Do Health Insurers Matter for Health Outcomes? Evidence from a Large Natural Experiment in Colombia*

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Abstract

The effects of health insurance coverage on health have been widely studied, yet evidence on the aspects of insurance that influence health outcomes remains scant, particularly in systems with private insurers. We study this question by leveraging the abrupt exit of Colombia's largest health insurer and examining the spillover effects on patients who were not enrolled with the terminated insurer. The termination caused a 10% decrease in the breadth of provider networks among incumbent insurers and a 26% increase in mortality rates. We show two mechanisms drove the change in mortality: increased congestion stemming from the surge of enrollees at incumbent insurers and reduced provider network breadth. Insurers narrowed their networks by excluding providers that served sicker, more expensive patients. Findings suggest insurers play a key role in patient health through their provider networks.

Keywords: Mortality, Provider networks, Health insurance, Congestion externalities.

JEL codes: I10, I11, I13, I18.

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1 Introduction

Insurers play a key role in the operation of health systems. It is well established that they influence the health of their enrollees through financial coverage (Chandra et al., 2024; Buitrago et al., 2021). However, beyond these financial aspects, insurers may also shape health outcomes through operational and organizational decisions that determine access to care. In particular, the rise of managed care has introduced a central mechanism through which insurers exert influence: the design of provider networks. By determining which hospitals, clinics, and physicians are covered under a health plan, insurers play a direct role in structuring the delivery of care (Cutler et al., 2000; Glied, 2000; Cutler and Reber, 1998). Despite the greater role that insurers play in structuring health care, causal evidence of whether they impact health outcomes is scant. One explanation for this dearth of evidence is the limited availability of health claims data from private insurers and of exogenous variation in enrollment.

In this paper, we leverage a unique, large natural experiment in Colombia's health insurance system and rich individual-level administrative data for the entire country to provide some of the first causal evidence on how insurers determine health outcomes. In December 2015, Colombia's largest health insurer, SaludCoop—which covered 20% of the country's population and operated in 43% of municipalities—was abruptly terminated due to political considerations and engagement in illegal activities unrelated to its overall performance. To preserve coverage, the government reassigned all of SaludCoop's enrollees to a much smaller insurer, Cafesalud, which covered less than 5% of the population and was present in 10% of municipalities in 2014. SaludCoop's enrollees were required to stay in Cafesalud for only 90 days, after which they could switch to any insurer of their choice.

Given the managed care nature of Colombia's system, we study how incumbent insurers (excluding SaludCoop and Cafesalud) strategically responded to the termination through supply-side channels, particularly provider network design, and assess the consequences of these responses for patient mortality. The novelty of our study lies in quantifying these equilibrium effects in a setting where access to care changed not be-

cause of financial barriers, but because of insurers' supply-side responses within a tightly regulated environment.

This distinguishes our analysis from prior research on insurance coverage by highlighting the role of non-financial design features in shaping health outcomes. We build on research examining health plan terminations (e.g., Abaluck et al., 2021) and the effects of insurance coverage on health (e.g., Duggan et al., 2022; Goldin et al., 2020; Miller et al., 2021; Ghosh et al., 2019; Currie and Gruber, 2001), and show that provider networks—a key lever in managed care—are a significant mechanism through which insurers influence the production of health.

Colombia's health system resembles other managed care systems like Medicaid managed care and Medicare Advantage in the US or the Netherlands' health insurance model. Insurers compete for enrollees by offering a single standardized plan. While premiums, cost-sharing, and service coverage are regulated, provider networks are not.[1] This regulatory structure implies that the primary margin through which insurers compete and adjust to market conditions is via their provider networks. These networks are shaped through decentralized negotiations: insurers and providers contract freely on which services are covered, the payment schemes used, and applicable fees.

Provider networks are thus not merely lists of participating providers, but a central mechanism through which insurers influence both access to care and the quality of services delivered. When insurers exclude providers offering essential services—particularly for treatment chronic conditions—care continuity can be disrupted and outcomes compromised, especially in systems where patients have limited access to out-of-network providers.

We combine our unique natural experiment with administrative data encompassing health claims, enrollment records, and mortality for all the population from 2013 to 2019. We also have detailed information on insurers' provider listings, which outline the hospitals, clinics, and physician practices included in their networks. As a first step, we use a difference-in-differences framework to compare outcome trends between incumbent

[1]Insurance premiums are zero and copays, coinsurance rates, and maximum out-of-pocket amounts are indexed to the enrollee's monthly income but are standardized across insurers and hospitals.

insurers in municipalities where SaludCoop operated (treatment group) versus municipalities where it did not operate (control group), before and after the termination. As a second step, to examine impacts on health, we compare mortality trends among non-SaludCoop enrollees in treated versus control markets, over a cohort of patients who never switched their insurer nor moved across municipalities (nearly 25 million individuals and 125 million observations).

Regarding insurers' strategic responses, we first find that incumbent insurers in treated municipalities reduced provider network breadth by approximately 10% relative to baseline. This reduction in the share of covered providers is consistent with strategic behavior aimed at discouraging enrollment from potentially unprofitable switchers from SaludCoop after the 90-day grace period. Reductions in network breadth were larger in markets where SaludCoop's enrollees had relatively worse baseline health, and we show that individuals in poor health have stronger preferences for broad provider networks.

Concerning mortality, we estimate a persistent 26% increase among non-SaludCoop enrollees in treated municipalities. This effect is substantial and comparable in magnitude to the 24% increase observed among SaludCoop's enrollees, who were more directly exposed to care disruptions following the abrupt insurer transition. The increase in mortality is concentrated among the elderly and individuals with chronic health conditions—such as chronic kidney disease, hepatic disease, and diabetes. The mortality results are likely driven by interruptions in care, as we observe reductions in the use of services essential for disease management, including oxygen and corticosteroids for patients with chronic obstructive pulmonary disease, and specialist consultations for patients with any chronic illness.

We investigate several potential mechanisms behind the increase in mortality among non-SaludCoop enrollees. A first mechanism, based on risk selection, is that provider network exclusions were targeted to discourage enrollment from unhealthy former Saludcoop enrollees. Consistent with this, we find that incumbent insurers disproportionately dropped coverage of large outpatient clinics, public hospitals, and cancer care providers that previously served sicker and more expensive patients. We also find that mortality increased substantially among consumers who had a high share of pre-termination claims

at providers that were later dropped from the network, whereas those with a low share of claims at these providers experienced no change in mortality. These patterns suggest that supply-side risk selection incentives are misaligned with the goal of maintaining patient health and are particularly harmful to those with greater healthcare needs.

The second mechanism involves provider networks but through congestion externalities, which we define as increased demand for limited provider capacity resulting from the sudden influx of patients into incumbent insurers' networks. These effects arise when former SaludCoop enrollees, after the 90-day grace period in Cafesalud, switch to incumbent insurers and concentrate demand on a narrower set of providers. To capture this mechanism, we measure the share of SaludCoop's in-network providers that were also covered by each incumbent insurer prior to the termination. When this overlap is high, incoming enrollees likely maintain access to their previous providers, minimizing disruptions in care and congestion. When overlap is low, patients must shift to new providers, increasing the patient load at those facilities. We find that mortality increased significantly only in municipalities where incumbent insurers had below-median network overlap with SaludCoop—consistent with congestion adversely impacting timely access to care and, ultimately, patient outcomes.

Finally, we rule out alternative mechanisms such as changes in market concentration or shifts in healthcare labor supply. The results highlight that provider network exclusions—a common cost-containment strategy—can significantly impair access to essential services and worsen patient health, even under universal insurance coverage.

Contributions and Relation to the Literature. This paper contributes to the growing literature analyzing the causal effects of narrow provider networks in managed care health systems, which has mostly focused on outcomes like utilization, spending, and premiums (e.g., Kreider et al., 2024; Wallace, 2023; Atwood and Sasso, 2016). It also relates more broadly to the study of how health insurance and managed care affects health outcomes (e.g., Conti and Ginja, 2023; Das and Do, 2023; Balsa and Triunfo, 2021; Goldin et al., 2020; Miller et al., 2021; Bauernschuster et al., 2020; Sood and Wagner, 2018; Wherry and Miller, 2016; Miller et al., 2013; Aizer et al., 2007; Cutler et al., 2000). We show that in managed care systems health insurers play an active role in the provision of health care

by establishing their provider networks. Thus, access to providers and the services they render is an important mechanism through which insurers impact patient mortality.

Our paper is also related to the literature analyzing interruptions in health care due to involuntary patient switches of insurer or provider (e.g., Bonilla et al., 2024; Chamorro et al., 2024; Sabety, 2023; Politzer, 2021; Barnett et al., 2017; Lavarreda et al., 2008). We contribute to this literature by studying a large insurer termination that was politically motivated (due to corruption scandals) and unrelated to its quality. In addition to quantifying effects on patient health, we provide some of the first estimates of the equilibrium effects on provider networks and congestion externalities. We document how incumbent insurers react strategically to a competitor's termination and how these decisions have downstream impacts on health. The negative effects in health that we find also serve as an estimate of the severe consequences of insurers' closures, and justify the stringent regulation that this sector is generally subject to.

Finally, this paper contributes to the literature studying insurer competition on provider networks and its regulation. Several papers examine the relationship between provider network breadth, premiums, and negotiated health service prices (e.g., Ghili, 2022; Liebman, 2022; Ho and Lee, 2019; Ho, 2009; Dafny et al., 2017, 2015). Other papers analyze insurers' incentives to establish narrow networks (e.g., Serna, 2024a; Shepard, 2022; Ho and Lee, 2017). Yet, to date, evidence on whether provider networks affect the production of health is limited. We bridge the literature on industrial organization of health care markets and health outcomes research by providing evidence that insurers' strategic interactions affect the health production function.[2]

The rest of the paper is organized as follows. Section 2 presents the institutional background. Section 3 describes the data. Section 4 shows how the termination affected provider networks and mortality. Section 5 explores the mechanisms behind these effects. Section 6 concludes.

[2]There are a few papers in this area, such as Gaynor et al. (2013), Cooper et al. (2011), and Propper et al. (2008) who estimate the impact of hospital competition and market power on patient outcomes in the context of the National Health Service in the UK.

2 Institutional Background

The Colombian healthcare system is divided into two schemes: contributory and subsidized. The contributory scheme covers approximately half of the population, consisting of formal workers and their families who pay payroll taxes, while the subsidized scheme is fully funded by the general budget. As of 2020, nearly 95% of the population was covered by this system (MinSalud, 2020).

Both contributory and subsidized scheme enrollees have access to the same national health insurance plan through a range of private and public insurers. Almost all aspects of the national insurance plan—including premiums, patient cost-sharing, and service coverage—are regulated, with the exception of provider networks. Insurers in Colombia have the flexibility to choose which providers to include for each health service covered by the national insurance plan and can freely establish contracts with them through bilateral negotiations.

Enrollees do not pay insurance premiums; instead, at the beginning of each year, insurers receive per-capita transfers from the government that are risk-adjusted based on the enrollee's sex, age, and municipality of residence. At the end of each year, insurers are also compensated for the health conditions of their enrollees based on a coarse list of diagnoses known as the High-Cost Account. However, these risk adjustment mechanisms are imperfect and do not fully eliminate incentives for risk selection (Riascos, 2013).

Insurers in Colombia respond to these selection incentives through their provider networks. Serna (2024a) demonstrates that while all consumers prefer broad networks, those with chronic diseases—who may be unprofitable—exhibit a significantly higher willingness to pay for network breadth. Consequently, to deter enrollment from these potentially unprofitable patients, insurers tend to offer narrower networks for services that such patients are likely to need.

In this paper, we leverage an exogenous insurer termination in Colombia to analyze whether and how incumbent insurers respond using their provider networks as well as the implications for patient health. The Colombian government can terminate insurers if they divert resources away from the health care system or if they cannot maintain their

risk-based capital requirements.[3]

In December 2015, the government terminated the *largest* health insurer in the country, called SaludCoop, due to engagement in illegal activities.[4] Its board of directors diverted nearly 1.3% of total health care spending in 2015 to investments outside the health system, engaged in financial malpractice, and submitted false health claims to the government for reimbursement. The CEO and board of directors were fined 50 monthly minimum wages, prohibited from working in public office, and prohibited from participating in public auctions for at least 18 years. Appendix C provides a timeline of the termination.[5]

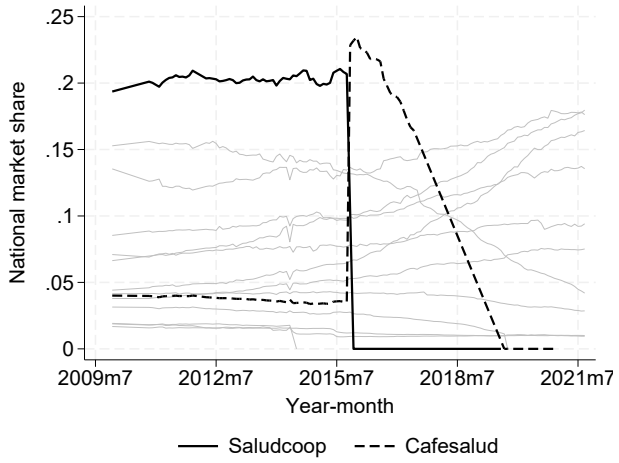


FIGURE 1: National Market Share

Note: Figure shows monthly national market share per insurer from 2009 to 2021 using publicly available enrollment counts for both the contributory and subsidized schemes.

SaludCoop's enrollees were transferred to an incumbent insurer called Cafesalud. The government chose Cafesalud as the reassignment insurer because (allegedly) it operated in almost the same municipalities as SaludCoop (see Appendix Figure 1). SaludCoop's enrollees had to remain in Cafesalud for 90 days, from January to March 2016, after which they were allowed to switch their insurer. During the reassignment period, Cafesalud

[3]Another reason for termination includes low enrollee satisfaction scores based on surveys conducted by the Ministry of Health and Social Protection. See Decree 780 of 2016.

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

[5]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*.

had to guarantee access to health care for SaludCoop's enrollees at the providers that SaludCoop used to cover in its network. The government made a $70 million loan to Cafesalud to facilitate this transition.

Figure 1 shows the national market share per insurer in the contributory scheme. We emphasize SaludCoop and Cafesalud in black, and the rest of the insurers are illustrated in gray. SaludCoop (solid black line) covered on average 20% of enrollees in the years before its termination.[6] SaludCoop and Cafesalud participated in both the contributory and the subsidized schemes. Cafesalud had a national market share under 5% before the termination, 23% in the first three months of 2016, and was itself terminated in 2019.[7]

SaludCoop's termination resulted in significant changes to the provision of healthcare in Colombia, with repercussions that continue to this day in the form of ongoing fines and debts. This termination not only decreased the number of available insurers but also impacted the hospital capacity in some municipalities. 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 from 2013 to 2019.

In 2014, SaludCoop owned 38 hospitals and clinics, which accounted for 2,354 out of the approximately 80,000 hospital beds nationwide. SaludCoop operated hospitals in 31 municipalities (out of 1,120 in the country), and in 12 of those, insurers other than SaludCoop and Cafesalud covered SaludCoop hospitals. Additionally, beyond these 31 municipalities, SaludCoop operated in 452 others without having its own hospitals.

3 Data

3.1 Data Sources and Definitions

Our enrollment data comprises all enrollees to the contributory and subsidized schemes, nearly the entire population in the country. We have a snapshot of enrollment data

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

[7] Cafesalud was terminated due to consistent patient complaints about the quality of care and flailing profits after the reassignment of SaludCoop's enrollees.

for every June from 2013 to 2019, corresponding to three years before and four after SaludCoop's termination. Because we do not see enrollment every month, we assume that if an individual is enrolled with insurer A in June 2013, they remain with this insurer every month until June 2014 when we see the next enrollment snapshot.[8] The enrollment files contain the individual's sex, age, municipality of residence, and insurer.

At the end of every year, insurers in the contributory and subsidized schemes report all of their enrollees' health claims to the government. The government uses this data annually to update the risk-adjusted transfers and imposes several data quality filters. We have health claims data only for insurers in the contributory scheme that passed these quality filters from 2013 to 2019, which represent 88% of enrollees in this scheme by the end of the sample period. We do not have claims data for individuals in the subsidized scheme.

The health claims data report the date the claim was filed, enrollee identifier, associated International Classification of Diseases Code 10 (ICD-10), provider that rendered the claim, insurer that reimbursed the claim, and negotiated service price between the insurer and the provider. We do not observe the patient's residence address but their municipality of residence.

From the Ministry of Health and Social Protection, we obtain individual-level mortality from 2013 to 2019. Anonymous individual identifiers are the same across datasets, allowing us to merge mortality with enrollment and health claim information. The death certificates data report date of death, cause of death, manner of death (fetal, violent, or natural), indicator for whether the individual died at the hospital or elsewhere, provider identifier, and insurer identifier.

