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# The role of hospital networks in individual mortality

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# The Role of Hospital Networks in Individual Mortality*

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**Abstract**

Narrow hospital networks have proliferated in health systems with managed care. We investigate the causal effect of network breadth on mortality leveraging the termination of the largest health insurer in Colombia. The termination caused a substantial increase in mortality accompanied by reductions in network breadth among incumbent insurers. We estimate that broad-network insurers reduce mortality because they steer patients to higher-quality providers and reduce hospital congestion. Results imply that patients should be reassigned to incumbent insurers based on the overlap of their network with the terminated insurer, and that policies requiring minimum network coverage are needed to maintain patient health.

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

JEL codes: I10, I11, I13, I18.

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# 1 Introduction

Health insurance companies 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, copayments, 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. This has lead to insurers differentiating across non-financial features, including the providers covered by each health plan (Glied, 2000; Glazer and McGuire, 2000).

As expected, consumers have preferences over the non-financial features of their health plans, such as what benefits are covered, how service utilization is monitored, whether referrals are required to see specialists, etc. One feature which has gained recent interest is network breadth, which encompasses the density and quality of health care providers covered by the health plan (Serna, 2024; Ghili, 2022; Liebman, 2022; Shepard, 2022; Ho and Lee, 2019; Dafny et al., 2017, 2015).

Existing research has focused on the effect of network breadth on insur-

[1] The literature is extensive and include theoretical and empirical contributions such as Pauly (1968); Zeckhauser (1970); Feldstein (1973); Rothschild and Stiglitz (1976); Keeler et al. (1977); Ellis (1986); Besley (1988); Buchanan et al. (1991); Manning and Marquis (1996); Cutler and Zeckhauser (2000); Vera-Hernández (2003); Einav et al. (2010); Aron-Dine et al. (2015); Handel et al. (2015); Kowalski (2015); Handel and Kolstad (2015); Brot-Goldberg et al. (2017); Einav and Finkelstein (2018a,b); Handel et al. (2019).

ance premiums and hospital prices 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 healthcare system. This system has near-universal coverage and provides access to a national health insurance plan through private insurers. Similar to Medicare Advantage, insurers compete only on the breadth of their hospital networks, but all other elements of the national insurance plan are regulated by the government, 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 which were vertically integrated with it. The government terminated SaludCoop because it diverted nearly 1 trillion pesos to investments outside of the health care system and because its board of directors engaged in illegal activities. SaludCoop covered 20 percent of enrollees in the country, who were transferred to an incumbent insurer called Cafesalud. Prior to the termination, Cafesalud had less than 5 percent 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 hospitals to quantify the effect of network breadth on patient health.

We have the universe of individual-level insurer choices and vital statistics from 2012 to 2019; health claims from half of the country's population enrolled in the contributory system for the same sample period; and data on insurers'

[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.

hospital network inclusions between 2013 and 2017. We study effects at insurers *other* than SaludCoop and Cafesalud in a difference-in-differences event study framework, comparing enrollees in municipalities where SaludCoop (and its hospitals) operated versus those where it did not operate, before and after the termination.

Our findings show that individual mortality increased 25 percent after the termination among fully inertial patients, an effect that is persistent over time. Most of the mortality effect comes from individuals with chronic health conditions who see their healthcare treatments interrupted. At the same time, we find evidence consistent with strategic firm behavior since incumbent insurers in treated municipalities dropped around 15 providers per 1,000 enrollees, or equivalently reduced network breadth between 2 and 4 percentage points, to make their network less appealing to unprofitable switchers. In addition to networks becoming narrower, each provider rendered 10 more consultations the year after the termination, a 19 percent increase over baseline.

Can part of the mortality increase be caused by the reduction in network breadth? SaludCoop's termination gives us ideal quasi-experimental variation in insurer and hospital choice sets to identify this causal effect. Using an instrumental variables regression, we estimate that broad hospital networks significantly reduce patient mortality. An interquartile-range increase in network breadth, which corresponds roughly to adding 15 providers to the network in the average municipality, reduces mortality by 2.6 per 1,000 individuals.

The increase in mortality among enrollees who were not directly affected by SaludCoop's termination is explained by narrow networks becoming congested.

We show that congestion effects are only salient when incumbent insurers have incomplete network overlap with SaludCoop. We define network overlap as the fraction of hospitals in SaludCoop's network that are also in the network of the incumbent insurer. We find that in municipalities where the average insurer had below-average network overlap, mortality was 50 percent higher than in municipalities with above-average overlap. Besides reducing the impact of hospital congestion, broad-network insurers tend to refer patients to higher-quality providers and are more likely to cover certain services, such as dialysis and chemotherapy, in the municipality where the enrollee lives compared to narrow-network insurers.

Our findings teach two important lessons for market regulation: first, we show that if the goal is to maintain patient health and guarantee continuity of care, patient reassignment to incumbent insurers after terminations should be made on the basis of network overlap rather than randomly. Second, by showing the importance of network breadth, our findings speak to the use of network adequacy standards or to regulation encouraging competition between insurers to achieve minimum network coverage.

**Contributions and relation to the literature.** This paper contributes to the literature on the impact of health insurance on health outcomes (e.g., Conti and Ginja, 2023; Miller et al., 2021; Goldin et al., 2020; Bauernschuster et al., 2020; Wherry and Miller, 2016; Gruber et al., 2014; Sommers et al., 2014; Baicker et al., 2013; Miller et al., 2013; Sommers et al., 2012; Finkelstein et al., 2012; Card et al., 2009, 2008; Dow and Schmeer, 2003). Related research has focused on estimating the value-added of insurance such as Abaluck et al.

(2021) who show that plan-level mortality has a causal impact on individual mortality. We complement these papers by exploring hospital network breadth as the mechanism by which insurance coverage can impact patient health.

Our paper is also related to the literature analyzing interruptions in healthcare due to involuntary patient switches of insurer or provider (Barnett et al., 2017; Lavarreda et al., 2008). For example, Politzer (2021) shows that plan terminations in Medicaid Managed Care lead to reductions in primary care visits and prescriptions. Sabety (2023) finds that adverse health events increase after Medicare patients lose their longstanding primary care physician. Using more than 20 years of Medicare claims, Chandra et al. (2023) find that hospital closures reduce patient mortality and readmissions because these hospitals were of relatively lower quality than those where patients sought care after the closures. And Bonilla et al. (2024) estimate reductions in mortality after insurers go bankrupt in Colombia. We differentiate from this literature in two ways. First, the closure of SaludCoop was politically motivated while most closures analyzed in the literature are related to under-performing providers or insurers. In that setting, enrollees reverse to the mean, enjoying better-quality providers after the closure. Second, we provide general equilibrium estimates of changes in health and market outcomes by analyzing how incumbent insurers react to the termination of a competitor. We also complement this literature by providing policy recommendations for how to handle insurer terminations and highlighting the importance of hospital networks and network overlap across insurers.

Finally, this paper makes contributions to the literature on insurer competition in hospital networks and its regulation. Several papers study the relation between hospital networks, premiums, and negotiated prices for health services (Ghili, 2022; Liebman, 2022; Ho and Lee, 2019; Gowrisankaran et al., 2015; Ho, 2009; Dafny et al., 2017, 2015). Other papers analyze insurers' incentives to establish narrow networks (Serna, 2024; Shepard, 2022; Ho and Lee, 2017). Yet, to date, no paper has shown the effect of hospital network breadth on patient health. In doing so, we bridge the literature on industrial organization of health care markets and health outcomes research. 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.

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

## 2 Institutional Background

We study the effect of hospital networks on patient mortality in the context of the Colombian statutory health care system. This system is divided into a contributory and a subsidized regime. The first covers the half of the population in the country who are formal workers (and their families) and pay payroll taxes. The second is fully funded by the general budget. As of 2020, nearly 95 percent of the population was covered by the system.[3] Both contributory and subsidized insurance enrollees have access to the same national health insurance plan through private and public insurers. Almost every aspect of this plan is regulated by the government, except for hospital networks: insurers in Colombia can choose which hospitals to cover for each health service included in the national insurance plan and can establish contracts freely with them.[4]

Enrollees pay zero insurance premiums. Instead, at the beginning of every year, insurers receive per-capita transfers from the government that are risk-adjusted for sex, age, and municipality of residence. At the end of every year, insurers are also compensated for their enrollees' health based on a coarse list of diagnoses, known as the High-Cost Account. After all risk-adjusted transfers are made substantial risk selection incentives remain. Serna (2024) shows that insurers respond to these incentives using their hospital networks. Selection incentives and hospital networks are determined in equilibrium as a result of insurer and hospital competition. Shocks to competition, such as insurer terminations, can therefore generate new network arrangements that may impact patient health.

The Colombian government can terminate insurers if they divert resources away from the health care system.[5] In December 2015, the government ter-

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

[4] For a more detailed description of the Colombian health care system see Serna (2024).

[5] 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.

