Public Externalities of Universal Health Care Access*

Tatiana Hiller Leonardo Peñaloza-Pacheco Sandra V. Rozo

Abstract

Does extending health coverage to an excluded population affect mortality among those already insured? In 2022 Colombia granted two million Venezuelan migrants full health coverage in place of emergency-only care. Comparing municipalities more and less exposed to the program before and after its onset, we find that one standard deviation of exposure reduced Colombian mortality by 1.8 percent, one native life a year per 300 migrants covered. After the program migrants made a more efficient use of the health services, providers were paid regularly rather than late for emergency episodes, and the deaths averted are those timely treatment prevents. The net fiscal cost is USD $58,000 per Colombian life saved.

*Hiller: University of California, Davis (hiller@ucdavis.edu). Peñaloza-Pacheco: Cornell University (ljp96@cornell.edu). Rozo: World Bank, Development Research Group (sandrarozo@worldbank.org), IZA, EGAP, and CEGA. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors and do not necessarily represent the views of the World Bank, its Board of Executive Directors, or the governments they represent.

1 Introduction

Every health system in the world has rules in place that create exclusions for some share of the population living in the same territory. They are uninsured citizens in some countries, informal workers in others, migrants without legal status, or refugees.[1] Most typically, however, the excluded populations still have a right to emergency care alone, as most countries honor the right to live. The excluded are not thereby removed from the health system, but often times they arrive later and sicker, and are treated at its most expensive point of entry alongside the insured population in the same hospitals and on the same public budget.[2] However, when deciding whether or not to fully insure excluded populations in the health system, the decision often weighs only the coverage fiscal costs against the health effects it buys for the newly covered. Typically, this decision does not account for the potential congestion externalities of keeping excluded populations partially covered over the population that is already insured.

This paper addresses this gap by examining the potential public health externalities of switching excluded populations from emergency-only health care access to full health coverage on the population previously insured in Colombia. In January 2022, the country granted a ten-year permit to nearly two million Venezuelan migrants, about four percent of its population. The permit, called the *Permiso por Protección Temporal* (PPT), offered full access to the public health system along with the right to work and to use financial markets, replacing an entitlement to emergency care alone that had left fewer than three percent of these migrants fully insured in the country. The PPT is the largest migrant regularization program ever implemented in a developing country. Three features make it useful for measuring the potential health externalities of including Venezuelans into the

[1] Among twenty-nine refugee-hosting countries with social health insurance, for instance, only twelve extend it to refugees (World Health Organization, 2026).

[2] In fact, most of the experimental evidence on public insurance concerns the covered themselves. Some notable examples include the RAND Health Insurance Experiment (Newhouse and the Insurance Experiment Group, 1993), the Oregon Health Insurance Experiment (Finkelstein et al., 2012), *Seguro Popular* in Mexico (King et al., 2009), and social health insurance for informal workers in Nicaragua (Thornton et al., 2010), among many others (Currie and Gruber, 1996; Miller et al., 2021; Card et al., 2009; Goodman-Bacon, 2021; Brown et al., 2020).

health system. First, health coverage arrived suddenly, on a date set by the government rather than by the migrants. Second, it arrived years after migrants had chosen where to live, so the geography of exposure was already fixed. Third, Colombians themselves were already insured, with 99 percent of the population affiliated to the health system by 2022 (Melo-Becerra et al., 2023), so any change in their mortality reflects the care they received rather than coverage they gained.

We study whether extending health coverage to these migrants reduced mortality among Colombians, and estimate the expansion's cost per Colombian life saved. To answer both questions, we assemble a monthly panel of Colombia's 1,121 municipalities from 2016 to 2024 using administrative records covering the universe of deaths, births, health-care services, and insurance enrollment. Death records include cause of death, and birth records include the mother's insurance status and prenatal care. The health-service data contain about 4.4 billion records, each reporting the type of care, diagnosis, and identification document presented by patient, allowing us to distinguish regularized migrants, undocumented migrants, and Colombians. We also observe every person enrolled in the health insurance system, by document type, so we can track the newly insured as they enter the system, as well as all 2.29 million ETPV registrations by municipality. Our empirical design compares municipalities with different exposure to the program, measured as permits delivered per 1,000 inhabitants, before and after the program massive onset, absorbing municipality and year-month fixed effects and differential pandemic recovery.

Our main finding is that extending health coverage to Venezuelan migrants reduced mortality among Colombians. A one standard deviation of exposure to the PPT program, representing about 30 newly covered migrants per 1,000 inhabitants in a given municipality, reduced mortality among Colombians by 1.8 percent, their infant mortality by 2.2 percent, and mortality among adults 65 and older by 2.7 percent. The mortality reductions survive excluding all COVID-coded deaths, at roughly half the magnitude for the overall and elderly margins and are essentially unchanged for infants, so the result is not an artifact of COVID-coded mortality reverting after the pandemic. We also find suggestive evidence that maternal mortality also declines, by about 7 percent, but this is our least precisely estimated

margin. Recorded deaths of migrants themselves show no decline on most margins, with two exceptions, external causes and young-adults, where migrant deaths concentrate, both of which fall significantly, a benefit consistent with full gained access. We read the migrant mortality rates carefully, since they were likely under-registered before the program and the migrant population is disproportionately young, with naturally low mortality.

Our estimated coefficients represent sizable effects. Take a municipality of 100,000 inhabitants at one standard deviation of exposure, a municipality where 3,000 migrants received permits. Our estimates imply that this municipality will have about 10 fewer deaths every year than an identical municipality the program passed by. Ten lives from 3,000 permits is one Colombian life saved each year for every 300 migrants covered. Applying the same rate to all 1.96 million permits implies roughly 6,500 deaths averted per year, life savings of the same order as the lives Colombia loses annually on its roads (Agencia Nacional de Seguridad Vial, 2024), or on the order of 19,500 lives over the three post-program years. The adult gains are overwhelmingly native lives since migrants are young and Venezuelan-born individuals account for only half a percent of deaths at ages 65 and older. These declines are of the same order of what flagship insurance expansions in the United States achieve for the people who gain coverage (Currie and Gruber, 1996; Miller et al., 2021), with the difference that here they benefit a population that was already insured, the Colombian host population.

The fiscal accounting reinforces our main conclusion. Because emergency care was already publicly financed for undocumented migrants, expanding insurance increased net health spending by only USD 72 to 374 million annually. Even under the most conservative assumptions, the implied fiscal cost is just USD 11,000 to USD 58,000 per Colombian life saved, an order of magnitude below the value of a statistical life. In fact, the mortality benefits to natives alone are large enough to justify the fiscal cost of insuring migrants, even before counting any gain to the migrants themselves.

Our analysis suggests that extending full health coverage for Venezuelan migrants relieved congestion in the health services Colombians and migrants share, through two main channels.

First, migrants who no longer had to wait for an emergency to be seen moved their care into routine services. A one standard deviation increase in permits per 1,000 inhabitants, about 30 permits, raises migrants' outpatient encounters by 88 percent of their mean and lowers the share of migrant care that takes place in emergency rooms by 12 percent. Second, the providers treating them began to be paid regularly, in advance, and per person, rather than late and partially for emergency episodes. In Colombia every person enrolled in the health system carries a fixed annual capitation payment, so the 14.6 additional enrollees per 1,000 inhabitants that the increase in permits produces, are directly translated in 14.6 additional payments arriving at the providers of that municipality.

Although we do not observe what providers did with the money, the data suggests that part of it was use to increase the service supply. On one hand, Colombians' outpatient consultations and procedures rose by 4 and 7 percent, which moving migrants across services cannot produce on its own, and the mortality decline is close to twice as large in municipalities that received more public health funding, a difference that appears only after the permits arrived. On the other hand, the deaths that fell are the ones a shorter wait in the emergency room prevents. Among the elderly, deaths from conditions that timely care manages, such as pneumonia and diabetes, fall by 3.7 percent, while deaths from heart attacks and strokes, which are treated first whatever the load, because of emergency rooms triage, do not move. Consistent with this idea, among infants, deaths amenable to postnatal care fall by 7.3 percent, while deaths determined at or before birth do not.

We rule out several alternative explanations including income effects, crime effects, lower contagion, or an expanded supply of migrant health professionals. Local economic activity and Colombians' employment and earnings do not respond to the permits, so the decline is not an income effect. Homicides and deaths from external causes do not fall, so it is not lower crime. Communicable-disease mortality is unchanged and childhood vaccination did not rise, so it is not reduced contagion. The 1,452 migrant health professionals registered by 2024 are seven tenths of one percent of Colombia's physicians, too few to have expanded supply. Moreover, Colombian enrollment did not fall where PPT enrollment rose by 63 percent of its mean, so Colombians were not crowded out of insurance.

Our findings survive a broad set of robustness checks. Estimates are consistent across the heterogeneity-robust difference-in-differences estimators of Callaway and Sant'Anna (2021), Borusyak et al. (2024), and de Chaisemartin and D'Haultfœuille (2020). The overall and elderly mortality results remain robust to alternative sample restrictions and exposure measures, controls for differential trends and the pandemic, and multiple-testing corrections. In fact, several specifications yield larger mortality reductions, suggesting that our baseline estimates may be conservative. Infant mortality estimates are significant in most specifications, whereas maternal mortality results remain suggestive.

Our findings suggest that the health benefits to Colombian hosts alone were sufficient to offset the costs of extending health care coverage to Venezuelan migrants. Evaluating expansion solely as a transfer to new beneficiaries overlooks how inclusion can improve care for existing patients. The Colombian experience suggests that these benefits arise primarily from changes in health system financing and service delivery rather than reduced disease transmission. Insurance coverage brings care into outpatient services and pays providers for it regularly, in place of emergency treatment reimbursed late and only partially. These changes ease pressure on shared services, benefiting patients who already have coverage. The gains depend on how coverage is designed and whether providers have the resources to meet increased demand. For governments choosing between emergency care alone and comprehensive coverage, our results show that inclusion can improve health even among those already insured.

Our main contribution is to provide causal evidence that extending standard health coverage to a previously excluded population already residing in the country reduces mortality among the insured host population. We show that this externality operates through reduced congestion in shared healthcare capacity rather than through disease transmission, and that the fiscal cost of the expansion is small relative to conventional valuations of the host-population lives saved. These findings contribute to three strands of the health and migration literatures.

First, we contribute to the literature on public health insurance. A large body of work

studies the effects of public insurance on newly covered populations (Currie and Gruber, 1996; Finkelstein et al., 2012; Goodman-Bacon, 2018; Goldin et al., 2021; Miller et al., 2021). Much less is known about the effects of coverage expansions on individuals who are already insured. Existing evidence shows that expansions shift hospital spending and the burden of uncompensated care (Finkelstein, 2007; Finkelstein et al., 2019; Garthwaite et al., 2018), but finds modest or no adverse effects on already-insured populations (McInerney et al., 2017; Carey et al., 2020; Barkowski et al., 2025). We study a setting with tighter hospital capacity and greater reliance on uncompensated emergency care among the newly covered, and document substantial mortality reductions among the already insured. These gains alone are sufficient to offset the fiscal cost of extending coverage. Welfare frameworks for public insurance and for immigration policy (Storesletten, 2000; Dustmann and Frattini, 2014; Hendren and Sprung-Keyser, 2020) gain an externality term once covering the excluded lowers mortality among the already insured, which raises the marginal value of public funds spent on it.

Second, we contribute to the literature on congestion and shared healthcare capacity. A growing body of work shows that congestion worsens patient outcomes and that reductions in emergency-department congestion or waiting times can lower mortality (Hoe, 2022; Woodworth, 2020; Gruber et al., 2023; Aguilar-Gomez et al., 2025). Related work shows that provider financing can be effective for expanding healthcare access and improving health (Gruber et al., 2014). In a migration setting, Ager et al. (2024) show that the 1920s U.S. immigration quotas reduced U.S.-born mortality largely by reducing residential crowding, with larger effects where hospital facilities were initially more congested. We identify a complementary mechanism through which migrant inclusion can benefit host health. When an excluded population already resides in the country and relies on emergency care, extending coverage with dedicated financing can shift demand toward funded outpatient services and relieve pressure on shared emergency capacity.

Third, we contribute to the literature on migrant inclusion and its effects on host populations. Existing work primarily examines the effects of migrant arrival (Card, 1990; Dustmann et al., 2017b; Clemens and Hunt, 2019; Tribín-Uribe et al., 2020; Rozo and Vargas, 2021) or

regularization on migrants themselves (Kossoudji and Cobb-Clark, 2002; Baker, 2015; Mastrobuoni and Pinotti, 2015; Devillanova et al., 2018; Dustmann et al., 2017a; Pinotti, 2017; Bahar et al., 2021; Elias et al., 2025; Ibáñez et al., 2025). Evidence on spillovers centers on migrants' families and communicable disease (Watson, 2014; Hainmueller et al., 2017; Giuntella and Lonsky, 2020; Alba-Vivar et al., 2022), with the latter operating through contagion and prevention (Cutler and Miller, 2005; Alsan and Goldin, 2019; White, 2021). The closest studies find no effect on native health from extending refugee coverage in Turkey (Aygün et al., 2021) and increased migrant mortality following coverage withdrawal in Spain (Juanmartí Mestres et al., 2021). We distinguish coverage expansion from migrant arrival by studying migrants already residing in Colombia. We show that coverage can reduce mortality in the broader host population, with evidence pointing to increased financing and reduced pressure on shared healthcare capacity rather than changes in communicable disease.

2 Background

2.1 The Venezuelan Exodus and Health Care Access in Colombia

Venezuela's political, social, and economic collapse, caused by the regime of Hugo Chávez and Nicolás Maduro between 1998 and 2025, produced the largest displacement episode in the contemporaneous history of the Americas. By 2024 nearly 7.9 million Venezuelans had left the country, 6.7 million of them living in Latin America and the Caribbean (UNHCR, 2025). Migrants were fleeing rampant poverty rates which reached 94 percent of the population in 2021, and intense food deprivation, with at least 75 percent of the population subsisting on a limited variety of foods by the same year (Cabra-Ruiz et al., 2024). The exodus was also a health crisis. Venezuela's health system had disintegrated with health coverage dropping from 45.8 to 3.4 percent between 2014 and 2021. By that year, approximately 60 percent of those with chronic illnesses had little to no access to essential medications (Cabra-Ruiz et al., 2024). Infant mortality rose by roughly 30 percent and maternal mortality by 65 percent in 2016 alone, so migrants fled the country with substantial unmet health needs, interrupted

vaccination schedules, and elevated exposure to communicable disease (Ibáñez et al., 2021).

Colombia, with a two-thousand-kilometer shared border and deep historical and linguistic ties, became the main migrant destination, hosting about 2.8 million Venezuelans as of 2025 (Migración Colombia, 2025). The inflow to Colombia reached its peak in 2018, and most migrants entered or remained irregularly in Colombia. Many migrants overstayed the 180-day tourist entry, and many others crossed through informal border paths, often without passports that Venezuela's collapsed public administration could no longer issue (Bahar et al., 2021; Ibáñez et al., 2025).

2.2 The Colombian Health System

Colombia's health system combines a payroll-financed contributory regime with a subsidized regime for the poor, targeted through Sisben, the national proxy-means-testing registry. Insurers in both regimes receive a per-affiliate capitation payment from the Colombian government and virtually the entire Colombian population is covered (Melo-Becerra et al., 2023). Access for migrants, however, depended on legal status. By law, anyone on Colombian territory is entitled to emergency care regardless of documentation, but undocumented migrants could not enroll in either insurance regime, could not be scored by Sisben, and therefore, had no access to primary, preventive, or specialized services. The result was a starkly two-tiered system, fewer than 3 percent of undocumented Venezuelans had any insurance affiliation, and their contact with the health system occurred overwhelmingly through emergency rooms, where conditions that could have been prevented or managed early were treated late and at high cost. The health costs of undocumented migrants were covered primarily by the central government, which reimbursed providers ex post at emergency tariff rates (Ibáñez et al., 2025).

2.3 Regularization Programs in Colombia

Colombia responded to the Venezuelan exodus with two successive large regularization programs. These programs granted temporary legal status together with full access to labor markets, financial markets, and social protection. Two large programs were launched after 2018.

The PEP Program. The first, the *Permiso Especial de Permanencia* (PEP), registered 281,000 undocumented migrants (Ibáñez et al., 2025). It was open to any migrant who arrived to the country before June 2018. A substantial literature evaluates this first generation of regularization, finding that it markedly improved migrants' well-being, health status, social capital, and service access, raising subsidized-regime enrollment by 27 percentage points from a base of under 2 percent, while reducing the net fiscal cost of hosting a migrant household by roughly 42 percent, in part because insured migrants substitute inexpensive preventive care for costly emergency care (Ibáñez et al., 2025; Rozo, 2026). At the same time the PEP program left native labor markets essentially unaffected (Bahar et al., 2021), lowered migrant fertility through improved access to family planning (Amuedo-Dorantes et al., 2025), reduced crime and increased crime reporting by migrant women (Ibáñez et al., 2026), and produced no detectable change in native voting behavior (Rozo et al., 2026).

The PPT program. The second program, announced on February 8, 2021, scaled the PEP model up in a new permit called *Permiso por Protección Temporal* (PPT). Every Venezuelan living in Colombia before January 31, 2021 became eligible for a ten-year PPT conferring legal status, the right to work, and full access to any social program including full health services (Urbina et al., 2025). Implementation proceeded in stages and operated through a legal statute called *Estatuto Temporal de Permanencia para Venezolanos* (ETPV). Throughout the paper we refer to the ETPV when describing the legal statute and the registration process, and to the PPT when describing the permit itself, the exposure measure, and the treatment. Online registration to the ETPV opened on May 5, 2021; biometric

appointments began in September 2021; and the PPT permit began to be issued to everyone who completed the whole process in October 2021, when roughly 3,000 pilot documents were issued. However, PPT mass delivery was effective only in January 2022, with about 240,000 permits delivered by the end of that month. Registrations to the PPT reached about 2.46 million by early 2023, and permit authorization ultimately exceeded 2.1 million. Administrative data from Migración Colombia counts 2.29 million registrants and 1.96 million permits delivered, implying a coverage of roughly 4 percent of Colombia's population, the largest regularization ever implemented in a developing country.

Because health system access was only possible once migrants received the physical permit, January 2022 marks the start of our post-treatment period. The timing is visible in the data, as the number of migrant patients holding a permit, relative to those without one, is flat through the announcement and registration phases and rises sharply after January 2022 (Figure 1). Figures 2 and 3 document the resulting transformation of migrant care.

PPT Take-Up and Health System Affiliation. The program transformed how migrants used the health system. Care delivered to undocumented migrants collapses after mass permit delivery, while PPT-based care expands to about three quarters of all migrant services by 2024 (Figure 2). Permits delivered through the PPT are reflected one-on-one on the number of people enrolled in the health system (Figure 4) and into PPT patients observed in the health records (Appendix Figure A.1).

Migrant Health Care Utilization. Newly regularized migrants use more care, but it is a different care. Total migrant utilization rises with the permit stock, and its composition shifts further toward outpatient and preventive services (Figure 3). This is the utilization margin through which any externality on the host population likely operates.

3 Data

We combine administrative registries on the universe of deaths, health service records, health insurance affiliations, and registrations in the PPT regularization program in Colombia. Each variable is aggregated to the municipality-month, our unit of observation, over the 108 months between January 2016 and December 2024. This section describes the data. For additional details, Section A.1 of Appendix A validates the panel against published aggregates (Figures A.1 to A.3), Section A.2 documents sources and variable construction and reports descriptive statistics (Tables A.1 to A.4), and Section C.1 of Appendix C gives the exact diagnosis code ranges behind every mortality outcome.

3.1 Health Outcomes and Health System Use

Mortality rates.—Mortality is our main outcome. It comes from the death records of Colombia's national statistical agency, the *Departamento Administrativo Nacional de Estadística* (DANE), which cover every death registered in the country. Each death certificate records the municipality of residence, the month of death, the age group, and the basic cause of death, coded by the certifying physician under the World Health Organization's International Classification of Diseases, tenth revision (ICD-10). We assign each death to the municipality of residence of the deceased, and to the municipality of occurrence in the few records where residence is missing. From these records, and from DANE's annual municipal population figures, we build the monthly mortality rates listed in Table 1, panel A, which describes each outcome, its definition, and the hypothesis it tests. According to the mortality rates we constructed, between 2016 and 2024 Colombia recorded 45 deaths per 100,000 inhabitants per month, about 5.4 per 1,000 per year, 11 infant deaths per 1,000 live births, 55 maternal deaths per 100,000 live births, and 309 deaths per 100,000 adults aged 65 and older per month (Table 2). These match the figures DANE publishes, and Appendix Figures A.2 and A.3 show that our monthly series track the published national series throughout, including the pandemic waves of 2020 and 2021 and the 2021 spike in maternal mortality.

We choose which mortality outcomes to analyze following the public insurance literature. We study infant and maternal mortality because these are the deaths that timely prenatal, obstetric, and neonatal care prevents, they respond to access within months, and they are the outcomes that literature has studied most closely (Currie and Gruber, 1996; Goodman-Bacon, 2018). We study mortality at ages 65 and older because it is where deaths concentrate, more than half of the total, and where adult access to care matters most, the margin the American literature measures at the Medicare threshold and among the near-elderly (Card et al., 2009; Miller et al., 2021). We include communicable-disease mortality to test the transmission externality that motivated the literature on migration and infectious disease (Ibáñez et al., 2021), and external-cause mortality as our falsification outcome. Finally, we study the all-cause mortality rate because it adds up every cause of death, those that coverage can prevent and those it cannot, so it tells us whether the program lowered the total number of Colombians who died overall.

We need to separate deaths of Colombians from deaths of migrants, because the externality we aim to measure is mortality among Colombians. The death certificate records the country where the deceased lived for the whole period, and the country of birth only from 2024. For the full panel we therefore classify a death as Colombian when the deceased lived in Colombia and as Venezuelan when the deceased lived in Venezuela. The 2024 records, the first with country of birth, show that this classification measures what we need. In that year Venezuelan-born individuals account for 2,726 deaths, about one percent of the total, and for half a percent of deaths at ages 65 and older, because the migrant population is young (Appendix Figure D.1). Migrant deaths are too few to move the Colombian series whatever label they carry, so the mortality we measure is Colombian mortality.

Population and births.—To construct mortality rates, we divide deaths by the population population living in each municipality each month. For infant and maternal mortality, we instead scale deaths by the number of live births. We take both population and birth counts from DANE. For population, we use DANE's official projections. DANE counts the population in a census, most recently in 2018, and projects it forward and backward year by year for every municipality and age group. These projections are the denominators DANE

itself uses to publish mortality rates and allow us to compute rates for the full population and age specific groups, such as adults aged 65 and older. Births come from DANE's birth statistics, which record births each month according to the municipality where the mother lives. The same birth certificates also report whether the mother was insured, the number of prenatal visits, whether delivery occurred in a health facility and with whom, and the baby's weight and gestational age. We use these as the birth outcomes listed in Table 1, panel B, in Section 6 and Appendix E.

Health service use.—To measure how people used the health system, we use the *Registro Individual de Prestación de Servicios de Salud* (RIPS), the individual service records that health providers in Colombia are required to submit to the Ministry of Health for each service they deliver. These records form the basis for reimbursement by insurers and cover the universe of health services delivered in the country, comprising about 4.4 billion records over our study period. We access it through the Ministry's *Sistema Integrado de Información de la Protección Social* (SISPRO), the data warehouse that consolidates the records and allows them to be tabulated across their different fields. Each record reports the type of service (consultation, procedure, emergency visit, or hospitalization), the ICD-10 diagnosis, the purpose of the visit, the patient's insurance regime, identification document type, the patient's sex and age group, and the municipality and month in which the service was delivered. We aggregate these records into two municipality-month extracts, whose construction is documented in Section A.2 of Appendix A.

The first extract covers all services at the municipality-month level, disaggregated by the identification document presented at the time of care. Among non-Colombian users, the document separates migrants regularized under the ETPV, who hold the *Permiso por Protección Temporal* (PPT) or its predecessor the *Permiso Especial de Permanencia* (PEP) and constitute our treatment group, from other legally present migrants, residents with a *cédula de extranjería* (the foreigner identification card), asylum seekers with a *salvoconducto* (a temporary safe-conduct permit), and short-term visitors with a passport. Children of migrants born in Colombia appear through the *registro civil de nacimiento* (the civil birth registration), and patients recorded without valid identification serve as a proxy for undoc-

umented migrants. Colombians are identified by the three Colombian document types, the *cédula de ciudadanía*, the national identity card of adults, and the *tarjeta de identidad* and *registro civil*, the identity card and birth registration of minors. This extract measures how migrant care shifted between emergency and routine services after the permits arrived, the utilization margin the congestion mechanism runs through.

The second extract covers all users by municipality-month and groups ICD-10 diagnoses into epidemiological categories following Ibáñez et al. (2021), vector-borne, vaccine-preventable, sexually transmitted, waterborne, and respiratory infections; maternal conditions; and chronic conditions, a non-communicable comparison group. Because RIPS records clinical diagnoses at the point of care, these are diagnosed-visit measures rather than laboratory-confirmed cases. This extract tests whether the coverage expansion changed the incidence of transmissible disease, an alternative channel to congestion relief.

Insurance enrollment.—Who was insured comes from the *Base de Datos Única de Afiliados* (BDUA), the national registry of every affiliate to Colombia's health insurance system, the *Sistema General de Seguridad Social en Salud*, also extracted from SISPRO. It records, for each municipality and month, the stock of everyone enrolled by insurance regime and by type of identification document, over 1,135 municipalities in 2016 to 2024. Because it is a stock at each month's cut-off rather than a flow, counts are never summed across months. The document type is what makes the registry useful here. It separates holders of the PPT from holders of the earlier PEP and from foreigners with ordinary residency, so PPT enrollment can be tracked apart from any other change in affiliation.

Vaccination.—Preventive care comes from the information system of the Ministry's Expanded Program on Immunization, which consolidates the vaccination records reported by all municipalities and gives, for each municipality, year, vaccine, and dose number from 2016 to 2024, the doses applied and the programmatic target, which is the number of children the program aimed to vaccinate in that municipality that year.