We merge the enrollment and mortality data based on the individual identifier. Because the enrollment data has information of June of every year, if we observe a death in any other month, we append this individual to the enrollment file, so that all deaths reported in the death certificates are accounted for.[9] This way our mortality variable reflects the

[8]Conditional on staying within the same insurance regime and having continuous enrollment spells, the assumption that individuals remain enrolled with their insurer during the 12 months from June to June is consistent with the low switching rate reported in Serna (2024a).

[9]Appendix Table 1 reports the match rates between the enrollment files and the death certificates in the full sample and by treatment status. Match rates decrease over the years but do not evolve differently

annual mortality from January to December.

The mortality 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 deaths from the analysis since there is no patient identifier associated with this type of death.

Finally, we have data on insurers' network of covered providers from 2013 to 2017 from the National Health Superintendency. These data report the hospitals, clinics, and physician practices that insurers in the contributory scheme include in their networks. Whenever we use the term "provider," we refer to these health care provider organizations. We do not have a corresponding dataset for insurers that operate in the subsidized scheme.

The provider listings report the Colombian Tax Identification Number (TIN) of every in-network provider. Each TIN has multiple facility locations within a municipality. The Colombian Ministry of Health and Social Protection assigns a unique provider ID to each of these locations. The provider ID matches the health claims data and the National Registry of Health Care Providers (REPS, by its Spanish acronym), which includes the universe of providers in the country, along with characteristics like the number of beds and the health services they can provide, such as urgent care, ICU admissions, cardiology, dialysis, etc.

We match the TINs in the provider listing with provider IDs from the national registry and compare this network to the providers reporting claims in the health claims data. Around 16% of insurer-provider pairs appear in the claims data but not in the provider listings. We incorporate these pairs into our final provider network dataset.[10]

3.2 Sample restrictions

Supply-side responses. To quantify the impact of the termination on insurers' network choices, we use the provider listings to calculate their provider network breadth, defined as

between treatment and control groups.

[10]Some insurer-provider contracts may appear in the claims data but not in the provider listings if the contracts are signed after January when insurers submit their provider listings to the National Health Superintendency.

the fraction of providers in a municipality that are covered by the insurer.[11] Our empirical strategy consists of comparing incumbent insurers in municipalities where SaludCoop operated (treatment group) against those in municipalities where SaludCoop did not operate (control group).

We exclude both SaludCoop and Cafesalud from this analysis for specific reasons. First, Cafesalud was mandated by the government to cover SaludCoop's network during the three-month grace period; hence we would not anticipate observing any effects from SaludCoop's exit for this insurer. Second, Cafesalud itself ceased operations in 2019, which could bias the impacts of SaludCoop's termination on its network coverage.

For the remaining incumbent insurers, the termination of SaludCoop—and the subsequent gradual influx of individuals from Cafesalud after the grace period—represents an exogenous shock to demand. This demand shock may lead to different effects on network coverage depending on whether the incoming individuals are in better or worse health than the current set of enrollees. We will use this variation to discern whether network breadth changes after the termination are consistent with insurers engaging in risk selection.

Mortality. We perform three types of analyses to measure the impact of the termination on individual mortality. In the first analysis, we compare outcome trends among non-SaludCoop enrollees between municipalities where SaludCoop operated at the time of the termination (treatment group) against municipalities where it did not operate (control group). We restrict our data in several ways to guarantee that treated and control groups are comparable. These restrictions help control for differential adverse selection patterns across treatment status before the termination.

First, *we exclude individuals who are enrolled with SaludCoop or Cafesalud* before SaludCoop's termination, thus our results are reflective of changes in outcomes at the rest of incumbent insurers. Second, we keep individuals with continuous enrollment spells, who

[11] We prefer this measure of network coverage over others that have been proposed in the literature—such as the share of inpatient admissions represented by the network as in Ericson and Starc (2015)— for a few reasons. First, provider network breadth can be micro-founded by models of provider choice (Serna, 2024a). Second, measures of coverage that take into account patient flows into hospitals are not appropriate to capture the relevance of other types of providers like outpatient clinics. Third, SaludCoop's termination itself impacted patient flows and healthcare utilization, hence those alternative measures may be capturing changes in healthcare use rather than changes in the network.

did not switch their insurer nor moved across municipalities *before the termination*. Third, we keep a balanced panel of insurer-municipalities to avoid changes in sample composition among our treatment and control groups. Lastly, we drop special insurers such as those that cover indigenous populations, railroad workers, and those that offer services outside of health care (known as *Cajas de Compensación Familiar*). We also drop individuals for whom we see enrollment data after they die as well as those with ages over 95 years. These sample restrictions limit selection on insurer choice that is endogenously caused by changes in insurer characteristics in the pre-period only, but we allow consumers to potentially respond to the termination by switching their insurer in the post-period. Appendix Table 2 shows the number of observations that result after imposing each sample restriction.

The second analysis uses the same sample described above but imposes that patients do not switch their insurer *throughout the sample period*. This stricter restriction allows us to measure how patients are potentially exogenously affected by supply-side responses after the termination. For example, since these individuals are exposed for as long as possible to any disruption of care induced by SaludCoop's termination, we might expect to see larger adverse effects on health compared to our first sample. We note that those who did not switch their insurer during the entire sample period represent nearly 70% of observations (as seen in Appendix Table 2), hence imposing this stricter condition does not necessarily come at a cost in terms of the representativeness of our results.

In the third analysis, we measure the direct impacts of the termination on SaludCoop's enrollees. We compare SaludCoop's enrollees in municipalities where SaludCoop operated (treatment group) against individuals in municipalities where it did not operate (control group), applying the same sample restrictions as in our first analysis.[12]

[12] An alternative specification would restrict to municipalities where SaludCoop operated, and compare outcome trends between SaludCoop enrollees and individuals enrolled with other insurers. Hence, treatment would be defined at the insurer level. However, this specification likely violates the Stable Unit Treatment Value Assumption (SUTVA), since individuals residing in municipalities where SaludCoop operated are all affected by the termination through the endogenous supply-side responses.

3.3 Summary statistics

Summary statistics for our final sample of insurers and enrollees are provided in Tables 1 and 2. In both tables, we report each variable's mean and standard deviation separately for treated and control municipalities in the pre- and post-termination periods. In Table 1, an observation is an insurer-municipality-year. Treated and control municipalities are similar in their pre-period network coverage levels, but the former see a relative decrease in average provider network breadth in the post-period.

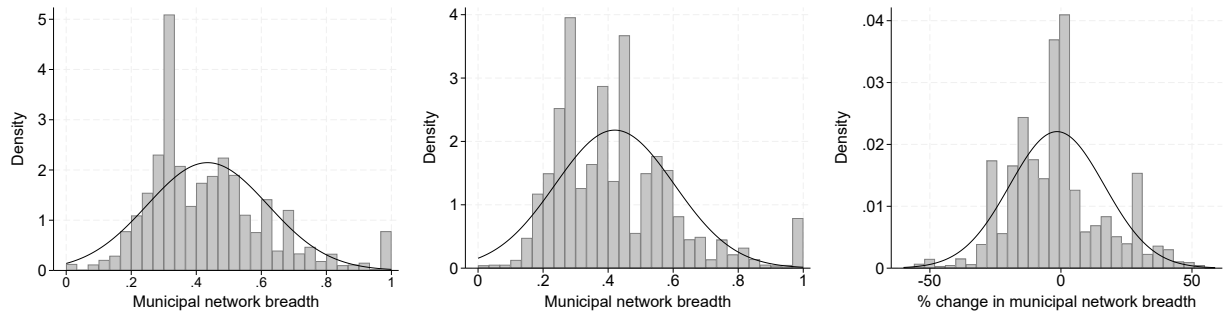
Figure 2, Panels A and B show that provider network breadth is substantially heterogeneous across insurers in the treated municipalities, both before and after the termination. As a result, percentage changes in provider network breadth in the post-period also vary significantly as seen in Panel C. Since these histograms are weighted by the number of enrollees in the pre-period, we can interpret Panel C as indicating that the majority of individuals were enrolled with insurers that narrowed the network in their municipality of residence. For example, 5.8 million individuals were enrolled with insurers that narrowed their network by more than 10%, and 1.7 million were enrolled with insurers that narrowed their network by more than 25%.

We also see that some insurers expanded their network in the post-period, which can be rationalized by the fact that not only sick, unprofitable consumers value broad networks but also healthy, profitable ones. We will return to this point in Section 4.2.

TABLE 1: Summary Statistics of Insurer Sample

Variable	Treated		Control	
	Pre	Post	Pre	Post
Provider network breadth	0.470	0.530	0.432	0.541
	(0.404)	(0.381)	(0.471)	(0.471)
Market share	0.097	0.157	0.228	0.336
	(0.144)	(0.197)	(0.281)	(0.364)
Insurers	10	10	10	9
Municipalities	483	483	627	627
Observations	6,195	2,970	7,395	3,052

Note: Table presents the mean and standard deviation in parenthesis of insurer characteristics. Summary statistics are presented separately for treated and control municipalities, in the pre- and post-termination periods. Treatment is defined as municipalities where SaludCoop operated in 2015. The data are from 2013 to 2017. An observation is a combination of insurer, municipality, and year. The sample of insurers excludes SaludCoop and Cafesalud as well as Cruz Blanca and those with less than 0.005% market share in a municipality. Provider network breadth is the fraction of providers in a market that are covered by the insurer. Market share is the insurer's share in the number of total enrollees in a municipality (without imposing sample restrictions).



(A) Pre-period (B) Post-period (C) Change

FIGURE 2: Distribution of Provider Network Breadth in Treated Municipalities

Note: Panel A shows the distribution of network breadth across insurers in treated municipalities in the pre-termination period. Panel B shows the distribution in the post-termination period. Panel C shows the distribution of percentage changes in network breadth in the post-period relative to the pre-period. Densities in these histograms are given by the number of enrollees. The solid black line is a normal distribution.

In Table 2 an observation is an enrollee-year in our first analysis sample, where we require individuals to be inertial to their insurer only in the pre-period. The table shows that treated municipalities experienced an increase in the average mortality rate and the Charlson index in the post-period.[13] Control municipalities see no change in the average mortality rate but have a similar increase in the Charlson index. Treated municipalities are slightly older and have a higher prevalence of chronic conditions at baseline than controls, perhaps raising concerns about the comparability of the two groups. In Appendix Figure 2 we corroborate that these are only level differences in characteristics that do not threat the identification assumptions of our empirical design; treated and control groups have parallel trends in socio-demographics traits and epidemiological profiles throughout the sample period. Summary statistics of individuals considered in our second and third analyses are reported in Appendix Tables 4 and 5.

Because the termination involved both the closure of the insurer and its vertically integrated hospitals, we will conduct analyses excluding the largest cities in the country where these hospitals operated, which allows us to isolate the impact of the insurer exit. Appendix Table 3 reports summary statistics excluding those municipalities. We see that treated and control groups are more similar in terms of baseline levels of comorbidities, further strengthening the comparability of the groups.

[13]The Charlson index is a measure of health status, with a higher index denoting a sicker individual (see comorbidity list). We constructed it using the claims data following Oliveros and Buitrago (2022).

TABLE 2: Summary Statistics of Pre-period Inertial non-SaludCoop Enrollees

Variable	Treated		Control	
	Pre	Post	Pre	Post
Mortality rate	0.003	0.005	0.002	0.002
	(0.053)	(0.067)	(0.040)	(0.047)
Charlson Index*	0.145	0.182	0.114	0.155
	(0.609)	(0.714)	(0.516)	(0.625)
Male	0.470	0.469	0.495	0.490
	(0.499)	(0.499)	(0.500)	(0.500)
Age	33.48	33.21	30.83	31.53
	(21.93)	(22.43)	(22.04)	(22.66)
Low income	0.085	0.062	0.112	0.081
	(0.279)	(0.242)	(0.315)	(0.272)
AMI*	0.002	0.002	0.002	0.002
	(0.041)	(0.048)	(0.040)	(0.045)
COPD*	0.022	0.023	0.016	0.017
	(0.147)	(0.150)	(0.126)	(0.131)
Hepatic disease*	0.001	0.001	0.000	0.000
	(0.024)	(0.026)	(0.020)	(0.022)
Renal disease*	0.016	0.020	0.013	0.020
	(0.124)	(0.139)	(0.113)	(0.139)
Cancer*	0.011	0.017	0.009	0.015
	(0.105)	(0.130)	(0.094)	(0.121)
Individuals	21,005,468	23,320,959	3,937,632	4,104,362
Municipalities	483	483	626	626
Individual x Year	53,332,639	77,787,629	9,873,670	13,050,775

Note: Table presents the mean and standard deviation in parenthesis of the sample of enrollees for the mortality analysis among non-SaludCoop enrollees. Summary statistics are presented separately for individuals living in treated and control municipalities, in the pre- and post-termination periods. Treatment is defined as municipalities where SaludCoop operated in 2015. An observation is an individual-year and the data are from 2013 to 2019. The sample of enrollees is restricted to those who never switched their insurer nor moved across municipalities before the termination, and who were enrolled with insurers other than SaludCoop and Cafesalud. Our final sample of enrollees does not constitute a fixed cohort. AMI stands for acute myocardial infarction and COPD for chronic obstructive pulmonary disorder. (*) Because the health claims data exists only for individuals in the contributory scheme enrolled with insurers that passed the Ministry of Health's data quality filters, summary statistics of commordities are conditional on these individuals.

4 Supply-side Responses

4.1 Provider Networks

We start our analysis by using a difference-in-differences design to estimate the effect of SaludCoop's termination on measures of provider network coverage among incumbent insurers. We compare municipalities where SaludCoop operated during 2015 (treated group) against municipalities where SaludCoop did not operate (control group) before and after the termination. The unit of treatment is, therefore, a *municipality*. The regression

of interest is:

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

where H_{jmt} is a measure of insurer j's network coverage in municipality m during year t, T_m is an indicator for treated municipalities, and γ_m and η_t are municipality and year fixed effects, respectively.

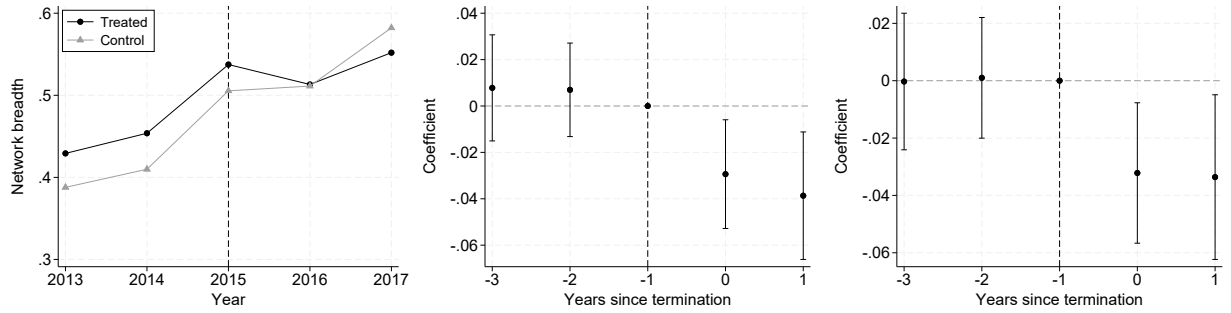
SaludCoop's termination ocurred in December 2015, which is visible in the 2016 enrollment data. The relative time indicators in equation (1) are thus constructed relative to 2016, and the omitted category is 2015. The coefficients β_k measure the average treatment effect in year k relative to 2015. Because the termination happens simultaneously for all municipalities in our treated group, we do not worry about the identification challenges from staggered treatment implementation. We cluster standard errors at the municipality level.

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

Figure 3 presents the results and Appendix F presents associated coefficients and standard errors. First of all, we see evidence of parallel trends in the pre-period from averages of the raw data in Panel A. Panel B shows that provider network breadth in treated markets decreased between 2 and 4 p.p after the termination, corresponding to a 10% reduction relative to baseline. These results are robust to excluding municipalities where SaludCoop owned hospitals as seen in Panel C, which means that the reductions in network breadth are not merely a mechanical consequence of SaludCoop hospitals closing, nor are they driven by the inherent characteristics of the highly urban markets where these hospitals operated.

These findings indicate that incumbent insurers' networks became less adequate after the termination. Insurers experienced an exogenous positive demand shock but made it

potentially more difficult for new and existing enrollees to access health care by narrowing their networks.