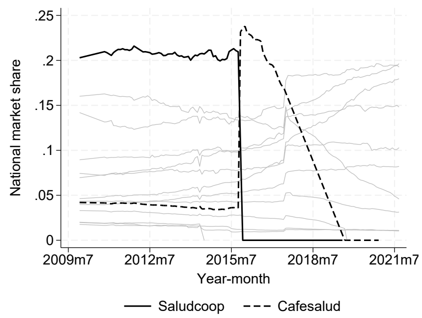
minated the largest health insurer in the country, SaludCoop, due to political considerations and engagement in illegal activities. Its board of directors diverted nearly 1 trillion pesos to investments outside the health system, engaged in financial malpractice, and submitted false health claims to the government for reimbursement. The CEO and board of directors were fined 50 monthly minimum wages, prohibited to work in public office, and prohibited from participating in public auctions for at least 18 years. Appendix B provides a timeline of the termination.[6] SaludCoop's enrollees were transferred to an incumbent insurer called Cafesalud. The government chose Cafesalud as the reassignment insurer because it had presence in almost the same municipalities as SaludCoop did (see appendix figure 1).

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

Figure 1 shows the national market share per insurer in the contributory regime. We emphasize SaludCoop and Cafesalud in black, and the rest of insurers are illustrated in gray. SaludCoop (solid black line) covered an average of 20 percent of enrollees in the years prior to its termination. SaludCoop and

[6] 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.

FIGURE 1: National Market Share




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

Cafesalud participated in both the contributory and the subsidized regimes. Cafesalud had a national market share under 5 percent before the termination, 23 percent in the first three months of 2016, and was itself terminated in 2019. We thus limit our analysis to the years 2012 to 2019.

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

In 2014, SaludCoop owned 38 hospitals and clinics, which accounted for 2,354 hospital beds. SaludCoop operated hospitals in 31 municipalities (out of 1,120 in the country) and in 12 of those there were insurers other than

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

SaludCoop and Cafesalud that covered SaludCoop hospitals. These insurers accounted for approximately 1.5 million enrollees, for whom hospital access changed after the termination. Apart from the 31 municipalities where SaludCoop operated with hospitals, it also operated in 427 municipalities without its own hospitals.[8]

# 3 Data

## 3.1 Data sources and definitions

Our enrollment data comprises all enrollees to the contributory and the subsidized regimes, nearly the entire population in the country. We have a snapshot of enrollment data for every June from 2012 to 2019, which correspond to 4 years before and 4 years after SaludCoop's termination. Because we do not see enrollment every month, we assume that if an individual is enrolled with insurer A in June 2012, they remain with this insurer every month until June 2013 when we see the next enrollment snapshot.[9] The enrollment files have information on the individual's sex, age, municipality of residence, and insurer.

At the end of every year, insurers in the contributory and the subsidized

[8] 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 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 it.

[9] 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 (2024).

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

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

From the Ministry of Health and Social Protection, we obtain individual level mortality and vital statistics from 2012 to 2019. Anonymous individual identifiers are the same across datasets, allowing us to merge mortality with enrollment and claims. The mortality data reports date of death, cause of death or associated diagnosis, manner of death (fetal, violent, or natural), indicator for whether the individual died at the hospital or elsewhere, provider identifier, and insurer identifier.

We construct our mortality outcome as an indicator for whether the individual died in each year from June to June, given that we observe enrollment in that month. The indicator takes the value of zero if the person is alive that year, and takes the value of one if they die that year. After the individual dies, they disappear from our data, hence mortality rates are measured relative to the population who is alive at the beginning of the year. We exclude fetal and maternal deaths from the analysis.

Finally, we have data on insurers' hospital networks from 2012 to 2017 from the National Health Superintendency. This data reports overall hospital network inclusions but does not distinguish networks per health service.

### 3.2 Sample restrictions

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

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

# 4 Reduced-Form Impact on Mortality

We start our analysis by using a difference-in-differences (*did*) event study design to estimate the reduced-form effect of the termination of SaludCoop on mortality. We compare mortality between enrollees living in municipalities where SaludCoop operated during 2015 (treated group) against enrollees living in municipalities where SaludCoop did not operate (control group). The unit of treatment is therefore a municipality.

Our regression of interest is:

$$y_{imt} = \sum_{\substack{k=-3 \\ k \neq -1}}^{3} \beta_k 1\{t - 2016 = k\} \times T_m + x'_{it}\lambda + \gamma_m + \gamma_t + \varepsilon_{imt}, \qquad (1)$$

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 whether SaludCoop operated in municipality $m$ in 2015, $x_{it}$ is a vector of (potentially time-varying) patient characteristics including sex, age, insurer dummies, and a dummy for being a contributor (versus a beneficiary).[10] Finally, $\gamma_m$ and $\gamma_t$ are municipality

[10] We do not include time-varying measures of patient health such as the Charlson index because the likelihood of receiving a diagnosis also changed after the termination.

and year fixed effects, respectively.

The termination of SaludCoop took place in December 2015, and it is visible in our June 2016 enrollment data snapshot. The relative time indicators in equation (1) are thus constructed relative to 2016, and the omitted category is 2015. The coefficients $\beta_k$ measure the average treatment effect on the treated in year $k$ relative to 2015. Because the termination happens at the same time for all individuals in our treated group, we do not worry about staggered implementation. We cluster our standard errors at the municipality level.

Identification of the dynamic treatment effect on the treated relies on treated and control groups being on similar mortality trends prior to the termination. Identification is threatened if SaludCoop selected which municipalities to operate in based on their mortality trends. Selection bias of this style would result in a violation of the classic parallel pre-trend assumption in *did* designs, which we can easily corroborate with our estimates.

## 4.1 Individual Mortality

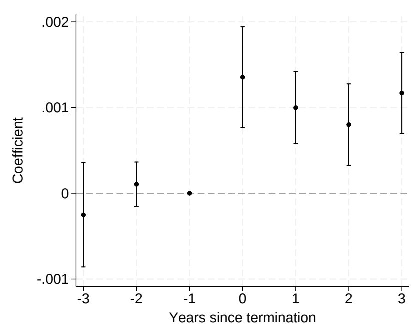
Figure 2 presents coefficients and 95 percent confidence intervals of our event study specification in equation (1), and appendix table 2 reports the associated coefficients and standard errors.[11] Prior to the termination, individuals in municipalities where SaludCoop operated and those where it didn't have parallel mortality trends as evidenced both by statistically zero estimates in 2013 and 2014 and by descriptive trends presented in appendix figure 3.

The year of the termination, mortality increases 1.3 per 1,000 enrollees in

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

treated municipalities, roughly a 25 percent increase over baseline. Appendix D confirms that the increase in mortality the year of the termination is driven by diseases which are more sensitive to sudden interruptions or disruptions of care (such as cancer, renal disease, and hepatic diseases). The magnitude of our estimate is in line with other studies on the effect of insurance coverage on mortality. For example, Miller et al. (2021) find that individuals in states that expand Medicaid experience a reduction of 11.9 percent in annual mortality three years after the expansion. Abaluck et al. (2021) estimate a 19 percent reduction in mortality from enrolling with a one-standard deviation higher-quality insurance plan in Medicare. And Card et al. (2009) find that Medicare eligibility reduces 7-day in-hospital mortality by 20 percent.

FIGURE 2: Mortality Effect




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

Although some of the increase in mortality is probably due to transitory disruptions in health care generated by the termination of SaludCoop, we find that the effects on mortality are persistent over time: three years after the termination, we estimate a mortality increase in treated municipalities equal to 0.8 per 1,000 enrollees, nearly 18 percent relative to baseline.[12] One possible explanation for this permanent effect on mortality is the decrease in hospital capacity that followed from the closure of the 38 hospitals owned by SaludCoop. However, appendix figure 5 shows that mortality increased permanently even in municipalities in which SaludCoop did not own hospitals. This suggests that the termination of SaludCoop triggered a reaction by insurers and/or health care providers that led to permanent effects. We investigate this reaction in the next section.

# 5 Impact on Hospital Networks

Why would mortality increase among enrollees who were not directly affected by SaludCoop's termination? And why would this increase be permanent? A natural explanation is that SaludCoop's termination led to a decrease in network breadth, which is a salient feature in the Colombian context (as premiums are zero and cost-sharing is the same across insurers) and has been linked to worse health outcomes although the evidence is scant (Schleicher et al., 2016).

We measure of network breadth as the fraction of providers in a municipality that are covered by an insurer. Providers can be either hospitals, small clinics, or physician practices.[13] Panel A of figure 3 shows that average network breadth fell 3 percentage points in 2016 relative to 2015, roughly a reduction

[12] These results are robust to excluding the largest cities, Bogotá and Medellín, as seen in appendix figure 11.

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

of one provider in the network of the average municipality. This reduction is not a mechanical effect of SaludCoop's hospitals closing, since patterns in network breadth are similar when we exclude municipalities where these hospitals operated in panel B. Panel C also shows that the average number of hospital beds per 1,000 residents in a municipality decreased 10 percent from 2015 to 2016.

