termination and then show how these decisions hurt patient health.

Finally, this paper contributes to the literature studying insurer competition on provider networks and its regulation. Several papers examine the relationship between provider network breadth, premiums, and negotiated health service prices (e.g., Ghili, 2022; Liebman, 2022; Ho and Lee, 2019; Ho, 2009; Dafny et al., 2017, 2015b). Other papers analyze insurers' incentives to establish narrow networks (e.g., Serna, 2024b; Shepard, 2022; Ho and Lee, 2017). Yet, to date, no paper has shown the effect of provider network breadth on patient health. In doing so, we bridge the literature on the industrial organization of health care markets and health outcomes research.[3]

The rest of this paper is structured as follows. Section 2 introduces the institutional background, section 3 describes our data, section 4 presents the results of the impact of the termination on provider network breadth, section 5 presents the results of the impact of the termination on mortality, section 6 estimates the causal effect of provider network breadth on mortality using the termination as instrument, and section 7 concludes.

2 Institutional Background

The Colombian health care system is divided into a contributory and a subsidized scheme. The first covers half of the population in the country who are formal workers (and their families) and pay payroll taxes. The general budget fully funds the second. As of 2020,

[3] There are a few papers in this area, such as Gaynor et al. (2013) and Propper et al. (2008) who estimate the impact of hospital market power on patient outcomes in the context of the National Health Service in the UK.

nearly 95% of the population was covered by the system.[4]

Insurance plan and provider networks. Both contributory and subsidized scheme enrollees have access to the same national health insurance plan through private and public insurers, constituting a managed care system. Almost every aspect of the national insurance plan, such as premiums, patient cost-sharing, and benefits package coverage, is regulated. However, provider networks are unregulated: insurers in Colombia can choose which providers to cover for each health service included in the national insurance plan and can establish contracts freely with them through bilateral negotiations.

Premiums and risk-adjustment. 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 compensated for their enrollees' health based on a coarse list of diagnoses known as the High-Cost Account. The risk adjustment mechanisms are imperfect and do not eliminate risk selection incentives (Riascos, 2013).

Adverse selection and competition. Insurers in Colombia respond to these selection incentives using their provider networks (Serna, 2024b). Although consumers, on average, have a preference for broader networks, this preference is stronger among those with chronic diseases who are potentially unprofitable. Hence, to discourage enrollment from these unprofitable patients, insurers offer narrow networks in the services those patients are likely to need. Incentives to establish broad provider networks are also affected by the degree of insurer competition.[5] Relatively more concentrated insurance markets tend to have narrower networks (Serna, 2024a). Shocks to competition, such as insurer terminations, may, therefore, incentivize incumbent insurers to reduce the number of covered providers with the goal of maximizing profits.

Network breadth and patient health. Changes in provider network breadth can in turn

[4]See https://www.minsalud.gov.co/Paginas/Colombia-sigue-avanzando-en-la-cobertura-universal-en-salud-.aspx

[5]Ho and Lee (2017) and Dafny et al. (2015a) also show that the degree of insurer competition impacts premiums in settings where insurers compete along that dimension.

impact patient health outcomes. Patients who seek most of their care from providers excluded from the network will likely experience interruptions in care, and remaining in-network providers will likely experience congestion. The medical literature documents that care interruptions are associated with adverse health outcomes (Neprash and Chernew, 2021; Baum et al., 2019), and that congestion is related to increased disease burdens (Yu et al., 2020; Cooper et al., 2019).

Insurer terminations in Colombia. In this paper, we leverage insurer terminations in Colombia to identify the causal effect of provider networks on patient health. The Colombian government can terminate insurers if they divert resources away from the health care system.[6] In December 2015, the government terminated the largest health insurer in the country, called SaludCoop, due to political considerations and engagement in illegal activities.[7] Its board of directors diverted nearly 1.3% of total health care spending in 2015 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 from working in public office, and prohibited from participating in public auctions for at least 18 years. Appendix B provides a termination timeline.[8]

SaludCoop's enrollees were transferred to an incumbent insurer called Cafesalud. The government chose Cafesalud as the reassignment insurer because (allegedly) it operated in almost the same municipalities as SaludCoop did (see Appendix Figure 1). SaludCoop's enrollees had to remain in Cafesalud for 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 providers

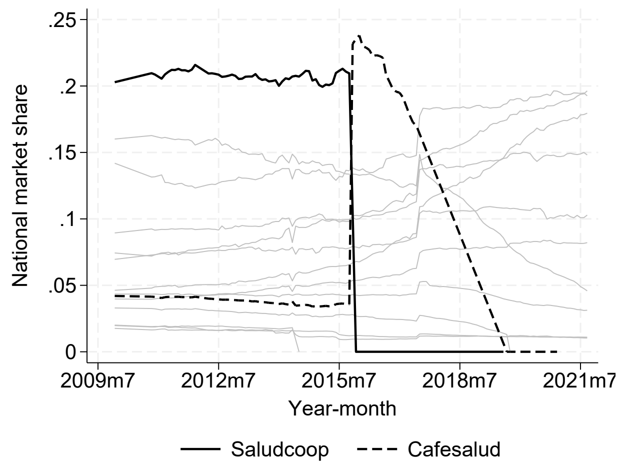
[6] Other reasons for termination include low enrollee satisfaction scores based on surveys conducted by the Ministry of Health and inability to maintain their risk-based capital requirements. See Decree 780 of 2016.

[7] More recently, other health insurers that operate in the subsidized regime have filed for bankruptcy and have been terminated by the government as a result (see e.g., Bonilla et al., 2024). These terminations have been made on the basis of insurers being unable to maintain their risk-based capital requirements and receiving enrollee complaints about their quality of care. This is unlike SaludCoop's termination, which was a profitable company when the government decided to intervene.

[8] More 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.

that SaludCoop used to cover in its network, in addition to those already in Cafesalud's network. The government made a $70 million loan to Cafesalud to facilitate this transition.

FIGURE 1: National Market Share



Note: Figure shows monthly national market share per insurer from 2009 to 2021 using data from all enrollees in the contributory and subsidized regimes.

Figure 1 shows the national market share per insurer in the contributory scheme. We emphasize SaludCoop and Cafesalud in black, and the rest of the insurers are illustrated in gray. SaludCoop (solid black line) covered an average of 20% of enrollees in the years before its termination.[9] SaludCoop and Cafesalud participated in both the contributory and the subsidized schemes. Cafesalud had a national market share under 5% before the termination, 23% in the first three months of 2016, and was itself terminated in 2019.

SaludCoop's termination forced substantial changes in the provision of health insurance and health care in Colombia. Fines and debts from this process continue to be paid to this day. Not only did the termination reduce the number of available insurers, 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 did not happen during our sample period (2013-2019).

[9] On average, SaludCoop's market share in a municipality was 50%.

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, insurers other than SaludCoop and Cafesalud covered SaludCoop hospitals. Apart from the 31 municipalities where SaludCoop operated with hospitals, it also operated in 452 municipalities without its own hospitals.

3 Data

3.1 Data sources and definitions

Our enrollment data comprises all enrollees to the contributory and subsidized schemes, nearly the entire population in the country. We have a snapshot of enrollment data for every June from 2013 to 2019, corresponding to three years before and four after SaludCoop's termination. Because we do not see enrollment every month, we assume that if an individual is enrolled with insurer A in June 2013, they remain with this insurer every month until June 2014 when we see the next enrollment snapshot.[10] The enrollment files contain the individual's sex, age, municipality of residence, and insurer.

At the end of every year, insurers in the contributory and subsidized schemes report all of their enrollees' health claims to the government. The government uses this data annually to update the risk-adjusted transfers and imposes several data quality filters. We have claims data only for insurers in the contributory scheme that passed these quality filters from 2013 to 2019, which represent 88% of enrollees by the end of the sample period.

The claims data correspond to enrollees in the contributory scheme, which comprise approximately half of the country's population. We do not have claims data for individuals in the subsidized scheme. The claims data report the date the claim was filed, enrollee identifier, associated ICD-10 diagnosis code, provider that rendered the claim, insurer that

[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 (2024b).

reimbursed the claim, and negotiated service price between the insurer and the provider. We do not observe the patient's residence address but their municipality of residence.

From the Ministry of Health and Social Protection, we obtain individual-level mortality and vital statistics from 2013 to 2019. Anonymous individual identifiers are the same across datasets, allowing us to merge mortality with enrollment and claim information. The mortality data report 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 of whether the individual died 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 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 violent deaths from the analysis since for fetal deaths there is no patient identifier and violent deaths are potentially unrelated to the provision of health care.

Finally, we have data on insurers' network of covered providers from 2013 to 2017 from the National Health Superintendency. This data reports all provider inclusions to each insurers' network.[11]

3.2 Sample restrictions

For our analysis, we compare mortality patterns among non-SaludCoop enrollees across municipalities where SaludCoop operated at the time of the termination (treated) against municipalities where SaludCoop did not operate (control). We restrict our data in several ways to guarantee that treated and control groups are similar before the termination. These restrictions help control for differential adverse selection patterns across treatment status before the termination.

[11] Our provider network data does not report which services are covered with each in-network provider.

First, as discussed before, *we exclude individuals who are enrolled with SaludCoop or Cafesalud* before SaludCoop's termination, thus 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. Third, we keep a balanced panel of insurer-municipalities. Lastly, we drop individuals for whom we see enrollment data after they die.

These above restrictions limit selection on insurer choice that is endogenously caused by changes in insurer characteristics such as the breadth of their provider network. However, the restrictions may come at a cost in terms of the representativeness of our results. Individuals who do not switch insurers are exposed for as long as possible to any disruption of care induced by SaludCoop's termination, maximizing the adverse effects on patient health. In any case, those who did not switch their insurer represent a sizable fraction of the population. Appendix Table 1 shows the number of observations that result after imposing each sample restriction.

3.3 Summary statistics

Summary statistics for our final sample of insurers and enrollees are provided in Tables 1 and 2. In both tables, we report each variable's mean and standard deviation separately for treated and control municipalities in the pre- and post-termination periods. In Table 1, an observation is a combination of insurer, municipality, and year. Summary statistics in this table are weighted by the number of enrollees. We measure provider network breadth as the fraction of covered municipal providers for every insurer. Treated municipalities see a decrease in average provider network breadth in the post-period, while control municipalities see an increase in this measure of coverage. Provider network breadth is substantially higher in control municipalities at baseline because they have fewer providers. For reference, the average change in annual provider network breadth in the pre-period was 2 percentage points (p.p).