(A) Raw trends (B) All markets (C) w/o SaludCoop hospitals

FIGURE 3: Impact on Provider Networks

Note: Figure shows event study coefficients and 95% confidence intervals using as outcome provider network breadth. This regression uses data at the insurer-municipality-year level and conditions on insurers that have more than 0.005% market share in a municipality. We exclude Cruz Blanca insurer. Specifications include municipality and year fixed effects. Panel A shows averages of the outcome by treatment status from the raw data. Panel B presents event study results using the full sample of municipalities. Panel C excludes municipalities with SaludCoop hospitals. Standard errors are clustered at the municipality level. Treatment is defined as municipalities where SaludCoop operated during 2015.

The reductions in network breadth shown in Figure 3 are immediate. As early as 2016, we observe significant declines in our measure of network coverage, raising questions about the nature of contracts negotiated between insurers and providers. These agreements are typically established on a yearly basis and can be renegotiated at any time, making the sharp reduction in network breadth in 2016 both plausible and expected.

For instance, in the case of patients with chronic conditions, insurers and providers often use bundled payment contracts, where the insurer pays a fixed amount per capita per episode of care. The services included in the bundle, along with their unit prices, are often subject to renegotiation. In the specific case of cancer care, specialized centers often make take-it-or-leave-it offers to insurers. These contracts may become financially unsustainable for incumbent insurers following SaludCoop's exit if there is a positive demand shock from cancer patients, which may lead to their exclusion from insurers' networks in 2016.

4.2 Adverse selection

Why would the average incumbent insurer respond to SaludCoop's termination by narrowing its network? Shepard (2022) and Serna (2024a) have shown that insurers respond to adverse selection by narrowing their networks because broader networks are more attractive to sicker consumers. However, all consumers remain insured because enrollment is mandatory and because insurance with narrow networks dominates uninsurance.

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

Table 3: Distribution of Enrollment Conditional on the 2015 Insurer

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

Note: Table reports the share of individuals enrolled with Cafesalud and other insurers according to their enrollment in 2015.

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

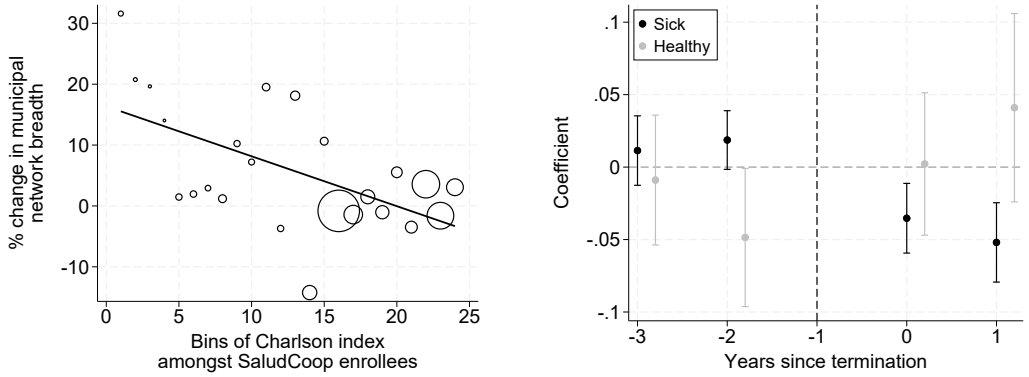
Table 4 shows how the probability of switching out of an insurer after the termination depends on its network breadth amongst individuals enrolled with SaludCoop in 2015.

Independently of whether individuals suffer from chronic health conditions, those enrolled with insurers that have broader networks are less likely to switch out, indicating their preference for broad networks.[14] However, this preference is stronger for individuals with chronic conditions, whose decision to switch out of their insurer is more sensitive to network breadth. Incumbent insurers can therefore avoid SaludCoop's enrollees with worse health status by narrowing their networks.

TABLE 4: Enrollees' Switching Decisions by Network Breadth

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

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



(A) SaludCoop Health Status (B) Provider Network Breadth

FIGURE 4: Heterogeneity of Changes in Networks by Health Status

Note: Panel A shows a scatter plot of the average percentage change in provider network breadth across insurers in treated municipalities by 25 equally-sized bins of the pre-period average Charlson Index among SaludCoop enrollees. Each circle is weighted by the number of SaludCoop enrollees. The solid line represents a linear fit. Panel B shows event study results using provider network breadth as outcome variable conditional on municipalities with above- and below-median average Charlson Index among SaludCoop enrollees in black and gray, respectively.

Finally, to gauge the responsiveness of insurers' network coverage decisions to unob-

[14]We determine whether individuals have a chronic health condition by whether their Charlson index is greater than zero.

served health status, Figure 4, Panel A shows that there is a tendency for network breadth to increase over time. However, the increase is smaller and even negative in municipalities with a relatively high average Charlson index among SaludCoop enrollees in the pre-period.

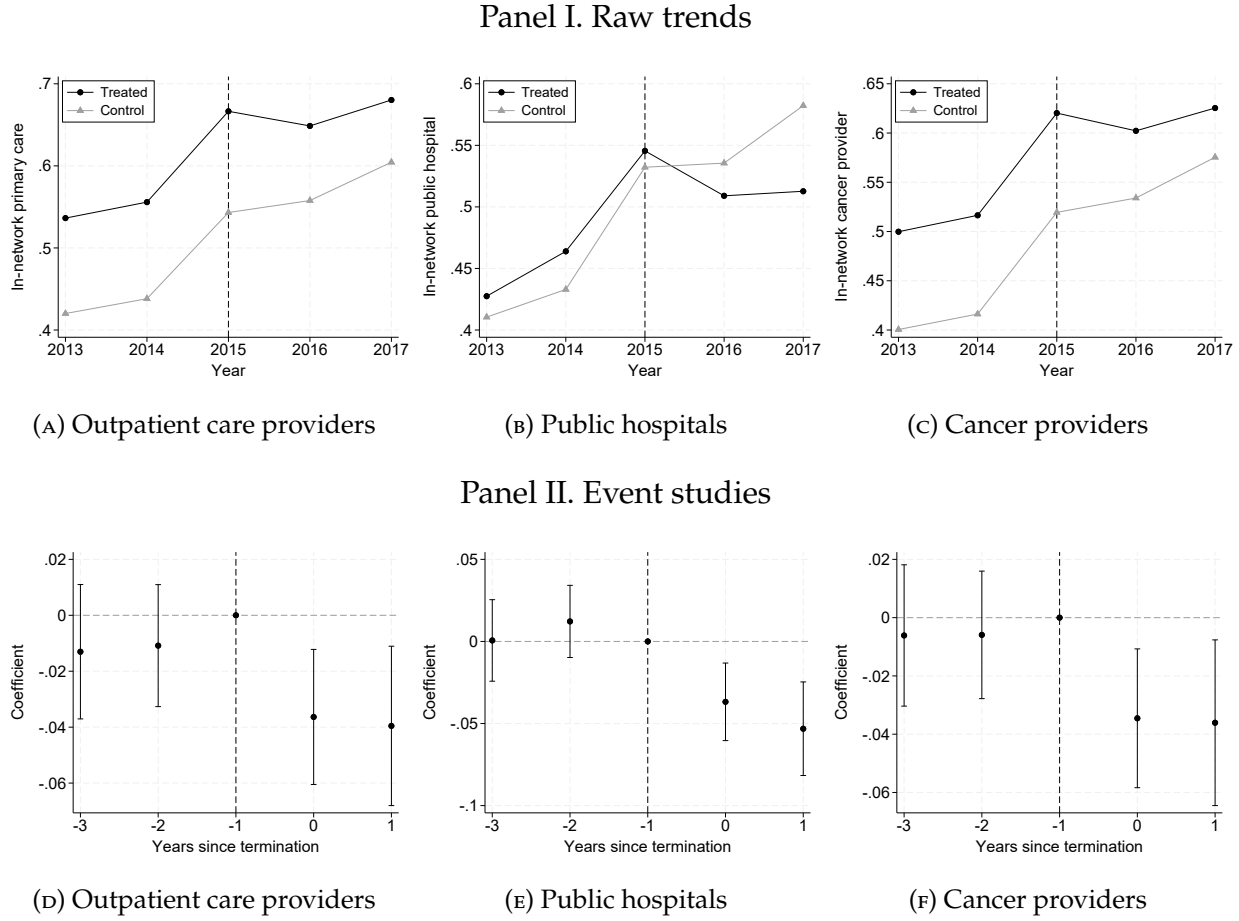
Based on this evidence, Panel B reproduces our event study on provider network breadth separately for municipalities in the treatment group where SaludCoop's enrollees had above- versus below-median average Charlson Index. In both specifications the control group is the set of municipalities where SaludCoop did not operate. We find that reductions in network breadth are only visible among the subset of municipalities where SaludCoop enrollees were relatively sicker, which suggests that insurers' strategic network coverage decisions are consistent with risk selection.

Excluded Providers. Figure 5 and Table 5 describe the characteristics of excluded providers and of the patients who visited these providers in the pre-period, respectively. In Figure 5, we detect causal reductions in the likelihood of covering outpatient care providers, public hospitals, and cancer care providers.[15] Appendix Figure 3 also shows that we cannot draw conclusions about strategic responses in the coverage of other specialties (such as cardiology, neurology, nephrology, etc.).

Whether it is profitable for insurers to exclude these types of providers depends on whether patients in poorer health are more likely to seek care from them. In Colombia, it is common for the ongoing clinical management of patients with chronic conditions—such as hypertension, diabetes, chronic kidney disease, or cancer—to be carried out in large outpatient clinics. These clinics offer a high volume of follow-up medical consultations, clinical laboratory tests, diagnostic imaging, and medication dispensing for disease control. These providers also deliver services that do not require hospitalization, such as dialysis and chemotherapy. Additionally, public hospitals are known to serve relative sicker and more expensive patients compared to private hospitals, which could explain insurers' incentives to exclude them from their networks.

Table 5 corroborates our intuition that reductions in network breadth were a strategic

[15]Providers considered in the event studies in Figure 5 are not mutually exclusive. For example, a public hospital is also a provider of cancer care and primary care.



(A) Outpatient care providers (B) Public hospitals (C) Cancer providers

(D) Outpatient care providers (E) Public hospitals (F) Cancer providers

FIGURE 5: Impacts on the Inclusion of Certain Types of Providers

Note: Panel I shows the raw average likelihood that an outpatient care provider, public hospital, and cancer care provider are in-network. Panel II shows event study coefficients and 95% confidence intervals using the same outcomes. The data are at the insurer-municipality-year level. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level. We exclude Cruz Blanca insurer and those with less that 0.005% market share in the municipality. Treatment is defined as municipalities where SaludCoop operated in 2015.

response of incumbent insurers looking to discourage the enrollment of costly individuals.[16] Patients who visited providers that were eventually excluded from the network had much higher rates of chronic diseases compared to patients who did not visit these providers. For instance, the rates of renal disease, cancer, dementia, and hepatic diseases were twice as high among these patients as among those who did not visit excluded providers. These patients were also nearly 1 million pesos ($500 of 2014) more expensive than those who visited providers that remained in-network. In general, excluded providers served a pool of patients and had total healthcare utilization and spending

[16]Insurers can narrow their networks without hurting their current market size because consumers exhibit substantial inertia. Therefore, insurers can minimize costs by dropping providers from the network for which the government's risk-adjusted transfer is inadequate.

TABLE 5: Pre-period Characteristics of Patients Who Visited Excluded Providers

Variable	Included		Excluded	
	mean	sd	mean	sd
Panel A. Patient characteristics				
Age	40.61	(23.50)	49.35	(22.70)
Total healthcare cost	1.131	(4.500)	2.136	(6.903)
Cancer	0.024	(0.153)	0.044	(0.205)
Dementia	0.004	(0.061)	0.007	(0.086)
Diabetes	0.070	(0.256)	0.124	(0.330)
CKD	0.040	(0.196)	0.066	(0.248
COPD	0.047	(0.213)	0.074	(0.261)
Hepatic diseases	0.001	(0.034)	0.003	(0.050)
AMI	0.003	(0.058)	0.008	(0.091)
Mortality rate	0.006	(0.079)	0.011	(0.102)
Panel B. Provider characteristics				
Total claims (thousand)	21.76	(205.6)	52.68	(428.9)
Total healthcare cost (million)	940.3	(6,907)	2,541	(14,888)
Number of patients	2,459	(14,773)	5,105	(31,921)

Note: Table presents the mean and standard deviation in parenthesis of pre-termination characteristics of patients who visited providers that were excluded from the network after SaludCoop's termination and of patients who did not visit these providers. Summary statistics are restricted to individuals in the contributory scheme for which health claims data exists.

between 2 to 3 times higher than in-network providers.

5 Impacts on Mortality

In this section, we quantify the impact of the termination on individual mortality. The regression of interest is:

$$y_{imt} = \sum_{\substack{k=-3 \\ k\neq -1}}^{3} \beta_k 1\{t - 2016 = k\} \times T_m + \gamma_m + \eta_t + \varepsilon_{imt}, \tag{2}$$

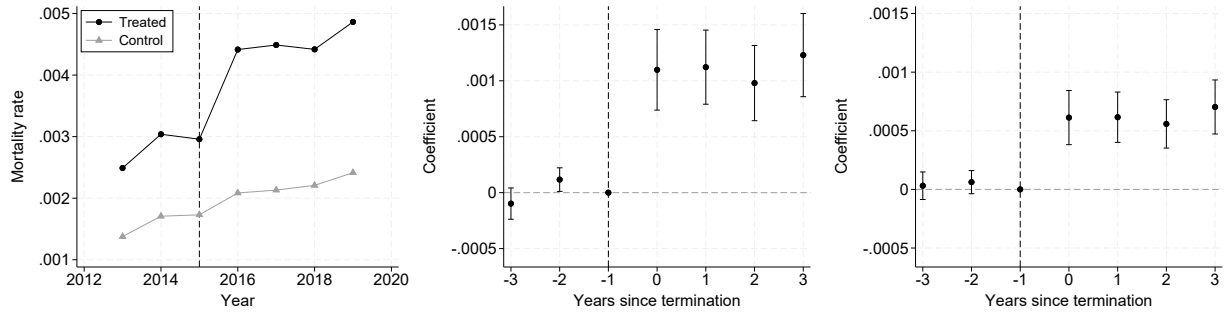
where y_{imt} takes the value of 1 if individual i who lived in municipality m died during year t and 0 otherwise, T_m is an indicator for treated municipalities, and γ_m and η_t are municipality and year fixed effects, respectively. We cluster standard errors at the municipality level.

For this analysis, treatment effects are identified based on the assumption that mortality rates in the treatment and control groups evolved in parallel before the termination.

Unobserved determinants of mortality that trend differently between the treatment and control groups might bias our estimates, as well as unobserved determinants of SaludCoop's location decisions that are correlated with individuals' health.

5.1 Non-SaludCoop Enrollees

Figure 6 presents the results using our first analysis sample in which individuals do not switch their insurer in the pre-period but are allowed to do so in the post-period. Panel A presents the average mortality rate among treated and control municipalities in the raw data, suggestive of parallel pre-termination trends. In Panel B where we implement equation (2), we find that mortality increased 1.1 per 1,000 non-SaludCoop enrollees. The change in mortality the year after the termination corresponds to a 32% increase over the counterfactual mortality rate.[17]



(A) Raw trends (B) All markets (C) w/o SaludCoop hosp

FIGURE 6: Mortality Effect on Pre-period Inertial non-SaludCoop Enrollees

Note: Figure shows event study coefficients and 95% confidence intervals using as outcome variable an indicator for individual mortality among non-SaludCoop enrollees. Panel A present the average mortality among treatment and control groups from the raw data. Panel B presents event study results using information from all markets. Panel C excludes markets with SaludCoop hospitals. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level. The sample is restricted to individuals who do not switch insurers and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud. Treatment is defined as municipalities where SaludCoop operated in 2015.

Although some of the increase in mortality is probably due to transitory disruptions in health care generated by SaludCoop's termination, we find that the effects on mortality are persistent over time: 3 years after the termination, we estimate a mortality increase

[17]Because our sample comprises individuals who age during the sample period, we calculate the appropriate counterfactual mortality rate by subtracting the did estimate from the average mortality rate in the treatment group each year of the post-period. Then, we divide the did estimate by this counterfactual mortality rate, obtaining percentage changes of 33%, 33%, 28%, and 34% from 2016 to 2019.

in treated municipalities equal to 0.9 per 1,000 non-SaludCoop enrollees. 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, Panel C shows that mortality increased permanently even in municipalities where SaludCoop did not own hospitals. In this sub-sample, we estimate an average increase in mortality equal to 26% in the post-period.

The smaller effect estimated in Panel C relative to Panel B indicates that some of the mortality effect is driven by the closure of SaludCoop hospitals. However, the fact that this effect remains relatively large suggests that they way in which incumbent insurers responded to their competitor's termination had a significant impact on patient health.

Figure 7 presents the results using our second analysis sample where we require individuals to be fully inertial throughout the sample period. Panel I uses information from consumers in both the contributory and subsidized schemes, while Panel II uses data only from those enrolled in the contributory scheme.