3.2 Program Exposure

ETPV registry (Migración Colombia).—Our treatment measure comes from the administrative records of the ETPV held by the national migration authority, obtained under an agreement with Migración Colombia as a snapshot taken in April 2026. The snapshot lists the cumulative stock of registrants by the municipality in which they registered and distinguishes those who had received their PPT from those still in process, 2.29 million registrants and 1.96 million with a PPT. We scale permits delivered by 2022 municipal population to obtain the fixed exposure measure defined in Section 4. Figure A.4 in Appendix A maps it across municipalities, with boundaries from DANE's official municipal map, the *Marco Geoestadístico Nacional* (Departamento Administrativo Nacional de Estadística , DANE), and Table A.1 reports descriptive statistics for outcomes and treatment.

Municipal exposure translates into actual insurance coverage. Enrollment of PPT holders is proportional to permits delivered across municipalities. PPT enrollment is mechanically zero before the permit existed and rises monotonically from 2022 (Figure 4). Permits delivered also map almost one-for-one into PPT patients observed in RIPS (Appendix Figure A.1), and Migración Colombia's published municipal snapshot of PPT holders matches our counts with a correlation above 0.99.

The average municipality received 17.9 permits per 1,000 inhabitants, with a median of 10 and a standard deviation of 29.4. Appendix Table A.1 reports the full distribution.

3.3 Auxiliary Data

We use complementary sources of data to test alternative explanations and control for pre-existing differences across municipalities. These sources are described below.

Municipal characteristics (CEDE).—The municipal panel constructed by Universidad de los Andes provides pre-shock characteristics that we use to test the robustness of our main effects. Each variable is measured at its latest available value between 2016 and 2019. The

variables employed include poverty, measured both by the share of households with unsatisfied basic needs and by the share the census classifies as multidimensionally poor; rurality, altitude, and distance to the capital cities; water and sewerage coverage; the municipal performance score that the national planning department assigns to each local government; health transfers under the *Sistema General de Participaciones*, the system through which the central government transfers earmarked funds for health, education, and water to municipalities, so this variable measures how much central health funding each municipality received per person; SISBEN registrants[3], and health service providers. They serve as baseline controls interacted with time trends (Appendix Table C.1) and as the supply- and demand-side moderators in Section 6.

Local economic activity (night lights).—Night-light density comes from the Visible Infrared Imaging Radiometer Suite (VIIRS), a satellite sensor that records the light emitted from the ground at night. We aggregate the data to mean radiance by municipality through 2024. We use it as a standard proxy for local economic activity where subnational GDP is unavailable.

Labor markets (GEIH).—Labor-market outcomes come from the *Gran Encuesta Integrada de Hogares*, the DANE labor force survey that underlies Colombia's official employment statistics. We use the 2016 to 2024 waves for the twenty-five main cities, identify Venezuelans by country of birth, and aggregate to city-year cells by nationality to measure employment, formality, labor income, and, through the economic-activity codes, employment in the health sector, the basis for the labor-market and workforce panels of Appendix Table D.4.

Health workforce.—The *Registro Único Nacional del Talento Humano en Salud* (ReTHUS), the national registry of health professionals, records every registered health professional by profile, municipality of work, and year, and separately identifies professionals holding a

[3]SISBEN, the *Sistema de Identificación de Potenciales Beneficiarios de Programas Sociales*, is the household registry Colombia uses to target social programs. Households are surveyed and scored by their living conditions, and the score determines eligibility for the subsidized health regime and other benefits. The number of registrants in a municipality therefore measures the size of its low-income population as the state identifies it.

foreign-issued degree, a PPT, or a *cédula de extranjería*. We use it in Section 6 to bound the size of the migrant health workforce.

Analysis sample.—The estimation panel is a balanced municipality-month panel. It includes 1,120 of Colombia's 1,121 municipalities over 108 months, for a total of 120,960 observations.[4] Some outcomes with births as the denominator lose the handful of municipality-months with no recorded births. The RIPS, BDUA, and vaccination extracts were accessed through the authors' institutional access to SISPRO, the vital statistics microdata are DANE's, and the ETPV registry was obtained from Migración Colombia.

4 Empirical Strategy

Migrants typically tend to go where economic activity is higher. Venezuelans settled where they could find jobs, along with adequate transportation, and housing and services, so exposure to the PPT is highest in the larger, richer and better connected municipalities. Consequently, we cannot simply compare municipalities with more and fewer permit holders, as this comparison would be biased. Those same municipalities have more doctors, more hospital beds and lower mortality for reasons that have nothing to do with the program, so the comparison would confound the effect of coverage with the level of local development.

We address this issue by exploiting the variation in the intensity of the PPT roll-out across municipalities, before and after the mass delivery of permits. Municipality fixed effects absorb any fixed difference between places, including the development gap that makes the cross-section uninformative, and year-month fixed effects absorb any national time trends, including the pandemic. What identifies the effect is the change in mortality in more-exposed municipalities relative to less-exposed ones, before and after the program roll-out.

For our estimation purposes, the timing of the post period follows the delivery of the physical permits rather than the announcement of the program. Although the program

[4] One municipality, a settlement of about 300 inhabitants that registered no deaths in 2017 to 2019, has no defined pandemic-severity control and drops from the regressions.

was announced in February 2021, physical PPT cards, the documents granting access to the health system, began to be issued at scale only in late January 2022, by the end of which about 12 percent of eventual permits had been delivered, and migrant health care utilization increased as the stock of issued permits accumulated sharply after January 2022. Accordingly, we define the post period as beginning in January 2022 and measure treatment exposure using the following time-invariant intensity:

$$PPT_m = 1000 \times \frac{\text{permits delivered}_m}{\text{population}_{m,2022}}, \tag{1}$$

standardized to mean zero and unit standard deviation across municipalities.

Specifically, we estimate the following equation:

$$y_{mt} = \beta \left(PPT_m \times Post_t \right) + \mathbf{X}'_{mt}\gamma + \alpha_m + \delta_t + \varepsilon_{mt}, \tag{2}$$

where m stands for the municipality and t denotes the year-month. y_{mt} represents the main outcomes of analysis, the monthly mortality rates described in Section 3. PPT_m is the standardized exposure measure of equation (1), and $Post_t$ is an indicator equal to one from January 2022 onward. $\mathbf{X}_{mt}$ is a vector of controls containing municipal COVID-19 severity, measured as standardized excess mortality in 2020 to 2021 relative to 2017 to 2019, interacted with year indicators to flexibly absorb the differential pandemic recovery correlated with exposure. α_m and δ_t are municipality and year-month fixed effects, respectively. Regressions are weighted by the relevant denominator, births for the infant and maternal rates, age-group population for the age-specific rates, and population otherwise. Standard errors are clustered at the municipality level to account for potential serial correlation within municipalities.

As such, β, our coefficient of interest, measures the change in the monthly mortality rate when exposure increases by one standard deviation, about 30 permits per 1,000 inhabitants, before and after mass permit delivery.

To assess pre-trends and dynamics we estimate the event-study given by the following specification:

$$y_{mt} = \sum_{\tau \neq 2019} \beta_\tau \left(PPT_m \times \mathbf{1}[t \in \tau] \right) + \mathbf{X}'_{mt}\gamma + \alpha_m + \delta_t + \varepsilon_{mt}, \tag{3}$$

with 2019, the last pre-pandemic year, as the omitted period. The event-study coefficients β_τ show whether more-exposed municipalities were already diverging before permits arrived, which is the check the difference-in-differences estimate cannot provide on its own.

Our estimates are valid as long as there are no time-varying covariates, not controlled for, that correlate with municipal exposure to the PPT and might affect mortality. Two of them are plausible here. Migrants settled where local economies were growing, so exposure could proxy for a favorable trend that predates the permits, and the pandemic hit municipalities unequally, so differential recovery could be confounded with exposure. We address the first by testing the pre-program coefficients of equation (3) and by allowing pre-shock municipal characteristics to follow their own trends, and the second by controlling directly for municipal COVID-19 severity interacted with year. We discuss these estimates in the next section.

5 Results

Table 2 illustrates our main results, and Figure 5 plots them as a percent of each outcome's mean so that the six outcomes can be read on one scale. The table reports the estimates of equation (2) for six outcomes: the overall mortality rate (column 1), infant mortality (column 2), maternal mortality (column 3), communicable-disease mortality (column 4), external-cause mortality (column 5), and mortality among adults aged 65 and older (column 6). Panel A measures each rate using all deaths registered in the municipality. Panel B restricts the numerator to deaths of Colombian residents, the population whose mortality captures the externality of interest. Each cell comes from a separate regression that includes municipality and month fixed effects, controls for COVID-19 severity interacted with year,

and standard errors clustered by municipality. Consistently, all the estimates show that municipalities more exposed to the PPT expansion experienced sharp reductions in mortality after January 2022, and that the declines concentrate in the causes of death that timely medical care can prevent.

Our preferred results are those in Panel B, which count only Colombian deaths, the circles in Figure 5. They suggest that a one standard deviation increase in exposure, equivalent to 30 permits per 1,000 inhabitants or roughly 3 percent of the local population gaining coverage, reduced overall mortality by 0.83 deaths per 100,000 inhabitants per month, infant mortality by 0.24 deaths per 1,000 live births, and mortality among adults aged 65 and older by 8.3 deaths per 100,000 adults in that age group. Relative to their means, these amount to declines of 1.8, 2.2, and 2.7 percent. These are meaningful magnitudes considering that the program reached a population that was never insured in Colombia to begin with, and that the deaths averted are those of people who already held coverage.

Maternal mortality falls by 3.9 deaths per 100,000 live births, or 7.0 percent, an estimate we treat as suggestive throughout given it is more sensitive to our robustness exercises. The estimates in Panel A, which use all deaths, are essentially identical to those in Panel B. This is expected because Venezuelans are young (Appendix Figure D.1): in 2024, the first year the records carry country of birth, Venezuelan-born individuals account for about one percent of all deaths, 2 percent of deaths at ages 45 to 64, and half a percent of deaths at ages 65 and older, so Panel B removes a handful of deaths per municipality from Panel A (Section 3). The overall, infant, maternal, and elderly effects survive correcting the false discovery rate across the six outcomes, with sharpened q-values below 0.01.

Migrants' own recorded deaths do not drive these gains. The series for Venezuelan residents shows no detectable effects on the main margins, with two exceptions, a significant decline at ages 20 to 44 and one in external causes, the margins where migrant deaths concentrate. Appendix Table B.1 reports these estimates, Panel C of Appendix Table B.2 the age-band estimate, and Appendix Figure B.3 the corresponding event studies.

The infant and maternal estimates need one more clarification, because births to newly

insured migrant mothers enter both outcomes. A legal rule and a feature of the death records help us interpret them. For infant mortality, children born in Colombia to Venezuelan parents since August 2015 are Colombian nationals under the *Primero la Niñez* measure (Congreso de la República de Colombia, 2019). Infant deaths therefore refer to Colombian children regardless of the mother's nationality. The infant estimate may capture both a direct effect on children in newly insured migrant households and an indirect effect operating through the health system shared with the rest of the population. The maternal outcome is different. Maternal deaths are classified using the mother's country of residence, as in the other mortality outcomes. Venezuelan-born women living in Colombia are therefore included in Panel B. In 2024, when country of birth becomes observable in the birth certificates, Venezuelan-born mothers account for 7.8 percent of births in the estimation sample. The maternal estimate in Panel B thus combines outcomes for Colombian-born and Venezuelan-born mothers residing in Colombia.

This mix raises a concern that we need to address. If Venezuelan-born mothers were more or less likely to die in childbirth than Colombian-born mothers, and if their share of births were larger where the program was larger, then the maternal rate in exposed municipalities could change simply because the mix of mothers changed, not because care improved. Both conditions would have to hold. The second does. Venezuelan-born mothers account for 2.5 percent of births in the least exposed quartile of municipalities and 12.1 percent in the most exposed. The first does not. On the birth outcomes we observe for both groups, low birth weight and preterm birth, Venezuelan-born and Colombian-born mothers are nearly identical, so changing the mix of mothers moves the municipal rates by less than two hundredths of a percentage point per standard deviation of exposure (Section 5.2; Appendix Table E.1 reports the birth outcomes by exposure). The maternal result is therefore not an artifact of who is giving birth. Because it is still the one outcome where the program changes who is counted, we report it as suggestive.

Effects by age profile. We next explore which age groups are driving the main results. Appendix Table B.2 reports the estimates of equation (2) separately for each age band, and

Appendix Table B.3 the corresponding descriptive rates. The gains concentrate in the ages where timely medical care makes the largest difference to survival.

Mortality declines among adults aged 45 to 64 and, and most prominently, among those aged 65 and older, where the post-period estimates fall below every pre-period point in the event study. Mortality among adults aged 20 to 44 also falls by between 2 and 3 percent of its mean, proportionally somewhat more than the infant effect. This is an age group where maternal, infectious, and other treatable acute conditions account for a large share of deaths, so it is where we would expect better access to care to show. By contrast, mortality at ages 1 to 19 does not move. This is the age group where deaths are mostly from external causes, accidents and violence, that health coverage cannot prevent, so it serves as a falsification test. The estimate is a precisely estimated zero. The same pattern holds when we restrict the analysis to Colombian residents.

Event studies results. The estimates so far pool the whole post-period into a single coefficient. To see when the decline began, and whether anything was already moving before the permits arrived, we estimate equation (3), which replaces the single post indicator with one coefficient per year. Figure 6 plots them for overall mortality, and Appendix Figures B.4 and B.5 report the same estimates by nationality and by age group. Each coefficient measures the difference in mortality between more- and less-exposed municipalities in that year, relative to the same difference in 2019, the omitted year. We chose 2019 as the reference because it is the last year before the pandemic, so the comparison is anchored in a normal year.

Before the program there were no differential trends. The coefficients for 2016 to 2018 are small, drift slightly upward, and are not statistically different from zero. The 2020 and 2021 coefficients rise. These are the pandemic years, shaded in the figure. More-exposed municipalities are urban and suffered about three quarters more COVID-19 mortality than less-exposed ones, 13.3 against 7.6 deaths per 100,000 over 2020 and 2021, which is why we control for pandemic severity throughout. The decline then appears in 2022, the year mass permit delivery began, and it persists through 2024. For the overall and elderly margins

every post-2022 coefficient lies below every pre-pandemic one. If anything, the slight upward pre-trend works against us, since a continuation of it would have raised mortality in exposed municipalities; the estimates that allow for such a trend, in Appendix Table C.1, are larger than our baseline.

5.1 Assessing the size of the effects

The estimates suggest that covering an additional 3 percent of the local population reduced the mortality rate of everyone by 1.8 percent. For a city of 100,000 inhabitants, this is about 0.8 fewer deaths per month, or 10 fewer deaths per year. Applying the estimated marginal effect linearly to the full covered population, the program's 1.96 million permits imply roughly 6,500 deaths averted per year, about one life saved annually for every 300 migrants covered, or on the order of 19,500 lives over 2022 to 2024.

To understand the size of these effects, a useful benchmark comes from the literature on public insurance expansions in the United States. Currie and Gruber (1996) find that the large Medicaid eligibility expansions of the 1980s reduced child mortality by about 5 percent, of the same order as our infant estimate though roughly twice as large, and Miller et al. (2021) estimate that the ACA Medicaid expansions reduced annual mortality among low-income near-elderly adults by roughly 9 percent.

Developing-country reforms give the same anchor from the direct side, Thailand's universal scheme implied infant mortality reductions of 13 to 30 percent, which the authors read as suggestive, concentrated among the covered poor (Gruber et al., 2014), about 10 percent in poor municipalities under Mexico's Seguro Popular, with no significant effect overall (Conti and Ginja, 2023), and 6 to 20 percent under Brazil's Family Health Program (Rocha and Soares, 2010). Our estimated declines among natives are smaller than each of these direct-effect benchmarks, as one would expect for spillover effects.

The most informative estimate is the effect per covered migrant, roughly one native life saved annually for every 300 migrants gaining a permit. Scaling instead by enrollment

actually observed rather than by permits issued, using the enrollment coefficient in Appendix Table D.1, gives one life for roughly every 150 people enrolled, since a standard deviation of exposure raises enrollment by about 1,450 people per 100,000 inhabitants while averting about 9.9 deaths per 100,000 per year. We report the permit-based figure in the text because permits are the policy instrument, and note that it is the conservative of the two. This is of the same order as the direct effect per enrollee in Miller et al. (2021), whose estimates imply roughly one life saved per 290 enrollment-years among low-income near-elderly adults, even though our effect operates on people who already held insurance. The difference reflects the mechanism.

A spillover operating through shared health system capacity is not constrained by the covered individual's own mortality risk. Because migrants are young, their baseline mortality is low. Yet, when coverage brings their care into routine services with regular financing, it frees emergency capacity for native patients, whose baseline mortality is substantially higher. As a result, relatively small reductions in emergency congestion can generate comparatively large reductions in native mortality. For instance, among the patients affected by an English waiting-time target, a roughly twenty-minute reduction in time spent in the emergency department cuts thirty-day mortality by fourteen percent (Gruber et al., 2023).

A simple back of the envelope exercise illustrates that the decongestion we measure is large enough to produce the mortality decline we estimate. The exercise has four steps. First, one standard deviation of exposure reduces Colombians' emergency visits by 7 percent (Figure 7, Panel B), a level decline that is imprecisely estimated and that we use here only to fix an order of magnitude. Second, if time spent in the emergency department falls with patient volume, with an elasticity between one half and one and a half, emergency patients' time falls by 3.5 to 10.5 percent. Third, applying the elasticity implied by the English estimates, a one percent reduction in emergency-department time lowers emergency-patient thirty-day mortality by about 1.2 percent (Gruber et al., 2023), mortality among emergency users falls by 4 to 12 percent. Fourth, this reduction applies only to deaths that pass through emergency care, namely 8.6 monthly visits per 1,000 inhabitants times a thirty-day mortality per visit of 1 to 2.5 percent, or 9 to 22 deaths per 100,000 per month. Multiplying

through, the mechanism predicts a decline of 0.4 to 2.7 deaths per 100,000 per month, with a central value near 1.3. Our estimate of 0.8 lies inside this range, toward its conservative end. The congestion relief we document, valued with externally estimated elasticities, is therefore quantitatively sufficient to account for the estimated mortality effect. The exercise is illustrative, its parameters come from the English health system and the volume-to-time elasticity is assumed rather than estimated, which is why we report ranges rather than a point prediction.

The simple accounting exercise also provides an informative benchmark. At the subsidized regime capitation payment of 1.26 million pesos per enrollee per year (Ministerio de Salud y Protección Social, 2023), insuring 300 migrants costs approximately USD $93,000 annually. Even if the entire cost is attributed to the external mortality benefit for natives, with no credit given to the substantial health gains experienced by the migrants themselves, the implied gross cost per native life saved per year is roughly two orders of magnitude below the value of a statistical life used in U.S. regulatory analysis, 13.2 million dollars in the 2024 guidance, expressed in 2023 dollars (U.S. Department of Transportation, 2024), and remains an order of magnitude below income-adjusted value-of-statistical-life estimates for Colombia (Viscusi and Masterman, 2017).

Two further features distinguish our estimates from previous benchmarks. First, the American estimates measure effects on the people who gained coverage, whereas ours measure effects on a population that was already insured. The PPT generated declines among natives of the same order as, though smaller than, what flagship insurance expansions achieve for their direct beneficiaries, which is the sense in which the externality is large. Second, the direct beneficiaries here are a marginalized non-citizen minority, so the gains accrued to a host population that never enrolled in anything. In the spirit of Miguel and Kremer (2004), the untreated captured a substantial share of the program's health returns.

5.2 Robustness tests

We assess the robustness of our results along several dimensions. We present two exercises in the main text because they directly address the central identification concern and the proposed mechanism, respectively. These examine the role of the pandemic and the behavior of the comparison outcomes. The remaining checks appear in Appendix C, Tables C.1, C.4, and C.5.

Effects are not driven by the COVID-19 pandemic. Table C.2 and the final rows of Appendix Table C.1 assess the role of the pandemic by re-estimating the mortality outcomes after excluding deaths coded as COVID-19. The overall, infant, and elderly estimates remain negative and statistically significant, although the overall and elderly effects are roughly halved once COVID-19 deaths are removed.

The maternal estimate becomes smaller and only marginally significant.[5] Table C.3 replaces the linear COVID-19 severity control with municipality pandemic-severity-bin-by-month fixed effects, absorbing any mortality trajectory common to municipalities with similar pandemic intensity and identifying the treatment effect solely from exposure variation within severity bins. The overall and infant estimates are virtually unchanged, the elderly estimate attenuates by about a fifth, and all three remain statistically significant, indicating that the results are not driven by the functional form of the COVID-19 control. The findings also remain robust when this nonparametric specification is combined with the exclusion of COVID-19-coded deaths.

Mortality patterns are consistent with improved access to care. Two comparison outcomes help assess the potential mechanisms explaining the effects (Table 2, columns 4 and 5). External-cause mortality declines by a small and statistically insignificant 0.10 relative

[5]We view this exercise as a robustness check rather than a preferred specification because excluding COVID-19 deaths mechanically distorts the pre-treatment period, since the pandemic's harvesting of frail individuals lowers subsequent non-COVID mortality in the most exposed municipalities beginning in 2020. Our baseline specification therefore retains all deaths while controlling for municipal COVID-19 severity.

to a mean of 5.2, with a confidence interval ruling out declines larger than about 5 percent of the mean. This outcome provides an imperfect placebo because survival after serious injuries may also depend on the emergency services that the PPT potentially decongests. Nevertheless, mortality declines are concentrated in, and statistically detectable only for, causes for which timely medical care is particularly important.

Communicable-disease mortality also shows a precisely estimated null effect, with confidence intervals excluding declines greater than about 3 percent of its mean. Together, these results support an interpretation centered on improved access to healthcare rather than broader changes affecting all causes of death. The null effect on communicable-disease mortality provides little support for reduced disease transmission as the main channel. Although it does not rule out changes in infection incidence, it is consistent with the PPT benefiting Colombians by relieving pressure on the health system, a mechanism we examine in Section 6.

Additional robustness. Section C.2 of Appendix C presents the remaining checks in Tables C.1, C.4, and C.5. These include difference-in-differences estimators robust to treatment effect heterogeneity, sensitivity analyses assessing the departures from parallel trends needed to overturn the results, alternative specifications and samples, controls for the determinants of exposure and differential trends associated with thirteen municipal characteristics measured before the shock, and tests of composition and measurement. The estimates for overall and elderly mortality remain stable throughout. The evidence for infant mortality is less robust, while the maternal mortality results remain suggestive. The 2024 records, the first to identify country of birth, also show that Venezuelan-born individuals account for a negligible share of recorded adult deaths, indicating that the adult mortality declines primarily reflect reductions among Colombians. Maternal mortality is the exception: migrant mothers account for a share of deaths that varies with treatment exposure, providing an additional reason to interpret this result cautiously.

6 Mechanisms

6.1 Efficient use of health services and regular payments

Our main hypothesis is that extending coverage relieved congestion in the health system for everyone, through two main changes: more efficient utilization of the health services by migrants and timely reception of regular payments by health providers.

More efficient utilization of health services by migrants.— First, since migrants no longer needed to wait for a grave health situation to go to emergency services, their health care utilization expanded predominantly in routine and preventive services. The data supports this argument. In fact, a one standard deviation increase in exposure to the PPT program (about 30 permits per 1,000 inhabitants), raises migrant outpatient encounters by 88 percent of their mean and lowers the emergency share of migrant care by 12 percent (Figure 7, Panel A); within the emergency room, migrant visits for avoidable, ambulatory-care-sensitive conditions fall and the admission probability of the remaining visits rises, as a switch to routine care predicts (Appendix Table D.9). Migrant emergency visits do not fall in levels, however, so what we observe is a shift in the composition of care: migrant care grew in routine services and fell in emergency use per covered migrant.

Timely and regular payments to providers.— Second, the providers treating migrants began to be paid timely and regularly per additional person insured, rather than late and partially for emergency episodes. Before the program, an irregular migrant could be treated only in an emergency room, and the provider was reimbursed by the government after the fact, at emergency tariffs, and often only in part. After it, each covered migrant carried a scheduled capitation payment, into the same providers that serve Colombians. A provider with predictable revenue and fewer avoidable emergency visits can treat everyone with less delay, and delay is what kills the patients in the middle of a health emergency (Francetic et al., 2024).

Colombia's health system pays insurers automatically a fixed annual amount for every

person enrolled, the *Unidad de Pago por Capitación* (UPC), and insurers pay providers from it, so every new enrollee in the affiliation registry is by construction a new UPC payment flowing to the providers of the municipality where that person is enrolled. In Colombia, therefore, an official health enrollment is automatically translated into its per capita payment.

In practice, we observe that an additional standard deviation PPT exposure (approximately 30 permits per 1,000 inhabitants), raises official health enrollment by 14.6 per 1,000 inhabitants (Figure 4), which means that 14.6 additional UPC payments per 1,000 inhabitants began arriving, every year and in advance, at the providers serving exposed municipalities, in place of the late and partial reimbursement of emergency episodes that preceded them. This is the flow of resources reaching the system exactly where migrants registered.

Although we do not observe what providers did with the additional funds, two data trends point to the answer. First, Colombians' outpatient consultations and procedures rose by 4 and 7 percent (Figure 7, Panel B), which a reallocation of migrants alone cannot produce, and instead, is more aligned with a better-financed provider pool. The second trend is that the mortality decline is larger where the health system had more public money to begin with. In municipalities one standard deviation above the mean in public health transfers per person, the decline in the overall mortality rate is close to twice as large, and so is the decline in elderly mortality (Table 3). This difference between better- and worse-funded municipalities appears only after mass delivery, not before, which is what a program effect looks like rather than a pre-existing difference (Appendix Figure D.3). The timing test holds for overall mortality.