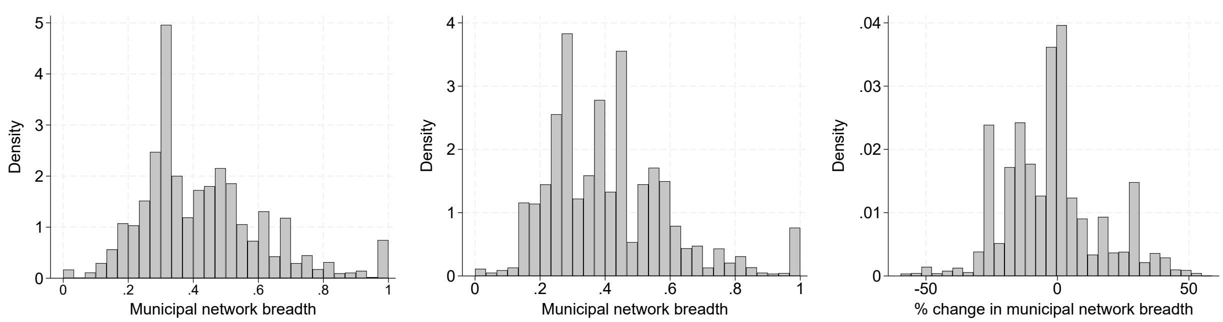
Table 1: Summary Statistics of Insurer Sample

Variable	Treated		Control	
	Pre	Post	Pre	Post
Provider network breadth	0.426 (0.189)	0.412 (0.187)	0.742 (0.326)	0.766 (0.299)
Market share (in total enrollees)	0.183 (0.128)	0.199 (0.141)	0.481 (0.237)	0.506 (0.248)
Insurers	11	11	11	11
Municipalities	483	483	636	635
Insurer×Municipality×Year	8,236	4,336	7,704	3,530

Note: Table presents the mean and standard deviation in parenthesis of insurer characteristics for the analysis of changes in provider networks. Summary statistics are weighted by the number of enrollees and presented separately for treated (where SaludCoop operated) and control municipalities, in the pre- and post-termination periods. The data is from 2013 to 2017. An observation is a combination of insurer, municipality, and year. The sample of insurers excludes SaludCoop and Cafesalud. Provider network breadth is the fraction of providers in a market that are covered by the insurer. Market share is the insurer's share in the number of total enrollees in a municipality (without imposing sample restrictions).

Figure 2: Distribution of Provider Network Breadth in Treated Municipalities

(a) Pre (b) Post (c) Change



Note: Panel A shows the distribution of network breadth across insurers in treated municipalities in the pre-termination period. Panel B shows the distribution in the post-termination period. Panel C shows the distribution of percentage changes in network breadth in the post-period relative to the pre-period. In panels A and B, the number of enrollees determines the density. In Panel C, the density is determined by the number of enrollees in the pre-period.

Panels A and B of Figure 2 show that provider network breadth is substantially heterogeneous across insurers in treated municipalities in the pre- and post-periods, respectively. Panel C also shows substantial heterogeneity in percentage changes in provider network breadth after the termination. These histograms have densities given by the number of enrollees in the pre-period. Thus, Panel C shows that most individuals were enrolled with insurers that narrowed the network in their municipality of residence. For example, 5.8 million individuals were enrolled with insurers that narrowed their network by more than 10%, and 1.7 million were enrolled with insurers that narrowed their network by more than 25%. We also see that some insurers expanded their network in the post-period. This increase can be rationalized by the fact that not only sick, unprofitable consumers value broad networks but also healthy, profitable ones. We will return to this point in section 4.1.

Table 2 shows an increase in the average mortality rate and the Charlson index among treated municipalities in the post-period (each observation is an enrollee-year).[12] Control municipalities see no change in the average mortality rate but a similar increase in the Charlson index. Treated municipalities have a higher prevalence of chronic conditions at baseline than controls, and this prevalence is fairly stable over time.

TABLE 2: Summary Statistics of Enrollee Sample

Variable	Treated		Control	
	Pre	Post	Pre	Post
Mortality	0.0037 (0.0609)	0.0048 (0.0688)	0.0019 (0.0435)	0.002 (0.0451)
Charlson index	0.280 (0.825)	0.318 (0.920)	0.261 (0.756)	0.307 (0.853)
Male	0.463 (0.499)	0.465 (0.499)	0.486 (0.500)	0.484 (0.500)
Age	36.04 (22.78)	34.20 (23.21)	32.73 (22.95)	31.13 (22.99)
Contributor (vs. beneficiary)	0.308 (0.462)	0.285 (0.452)	0.040 (0.196)	0.035 (0.185)
Acute myocardial infarction	0.001 (0.036)	0.002 (0.040)	0.0002 (0.014)	0.0002 (0.015)
Chronic obstructive pulmonary disorder	0.016 (0.126)	0.016 (0.126)	0.002 (0.043)	0.002 (0.043)
Hepatic disease	0.0004 (0.02)	0.0005 (0.022)	4e-5 (0.007)	0.0001 (0.007)
Renal disease	0.012 (0.109)	0.014 (0.117)	0.002 (0.04)	0.002 (0.047)
Cancer	0.008 (0.091)	0.012 (0.109)	0.001 (0.032)	0.002 (0.040)
Individuals	14,187,207	18,272,675	2,908,495	3,749,307
Individuals×Years	38,744,205	61,907,372	7,921,815	12,435,689
Municipalities	482	482	624	624

Note: Table presents the mean and standard deviation in parenthesis of the sample of enrollees for the mortality analysis. Summary statistics are presented separately for individuals living in treated (where SaludCoop operated) and control municipalities, in the pre- and post-termination periods. An observation is an individual-year and the data is from 2013 to 2019. The sample of enrollees is restricted to those who never switched their insurer during the years where we observe them, who never moved across municipalities before the termination, and who were enrolled with insurers other than SaludCoop and Cafesalud. Our final sample of enrollees does not constitute a fixed cohort.

[12] The Charlson index is a measure of health status, with a higher index denoting a sicker individual (see `https://healthcaredelivery.cancer.gov/seermedicare/considerations/comorbidity.html`). We constructed it using the claims data following Oliveros and Buitrago (2022).

4 Reduced-Form Impact on Provider Networks

We start our analysis by using a difference-in-differences (*did*) design to estimate the reduced-form effect of SaludCoop's termination on measures of provider network coverage. We compare municipalities where SaludCoop operated during 2015 (treated group) against 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:

$$H_{jmt} = \sum_{\substack{k=-3 \\ k \neq -1}}^{3} \beta_k 1\{t - 2016 = k\} \times T_m + \delta_j + \gamma_m + \eta_t + \varepsilon_{jmt} \tag{1}$$

where H_{jmt} is insurer j's provider network breadth in municipality m during year t, T_m is an indicator for treated municipalities, and δ_j, γ_m, and η_t are insurer, municipality, and year fixed effects, respectively. Providers can be either hospitals, small clinics, or physician practices. Whenever we use the term "provider," we refer to these health care provider organizations.

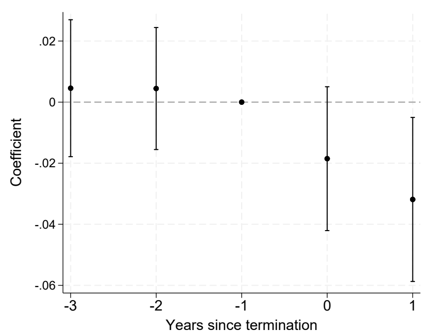
SaludCoop's termination occurred in December 2015, which is visible in our June 2016 enrollment data snapshot. The relative time indicators in equation (2) are thus constructed relative to 2016, and the omitted category is 2015. The coefficients β_k measure the average treatment effect in year k relative to 2015. Because the termination happens simultaneously for all municipalities in our treated group, we do not worry about the identification challenges from staggered treatment implementation. We cluster our standard errors at the municipality level.

Identification of the dynamic treatment effect relies on the assumption that outcomes in the treated group would have evolved as in the control group had the termination not occurred. Identification can be threatened if there are unobserved variables related to SaludCoop's location decisions and post-termination provider network trends. A violation of this assumption would likely result in significant pre-trends.

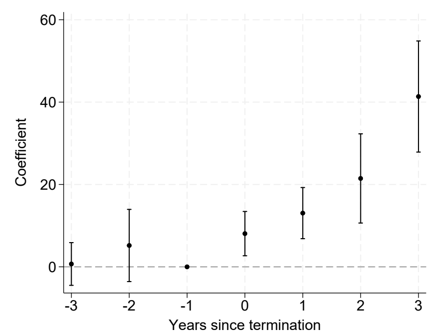
Figure 3 presents the results. First of all, we see evidence of parallel pre-trends in network coverage in line with descriptive patterns presented in Appendix Figure 3. Panel A shows that provider network breadth decreased between 2 and 4 p.p after the termination, a 10% reduction relative to baseline. As a result of narrower networks, Panel B shows that providers in treated municipalities had approximately 20 more visits or consultations two years after the termination relative to providers in control municipalities, an effect that represents a 19% increase over baseline. These results are robust to excluding municipalities where SaludCoop owned hospitals as seen in Panels C and D.

Figure 3: Impact on Providers Networks

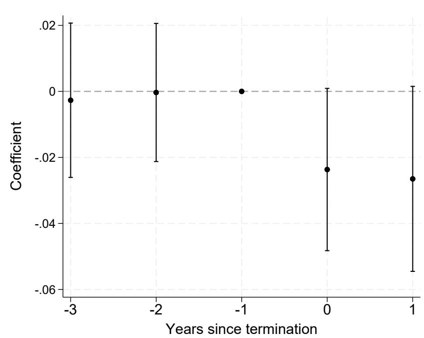
(a) All markets: Provider network breadth



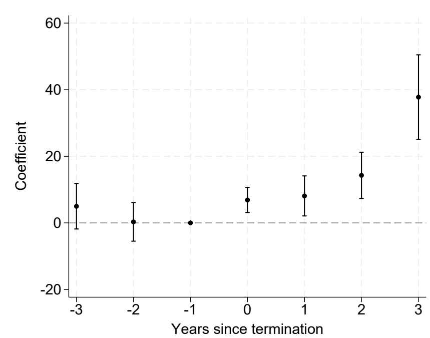
(b) All markets: Visits per provider



(c) Without SaludCoop hospitals: Provider network breadth



(d) Without SaludCoop hospitals: Visits per provider



Note: Figure shows event study coefficients and 95% confidence intervals of measures of network coverage. In Panel A the outcome is provider network breadth. This regression uses data at the insurer-municipality-year and conditions on insurers that have more than 0.05% market share in the municipality. We include insurer, municipality, and year fixed effects. In Panel B the outcome is the number of visits per provider. This specification uses data at the provider-insurer-year level and includes municipality, insurer, provider, and year fixed effects. Panels C and D report corresponding results for provider network breadth and number of visits per provider excluding municipalities with SaludCoop hospitals. Standard errors in all specifications are clustered at the municipality level. Treatment is defined as municipalities where SaludCoop operated during 2015.

4.1 Adverse selection

Why would incumbent insurers respond to SaludCoop's termination by narrowing their networks? Shepard (2022) and Serna (2024b) have shown that insurers respond to adverse selection by narrowing their networks because broader networks are more attractive to sicker consumers. Because insurers distort their contracts to avoid unhealthy consumers, equilibrium contracts are not first-best (Glazer and McGuire, 2000). However, all consumers remain insured because premiums are zero, and no insurance is worse than insurance with narrow networks.

We proceed in three steps to see whether adverse selection can explain why provider networks become narrower after the termination. First, we show that there was a relatively high switching rate out of Cafesalud among individuals previously enrolled with SaludCoop. Second, we show that individuals with chronic diseases have a stronger preference for broader networks than those without chronic diseases. Third, we show that municipalities with sicker SaludCoop's enrollees at baseline saw larger reductions in provider network breadth.