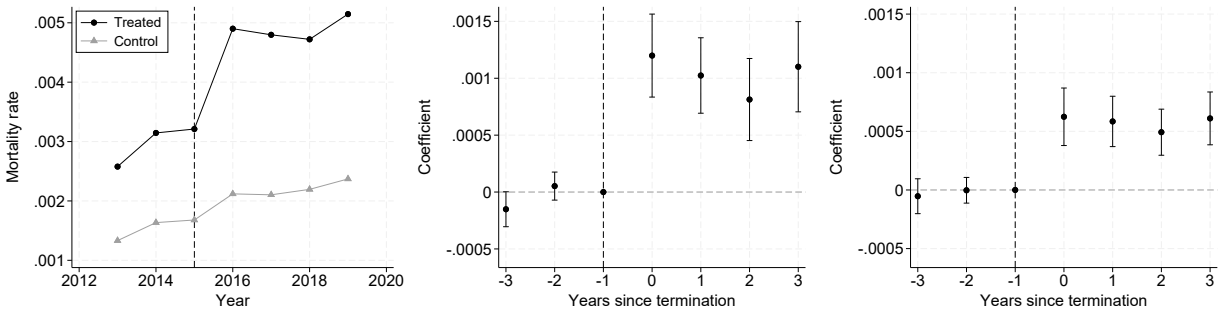
Panel A shows that average mortality rates in the raw data trended in parallel before the termination between treated and control municipalities across the country. The year of the termination, we find that mortality increased 1.2 per 1,000 non-SaludCoop enrollees, on average a 26% increase relative to the counterfactual mortality rate in the post-period as seen in Panel B.[18] Excluding municipalities where SaludCoop hospitals operated in Panel C, we also estimate a persistent 22% increase in mortality.

The impact on mortality in our second sample is similar in magnitude to our first sample because individuals do not switch to insurers who potentially become a better match for their health status given the change in their provider networks. This limited switching might be due to information frictions or switching costs that prevent consumers from re-optimizing following SaludCoop's termination.

Panel E displays event study results in the subsample of individuals covered by insurers in the contributory scheme (for which we also have provider network data). In this sample, we estimate an average increase in mortality equal to 12% throughout the post-termination period. As in the previous results, Panel F shows that increases in mortality

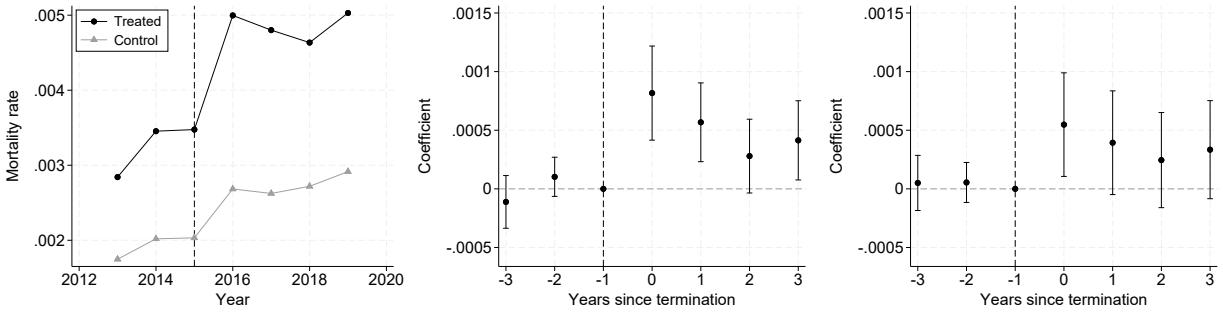
[18]The percentage change in mortality equals 32%, 27%, 21%, and 27% in each year from 2016 to 2019.

Panel I: Country



(A) Raw trends (B) All markets (C) w/o SaludCoop hosp

Panel II: Contributory scheme



(D) Raw trends (E) All markets (F) w/o SaludCoop hosp

FIGURE 7: Mortality Effect on Fully Inertial non-SaludCoop Enrollees

Note: Figure shows event study coefficients and 95% confidence intervals using as outcome variable individual mortality in the full country in Panel I and conditional on the contributory scheme in Panel II among non-SaludCoop enrollees. Panels A and D present average mortality among treatment and control groups from the raw data. Panels B and E present event study results using information from all markets. Panels C and F exclude markets with SaludCoop hospitals. Specifications include municipality and year-fixed effects. Standard errors are clustered at the municipality level. The sample is restricted to individuals who do not switch insurers throughout the sample period and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud. Treatment is defined as municipalities where SaludCoop operated in 2015.

in the contributory scheme are not necessarily due to the closure of SaludCoop hospitals. Although our estimates in this panel are noisier, we find significant increases in mortality (at a 90% confidence level) up to two years after the termination, which tend to dissipate in the last couple of years of our sample.

Some explanations for why impacts in the contributory scheme are smaller than in the rest of the country are that insurers in this scheme are more efficient and profitable than those in the subsidized scheme and that providers mostly contract with insurers in the contributory scheme leading to better service coverage.[19]

[19]Insurers in the subsidized scheme are substantially smaller and most of them are public institutions.

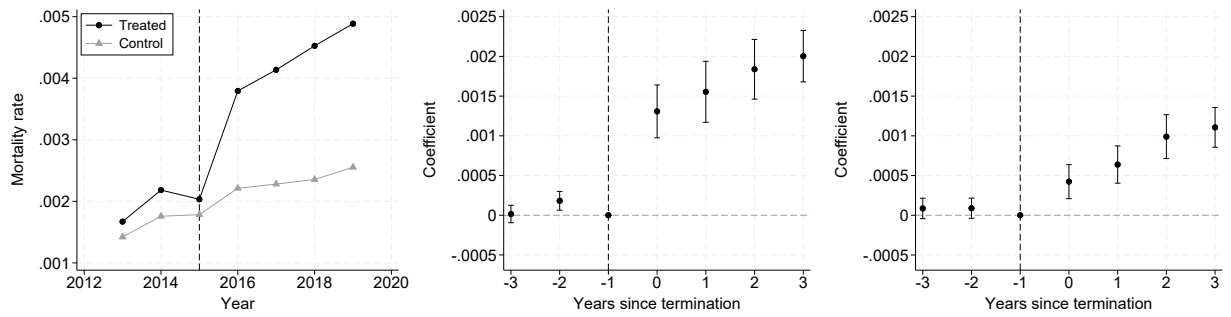
The permanent increase in mortality, even after excluding markets where SaludCoop hospitals operated, suggests that health outcomes are influenced not only by hospital capacity but also by how insurers change their provider networks. Further evidence of this comes from the fact that in our setting, individuals retain insurance coverage even when their insurer is terminated. This allows us to explore the role of insurers in shaping health outcomes, expanding on existing research which has largely focused on the impact of insurance coverage (e.g., Card et al., 2009; Finkelstein et al., 2012; Miller et al., 2021). Although there are a few studies highlighting the significance of managed care and health insurers for patient health (e.g., Abaluck et al., 2021; Aizer et al., 2007), the mechanisms driving these effects are still largely unexplored. Our unique setting and rich administrative data provides an opportunity to delve into these mechanisms.

Robustness checks. We conduct several robustness checks to our mortality specification using the first analysis sample. In Appendix Table 6 we report results using a Poisson regression to account for potential non-linearities in the evolution of mortality rates. In this case, we use a random sample of 500K individuals for ease of computation. Appendix Figure 4 reports event study results controlling for patient characteristics (sex, age, and an indicator for having low income) using De Chaisemartin and d'Haultfoeuille (2020)'s estimator.

5.2 SaludCoop Enrollees

In Figure 8 we explore the direct impact of the termination on SaludCoop's enrollees. We compare individuals enrolled with SaludCoop in municipalities where it operated against individuals in control municipalities. Therefore, our specification is the same as in equation (2) except that we condition the treatment group to the subset of individuals enrolled with SaludCoop.

Panel A reports the trend in the average mortality rate among treated and control groups. Unlike the trends observed in all the contributory scheme, we see that mortality rates among SaludCoop enrollees not only spike the year of the termination but continue to grow systematically in the post-period. In Panel B, this translates into increasing estimates



(A) Raw trends (B) All markets (C) w/o SaludCoop hosp

FIGURE 8: Mortality Effect on Pre-period Inertial SaludCoop Enrollees

Note: Figure shows event study coefficients and 95% confidence intervals using as outcome variable an indicator for individual mortality. The treatment group is restricted to individuals enrolled with SaludCoop from 2013 to 2015. The control group are those who reside in markets where SaludCoop did not operate. Panel A presents average mortality among treatment and control groups from the raw data. Panel B presents event study results using information from all markets. Panel C excludes markets with SaludCoop hospitals. Specifications include municipality and year-fixed effects. Standard errors are clustered at the municipality level. The sample is restricted to individuals who did not move across municipalities before the termination.

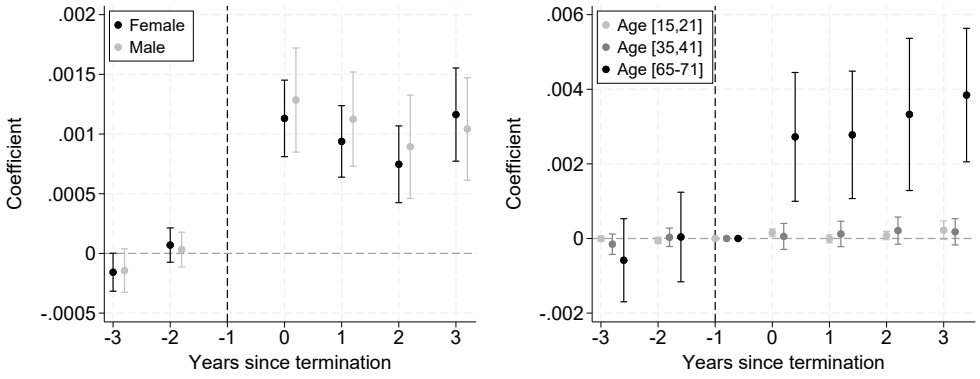
of the effect of the termination. However, the evidence is inconclusive because the parallel pre-trends assumption is not satisfied in this case.

Moving to Panel C where we exclude municipalities where SaludCoop hospitals operated, we find causal evidence that mortality rates among SaludCoop enrollees increased 24% the year of the termination relative to the counterfactual rate. This effect is nearly 2 p.p larger than the one estimated in the contributory scheme in Figure 7, Panel F, consistent with the more pronounced disruptions in care that SaludCoop enrollees experienced relative to individuals enrolled with other incumbent insurers.

5.3 Heterogeneity

In this subsection we analyze the heterogeneity in mortality effects across demographic characteristics and health status using our second estimation sample. In each exercise we estimate equation (2) conditional on individuals with certain demographic traits both in the treatment and control groups, so results can be interpreted as group-specific mortality rates.

We begin by analyzing the effect of SaludCoop's termination on the mortality of men and women separately. One reason why we might to expect see differential effects by sex is that men and women typically claim different kinds of health services which may be more



(A) Sex (B) Age

FIGURE 9: Mortality Effect by Sex and Age Among Fully Inertial non-SaludCoop Enrollees

Note: Figure shows event study coefficients and 95% confidence intervals using as outcome variable an indicator for individual mortality among non-SaludCoop enrollees. Panel A presents event study results for males and females. Panel B presents results by age group. Specifications include municipality and year-fixed effects. Standard errors are clustered at the municipality level. The sample is restricted to individuals who do not switch insurers throughout the sample period and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud. Treatment is defined as municipalities where SaludCoop operated in 2015.

or less susceptible to disruptions after the termination. For instance, women might be differentially affected if their obstetrician is excluded from the network. However, Figure 9, Panel A shows that mortality effects are similar across men and women.

In Panel B we estimate our event study specification conditional on patients in different age groups. In line with our previous evidence that excluded providers mostly served older adults and patients in worse health, we find that mortality effects are visible only among individuals aged at least 65 during the sample period. This finding highlights a fundamental trade-off in managed care systems: balancing insurers' profit maximizing incentives with providing adequate healthcare access for those who need it the most.

Next, we explore the heterogeneity in mortality effects by diagnosis. In these exercises we condition on individuals who had a Charlson Index equal to zero in 2013 to control for disease severity at the start of the sample period, but allow individuals to receive a diagnosis any point thereafter. We determine each individual's health conditions by looking at the diagnosis codes (ICD-10) that accompany each health claim, therefore our results apply only to consumers in the contributory scheme for which we have claims data.[20] We focus on the most prevalent conditions in the country to have sufficient power

[20]We construct the list of chronic diseases for each individual using the methodology for the Charlson Index.

to detect effects.

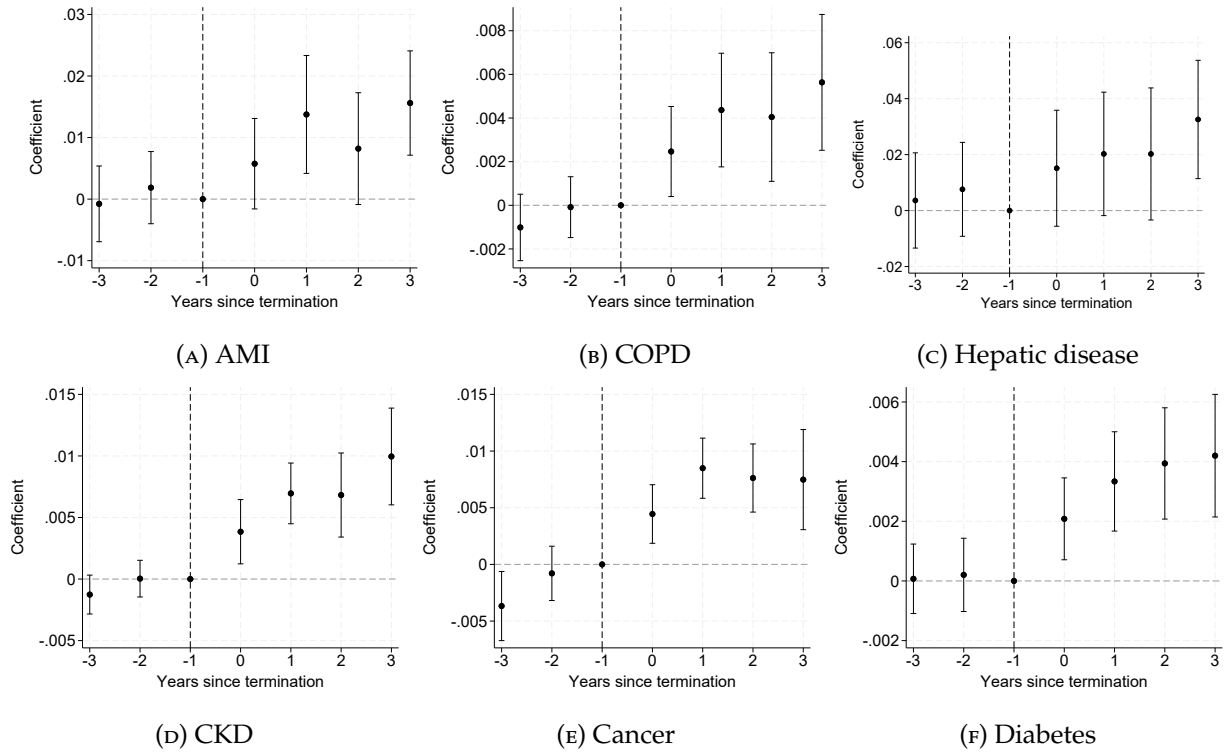


FIGURE 10: Mortality Effect by Diagnosis Among Fully Inertial non-SaludCoop Enrollees

Note: Figure shows event study coefficients and 95% confidence intervals using as outcome variable an indicator for individual mortality among non-SaludCoop enrollees conditional on being diagnosed at any point during the sample period with Acute Myocardial Infarction (AMI) in Panel A, Chronic Obstructive Pulmonary Disease (COPD) in Panel B, hepatic disease in Panel C, Chronic Kidney Disease (CKD) in Panel D, Cancer in Panel E, and Diabetes in Panel F. Sample is restricted to individuals who do not switch insurers throughout the sample period, do not move across municipalities before the termination, and who had Charlson index equal to zero in 2013. We exclude individuals enrolled with SaludCoop and Cafesalud. Treatment is defined as municipalities where SaludCoop operated in 2015.

Figure 10 shows that the increase in mortality is likely due to diseases that are more sensitive to sudden interruptions or disruptions of care, such as diabetes, renal disease, and hepatic diseases. Across these diseases we estimate an increase in mortality of about 30% on average relative to the counterfactual mortality rate.

A more direct evidence of the implications of these interruptions in care comes from patients with COPD for whom failure to receive oxygen or corticosteroids can have sudden adverse health consequences (and indeed below we find a decrease in these treatments). In the case of cancer we cannot conclude the termination has an impact on mortality rates since the parallel trend assumption might not hold.