The consequence of both a more efficient use of the health services by migrants and regular and timely payments to providers predict what we observe. Colombians use more outpatient consultations and routine procedures, whereas the emergency share of all Colombian care falls by 7 percent (Figure 7). Moreover, the deaths that fell are the ones of a less congested emergency room. In emergency rooms acute emergencies get triage to first priority, so in that margin we should see little change in mortality. However, in line with this hypothesis, we see a reduction in mortality in those emergency cases that are less acute and might have been

postponed to a longer wait in triage on a congested emergency room. Figure 8 illustrates this pattern in two independent populations. Among the elderly, deaths from ambulatory-care-sensitive causes fall 3.7 percent while deaths from acute, emergency-dependent causes such as heart attacks and strokes are a precise zero (Figure 8, Panel A); among infants, deaths amenable to postnatal care fall 7.3 percent and carry four fifths of the infant decline, while deaths determined at or before birth do not move (Panel B). Pooling the two margins, the decline in care-amenable causes exceeds that in non-amenable causes by about four percentage points ($p \approx 0.01$). On the maternal margin, mortality falls most where births had previously been most likely to occur outside health facilities, which is where newly insured mothers gained institutional maternity care (Appendix Table D.10; Appendix Figure E.1 and Table E.1 report the corresponding decline in uninsured births). Changes in income, crime, or disease transmission cannot explain why mortality caused by heart attacks does not change, while that caused by pneumonia is reduced.

6.2 Ruling out other potential mechanisms

In this subsection we provide evidence that the mortality declines among Colombians are not driven by higher local incomes, by lower crime, by less disease transmission, or by migrants joining the health workforce. We also provide evidence that the expansion of health services to Venezuelans was not reflected in the displacement of Colombians from the health system.

Higher local incomes.—Considering that the PPT permits also granted the right to work, the mortality declines that we identify could reflect a local economic boom. Regularization raised migrants' earnings and consumption (Ibáñez et al., 2025), and richer households spend more in the local economy, which could translate in indirect positive effects in health outcomes for Colombian nationals. We examine the validity of this argument by estimating the effects of the program on local economic activity and Colombians' employment and earnings. Our estimates indicate that economic activity, which we proxy through night light density, does not rise after 2022 with the number of permits per 1,000 inhabitants, and is if anything slightly lower. Additionally, we also show that municipal tax revenues do not change with

the program roll-out (Appendix Table D.4, Panel A), and that, among Colombian workers in the twenty-five main cities, employment, formality, and labor income were also not affected by the PPT program roll-out (Appendix Table D.4, Panel B).

Lower crime.—Earlier research showed that a smaller version of the PPT program called the PEP, which granted the same benefits but only reached approximately 280 thousand migrants in Colombia, successfully reduced crime and raised crime reporting by migrant women (Ibáñez et al., 2026). If these effects scaled, the PPT program may induce safer municipalities with fewer deaths. If this channel explains the reductions we document in mortality rates we would have to see that deaths from external causes, and homicides in particular, should fall where more permits per 1,000 inhabitants were delivered. The evidence does not support this hypothesis. External-cause mortality among Colombians changes by a statistically insignificant 0.10 deaths per 100,000 per month relative to a mean of 5.2, and the confidence interval rules out declines larger than about 5 percent of that mean (Table 2, column 5). Additionally, homicide deaths show no response to the program (Appendix Table D.5).

Less disease transmission.—Venezuelan migrants arrived with interrupted vaccination schedules and untreated infections (Ibáñez et al., 2021), so insuring them could have lowered Colombians' mortality by reducing contagion rather than by changing how care is delivered. If this channel waa related to the mortality decline we document, its reduction should concentrate in communicable diseases and, among infants, in infectious causes. We find no support for this hypothesis in the data. First, communicable-disease mortality among Colombians is unchanged by the program, with a confidence interval that excludes declines larger than about 3 percent of its mean (Table 2, column 4). Across the cause-specific estimates, the only infectious cause that declines is influenza and pneumonia, by 4.6 percent (Appendix Table D.6), and pneumonia is the infectious cause most responsive to being treated in time. COVID-19 mortality also declines, by 13.4 percent (Appendix Table D.6), but its interpretation mixes the unwinding of the pandemic with better protection, which is why Table C.2 re-estimates every outcome excluding COVID-coded deaths; all the effects survive this correction. Second, infectious causes account for only 8.5 percent of infant deaths and their

estimate is a null, while deaths amenable to postnatal care carry four fifths of the infant decline (Table D.7, Panel B). Third, childhood vaccination did not rise. For five of the six core childhood vaccines, the doses applied change by less than one percent where more permits per 1,000 inhabitants were delivered, while the immunization program's target population grows by 1.8 percent, so the coverage ratios the program reports fall by about one dose per 100 children, on a mean of 90, only because migrant children enlarge the denominator (Appendix Table D.8). The exception is BCG, the vaccine given at birth, whose doses rise by 4.7 percent as more births take place in health facilities (Appendix Table E.1).

A larger health workforce.—Regularization allowed migrants to practice as health professionals, so part of any added capacity could have been supplied by migrants themselves rather than by better-financed providers. We do not find evidence for this channel. The national registry of health professionals (ReTHUS) recorded 1,452 registered professionals holding a PPT by 2024. This accounts for only seven tenths of one percent of Colombia's physicians. So even if every one of them were a physician, the supply shift would be too small to account for effects of the size we identify.

Crowding out of Colombians from insurance.—Finally, a natural objection to a congestion story is that adding two million people to a system financed by a fixed budget could have pushed Colombians out of insurance, and a Colombian who lost coverage would use the emergency room exactly as an uninsured migrant did before the program. If this had happened, Colombian enrollment should fall where PPT enrollment rose most. The data does not support this hypothesis. The affiliation registry records every affiliate by identification document, so we can measure Colombian enrollment on its own. In the municipalities where enrollment of PPT holders rose by 63 percent of its mean, Colombian enrollment actually increased by 0.05 percent of its mean (Figure 7, Panel C; Appendix Table D.1). We do observe, however, that there is a small reshuffling of Colombians between the subsidized and the contributory regimes, of about one percent each way, which changes who pays for a person's coverage but not whether the person is covered, since both regimes give access to the same providers.

Taken together, the evidence rejects the mechanisms operating outside the health system. What remains are changes operating inside it, migrant care moving from emergency into routine services and being paid for timely and regularly.

7 Fiscal Cost per Life Saved

This section estimates the net fiscal cost per Colombian life saved by subtracting savings in publicly funded emergency care for migrants from the annual cost of extending coverage to nearly two million migrants and dividing the resulting net cost by the estimated number of Colombian lives saved. Previous studies document the broader fiscal and economic benefits of regularization (Ibáñez et al., 2025; Rozo, 2026) and the cost-effectiveness of coverage based on migrants' own health gains (Espinosa et al., 2025), but do not account for health benefits to the host population. Our exercise captures this mortality externality while excluding migrants' own health gains, additional fiscal revenues from regularization, savings in emergency care for Colombians. The benefits considered are therefore a lower bound. Table 4 presents the results, and Appendix F documents all inputs, sources, and formulas.

Costs. Covering 1.96 million migrants requires the government to pay one annual capitation payment per enrollee, equivalent to the 2024 subsidized-regime UPC of 1.26 million pesos (about 309 US dollars), for a total annual cost of 605 million US dollars. However, much of this spending replaced costs the government was already incurring. Colombian law requires emergency rooms to treat all patients regardless of nationality or migratory status, so irregular migrants were already entitled to emergency care before the program. Emergency-only care is also substantially more expensive than regular insured care. Ibáñez et al. (2025) price the emergency-only regime at 898 dollars of 2020 for a four-member household. Carried to 2024 prices and to a per-person basis as Section F.1 of Appendix F sets out, that is 272 dollars per migrant per year, against about 167 dollars under full insurance coverage.

This difference explains why regularization reduced, rather than increased, the per-migrant health cost of Colombia's first-generation regularization program; our accounting is more conservative, since it charges the statutory capitation payment rather than the lower observed cost of full access. To calculate the additional fiscal cost of the PPT, we subtract the emergency spending that the government would have incurred even in the absence of the program. We consider two scenarios that parallel the *de jure* and *de facto* assumptions in Ibáñez et al. (2025). The *de jure* scenario assumes that every covered migrant would have exercised the legal entitlement to emergency care, implying net new health spending of 72 million US dollars per year. Our preferred *de facto* scenario instead uses observed emergency care utilization, applying the emergency-regime cost only to the roughly 850,000 migrants (43 percent of those covered) who used health services at baseline according to the 2021 administrative records. Under this more realistic assumption, net new health spending is 374 million US dollars per year.

Mortality benefits. We count only the external benefit to the host population, namely the roughly 6,500 Colombian deaths averted each year under the illustrative aggregation described in Section 5. We value each life using a Colombian value of a statistical life of $1 million USD, obtained by transferring the United States estimate using the income elasticity proposed by Viscusi and Masterman (2017). Restricting the analysis to native lives avoids double counting because the direct health benefits to migrants are already included in the earlier fiscal accounting exercises. The valuation is also conservative. Even if the value of a statistical life were only one tenth as large, the conclusions remain unchanged.

Net cost. The main conclusion does not depend on the scenario considered. Annual mortality benefits of 6.5 billion US dollars exceed net health spending by one to two orders of magnitude depending on the scenario. The net fiscal cost per Colombian life saved is $11,000 USD under the *de jure* scenario and $58,000 USD under our preferred *de facto* scenario. Both figures are an order of magnitude below the value of a statistical life. The resulting annual net social benefit ranges from $6.1 to $6.4 billion USD.

These estimates are conservative for three reasons. They assign no value to migrants' own health gains, give no credit for the additional fiscal revenues generated by regularized migrants, and exclude the savings from the emergency care that Colombians no longer require because congestion in the health system declines, the mechanism documented in Section 6. Even if the number of deaths averted were cut in half, the cost per life saved would simply double and the qualitative conclusions would remain unchanged. More broadly, the fiscal case for regularization was already positive before assigning any value to the health benefits experienced by natives. Accounting for those external benefits strengthens the conclusion further. A program that is fiscally sustainable under conventional accounting generates even larger social returns once the positive health externalities for the host population are taken into account.

8 Conclusion

Every health system excludes some share of the people living under it, and in most of them the excluded keep a right to emergency care alone. This paper shows what happens when that arrangement ends. When Colombia switched nearly two million Venezuelan migrants from emergency-only care to full health coverage, mortality among Colombians fell. Two features of the program explain why. First, an irregular migrant who used the emergency system before the permits was taking a risk that only the very sick would take, so migrants were entering the system where it was most costly and at the point where care was least likely to help; fewer than half of those eventually covered appear in the records at all. Coverage brought migrant care into clinics, with a regular payment attached to each patient, and brought the rest of the migrants into the system for the first time. Second, regularization arrived not only with access but with financing. A registered person carries a capitation payment, so providers were paid promptly for routine care rather than late for emergency care. The services Colombians share decongested, and the deaths that fell are the ones timely treatment prevents.

Any country in which an excluded population is entitled to emergency care only, and uses even that reluctantly because contact with the system is dangerous for them, faces the same decision Colombia faced. Our evidence suggests that offering that population full coverage, coupled with financing that follows the person, can lower mortality not only among the newly covered but among everyone who shares the system with them. Governments weighing that decision count the cost of covering the newly insured against the health those people gain. The benefit to everyone else does not appear, not because anyone believes it is zero, but because it had never been measured. In Colombia it was large enough to justify the program by itself, in a system with the capacity to absorb the redirected demand; where none exists, the same policy may only move the queue.

For countries receiving forcibly displaced populations, the finding adds to a body of evidence that points consistently in one direction. Regularization improved migrants' well-being and health, raised their formalization, and built the trust and belonging that integration depends on (Ibáñez et al., 2025; Rozo, 2026). It reduced crime (Ibáñez et al., 2026). It left native labor markets essentially unaffected (Bahar et al., 2021). What it had not been shown to do is help the host population directly, and this paper shows that it does. Through the health system, allowing the displaced to use services on the same terms as everyone else saved Colombian lives. The concern most often raised about inclusion is what it costs the people already there. Here the answer is that it made them safer.

Colombia took a considerable political risk in regularizing migrants who amount to four percent of its population and giving them full access to its services. More work is needed on what happens over the medium and long run, as these migrants settle and their children grow up in the country. What the experiment already shows is that self-reliance is a hosting model that works for the migrants and for the communities that receive them.

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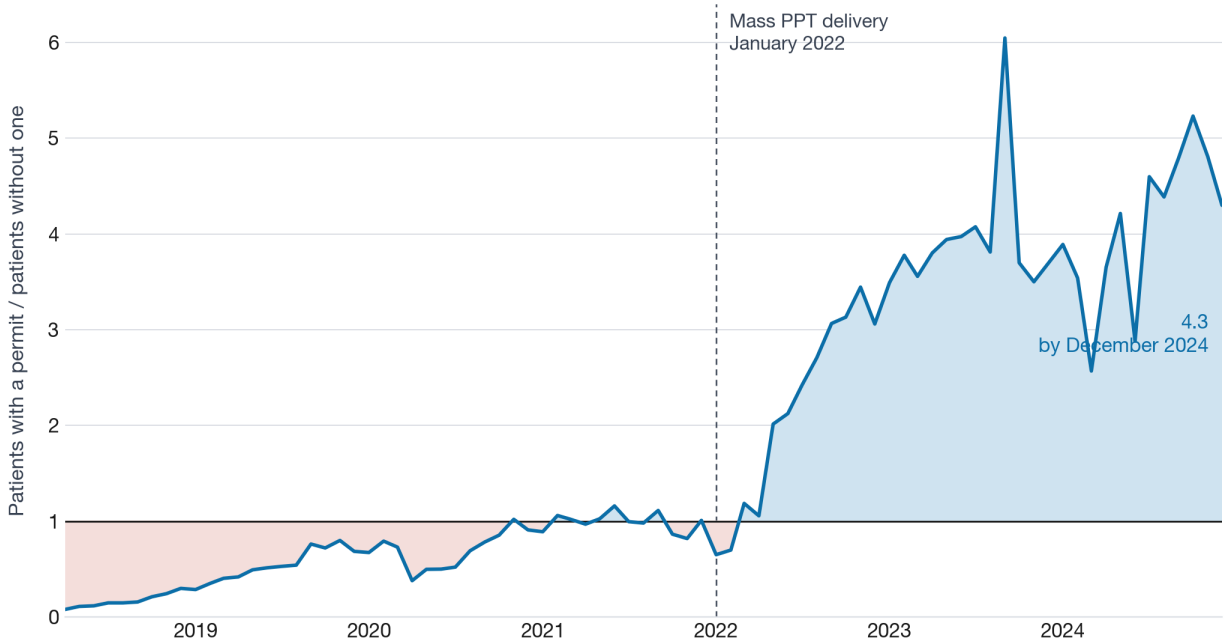
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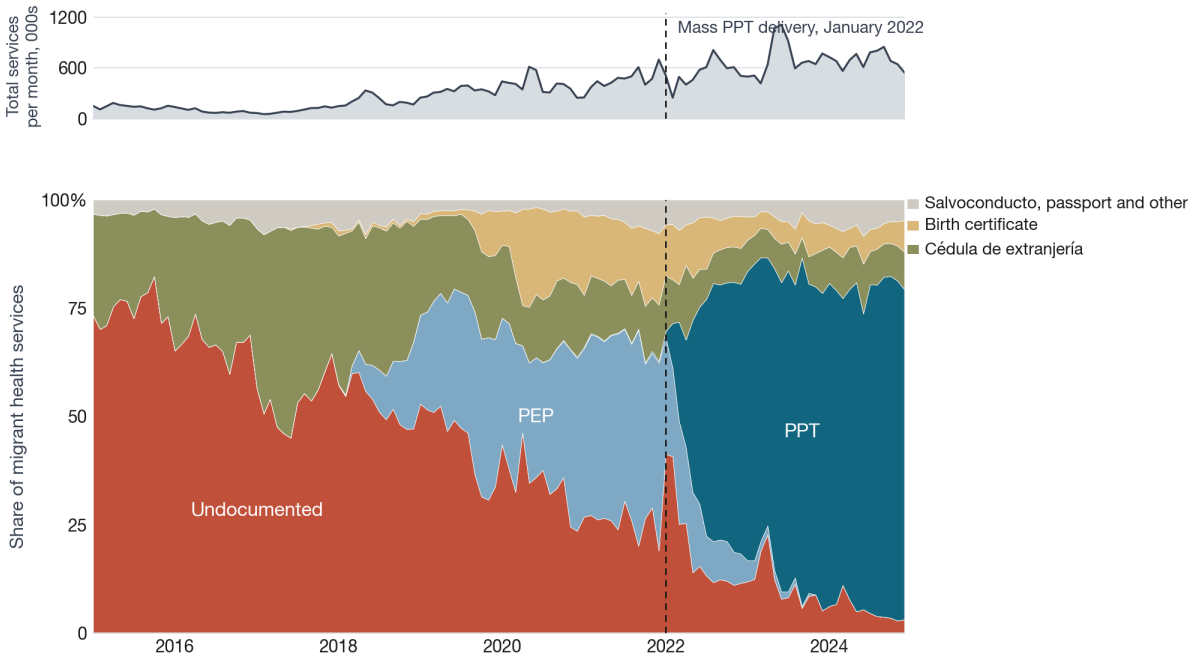
Figures and Tables

Figure 1: Ratio of Regularized to Non-Regularized Migrant Care



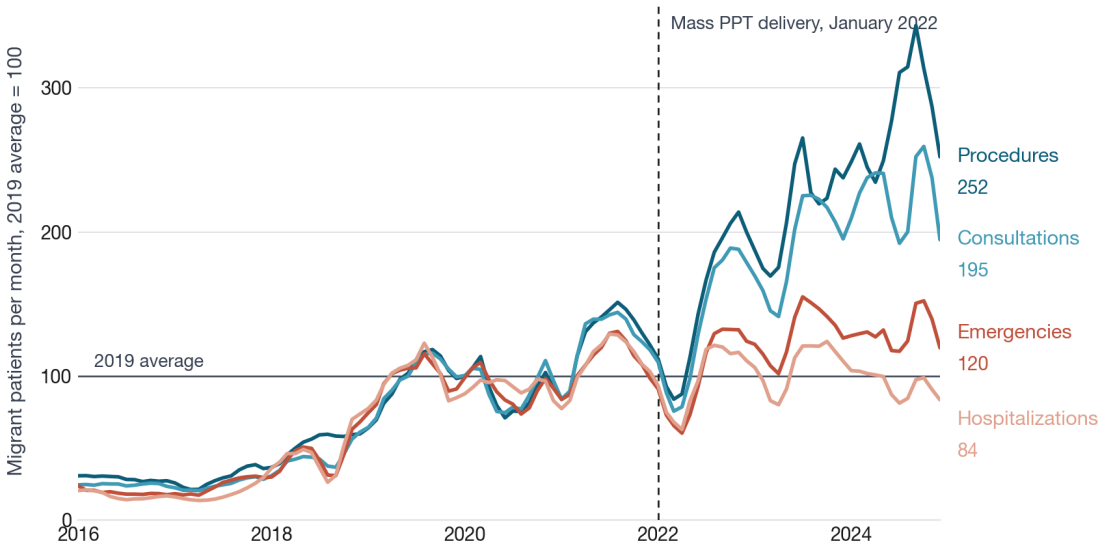
Notes: The series divides the number of distinct migrant patients holding a regularization permit (PEP or PPT) in each month by the number of distinct migrant patients holding any other document, that is, undocumented users and holders of remaining document types. It counts people rather than visits, so it describes how migrant patients divide between the two groups and not the intensity of care each group receives. The horizontal line at one marks the point where the two groups are equal in number. The shading changes color there, from months in which fewer migrant patients held a permit than did not, to months in which more did. The series begins in April 2018 because the first permit, the PEP, was created in late 2017 and there were almost no holders to count before then. The dashed vertical line marks the beginning of mass PPT delivery in January 2022. The ratio climbs from 0.08 in April 2018 as the PEP was taken up, reaches one during 2021, and rises steeply again after mass PPT delivery, reaching 4.3 by December 2024, a kink rather than a level jump, consistent with the gradual delivery of permits. Series end in December 2024, the last month of the analysis window; RIPS reporting is incomplete for later months. Source. RIPS, Ministry of Health.

Figure 2: Composition of Migrant Health Services. All Document Categories



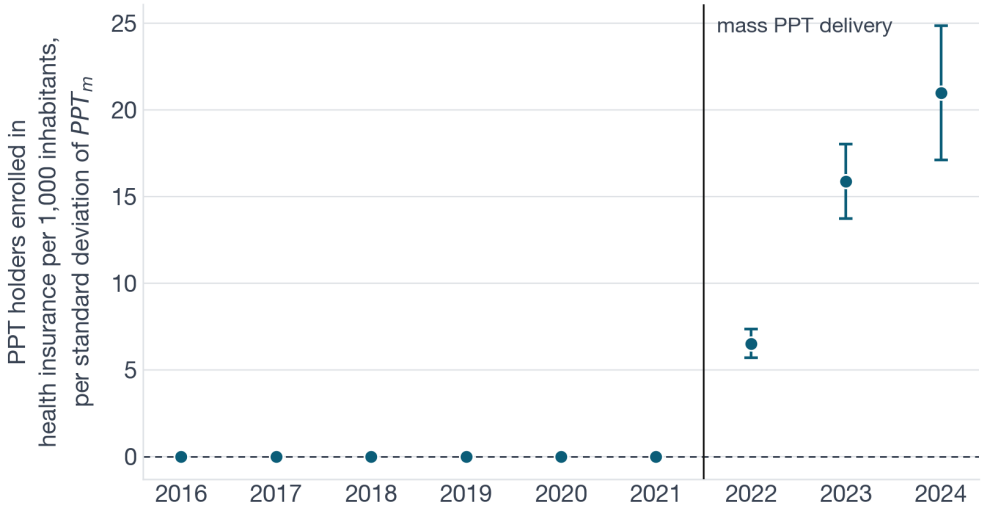
Notes: Monthly health services provided to migrants, disaggregated by the document under which each service was recorded. The upper strip plots total services per month in thousands. The lower panel plots the share of those services accounted for by each document category, so the bands sum to 100 percent in every month. Categories are mutually exclusive and comprise holders of the *Permiso por Protección Temporal* (PPT, the ETPV's ten-year permit), holders of the *Permiso Especial de Permanencia* (PEP, the predecessor permit), undocumented migrants (users without valid identification, recorded as unidentified adults and minors or undefined documents), *cédula de extranjería* holders (foreigners with regular residency), users recorded under a civil birth registration (children of migrants born in Colombia), and a residual band combining *salvoconducto* holders (asylum seekers awaiting resolution) with passport and remaining categories (short-stay entrants), which account for 2.2 and 2.5 percent of services over the sample as a whole, though each is larger in individual months. Section 3 defines each category. The three largest bands are named within the figure and the three smallest are keyed in the right margin, at the height at which each ends. The dashed vertical line marks the beginning of mass PPT delivery (January 2022). Series end in December 2024, the last month of the analysis window; RIPS reporting is incomplete for later months. Source. RIPS, Ministry of Health.

Figure 3: Migrant Health Care by Type of Service

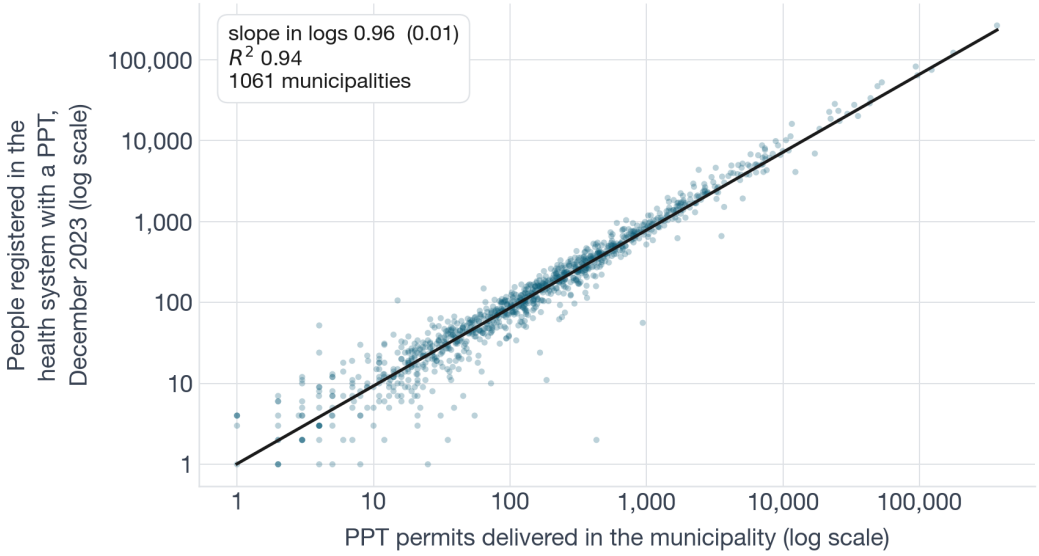


Notes: Monthly migrant patients receiving each type of care, all non-Colombian document categories including the undocumented, indexed to the series' 2019 average (=100), three-month moving averages, 2016 to 2024. The dashed vertical line marks the beginning of mass PPT delivery (January 2022). All four series move together through the arrival wave; after mass delivery, consultations and procedures average roughly twice their 2019 level and peak at two and a half to three and a half times it, while emergencies average about a fifth above 2019 and hospitalizations average their 2019 level, the compositional switch from acute to routine care that Section 6 exploits. Series are labeled at the right with their final value. Series end in December 2024, the last month of the analysis window; RIPS reporting is incomplete for later months. Source. RIPS, Ministry of Health.