Table 3 shows that 76% of individuals who were enrolled with SaludCoop during 2015 remained in Cafesalud during 2016, but 24% switched to other insurers in that year after the 90-day grace period. An additional 23% of SaludCoop's enrollees moved to other insurers during 2017, which may reflect a large influx of "new enrollees" to these incumbent insurers. Of those enrolled with Cafesalud during 2015, 82% were inertial in 2016, but 41% switched out by 2018 perhaps as a preemptive response to Cafesalud's termination.

Table 3: Distribution of Enrollment Conditional on the 2015 Insurer

	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 beneficiaries enrolled with Cafesalud and other insurers according to their enrollment in 2015.

Table 4 shows how the probability of switching out of an insurer after the termination depends on its network breadth amongst individuals enrolled with SaludCoop in 2015. Independently of whether individuals suffer from chronic health conditions, those enrolled with insurers that have broader networks are less likely to switch out, indicating their preference for broad networks. However, this preference is stronger for individuals with chronic conditions, whose decision to switch out of their insurer is more sensitive to network breadth. Incumbent insurers can therefore avoid SaludCoop's enrollees with worse health status by narrowing their networks. Moreover, the reduction in network breadth could happen soon after the termination because insurers and providers in Colombia negotiate service prices and network inclusions typically at the beginning of every calendar year, so we can expect network changes to happen as soon as the beginning of 2016.

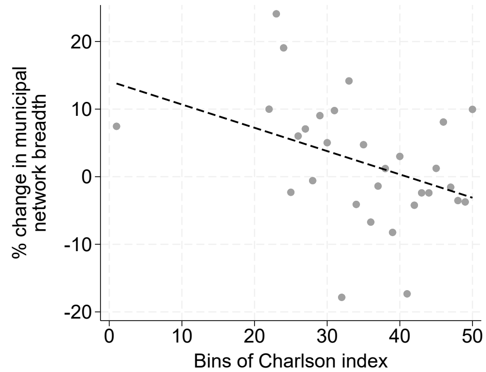
Table 4: Enrollees' Switching Decisions by Network Breadth

	Switch out	
	(1) Without diseases	(2) With chronic diseases
Provider network breadth	-0.0024	-0.0504
	(0.0011)	(0.0030)
Observations	3,057,795	395,464

Note: Table presents pooled OLS regressions of an indicator for switching out of an insurer on that insurer's municipal network breadth. All specifications use data from 2017 to 2019 and are conditional on the subsample of individuals who were enrolled with SaludCoop in 2015, transferred to Cafesalud in 2016, and did not move across municipalities. Column (1) uses the subsample of individuals with Charlson index equal to zero and column (2) uses those with Charlson index greater than zero. Specifications include municipality fixed effects. Standard errors in parenthesis are clustered at the individual level.

To close the adverse selection argument, Figure 4 shows that municipalities with a relatively higher average Charlson index among SaludCoop's enrollees in the pre-period had more significant reductions in average provider network breadth in the post-period.

Figure 4: Correlation between Changes in Networks and Health Status



Note: Figure shows a scatter plot of the average percentage change in network breadth across insurers in treated municipalities (weighted by the number of enrollees) per 50 equally-sized bins of the average Charlson Index among SaludCoop enrollees in those municipalities in the pre-period.

5 Reduced-Form Impact on Mortality

In this section, we quantify the impact of the termination on the mortality of non-SaludCoop enrollees. Our regression of interest is:

$$y_{imt} = \sum_{\substack{k=-3 \\ k\neq -1}}^{3} \beta_k 1\{t - 2016 = k\} \times T_m + x_i'\lambda + \delta_{j(i)} + \gamma_m + \eta_t + \varepsilon_{imt}, \tag{2}$$

where y_{imt} takes the value of 1 if individual i who lived in municipality m died during year t and 0 otherwise, T_m is an indicator for treated municipalities, x_i is a vector of patient characteristics including dummies for the risk adjustment groups and a dummy for being a contributor (vs. a beneficiary or an individual in the subsidized scheme).[13] Finally, $\delta_{j(i)}$, γ_m and η_t are insurer, municipality, and year fixed effects, respectively. We cluster the standard errors at the municipality level.

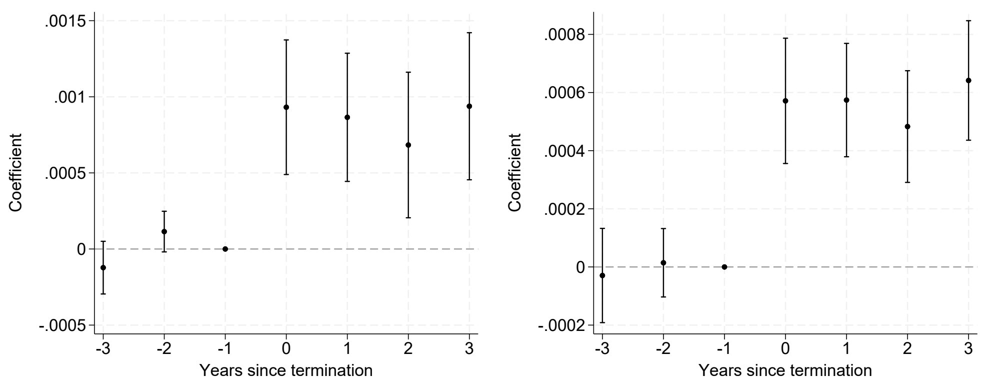
Panel A of Figure 5 presents the results and Appendix Table 4 reports the associated coefficients and standard errors.[14] We find that before the termination, individuals in treated

[13] The risk adjustment groups are: age <1, 1-4, 5-14, male 15-18, female 15-18, male 19-44, female 19-44, 45-49, 50-54, 55-59, 60-64, 65-69, 70-74, 75 or older. We do not include time-varying measures of patient health, such as the Charlson index, because the likelihood of receiving a diagnosis may have also changed after the termination.

[14] For completeness, Appendix C reports mortality trends among SaludCoop's enrollees.

Figure 5: Mortality Effect on non-SaludCoop Enrollees

(a) All markets (b) Without SaludCoop hospitals



Note: Figure shows event study coefficients and 95% confidence intervals of enrollee mortality. Panel A uses information from all markets, and Panel B excludes markets with SaludCoop hospitals. Specifications control for the patient's pre-period sex and age, an indicator for whether they received a low-income score, and an indicator for whether they are a contributor (vs a beneficiary). Specifications include insurer, municipality, and year-fixed effects. Standard errors are clustered at the municipality level. The sample is restricted to individuals who do not switch insurers, had continuous enrollment spells, and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud. Treatment is defined as municipalities where SaludCoop operated in 2015.

and control municipalities had parallel mortality trends, evidenced by statistically zero estimates in 2013 and 2014 and by descriptive trends presented in Appendix Figure 3. The year of the termination, mortality increases 0.93 per 1,000 among non-SaludCoop enrollees, on average a 22% increase relative to the counterfactual mortality rate in the post-period.[15] Appendix Figure 4 shows that the increase in mortality is likely due to diseases which are more sensitive to sudden interruptions or disruptions of care, such as cancer, renal disease, and hepatic diseases. Appendix Figure 5 also shows that mortality increases among individuals covered by the contributory scheme.

Although some of the increase in mortality is probably due to transitory disruptions in health care generated by SaludCoop's termination, we find that the effects on mortality are persistent over time: 3 years after the termination, we estimate a mortality increase in treated municipalities equal to 0.94 per 1,000 among non-SaludCoop enrollees. One possible explanation for this permanent effect on mortality is the decrease in hospital capacity that

[15] Because our sample comprises individuals who do not switch and age during the sample period, we calculate the appropriate counterfactual mortality rate by subtracting the *did* estimate from the average mortality rate in the treatment group each year of the post-period. Then, we divide the *did* estimate by this counterfactual mortality rate, obtaining percentage changes of 24%, 23%, 17%, and 23% from 2016 to 2019.

followed from the closure of the 38 hospitals owned by SaludCoop. However, Panel B of Figure 5 shows that mortality increased permanently even in municipalities where SaludCoop did not own hospitals. In this sub-sample, we estimate an average increase in mortality equal to 23% in the post-period.

The magnitude of our estimates 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 of 11.9% in annual mortality three years after the expansion. Abaluck et al. (2021) estimate a 19% reduction in individual mortality from enrolling with an insurance plan in Medicare Advantage that has a one-standard deviation lower mortality rate. And Card et al. (2009) find that Medicare eligibility reduces 7-day in-hospital mortality by 20%.

5.1 Case Studies

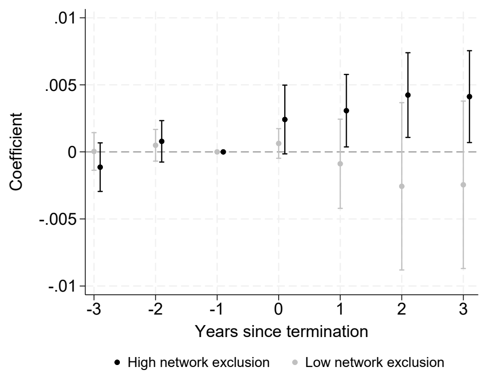
Why would mortality increase among non-SaludCoop enrollees in municipalities where SaludCoop did not own hospitals? And why would this increase be permanent? A potential explanation is the reduction in provider network breadth, which in the U.S. has been associated with worse health outcomes (Schleicher et al., 2016). To shed light on provider network breadth as a potential mechanism for mortality, in this section, we investigate the heterogeneity in mortality effects by whether patients had most of their health care at providers that were excluded from the network after the termination (along the lines of Shepard, 2022).

In Figure 6, we present two sets of results for our event study specification.[16] The estimates in light gray and black compare the control group against non-SaludCoop enrollees in treated municipalities who had a below- and an above-average fraction of pre-period claims at providers that were dropped from the network after SaludCoop's termination, respectively.

[16]In this analysis, we focus on the sub-sample of insurers in the contributory scheme for which we have provider network data and on the sub-sample of enrollees who made at least one claim every year (in addition to the sample restrictions from section 3.2). The latter restriction is needed to obtain the composition of providers for every consumer and this variable is not defined for consumers who do not make claims.

The results show evidence consistent with provider network exclusions driving the mortality effects. There are no significant changes in mortality for consumers with a relatively small fraction of pre-period claims delivered at providers that were eventually dropped from the network. But, we estimate substantial increases in mortality throughout the post-period for their counterparts, which supports the hypothesis that network breadth and provider network exclusions impact health.

FIGURE 6: Mortality Effect by Provider Network Exclusions



Note: Figure presents coefficients and 95% confidence intervals of two event study specifications. In all specifications, the control group are municipalities where SaludCoop did not operate. In light gray, the treated group are municipalities conditional on individuals with a below-average fraction of pre-period claims delivered at providers that were dropped from the network in the post-period. In black, the treated group are municipalities conditional on individuals with above-average fraction of pre-period claims delivered at providers that were dropped from the network in the post-period. The estimation uses the sub-sample of insurers in the contributory system for which we have provider network data and on the sub-sample of enrollees who made at least one claim every year, did not switch insurers, had continuous enrollment spells, and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop or Cafesalud. Treated units are municipalities where SaludCoop operated. Of the 4,257,798 individuals in treated municipalities in our analysis sub-sample, 5% and 95% are represented in black and light gray, respectively. Specifications include insurer, municipality, and year-fixed effects. Standard errors are clustered at the municipality level.