The results by diagnosis speak to the health conditions that are most affected by

TABLE 6: Causes of Death Among Fully Inertial non-SaludCoop Enrollees

	AMI	Cancer	CKD	Diabetes	Hepatitis	Hyperten.	Gunshot	Trauma
Treated × Post	0.00020	0.00015	0.00004	0.00003	0.0000002	0.00006	0.00001	0.00006
	(0.00005)	(0.00002)	(0.00001)	(0.00001)	(0.0000005)	(0.00002)	(0.00001)	(0.00002)
Constant	0.00098	0.00043	0.00007	0.00006	0.000003	0.00024	0.00004	0.00013
	(0.00003)	(0.00001)	(0.00001)	(0.00000)	(0.0000003)	(0.00001)	(0.00001)	(0.00001)
Observations	125526626	125526626	125526626	125526626	125526626	125526626	125526626	125526626
Individuals	24769993	24769993	24769993	24769993	24769993	24769993	24769993	24769993

Note: Table presents the mean and standard deviation in parenthesis of the most common causes of death from the death certificates separately for the treatment and control groups, before and after the termination. The sample is restricted to individuals who do not switch insurers and did not move across municipalities before the termination. We condition on individuals who die and keep those enrolled with SaludCoop or Cafesalud. Treatment is defined as municipalities where SaludCoop operated in 2015.

SaludCoop's termination, but do not necessarily mean these individuals die because of their diseases. In Table 6 we estimate DID regressions using as outcomes indicator variables for cause of death that take the value of 1 if an individual died from a particular health condition and zero if they died from other causes or did not die.[21] Thus, DID coefficients can be interpreted as the change in prevalence for each cause of death.

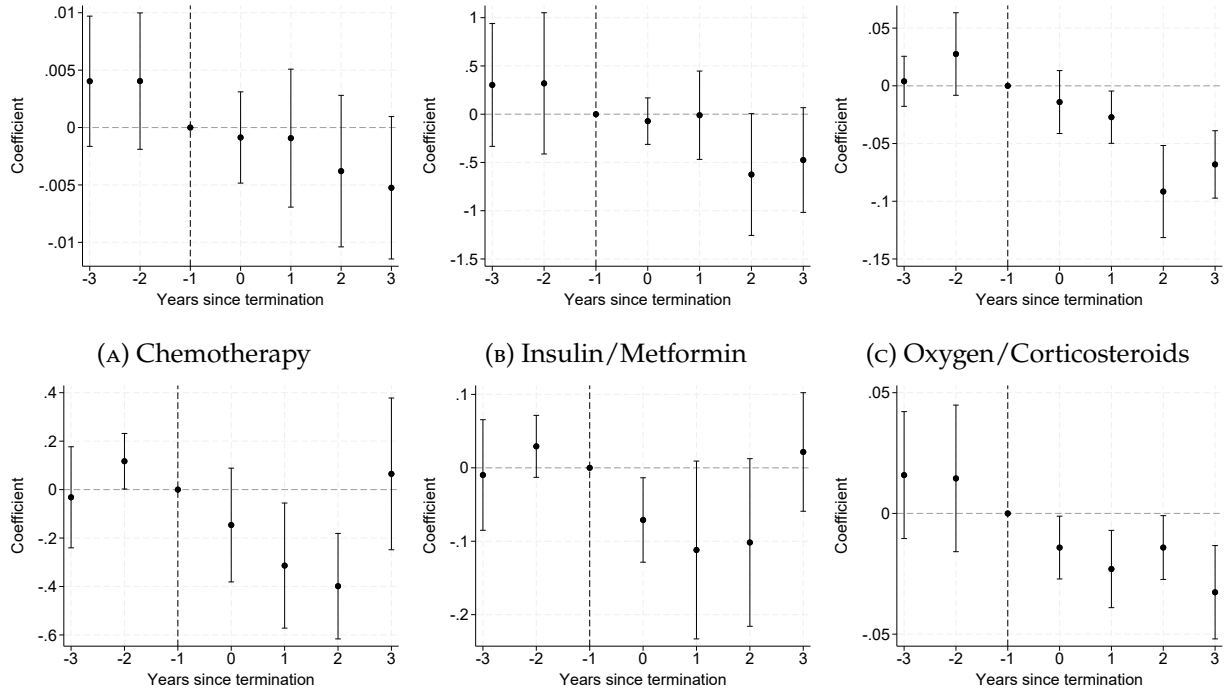
We find significant increases in mortality due to AMI, CKD, and diabetes, in line with our previous results (and with the effects on health care utilization that we report below). We also evaluate whether deaths due to gunshot wounds change after the termination as a placebo test, since we do not expect violent deaths to be necessarily affected by the termination. Indeed, we find null effects on this outcome.

5.4 Healthcare Utilization

The effects on diagnosis-specific mortality rates rest on the assumption that the termination generated interruptions in potentially life-saving treatments. To support this assumption, we quantify the impact of the termination on the following types of claims: chemotherapy among patients with cancer, insulin or metformin among patients with diabetes, oxygen and corticosteroids among patients with COPD, and renal function tests, hepatic function

[21]The death certificates report 4 string variables on cause of death. We concatenate these variables and match the following regular expressions in Spanish to create our indicator variables: "infarto|miocardio|cardiaca" for AMI, "cancer|carcinoma|neoplasia" for cancer, "renal cronica|insuficiencia renal|rinon" for CKD, "diabetes|mellitus|diabet" for diabetes, and "trauma|contundente|fractura|herida|laceracion|multiple" for trauma, "hipertension" for hypertension, and "hepatitis."

tests, and outpatient specialist consultations among patients with any chronic disease.



(A) Chemotherapy (B) Insulin/Metformin (C) Oxygen/Corticosteroids

(D) Renal function tests/Dialysis (E) Hepatic function tests (F) Specialist consultations

FIGURE 11: Impact of the Termination on Critical Healthcare Treatments

Note: Figure shows event study coefficients and 95% confidence intervals using as outcome variables the number of chemotherapy claims among patients with cancer in Panel A, the number of insulin or metformin claims among patients with diabetes in Panel B, an indicator for oxygen and corticosteroid claims among patients with COPD in Panel C, and the number of renal function tests, hepatic function tests, and outpatient specialist consultations among patients with any chronic disease in Panels D to F, respectively. We identify patients with chronic diseases as those with Charlson Index greater than zero. Sample is restricted to individuals who do not switch insurers throughout the sample period and do not move across municipalities before the termination. We allow individuals to receive a diagnosis throughout the sample period. We exclude individuals enrolled with SaludCoop and Cafesalud. Treatment is defined as municipalities where SaludCoop operated in 2015.

Appendix Table 7 lists the procedure codes for these diagnostic tests provided to us by clinicians at the National University of Colombia. In these estimations we use health claim information from individuals enrolled in the contributory scheme for whom the data exists.

Figure 11 shows significant reductions in the majority of these treatments, consistent with the increases in mortality reported in the previous subsection. In Panel A, although our estimates are noisy, there is also evidence of a decline in the number of chemotherapy claims after the termination.

6 Mechanisms

Why would mortality increase among non-SaludCoop enrollees in municipalities where SaludCoop did not own hospitals? And why would this increase be permanent? There may be several explanations for this. SaludCoop was a relatively high-quality insurer whose termination forced individuals to revert to potentially lower-quality incumbents. This type of mean quality reversion can lead to worsening of health outcomes as seen in Abaluck et al. (2021). However, this can not be an explanation for mortality increases among the group of enrollees who did not switch their insurer over the sample period.

Another explanation is incumbent insurers' strategic reductions in provider network breadth. Several studies in the US show that provider closures or forced switches of provider are associated with worse health outcomes (e.g., Schleicher et al., 2016; Sabety, 2023; Politzer, 2021), but there is no evidence to date of whether provider exclusions that are the result of strategic insurer responses matter for patient health.

Provider network exclusions may affect mortality by interrupting essential services for individuals managing chronic health conditions. And even if insurers do not change their networks after the termination, they may experience congestion from the surge of new enrollees. Congestion externalities can also disrupt care, potentially leading to higher mortality.

Finally, SaludCoop's termination and the closure of its hospitals may have impacted the structure of health insurance and health care labor markets. Serna (2024b) shows that the use of contracts that place the financial risk on the insurer, such as fee-for-service, causally increased after the termination, particularly in markets where insurer concentration was predicted to be small relative to provider concentration. If the use of fee-for-service contracts is associated with the provision of low-value care, then it is possible that mortality effects are driven by this choice of contracts.

Moreover, if SaludCoop's termination resulted in doctors being laid off, delaying their employment, moving to different locations, or retiring altogether, we might expect to see changes in mortality due to the lower availability of nurses and physicians or to the change in the health care labor-capital ratio. In this section, we explore the extent to which each

of these factors may explain the mortality increase after SaludCoop's termination.

We start with provider network breadth. To do so, we investigate the heterogeneity in mortality effects by whether patients visited providers in the pre-period that were eventually excluded from the network. Figure 13, Panel A presents two sets of results for our event study specification.[22] The estimates in light gray and black compare the control group against non-SaludCoop enrollees in treated municipalities who had a below- and an above-average share of pre-period claims at providers that were dropped from the network after SaludCoop's termination, respectively.

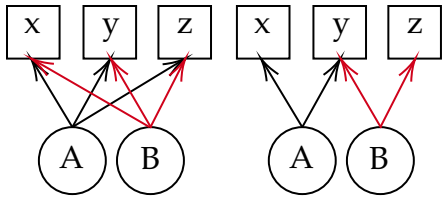
The results show evidence of provider network exclusions contributing to the mortality effects. We find no significant changes in mortality for consumers with a relatively small share of pre-period claims at providers that were dropped from the network. But, we estimate substantial increases in mortality throughout the post-period for their counterparts. Appendix Figure 5 reproduces all the results excluding municipalities where SaludCoop hospitals operated.

Next, to test whether congestion externalities factor into the mortality effects, we first need to derive an appropriate measure of congestion that does not conflate endogenous changes in networks that happen after the termination. To do this, consider the toy example in Figure 12. There are two insurers $\{A, B\}$ and three providers $\{x, y, z\}$.

In Panel A both insurers have *complete* provider networks. If insurer B is terminated, its enrollees will switch towards A, but in-network providers in A's network will treat the same number patients after the termination as they did before the termination because A has complete network overlap with B. Therefore, holding everything else fixed, we should not expect to see much congestion in A's network nor significant changes in mortality.

In Panel B insurers have *incomplete* provider networks. Insurer A covers providers $\{x, y\}$ and insurer B covers providers $\{y, z\}$, so network overlap equals $1/2$. If B is terminated and its enrollees switch to A, providers $\{x, y\}$ will treat the patients that were previously treated by $\{z\}$, creating a "congestion effect" at $\{x, y\}$.

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



(A) No congestion (B) Congestion

FIGURE 12: Capturing Congestion Through Network Overlap

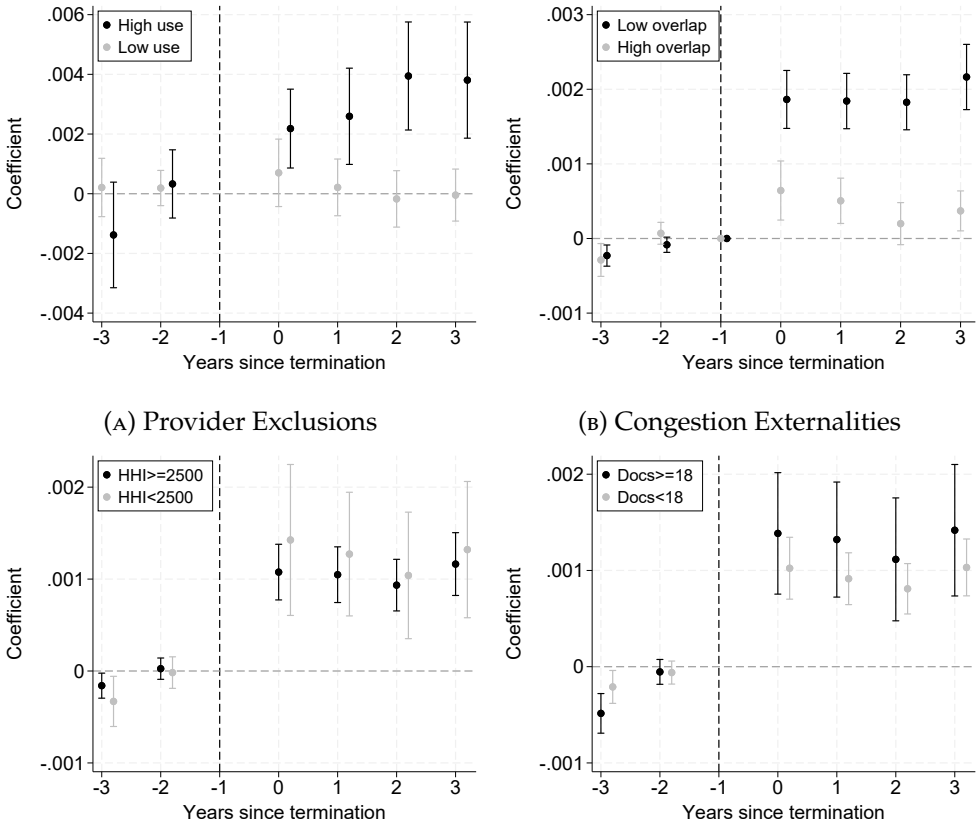
Note: Figure shows a hypothetical scenario with three hospitals x, y, z, two insurers A and B, and their network inclusions. Panel A shows a situation where B's termination does not generate congestion effects. Panels B shows a situation where B's termination would lead to a congestion in A's network.

This example illustrates that one way to measure congestion is through the pre-period network overlap between each incumbent insurer and SaludCoop. We implement this analysis in Figure 13, Panel B, which explores the heterogeneity in mortality effects by insurers in treated municipalities with above- or below-median network overlap with SaludCoop.[23] We find that mortality effects are significantly larger when overlap is relatively low, in line with our intuition.

Our findings evidence that congestion generated by incomplete provider networks is another mechanism for changes in health outcomes. However, a comparison of the magnitude of the effect between Panels A and B suggests the direct impact of provider network exclusions for patients who use these providers is twice as large as the indirect effect on other patients caused by congestion externalities.

In Figure 13, Panel C we explore whether predicted changes in insurer market concentration—which generate changes in the types of contracts established between insurers and providers—contribute to the mortality increase. We use predicted insurer market shares assuming SaludCoop's enrollees are assigned to each insurer in proportion to their 2014 market share, to construct the Herfindahl-Hirschman Index (HHI). Splitting our sample by municipalities with insurer HHI above or below 2,500, we find no evidence of heterogeneous mortality effects. This suggests that shifts toward fee-for-service contracts brought by the termination and the incentives these contracts create are not large enough to explain our mortality results.

[23] We construct network overlap for each insurer and municipality as the fraction of SaludCoop's in-network providers (denominator) that were also in the network of the incumbent insurer during 2015 (numerator).



(A) Provider Exclusions

(B) Congestion Externalities

(C) Predicted Insurer HHI

(D) Doctors per 1,000

FIGURE 13: Heterogeneity in Mortality Effect Among Fully Inertial Non-SaludCoop Enrollees

Note: Figure shows coefficients and 95% confidence intervals using as outcome variable individual mortality. In all specifications, the control group are municipalities where SaludCoop did not operate. Panel A, presents results for individuals in the treated group that had a below- and above-average share of pre-period claims delivered at providers that were dropped from the network in the post-period. Panel B shows results using individuals enrolled with insurers in the treated group that had above- or below-median network overlap with SaludCoop in 2015. Panel C displays results for municipalities in the treated group that had predicted insurer HHI below and above 2,500. Panel D presents results by municipalities in the treated group where the number of doctors per 1,000 enrollees was below and above 18. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level.

Finally, Panel D examines baseline health care labor market thickness as a mechanism for patient mortality. Here too we find no evidence of heterogeneous treatment effects by whether municipalities had above- or below-average number of doctors (physicians plus nurses) per 1,000 enrollees at baseline.

7 Conclusion

In studying the factors that contribute to the production of health, most research has focused on the impact of health insurance coverage and place-based drivers of mortality

(e.g., Miller et al., 2021; Finkelstein et al., 2021). Building on this literature, in this paper we study the role of health insurers in shaping patient health. We use data from the Colombian health care system, where the largest health insurer was abruptly terminated by the government in December 2015. We show how incumbent insurers change the characteristics of their health plan, namely their provider networks, in response to the exit of a competitor and how this exit affected mortality among other insurers' patients.