Figure 4: First Stage. PPT Exposure and Health-System Enrollment



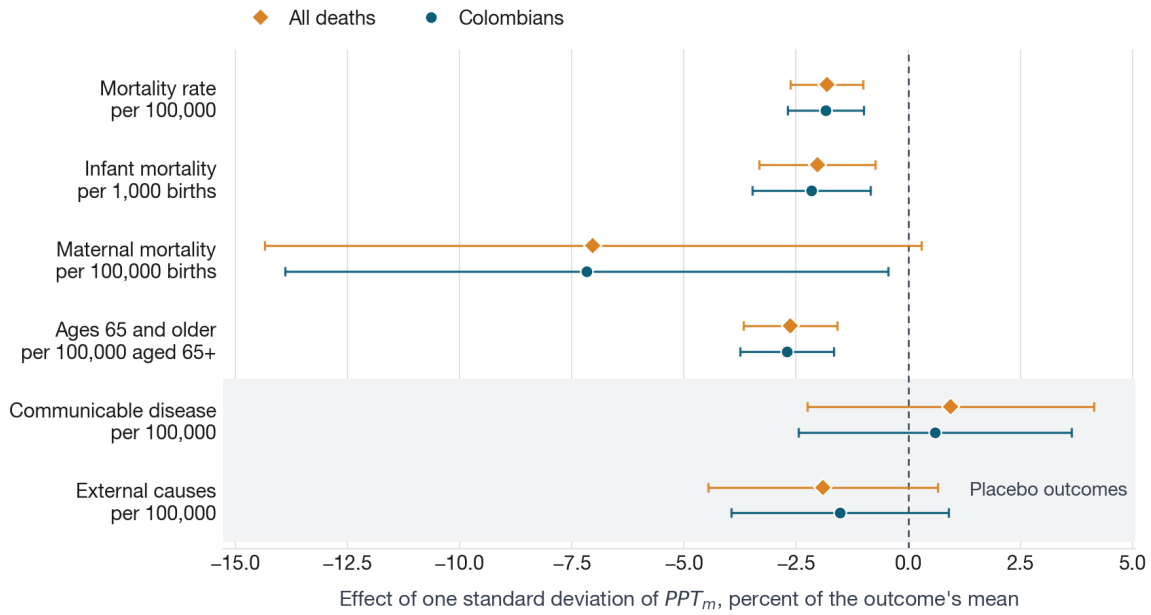
Panel A. Enrollment of PPT holders in health insurance, by year



Panel B. Enrollment against permits delivered, across municipalities

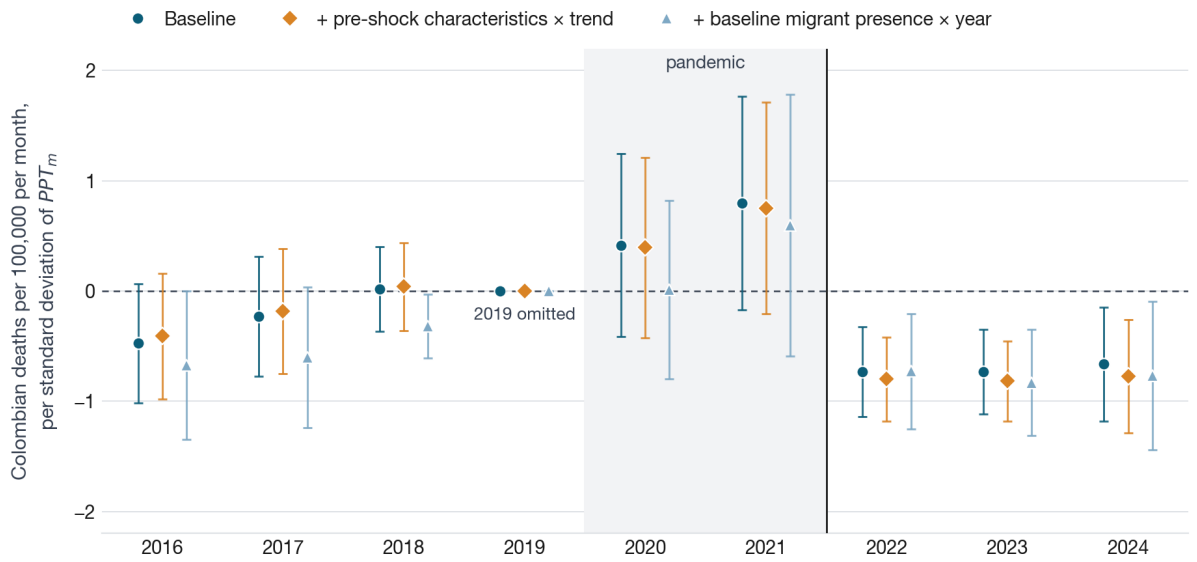
Notes: Enrollment is the monthly stock of people affiliated to the Colombian health system (SGSSS) holding a *Permiso por Protección Temporal*, from the BDUA affiliation registry, by municipality of affiliation. Panel A plots the coefficients on PPT_m interacted with year indicators, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, from a regression of PPT holders enrolled per 1,000 inhabitants with municipality and month fixed effects and COVID-19 severity × year controls, weighted by population, with 2021 omitted and standard errors clustered by municipality; bars are 95% confidence intervals. The pre-program coefficients are indistinguishable from zero because the permit did not exist before 2021. Panel B plots, for each of the 1,061 municipalities with a positive count of each, the number of people registered in the health system with a PPT in December 2023 against the number of PPT permits delivered there, both on logarithmic scales; the fitted slope in logs is 0.96 (robust s.e. 0.01, $R^2 = 0.94$), so enrollment rises almost one for one with permits. Sources. BDUA (Ministry of Health, SISPRO) and ETPV municipal registration records (Migración Colombia).

Figure 5: Difference-in-Differences Estimates. PPT Exposure and Mortality, Effects Relative to the Mean



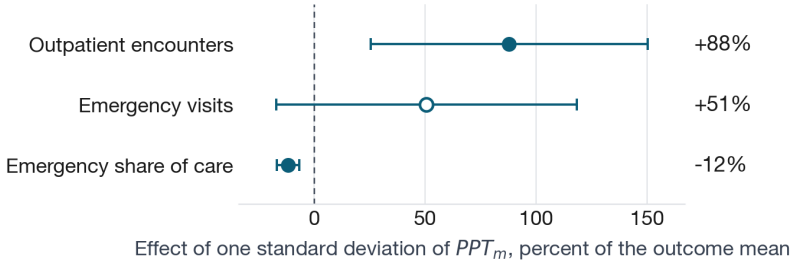
Notes: The figure plots the estimates of Table 2. Each marker is the coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and $Post_t$ equals one from January 2022 onward, from a separate municipality-month regression, 2016 to 2024, divided by the mean of the outcome and multiplied by 100, so that outcomes measured in different units share one axis; bars are 95 percent confidence intervals on the same scale. Diamonds use all deaths registered in the municipality (Table 2, Panel A) and circles only deaths of Colombian residents (Panel B). The shaded rows are the two placebo outcomes, deaths that health coverage should not affect. All regressions include municipality and month fixed effects and COVID-19 severity × year controls, are weighted by births (infant, maternal), population aged 65 and older (elderly), and population otherwise, and cluster standard errors by municipality. Source. DANE EEVV, population projections, and birth statistics; Migración Colombia ETPV registrations.

Figure 6: Event Studies. Overall Mortality and PPT Exposure

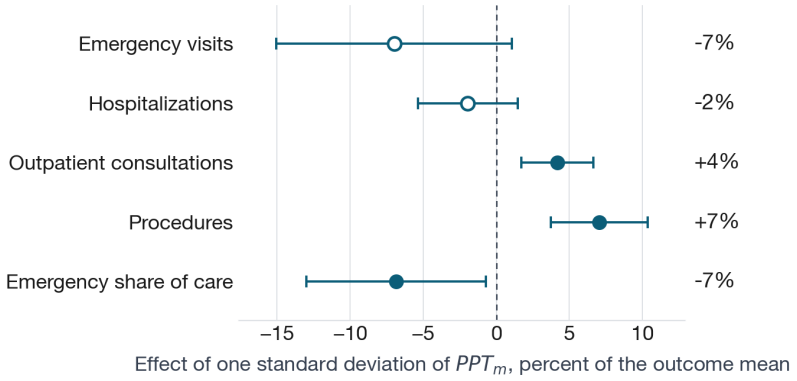


Notes: Yearly coefficients on the interaction between PPT_m, PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and year indicators, for the overall mortality rate per 100,000 inhabitants per month. Municipality and month fixed effects throughout, with municipal COVID-19 severity interacted with year indicators (severity is standardized excess mortality in 2020 to 2021 relative to 2017 to 2019). 2019, the last pre-pandemic year, is the omitted period, so the reference lies outside the pandemic; the shaded band marks 2020 and 2021. Three specifications are shown at each year: the baseline, the baseline adding thirteen pre-shock municipal characteristics interacted with a linear trend, and the baseline adding 2018 migrant patients per 1,000 inhabitants interacted with year indicators. Regressions are weighted by population and standard errors are clustered by municipality; bars are 95% confidence intervals. Coefficients measure the change in the mortality rate per one standard deviation of exposure relative to 2019. The remaining outcomes, for Colombians, are in Appendix Figure B.6. Source. clean municipal panel (DANE EEVV, population projections, births; Migración Colombia ETPV registrations).

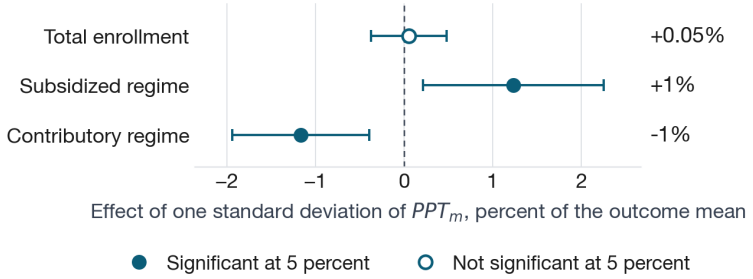
Figure 7: Care Use and Insurance Enrollment. PPT Exposure Effects Relative to the Mean



Panel A. Migrant care: more of it, and a smaller share in the emergency room



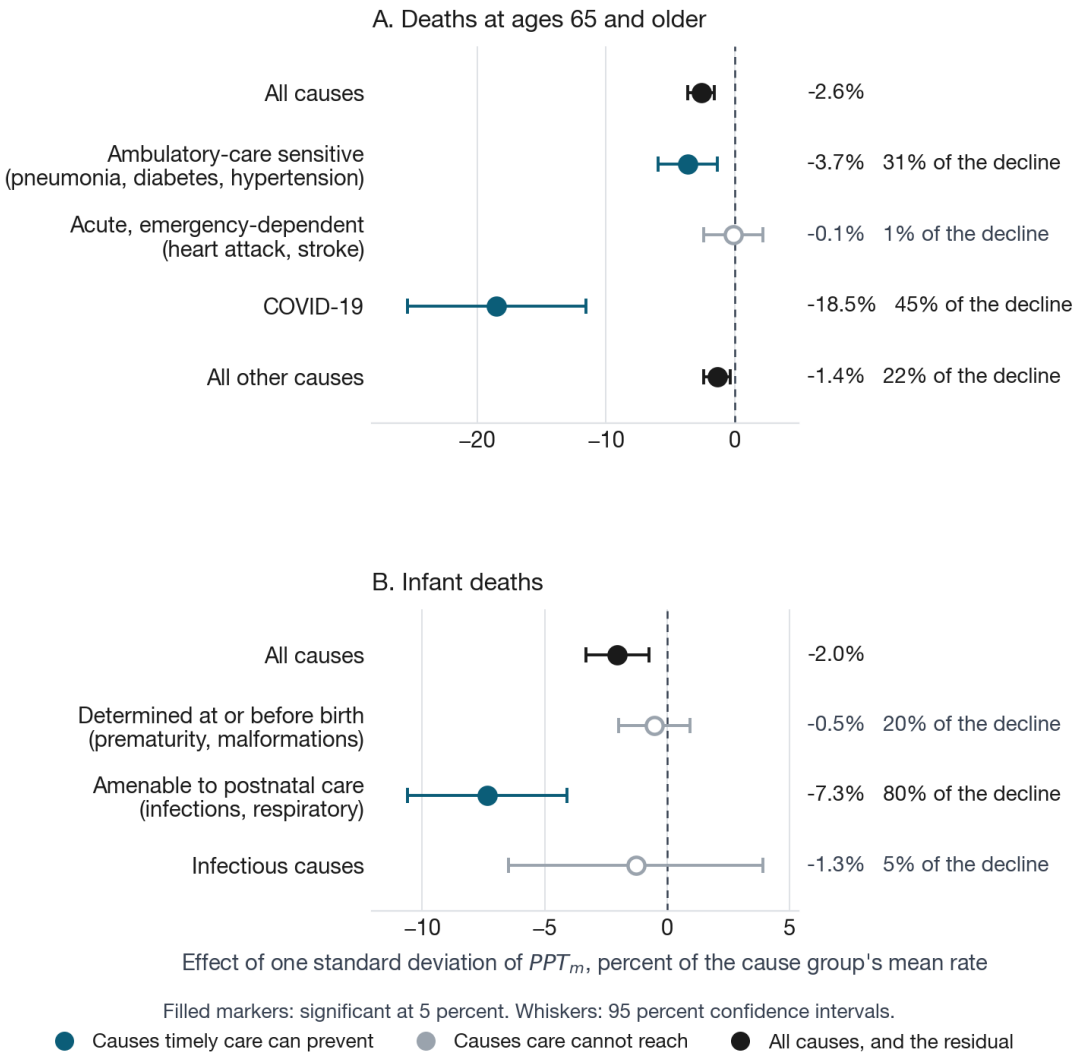
Panel B. Colombian care: fewer emergency visits, more routine care



Panel C. Colombian insurance: no crowd-out

Notes: Each marker is the coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and $Post_t$ equals one from January 2022 onward, from a separate municipality-month regression, divided by the mean of the outcome and multiplied by 100; whiskers are 95 percent confidence intervals on the same scale, and filled markers are estimates significant at the 5 percent level. Each panel has its own axis because the migrant effects are an order of magnitude larger than the Colombian ones. Panel A uses migrant care of all document types from the RIPS encounter records, 2016 to 2024 (Appendix Table D.3). Panel B uses Colombians' care from the same records (Appendix Table D.2). Panel C uses Colombian affiliates per 1,000 inhabitants in the BDUA registry, 2018 to 2024; the enrollment of PPT holders in the same registry, which rises by 63 percent of its mean, is the first stage in Figure 4 (Appendix Table D.1). The emergency share is emergency encounters as a percent of outpatient-plus-emergency care. All regressions include municipality and month fixed effects and COVID-19 severity × year controls, are weighted by population, and cluster standard errors by municipality. Source. RIPS (SISPRO), BDUA, and Migración Colombia ETPV registrations.

Figure 8: Cause Decomposition of the Elderly and Infant Mortality Effects



Notes: The figure plots the estimates of Appendix Table D.7. Each marker is the coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and $Post_t$ equals one from January 2022 onward, from a separate municipality-month regression, 2016 to 2024, of the monthly death rate from the indicated cause group, divided by the mean of that rate and multiplied by 100; whiskers are 95 percent confidence intervals on the same scale, and filled markers are estimates significant at the 5 percent level. Panel A uses deaths at ages 65 and older per 100,000 population in that age group; Panel B uses infant deaths per 1,000 live births. Blue marks the cause groups that timely treatment can prevent (ambulatory-care-sensitive conditions, deaths amenable to postnatal care, and COVID-19), grey the groups it cannot reach (acute emergency-dependent events, conditions determined at or before birth, and infant infectious causes), and black the all-cause total and the residual. The text at the right gives each group's share of the total decline in its panel. All regressions include municipality and month fixed effects and COVID-19 severity × year controls, are weighted by population aged 65 and older (Panel A) or births (Panel B), and cluster standard errors by municipality. Source. DANE EEVV, population projections, and birth statistics; Migración Colombia ETPV registrations.

Table 1: Main Outcomes of Analysis: Definition and Purpose

Outcome	Definition	What it tests
Panel A. Mortality (dependent variables; monthly rates by municipality)		
Mortality rate	All-cause deaths per 100,000 inhabitants.	The net effect across every margin below; the headline estimate.
Infant mortality	Deaths before the first birthday per 1,000 live births.	The canonical care-amenable margin; responds to prenatal and neonatal access within months.
Maternal mortality	Deaths from obstetric causes per 100,000 live births.	Access to obstetric care; the least precise margin, reported as suggestive.
Mortality by age	Deaths in an age band per 100,000 of that band's population; bands infant, 1–4, 5–19, 20–44, 45–64, and 65 and older.	Where care-amenable deaths concentrate. Ages 65 and older hold more than half of all deaths; 20–44 is where migrants concentrate; 5–19, mostly external deaths, is a second quasi-placebo.
Communicable disease	Deaths from infectious and parasitic diseases (ICD-10 A00–B99) per 100,000; influenza and pneumonia (J09–J18) tracked separately.	The transmission channel. If coverage reduced contagion, the decline should concentrate here.
External causes	Deaths from injuries and external events (V01–Y98) per 100,000, including homicides.	Falsification. Coverage cannot prevent an accident or a homicide, though serious injuries use the same emergency services, so a soft rather than sharp placebo.
Cause-specific	Hypertensive, diabetic, ischemic, cerebrovascular, chronic respiratory, cancer, dementia, and renal deaths; COVID-19-coded deaths (U07) identified throughout.	Separates causes that timely treatment prevents from those it does not, in the mechanism analysis.
Panel B. Health system use (mechanism outcomes)		
Service use	Emergency visits, hospitalizations, outpatient consultations, and procedures per 1,000 inhabitants, and the emergency share of care, by nationality (RIPS).	Whether migrant care moved from emergency to routine settings, and whether the services Colombians use decongested.
Enrollment	Stock of affiliates by regime and by identification document (BDUA).	The first stage, permits into coverage, and whether Colombians were crowded out.
Vaccination	Doses applied against programmatic targets, by vaccine and dose (PAIWEB).	Whether preventive care changed, the channel that would operate through transmission.
Births	Mother's insurance at delivery, prenatal visits, site of birth and attendant, birth weight, and gestation (birth certificates).	The maternal and infant channel, coverage at birth and institutional delivery.

Notes: Rates use the denominator named in each definition, so the columns of the mortality tables are separate rates rather than a decomposition; the cause-of-death tables, which share one denominator, sum to their totals. Suicide is the one external cause with a plausible care margin, through mental health services, and is roughly a tenth of external-cause deaths. Section C.1 of Appendix C lists every ICD-10 code range and the construction of the age bands.

Table 2: Difference-in-Differences Estimates. PPT Exposure and Mortality

	(1) Mortality rate	(2) Infant	(3) Maternal	(4) Communi-cable	(5) External	(6) Age 65+
Panel A. Mortality rates, all deaths						
$PPT_m \times Post_t$	-0.816***	-0.225***	-3.882*	0.011	-0.099	-8.117***
	(0.186)	(0.073)	(2.064)	(0.019)	(0.068)	(1.646)
	[0.000]	[0.003]	[0.047]	[0.213]	[0.096]	[0.000]
Mean dep. var.	44.948	11.091	55.235	1.166	5.203	308.650
R^2	0.584	0.064	0.021	0.126	0.241	0.425
Panel B. Mortality rates, Colombians (non-Venezuela residents)						
$PPT_m \times Post_t$	-0.827***	-0.237***	-3.865**	0.007	-0.079	-8.342***
	(0.195)	(0.074)	(1.849)	(0.018)	(0.064)	(1.637)
	[0.000]	[0.002]	[0.028]	[0.298]	[0.093]	[0.000]
Mean dep. var.	44.863	10.997	53.901	1.159	5.180	308.418
R^2	0.584	0.060	0.020	0.123	0.239	0.424
Controls (both panels)						
Municipality FE	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓
COVID severity × year	✓	✓	✓	✓	✓	✓
Observations	120,960	120,888	120,888	120,960	120,960	120,960

Notes: Each cell reports the coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and $Post_t$ equals one from January 2022 onward, from a separate municipality-month regression, 2016 to 2024. Outcomes are (1) deaths per 100,000 inhabitants; (2) infant deaths per 1,000 live births; (3) maternal deaths per 100,000 live births; (4) communicable-disease deaths (A00 to B99) per 100,000; (5) external-cause deaths (V01 to Y98) per 100,000; (6) deaths at ages 65 and older per 100,000 population 65 and older. All rates are monthly. Appendix C.1 defines a robustness adjustment that excludes COVID-19-coded deaths, and Table C.1 reports the corresponding estimates. Nationality is measured by the country of habitual residence of the deceased, the only marker available for the full panel; Panel B (Colombians) closely tracks Panel A. The corresponding estimates for non-Colombians (Venezuela residents), a severe undercount of migrant deaths since country of birth enters the records only in 2024, are reported in Appendix Table B.1. Denominators are common across panels (total population or births), so Panel B gives Colombians' contribution to the per-capita rate. All regressions include municipality and month fixed effects and COVID-19 severity × year controls; regressions are weighted by births (columns 2 to 3), population 65+ (column 6), and population otherwise, and the reported means are weighted accordingly, so they differ from the unweighted descriptive statistics of Table A.1; standard errors are clustered by municipality. Sharpened two-stage false-discovery-rate q-values (Benjamini et al., 2006; Anderson, 2008), correcting across the six outcomes within each panel, in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Mechanisms. Public Health Funding as a Moderator

	Mortality rate	Infant	Age 65+
$PPT_m \times Post_t$ $[\beta_1]$	-0.926***	-0.235***	-8.169***
	(0.183)	(0.074)	(1.269)
$PPT_m \times Post_t \times$ Supply-side health transfers p.c. (2019) $[\beta_2]$	-0.720**	-0.033	-6.446***
	(0.312)	(0.103)	(1.801)
	[0.037]	[0.132]	[0.003]

Notes: Each column reports β_1 and β_2 from a separate municipality-month regression, 2016 to 2024, of the specification $y_{mt} = \beta_1(PPT_m \times Post_t) + \beta_2(PPT_m \times Post_t \times X_m) + \beta_3(Post_t \times X_m) + \mathbf{C}_{mt}\gamma + \alpha_m + \delta_t + \varepsilon_{mt}$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, X_m is the standardized 2019 supply-side health transfer per capita under the *Sistema General de Participaciones*, $\mathbf{C}_{mt}$ are COVID-19 severity × year controls, and α_m, δ_t are municipality and month fixed effects; outcome definitions, weights, and samples as in Table 2. A negative β_2 means larger mortality reductions where public health funding was higher; Appendix Figure D.3 reports the corresponding event study. Standard errors clustered by municipality in parentheses; sharpened false-discovery-rate q-values in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Fiscal Implications. Annual Health Costs of the ETPV Net of Lives Saved

	De jure access (all covered migrants)	De facto access (observed users, 43% of migrants)
Panel A. Annual health-system costs (millions of 2024 USD)		
Capitation payments, 1.96 million covered migrants	605	605
Averted pre-program emergency-regime spending	−533	−231
Net new health spending	72	374
Panel B. Annual mortality benefits among Colombians		
Colombian deaths averted per year (lives)	6,500	6,500
Value at a Colombian value of statistical life (millions of USD)	6,500	6,500
Panel C. Bottom line		
Net annual cost, Panel A minus Panel B (millions of USD)	−6,428	−6,126
Net fiscal cost per Colombian life saved (USD)	11,013	57,546

Notes: All quantities are annual and expressed in 2024 US dollars at the average 2024 exchange rate of 4,070 pesos per dollar. Capitation payments value each covered migrant at the 2024 subsidized-regime UPC of 1,256,076 pesos per affiliate per year (Ministerio de Salud y Protección Social, 2023), about 309 dollars. Averted emergency-regime spending values each migrant who used care before the program at the emergency-only health cost estimated for a representative non-regularized migrant in the fiscal accounting of Ibáñez et al. (2025), 898 dollars of 2020 per four-member household, or 224 dollars per person, adjusted to 2024 prices (272 dollars); before regularization, undocumented migrants were entitled to emergency care only, which is more expensive per contact than insured routine care. The de jure scenario assumes every covered migrant generated that cost, since all held the legal entitlement to emergency care; the de facto scenario, the preferred one, assumes only migrants observed in the health records at baseline did, approximated from the RIPS migrant panel as 2021 patient-months divided by an assumed two contact-months per user per year; assuming four contact-months instead raises de facto net new spending from 374 to 489 million dollars, and one contact-month lowers it toward the de jure floor, leaving every conclusion unchanged (Appendix F). Deaths averted come from the illustrative aggregation of Section 5, which applies the estimated marginal effect linearly to the full covered population. The value of a statistical life for Colombia, one million dollars, transfers the United States value using the income elasticity of Viscusi and Masterman (2017). The exercise is deliberately partial. It omits the fiscal revenue migrants generate, the value of migrants' own health gains, averted emergency spending on Colombians, and administrative costs, and the negative bottom line means the program's mortality benefits alone exceed its health-system costs by one to two orders of magnitude depending on the scenario.