6 The Causal Effect of Provider Network Breadth

Having shown substantial reduced-form effects of SaludCoop's termination on mortality by whether a patient had most of their health care at providers excluded from the network, we now estimate the causal effect of provider network breadth on mortality in a more parsimonious way. In general, identifying this effect is a difficult exercise because differences in mortality can be explained by individuals non-randomly selecting their insurer based on their provider networks. For example, if sick patients have strong preferences for a high-quality provider and this provider is more likely to be covered under a broad-network insurer, we would predict that broad networks increase mortality. Also, if unobservably healthy patients disproportionately enroll with narrow-network insurers, then we would predict that narrow-network insurers reduce patient mortality when, in fact, these insurers had a healthier population of enrollees to begin with.

6.1 Microfoundation

Our measure of provider network breadth can be micro-founded with a discrete choice model where consumers care about specific providers being included in the network (see e.g., Serna, 2024b). This micro-foundation shows that provider network breadth is a proper and convenient way to summarize an insurer's network when consumers have heterogeneous preferences over providers, and that selection biases of the style described in the previous paragraph apply to provider network breadth as well.

Consider a simple model of provider choice where individual i's indirect utility from choosing provider h in the network of insurer j in market m is:

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

where ξ_{hm} captures provider 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 and following McFadden (1996), individual i's value for insurer j's network of providers G_{jm} is:

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

If this model were feasible to estimate, identifying the causal effect of consumers' preferences for provider networks would involve regressing individual mortality on w_{ijm}. However, this provider demand model may be infeasible to estimate due to dimensionality problems or to the fact that the relevant network may be different for different patients. In that case, we can approximate the potentially heterogeneous valuation for the network with a simpler measure of provider network breadth as follows. Let $|G_m|$ be the total number of providers in the market and $|G_{jm}|$ the number of providers in insurer j's network, then:

$$\begin{aligned} w_{ijm} &= \log\Big(\sum_{h\in G_{jm}} \exp(\xi_{hm})\Big) \geq \log\Big(\frac{1}{|G_m|}\sum_{h\in G_{jm}} \exp(\xi_{hm})\Big) \geq \frac{1}{|G_m|}\sum_{h\in G_{jm}} \log(\exp(\xi_{hm})) \quad (3) \\ &= \frac{1}{|G_m|}\sum_{h\in G_{jm}} \xi_{hm} = \frac{|G_{jm}|}{|G_m|}\sum_{h\in G_{jm}} \frac{1}{|G_{jm}|}\xi_{hm} = \bar{\xi}_{jm} H_{jm} \end{aligned}$$

where the second inequality uses Jensen's inequality, $\bar{\xi}_{jm} = |G_{jm}|^{-1}\sum_{h\in G_{jm}} \xi_{hm}$ is the average quality of providers in insurer j's network, and H_{jm} is insurer j's provider network breadth in market m.[17] This relation between valuation for the network and provider network breadth suggests that the regression that is feasible to estimate is:

$$y_{imt} = \alpha\bar{\xi}_{j(i)mt} H_{j(i)mt} + x'_{it}\beta + \delta_{j(i)} + \gamma_{mt} + \epsilon_{imt}, \quad (4)$$

where y_{imt} is observed mortality, x_{it} are exogenous potentially time-varying enrollee characteristics (such as age and sex), $\delta_{j(i)}$ is an insurer fixed effect, and γ_{mt} is a municipality-by-year fixed effect. Estimating equation (4) via OLS would yield an estimate of α that is biased towards zero due to measurement error in the explanatory variable (since $\bar{\xi}_{jmt}$ is estimated and $\bar{\xi}_{jmt} H_{j(i)mt}$ is a downward measure of w_{ijm}) as well as bias arising from the relation between insurer choice and unobservable health status.[18] To consider the latter, rewrite equation (4) as

$$y_{imt} = \alpha\sum_{j} \bar{\xi}_{j(i)mt} H_{j(i)mt} D_{ijmt} + x'_{it}\beta + \delta_{j(i)} + \gamma_{mt} + \epsilon_{imt}, \quad (5)$$

where D_{ijmt} is an indicator variable for whether individual i chooses insurer j in market m and year t. This formulation makes explicit the endogeneity problem since $cov(D_{ijmt}, \epsilon_{imt}) \neq 0$

[17] Appendix F extends this relation to a model of hospital choice that allows for observed preference heterogeneity.

[18] Appendix G derives an expression for the bias.

due to unobserved individual health status. Estimation of (5) is likely infeasible and underpowered because it would require one instrument for every insurer in the choice set. Instead, equation (4) identifies the *average effect* of provider 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 average causal effect on mortality from enrolling with a particular health plan.

6.2 Provider Quality

To construct our main independent variable and later on our instrument, we first calculate provider quality, ξ_{hm}, using provider readmissions data. We estimate the following regression:

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

where b_{it} is an indicator that equals 1 if the t-th admission for individual i does not result in a readmission within 30 days, $\xi_{h(t)}$ is a hospital fixed effect, and x_i is a vector of enrollee characteristics including sex, and dummies for age group (0-24, 25-44, 45-64, 65+) and insurer, and τ_t represents year fixed effects. 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 provider fixed effects toward their municipality-level mean.[19] These fixed effects are invariant over time and insurers. However, to the extent that different insurers cover different providers and change their network inclusions over time, the average quality of in-network providers $\overline{\xi}_{jmt} = |G_{jmt}|^{-1} \sum_{h \in G_{jmt}} \hat{\xi}_h$ will vary across insurers, markets, and years in our final specification. Appendix Figure 6 presents the distribution of the Bayes-adjusted provider fixed effects.

[19]We use the `ebayes` and `fese_fast` codes in Chandra et al. (2016) and Nichols (2008).

6.3 Identification

To overcome the two biases arising from measurement error and non-random selection into insurers and providers, we leverage exogenous *changes* in provider 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 provider network breadth in 2015, $\overline{\xi}_{j(i)m,2015}H_{j(i)m,2015}$, while conditioning on municipality-by-year interactions.

To understand the intuition of our instrument, recall that SaludCoop's termination generated an influx of patients into incumbent insurers after the 90-day grace period with Cafesalud. In Figure 4, we showed that markets where SaludCoop's enrollees were sicker, had larger reductions in network breadth. These changes in provider network breadth relative to 2015 are exogenous to the enrollment decisions of inertial consumers, on whom we focus for estimation. Thus, we want to exploit the temporal rather than the cross-sectional variation in provider network breadth for the identification of the causal effect on mortality.

Formally, the first-stage regression is:

$$\overline{\xi}_{j(i)mt}H_{j(i)mt} = \psi_1\Big(T_m \cdot P_t \cdot \overline{\xi}_{j(i)m,2015} \cdot H_{j(i)m,2015}\Big) + x'_{it}\psi_2 + \delta_{j(i)} + \gamma_{mt} + \nu_{j(i)mt}$$

where variables are the same as described in previous paragraphs. Then, we estimate equation (4) using 2SLS, clustering our standard errors at the municipality level. The estimation sample consists of individuals covered by insurers in the contributory scheme from 2013 to 2017, for which we have provider network data, and who were *not enrolled with either SaludCoop or Cafesalud* (in addition to the sample restrictions from section 3.2).

6.4 Results

Table 5 presents OLS results, Table 6 presents 2SLS results, and Appendix Table 7 presents reduced-form estimates. In each table, column (1) uses provider network breadth, column (2) uses provider network breadth interacted by average provider quality, and column (3) uses provider network breadth interacted by the weighted average of provider quality, weighting by the number of readmissions.[20] In the latter specification, weights for each in-network provider are calculated relative to the total number of admissions for each insurer over the sample period, and thus are constant over time. All columns include demographic controls that perfectly account for the set of variables that the government uses to calculate risk-adjusted transfers to insurers (sex and age).

The main takeaway from the different specifications is that broad provider networks significantly reduce patient mortality. In column (1) of Table 5 we find that increasing provider network breadth from the first to the second quartile of the distribution, which corresponds roughly to adding 9 provider organizations to the network in the average municipality, reduces mortality by 0.2 per 1,000.[21] The reduction in mortality of 0.2 is sizeable given that the national mortality rate in 2015 was 4 per 1,000 individuals excluding violent deaths. The results are very similar when we consider quality-adjusted network breadth in column (2) and quality-adjusted network breadth weighted by readmissions in column (3): an increase in provider network breadth equal to the lower interquantile range would lead to a reduction in mortality of 0.1 per 1,000.

TABLE 5: OLS Regression of Mortality on Provider Network Breadth

	(1) Raw	(2) Quality-adjusted	(3) Weighted quality
Network breadth	-0.0016	-0.0014	-0.0015
	(0.0011)	(0.0012)	(0.0013)
IQ range network breadth	[0.289, 0.512]	[0.234, 0.429]	[0.241, 0.433]
Individuals $\times$ Years	38,458,349	38,458,349	38,458,349

Note: Table reports coefficients and standard errors in parenthesis of an OLS regression of individual mortality on provider network breadth. Column (1) uses provider network breadth. Column (2) uses provider network breadth interacted by the average in-network provider quality. Column (3) uses provider network breadth interacted by the weighted average in-network provider quality, with weights given by the number of admissions per provider-insurer over the full sample period. All specifications include demographic controls (sex and age) and insurer and municipality-by-year fixed effects. Standard errors are clustered at the municipality level. Interquartile range of network breadth reported in brackets.

[20] The specification in column (3) of Tables 5 and 6 does not directly follow the derivations in equation (3) but is presented for completeness.

[21] We obtain the number of providers by taking the difference between the 50th and the 25th percentiles of network breadth and multiplying this difference by the average number of providers across municipalities weighted by the number of enrollees.

Table 6: IV Regression of Mortality on Provider Network Breadth

	(1) Raw	(2) Quality-adjusted	(3) Weighted quality
Network breadth	-0.0093	-0.0099	-0.0092
	(0.0040)	(0.0044)	(0.0045)
F statistic	70.54	65.81	67.75
IQ range network breadth	[0.289, 0.512]	[0.234, 0.429]	[0.241, 0.433]
Individuals × Years	38,458,349	38,458,349	38,458,349

Note: Table reports coefficients and standard errors in parenthesis of an instrumental variables regression of individual mortality on network breadth. Column (1) uses provider network breadth. Column (2) uses provider network breadth interacted by the average in-network provider quality. Column (3) uses provider network breadth interacted by the weighted average in-network provider quality, with weights given by the number of admissions per provider-insurer over the full sample period. The instrument is the measure of network breadth in 2015 interacted with the treatment indicator and the post-termination period indicator. All specifications include demographic controls (sex and age) and insurer and municipality-by-year fixed effects. Standard errors are clustered at the municipality level. Interquartile range of network breadth in reported in brackets.