Figure 3: Trends in Network Breadth and Hospital Beds

(a) Municipal Networks

(b) Municipal Networks w/o SaludCoop hospitals

(c) Beds


*Note*: Panel A shows average municipal network breadth across insurers and markets. Panel B shows average municipal network breadth excluding municipalities where SaludCoop operated with its own hospitals. Panel C shows the average number of hospital beds per 1,000 residents across municipalities. Panels A and B use data only from insurers in the contributory system and exclude SaludCoop and Cafesalud.

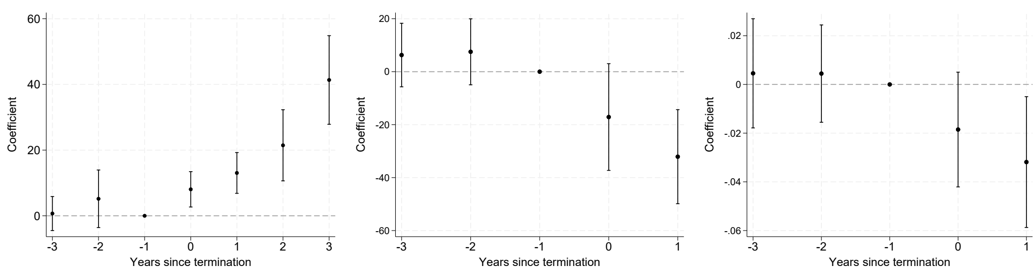
The reduction in network breadth was a causal result of incumbent insurers responding to the termination either indirectly by holding networks fixed but having an influx of new enrollees or directly by dropping providers from their networks. Panel A of figure 4 shows that providers in treated municipalities had approximately 10 more visits or consultations the year of the termination relative to providers in control municipalities, a 19 percent increase over baseline. This effect increases over time, as providers in treated municipalities saw nearly 40 more visits three years after the termination. In addition to each provider rendering more visits, insurers in treated municipalities dropped providers from their networks. Panel B shows a reduction in coverage of around 15 providers per 1,000 enrollees the year of the termination relative to controls, which represents a 19 percent decrease relative to baseline. Panel C corroborates this finding by showing that municipal network breadth decreased between 2 and 4 percentage points after the termination. These results are robust to excluding municipalities with SaludCoop hospitals as seen in appendix figure 7. We also see evidence of parallel pre-trends in network coverage in line with descriptive patterns presented in appendix figure 4.

FIGURE 4: Impact on Networks

(A) Visits per provider

(B) Providers per 1,000 enrollees

(C) Municipal network breadth




*Note*: Panel A shows event study coefficients and 95 percent confidence intervals of number of visits per provider. Specification uses data at the provider-insurer-year level and includes municipality, insurer, provider, and year fixed effects. Standard errors are clustered at the municipality level. Panels B and C show event study coefficients and 95 confidence intervals of providers per 1,000 enrollees and municipal network breadth, respectively. Specifications use data at the insurer-market-year level and condition on insurers that have more than 0.05% market share in the municipality. We include municipality and year fixed effects in these specifications. Because we have hospital network data from 2013 to 2017, we exclude years +2 and +3 relative to the termination from panels B and C. In each specification, treatment is defined as municipalities where SaludCoop operated during 2015.

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

For the specific shock of SaludCoop's termination, table 1 provides evidence that a substantial share of individuals enrolled with SaludCoop in 2015 switched out of Cafesalud after the three mandatory months that they needed to stay in Cafesalud. In particular, 76 percent of individuals who were enrolled with SaludCoop during 2015 remained in Cafesalud for 2016, but 24 percent switched to other insurers in that year after the 90-day period. An additional 23 percent of SaludCoop's enrollees moved to other insurers during 2017, a potentially large influx of new enrollees to these incumbent insurers. Of those enrolled with Cafesalud during 2015, 82 percent were inertial in 2016, but 41 percent switched out by 2018 potentially as a preemptive response to Cafesalud's termination.

TABLE 1: Switching rate

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

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

To close the adverse selection argument, we investigate whether those who switched out of Cafesalud were in worse health status than those who did not switch. Table 2 shows that, amongst those enrolled with SaludCoop in 2015 and transferred to Cafesalud in 2016, sick individuals were much less likely to switch out of incumbent insurers with broad networks compared to healthy individuals. The fact that the estimate for healthy individuals is negative although marginally significant, indicates that consumers value having broad networks overall, but this preference is stronger for those with chronic diseases. As a result of a greater pool of sick "new enrollees," incumbent insurers may have responded by narrowing their networks. Moreover, this could happen soon after the termination because insurers and hospitals in Colombia negotiate service prices and network inclusions typically at the beginning of every calendar year, hence we can expect changes in networks to happen as soon as of the beginning of 2016. Appendix F explores the causal effect of SaludCoop's termination on healthcare utilization and spending, finding evidence consistent with worsened health and with providers gaining bargaining power relative to insurers after the termination.

Table 2: Evidence of Adverse Selection on Network Breadth

| | Switch out | |
|---|---|---|
| | (1) Healthy | (2) Sick |
| 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 are clustered at the individual level.

# 6 The Causal Effect of Hospital Network Breadth

Having shown substantial reduced-form effects of SaludCoop's termination on mortality as well as on network breadth, we move now to estimating the causal effect of network breadth on mortality. Identifying this effect is a difficult exercise because differences in mortality can be explained by individuals non-randomly selecting their insurer on the basis of the hospitals they cover in their networks. For example, if sick patients have strong preferences for a high-quality hospital and this hospital is more likely to be covered under a broad-network insurer, failure to account for hospital choice would yield a biased estimate of network breadth on mortality. Also, if unobservably healthy patients disproportionately enroll with narrow network insurers, then we would predict that narrow-network plans reduce patient mortality when in fact these plans had a healthier population of enrollees to begin with.

Our measure of network breadth can be micro-founded with a discrete choice model where consumers care about specific hospitals being included in the network as in Serna (2024). This micro-foundation shows that selection biases of the style described in the previous paragraph apply to network breadth as well. Consider a simple model of hospital choice where individual $i$'s indirect utility from choosing hospital $h$ in the network of insurer $j$ in market $m$ is:

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

$\xi_{hm}$ captures hospital $h$'s quality and $\varepsilon_{ijhm}$ 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 hospitals $G_{jm}$ is:

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

If this model were feasible to estimate, to identify the causal effect of consumers' preferences for hospital networks, we would regress individual mortality on $w_{ijm}$. However, in practice this hospital demand model may be infeasible due to dimensionality problems or due 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 network breadth as follows. Let $|G_m|$ be the total number of hospitals in the market and $|G_{jm}|$ the number of hospitals in insurer $j$'s network, then:

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

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

where the second inequality uses Jensen's inequality and $\bar{\xi}_{jm} = |G_{jm}|^{-1} \sum_{h \in G_{jm}} \xi_{hm}$ is the average quality of the hospitals in insurer $j$'s network.[14]

The relation between valuation for the network and network breadth sug-

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

gests that the regression that is feasible to estimate is:

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

where $y_{imt}$ is observed mortality, $x_{it}$ are exogenous potentially time-varying characteristics (such as age and sex), $\delta_{j(i)}$ is an insurer fixed effect, and $\gamma_{mt}$ is a municipality-by-year fixed effect. Estimating equation (2) via OLS would yield $\hat{\alpha}$ that is biased towards zero due to measurement error in the explanatory variable (because $\overline{\xi}_{jmt}$ is estimated and $\overline{\xi}_{jmt} H_{j(i)mt}$ is a downward measure of $w_{ijm}$) and bias arising from insurer choice.[15]

Note that we can write equation (2) more generally as

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

where $D_{ijmt}$ is an indicator variable for individual $i$ choosing insurer $j$ in market $m$ and year $t$. This formulation makes explicit the second endogeneity problem since $cov(D_{ijmt}, \epsilon_{imt}) \neq 0$ due to individual health status being unobserved. Estimation of (3) is likely infeasible and under-powered because it would require one instrument for every insurer and hospital. Instead, equation (2) identifies the *average effect* of network breadth on the outcome of interest requiring only one instrument. This is similar to the formulation in Abaluck et al. (2021) who use one forecast coefficient to estimate the average causal effect on mortality from enrolling with a particular health plan.

To construct our main independent variable and later on our instrument,

[15] Appendix I derives an expression for the bias.

we first calculate hospital quality, $\xi_{hm}$, using hospital readmissions data for the entire sample period. We estimate the following regression:

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

where $b_{it}$ is an indicator for individual $i$'s hospital admission $t$ not resulting in a readmission within 30 days, and $x_i$ is a vector of characteristics including sex, and dummies for age group (0-24, 25-44, 45-64, 65+), insurer, and year. To account for statistical noise, we apply an empirical Bayes shrinkage procedure to our estimated hospital fixed effects $\hat{\xi}_h$, following Morris (1983). We shrink our estimated hospital fixed effects toward their municipality-level mean.[16] These fixed effects are invariant over time and insurers. However, to the extent that different insurers cover different hospitals and change their network inclusions over time, the average quality of in-network hospitals $\overline{\xi}_{jmt}$ will vary across insurers, markets, and years in our final specification. Appendix figure 12 presents the distribution of the Bayes-adjusted hospital fixed effects.