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A Data Construction and Validation

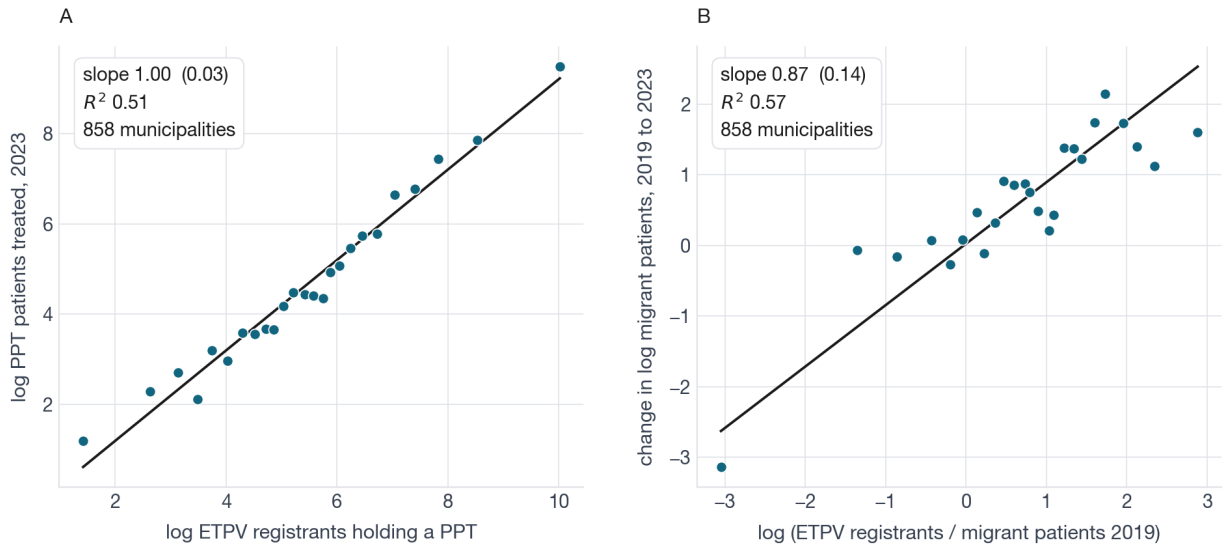
A.1 Validation

We validate the mortality panel against published national aggregates. Figure A.2 plots total and per-capita deaths over the sample, reproducing the pandemic waves of 2020 and 2021, and Figure A.3 shows that the national trends in each outcome, including the 2021 spike in maternal mortality, track the official series. We also validate the exposure measure. Migración Colombia's published municipal snapshot of PPT holders matches our municipal permit counts almost exactly, with a correlation above 0.99 across the 924 municipalities whose names are unique and appear in both sources; the snapshot also supplies the within-name department shares used to assign the roughly 67 duplicated municipality names. Figure A.1 shows the corresponding first stage in utilization, municipal permits against migrant patients treated.

A.2 Sources and Variables

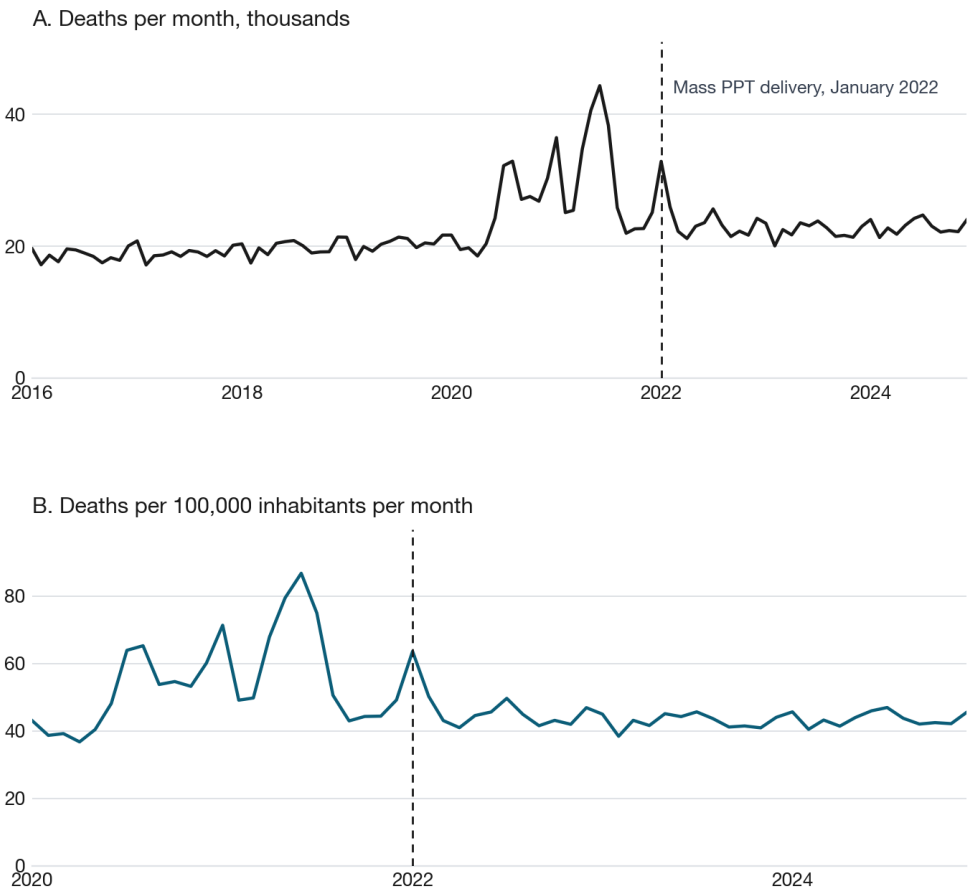
Tables A.2, A.3, and A.4 report descriptive statistics for the principal analysis variables, organized by source, in the raw units in which each is measured. The sources are as follows. **Deaths** come from the DANE vital statistics (EEVV) non-fetal death microdata, 2016 to 2024, accessed through the DANE microdata catalog; each record carries the municipality of residence, month, five-year age group, and ICD-10 basic cause. **Denominators** are the DANE post-census population projections and retroprojections by municipality and single age, and the DANE birth statistics by municipality of residence of the mother. **Births** additionally provide the mother's insurance regime, prenatal visits, site of birth, birth weight, and gestation, from the EEVV birth-certificate panel. **Migrant health care** comes from the RIPS registry of health service encounters in the SISPRO cube, extracted for all non-Colombian document categories by municipality, month, document type, and type of care. **Treatment** is built from Migración Colombia administrative counts of ETPV registrants

Figure A.1: Municipal First Stage. ETPV Registrations and Migrant Use of Health Care



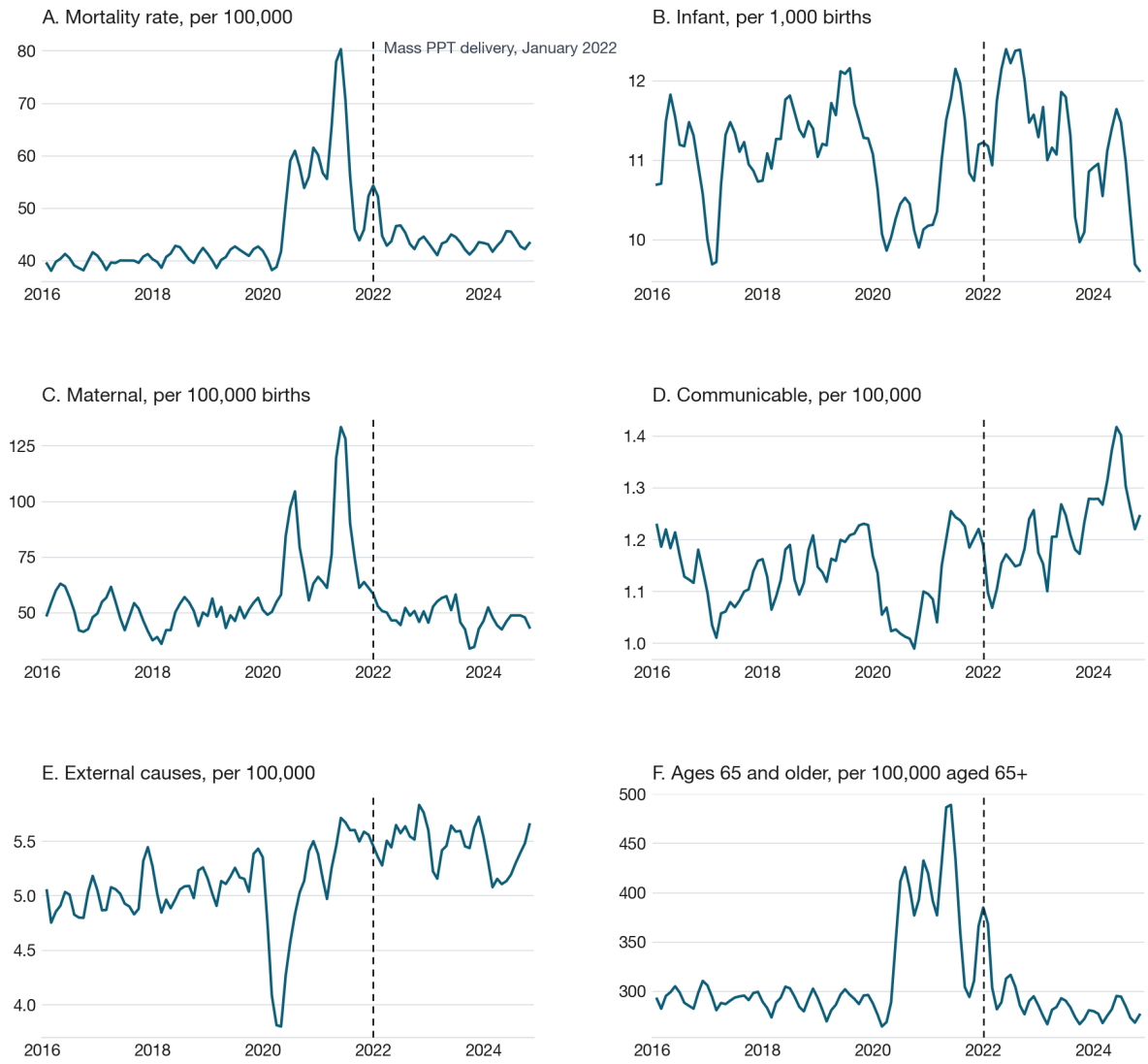
Notes: Binned scatter plots (25 quantile bins) across Colombian municipalities. Panel A plots the log number of patients holding a *Permiso por Protección Temporal* (PPT) treated by the health system in 2023 (RIPS) against the log number of ETPV registrants who received a PPT in the municipality; the fitted slope is 1.00 (robust s.e. 0.03, $R^2 = 0.51$), indicating that municipal ETPV registrations translate approximately one-for-one into regularized migrants treated by the health system. Panel B plots the change in the log number of migrant patients (all document types) between 2019 and 2023 against ETPV intensity, measured as the log ratio of ETPV registrants to migrant patients treated in 2019; observations weighted by 2019 migrant patients (slope 0.87, robust s.e. 0.14). The intensity measure in Panel B shares its denominator with the baseline of the outcome, which mechanically inflates the slope; the relationship remains positive and significant using log registrants instead (0.18, s.e. 0.07). Sample, municipalities with at least 12 migrant patients in 2019 and at least one in 2023 ($N = 858$). Sources. RIPS (Ministry of Health) and ETPV municipal registration records (Migración Colombia).

Figure A.2: National Mortality Trends



Notes: Non-fetal deaths from DANE vital statistics (EEVV), aggregated across all Colombian municipalities, with deaths assigned to the municipality of residence (municipality of occurrence when residence is missing or abroad). Panel A plots total deaths per month, 2016 to 2024; the 2020 to 2021 peaks correspond to the COVID-19 waves. Panel B plots deaths per 100,000 inhabitants per month from 2020, the first year covered by the post-census DANE population projections. Migrant deaths cannot be separated before 2024, when country of birth was added to the death records; in 2024, 2,726 deaths correspond to Venezuelan-born individuals (about 1% of the total). The dashed vertical line marks the beginning of mass PPT delivery (January 2022). Source. DANE EEVV and population projections.

Figure A.3: National Trends in Main Mortality Outcomes



Notes: Monthly national rates built from the clean municipal panel (DANE EEVV deaths, DANE population projections by age, and DANE live births by municipality of residence of the mother), shown as three-month moving averages, 2016 to 2024. Crude mortality per 100,000 inhabitants; infant deaths per 1,000 live births; maternal deaths (ICD-10 O00 to O95, O98, O99) per 100,000 live births; communicable-disease deaths (A00 to B99) and external-cause deaths (V01 to Y98) per 100,000 inhabitants; deaths of persons aged 65 and older per 100,000 population aged 65 and older. Monthly birth denominators are annual births divided by twelve. The dashed vertical line marks the beginning of mass PPT delivery (January 2022). Source. DANE EEVV, population projections, and birth statistics.

and PPT deliveries by municipality, scaled by 2022 population. **Baseline municipal characteristics** come from the CEDE municipal panel of Universidad de los Andes, taking the latest pre-2020 value of each variable. **Local economic activity** combines VIIRS satellite night-light composites with the municipal fiscal accounts. **Labor markets** come from the GEIH household surveys for the twenty-five main cities, with nativity from country of birth. The fiscal parameters of Section 7 are described in Appendix F. The exact construction of every variable, including all cause-of-death code lists, is in the replication do-files, and the README maps each exhibit to its generator.

Table A.1: Descriptive Statistics

	Total			Colombians			Venezuelans		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Panel A. Mortality outcomes (municipality × month)									
Mortality rate (per 100,000)	42.5	28.4	121,116	42.5	28.4	121,116	0.0	0.8	121,116
Infant (per 1,000 births)	11.32	51.45	121,044	11.28	51.33	121,044	0.05	3.49	121,044
Maternal (per 100,000 births)	65.1	1144.5	121,044	63.6	1077.9	121,044	1.5	385.0	121,044
Communicable (per 100,000)	0.92	3.31	121,116	0.92	3.31	121,116	0.00	0.17	121,116
External (per 100,000)	5.32	8.86	121,116	5.31	8.84	121,116	0.02	0.44	121,116
Age 65+ (per 100,000 aged 65+)	260.7	190.5	121,116	260.6	190.4	121,116	0.1	3.0	121,116
Panel B. Treatment variables (municipality)									
ETPV registrants	2038	16201	1,121						
ETPV registrants with PPT	1750	13838	1,121						
ETPV registrants per 1,000 inhab.	21.4	35.5	1,121						
PPT permits per 1,000 inhab.	17.9	29.4	1,121						

Notes: Statistics over municipality-month observations, 2016 to 2024 (Panel A) and across municipalities (Panel B). In Panel A, age-specific rates divide deaths in the band by the band's population; nationality is the country of habitual residence of the deceased. Venezuelans are Venezuela residents, a severe undercount of migrant deaths (country of birth enters the records only in 2024); Colombians are all other residents. Denominators are common across groups (total population or births), so the Total column equals the sum of the Colombian and Venezuelan columns. Treatment variables in Panel B are municipal and have no nationality split. Sources. DANE EEVV, population projections, and birth statistics; Migración Colombia ETPV registrations.

Table A.2: Variables, Sources, and Descriptive Statistics

	Mean	SD	N
Panel A. Mortality outcomes. Municipality × month, 2016 to 2024. Source. DANE EEVV death microdata; denominators from DANE population projections and birth statistics.			
Mortality rate (deaths per 100,000)	42.538	28.354	121,068
Infant mortality (per 1,000 births)	11.33	51.45	120,972
Maternal mortality (per 100,000 births)	64.1	1112.3	120,972
Communicable mortality (per 100,000)	0.9203	3.3037	121,068
External-cause mortality (per 100,000)	5.3261	8.8586	121,068
Age 65+ mortality (per 100,000 aged 65+)	260.713	189.610	121,068
Ages 1 to 19 (per 100,000)	3.9	12.4	121,068
Ages 20 to 44 (per 100,000)	13.1	22.2	121,068
Ages 45 to 64 (per 100,000)	36.4	49.5	121,068
Age 65+, ambulatory-care-sensitive causes (per 100,000)	64.536	88.518	121,068
Age 65+, acute emergency-dependent causes (per 100,000)	81.814	97.801	121,068
Age 65+, COVID-19 (per 100,000)	10.563	47.425	121,068
Infant, determined at or before birth (per 1,000 births)	8.40	43.67	120,972
Infant, amenable to postnatal care (per 1,000 births)	2.93	26.70	120,972
Panel B. Treatment and exposure. Municipality level. Source. Migración Colombia ETPV registrations; COVID-19 severity built from DANE EEVV.			
PPT permits delivered per 1,000 inhabitants (2022 population)	17.9	29.4	1,121
All ETPV registrants per 1,000 inhabitants	21.4	35.5	1,121
COVID-19 severity (excess mortality 2020 to 2021 vs 2017 to 2019)	0.325	0.266	1,120

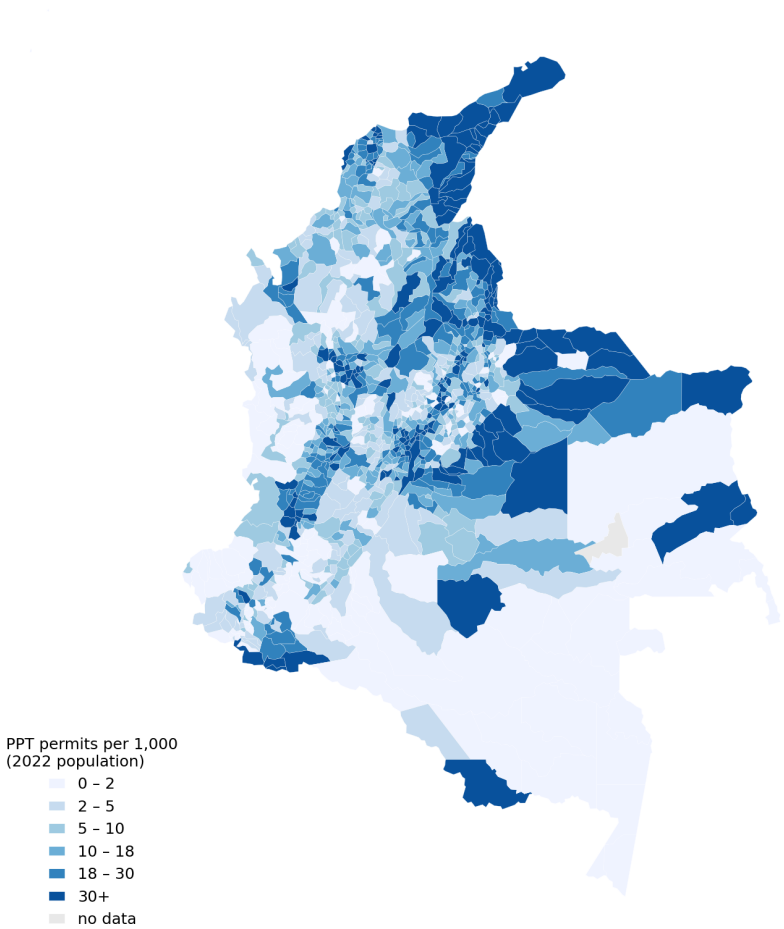
Notes: Unweighted statistics over the estimation cells indicated in each panel header. Panel A rates are monthly; the ambulatory-care-sensitive group comprises influenza and pneumonia, diabetes, chronic lower respiratory disease, and hypertensive disease; the acute group comprises ischemic heart disease and cerebrovascular disease; infant deaths determined at or before birth are the perinatal and congenital chapters. Panel B is a municipal cross-section; the regressions use the standardized permit measure interacted with the post indicator. Tables A.3 and A.4 continue with the remaining sources.

Table A.3: Variables, Sources, and Descriptive Statistics, Continued

	Mean	SD	N
Panel C. Baseline municipal characteristics, latest pre-2020 value, raw units. Source. CEDE municipal panel (Universidad de los Andes). Standardized versions enter the regressions.			
Unsatisfied basic needs index (percent, 2018 census)	22.7	17.7	1,122
Households deprived of health access (percent, census MPI)	5.1	4.1	1,122
Households without health insurance (percent, census MPI)	14.8	6.4	1,122
Rurality index (0 to 100)	0.6	0.2	1,122
Altitude (meters)	1133	920	1,121
Distance to department capital (km)	81.5	60.6	1,122
Distance to Bogotá (km)	321.6	194.6	1,122
Aqueduct coverage (percent)	57.8	29.1	1,098
Sewerage coverage (percent)	39.9	28.3	1,098
Municipal performance score (MDM, 0 to 100)	54.4	10.5	1,101
Municipal health results score (MDM, 0 to 100)	83.9	8.1	1,101
SGP health transfers per capita (pesos)	254914	81643	1,122
SGP supply-side health transfers per capita (pesos)	3443	6139	1,122
Health service providers per 10,000 inhabitants	1.03	1.10	869
SISBEN registrants per capita	0.97	0.18	1,100
Homicides per 100,000 inhabitants	26.2	34.4	1,122
Internally displaced arrivals per 1,000 inhabitants	2.5	6.0	1,064
Panel D. Birth outcomes. Municipality × month, 2016 to 2024. Source. DANE EEVV birth certificates.			
Live births	58787.3	1391164.6	116,882
Births to uninsured mothers (percent)	4.0	8.9	116,882
Births in the subsidized regime (percent)	74.8	24.3	116,882
Mean prenatal visits	5.93	1.43	116,882
Births with seven or more prenatal visits (percent)	45.1	25.5	116,882
Births at home (percent)	2.9	10.5	116,882
Low birth weight (percent)	8.8	12.2	116,882
Preterm births (percent)	8.2	12.0	116,882

Notes: Continuation of Table A.2. Panel C reports raw values of the controls whose standardized versions enter Tables 3 and C.1; the latest nonmissing pre-2020 value is taken per municipality. Panel D statistics are over municipality-month cells with at least one recorded birth.

Figure A.4: Geographic Distribution of PPT Exposure



Notes: PPT permits delivered per 1,000 inhabitants (2022 municipal population), the fixed exposure measure of Section 4, shaded in six quantile bins with darker shading denoting higher exposure. The median municipality received 9.9 permits per 1,000 and the mean 17.9; the highest values are small border municipalities where permit holders are a large share of the population. Every one of the 1,121 municipalities in the estimation sample matches a polygon in the 1,122-municipality national framework. Municipal boundaries are from DANE's Marco Geoestadístico Nacional (Departamento Administrativo Nacional de Estadística , DANE). Source. Migración Colombia and DANE.

Table A.4: Variables, Sources, and Descriptive Statistics, Continued

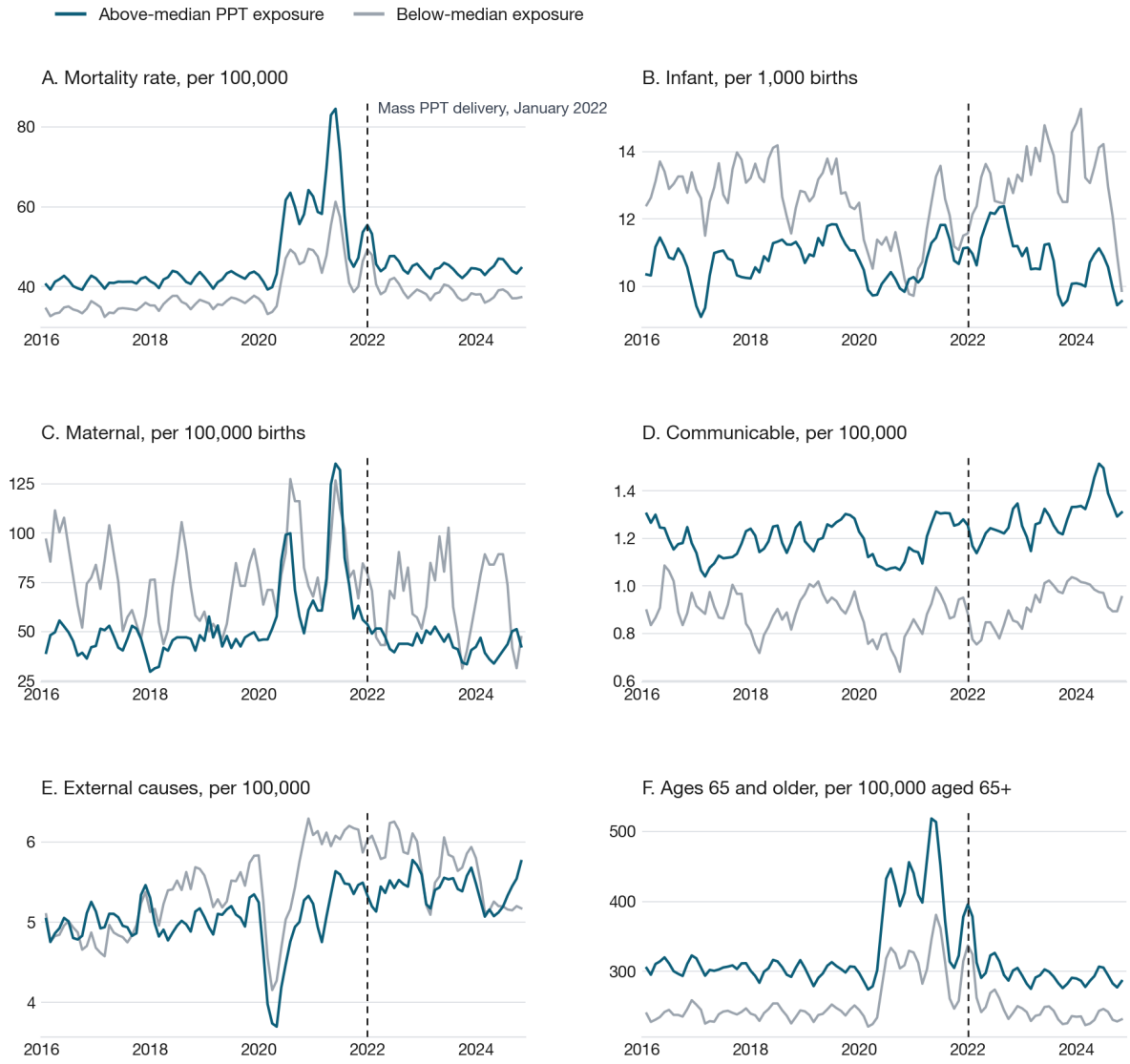
	Mean	SD	N
Panel E. Local economic activity. Municipality × year. Source. VIIRS night-light composites; municipal fiscal accounts (regularization-growth project files).			
Night-light density (mean radiance)	3.50	3.93	10,062
Local tax revenue per capita (millions of pesos)	0.003	0.003	8,784
Panel F. Labor markets. City × year × nativity cells, 25 main cities, ages 15 to 65. Source. GEIH (DANE).			
Employment rate (share of working-age, both nativities pooled)	0.627	0.073	439
Formality rate (share of employed)	0.312	0.193	439
Log real labor income	8.79	0.29	438
Panel G. Migrant health care. Municipality × month, 2016 to 2024. Source. RIPS registry of health service encounters (SISPRO), all non-Colombian document categories.			
Migrant outpatient patients (per 1,000 inhabitants)	0.96	2.76	121,068
Migrant emergency patients (per 1,000 inhabitants)	0.047	0.268	121,068
Emergency share of migrant care (percent)	4.2	8.7	85,050

Notes: Continuation of Table A.2. Panel F pools Colombian and Venezuelan-born city-year cells; the regressions in Table D.4 estimate them separately by nativity. Panel G is built from the raw RIPS extract by the replication file `62_care_switch.do`. The exact construction of every variable is in the replication do-files listed in the README.