Table 7: IV Regression of Mortality Excluding Municipalities with SaludCoop Hospitals

	(1) Raw	(2) Quality-adjusted	(3) Weighted quality
Network breadth	-0.0057	-0.0057	-0.0058
	(0.0025)	(0.0027)	(0.0025)
F statistic	47.07	50.36	66.94
IQ range network breadth	[0.351, 0.698]	[0.319, 0.636]	[0.315, 0.625]
Individuals × Years	9,574,747	9,574,747	9,574,747

Note: Table reports coefficients and standard errors in parenthesis of an instrumental variables regression of individual mortality on network breadth. Column (1) uses municipal network breadth. Column (2) uses municipal network breadth interacted by the average in-network provider quality. Column (3) uses municipal network breadth interacted by the weighted average in-network provider quality, with weights given by the number of readmissions per insurer-provider pair over the full sample period. The instrument is the measure of network breadth in 2015 interacted with the treatment indicator and the post-termination period indicator. Sample excludes rural municipalities as defined by the Ministry of Health. All specifications include demographic controls (sex and age) and insurer and municipality-by-year fixed effects. Standard errors are clustered at the municipality level. Interquartile range of network breadth in reported in brackets.

Selection of sicker individuals into broad-network insurers biases the mortality effect towards zero. When we use the instrument in Table 6, we find larger effects consistent with our intuition on the direction of the bias. First-stage results in Appendix Table 8 report coefficients on our instrument, $T_m \cdot P_t \cdot \overline{\xi}_{j(i)m,2015} H_{j(i)m,2015}$, which are positive and less than one, indicating that insurers dropped providers from their networks, and that insurers which had relatively broad networks at baseline continued to have broad networks relative to their rivals after the termination.

In the second stage, we find that a lower interquartile-range increase in provider network breadth reduces mortality by 0.9 per 1,000 enrollees as seen in column (1). Similarly, column (2) shows that increasing quality-adjusted provider network breadth from the first to the second quartile of the distribution reduces mortality by 0.8 per 1,000. The magnitude of this estimate is comparable in column (3).

In Table 7 we reproduce our IV estimates excluding municipalities with SaludCoop hospitals, where there could be congestion effects caused by hospital closures. We find consistent negative treatment effects across the different specifications that are similar in magnitude to those reported in Table 6. For example, column (2) indicates that a lower interquartile-range increase in provider network breadth reduces mortality by 0.8 per 1,000 individuals.

6.5 Threats to the Exclusion Restriction

For our estimates of provider network breadth on mortality to be valid, we require that SaludCoop's termination affected mortality only through network breadth. In this subsection, we dispel concerns that the termination might have impacted mortality through other channels.

SaludCoop hospital closures. One of them is the closure of SaludCoop hospitals which may have generated (i) a mechanical reduction in provider network breadth among incumbent insurers, (ii) a reduction in municipal hospital capacity, and (iii) potential congestion at other providers in the market. We address this concern in several ways. To start, note that our model includes municipality-by-year fixed effects, hence it controls for changes in municipal hospital capacity over time that could be related to the termination. Then, in Table 7 we verify that results are robust to explicitly excluding municipalities where SaludCoop owned hospitals.

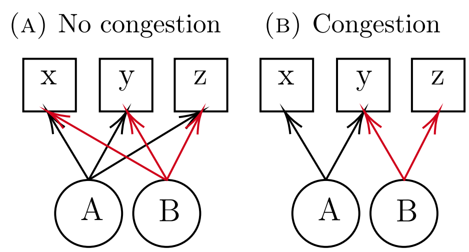
Uncertainty. Another threat to the exclusion restriction is that SaludCoop's termination could have generated uncertainty among enrollees with incumbent insurers who as a response reduced their consumption of health care in the post-period. To provide evidence that this is not a significant identification threat, in Appendix Table 10 we re-estimate our IV regression including the individual's log number of claims as a control. This control variable helps distinguish the effect of provider network breadth on mortality from a potential behavioral response of consumers who either delayed care or decided to forego care altogether after the termination. We also believe to be unlikely that this possible uncertainty effect could have lasted three years after the termination.

Adverse selection. Cross-sectional differences in provider network breadth across insurers in the pre-period and consumers' heterogeneous preferences for provider network breadth, indicate that the market is adversely selected even before the termination (with broad-network insurers enrolling a higher fraction of sick individuals). We address the bias arising from adverse selection in two ways. First, recall that our sample encompasses individuals who never switched their insurer during the sample period. Our instrument identifies the causal effect of provider network breadth on mortality using only the temporal variation in network breadth after the termination. For individuals who never switch, these changes in network breadth over time are exogenous to their enrollment decision (this is similar to arguments in Abaluck et al., 2018; Prager, 2020). This strategy, however, does not exclude individuals who enroll for the first time after 2015 and whose enrollment decision may be characterized by adverse selection. Thus, our second strategy is to exclude these individuals in Appendix Table 9, where we find that our baseline estimates are robust to such exclusion.

Labor market effects. SaludCoop's termination may have also generated impacts on the health care labor market in municipalities where it operated. One worry is that the increase in mortality may be explained by changes in the supply of doctors and nurses in these municipalities rather than by changes in network breadth after the termination. We explore this potential threat to the exclusion restriction by estimating an event study specification at the municipality-year level using as outcome variable the log of the number of doctors. Our data comes from the publicly available National Registry of Health Care Labor Force (RETHUS for its Spanish acronym). In Appendix Figure 7 we find no significant changes in health care labor supply after the termination in the full sample of municipalities nor in the sample without municipalities where SaludCoop owned hospitals.

Congestion. Finally, the influx of "new enrollees" from SaludCoop may have generated congestion at incumbent insurers even if insurers do not change their provider networks, which can directly affect patient mortality. We now demonstrate that congestion is only a problem when insurers have narrow provider networks, validating our instrument.

FIGURE 7: Congestion due to Network Overlap



Note: Figure shows a hypothetical scenario with three hospitals x, y, z, and two insurers A and B, with their network inclusions. Panel A shows a situation where B's termination does not generate congestion effects. Panels B shows a situation where B's termination would lead to a congestion in A's network.

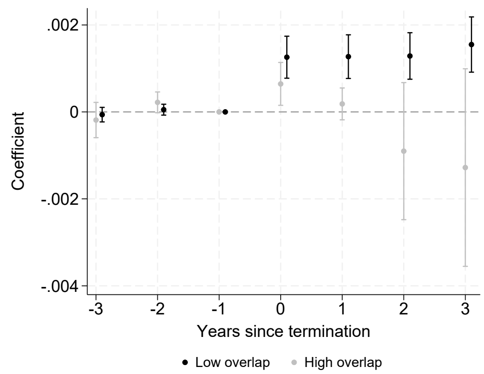
Consider the toy example in Figure 7. There are two insurers $\{A, B\}$ and three providers $\{x, y, z\}$. In Panel A, suppose both insurers have *complete provider networks*. If insurer B is terminated, its enrollees will switch towards A, but in-network providers in A's network will treat the same number patients after the termination as they did before the termination because A has complete network overlap with B. Therefore, we should not expect to see congestion in A's network nor changes in mortality. In Panel B, suppose that insurers have *incomplete provider networks*. Insurer A covers providers $\{x, y\}$ and insurer B covers providers $\{y, z\}$, so that network overlap equals 1/2. If B is terminated and its enrollees switch to A, providers $\{x, y\}$ will treat the patients that were previously treated by $\{z\}$, creating a "congestion effect" at $\{x, y\}$.

The example illustrates that congestion effects exist only when insurers have narrow networks and thus incomplete overlap. Figure 8 corroborates this intuition by exploring the heterogeneity of mortality effects across insurers in treated municipalities with above ("High") or below ("Low") median network overlap with SaludCoop.[22] For instance, we find

[22]We construct network overlap for each insurer and municipality as the fraction of SaludCoop's in-network

that mortality effects are larger when overlap is low. Then, to test the robustness of our results to these potential congestion effects, in Appendix Table 11 we re-estimate our IV specification conditional on incumbent insurers that had complete network overlap with SaludCoop, and hence that do not experience congestion after the termination. Although the estimates are not as precise as of those in our baseline specification in Table 6 due to the reduction in sample size, the coefficients are very similar in size.

FIGURE 8: Mortality Effects by Network Overlap



Note: Figure shows event study coefficients and 95% confidence intervals of individual mortality comparing control municipalities against treated municipalities where the median enrollee's insurer had above ("High") or below ("Low") median network overlap with SaludCoop. Specifications include insurer, municipality, and year fixed effects. Standard errors are clustered at the municipality level. Sample is restricted to individuals who do not switch insurers, had continuous enrollment spells, and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud. Treatment is defined as municipalities where SaludCoop operated in 2015.

6.6 Other Robustness Checks

To further verify the robustness of our results we conduct several exercises. In Appendix Tables 12 to 14 we conduct several placebo or falsification tests of our instrument. 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' network of covered providers, we do not expect our instrument to be correlated with these outcomes. Indeed, we find zero correlation between our instrument and these types of deaths. Moreover, because rural markets have very few providers, network

providers (denominator) that were also in the network of the incumbent insurer during 2015 (numerator).

breadth in such markets can be mechanically high. After excluding rural markets from the sample, Appendix Table 15 reports coefficients that are very similar to our baseline specification which includes both rural and urban markets.

7 Conclusion

Narrow-network insurers have proliferated in health systems with managed care competition, yet the literature that studies the impacts of provider network breadth on patient health is scarce. We fill this gap in knowledge by quantifying the causal effect of provider network breadth on patient mortality using data from the Colombian health care system, where the largest health insurer, which covered 20% of enrollees in the country, was terminated by government in December 2015. The termination provides valuable exogenous variation in insurer and provider choice sets to identify this effect.

Using a difference-in-differences framework, we find that provider networks among incumbent insurers became 10% narrower after the termination and that individual mortality increased among those enrolled with incumbent insurers who never switched. We link these two findings in an instrumental variables approach to show that provider network breadth, defined as the fraction of providers in a municipality that are covered by an insurer, has a negative causal effect on individual mortality. That is, a lower interquartile-range increase in network breadth, which corresponds roughly to adding 9 provider organizations to the network, reduces mortality by 0.8 per 1,000 enrollees who do not switch insurers.

The finding that broad provider networks reduce mortality is relevant for the design of regulations that target narrow networks in health insurance. One such type of regulation is network adequacy rules, which may require insurers to meet minimum provider-to-enrollee ratios, minimum distance from enrollee population centroids to nearest providers, or to cover specific providers.

The implementation of these rules is currently debated in health care systems such as the U.S. (Centers for Medicaid and Medicare Services, 2023; National Conference of State Legislatures, 2023) where the problem of narrow networks is particularly stark. For example, 1 in every 6 Medicare Advantage plans cover less than 30% of hospitals (Jacobson et al., 2016) and physician networks tend to be even narrower than hospital networks (Dafny et al., 2017). Our results suggest that these ultra-narrow networks have detrimental effects on mortality.