To overcome the two biases arising from measurement error and non-random selection into insurers and hospitals, we leverage exogenous changes in network breadth generated by SaludCoop's termination. Our instrument is the interaction between the treatment indicator $T_m$, a post-termination period indicator $P_t$, and network breadth in 2015, $\overline{\xi}_{j(i)m,2015}H_{j(i)m,2015}$, while conditioning on municipality-year interactions. To understand the intuition of our instrument, recall that SaludCoop's termination generated an influx of pa-

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

tients into incumbent insurers. These patients were the ones who switched out of Cafesalud after the 90-day grace period. This pool of new enrollees was in worse health status than current enrollees, which led incumbent insurers to narrow their networks. Because this exogenous change in network breadth is measured relative to 2015, we further interact with baseline network breadth in this year.

Our instrument is relevant for several reasons discussed in section 2. SaludCoop operated in 458 out of the 1,120 municipalities in the country during 2014. Municipalities with presence of SaludCoop accounted for 96 percent of all enrollees in the Colombian health insurance system. In terms of hospital choice sets, our data shows that in markets with SaludCoop hospitals, at least three other insurers covered these hospitals as well. SaludCoop hospitals accounted on average for 34 percent of all hospital admissions at insurers that included these hospitals in their networks.

For our estimates of network breadth on mortality to be valid, we require that the termination of SaludCoop affected mortality only through network breadth. To start, note that our model includes for municipality-year fixed effects, hence it controls for changes in municipal hospital capacity which could be related to the termination. Moreover, the benefits package and cost-sharing rules are regulated, and are the same across municipalities, leaving network breadth as the only variable that the insurer can respond with.

Formally, the first-stage regression is:

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

Then we estimate equation (2) using 2SLS, clustering our standard errors at the municipality level. The estimation sample consists of individuals covered by the contributory system from 2013 to 2017, which corresponds to the period for which we have hospital network data, and who were not enrolled with either SaludCoop or Cafesalud.

Table 3 presents OLS results and table 4 presents 2SLS results using our instrument. In each table, column (1) uses municipal network breadth and column (2) uses municipal network breadth weighted by average hospital quality. Both 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 hospital networks significantly reduce patient mortality. In column (1) of table 3 we find that increasing network breadth from the first to the third quartile of the distribution, which corresponds roughly to adding 15 providers to the network in the average municipality, reduces mortality by 0.52 per 1,000.[17] The reduction in mortality of 0.52 is sizeable given than the national mortality rate in 2015 was 4 per 1,000 excluding violent deaths. The results are very

[17] We obtain the number of providers by taking the difference between the 75th and the 25th percentiles of network breadth and multiplying this difference by the average number of providers in a municipality.

similar when we consider quality-adjusted network breadth in column (2): a reduction in network breadth equal to the interquantile range would lead to a reduction in mortality of 0.43 per 1,000.

Table 3: OLS Regression of Mortality on Municipal Network Breadth

| | (1) Raw | (2) Quality-adjusted |
|---|---|---|
| Network breadth | -0.0023 | -0.0022 |
| | (0.0012) | (0.0013) |
| IQ range network breadth | [0.289, 0.516] | [0.234, 0.429] |
| Individuals x Years | 38,580,349 | 38,580,349 |

*Note*: Table reports coefficients and standard errors in parenthesis of an OLS regression of individual mortality on municipal network breadth. Column (1) uses municipal network breadth. Column (2) uses municipal network breadth weighted by the average in-network provider quality. 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.

Table 4: IV Regression of Mortality on Municipal Network Breadth

| | (1) Raw | (2) Quality-adjusted |
|---|---|---|
| Network breadth | -0.0114 | -0.0123 |
| | (0.0043) | (0.0047) |
| F statistic | 70.54 | 69.83 |
| IQ range network breadth | [0.289, 0.516] | [0.234, 0.429] |
| Individuals x Years | 38,580,349 | 38,580,349 |

*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 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 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 instrument for insurer and hospital choice in table 4, we find larger effects consistent with our intuition on the direction of the bias. First-stage results in appendix table 6 show that insurers dropped providers from their networks and that insurers that had relatively broad networks at baseline continued to have broad networks relative to their rivals after the termination. These effects can be seen respectively in our first-stage estimate being less than 1 and positive. In the second stage, we find that an interquartile-range increase in municipal network breadth reduces mortality by 2.6 per 1,000 as seen in column (1). This effect is more than three times larger than the corresponding estimate in table 3. Similarly, column (2) shows that increasing quality-weighted network breadth from the first to the third quartile of the distribution reduces mortality by 2.4 per 1,000.

**Robustness checks.** To verify the robustness of our results and the validity of our instrument we conduct several exercises. In appendix tables 7 to 9 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' hospital networks, 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. Appendix table 10 presents reduced-form estimates of our main specification.

In appendix table 11 we report OLS and IV results using an admission-weighted average of $\hat{\xi}_{hm}$ to construct our measure of quality-adjusted network breadth. 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. Results in the appendix are qualitatively and quantitatively similar to the ones reported here. In appendix table 12 we include the individual's log number of claims as a control in all our specifications, to distinguish the effect of network breadth on mortality from a potential behavioral response of consumers who either delayed care or decided to forego care altogether after the termination. Appendix table 13 explores the robustness of our results to excluding rural markets with very few providers. Finally, appendix table 14 uses a mortality-weighted rather than readmission-weighted measure of network breadth. First, we estimate a linear regression of individual mortality on hospital fixed effects, controlling for patient characteristics on the sample of deaths prior to the termination. Then, we apply an empirical Bayes shrinkage to these estimates to account for statistical noise. Finally, we interact network breadth with the negative of the average fixed effect among in-network providers.

# 7 Mechanisms and Policy Implications

The previous section showed that hospital network breadth has a negative causal effect on patient mortality, that is, individuals enrolled with broad-network insurers have lower mortality rates. Although these results suggest that continued access to hospitals is important, they still beg the question of what are the mechanisms by which network breadth affects health outcomes.

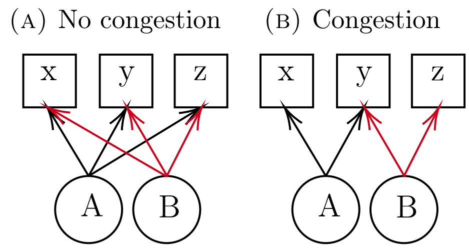
## 7.1 Network Overlap

One possible explanation for why network breadth matters in the context of insurer terminations is that increasing network breadth also increases network overlap across insurers, which in turn makes the network more resilient to shocks. In our particular setting, SaludCoop's termination is a shock that leads to an influx of new enrollees at incumbent insurers. Enrollees of narrow-network insurers are more likely to be affected by the termination as their insurer is less able to allocate patients across several providers leading to congestion.

To see this more clearly, consider the example of figure 5 where there are two insurers $\{A, B\}$ and three hospitals $\{x, y, z\}$. As in panel A, suppose both insurers have complete hospital networks. If insurer $B$ is terminated, its enrollees will switch towards $A$, but in-network hospitals 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. Moving to panel B, suppose that insurer $A$ covers hospitals $\{x, y\}$ and insurer $B$ covers hospitals $\{y, z\}$, so that network overlap equals 1/2. If $B$ is terminated and its enrollees switch to $A$, hospitals $\{x, y\}$ will treat the patients that were previously treated by $\{z\}$. This "congestion effect" at $\{x, y\}$ potentially reduces access to health care for current enrollees and worsens their health outcomes.

The example illustrates that congestion effects exist when insurers have incomplete network overlap, and that low overlap is more likely to happen under narrow networks. To test this congestion mechanism, in figure 6 we explore the heterogeneity of treatment effects across municipalities where the average insurer had above- or below-average overlap with SaludCoop. We

Figure 5: Congestion due to Network Overlap

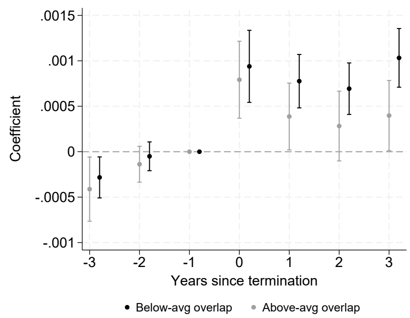



*Note*: Figure shows a hypothetical scenario with three hospitals x, y, z, and two insurers A and B, with their network inclusions. Panel A shows a situation where B's termination does not generate congestion effects. Panels B shows a situation where B's termination would lead to a congestion in A's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

construct network overlap for each insurer and municipality as the fraction of SaludCoop's in-network providers that were also in the network of the incumbent insurer during 2015. We exclude municipalities where SaludCoop hospitals operated to avoid confounding biases coming from reductions in hospital capacity at treated municipalities. The figure shows that the increase in mortality in markets where insurers had below-average network overlap was 50 percent higher than in markets where insurers had above-average overlap. In fact, estimates in gray show increases in mortality that are only significant in the first year after the termination.