B Additional Results

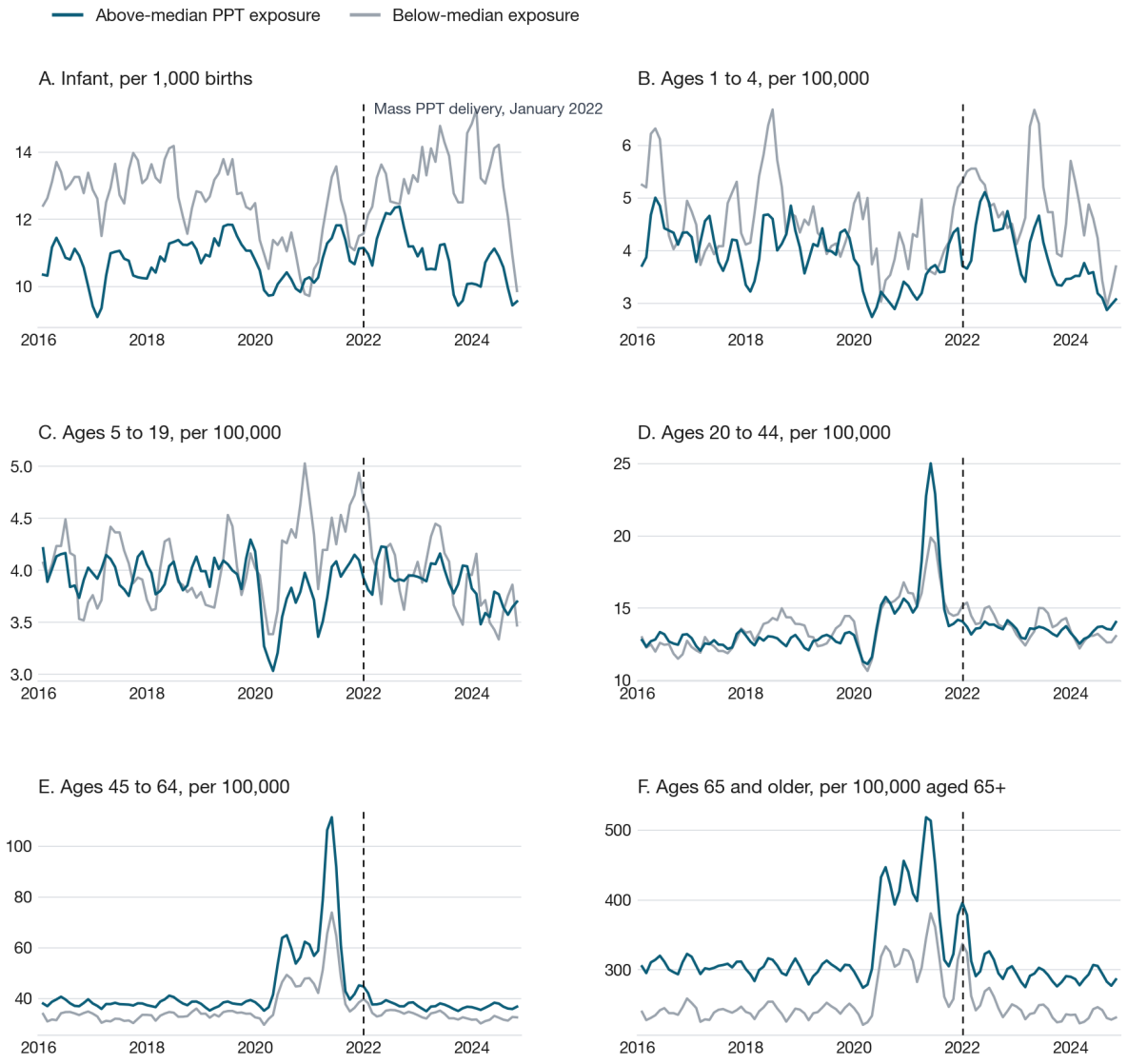
This appendix collects the age-specific analysis and supporting descriptive figures. Appendix Figure B.4 shows the event study for total and Colombian mortality separately. Table B.3 reports mortality rates by age band and Table B.2 the corresponding difference-in-differences estimates, with the event studies in Figure B.5. Figures B.1 and B.2 plot raw mortality trends by exposure group, overall and by age, showing the post-2022 divergence non-parametrically.

Figure B.1: Mortality Trends by PPT Exposure Group



Notes: Municipalities are split at the unweighted median of fixed PPT exposure (permits delivered per 1,000 inhabitants, 2022 population; median 9.9). Rates are computed within each group as group deaths over the group denominator, monthly, shown as three-month moving averages, 2016 to 2024. Outcome definitions as in Figure A.3. The dashed vertical line marks the beginning of mass PPT delivery (January 2022). Source. DANE EEVV, population projections, and birth statistics.

Figure B.2: Age-Specific Mortality Rates by PPT Exposure Group



Notes: Municipalities are split at the unweighted median of fixed PPT exposure (permits delivered per 1,000 inhabitants, 2022 population; median 9.9). Age-specific rates are computed within each group as group deaths over the group population in the age band (DANE projections by single age), monthly, three-month moving averages, 2016 to 2024. Deaths under age one are expressed per 1,000 live births (annual births divided by twelve). The dashed vertical line marks the beginning of mass PPT delivery (January 2022). Source. DANE EEVV, population projections by age, and birth statistics.

Table B.1: Difference-in-Differences Estimates. PPT Exposure and Mortality of Non-Colombians (Venezuela Residents)

	(1) Mortality rate	(2) Infant	(3) Maternal	(4) Communi-cable	(5) External	(6) Age 65+
$PPT_m \times Post_t$	0.011	0.013	-0.017	0.004	-0.019**	0.225
	(0.023)	(0.047)	(0.514)	(0.004)	(0.008)	(0.153)
	[1.000]	[1.000]	[1.000]	[1.000]	[0.095]	[0.550]
Mean dep. var.	0.086	0.094	1.333	0.007	0.022	0.233
R^2	0.497	0.187	0.019	0.167	0.141	0.365
Municipality FE	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓
COVID severity × year	✓	✓	✓	✓	✓	✓
Observations	120,960	120,888	120,888	120,960	120,960	120,960

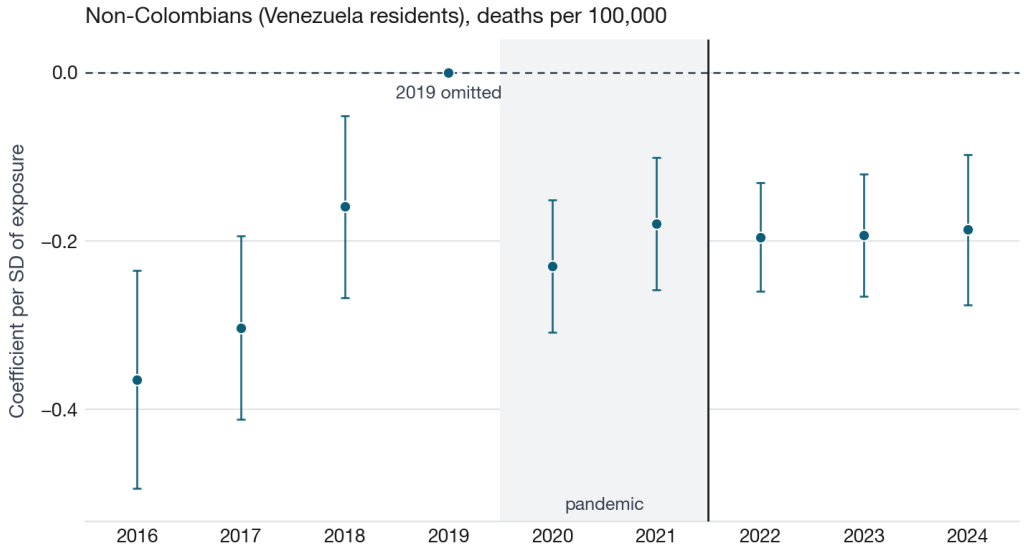
Notes: Companion to Table 2. Each cell reports the coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, from a separate municipality-month regression, 2016 to 2024, for deaths of non-Colombians (Venezuela residents). Outcomes, denominators, weights, fixed effects, and clustering are identical to Table 2; denominators are common across nationality groups, so this panel gives Venezuela residents' contribution to the per-capita rate. Nationality is measured by country of habitual residence, which captures only a small and undercounted share of migrant deaths (country of birth enters the death records only in 2024), so these estimates are noisy and bound little on their own. Sharpened two-stage false-discovery-rate q-values, correcting across the six outcomes, in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: Difference-in-Differences Estimates. PPT Exposure and Mortality by Age

	(1) Infant (per 1,000 births)	(2) 1 to 19	(3) 20 to 44	(4) 45 to 64	(5) 65+
Panel A. Mortality rates by age, all deaths					
$PPT_m \times Post_t$	-0.225***	0.010	-0.365**	-1.144***	-8.117***
	(0.073)	(0.040)	(0.146)	(0.303)	(1.646)
	[0.002]	[0.189]	[0.006]	[0.000]	[0.000]
Mean dep. var.	11.091	3.947	13.691	41.368	308.650
R^2	0.064	0.062	0.240	0.353	0.425
Panel B. Mortality rates by age, Colombians (non-Venezuela residents)					
$PPT_m \times Post_t$	-0.237***	-0.005	-0.301**	-1.198***	-8.342***
	(0.074)	(0.042)	(0.140)	(0.318)	(1.637)
	[0.002]	[0.221]	[0.016]	[0.000]	[0.000]
Mean dep. var.	10.997	3.917	13.620	41.280	308.418
R^2	0.060	0.060	0.237	0.351	0.424
Panel C. Mortality rates by age, non-Colombians (Venezuela residents)					
$PPT_m \times Post_t$	0.013	0.015	-0.064***	0.054	0.225
	(0.047)	(0.014)	(0.016)	(0.041)	(0.153)
	[0.473]	[0.383]	[0.000]	[0.345]	[0.396]
Mean dep. var.	0.094	0.030	0.071	0.088	0.233
R^2	0.187	0.142	0.187	0.245	0.365
Controls (all panels)					
Municipality FE	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓
COVID severity × year	✓	✓	✓	✓	✓
Observations	120,888	120,960	120,960	120,960	120,960

Notes: Each cell reports the coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and $Post_t$ equals one from January 2022 onward, from a separate municipality-month regression, 2016 to 2024. Column (1) is infant deaths per 1,000 live births; columns (2)–(6) are deaths per 100,000 population in the age group; all rates are monthly. The childhood band pools ages 1 to 19 because the DANE five-year age codes do not permit an exact 1 to 18 cut and because the separate 1 to 4 band is too small to support inference, its minimum detectable effect at conventional power being roughly ten percent of its mean; pooled, the band has a minimum detectable effect of 2.9 percent of its mean, comparable to the effects estimated in the other columns, which is what makes its precise zero informative. Nationality is measured by the country of habitual residence of the deceased. Colombians are non-Venezuela residents; non-Colombians are Venezuela residents, a severe undercount of migrant deaths (country of birth enters the records only in 2024), so Panel C is small and Panel B closely tracks Panel A. Denominators are common across panels, so Panels B and C give each group's contribution to the age-specific rate. All regressions include municipality and month fixed effects and COVID-19 severity × year controls; regressions are weighted by births (column 1) and age-group population otherwise; standard errors are clustered by municipality. Sharpened two-stage false-discovery-rate q-values (Benjamini et al., 2006; Anderson, 2008), correcting across the five columns within each panel, in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure B.3: Event Study. Mortality Rates of Non-Colombians (Venezuela Residents)



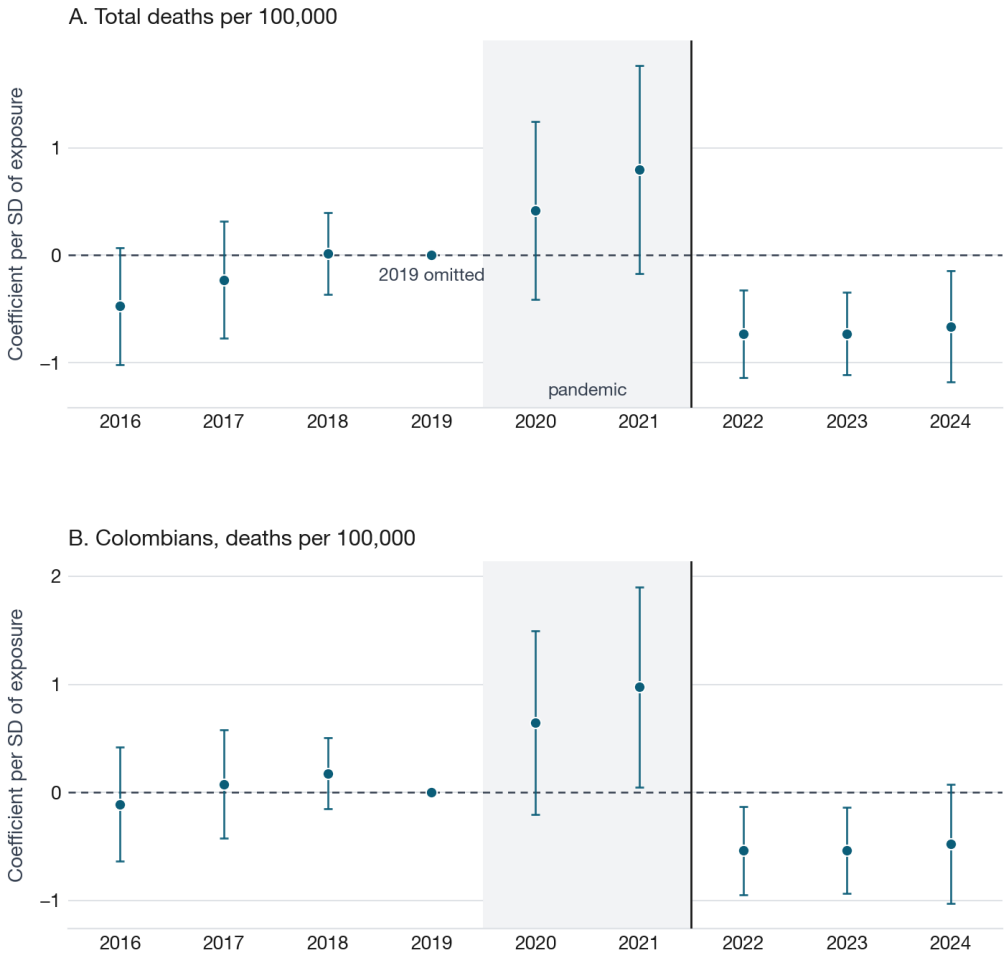
Notes: Companion to Appendix Figure B.4. Yearly coefficients on the interaction between PPT_m, PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and year indicators, from a regression of the mortality rate of non-Colombians (Venezuela residents) per 100,000 inhabitants, on the same scale as the total and Colombian panels; municipality and month fixed effects and municipal COVID-19 severity interacted with year indicators; 2019 omitted. Standard errors clustered by municipality; bars are 95% confidence intervals. Venezuelans are people who lived in Venezuela, the residence-based label of Section 3; the 2019 pre-period spike coincides with the peak of the migration inflow, and there is no post-delivery movement. Source. clean municipal panel (DANE EEVV, population projections).

Table B.3: Descriptive Statistics. Mortality Rates by Age

	Total			Colombians			Venezuelans		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
Infant (per 1,000 births)	11.32	51.45	121,044	11.28	51.33	121,044	0.05	3.49	121,044
Ages 1 to 4 (per 100,000)	3.9	26.0	121,116	3.9	25.9	121,116	0.0	1.1	121,116
Ages 5 to 19 (per 100,000)	3.9	14.0	121,116	3.9	14.0	121,116	0.0	0.7	121,116
Ages 1 to 19, pooled (per 100,000)	3.9	12.4	121,116	3.9	12.4	121,116	0.0	0.6	121,116
Ages 20 to 44 (per 100,000)	13.1	22.3	121,116	13.0	22.2	121,116	0.0	1.5	121,116
Ages 45 to 64 (per 100,000)	36.4	49.9	121,116	36.4	49.8	121,116	0.0	2.0	121,116
Age 65+ (per 100,000 aged 65+)	260.7	190.5	121,116	260.6	190.4	121,116	0.1	3.0	121,116

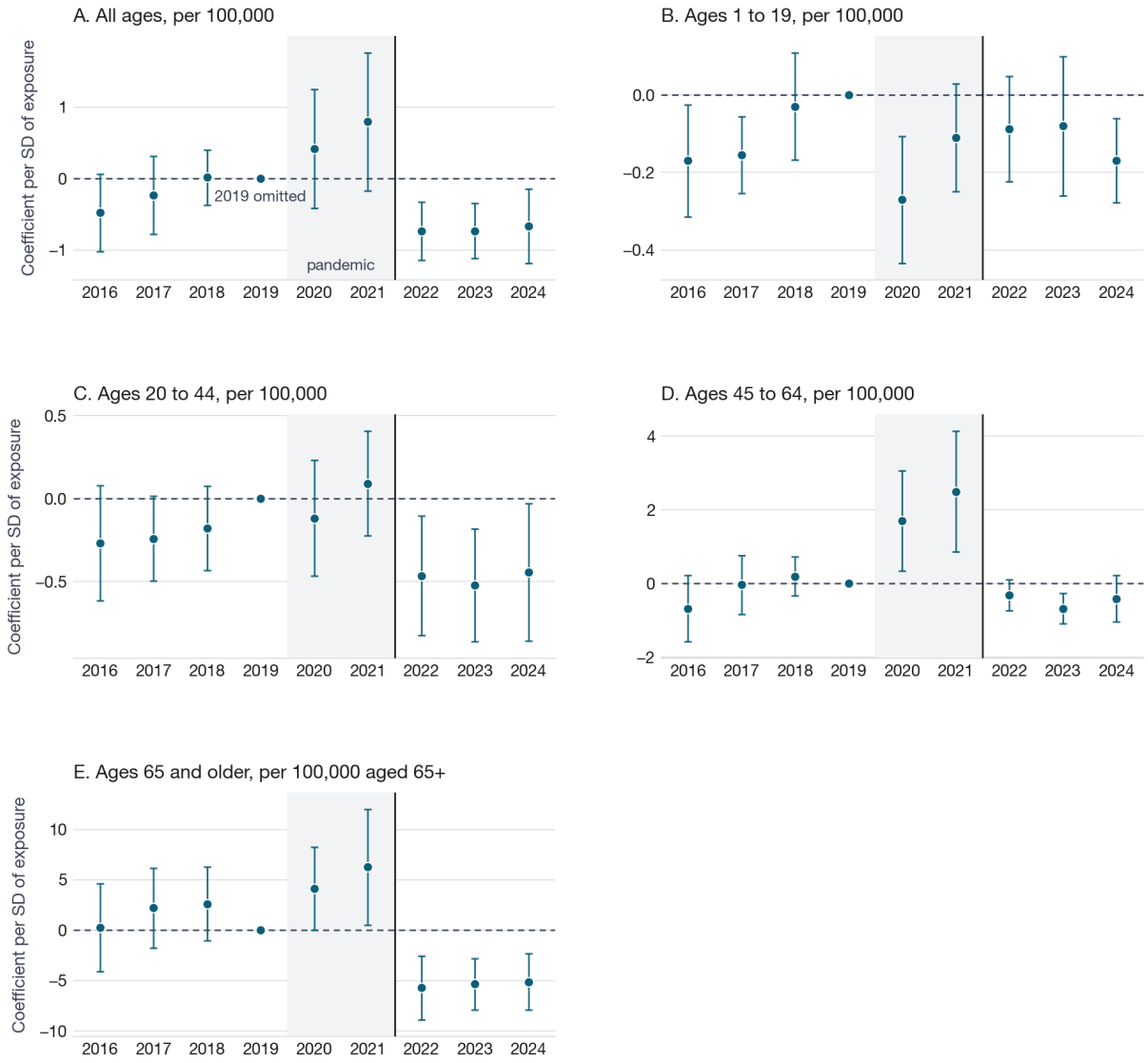
Notes: Statistics over municipality-month observations, 2016 to 2024. Age-specific rates divide deaths in the band by the band's population from the DANE projections; the infant rate divides by monthly births. Nationality is the country of habitual residence of the deceased. Venezuelans are Venezuela residents, a severe undercount of migrant deaths (country of birth enters the records only in 2024); Colombians are all other residents. Denominators are common across groups, so the Total column equals the sum of the Colombian and Venezuelan columns. The pooled 1 to 19 band is the childhood band used in Table B.2. Sources. DANE EEVV, population projections, and birth statistics.

Figure B.4: Event Studies. Mortality Rates, Total and Colombian Residents



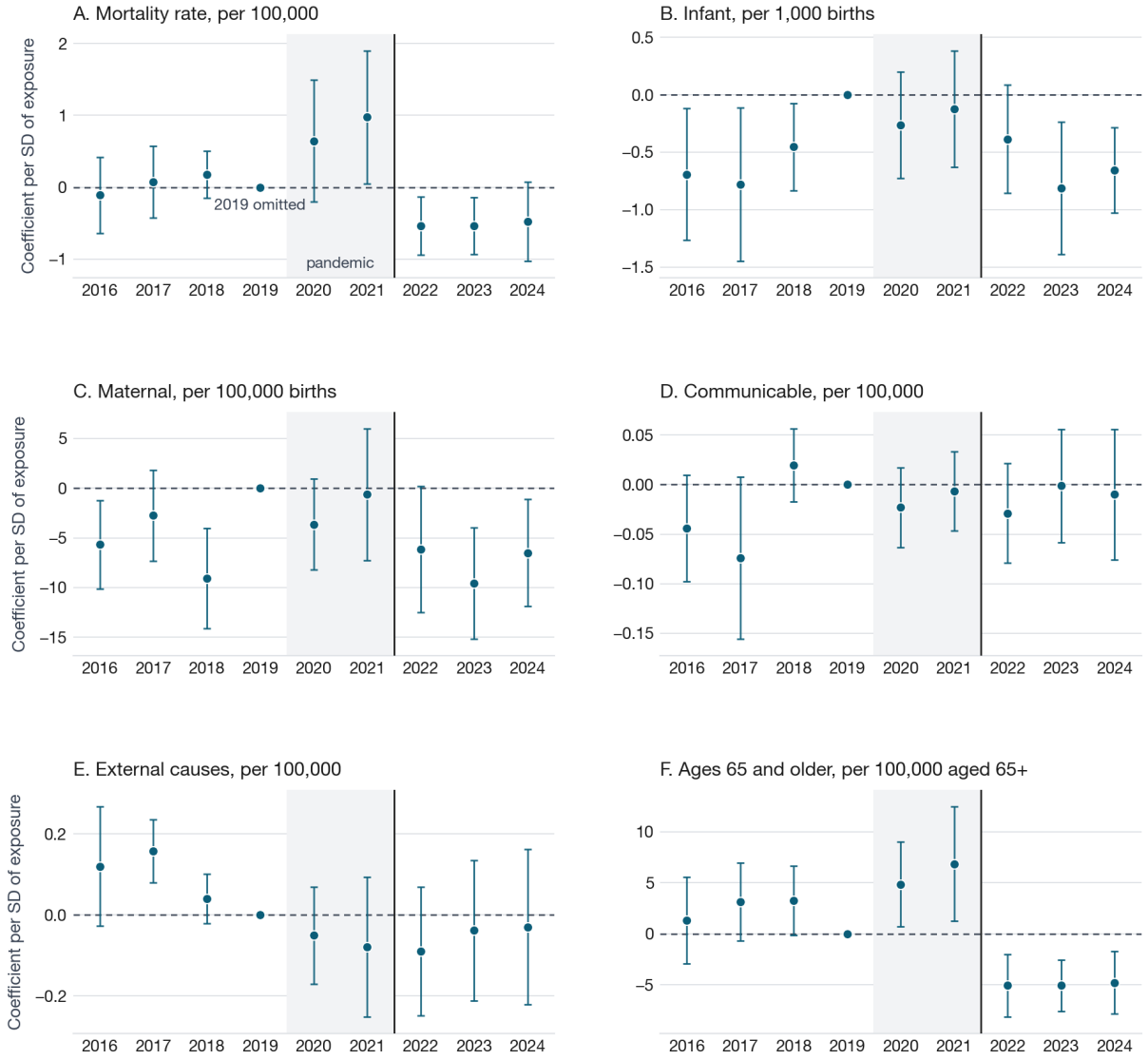
Notes: Yearly coefficients on the interaction between PPT_m, PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and year indicators, from regressions of the mortality rate with municipality and month fixed effects and municipal COVID-19 severity interacted with year indicators; 2019 is the omitted period. Standard errors clustered by municipality; regressions weighted by population; bars are 95% confidence intervals. Panel A shows all deaths per 100,000 inhabitants and Panel B deaths of Colombians, people who lived in Colombia (Section 3), per 100,000 inhabitants; the two move together, with pre-period coefficients drifting upward toward the 2019 reference and a decline from mass permit delivery in 2022 that falls below every pre-period point and persists through 2024, indicating that the mortality externality operates on the host population. The corresponding series for non-Colombians (Venezuela residents), is reported in Appendix Figure B.3. Source. clean municipal panel (DANE EEVV, population projections).