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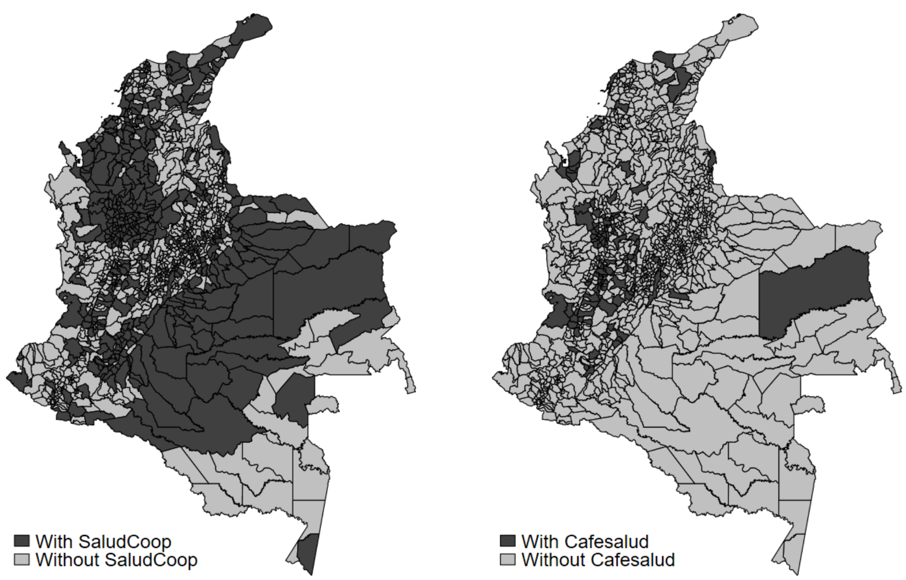
Appendix A Descriptives

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
Balanced panel of insurer-municipalities	22,064,122

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 Timeline of SaludCoop's termination

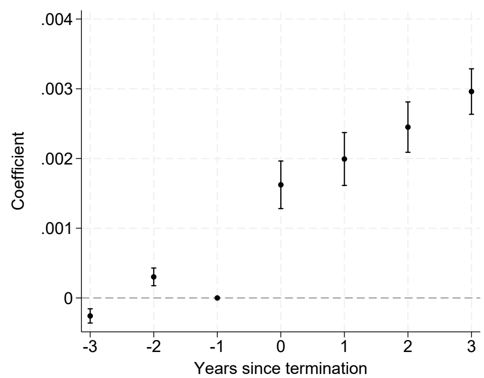
- **2010** — The National Health Superintendency (NHS) ordered SaludCoop to return diverted funds from the health system. These funds, valued at $250 billion, were used for activities unrelated to medical care for affiliates. The diversion of funds occurred between 2004 and 2008.
- **2011** — Diverted funds were not returned and interventions began under Guillermo Grosso from the NHS.
- **05/2011** — NHS intervened SaludCoop's operation: it took possession of all its assets and businesses after detecting administrative and accounting deficiencies that endangered its enrollees and the health system.
- **10/2012** — The Procuraduría General de la Nación sanctions Carlos Palacino, former CEO of SaludCoop. Palacino was banned from holding public office for 18 years and fined, along with 11 other executives of the company.
- **11/2013** — The Contraloría General de la Nación sentences former members of SaludCoop's board of directors: board members were ordered to pay 1.4 trillion pesos to the State.
- **03/2014** — The NHS announced the intervention of SaludCoop's hospitals and clinics due to "repeated failures" found during audit visits. These included missing contracts with other insurers, billing deficiencies, and financial and accounting inconsistencies.
- **11/2015** — Gustavo Palacino was summoned to the prosecutor's office: the former CEO attended meetings to explain the billing irregularities. Up to this date, only Palacino's former advisor had been charged.
- **11/2015** — SaludCoop is terminated: all enrollees were transferred to Cafesalud. The State made a $70 million loan to Cafesalud.
- **12/2015** — SaludCoop's creditors threaten to seize the medical equipment of the María Auxiliadora clinic in southern Huila. Around 45,000 SaludCoop users are affected by the decision of the civil court of Pitalito, which authorized this seizure of assets. In Bogotá, former SaludCoop's employees staged a protest demanding payment of their salaries.

2016	SaludCoop's enrollees were transferred to Cafesalud and had to remain there for 90 days before switching insurers. During this period, Cafesalud had to ensure access to medical care at both SaludCoop's and its own network hospitals.
07/2016	The Prosecutor's Office requested Cafesalud's intervention due to complaints about fundamental rights violations. Many enrollees complained about not having access to appointments with specialists and were referred to other cities and states. In nearly 233 municipalities, Cafesalud had no service network.
08/2016	Cafesalud faced a debt of around 520 billion pesos with the State. High-cost patients reported delays in obtaining medications and complained that in-network clinics and hospitals delayed treatments due to debts inherited from SaludCoop.
09/2016	The Prosecutor's Office summoned Carlos Palacino for questioning. Palacino is investigated for illicit enrichment, diversion of enrollee resources, and unjustified reimbursements made to the government.
10/2016	Thousands of patients, especially from the former SaludCoop now enrolled with Cafesalud, do not receive timely or dignified care. Patients spend days on stretchers waiting for the insurer to authorize medical treatments. The crisis of confidence in the health sector led many healthcare providers to close service lines.
01/2017	The termination of SaludCoop's hospitals and clinics is completed, leading to the dismissal of 1,800 workers. While unions denounce injustices, the interventor affirms that it is a measure of financial sustainability.
02-05/2017	Prestasalud consortium was formed, composed of eight business groups representing 192 hospitals and healthcare centers. Prestasalud purchases Cafesalud. They offered twice the base price to acquire Saludcoop's assets.
08/2017	While the sale was being completed, a new insurer Medimás enters the market. More than 700,000 enrollees switched to other insurers before Medimás started operating.
2018	Medimás was fined nearly 2 billion pesos. The National Health Superintendent resigns due to alleged failures in authorizing Medimás' operations.
2019	The NHS orders Cafesalud's termination.

Appendix C 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 series analysis to compare mortality every year of our data relative to 2015. Our specification includes municipality fixed effects.

Appendix Figure 2: Interrupted Time Series of Mortality for SaludCoop Enrollees



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

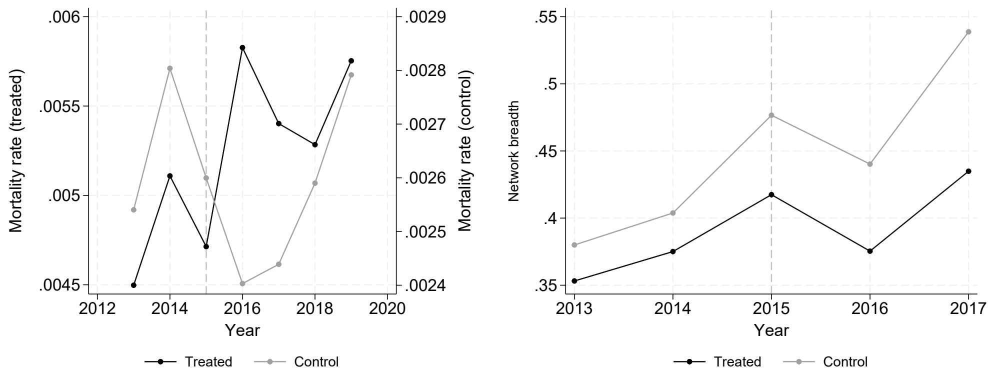
Appendix Figure 2 presents the results. The figure plots the coefficients and 95% confidence intervals associated with each year dummy. We find that there is no systematic trend in individual mortality prior to the termination. In 2016 mortality increases by 1.5 per 1,000 individuals or 26% relative to baseline. This effect grows over time to 3 per 1,000 individuals by the end of our sample period.

Appendix D Additional Results

This Appendix presents additional results of our *did* specification on mortality. Appendix Figure 3 shows time trends of mortality and provider network breadth across treated and control municipalities.

Appendix Figure 3: Trends in Mortality and Provider Network Breadth

(a) Mortality (b) Provider Network Breadth

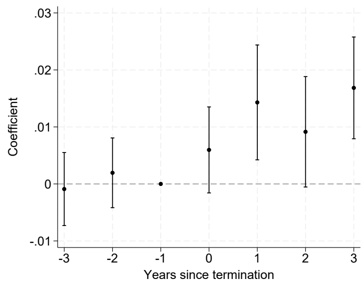


Note: Panel A shows average mortality rates in treated and control municipalities during the sample period. Panel B shows average provider network breadth in treated and control municipalities during the sample period.

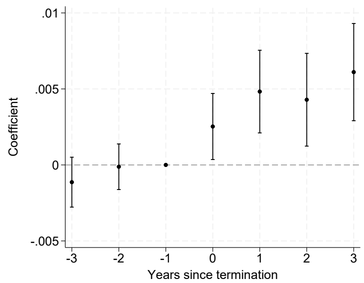
In Appendix Figure 4 we estimate our event study specification conditional on individuals (treated and controls) who received a particular diagnosis at any point during the sample period and who had Charlson index equal to zero in 2013. This latter restriction allows us to compare patients who had the same disease severity at the start of the sample period. We obtain an individual's diagnoses using the ICD-10 codes that accompany their claims. We focus on the following conditions: Acute Myocardial Infarctions (AMI), Chronic Obstructive Pulmonary Disease (COPD), Hepatic diseases, Chronic Kidney Disease (CKD), and Cancer. Moreover, Appendix Figure 5 conditions the main sample to enrollees in the contributory scheme.

Appendix Figure 4: Mortality Effect by Diagnosis

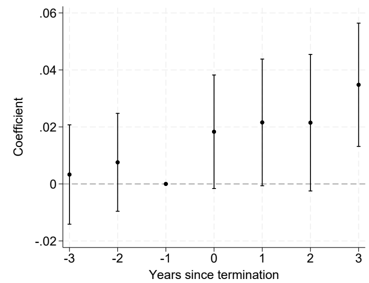
(a) AMI



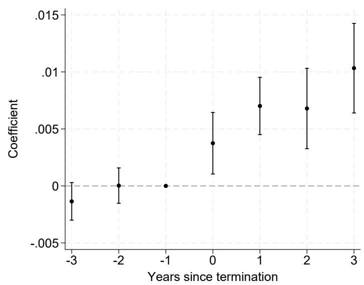
(b) COPD



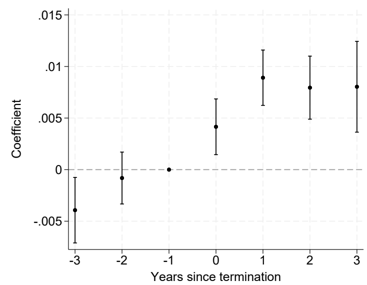
(c) Hepatic disease



(d) CKD

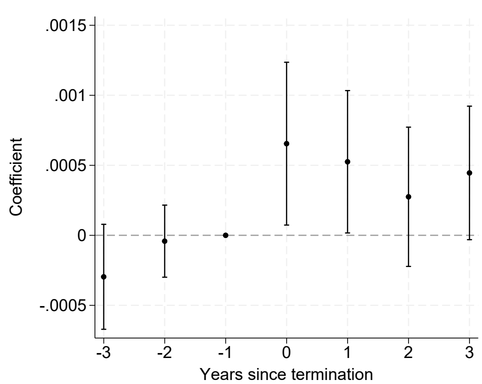


(e) Cancer



Note: Figure shows event study coefficients and 95% confidence intervals of individual mortality conditional on patients who were diagnosed at any point during the sample period with Acute Myocardial Infarction (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. Sample is restricted to individuals who do not switch insurers, who do not move across municipalities before the termination, and who had Charlson index equl to zero in 2013. We exclude individuals enrolled with SaludCoop and Cafesalud. Treatment is defined as municipalities where SaludCoop operated in 2015.

Appendix Figure 5: Mortality Effect in the Contributory System



Note: Figure shows event study coefficients and 95% confidence intervals of enrollee mortality conditioning to insurers operating in the contributory system. Specification includes insurer, municipality, and year 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 operated in 2015.