Using a difference-in-differences design, we find that provider networks among incumbent insurers in treated markets (those where the terminated insurer operated) became 10% narrower after the termination relative to insurers in control markets. Incumbent insurers responded by narrowing their networks to avoid the potentially sick, unprofitable enrollees from the terminated insurer. We also find that individual mortality increased 22% among fully inertial patients enrolled with these incumbent insurers. In examining the mechanisms that may explain the rise in mortality, we find that provider network exclusions as well as congestion externalities—caused by narrow networks and the influx of new enrollees following the termination—, are the main explanations of the observed mortality effects. The important negative health effects following the exit of a major insurer reinforce the idea that insurers should be tightly regulated—at least in a managed care setting where they shape enrollees' health.

The two mechanisms that we examine point to the importance of broad provider networks on the production of health. In this setting, regulations such as network adequacy standards, which require insurers to meet minimum provider-to-enrollee ratios, minimum distance from enrollee population centroids to nearest providers, or to cover specific providers, may be beneficial for consumers. The implementation of these rules is currently debated in health care systems such as those in the US (Centers for Medicaid and Medicare Services, 2023; National Conference of State Legislatures, 2023) where the problem of narrow networks is particularly stark. Our results suggest that these ultra-narrow networks may have detrimental effects on health.

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Online Appendix

Appendix A Data Details

Appendix Table 1: Match Rates between Enrollment Files and Death Certificates

Year	Obs	Enrollment obs matched to death records	Death record obs matched to enrollment	Appended death records to enrollment
	(1)	(2)	(3)	(4)
Full sample				
2013	44,221,359	0.27	80.00	0.07
2014	44,429,334	0.30	67.22	0.14
2015	43,760,133	0.30	66.31	0.15
2016	44,053,138	0.29	63.91	0.17
2017	43,735,736	0.28	54.15	0.24
2018	44,268,562	0.27	52.16	0.25
2019	49,906,928	0.31	60.06	0.20
Treated				
2013	37,808,706	0.29	80.58	0.07
2014	38,108,170	0.32	67.25	0.15
2015	37,494,941	0.33	66.35	0.16
2016	37,882,350	0.31	63.56	0.18
2017	37,718,454	0.30	54.27	0.25
2018	38,208,301	0.29	52.05	0.27
2019	43,579,111	0.32	60.14	0.21
Control				
2013	6,267,204	0.15	77.94	0.04
2014	6,171,204	0.17	66.91	0.08
2015	6,095,810	0.18	65.91	0.09
2016	6,131,812	0.16	63.42	0.09
2017	5,982,197	0.16	52.78	0.14
2018	6,025,136	0.17	53.37	0.14
2019	6,294,866	0.18	59.05	0.13

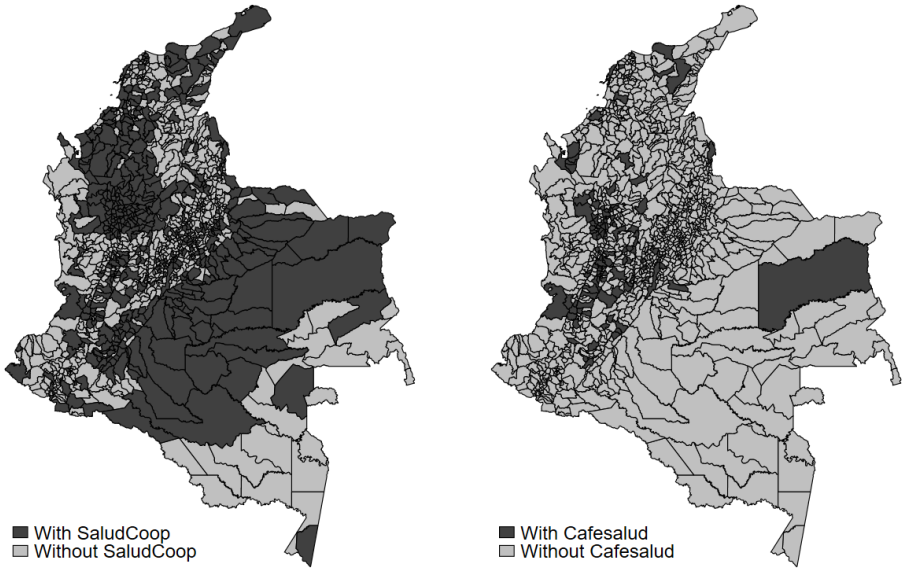
Note: Column (1) reports the total number of observations (individuals) in the enrollment files. Column (2) reports the percentage of observations in the enrollment files that match the death certificates. Column (3) reports the percentage of observations in the death certificates that match the enrollment files. Column (4) shows the percentage of observations in the enrollment files that are appended from the unmatched death certificates.

Appendix B Descriptives

Appendix Table 2: Sample Restrictions

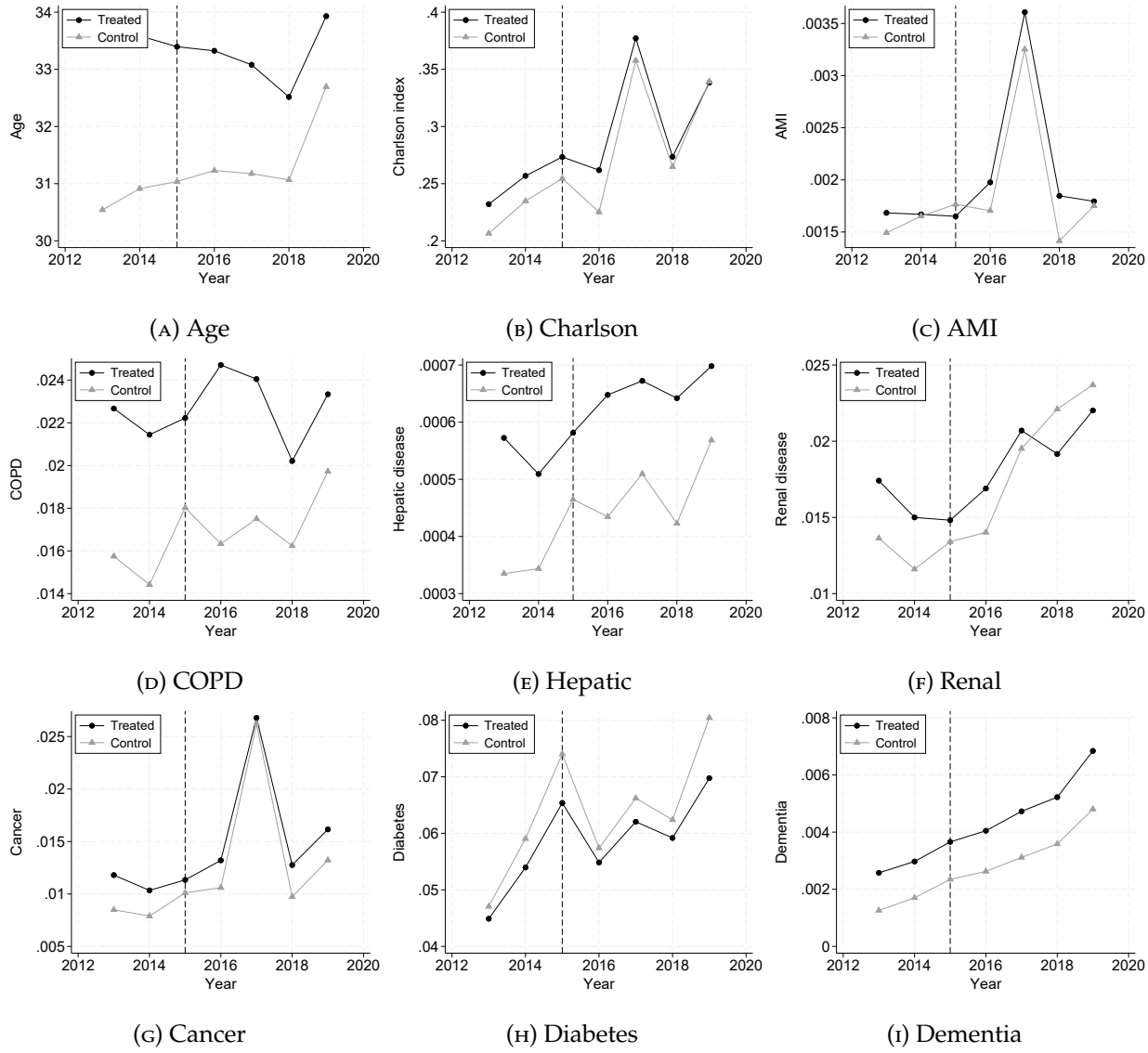
Sample Restriction	Sample 1	Sample 2
Full sample (all years)	66,498,109	66,498,109
Exclude SaludCoop and Cafesalud	—	56,934,602
Exclude enrollment after death	58,814,688	49,755,433
No insurer switching in pre-period	45,731,874	—
No insurer switching in sample period	—	33,772,092
No moving across municipalities in pre-period	43,993,552	31,262,574
Balanced panel of insurer-municipalities	43,929,132	28,295,722
Exclude special insurers + ages above 95 years	36,937,355	24,788,119

Note: Table reports the number of individuals left in each of our analysis samples 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.



(A) Age (B) Charlson (C) AMI

(D) COPD (E) Hepatic (F) Renal

(G) Cancer (H) Diabetes (I) Dementia

APPENDIX FIGURE 2: Trends in Individual Characteristics In Sample With Switches

Note: Figure shows raw average characteristics in treated and control municipalities over time. Data are at the individual-year level and are collapsed at the treatment status-year level. The sample of enrollees is restricted to those who never switched their insurer and never moved across municipalities before the termination, and who were enrolled with insurers other than SaludCoop and Cafesalud.

Appendix Table 3: Summary Statistics Excluding Markets With SaludCoop Hospitals Among Pre-period Inertial non-SaludCoop Enrollees

Variable	Treated		Control	
	Pre	Post	Pre	Post
Mortality rate	0.002	0.003	0.002	0.002
	(0.043)	(0.055)	(0.040)	(0.047)
Charlson Index*	0.138	0.166	0.114	0.155
	(0.569)	(0.652)	(0.516)	(0.625)
Male	0.478	0.475	0.495	0.490
	(0.500)	(0.499)	(0.500)	(0.500)
Age	31.41	31.23	30.83	31.53
	(21.71)	(22.20)	(22.04)	(22.66)
Low income	0.093	0.063	0.112	0.081
	(0.291)	(0.243)	(0.315)	(0.272)
AMI*	0.002	0.002	0.002	0.002
	(0.040)	(0.042)	(0.040)	(0.045)
COPD*	0.023	0.022	0.016	0.017
	(0.148)	(0.147)	(0.126)	(0.131)
Hepatic disease*	0.000	0.001	0.000	0.000
	(0.021)	(0.023)	(0.020)	(0.022)
Renal disease*	0.016	0.020	0.013	0.020
	(0.126)	(0.141)	(0.113)	(0.139)
Cancer*	0.010	0.014	0.009	0.015
	(0.099)	(0.119)	(0.094)	(0.121)
Individuals	8,717,859	10,120,939	3,937,632	4,104,362
Municipalities	452	452	626	626
Individuals x Years	21,810,409	31,760,084	9,873,670	13,050,775

Note: Table presents the mean and standard deviation in parenthesis of the sample of enrollees for the mortality analysis among non-SaludCoop enrollees excluding municipalities with SaludCoop hospitals. Summary statistics are presented separately for individuals living in treated and control municipalities, in the pre- and post-termination periods. An observation is an individual-year and the data are from 2013 to 2019. The sample of enrollees is restricted to those who never switched their insurer and never moved across municipalities before the termination, and who were enrolled with insurers other than SaludCoop and Cafesalud. Our final sample of enrollees does not constitute a fixed cohort. AMI stands for acute myocardial infarction and COPD for chronic obstructive pulmonary disorder. (*) Because the health claims data exists only for individuals in the contributory scheme enrolled with insurers that passed the Ministry of Health's data quality filters, summary statistics of commordities are conditional on these individuals.

Appendix Table 4: Summary Statistics Among Fully Inertial non-SaludCoop Enrollees

Variable	Treated		Control	
	Pre	Post	Pre	Post
Mortality	0.003	0.005	0.002	0.002
	(0.055)	(0.070)	(0.039)	(0.047)
Charlson Index*	0.254	0.312	0.232	0.297
	(0.786)	(0.913)	(0.712)	(0.839)
Male	0.466	0.462	0.490	0.484
	(0.499)	(0.499)	(0.500)	(0.500)
Age	34.40	35.40	31.50	32.68
	(22.42)	(23.00)	(22.56)	(23.18)
Low income	0.090	0.070	0.117	0.088
	(0.286)	(0.256)	(0.321)	(0.283)
AMI*	0.003	0.004	0.003	0.004
	(0.054)	(0.063)	(0.058)	(0.062)
COPD*	0.039	0.039	0.033	0.033
	(0.194)	(0.194)	(0.177)	(0.178)
Hepatic disease*	0.001	0.001	0.001	0.001
	(0.031)	(0.034)	(0.028)	(0.031)
Renal disease*	0.027	0.033	0.026	0.037
	(0.162)	(0.179)	(0.158)	(0.189)
Cancer*	0.019	0.030	0.018	0.029
	(0.138)	(0.170)	(0.132)	(0.167)
Individuals	17,232,780	18,164,555	3,226,028	3,310,372
Municipalities	482	482	624	624
Individuals x Years	44,576,996	62,010,285	8,156,334	10,998,400

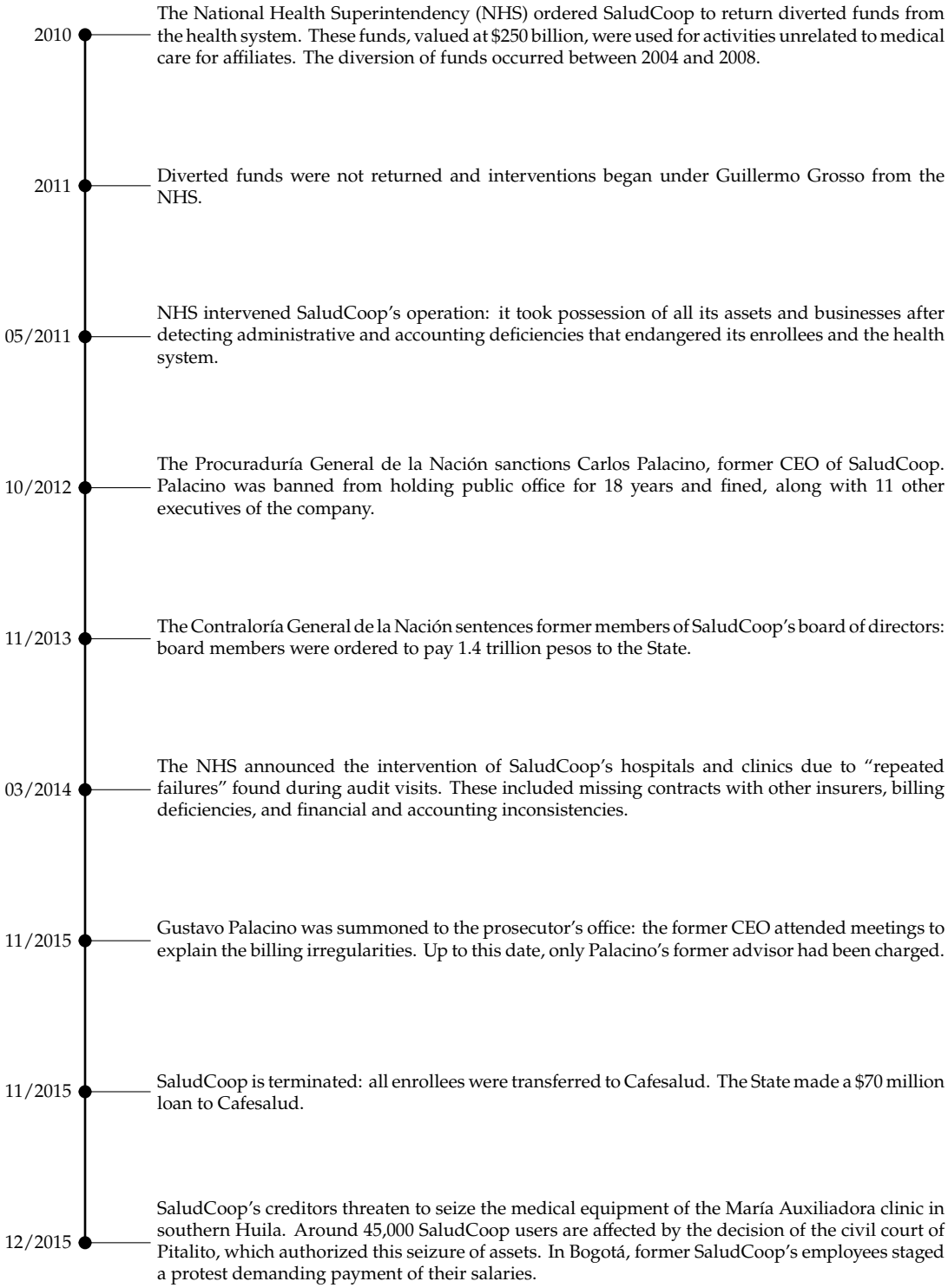
Note: Table presents the mean and standard deviation in parenthesis of the sample of enrollees for the mortality analysis among non-SaludCoop enrollees. Summary statistics are presented separately for individuals living in treated and control municipalities, in the pre- and post-termination periods. Treatment is defined as municipalities where SaludCoop operated in 2015. An observation is an individual-year and the data are from 2013 to 2019. The sample of enrollees is restricted to those who never switched their insurer during the sample period, who never moved across municipalities before the termination, and who were enrolled with insurers other than SaludCoop and Cafesalud. Our final sample of enrollees does not constitute a fixed cohort. AMI stands for acute myocardial infarction and COPD for chronic obstructive pulmonary disorder. (*) Because the health claims data exists only for individuals in the contributory scheme enrolled with insurers that passed the Ministry of Health's data quality filters, summary statistics of commordities are conditional on these individuals.