Figure B.5: Event Studies. Total and Age-Specific Mortality and PPT Exposure



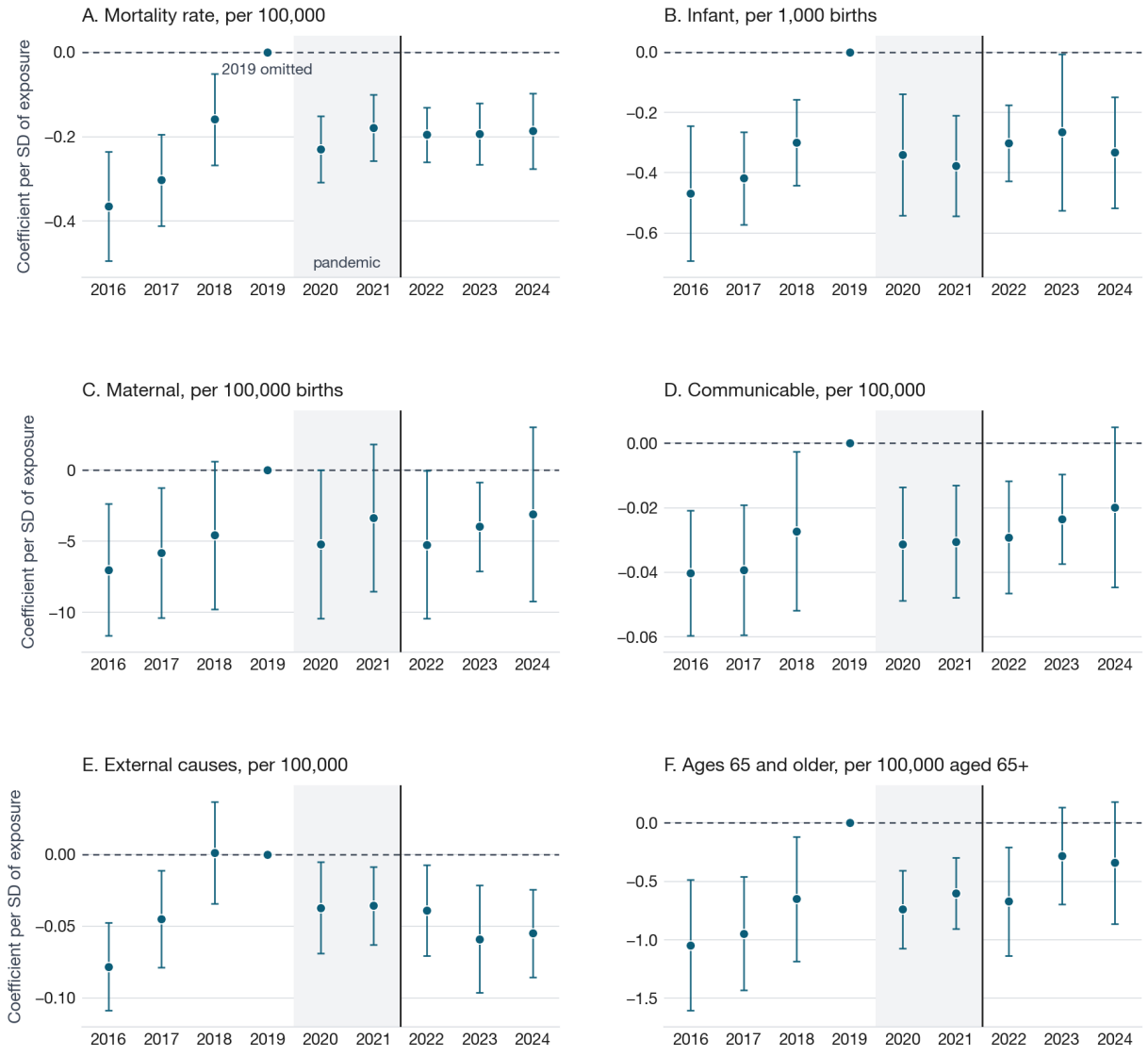
Notes: Each panel plots yearly coefficients on the interaction between PPT_m, PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and year indicators, from regressions with municipality and month fixed effects and municipal COVID-19 severity interacted with year indicators (severity is standardized excess mortality in 2020 to 2021 relative to 2017 to 2019); 2019 is the omitted period. Standard errors clustered by municipality; bars are 95% confidence intervals. Regressions weighted by the population of the relevant age group (total population for the mortality rate). Rates are monthly deaths per 100,000; the childhood band pools ages 1 to 19, matching Table B.2. Source. clean municipal panel.

Figure B.6: Event Studies by Nationality. Main Outcomes, Colombians



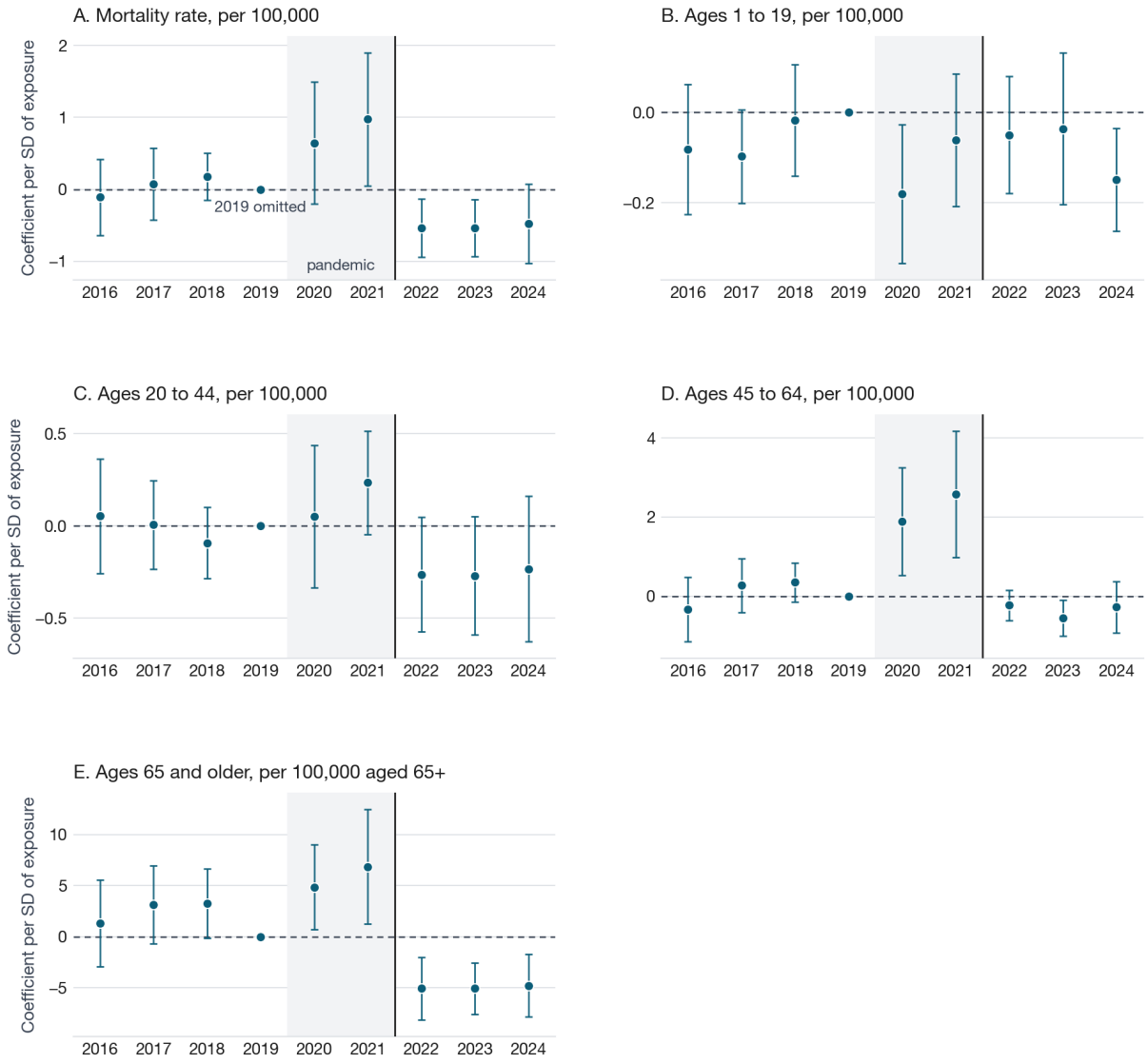
Notes: Yearly coefficients on PPT_m × year indicators, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, from regressions of monthly mortality rates counting only deaths of Colombians, people who lived in Colombia (Section 3); 2019 omitted. Denominators are common (total population, births, or band population), so the panels give the Colombian contribution to each rate. Municipality and month fixed effects and COVID-19 severity × year controls; weighted by the relevant denominator; standard errors clustered by municipality; bars are 95% confidence intervals. Source. DANE EEVV and clean municipal panel.

Figure B.7: Event Studies by Nationality. Main Outcomes, Venezuelans



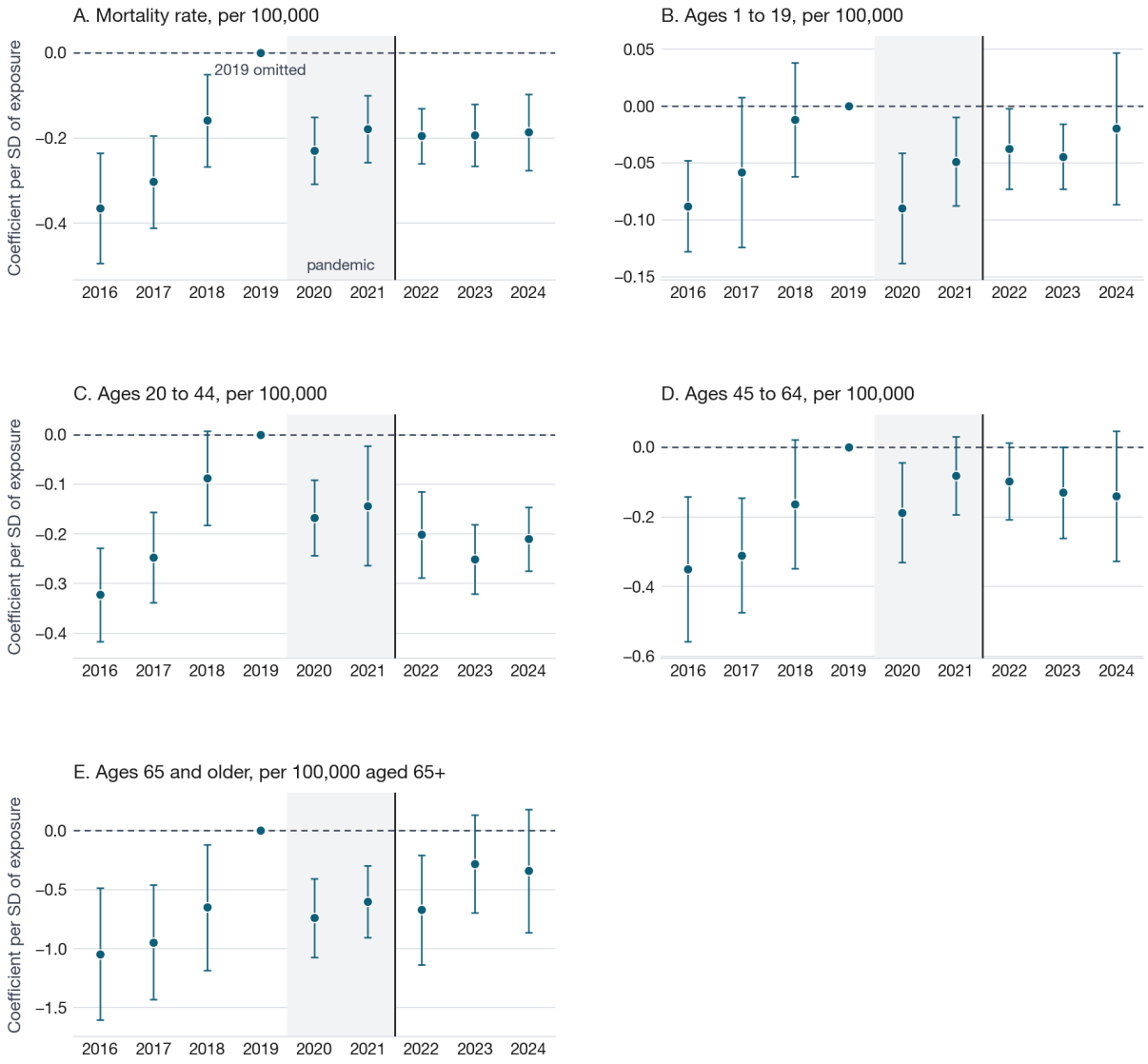
Notes: As in Figure B.6, counting only deaths of Venezuelans, people who lived in Venezuela (Section 3). Rates per 100,000 except the births-based outcomes. Source. DANE EEVV and clean municipal panel.

Figure B.8: Event Studies by Nationality. Age-Specific Mortality, Colombians



Notes: As in Figure B.6, for the age-specific mortality rates of Colombians, people who lived in Colombia (Section 3), with the age bands of Table B.2. Source. DANE EEVV and clean municipal panel.

Figure B.9: Event Studies by Nationality. Age-Specific Mortality, Venezuelans



Notes: As in Figure B.8, counting only deaths of Venezuelans, people who lived in Venezuela (Section 3), per 100,000. Source. DANE EEVV and clean municipal panel.

C Robustness

This appendix reports the robustness exercises that Section 5 refers to. Table C.1 collects the alternative specifications and samples. Section C.1 defines the mortality outcomes and the COVID-19 adjustment, and Section C.2 reports the estimators robust to treatment-effect heterogeneity, the honest-inference bounds, and the exposure-determinant checks.

Table C.1: Robustness. Alternative Specifications and Samples

	(1) Mortality rate	(2) Infant	(3) Maternal	(4) Age 65+
Baseline (Table 2 specification)	-0.816***	-0.225***	-3.882*	-8.117***
	(0.186)	(0.073)	(2.064)	(1.646)
Unweighted	-0.571***	-0.210	-6.784*	-5.463***
	(0.134)	(0.201)	(3.763)	(1.017)
Excluding 2020 to 2021	-0.550***	-0.157**	-3.163	-6.668***
	(0.127)	(0.078)	(2.055)	(1.540)
Department × month fixed effects	-0.796***	-0.232	-0.827	-7.230***
	(0.160)	(0.149)	(2.813)	(1.339)
Excluding border departments	-1.610***	-0.207	1.359	-15.460***
	(0.471)	(0.167)	(2.948)	(3.102)
Excluding five largest cities	-0.625***	-0.229***	-4.220**	-5.817***
	(0.164)	(0.075)	(2.085)	(1.440)
Exposure, all ETPV registrants	-0.831***	-0.226***	-4.036*	-8.279***
	(0.184)	(0.073)	(2.068)	(1.704)
No COVID-severity controls	-0.838***	-0.216***	-3.977*	-8.594***
	(0.197)	(0.073)	(2.063)	(1.846)
Baseline municipal characteristics × trend	-1.030***	-0.352***	-4.388*	-8.033***
	(0.238)	(0.102)	(2.488)	(1.584)
Treatment × linear time trend	-1.883***	-0.850***	-10.076**	-13.098***
	(0.379)	(0.148)	(4.433)	(2.412)
Baseline migrant presence (2018) × post	-0.633***	-0.095	-3.840	-6.630***
	(0.241)	(0.074)	(2.872)	(2.230)
Neighboring municipalities' exposure × post control	-1.240***	-0.369**	-3.052	-11.099***
	(0.294)	(0.154)	(3.192)	(3.142)
Internal-displacement inflows × year	-0.820***	-0.226***	-3.920*	-8.106***
	(0.185)	(0.073)	(2.071)	(1.638)
Excluding COVID-19-coded deaths (Appendix C.1)	-0.426***	-0.225***	-2.597*	-4.438***
	(0.110)	(0.074)	(1.507)	(1.144)
Also excluding ill-defined causes (chapter R)	-0.340***	-0.200***	-2.597*	-3.871***
	(0.099)	(0.071)	(1.507)	(1.153)

Notes: Each cell reports the coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and $Post_t$ equals one from January 2022 onward, from the monthly municipality-level specification of Table 2 under the indicated variation. Outcomes are monthly deaths per 100,000 inhabitants (the mortality rate), infant deaths per 1,000 live births, maternal deaths per 100,000 live births, and deaths of persons 65 and older per 100,000 population 65 and older. Unless noted, regressions include municipality and month fixed effects and COVID-19 severity × year controls, use baseline weights (births for infant and maternal, population 65+ for the elderly rate, population otherwise), and cluster standard errors by municipality. The row labeled baseline municipal characteristics adds thirteen standardized pre-shock municipal characteristics from the CEDE municipal panel (poverty, health-access and insurance deprivation, rurality, altitude, distance to the department capital and to Bogotá, water and sewerage coverage, state capacity, health transfers per capita, homicides, and displaced-population inflows; latest pre-2020 values) interacted with a linear time trend. The row labeled treatment × linear time trend allows municipalities to follow exposure-specific linear trajectories across the whole sample, so the effect is identified only from deviations around a trend fitted partly on pre-treatment data. Fitted on the pre-treatment window alone, that slope is positive and significant for all four outcomes, so the baseline estimates are conservative (see the text). The row labeled baseline migrant presence controls for standardized 2018 migrant patients per 1,000 inhabitants interacted with the post indicator (its correlation with standardized exposure is 0.41). The row labeled neighboring municipalities' exposure adds the average standardized exposure of municipalities within 50 kilometers, interacted with the post indicator, so the own-exposure effect is identified conditional on regional exposure; the

C.1 Mortality Definitions and the COVID-19 Adjustment

This appendix defines the mortality outcomes used in the paper, states what the COVID-19 adjustment excludes and why, and shows that the adjusted series are free of the pandemic disturbance.

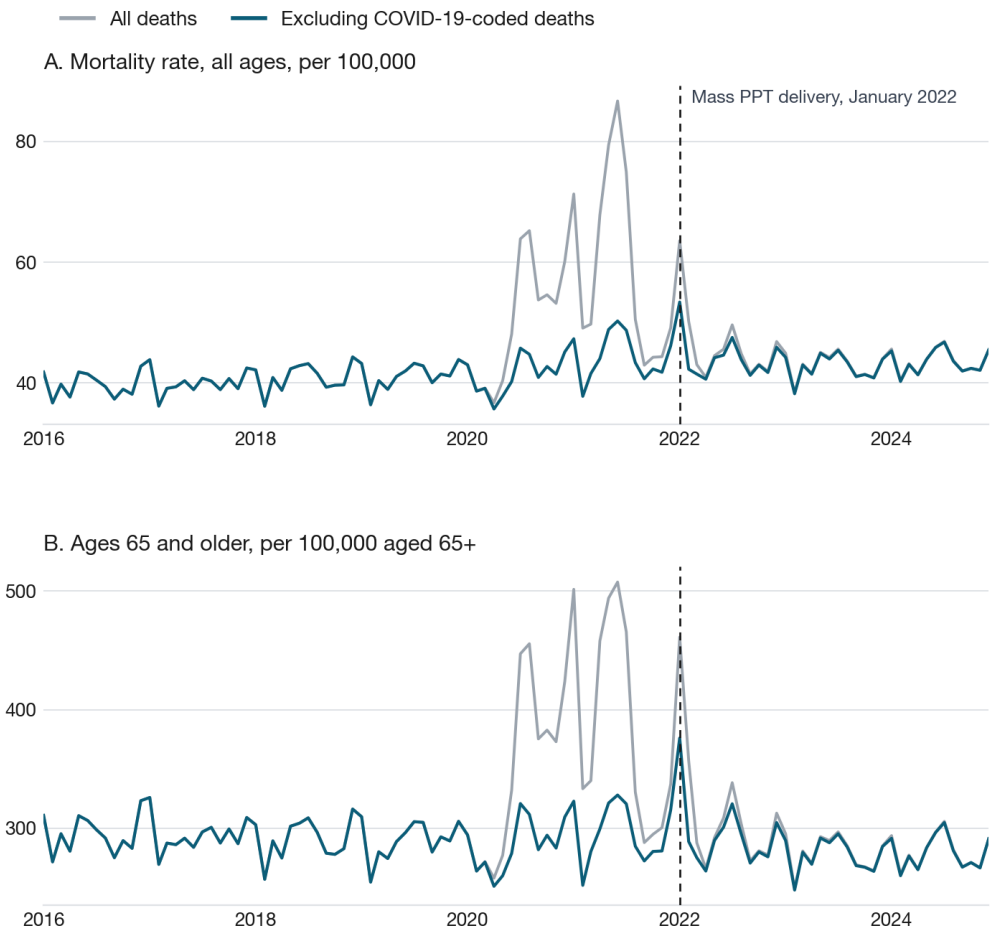
The outcome definitions follow Section 3. The mortality rate is all-cause deaths per 100,000 inhabitants per month; infant mortality is deaths before the first birthday per 1,000 live births; maternal mortality is deaths from obstetric causes (O00 to O95, O98, O99) per 100,000 live births; age-specific rates divide deaths in a band by the band's population; communicable mortality is ICD-10 chapter A00 to B99, deaths from infectious and parasitic diseases such as diarrheal infections, tuberculosis, sepsis, dengue, malaria, and HIV; and external-cause mortality is V01 to Y98, deaths from injuries and events such as traffic accidents, falls, poisonings, suicides, and homicides. The cause-specific outcomes of the mechanism analyses are hypertensive disease (I10 to I15), diabetes (E10 to E14), ischemic heart disease (I20 to I25), cerebrovascular disease (I60 to I69), the full circulatory chapter (I00 to I99), cancer (C00 to C97), chronic respiratory disease (chapter J excluding influenza and pneumonia), dementia (F01, F03, G30, G31), renal disease (chapter N), and ill-defined causes (chapter R). The death certificate records give age in five-year groups, so the age bands are unions of those groups. Infant and 1 to 4 together form under-five mortality, the standard child-health margin sensitive to maternal and child services. Ages 5 to 19 span school age and adolescence, the lowest-mortality years, whose deaths are mostly external, so the band acts as a second quasi-placebo. Ages 20 to 44 are prime working age, where mortality is dominated by external, maternal, and infectious causes and where the migrant population itself concentrates. Ages 45 to 64 mark the onset of chronic-disease mortality, the care-amenable adult margin, and ages 65 and older concentrate more than half of all deaths.

The pandemic requires an adjustment because its waves were not proportional across municipalities. High-exposure municipalities are urban and suffered about three quarters more COVID-19 mortality than low-exposure municipalities, 13.3 against 7.6 deaths per

100,000 over 2020 and 2021, so their subsequent return to baseline generates a mechanical post-2022 decline correlated with exposure that month fixed effects cannot absorb and that severity controls absorb only in part. Our baseline outcomes count all deaths and absorb the pandemic's differential path with the COVID-severity controls, which leave an upward pre-period drift in the event studies that makes the baseline conservative; excluding COVID-coded deaths instead distorts the pre-period, because the pandemic's harvesting of the frail depresses non-COVID mortality in high-exposure municipalities from 2020 onward. We therefore use the exclusion as a robustness exercise rather than the baseline, keeping it as narrowly COVID-specific as the coding allows. The adjusted outcomes exclude only deaths coded to COVID-19 itself (U07). Deaths coded to ill-defined causes (chapter R) carry some misclassified COVID-19, their decline follows the same exposure pattern as the U07 deaths, but the chapter also has a substantial non-pandemic baseline, so we keep it in the outcomes and treat the stricter variant that also excludes chapter R as a robustness check. For maternal mortality the COVID-19 pathway runs through code O98.5, viral disease complicating pregnancy, the code the WHO designated for COVID-19 in pregnant women. O98 deaths averaged about eighteen per year in 2016 to 2019, rose to 81 in 2020 and 193 in 2021, nearly forty percent of that year's maternal deaths, and 179 of the 193 were coded O98.5 specifically. The adjusted maternal series excludes only O98.5, keeping the genuine maternal infections in the remainder of O98.

Figure C.1 shows the effect of the adjustment on the national monthly series. The raw series carry the three pandemic waves of 2020 and 2021; the adjusted series remove nearly all of the disturbance, leaving flat trends before and after mass permit delivery. A residual one-month spike in January 2022 remains in the adjusted series, pandemic-period excess recorded under codes other than U07, and we account for it with the COVID-severity controls.

Figure C.1: National Mortality Before and After the COVID-19 Adjustment



Notes: National monthly mortality per 100,000 inhabitants (Panel A) and per 100,000 aged 65 and older (Panel B), 2016 to 2024. The gray series counts all deaths; the blue series excludes deaths coded to COVID-19 (ICD-10 U07). The dashed line marks mass permit delivery in January 2022. Source. DANE EEVV and population projections.

Table C.2: Mortality Effects Excluding COVID-19 Deaths

	Overall	Infant	Maternal	Elderly 65+
Panel A. Baseline (all deaths)				
Exposure × post	-0.816***	-0.225***	-3.882*	-8.117***
	(0.186)	(0.073)	(2.064)	(1.646)
Mean dep. var.	44.948	11.091	55.235	308.650
Panel B. Excluding COVID-19 deaths (U07; O98.5 for maternal)				
Exposure × post	-0.426***	-0.225***	-2.597*	-4.438***
	(0.110)	(0.074)	(1.507)	(1.144)
Mean dep. var.	42.042	11.039	49.788	288.779
Observations	120,960	120,888	120,888	120,960

Notes: Each entry is the coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, from the baseline municipality-month specification of Table 2, Panel A (all deaths), with municipality and year-month fixed effects, COVID-19 severity × year controls, and standard errors clustered by municipality; weights are population (overall, elderly), births (infant, maternal). Panel A repeats the baseline all-deaths estimates. Panel B removes deaths coded to COVID-19 (U07) from the overall, infant, and elderly outcomes and deaths coded O98.5 (viral disease complicating pregnancy) from the maternal outcome. The overall, infant, and elderly estimates remain negative and significant, attenuating for the overall and elderly margins; the maternal estimate is smaller and only marginally significant. As discussed in the text, excluding COVID-coded deaths itself distorts the pre-period through pandemic harvesting, so this exercise is a robustness check rather than a clean re-estimation. *, **, *** denote significance at the 10, 5, and 1 percent levels. Source. DANE EEVV, population projections, and birth statistics.