Appendix E Event Study Coefficients

Appendix Table 2: Provider Network Breadth

	Main		Without SaludCoop hosp	
Relative time	coef	se	coef	se
-3	0.0046	(0.0114)	-0.0027	(0.0119)
-2	0.0045	(0.0102)	-0.0003	(0.0107)
-1	—	—	—	—
0	-0.0185	(0.0120)	-0.0237	(0.0125)
1	-0.0319	(0.0137)	-0.0265	(0.0143)
2				
Observations	20,264		19,419	

Note: Table reports event study coefficients and standard errors in parenthesis of provider network breadth using the full sample sample of municipalities and excluding municipalities with SaludCoop hospitals. We exclude insurers with less than 0.005% market share in a municipality, and specifications include insurer, municipality, and year fixed effects. Standard errors in all specifications are clustered at the municipality level.

Appendix Table 3: Visits per Provider

	Main		Without SaludCoop hosp	
Relative time	coef	se	coef	se
-3	0.69	(2.64)	4.97	(3.46)
-2	5.18	(4.47)	0.31	(2.95)
-1	—	—	—	—
0	8.07	(2.74)	6.88	(1.92)
1	13.05	(3.16)	8.10	(3.06)
2	21.46	(5.52)	14.28	(3.54)
3	41.35	(6.87)	37.77	(6.48)
Observations	7,444,963		2,420,595	

Note: Table reports event study coefficients and standard errors in parenthesis of visits per provider using the full sample sample of municipalities and excluding municipalities with SaludCoop hospitals. We exclude insurers with less than 0.005% market share in a municipality, and specifications include insurer, provider, municipality, and year fixed effects. Standard errors in all specifications are clustered at the municipality level.

Appendix Table 4: Mortality Effect

	Main		Without SaludCoop hosp	
Relative time	coef	se	coef	se
-3	-0.00012	(0.00009)	-0.00003	(0.00008)
-2	0.00011	(0.00007)	0.00001	(0.00006)
-1	—	—	—	—
0	0.00093	(0.00023)	0.00057	(0.00011)
1	0.00087	(0.00021)	0.00057	(0.00010)
2	0.00068	(0.00024)	0.00048	(0.00010)
3	0.00094	(0.00025)	0.00064	(0.00010)
Observations	120,985,368		63,102,939	

Note: Table reports event study coefficients and standard errors in parenthesis of individual mortality using the full sample of municipalities and excluding municipalities with SaludCoop hospitals. Specifications control for risk adjustment group dummies and a dummy for whether the individual is a contributor (vs. a beneficiary or enrollee in the subsidized system). Specifications include insurer, municipality, and year fixed effects. Standard errors are clustered at the municipality level. Sample is restricted to individuals who do not switch insurers, had continuous enrollment spells, and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud.

Appendix Table 5: Mortality Effect by Provider Network Exclusions

	High exclusions		Low exclusions	
Relative time	coef	se	coef	se
-3	-0.00114	(0.00092)	0.00003	(0.00072)
-2	0.00079	(0.00079)	0.00049	(0.00060)
-1	—	—	—	—
0	0.00242	(0.00131)	0.00063	(0.00056)
1	0.00307	(0.00138)	-0.00089	(0.00170)
2	0.00424	(0.00161)	-0.00257	(0.00318)
3	0.00412	(0.00175)	-0.00245	(0.00318)
Observations	1,792,734		22,236,156	

Note: Table reports event study coefficients and standard errors in parenthesis of individual mortality. Specifications control for risk adjustment group dummies and a dummy for whether the individual is a contributor (vs. a beneficiary or an enrollee in the subsidized system). Specifications include insurer, municipality, and year fixed effects. Standard errors are clustered at the municipality level. In all specifications the control group are municipalities where SaludCoop did not operate. In the "high interruption" specification the treated group are municipalities where individuals had an above-average fraction of claims from 2013 to 2015 delivered at providers that were dropped from the network in the post-period. In the "low interruption" specification the treated group are municipalities where individuals a below-average fraction of claims from 2013 to 2015 delivered at providers that were dropped from the network in the post-period. Estimations use the sub-sample of insurers in the contributory system for which we have provider network data and focus on enrollees who made at least one claim every year, did not switch insurers, had continuous enrollment spells, and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop or Cafesalud. Treated units are municipalities where SaludCoop operated.

Appendix Table 6: Mortality Effect by Network Overlap

	High overlap		Low overlap	
Relative time	coef	se	coef	se
-3	-0.00019	(0.00021)	-0.00006	(0.00009)
-2	0.00022	(0.00012)	0.00005	(0.00006)
-1	—	—	—	—
0	0.00064	(0.00025)	0.00126	(0.00025)
1	0.00018	(0.00019)	0.00127	(0.00026)
2	-0.00090	(0.00080)	0.00129	(0.00027)
3	-0.00128	(0.00116)	0.00155	(0.00032)
Observations	70,605,459		70,733,420	

Note: Table reports event study coefficients and standard errors in parenthesis of individual mortality. Specifications include insurer, municipality, and year fixed effects. Standard errors are clustered at the municipality level. Sample is restricted to individuals who do not switch insurers, had continuous enrollment spells, and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud. The "high overlap" specification uses the sub-sample of insurers in treated municipalities with above-median overlap with SaludCoop. The "low overlap" specification uses the sub-sample of insurers in treated municipalities with below-median overlap with SaludCoop.

Appendix F Extension of Hospital Choice Model

In this appendix we extend the relation between the measure of willingness-to-pay for provider networks and quality-adjusted network breadth by allowing for observed individual heterogeneity. Consider a model where individual i's indirect utility from choosing hospital h in the network of insurer j in market m is:

$$u_{ijhm} = x_{\theta(i)}\xi_{hm} + \varepsilon_{ijhm}$$

where $x_{\theta(i)}$ is a vector of observed consumer characteristics describing a consumer type θ, ξ_{hm} captures shared preferences across consumers for hospital h, and ε_{ijhm} is a preference shock that follows a T1EV distribution. Individual i's value for insurer j's network of hospitals G_{jm} is:

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

Let γ_θ be the fraction of consumers type θ in the population, $|G_m|$ the total number of hospitals in the market, and $|G_{jm}|$ the number of hospitals in insurer j's network. We obtain the following relation between the measure of network value derived from a hospital choice model and our measure of network breadth:

$$\begin{aligned}
\sum_\theta \gamma_\theta w_{\theta(i)jm} &= \sum_\theta \gamma_\theta \log\Big(\sum_{h\in G_{jm}} \exp(x_{\theta(i)}\xi_{hm})\Big) \geq \sum_\theta \gamma_\theta \log\Big(\frac{1}{|G_m|}\sum_{h\in G_{jm}} \exp(x_{\theta(i)}\xi_{hm})\Big) \\
&\geq \sum_\theta \gamma_\theta \frac{1}{|G_m|}\sum_{h\in G_{jm}} \log(\exp(x_{\theta(i)}\xi_{hm})) = \sum_\theta \gamma_\theta \frac{1}{|G_m|}\sum_{h\in G_{jm}} x_{\theta(i)}\xi_{hm} \\
&= \sum_\theta \gamma_\theta \frac{|G_{jm}|}{|G_m|}\sum_{h\in G_{jm}} \frac{1}{|G_{jm}|} x_{\theta(i)}\xi_{hm} = \sum_\theta \gamma_\theta x_{\theta(i)}\bar{\xi}_{jm} H_{jm}
\end{aligned}$$

where $\bar{\xi}_{jm} = |G_{jm}|^{-1}\sum_{h\in G_{jm}} \xi_{hm}$ is the average quality of the hospitals in insurer j's network. The relationship between network valuation and quality-weighted network breadth holds when allowing for observed preference heterogeneity. As in the main text, estimating our regression of individual mortality on this measure of network breadth would require only one instrument.

Appendix G Measurement Error Bias

Suppose the true model for how network breadth causally impacts individual mortality is:

$$y_{imt} = \alpha w_{j(i)mt} + \epsilon_{imt}$$

We proxy $w_{j(i)mt}$ with $\bar{\xi}_{jmt} H_{j(i)mt}$ which introduces measurement error. Unlike the classic case of measurement error in an explanatory variable, in our setting the mean of this error is strictly positive. Suppose $\bar{\xi}_{jmt} H_{j(i)mt} = w_{j(i)mt} - \nu_{imt}$. Assume that $E[x_{it}\nu_{imt}] = 0$, $E[w_{j(i)mt}\nu_{imt}] = 0$, and $E[\epsilon_{imt}\nu_{imt}] = 0$. Because of the logarithmic nature of $w_{j(i)mt}$, we know that $E[\nu_{imt}] = l > 0$ and that $E[w_{j(i)mt}] > l$. Moreover, let $var(\nu_{imt}) = \sigma_\nu^2$ and $var(w_{j(i)mt}) = \sigma_w^2$. The feasible equation is given by:

$$y_{imt} = \alpha \bar{\xi}_{jmt} H_{j(i)mt} + (\epsilon_{imt} + \alpha \nu_{imt})$$

The OLS estimator for α in this equation is:

$$\hat{\alpha} = \frac{cov(w_{j(i)mt} - \nu_{imt}, \alpha w_{j(i)mt} + \epsilon_{imt})}{var(w_{j(i)mt} - \nu_{imt})}$$

and

$$plim\ \hat{\alpha} = \frac{\sigma_w^2 + lE[w]}{\sigma_w^2 + \sigma_\nu^2}\alpha$$

Let $\lambda = \frac{\sigma_w^2 + lE[w]}{\sigma_w^2 + \sigma_\nu^2}$. The bias in the OLS estimator is:

$$plim\ \hat{\alpha} - \alpha = -(1-\lambda)\alpha = -\frac{\sigma_\nu^2 - lE[w]}{\sigma_w^2 + \sigma_\nu^2}\alpha < -\frac{\sigma_\nu^2 - l^2}{\sigma_w^2 + \sigma_\nu^2}\alpha$$

This corresponds to attenuation bias of the classic measurement error setting if and only if $E[\nu_{imt}^2] \geq 2E[\nu_{imt}]^2$.

Appendix H First-Stage Regressions and Robustness Checks

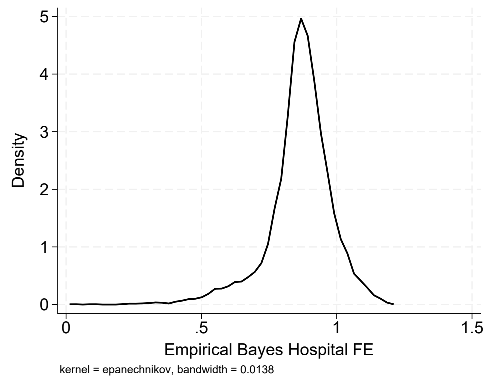
In this Appendix we provide additional results and robustness checks for our *did* and IV strategies. First, we present the distribution of provider fixed effects for our calculation of provider quality in Appendix Figure 6. Then, we present reduced-form estimates and first-stage estimates of our IV specification in Appendix Tables 7 and 8. Finally, we display robustness checks of the IV strategy excluding municipalities with SaludCoop hospitals in Appendix Table 7; excluding individuals who enroll for the first time after 2015 in Appendix Table 9; including the log of the number of claims as a control in Appendix Table 10; using violent deaths, fetal deaths, and deaths by suicide as placebo outcomes in Appendix Tables 12-14; and excluding rural municipalities in Appendix Table 15. Finally, Appendix Figure 7 displays event study results using as outcome the log of the number of doctors and nurses in each municipality as reported in the National Registry of Health Care Labor Force. An observation in this regression is a municipality-year.