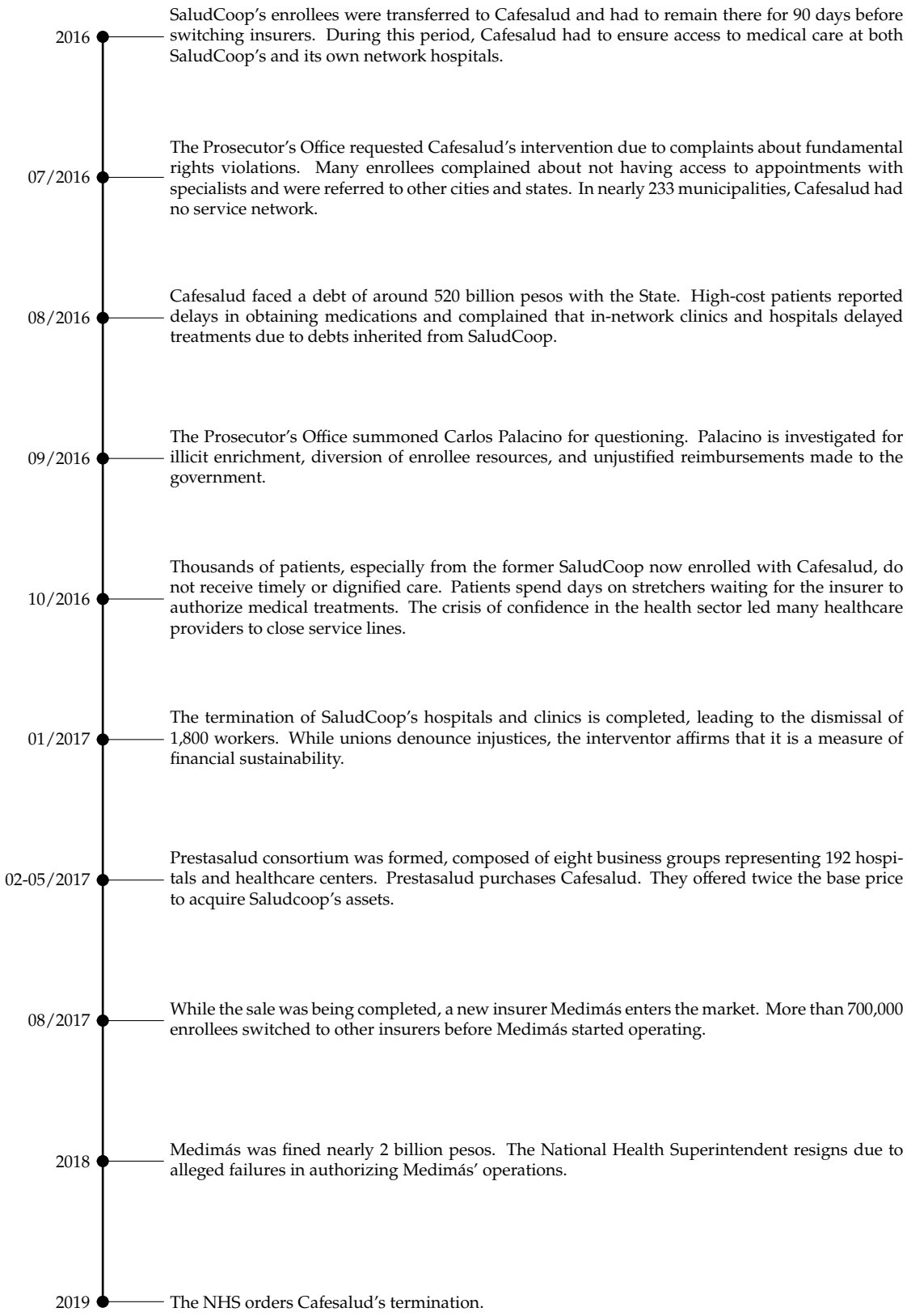
Appendix Table 5: Summary Statistics Among Pre-period Inertial SaludCoop Enrollees and Rest

Variable	Treated		Control	
	Pre	Post	Pre	Post
Mortality rate	0.002	0.004	0.002	0.002
	(0.044)	(0.065)	(0.041)	(0.048)
Male	0.484	0.477	0.495	0.490
	(0.500)	(0.499)	(0.500)	(0.500)
Age	32.52	37.97	31.08	31.95
	(20.61)	(20.53)	(22.08)	(22.71)
Low income	0.064	0.057	0.112	0.080
	(0.245)	(0.233)	(0.315)	(0.272)
Individuals	3,081,622	2,546,421	4,272,058	4,543,439
Municipalities	469	469	626	626
Individuals x Years	8,091,327	8,779,500	10,747,973	14,349,919

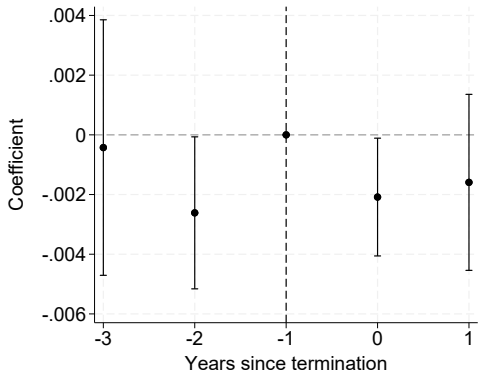
Note: Table presents the mean and standard deviation in parenthesis of the sample of enrollees for the mortality analysis. Summary statistics are presented separately for individuals living in treated and control municipalities, in the pre- and post-termination periods. Treatment is defined as municipalities where SaludCoop operated in 2015. An observation is an individual-year and the data are from 2013 to 2019. Individuals in the treatment group are restricted to those who were enrolled with SaludCoop from 2013 to 2015. Our final sample of enrollees does not constitute a fixed cohort.

Appendix C Timeline of SaludCoop's termination




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 D Additional Results



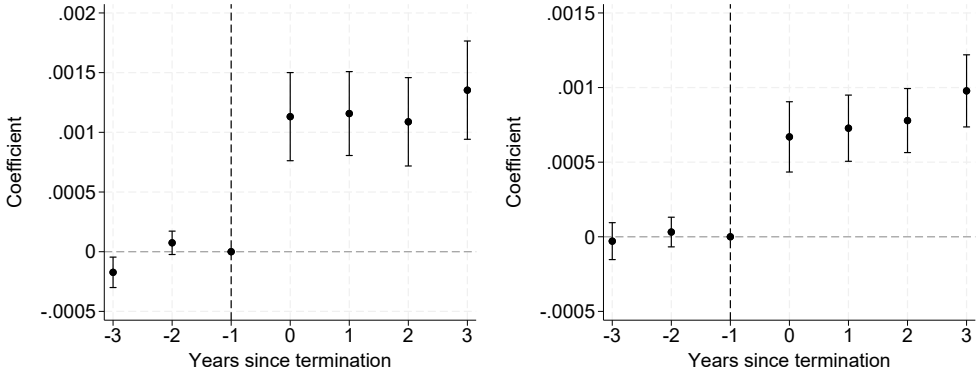
Appendix Figure 3: Impacts on the Inclusion of Other Specialties to the Network

Note: Figure shows event study coefficients and 95% confidence intervals using as outcome an indicator for covering specialty care (cardiology, neurology, nephrology, OB/GYN, dermatology, ophthalmology). The data are at the insurer-municipality-year level. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level. We exclude Cruz Blanca insurer and those with less that 0.005% market share in the municipality. Treatment is defined as municipalities where SaludCoop operated in 2015.

Appendix Table 6: Mortality Effect Among Pre-period Inertial non-SaludCoop Enrollees using Poisson Regression

Relative time	Main	No SaludCoop hosp
-3	0.250	0.108
	(0.180)	(0.186)
-2	-0.0630	-0.119
	(0.155)	(0.180)
0	0.361	0.353
	(0.174)	(0.189)
+1	0.147	0.0441
	(0.161)	(0.177)
+2	0.266	0.249
	(0.156)	(0.177)
+3	0.241	0.224
	(0.160)	(0.164)
Constant	-6.017	-5.964
	(0.171)	(0.178)
Observations	2,445,338	1,213,077
Individuals	495,958	263,179

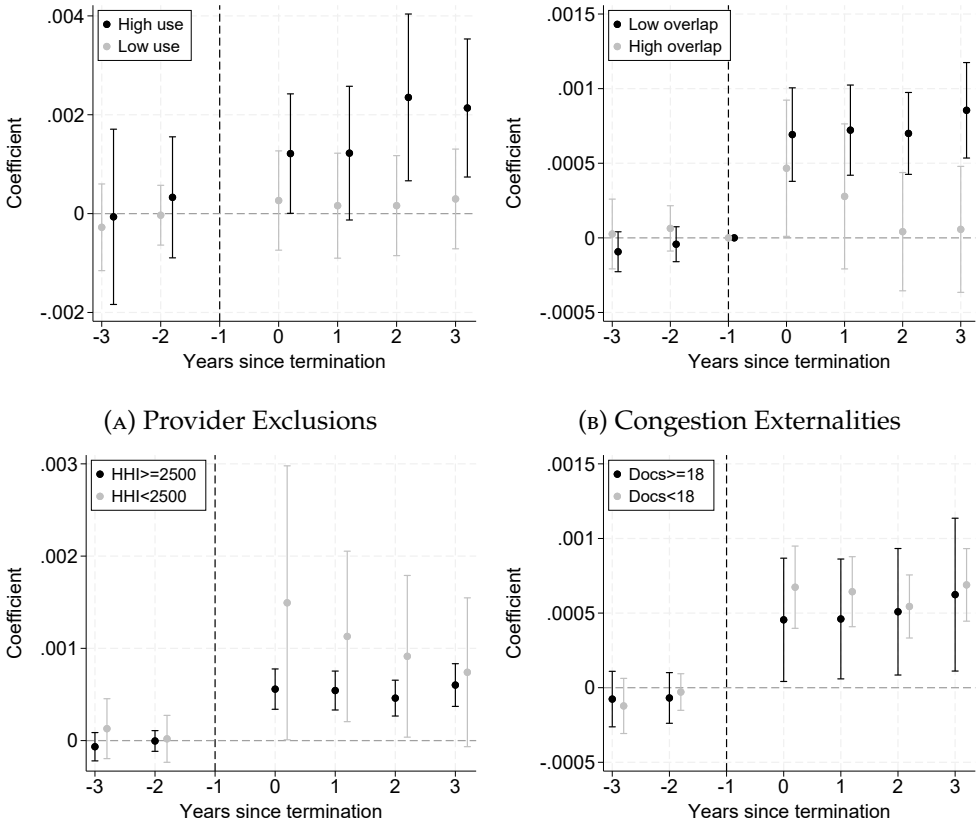
Note: Table reports results of a Poisson regression of individual mortality using the full sample of markets and excluding markets where SaludCoop hospitals operated. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level. The sample is restricted to individuals who do not switch insurers and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud. We use a random sample of 500,000 individuals for ease of computation.



(a) All markets (b) w/o SaludCoop hosp

Appendix Figure 4: Mortality Effect Among Pre-Period Inertial non-SaludCoop Enrollees Controlling for Patient Characteristics

Note: Figure shows event study coefficients and 95% confidence intervals using as outcome variable an indicator for individual mortality among non-SaludCoop enrollees. Panel A presents results using information from all markets. Panel B excludes markets with SaludCoop hospitals. Specifications include municipality and year fixed effects and control for patient characteristics (sex age, and an indicator for having low income). Estimation uses De Chaisemartin and d'Haultfoeuille (2020)'s estimator. The sample is restricted to individuals who do not switch insurers and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud. Treatment is defined as municipalities where SaludCoop operated in 2015.



(A) Provider Exclusions

(B) Congestion Externalities

(C) Predicted Insurer HHI

(D) Doctors per 1,000

Appendix Figure 5: Heterogeneity in Mortality Effect Among Fully Inertial Non-SaludCoop Enrollees in Markets Without SaludCoop Hospitals

Note: Figure shows coefficients and 95% confidence intervals using as outcome variable individual mortality. In all specifications, the control group are municipalities where SaludCoop did not operate. We exclude markets with SaludCoop hospitals. Panel A, presents results for individuals in the treated group that had a below- and above-average share of pre-period claims delivered at providers that were dropped from the network in the post-period. Panel B shows results using individuals enrolled with insurers in the treated group that had above- or below-median network overlap with SaludCoop in 2015. Panel C displays results for municipalities in the treated group that had predicted insurer HHI below and above 2,500. Panel D presents results by municipalities in the treated group where the number of doctors per 1,000 enrollees was below and above 18. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level.

Appendix E List of Diagnostic Tests

Appendix Table 7: Codes for Diagnostic Tests from the National Insurance Plan

Category	Test	Code 2009	Code 2017	Code 2018	Code 2020
Hepatic function	Alanine Aminotransferase (ALT/TGP)	903866	903866	903866	903866
	Aspartate Aminotransferase (AST/TGO)	903867	903867	903867	903867
	Total Bilirubin	903810	903810	903810	903810
	Conjugated and unconjugated Bilirubin	903811	903811	903811	903811
	Serum Albumin	903803	903803	903803	903803
	Alkaline Phosphatase	903837	903837	903837	903837
	Gamma-Glutamyl Transferase (GGT)	903838	903838	903838	903838
	Prothrombin Time (TP/INR)	902045	902045	902045	902045
	Lactate Dehydrogenase (LDH)	903851	903851	903851	903851
Renal function	Serum Creatinine	903825	903895	903895	903895
	Blood Urea Nitrogen (BUN)	903856	903856	903856	903856
	Urinalysis with Sediment and Specific Gravity	907106	907106	907106	907106
	Creatinine Clearance	903823	903823	903823	903823
	24-Hour Urine Microalbuminuria	903854	903854	903854	903854
	Urine Albumin-to-Creatinine Ratio	NA	907111	907111	907111
Dialysis	Venous Catheterization for Renal Dialysis - SOD	389500	389500	389500	389500
	Creation of Peripheral AV Fistula for Renal Dialysis	392701	392701	392701	392701
	Creation of Peripheral AV Fistula for Renal Dialysis with Graft	392702	392702	392702	392702
	Revision of Arteriovenous (AV) Shunt/Fistula for Renal Dialysis - SOD	394200	394200	394200	394200
	Removal of Arteriovenous (AV) Shunt for Renal Dialysis - SOD	394300	394300	394300	394300
	Standard Hemodialysis with Bicarbonate	395001	395001	395001	395001
	Insertion of Permanent Catheter for Hemodialysis	549002	549002	549002	549002
	Placement of Peritoneal Dialysis Catheter via Open Surgery	549001	549001	549001	549001
	Placement of Peritoneal Dialysis Catheter via Percutaneous Approach	549005	549005	549005	549005
	Placement of Peritoneal Dialysis Catheter via Laparoscopic Approach	549006	549006	549006	549006
	Removal of Permanent Hemodialysis Catheter	549012	549012	549012	549012
	Removal of Other Peritoneal Catheter	549013	549013	549013	549013
	Manual Peritoneal Dialysis	549801	549801	549801	549801
	Automated Peritoneal Dialysis	549802	549802	549802	549802
	Peritoneal Dialysis Room Charges	NA	7DS001	7DS001	7DS001
	Hemodialysis Room Charges	NA	7DS002	7DS002	7DS002
	Venous Catheterization for Renal Dialysis - SOD	305900	305900	305900	305900

Note: Table reports procedure codes from the national insurance plan for several types of services.