**How should we handle insurer terminations?** Although a common policy across countries is to reassign patients randomly to incumbent insurers after a termination with the goal of minimizing adverse selection, our results suggest a different reassignment policy. If the objective is to maintain patient health and guarantee continuity of care, event study results in figure 6 indicate that patient reassignment to incumbent insurers after a termination should be made on the basis of network overlap. More generally, given that network overlap is increasing in network breadth, our results also suggest that policies

FIGURE 6: Congestion by Network Overlap




*Note*: Figure shows event study coefficients and 95 percent confidence intervals of individual mortality conditional on treated municipalities where the average insurer had below-average network overlap with SaludCoop in 2015 in black, and where the average insurer had above-average network overlap with SaludCoop in 2015 in gray. Sample excludes municipalities where SaludCoop hospitals operated. Treatment is defined as municipalities where SaludCoop was present in 2015. Specifications include municipality, insurer, and year fixed effects. Standard errors are clustered at the municipality level.

requiring minimum network coverage are needed to make incumbent insurers more resilient to shocks to competition.

## 7.2 Suitability of Broad Networks

Another explanation for why network breadth matters is the suitability of broad hospital networks for treating patients of different health conditions. To test this mechanism, in panel A of table 5 we regress different characteristics of the network, such as which types of services they cover, on municipal network breadth. We determine this set of services using the claims data and require that a service is considered in-network for an insurer if there are more than 10 claims for that service from its enrollees. An observation in these regressions is an insurer-municipality-year.

Results show that broad-network insurers are more suitable for patient health along several dimensions. A one percentage point (p.p.) increase in

Table 5: Network Breadth Mechanisms

| Mechanism | coef | se |
|---|---|---|
| Panel A. Overall | | |
| Dialysis | 0.029 | (0.005) |
| Cardiology | 0.006 | (0.002) |
| Chemotherapy | 0.025 | (0.005) |
| Beds | 35.92 | (8.594) |
| Panel B. Referral to high-quality provider | | |
| All sick | 0.077 | (0.001) |
| Low severity | 0.074 | (0.001) |
| Medium severity | 0.091 | (0.003) |
| High severity | 0.130 | (0.007) |

*Note*: Table presents OLS regressions of the outcome in the row on municipal network breadth. The data in panel A is at the insurer-municipality-year level and in panel B is at the individual level. Panel A specifications include municipality and year fixed effects and standard errors in parenthesis are clustered at the municipality level. Panel B specifications include individual and year fixed effects and standard errors in parenthesis are clustered at the individual level. High-quality providers are those with zero 48-hour mortality rate after admission. Sick individuals are identified based on a Charlson index greater than zero. Low, medium, and high severity patients are those with Charlson index between 1-3, 4-6, 6+, respectively.

municipal network breadth is associated with a 2.9, 0.6, and 2.5 p.p increase in the likelihood of covering dialysis, cardiology, and chemotherapy providers, respectively. Put differently, if a patient with renal disease is enrolled with a narrow-network insurer and needs dialysis, this patient will need to travel to other municipalities where the insurer covers the service. Broad-network insurers also tend to cover larger hospitals as measured by the number of beds, which means that they are better able to deal with hospital congestion.

In panel B of table 5 we explore insurers' steering mechanisms. We regress an indicator for whether the patient is treated at a high-quality provider on municipal network breadth, and condition on individuals with chronic diseases to control for adverse selection. We define a high-quality provider as one that had zero 48-hour mortality rate after admission during 2014. This measure is obtained from the National Health Superintendency's provider quality indicators. Findings show that broad-network insurers are more likely to steer patients to better providers and that this likelihood is increasing in the severity of the patient as measured by their Charlson index.

# 8 Conclusion

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

Using a difference-in-differences event study framework we find that individual mortality increased nearly 25 percent and that hospital networks became much narrower after the termination. We link these two findings in an instrumental variables regression to show that network breadth, defined as the fraction of providers in a market that are covered by an insurer, has a negative causal effect on individual mortality. That is, an interquartile-range increase in network breadth, which corresponds roughly to adding 15 providers to the network, reduces mortality by 2.6 per 1,000 enrollees.

We hypothesize two mechanisms that explain this negative causal effect. First, we show that if incumbent insurers have broad networks, they likely have high network overlap with the terminated insurer. High overlap in turn reduces the impact of hospital congestion after the termination. Indeed, we find that markets where incumbent insurers have low overlap see mortality increases that are 50 percent larger than markets where incumbents have high overlap. Second, we show that broad networks are better suited to treat patients of different health conditions. We find that broad networks are more likely to cover certain health services in the same municipality where the enrollee lives, and are more likely to steer patients toward higher-quality providers.

The findings of our paper have implications for how to reassign patients after insurer terminations and for how to design network adequacy standards to achieve broad network coverage across insurers. While market regulators across different countries use random reassignment after insurer terminations to reduce the impact of adverse selection, our results suggest that if the goal is to reduce disruptions in care and maintain patient health, reassignments should be made on the basis of network overlap.

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# For Online Publication

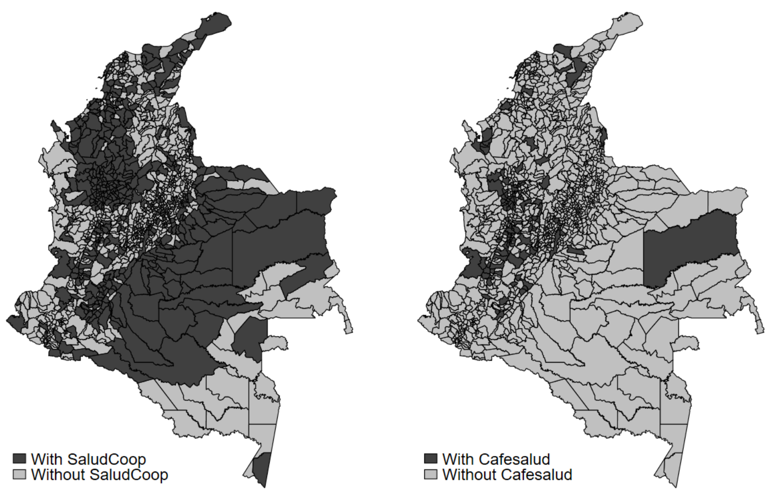
# Appendix A Descriptives

Appendix Table 1: Sample restrictions

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

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

Appendix Figure 1: Municipal Presence of SaludCoop and Cafesalud




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

# Appendix B Timeline of SaludCoop's termination

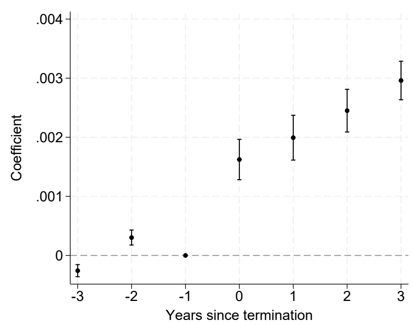
- **2010** — The National Health Superintendency (NHS) ordered SaludCoop to return diverted funds from the health system. These funds, valued at $250 billion, were used for activities unrelated to medical care for affiliates. The diversion of funds occurred between 2004 and 2008.
- **2011** — Diverted 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 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



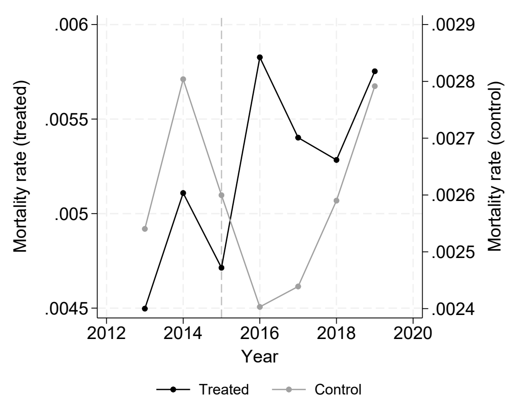

*Note*: Figure presents interrupted time series coefficients and 95 percent confidence intervals of individual mortality conditional on consumers who were enrolled with SaludCoop prior to its termination. Specification includes municipality fixed effects.

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

## Appendix D Additional Results

This appendix presents additional results of our *did* specification on mortality. Appendix figures 3 and 4 show descriptive evidence of parallel pre-trends in mortality and networks across treated and control municipalities. Appendix figure 5 replicates our main specification excluding markets where SaludCoop operated with its own hospitals.

Appendix Figure 3: Descriptive Evidence of Parallel Mortality Trends

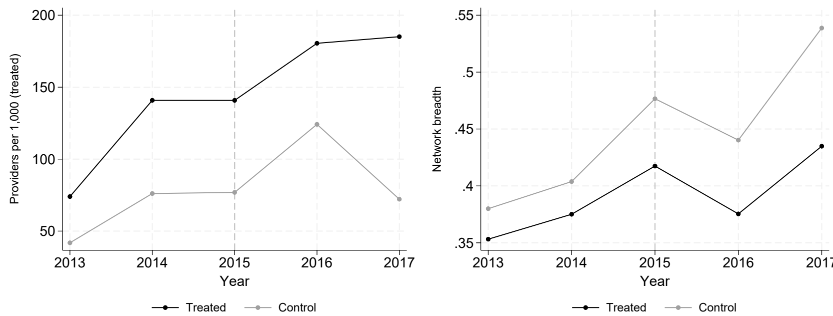



*Note*: Figure shows average mortality rates in treated and control municipalities during the sample period.