Appendix Table 7: Reduced-Form Estimates

	(1) Raw	(2) Quality-adjusted	(3) Weighted quality
Instrument	-0.0032 (0.0013)	-0.0034 (0.0015)	-0.0035 (0.0016)
Individuals × Years	38,458,349	38,458,349	38,458,349

Note: Table presents reduced-form results of individual mortality. The instrument is the interaction between the treatment indicator, the post-termination period indicator and: municipal network breadth in 2015 in column (1), quality-adjusted municipal network breadth in 2015 in column (2), and quality-adjusted municipal network breadth weighted by the number of readmissions in 2015 in column (3). All specifications include demographic controls (sex and age) and insurer and municipality-by-year fixed effects. Standard errors in parenthesis are clustered at the municipality level.

Appendix Figure 6: Distribution of Bayes-Adjusted Provider Fixed Effects



Note: Figure presents the distribution of Bayes-adjusted provider fixed effects. Provider fixed effects are estimated from a regression of an indicator for an admission not resulting in a readmission, controlling for the patient's sex and age. Provider fixed effects are then adjusted towards their municipality mean to account for statistical noise using the Nichols (2008) and Chandra et al. (2016) Stata packages.

Appendix Table 8: First-Stage Regression of Network Municipal Breadth

	(1) Raw	(2) Quality-adjusted	(3) Weighted quality
$T_m \cdot P_t \cdot \bar{\xi}_{j(i)m,2015} H_{j(i)m,2015}$	0.3421	0.3439	0.3773
	(0.0421)	(0.0424)	(0.0458)
F statistic	66.13	65.81	67.75
IQ range network breadth	[0.289, 0.512]	[0.234, 0.429]	[0.241, 0.433]
Individuals × Years	38,461,806	38,458,349	38,458,349

Note: Table presents first-stage regression results. Column (1) uses municipal network breadth, column (2) uses quality-adjusted municipal network breadth, and column (3) uses quality-adjusted municipal network breadth weighted by the number of readmissions. In each column the instrument is the interaction between the treatment indicator, the post-termination period indicator, and the respective measure of network breadth in 2015. All specifications include demographic controls (sex and age) and insurer and municipality-by-year fixed effects. Standard errors in parenthesis are clustered at the municipality level.

Appendix Table 9: IV Regression of Mortality Excluding First Enrollment After 2015

	(1) Raw	(2) Quality-adjusted	(3) Weighted quality
Network breadth	-0.0114	-0.0123	-0.0113
	(0.0046)	(0.0051)	(0.0051)
F statistic	62.49	62.23	63.83
IQ range network breadth	[0.289, 0.512]	[0.234, 0.429]	[0.241, 0.433]
Individuals × Years	36,548,298	36,548,298	36,548,298

Note: Table reports coefficients and standard errors in parenthesis of an instrumental variables regression of individual mortality on network breadth. Column (1) uses municipal network breadth. Column (2) uses municipal network breadth interacted by the average in-network provider quality. Column (3) uses municipal network breadth interacted by the weighted average in-network provider quality, with weights given by the number of readmissions per insurer-provider pair over the full sample period. The instrument is the measure of network breadth in 2015 interacted with the treatment indicator and the post-termination period indicator. Sample excludes individuals who enroll for the first time after 2015. All specifications include demographic controls (sex and age) and insurer and municipality-by-year fixed effects. Standard errors are clustered at the municipality level. Interquartile range of network breadth in reported in brackets.

Appendix Table 10: IV Regression of Mortality Controlling for Claims

	(1) Raw	(2) Quality-adjusted	(3) Weighted quality
Network breadth	-0.0082	-0.0086	-0.0079
	(0.0040)	(0.0043)	(0.0042)
F statistic	66.46	66.15	68.20
IQ range network breadth	[0.289, 0.512]	[0.234, 0.429]	[0.241, 0.433]
Individuals x Years	38,458,349	38,458,349	38,458,349

Note: Table reports coefficients and standard errors in parenthesis of an instrumental variables regression of individual mortality on network breadth. Column (1) uses municipal network breadth. Column (2) uses municipal network breadth interacted by the average in-network provider quality. Column (3) uses municipal network breadth interacted by the weighted average in-network provider quality, with weights given by the number of readmissions per insurer-provider pair over the full sample period. The instrument is the measure of network breadth in 2015 interacted with the treatment indicator and the post-termination period indicator. All specifications control for the individual's log of number of claims, include demographic controls (sex and age), and insurer and municipality-by-year fixed effects. Standard errors are clustered at the municipality level. Interquartile range of network breadth in reported in brackets.

Appendix Table 11: IV Regression of Mortality in Municipalities with Full Incumbent Overlap

	(1) Raw	(2) Quality-adjusted	(3) Weighted quality
Network breadth	-0.0090	-0.0088	-0.008
	(0.0053)	(0.0055)	(0.0048)
F statistic	27.90	33.60	36.29
IQ range network breadth	[0.345, 0.667]	[0.311, 0.623]	[0.311, 0.615]
Individuals x Years	5,108,471	5,108,471	5,108,471

Note: Table reports coefficients and standard errors in parenthesis of an instrumental variables regression of individual mortality on network breadth. Column (1) uses municipal network breadth. Column (2) uses municipal network breadth weighted by the average quality of in-network providers. The instrument is the measure of network breadth in 2015 interacted with the treatment indicator and the post-termination period indicator. All specifications include demographic controls (sex and age) and municipality-by-year fixed effects. Sample includes only municipalities where all incumbent insurers have complete network overlap with SaludCoop. Standard errors are clustered at the municipality level. Interquartile range of network breadth in reported in brackets.

Appendix Table 12: Placebo Test on Violent Deaths

	(1) Raw	(2) Quality-adjusted	(3) Weighted quality
Instrument	-0.0001	-0.0001	-0.0001
	(0.00004)	(0.0001)	(0.00004)
Individuals x Years	38,290,446	38,290,446	38,290,446

Note: Table reports coefficients and standard errors in parenthesis of an OLS reduced-form regressions of an indicator for violent deaths on our instrument. Column (1) uses our instrument for municipal network breadth. Column (2) uses municipal network breadth interacted by the average in-network provider quality. Column (3) uses municipal network breadth interacted by the weighted average in-network provider quality, with weights given by the number of readmissions per insurer-provider pair over the full sample period. All specifications include demographic controls (sex and age) and insurer and municipality-by-year fixed effects. Standard errors are clustered at the municipality level.

Appendix Table 13: Placebo Test on Fetal Deaths per 1,000 Enrollees

	(1) Raw	(2) Quality-adjusted	(3) Weighted quality
Instrument	0.493	0.501	0.464
	(0.290)	(0.293)	(0.276)
Individuals x Years	4,522	4,522	4,522

Note: Table reports coefficients and standard errors in parenthesis of 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 municipal network breadth interacted by the average in-network provider quality. Column (3) uses municipal network breadth interacted by the weighted average in-network provider quality, with weights given by the number of readmissions per insurer-provider pair over the full sample period. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level.

Appendix Table 14: Placebo Test on Deaths by Suicide

	(1) Raw	(2) Quality-adjusted	(3) Weighted quality
Instrument	-0.00001	-0.000011	-0.000010
	(0.000008)	(0.000010)	(0.000010)
Individuals × Years	38,287,474	38,287,474	38,287,474

Note: Table reports coefficients and standard errors in parenthesis of OLS reduced-form regressions of an indicator for deaths by suicide on our instrument. Column (1) uses our instrument for municipal network breadth. Column (2) uses municipal network breadth interacted by the average in-network provider quality. Column (3) uses municipal network breadth interacted by the weighted average in-network provider quality, with weights given by the number of readmissions per insurer-provider pair over the full sample period. All specifications include demographic controls (sex and age) and insurer and municipality-by-year fixed effects. Standard errors are clustered at the municipality level.

Appendix Table 15: IV Regression of Mortality Excluding Rural Areas

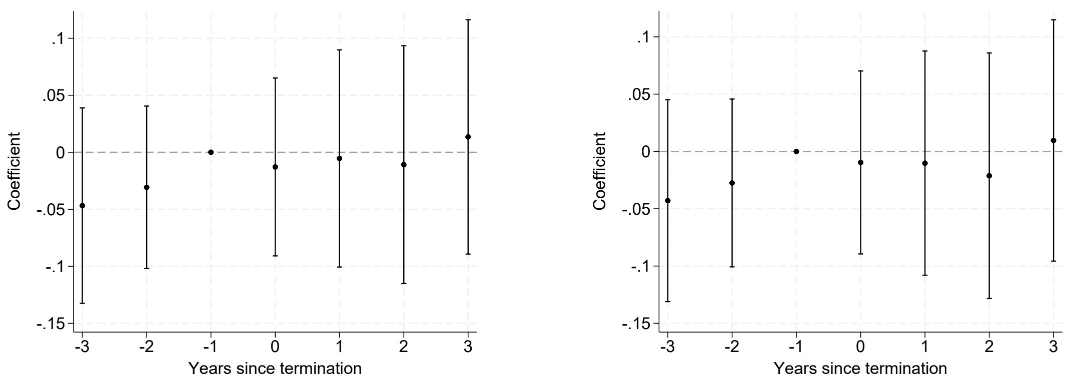
	(1) Raw	(2) Quality-adjusted	(3) Weighted quality
Network breadth	-0.0101	-0.0109	-0.0100
	(0.0040)	(0.0044)	(0.0043)
F statistic	72.96	72.47	78.28
IQ range network breadth	[0.289, 0.500]	[0.234, 0.416]	[0.241, 0.421]
Individuals × Years	37,975,894	37,975,894	37,975,894

Note: Table reports coefficients and standard errors in parenthesis of an instrumental variables regression of individual mortality on network breadth. Column (1) uses municipal network breadth. Column (2) uses municipal network breadth interacted by the average in-network provider quality. Column (3) uses municipal network breadth interacted by the weighted average in-network provider quality, with weights given by the number of readmissions per insurer-provider pair over the full sample period. The instrument is the measure of network breadth in 2015 interacted with the treatment indicator and the post-termination period indicator. Sample excludes rural municipalities as defined by the Ministry of Health. All specifications include demographic controls (sex and age) and insurer and municipality-by-year fixed effects. Standard errors are clustered at the municipality level. Interquartile range of network breadth in reported in brackets.

Appendix Figure 7: Impact of SaludCoop's Termination of Health Care Labor Supply

(a) All markets: Log number of doctors

(b) Without SaludCoop hosp.: Log number of doctors



Note: Figure shows event study coefficients and 95% confidence intervals using as outcome the log of the number of doctors or physicians. Panel A uses the full sample of municipalities. Panel B excludes municipalities where SaludCoop owned hospitals. An observation in these regressions is a municipality-year. Data comes from the National Registry of Health Care Labor Force. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level. Treatment is defined as municipalities where SaludCoop operated in 2015.