Appendix F Event Study Coefficients

Appendix Table 8: Impact on Provider Network Breadth

Relative time	Main	No SaludCoop hosp
-3	0.00781	-0.000284
	(0.0117)	(0.0121)
-2	0.00697	0.00101
	(0.0103)	(0.0107)
0	-0.0294	-0.0322
	(0.0119)	(0.0125)
+1	-0.0387	-0.0336
	(0.0140)	(0.0146)
Constant	0.480	0.486
	(0.00325)	(0.00322)
Observations	19612	18788

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

Appendix Table 9: Impact on the Inclusion of Types of Providers

Relative time	Public hospital	Cancer	Primary care
-3	0.000610	-0.00612	-0.0130
	(0.0127)	(0.0124)	(0.0122)
-2	0.0122	-0.00591	-0.0108
	(0.0112)	(0.0112)	(0.0111)
0	-0.0368	-0.0345	-0.0364
	(0.0120)	(0.0121)	(0.0123)
+1	-0.0532	-0.0361	-0.0396
	(0.0145)	(0.0145)	(0.0145)
Constant	0.491	0.523	0.558
	(0.00351)	(0.00346)	(0.00345)
Observations	19612	19612	19612

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

Appendix Table 10: Mortality Among Pre-period Inertial non-SaludCoop Enrollees

Relative time	Main	No SaludCoop hosp
-3	-0.0000980	0.0000312
	(0.0000713)	(0.0000599)
-2	0.000117	0.0000619
	(0.0000537)	(0.0000503)
0	0.00110	0.000613
	(0.000184)	(0.000118)
+1	0.00112	0.000616
	(0.000169)	(0.000109)
+2	0.000980	0.000559
	(0.000171)	(0.000105)
+3	0.00123	0.000703
	(0.000190)	(0.000117)
Constant	0.00301	0.00210
	(0.0000830)	(0.0000449)
Observations	154,004,263	76,472,807
Individuals	31,482,830	16,731,235

Note: Table reports event study coefficients and standard errors in parenthesis of individual mortality using the full sample of markets and excluding markets where SaludCoop hospitals operated. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level. The sample is restricted to individuals who do not switch insurers and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud.

Appendix Table 11: Mortality Among Fully Inertial non-SaludCoop Enrollees

	Full sample		Contributory	
Relative time	Main	No SaludCoop hosp	Main	No SaludCoop hosp
-3	-0.00015	-0.00005	-0.00011	0.00005
	(0.00008)	(0.00008)	(0.00012)	(0.00012)
-2	0.00005	-0.000003	0.00010	0.00005
	(0.00006)	(0.00006)	(0.00009)	(0.00009)
0	0.00120	0.00062	0.00082	0.00055
	(0.00019)	(0.00013)	(0.00021)	(0.00023)
+1	0.00102	0.00059	0.00057	0.00039
	(0.00017)	(0.00011)	(0.00017)	(0.00023)
+2	0.00081	0.00049	0.00028	0.00025
	(0.00018)	(0.00010)	(0.00016)	(0.00021)
+3	0.00110	0.00061	0.00041	0.00033
	(0.00020)	(0.00012)	(0.00017)	(0.00021)
Constant	0.00326	0.00220	0.00380	0.00238
	(0.00008)	(0.00004)	(0.00010)	(0.00011)
Observations	125,719,028	59,988,442	77,810,895	23,826,357
Individuals	24,787,324	12,424,794	15,089,894	5,035,079

Note: Table reports event study coefficients and standard errors in parenthesis of individual mortality using the full sample of individuals and the subsample covered by the contributory scheme. Table presents results in the full sample of municipalities and excluding municipalities with SaludCoop hospitals. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level. The sample is restricted to individuals who do not switch insurers throughout the sample period and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud.

Appendix Table 12: Mortality Among SaludCoop Enrollees

Relative time	Main	No SaludCoop hosp
-3	0.0000155	0.0000861
	(0.0000560)	(0.0000656)
-2	0.000182	0.0000887
	(0.0000600)	(0.0000647)
0	0.00131	0.000423
	(0.000170)	(0.000109)
+1	0.00155	0.000638
	(0.000196)	(0.000120)
+2	0.00184	0.000990
	(0.000192)	(0.000141)
+3	0.00200	0.00111
	(0.000165)	(0.000127)
Constant	0.00215	0.00193
	(0.0000376)	(0.0000130)
Observations	41,958,238	31,916,867
Individuals	8,528,529	6,898,086

Note: Table reports event study coefficients and standard errors in parenthesis of individual mortality using the full sample of markets and excluding markets with SaludCoop hospitals. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level. The treatment group is restricted to individuals enrolled with SaludCoop between 2013 and 2015. The control group are individuals in municipalities where SaludCoop did not operate.

Appendix Table 13: Mortality by Sex Among Fully Inertial non-SaludCoop Enrollees

Relative time	Female	Male
-3	-0.000158	-0.000144
	(0.0000811)	(0.0000931)
-2	0.0000699	0.0000322
	(0.0000733)	(0.0000742)
0	0.00113	0.00128
	(0.000163)	(0.000222)
+1	0.000938	0.00113
	(0.000153)	(0.000201)
+2	0.000747	0.000894
	(0.000164)	(0.000221)
+3	0.00116	0.00104
	(0.000199)	(0.000219)
Constant	0.00310	0.00345
	(0.0000788)	(0.0000958)
Observations	67,001,320	58,717,708
Individuals	13,042,880	11,930,450

Note: Table reports event study coefficients and standard errors in parenthesis of individual mortality by females and males. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level. The sample is restricted to individuals who do not switch insurers throughout the sample period and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud.

Appendix Table 14: Mortality by Age Among Fully Inertial non-SaludCoop Enrollees

Relative time	Age [15-21]	Age [35-41]	Age [65-71]
-3	-0.00000568	-0.000153	-0.000583
	(0.0000348)	(0.000139)	(0.000567)
-2	-0.0000524	0.0000313	0.0000400
	(0.0000421)	(0.000126)	(0.000612)
0	0.000157	0.0000552	0.00272
	(0.0000517)	(0.000177)	(0.000880)
+1	-0.00000778	0.000122	0.00278
	(0.0000552)	(0.000174)	(0.000870)
+2	0.0000791	0.000211	0.00333
	(0.0000567)	(0.000185)	(0.00104)
+3	0.000221	0.000180	0.00384
	(0.000126)	(0.000179)	(0.000912)
Constant	0.0000918	0.000620	0.00790
	(0.0000210)	(0.0000837)	(0.000427)
Observations	1,879,280	1,510,500	948,477
Individuals	481,050	238,018	146,328

Note: Table reports event study coefficients and standard errors in parenthesis of individual mortality by age group. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level. The sample is restricted to individuals who do not switch insurers throughout the sample period and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud.

Appendix Table 15: Mortality by Diagnosis Among Fully Inertial non-SaludCoop Enrollees

Relative time	AMI	COPD	Hepatic	Cancer	Renal	Diabetes	Dementia
-3	-0.000771	-0.00101	0.00361	-0.00367	-0.00126	0.0000719	-0.00589
	(0.00313)	(0.000776)	(0.00865)	(0.00155)	(0.000805)	(0.000593)	(0.00320)
-2	0.00186	-0.0000851	0.00759	-0.000790	0.0000337	0.000204	-0.00478
	(0.00298)	(0.000710)	(0.00853)	(0.00122)	(0.000759)	(0.000626)	(0.00340)
0	0.00575	0.00247	0.0151	0.00445	0.00384	0.00208	0.00964
	(0.00374)	(0.00105)	(0.0105)	(0.00132)	(0.00133)	(0.000700)	(0.00384)
+1	0.0138	0.00437	0.0203	0.00849	0.00696	0.00334	0.0104
	(0.00487)	(0.00133)	(0.0112)	(0.00135)	(0.00126)	(0.000848)	(0.00501)
+2	0.00820	0.00404	0.0202	0.00762	0.00682	0.00394	0.00331
	(0.00462)	(0.00150)	(0.0120)	(0.00153)	(0.00174)	(0.000950)	(0.00715)
+3	0.0156	0.00563	0.0326	0.00748	0.00996	0.00420	0.0118
	(0.00431)	(0.00159)	(0.0107)	(0.00225)	(0.00200)	(0.00105)	(0.00914)
Constant	0.0133	0.00605	0.00913	0.0101	0.00648	0.00357	0.0254
	(0.00262)	(0.000647)	(0.00707)	(0.000658)	(0.000740)	(0.000457)	(0.00265)
Observations	371,274	2,681,220	74,715	2,093,449	1,565,642	2,765,757	268,841
Individuals	55,263	405,471	11,249	311,496	229,201	403,948	40,451

Note: Table reports event study coefficients and standard errors in parenthesis of individual mortality by being diagnosed with a specific chronic disease at any point during the sample period. Specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level. The sample is restricted to individuals who do not switch insurers throughout the sample period and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud.

Appendix Table 16: Impact on Utilization of Critical Healthcare Treatments

Relative time	Insulin/Metfor.	Any oxygen/corti.	Any specialist	Renal test/Dialysis	Chemotherapy	Hepatic test
-3	0.303	0.00390	0.0159	-0.0318	0.0183	-0.00978
	(0.324)	(0.0110)	(0.0134)	(0.106)	(0.0305)	(0.0384)
-2	0.320	0.0276	0.0145	0.117	0.00937	0.0293
	(0.373)	(0.0182)	(0.0155)	(0.0585)	(0.0238)	(0.0215)
0	-0.0718	-0.0141	-0.0141	-0.146	-0.0496	-0.0711
	(0.123)	(0.0139)	(0.00663)	(0.120)	(0.0143)	(0.0293)
+1	-0.0105	-0.0272	-0.0230	-0.314	-0.0293	-0.112
	(0.233)	(0.0115)	(0.00816)	(0.132)	(0.0228)	(0.0617)
+2	-0.625	-0.0916	-0.0141	-0.399	-0.0129	-0.102
	(0.321)	(0.0203)	(0.00673)	(0.111)	(0.0179)	(0.0582)
+3	-0.475	-0.0681	-0.0326	0.0649	-0.0312	0.0216
	(0.276)	(0.0149)	(0.00985)	(0.160)	(0.0224)	(0.0412)
_cons	3.903	0.348	0.363	1.640	0.264	0.715
	(0.181)	(0.00697)	(0.00490)	(0.0686)	(0.0136)	(0.0253)
Observations	5126808	4487091	46118013	4621208	2691656	4621208
Individuals	793610	768818	9003709	1214315	441982	1214315

Note: Table reports event study coefficients and standard errors in parenthesis using as outcome variables the number of insulin or metformin claims among patients with diabetes in column (1), an indicator for any oxygen and corticosteroid claims among patients with COPD in column (2), an indicator for specialist consultations among patients with any chronic diseases in column (3), an indicator for dialysis claims among patients with renal disease in column (4), the number of chemotherapy among patients with cancer in column (5), and an indicator for any hepatic function tests among patients with hepatic diseases in column (6). Specifications include municipality, insurer, and year fixed effects. Standard errors are clustered at the municipality level. The sample is restricted to individuals who do not switch insurers throughout the sample period and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop and Cafesalud.

Appendix Table 17: Mortality Effect by Provider Exclusions Among Fully Inertial non-SaludCoop Enrollees

	High use		Low use	
Relative time	All markets	w/o SaludCoop hosp	All markets	w/o SaludCoop hosp
-3	-0.00138	-0.0000651	0.000209	-0.000277
	(0.000901)	(0.000903)	(0.000498)	(0.000447)
-2	0.000328	0.000329	0.000191	-0.0000322
	(0.000582)	(0.000624)	(0.000301)	(0.000308)
0	0.00218	0.00121	0.000700	0.000265
	(0.000673)	(0.000616)	(0.000576)	(0.000512)
+1	0.00259	0.00122	0.000212	0.000160
	(0.000821)	(0.000689)	(0.000484)	(0.000541)
+2	0.00394	0.00235	-0.000173	0.000161
	(0.000922)	(0.000860)	(0.000482)	(0.000516)
+3	0.00381	0.00214	-0.0000454	0.000297
	(0.000991)	(0.000711)	(0.000444)	(0.000514)
Constant	0.00751	0.00425	0.00673	0.00405
	(0.000311)	(0.000180)	(0.000283)	(0.000308)
Observations	1,882,347	1,033,792	25,078,263	6,908,557
Individuals	337,559	209,675	4,988,619	1,508,432

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

Appendix Table 18: Mortality Effect by Network Overlap Among Fully Inertial non-SaludCoop Enrollees

	High overlap		Low overlap	
Relative time	All markets	w/o SaludCoop hosp	All markets	w/o SaludCoop hosp
-3	-0.000288	0.0000259	-0.000229	-0.0000931
	(0.000112)	(0.000119)	(0.0000724)	(0.0000682)
-2	0.0000696	0.0000635	-0.0000839	-0.0000426
	(0.0000748)	(0.0000775)	(0.0000521)	(0.0000597)
0	0.000643	0.000467	0.00186	0.000692
	(0.000202)	(0.000233)	(0.000198)	(0.000160)
+1	0.000505	0.000278	0.00184	0.000722
	(0.000155)	(0.000248)	(0.000189)	(0.000154)
+2	0.000199	0.0000414	0.00183	0.000700
	(0.000144)	(0.000202)	(0.000188)	(0.000140)
+3	0.000370	0.0000570	0.00216	0.000855
	(0.000137)	(0.000215)	(0.000223)	(0.000163)
Individuals	14701485	14701485	14701485	14701485

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

Appendix Table 19: Mortality Effect by Insurer HHI Among Fully Inertial non-SaludCoop Enrollees

	Predicted HHI≥2500		Predicted HHI<2500	
Relative time	All markets	w/o SaludCoop hosp	All markets	w/o SaludCoop hosp
-3	-0.000159	-0.0000664	-0.000331	0.000129
	(0.0000694)	(0.0000782)	(0.000139)	(0.000165)
-2	0.0000254	-0.00000409	-0.0000172	0.0000190
	(0.0000591)	(0.0000572)	(0.0000873)	(0.000130)
0	0.00108	0.000558	0.00143	0.00149
	(0.000154)	(0.000111)	(0.000418)	(0.000756)
+1	0.00105	0.000543	0.00127	0.00113
	(0.000154)	(0.000107)	(0.000342)	(0.000471)
+2	0.000934	0.000460	0.00104	0.000914
	(0.000143)	(0.0000990)	(0.000350)	(0.000446)
+3	0.00116	0.000602	0.00132	0.000741
	(0.000174)	(0.000118)	(0.000377)	(0.000411)
Constant	0.00264	0.00219	0.00348	0.00196
	(0.0000645)	(0.0000406)	(0.000142)	(0.0000430)
Observations	74799393	57017861	70070684	22121630
Individuals	15194270	11789308	13849818	4584345

Note: Table reports event study coefficients and standard errors in parenthesis of individual mortality by whether the predicted insurer HHI in treated markets is below or above 2,500. Predicted insurer HHI is calculated based on predicted market shares assuming SaludCoop's enrollees in 2014 are assigned to incumbent insurers in proportion to their market shares. Sample is restricted to individuals who do not switch insurers throughout the sample period and did not move across municipalities before the termination. We exclude individuals enrolled with SaludCoop or Cafesalud. Treated units are municipalities where SaludCoop operated.

Appendix Table 20: Mortality Effect by Doctors per Capita Among Fully Inertial non-SaludCoop Enrollees

	Docs per 1,000≥18		Docs per 1,000<18	
Relative time	All markets	w/o SaludCoop hosp	All markets	w/o SaludCoop hosp
-3	-0.000485	-0.0000763	-0.000210	-0.000122
	(0.000105)	(0.0000948)	(0.0000871)	(0.0000942)
-2	-0.0000539	-0.0000688	-0.0000610	-0.0000291
	(0.0000658)	(0.0000865)	(0.0000610)	(0.0000625)
0	0.00139	0.000455	0.00102	0.000673
	(0.000321)	(0.000210)	(0.000164)	(0.000140)
+1	0.00132	0.000461	0.000914	0.000643
	(0.000304)	(0.000205)	(0.000137)	(0.000120)
+2	0.00112	0.000509	0.000810	0.000544
	(0.000325)	(0.000216)	(0.000133)	(0.000108)
+3	0.00142	0.000624	0.00103	0.000689
	(0.000348)	(0.000261)	(0.000150)	(0.000124)
Constant	0.00342	0.00191	0.00258	0.00223
	(0.000138)	(0.0000330)	(0.0000567)	(0.0000447)
Observations	83308196	26106531	61561881	53032960
Individuals	16626596	5371038	12718089	11010306

Note: Table reports event study coefficients and standard errors in parenthesis of individual mortality by whether the number of doctors (physicians plus nurses) per 1,000 enrollees during 2015 is below or above 18. Estimation excludes individuals enrolled with SaludCoop or Cafesalud. Treated units are municipalities where SaludCoop operated.