In appendix figure 6 we estimate our event study specification conditional on individuals (treated and controls) who received a particular diagnosis at any point during the sample period 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. Coefficients and standard errors are re-

Appendix Figure 4: Descriptive Evidence of Parallel Network Trends

(a) Providers per 1,000 enrollees (b) Municipal network breadth

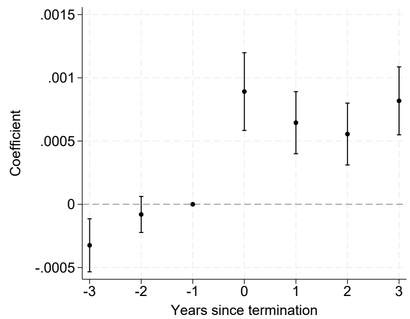



*Note*: Panel A shows average number of in-network providers per 1,000 enrollees and panel B shows average municipal network breadth in treated and control municipalities during the sample period.

ported in appendix table 5. In all cases we see that mortality increases the year after the termination and that this effect is persistent over time. The rapid response of mortality rates to SaludCoop's termination is therefore explained by individuals with chronic diseases who see their healthcare treatments interrupted or compromised due to congestion.

Appendix figure 7 replicates our event study specification on the number of visits per provider, number of providers per 1,000 enrollees, and municipal network breadth excluding markets where SaludCoop operated with its own hospitals.

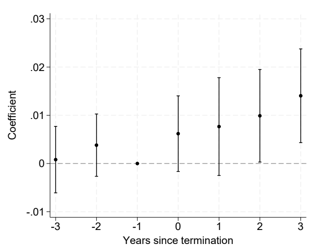
Appendix Figure 5: Mortality Effect Excluding Markets with SaludCoop Hospitals



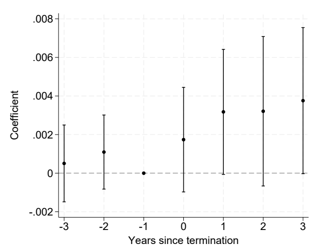

*Note*: Figure shows event study coefficients and 95 percent confidence intervals of enrollee mortality excluding municipalities where SaludCoop hospitals operated. Specification includes demographic controls, and municipality, year, and insurer fixed effects. Standard errors are clustered at the municipality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

Appendix Figure 6: Mortality Effect by Diagnosis

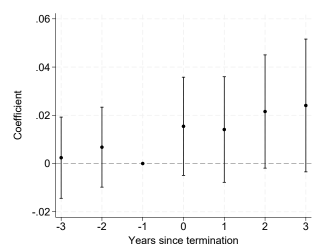
(a) AMI



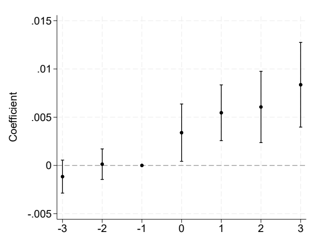

(b) COPD



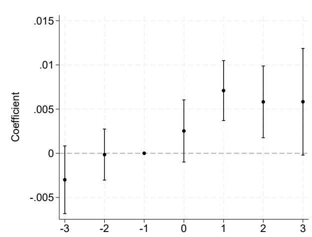

(c) Hepatic disease




(d) CKD



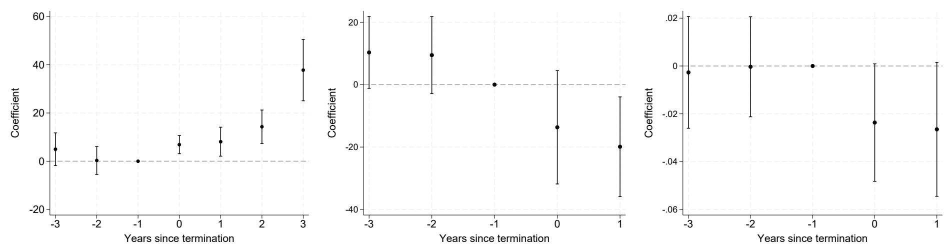

(e) Cancer




*Note*: Figure shows event study coefficients and 95 percent confidence intervals conditional on patients who were diagnosed at any point during the sample period with Acute Myocardial Infarctions (AMI) in panel A, Chronic Obstructive Pulmonary Disease (COPD) in panel B, hepatic disease in panel C, Chronic Kidney Disease (CKD) in panel D, and cancer in panel E.

Appendix Figure 7: Impact on Networks Excluding Markets with SaludCoop Hospitals

(a) Visits per provider (b) Providers per 1,000 (c) Munic. network breadth




*Note*: Panel A shows event study coefficients and 95 percent confidence intervals of number of visits per provider. Specification uses data at the provider-insurer-year level and includes municipality, insurer, provider, and year fixed effects. Standard errors are clustered at the municipality level. Panels B and C show event study coefficients and 95 confidence intervals of providers per 1,000 enrollees and municipal network breadth, respectively, conditional on insurers having more than 0.05% market share in the municipality. Specifications use data at the insurer-market-year level and include municipality and year fixed effects. We have hospital network data from 2013 to 2017, thus we exclude years 2 and 3 relative to the termination from panels B and C. In each specification, treatment is defined as municipalities where SaludCoop was present in 2015 and we exclude municipalities where SaludCoop operated with its own hospitals.

# Appendix E Event Study Coefficients

Appendix Table 2: Mortality Effect

| | Main | Without SaludCooop hosp |
|---|---|---|
| | (1) | (2) |
| t-3 | -0.0003 | -0.0003 |
| | (0.0003) | (0.0001) |
| t-2 | 0.0001 | -0.0001 |
| | (0.0001) | (0.0001) |
| t-1 | (ref) | (ref) |
| t+0 | 0.0014 | 0.0009 |
| | (0.0003) | (0.0002) |
| t+1 | 0.0010 | 0.0006 |
| | (0.0002) | (0.0001) |
| t+2 | 0.0008 | 0.0006 |
| | (0.0002) | (0.0001) |
| t+3 | 0.0012 | 0.0008 |
| | (0.0002) | (0.0001) |
| Individuals x Year | 124,796,233 | 65,695,465 |
| Individuals | 23,264,825 | 12,751,521 |

*Note*: Table reports coefficients and standard errors in parenthesis of a regression of individual mortality on time indicators relative to SaludCoop's termination. Specifications include demographic controls, and insurer and municipality fixed effects. Column (1) uses the full sample and column (2) exclude municipalities where S aludCoop operated with its own hospitals. Standard errors are clustered at the municipality level.

## Appendix Table 3: Hospital Networks

| | Visits per provider (1) | Providers per enrollee (2) | Network breadth (3) |
|---|---|---|---|
| t-3 | 0.6907 | 11.299 | 0.0046 |
| | (2.6427) | (5.6854) | (0.0114) |
| t-2 | 5.1827 | 8.7225 | 0.0045 |
| | (4.4660) | (6.0845) | (0.0102) |
| t-1 | (ref) | (ref) | |
| t+0 | 8.0661 | -15.164 | -0.0185 |
| | (2.7401) | (9.061) | (0.0121) |
| t+1 | 13.045 | -19.770 | -0.0319 |
| | (3.1637) | (7.9368) | (0.0137) |
| t+2 | 21.464 | — | — |
| | (5.5171) | | |
| t+3 | 41.355 | — | — |
| | (6.8737) | | |
| Observations | 7,444,963 | 20,264 | 20264 |

*Note*: Table reports coefficients and standard errors in parenthesis of visits per provider in column (1), providers per 1,000 enrollees in column (2), and municipal network breadth in column (3) on time indicators relative to SaludCoop's termination. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level.

## Appendix Table 4: Mortality Effect by Network Overlap

| | Above-average overlap (1) | Below-average overlap (2) |
|---|---|---|
| t-3 | -0.0004 | -0.0003 |
| | (0.0002) | (0.0001) |
| t-2 | -0.0001 | -0.0001 |
| | (0.0001) | (0.0001) |
| t-1 | (ref) | (ref) |
| t+0 | 0.0008 | 0.0009 |
| | (0.0002) | (0.0002) |
| t+1 | 0.0004 | 0.0008 |
| | (0.0002) | (0.0001) |
| t+2 | 0.0003 | 0.0007 |
| | (0.0002) | (0.0001) |
| t+3 | 0.0004 | 0.0010 |
| | (0.0002) | (0.0002) |
| Individuals x Year | 36,205,611 | 50,481,424 |
| Individuals | 7,053,206 | 9,790,330 |

*Note*: Table reports coefficients and standard errors in parenthesis of a regression of individual mortality on time indicators relative to SaludCoop's termination. Specifications include demographic controls, and insurer and municipality fixed effects. Column (1) uses the subsample of treated municipalities where the average insurer had below-average overlap with SaludCoop. Column (2) uses the subsample of treated municipalities where the average insurer had above-average overlap with SaludCoop. Standard errors are clustered at the municipality level.

Appendix Table 5: Mortality Effect by Diagnosis

| | AMI (1) | COPD (2) | Hepatic (3) | CKD (4) | Cancer (5) |
|---|---|---|---|---|---|
| t-3 | 0.0008 | 0.0005 | 0.0024 | -0.0012 | -0.0030 |
| | (0.0035) | (0.0010) | (0.0086) | (0.0009) | (0.0020) |
| t-2 | 0.0038 | 0.0011 | 0.0068 | 0.0001 | -0.0001 |
| | (0.0033) | (0.0010) | (0.0084) | (0.0008) | (0.0015) |
| t-1 | (ref) | (ref) | (ref) | (ref) | (ref) |
| | 0.0062 | 0.0017 | 0.0154 | 0.0034 | 0.0025 |
| t+0 | (0.0040) | (0.0014) | (0.0104) | (0.0015) | (0.0018) |
| | 0.0077 | 0.0032 | 0.0141 | 0.0055 | 0.0071 |
| t+1 | (0.0052) | (0.0017) | (0.0111) | (0.0015) | (0.0017) |
| | 0.0099 | 0.0032 | 0.0216 | 0.0061 | 0.0058 |
| t+2 | (0.0049) | (0.0020) | (0.0119) | (0.0019) | (0.0021) |
| | 0.0140 | 0.0038 | 0.0241 | 0.0084 | 0.0058 |
| t+3 | (0.0049) | (0.0019) | (0.0140) | (0.0022) | (0.0031) |
| Individuals x Year | 355,543 | 2,519,182 | 71,598 | 1,517,878 | 1,956,568 |
| Individuals | 52,880 | 376,268 | 10,742 | 221,515 | 290,291 |

*Note*: Table reports coefficients and standard errors in parenthesis of a regression of individual mortality on time indicators relative to SaludCoop's termination. Specification includes demographic controls, and insurer and municipality fixed effects. Standard errors are clustered at the municipality level. Results use the subsample of individuals who were diagnosed at any point during the sample period with Acute Myocardial Infarctions (AMI) in column (2), Chronic Obstructive Pulmonary Disease (COPD) in column (2), hepatic diseases in column (3), Chronic Kidney Disease (CKD) in column (4), and cancer in column (5).

# Appendix F Health Claims

The reduction in the number of in-network providers is compatible with the idea that insurers engage in risk selection using their hospital networks. Leveraging strong inertia among their current enrollees, incumbent insurers may drop provider coverage to potentially discourage enrollment from individuals previously enrolled to SaludCoop.[18]

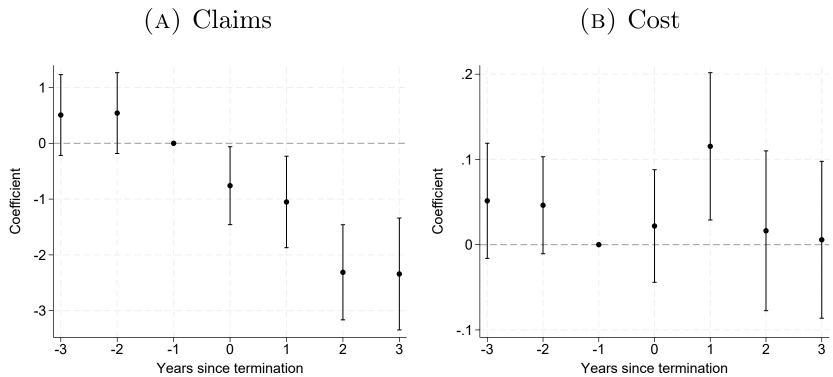
The bargaining literature in health care suggests that insurers who were effective at narrowing their networks, would have negotiated lower prices with in-network providers. This is because providers' disagreement payoffs –defined as the profits they would enjoy from dropping an insurer– likely decreased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

To conduct this analysis we use the claims data. Because the Ministry of Health imposes several data quality filters before releasing the data, we do not observe all insurers every year.[19] This means that individual-level measures of utilization and costs will have missing values. Because of this, we aggregate our data to the municipality-year level, calculating averages across all individuals

[18]This is incentive is similar to the "invest-then-harvest" incentive in markets with consumer switching costs that has been the focus of much theoretical and empirical research (Farrell and Klemperer, 2007).

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

Appendix Figure 8: Impact of Congestion on Prices



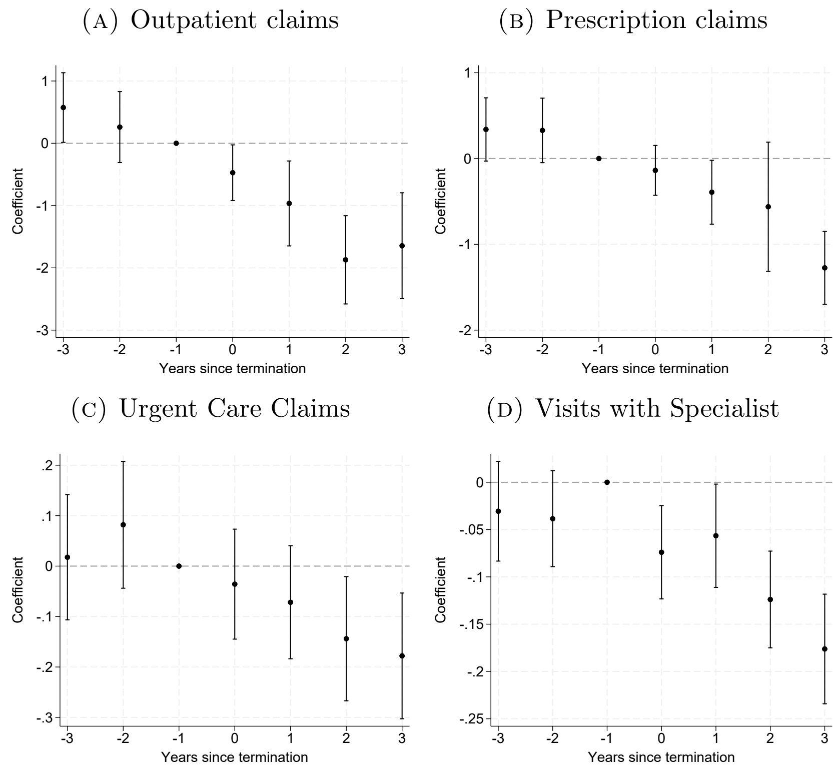

*Note*: Figure shows event study coefficients and 95 percent confidence intervals of annual number of claims in panel (A) and annual health care cost in millions of pesos in panel (B). Specifications use individual level data from enrollees in the contributory system aggregated or averaged to the municipality-year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

enrolled with the insurers that we observe.[20] Our analysis therefore will be indicative of changes in utilization and costs for the *average enrollee* in the contributory system.

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

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

Appendix Figure 9: Impact of Congestion on Types of Claims




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

Despite significant declines in utilization after 2015, the cost of the average enrollee did not change as seen in panel B of the figure. These findings imply that the price per claim increased after the termination. Our results in appendix figure 8 reinforce the importance of narrow networks in generating a congestion effect. The reduction in the number of covered providers in each municipality must be substantial to explain why each provider renders more visits even when the total number of claims falls after the termination.

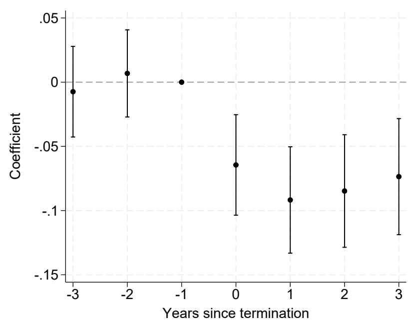
The reduction in utilization happens across different types of claims. Panel

A of appendix figure 9 shows that the average consumer made 1.5 fewer outpatient claims a year after the termination. Likewise, in panels B and C we see that the average consumer filed 1.5 fewer prescription claims and 0.2 fewer urgent care claims around 2018. Finally, panel D shows that the average consumer in treated municipalities had 0.2 fewer visits to the specialist right after the termination. Importantly, average enrollees in treated and control municipalities had parallel utilization trends across these types of claims in the pre-period. Therefore, reductions in utilization after 2015 are suggestive of consumers in treated municipalities not receiving the type of care that they need.

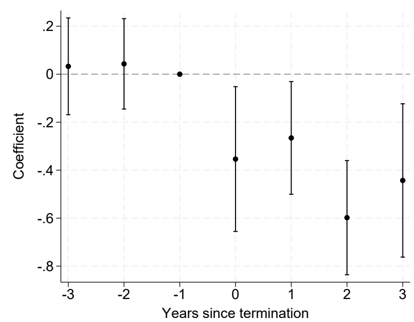
We find that utilization of health services needed for prevention or early detection of serious health conditions also decreased after the termination. Panels A and B of figure appendix 10 show that the average consumer made 0.2 fewer imaging claims and received 1 fewer lab test in treated municipalities two years after the termination. In panel C we find that the average diabetic in treated municipalities received 0.2 fewer A1C lab tests every year after the termination, a service that is required for adequate diabetes management. Additionally, panel D shows that the average woman experienced a reduction of 1.5 percentage points in the likelihood of claiming services related to breast cancer screening, such as mammograms and breast magnetic resonance imaging.

Appendix Figure 10: Impact of Congestion on Preventive and Diagnostic Aid

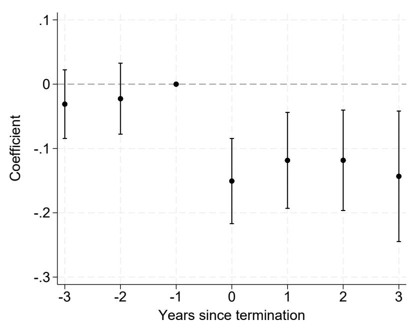
(A) Imaging



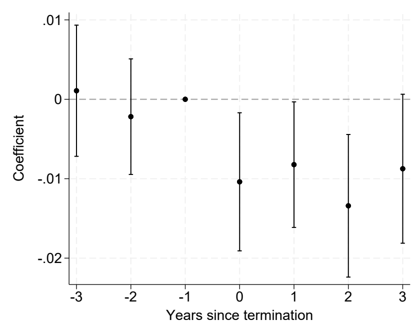

(B) Laboratory




(C) A1C tests



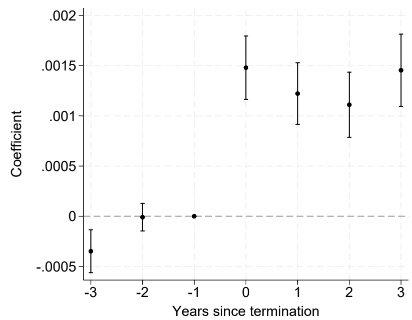

(D) Breast Cancer Screening




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

# Appendix G Robustness Checks

Appendix Figure 11: Mortality Effect Excluding Bogotá and Medellín




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

# Appendix H Extension of Hospital Choice Model

In this appendix we extend the relation between the measure of willingness-to-pay for hospital 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 $\theta$, $\xi_{hm}$ captures shared preferences across consumers for hospital $h$, and $\varepsilon_{ijhm}$ 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 $\gamma_\theta$ be the fraction of consumers type $\theta$ 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:

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

where $\overline{\xi}_{jm} = |G_{jm}|^{-1}\sum_{h\in G_{jm}}\xi_{hm}$ is the average quality of the hospitals in insurer $j$'s network. 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 I 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 $\overline{\xi}_{jmt}H_{j(i)mt}$ 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 $\overline{\xi}_{jmt}H_{j(i)mt} = w_{j(i)mt} - \nu_{imt}$. 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 $var(\nu_{imt}) = \sigma^2_\nu$ and $var(w_{j(i)mt}) = \sigma^2_w$. 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 $\alpha$ 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

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

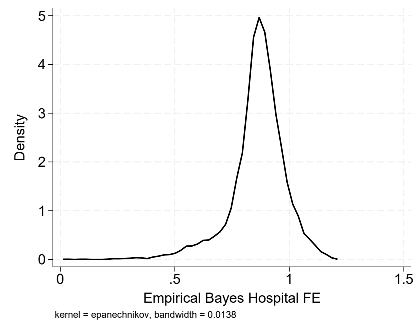
Let $\lambda = \frac{\sigma_w^2 + lE[w]}{\sigma_w^2 + \sigma_\nu^2}$. The bias in the OLS estimator is:

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

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 J First-Stage Regressions and Robustness Check

Appendix Figure 12: Distribution of Bayes-Adjusted Hospital Fixed Effects




Appendix Table 6: First-Stage Regression of Network Municipal Breadth

| | (1)<br>Raw | (2)<br>Quality-adjusted |
|---|---|---|
| Instrument | 0.3467 | 0.3485 |
| | (0.0413) | (0.0417) |
| F statistic | 70.54 | 69.83 |
| IQ range network breadth | [0.289, 0.516] | [0.234, 0.429] |
| Individuals x Years | 38,580,349 | 38,580,349 |

*Note*: Column (1) reports first-stage regression results of municipal network breadth on the interaction between treatment indicator, post-termination period indicator, and municipal network breadth in 2015. Column (2) reports first-stage regression results of quality-weighted municipal network breadth on the interaction between treatment indicator, post-termination period indicator, and quality-weighted municipal network breadth in 2015. 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 7: Placebo Test on Violent Deaths

| | (1)<br>Raw | (2)<br>Quality-adjusted |
|---|---|---|
| Instrument | -0.0002 | -0.0002 |
| | (0.00004) | (0.0001) |
| Individuals x Years | 38,401,034 | 38,401,034 |

*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 our instrument for municipal network breadth weighted by the average in-network provider quality. 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 8: Placebo Test on Fetal Deaths per 1,000 Enrollees

| | (1) Raw | (2) Quality-adjusted |
|---|---|---|
| Instrument | -7.991 | -8.128 |
| | (5.193) | (5.220) |
| Individuals x Years | 6,948 | 6,948 |

*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 our instrument for municipal network breadth weighted by the average in-network provider quality. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level.

### Appendix Table 9: Placebo Test on Deaths by Suicide

| | (1) Raw | (2) Quality-adjusted |
|---|---|---|
| Instrument | -0.000011 | -0.000014 |
| | (0.000010) | (0.000011) |
| Individuals x Years | 38,397,964 | 38,397,964 |

*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 our instrument for municipal network breadth weighted by the average in-network provider quality. 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 10: Reduced-Form Estimates

| | (1) Raw | (2) Quality-adjusted |
|---|---|---|
| Instrument | -0.0040 | -0.0043 |
| | (0.0014) | (0.0016) |
| Individuals x Years | 38,580,349 | 38,580,349 |

*Note*: Column (1) reports reduced-form estimates of individual mortality on the interaction between treatment indicator, post-termination period indicator, and municipal network breadth in 2015. Column (2) reports reduced-form estimates of individual mortality on the interaction between treatment indicator, post-termination period indicator, and quality-weighted municipal network breadth in 2015. All specifications include demographic controls (sex and age) and insurer and municipality-by-year fixed effects. Standard errors in parenthesis clustered at the municipality level.

Appendix Table 11: Regression of Mortality on Admission-Weighted Quality-Adjusted Network Breadth

| | (1) OLS | (2) IV |
|---|---|---|
| Network breadth | -0.0024 | -0.0113 |
| | (0.0014) | (0.0046) |
| F-statistic | — | 76.35 |
| IQ range network breadth | [0.241, 0.433] | [0.241, 0.433] |
| Individuals x Years | 38,580,349 | 38,580,349 |

*Note*: Table reports coefficients and standard errors in parenthesis of a regression of individual mortality on admission-weighted quality-adjusted network breadth. Weights for each in-network provider are calculated relative to the total number of admissions for each insurer over the sample period, and are constant over time. Column (1) estimates the equation of interest using OLS. Column (2) uses our instrumental variable specification. The instrument corresponds to 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. First-stage F-statistic reported in columns (3) and (4). Interquartile range of network breadth in reported in brackets.

Appendix Table 12: IV Regression of Mortality Controlling for Claims

| | (1) Raw | (2) Quality-adjusted |
|---|---|---|
| Network breadth | -0.0103 | -0.0110 |
| | (0.0042) | (0.0046) |
| F statistic | 70.88 | 70.17 |
| IQ range network breadth | [0.289, 0.516] | [0.234, 0.429] |
| Individuals x Years | 38,580,349 | 38,580,349 |

*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 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 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 13: IV Regression of Mortality Excluding Rural Areas

| | (1)<br>Raw | (2)<br>Quality-adjusted |
|---|---|---|
| Network breadth | -0.0111 | -0.0120 |
| | (0.0043) | (0.0047) |
| F statistic | 72.91 | 72.43 |
| IQ range network breadth | [0.289, 0.500] | [0.234, 0.416] |
| Individuals x Years | 38,010,809 | 38,010,809 |

*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. 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 Table 14: IV Regression of Mortality on Mortality-Weighted Network Breadth

| | (1)<br>Mortality-adjusted |
|---|---|
| Network breadth | -0.4146 |
| | (0.5421) |
| F statistic | 134.64 |
| IQ range network breadth | [0.0005, 0.0015] |
| Individuals x Years | 38,539,089 |

*Note*: Table reports coefficients and standard errors in parenthesis of an instrumental variables regression of individual mortality on municipal network breadth weighted by the negative of the mortality rates for in-network providers. Weights are calculated from a linear regression of individual mortality on provider fixed effects controlling for patient characteristics. We apply an empirical Bayes shrinkage procedure to the estimated provider fixed effects. The instrument is the measure of network breadth in 2015 interacted with the treatment indicator and the post-termination period indicator. Specification includes demographic controls (sex and age) and insurer and municipality-by-year fixed effects. Standard errors are clustered at the municipality level. Interquartile range of mortality-weighted network breadth in reported in brackets.