Table C.3: Robustness to a Nonparametric Control for Differential Pandemic Recovery

	(1) Baseline (sev. × yr)	(2) Sev. tercile × month FE	(3) Sev. decile × month FE	(4) Excl. COVID + decile × mo.
Overall mortality	-0.816***	-0.778***	-0.720***	-0.451***
	(0.186)	(0.167)	(0.161)	(0.101)
% of mean	-1.8	-1.7	-1.6	-1.0
Infant	-0.225***	-0.236***	-0.215***	-0.214***
	(0.073)	(0.075)	(0.081)	(0.082)
% of mean	-2.0	-2.1	-1.9	-1.9
Maternal	-3.882*	-4.104**	-4.403**	-3.103**
	(2.064)	(2.044)	(1.982)	(1.518)
% of mean	-7.0	-7.4	-8.0	-5.6
Elderly 65+	-8.117***	-7.526***	-6.605***	-3.647***
	(1.646)	(1.579)	(1.414)	(1.096)
% of mean	-2.6	-2.4	-2.1	-1.2
Municipality FE	✓	✓	✓	✓
Month FE	✓	✓	✓	✓
COVID sev. × year	✓			
Severity-bin × month FE		tercile	decile	decile

Notes: Coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, from the baseline specification of Table 2, Panel A. Column (1) is the baseline, with the linear COVID-19 severity × year control. Columns (2) and (3) replace that control with fixed effects for the interaction of the municipality's pandemic-severity bin (tercile and decile of standardized 2020-2021 excess mortality, respectively) with month-of-sample, absorbing any mortality time-path shared by municipalities of similar pandemic intensity; identification then comes only from exposure variation *within* pandemic-severity bins. Column (4) additionally removes COVID-coded deaths from the outcomes (U07; O98.5 for maternal), the most conservative specification. All columns include municipality and month fixed effects, are weighted as in Table 2 (population for the overall and elderly rates, births for infant and maternal), and cluster standard errors by municipality. The overall, infant, and elderly effects are stable across the nonparametric controls and survive the combined column (4); the linear severity control is therefore not driving the estimates. *, **, *** denote significance at the 10, 5, and 1 percent levels. Source. DANE EEVV, population projections, and birth statistics.

C.2 Alternative Estimators, Inference, and Specification

This appendix probes the difference-in-differences design with the estimators and diagnostics developed in the recent methodological literature. Because the PPT switched on at a single date (mass permit delivery in January 2022), the biases associated with staggered adoption do not arise here; the relevant concerns are instead the continuous nature of the treatment, which requires stronger parallel-trends assumptions (Callaway et al., 2024), and potential violations of parallel trends. Following Roth (2022), we emphasize the magnitude of trends the pre-period can rule out rather than the pass/fail outcome of a pre-test.

Effects survive estimators robust to treatment-effect heterogeneity. Appendix Table C.4 re-estimates the mortality results on an annual panel (the annual panel is unweighted, so the continuous two-way fixed-effects estimator in the first row is the annual analog of the unweighted monthly specification in Appendix Table C.1, which it reproduces closely) under estimators robust to treatment-effect heterogeneity, two-way fixed effects with continuous and binarized (above-median) exposure, Callaway and Sant'Anna (2021) doubly robust, Borusyak et al. (2024) imputation, and de Chaisemartin and D'Haultfœuille (2020). The infant result is stable in sign across all five estimators and statistically significant under two; the binarized estimators answer a different question and their magnitudes are not comparable to the per-standard-deviation dose estimates.

The infant estimate warrants two further caveats. It survives excluding La Guajira, where the 2019 deterioration in infant mortality was sharpest (-0.246, $t = -2.53$, against -0.225 in the all-deaths baseline of Table 2, Panel A), excluding the pandemic years (-0.157, $t = -2.01$), and estimating counts by Poisson with births as the offset rather than a constructed rate ($t = -2.70$).

It is imprecise unweighted ($t = -1.05$), so it is carried by large municipalities, and it loses significance if 2019 is dropped from the comparison base (-0.078, $t = -0.82$). Restricting the comparison to 2016 to 2018, which drops 2019 and the pandemic years together, reverses the sign ($+0.132$, $t = 1.19$), the sharpest statement of the same dependence. We therefore

Table C.4: Robustness. Alternative Difference-in-Differences Estimators

	(1) Mortality rate	(2) Infant	(3) Maternal	(4) Age 65+
TWFE, continuous exposure (per 1 SD)	-6.855***	-0.210	-6.784*	-65.559***
	(1.612)	(0.201)	(3.764)	(12.206)
TWFE, binary (above-median exposure)	-9.145**	-0.307	-13.591	-140.387***
	(4.484)	(0.650)	(12.474)	(31.990)
Callaway–Sant'Anna (binary)	-40.100***	-2.001**	-77.081***	-357.380***
	(8.511)	(0.885)	(28.424)	(74.633)
Borusyak–Jaravel–Spiess (binary)	-8.417*	-0.389	-13.806	-132.539***
	(4.443)	(0.652)	(12.382)	(31.405)
de Chaisemartin–D'Haultfœuille (binary)	-44.984***	-2.043**	-80.060***	-344.375***
	(7.983)	(0.898)	(28.510)	(53.727)
Pre-trend diagnostics				
Linear pre-trend slope (per year)	5.933*	0.559	26.651***	27.719
	(3.376)	(0.564)	(10.265)	(47.928)
Bilinski–Hatfield bound (one-sided 95%)	22.974	2.973	87.075	213.121
Rambachan–Roth robust CI ($\bar{M} = 1$)	[-37.3, -2.1]	[-2.3, 1.5]	[-81.1, 23.5]	[-271.5, 6.2]

Notes: Annual municipality panel, 2016 to 2024; unweighted; standard errors clustered by municipality in parentheses. Outcomes are annual rates, deaths per 100,000 inhabitants (the mortality rate), infant deaths per 1,000 live births, maternal deaths per 100,000 live births, and deaths of persons 65 and older per 100,000 population 65 and older. Rows 1 to 2 are two-way fixed-effects estimates with COVID-19 severity × year controls, using the continuous standardized exposure and an above-median indicator. Rows 3 to 5 binarize treatment at the median exposure, namely Callaway and Sant'Anna (2021), doubly robust with COVID severity as covariate; Borusyak, Jaravel, and Spiess (2024) imputation; de Chaisemartin and D'Haultfœuille (2020), average of three post-period dynamic effects. The linear pre-trend slope is the coefficient on exposure × linear trend estimated on 2016 to 2021 with COVID controls; the Bilinski and Hatfield (2018) bound is the largest absolute parallel-trends violation not rejected at the one-sided 5% level, scaled by the two-year average post horizon; the Rambachan and Roth (2023) row reports the robust confidence set for the average post-period effect allowing post-period violations as large as the maximum pre-period violation ($\bar{M} = 1$). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

read the infant margin as supported but less firmly than the overall and elderly margins.

The above-median contrast compares dramatically different municipalities (mean exposure 32 versus 4 permits per 1,000; mean population 73,000 versus 16,000; computed from the annual estimation panel of Appendix Table C.4 and reported in the replication package), and the doubly-robust estimators absorb differential COVID recovery only partially, where the two-way fixed-effects rows control for it directly, which is why the Callaway and Sant'Anna (2021) and de Chaisemartin and D'Haultfœuille (2020) magnitudes are several times larger while the imputation estimator, which nets out fitted counterfactuals, sits close to the two-way fixed-effects estimate. We therefore read these rows as confirming the sign and significance of the effect, not its magnitude.

Because our treatment is a continuous dose, we follow Callaway et al. (2024) in reading the two-way fixed-effects coefficient as an average of dose-specific effects rather than a structural slope, and we lean on the exhibits that do not impose linearity in the dose, namely the binarized estimates here and the monotonic exposure-quartile profile in Table C.5, Panel B, which together show the conclusions do not hinge on the linear-in-dose specification.

Effects survive relaxing the parallel-trends assumption. Appendix Table C.4 also reports several pre-trend diagnostics. Estimated linear pre-trends are small and statistically insignificant except for a marginal trend in overall mortality and a significant maternal trend, which is why the maternal margin is reported as suggestive and probed with the honest-inference bounds reported in the same table. These diagnostics come from the unweighted annual panel; the exposure-specific trend rows of Appendix Table C.1 fit the weighted monthly specification.

Both find positive fitted pre-period trajectories, and the trend-augmented estimates are accordingly larger, so both imply that the baseline estimates are not upward biased. The non-inferiority analysis of Bilinski and Hatfield (2026) shows that the estimated violations are well below the Callaway and Sant'Anna (2021) treatment effect for the elderly outcome; for the maternal outcome the non-inferiority bound exceeds the estimated effect, so a pre-

trend violation of the observed size could account for it, a further reason we treat that margin as suggestive. We also implement the sensitivity analysis of Rambachan and Roth (2023), allowing post-treatment deviations from parallel trends as large as the largest violation observed before treatment. Under this worst-case assumption ($\bar{M} = 1$), the robust confidence interval for the overall mortality rate excludes zero, the elderly interval only marginally includes it, and the infant and maternal intervals include zero. In the weighted specification the overall and elderly margins have breakdown values of $\bar{M}^* \approx 1$. Inference is also robust to the number of clusters, wild-cluster bootstrap p-values for the baseline coefficients are 0.000 for the mortality rate, 0.014 for infants, and 0.000 for adults 65 and older, while the maternal estimate remains imprecise at 0.113 (Appendix Table C.5, Panel B).

Other specification and sample alternatives. Appendix Table C.1 shows that the results are robust to a wide range of alternative specifications.

These include dropping the regression weights, excluding the pandemic years (2020-2021), adding department × month fixed effects, excluding the seven border departments, excluding the five largest cities, replacing permits delivered with the total number of ETPV registrants as the exposure measure, removing the COVID severity controls, and controlling for the average exposure of municipalities within fifty kilometers, which addresses cross-municipality spillovers and leaves the estimates larger rather than smaller. Across these specifications the coefficients remain stable in sign and magnitude for the overall and elderly outcomes (the imprecise maternal estimate changes sign when border departments are excluded), and the elderly effect remains statistically significant in all of them; the infant estimate loses precision without the population weights, with department × month fixed effects, when controlling for baseline 2018 migrant presence, and when the border departments are excluded.

Effects are not driven by the determinants of exposure or by the reference period. The thirteen standardized pre-shock municipal characteristics used in our trend-augmented robustness jointly explain only 16 percent of the cross-municipal variation in exposure (Appendix Table C.5, Panel A). The largest loadings are rurality (negative), baseline insurance

deprivation (positive), and poverty (negative), followed by distance to Bogotá and the municipal performance score; all are modest and absorbed by the characteristic-specific trends in the robustness battery. The estimates are robust to allowing each characteristic to have its own time trend. Because the program was announced in February 2021 and registration continued throughout the year, one concern is that the pre-period running through 2021 may be contaminated by anticipation effects. Re-estimating the model after excluding the pandemic years entirely, using 2016 to 2019 as the reference period, leaves the mortality, infant, and elderly estimates statistically significant and within one-third of the baseline magnitudes.

The maternal estimate keeps its sign and its magnitude but loses significance, the one outcome of the four that does. Another concern is the staggered rollout of COVID-19 vaccines, which reached large cities first. The department × month specification in Appendix Table C.1 fully absorbs department-level differences in vaccine roll-out, leaving only within-department variation as a potential source of bias. Finally, the dose response for the overall mortality rate is monotonic. Estimated effects across exposure quartiles increase from −0.7 to −1.3 to −2.8 deaths per 100,000 (Appendix Table C.5, Panel B), the maternal quartile profile, by contrast, is flat to declining, another reason that margin is suggestive. The estimates therefore are not driven solely by the municipalities with the greatest exposure.

Effects are not driven by differential trends in municipal characteristics. A natural concern is that municipalities receiving more migrants differed systematically from others in ways that generated distinct mortality trajectories after 2022, independently of expanded health coverage. Additional rows of Appendix Table C.1 address this concern by augmenting the baseline specification with a demanding set of controls. We include thirteen standardized pre-shock municipal characteristics from the CEDE panel, including poverty (NBI), census measures of health-access and insurance deprivation, the rurality index, altitude, distance to the department capital and to Bogotá, water and sewerage coverage, the municipal performance score, health transfers per capita, homicides per capita, and internal-displacement inflows per capita, allowing each characteristic to have its own linear time trend. We then

estimate two even more demanding specifications.

One interacts baseline internal-displacement inflows with a full set of year indicators. The other allows high-exposure municipalities to follow their own linear time trend, so identification comes only from deviations around an exposure-specific trajectory. Across all three specifications, the estimated effects remain stable or become larger than in the baseline. Indeed, allowing exposure-specific trends raises the estimated effects by between a half and nearly fourfold because the fitted pre-treatment trend is positive and statistically significant for every outcome, that is, mortality in high-exposure municipalities was worsening before permits were issued. These results indicate that differential trends associated with observable municipal characteristics cannot explain the estimated mortality declines. If anything, accounting for pre-existing trajectories suggests that the baseline estimates are conservative.

Effects are not mechanically driven by changes in population composition. The estimated mortality declines are not driven by changes in population composition or measurement. Mortality rates are calculated using predetermined, census-based population projections rather than contemporaneous population estimates, so changes in migration flows cannot mechanically affect the denominators. The nationality-disaggregated data show that the declines are concentrated among resident deaths, which are overwhelmingly Colombian, whereas recorded deaths of Venezuelan migrants, despite likely undercounting, show no comparable decline.

The nationality-specific event studies reinforce this conclusion dynamically (Figures B.6, B.7, B.8, and B.9). Deaths among Colombians closely replicate the main event-study results, with the same upward pre-period drift followed by post-delivery declines in causes of death amenable to medical care. By contrast, recorded deaths among Venezuelans increase only during the 2019 migration wave and remain flat after the large-scale permit roll-out. The post-2022 mortality reductions therefore cannot be explained by changes in the number of migrant deaths entering or leaving the data.

Who is in the counts. The 2024 vital-statistics records, the first to identify country of birth, let us measure directly how many non-Colombians sit inside our outcome counts. For the adult margins the answer is almost none. Venezuelan-born individuals account for about one percent of all deaths and one half of one percent of deaths at ages 65 and older, so the mortality declines we estimate are declines among Colombians as a matter of arithmetic, not assumption. For the birth-based margins the composition is larger and varies with treatment.

Venezuelan-born mothers account for 7.8 percent of 2024 births in the estimation sample, rising from 2.5 percent in the lowest exposure quartile to 12.1 percent in the highest, about 2.7 percentage points per standard deviation of exposure. The birth-certificate microdata bound the resulting distortion directly, since Venezuelan-born and Colombian-born mothers have nearly identical outcome levels in 2024 (low birth weight 11.5 versus 11.2 percent, preterm birth 10.7 versus 11.2 percent), so the treatment-correlated composition shift moves municipal birth-outcome rates by under two hundredths of a percentage point per standard deviation of exposure. Colombian nationality law resolves most of the resulting ambiguity. Children born in Colombia to Venezuelan parents since August 2015 are Colombian nationals under the Primero la Niñez measure (Congreso de la República de Colombia, 2019), in force through 2027, so the infant mortality declines are declines in the deaths of Colombian children under either reading. Only the channel, migrant household or shared health system, is mixed. The maternal margin is the one outcome where Venezuelan nationals enter the numerator in a non-negligible and treatment-correlated share, which is an additional reason we report it as suggestive.

Table C.5 reports the underlying estimates: Panel A the regression of exposure on pre-shock characteristics, Panel B the exposure-quartile estimates and the wild-cluster bootstrap p-values.

Table C.5: Exposure Determinants, Dose Response, and Wild-Cluster Inference

Panel A. Determinants of exposure (cross-section of municipalities)

	Coefficient	Robust s.e.
Poverty (NBI)	-0.156***	(0.051)
Health-access deprivation (IPM)	0.006	(0.022)
Insurance deprivation (IPM)	0.228***	(0.078)
Rurality index	-0.243***	(0.051)
Altitude	0.022	(0.028)
Distance to department capital	-0.084*	(0.045)
Distance to Bogotá	0.153***	(0.038)
Water coverage	-0.049	(0.031)
Sewerage coverage	-0.017	(0.049)
Municipal performance score	0.122***	(0.029)
Health transfers per capita	0.064	(0.068)
Homicides per capita	0.100**	(0.045)
Displacement inflows per capita	-0.033	(0.024)
R^2	0.162	
Municipalities	1,121	

Panel B. Dose response and wild-cluster inference (headline outcomes)

	Mortality rate	Infant	Maternal	Age 65+
Quartile 2 × post	-0.682	-1.032	-27.875**	-4.774
	(0.556)	(0.818)	(12.425)	(3.777)
Quartile 3 × post	-1.301***	-0.976	-17.227	-12.464***
	(0.481)	(0.803)	(11.718)	(3.404)
Quartile 4 × post	-2.772***	-1.200	-17.556	-26.306***
	(0.591)	(0.766)	(11.326)	(4.901)
Wild-cluster bootstrap p (baseline)	0.000	0.014	0.113	0.000

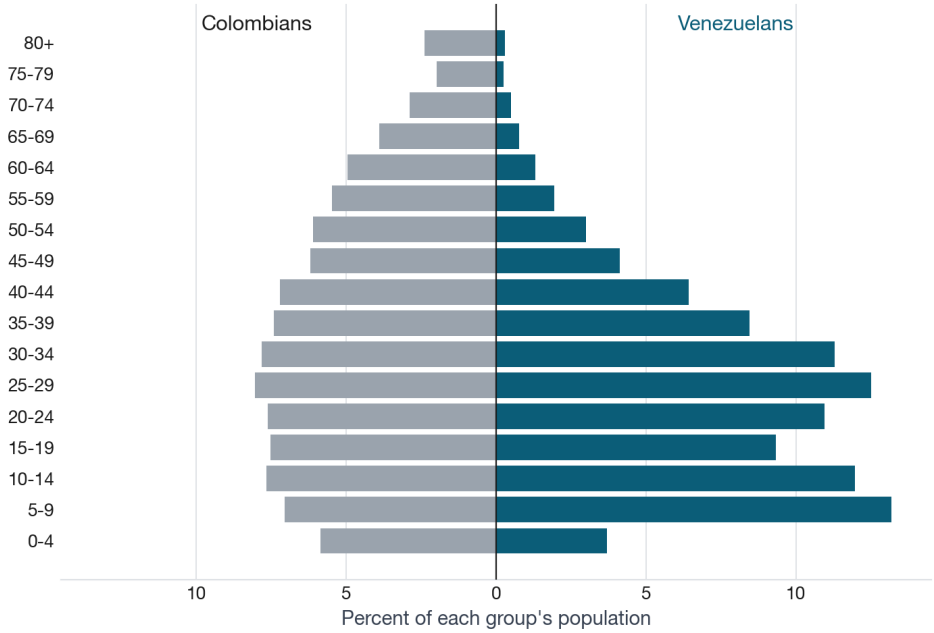
Notes: Panel A reports a cross-sectional regression of standardized fixed PPT exposure on the thirteen standardized pre-shock CEDE municipal characteristics used in the trend-augmented robustness row of Appendix Table C.1, with robust standard errors. Panel B reports coefficients on exposure-quartile × post indicators (bottom quartile omitted), in each outcome's units (deaths per 100,000 for the mortality rate and 65+; per 1,000 live births for infants; per 100,000 live births for maternal), from the baseline municipality-month specification with municipality and month fixed effects and COVID-19 severity × year controls, weighted (population, births, or population 65 and older as appropriate) and clustered by municipality. The final row reports wild-cluster bootstrap p-values (999 replications, Rademacher weights) for the baseline continuous exposure × post coefficient of each outcome, from the same specification with month fixed effects entered as regressors. Source. Clean municipal panel and CEDE. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Mechanisms, Additional Evidence

D.1 The age structure of migrants

Figure D.1 shows the population pyramids of Colombians and Venezuelans in the GEIH surveys for 2022 to 2024. The migrant population is concentrated between ages 15 and 44 and thins out rapidly after 50, visual confirmation of the demographic argument in Section 5, migrants are simply too young to contribute meaningfully to deaths at older ages, so the adult mortality declines are native by arithmetic.

Figure D.1: Population Pyramids. Colombians and Venezuelans



Notes: Survey-weighted shares of each nativity group by five-year age bins, GEIH 2022 to 2024 pooled, 25 main cities. Colombians shown to the left of the axis and Venezuelans (born abroad) to the right; each side sums to one hundred percent of its group. Source. GEIH.

D.2 Care use and enrollment, estimates in levels

Tables D.1 to D.3 report in levels, with standard errors and outcome means, the estimates that Figure 7 plots as a percent of the mean: enrollment of Colombians and of PPT holders,

Colombians' use of care, and migrants' use of care.

Table D.1: Insurance Coverage of Colombians and of PPT Holders

	(1) PPT holders enrolled	(2) Colombians enrolled	(3) Colombians, subsidized	(4) Colombians, contributory
$PPT_m \times Post_t$	14.558***	0.562	6.500**	-5.963***
	(1.108)	(2.252)	(2.736)	(2.022)
Percent of mean	63.06	0.05	1.24	-1.17
95% CI, percent of mean	[53.7, 72.5]	[-0.4, 0.5]	[0.2, 2.3]	[-1.9, -0.4]
Mean dep. var.	23.08	1038.49	525.77	511.84
Observations	93,996	93,996	93,996	93,996

Notes: Each column reports the coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and $Post_t$ equals one from January 2022 onward, from a separate municipality-month regression, 2018 to 2024, of affiliates per 1,000 inhabitants on the indicated group, with municipality and month fixed effects and COVID-19 severity × year controls, weighted by population and clustered by municipality. Affiliates are counted from the BDUA registry by identification document: Colombians hold the *cédula de ciudadanía*, *tarjeta de identidad*, *registro civil* or *certificado de nacido vivo*; PPT holders are identified by their permit. Columns (3) and (4) do not sum to column (2), because a small number of Colombians are enrolled in the special regimes (armed forces, police, and the state oil company) that are counted in the total but shown in neither column. Means are taken over the post-program period. The 95 percent confidence interval is expressed as a percentage of that mean, since the informative content of columns (2) to (4) is what they rule out. The registry reports identification type only from 2017 and is incomplete in that year, hence the 2018 sample start, and it retains duplicate and inactive records, so enrollment exceeds projected population; municipality fixed effects absorb these level differences. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.3 Ruling out a local income channel

Because the PPT bundled health coverage with the right to work, the mortality effects could in principle reflect a local economic boom rather than health care. Two facts argue against an income channel. First, the prior literature finds regularization left native labor markets essentially unaffected (Bahar et al., 2021), and the migrant population is too young for its own income gains to appear in adult mortality, Venezuelan-born individuals account for under one percent of deaths at ages 65 and older in 2024. Second, Appendix Table D.4 tests the local-activity margin directly, night-light density and municipal tax revenue show no increase with exposure after 2022. The population-weighted night-light coefficient is in fact slightly negative, the unweighted coefficient is zero, and tax revenues are flat. Whatever

Table D.2: Congestion Relief. Colombian Health Service Use by Type of Care

	(1) Emergency visits	(2) Hospital-izations	(3) Outpatient consults	(4) Procedures	(5) Emergency share
$PPT_m \times Post_t$	-0.601*	-0.084	9.841***	30.995***	-0.096**
	(0.354)	(0.075)	(2.945)	(7.409)	(0.044)
with 2018 migrant presence × Post control	-0.604	-0.013	14.210***	45.379***	-0.143**
	(0.443)	(0.077)	(3.501)	(9.666)	(0.056)
Percent of mean (baseline)	-7.0	-1.9	4.2	7.1	-6.7
Mean dep. var.	8.6	4.3	235.3	439.2	1.4
Observations	120,960	120,960	120,960	120,960	120,634

Notes: Each cell reports the coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and $Post_t$ equals one from January 2022 onward, from a separate municipality-month regression, 2016 to 2024. Outcomes in columns 1 to 4 are encounters by Colombian citizens (document types CC, TI, and RC) of the indicated care type per 1,000 inhabitants; column 5 is emergency visits as a percentage of all Colombian encounters. All regressions include municipality and month fixed effects and COVID-19 severity × year controls, are weighted by population, and cluster standard errors by municipality. The second coefficient row adds migrant patients observed in the health records in 2018 per 1,000 inhabitants, standardized and interacted with the post indicator, identifying the effect from permit coverage conditional on baseline migrant presence. Source. RIPS (SISPRO), Colombian-citizen utilization panel. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.3: Migrant Care Composition and PPT Exposure

	Outpatient care (per 1,000)	Emergency care (per 1,000)	Emergency share (% of migrant care)
Exposure × post	1.940***	0.041	-0.469***
	(0.704)	(0.028)	(0.101)
% of mean	87.9	51.3	-11.8
Mean dep. var.	2.207	0.081	3.969
Observations	120,960	120,960	85,013

Notes: Municipality-month regressions of migrant care volumes and composition on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, with municipality and year-month fixed effects, COVID-19 severity × year controls, weighted by population, standard errors clustered by municipality. Migrant care pools all migrant document types (PEP, PPT, undocumented, and other non-citizen documents) from the RIPS encounter records; outpatient care combines consultations and procedures, emergency care is urgent care, and the emergency share is emergency encounters as a percent of migrant outpatient-plus-emergency care. Because irregular migrants are undercounted in the records before regularization, the outpatient coefficient combines real increases in care with improved recording of previously undocumented patients. Both margins reflect incorporation into the formal system. *, **, *** denote significance at the 10, 5, and 1 percent levels. Source. RIPS (SISPRO) and clean municipal panel.

is reducing mortality in high-exposure municipalities, it is not a contemporaneous economic expansion.

Table D.4: Mechanisms. Growth, Labor Markets, and the Health Workforce

Panel A. Local economic activity						
	Night lights (weighted)	Night lights (unweighted)	Tax revenue (weighted)	Tax revenue (unweighted)		
$PPT_m \times Post_t$	-0.007*	0.006	-0.001	0.007		
	(0.004)	(0.005)	(0.010)	(0.007)		
Observations	10,044	10,044	8,784	8,784		
Panel B. Labor markets by nativity (GEIH, 25 main cities)						
	Employ. Col.	Employ. Ven.	Formal. Col.	Formal. Ven.	Log inc. Col.	Log inc. Ven.
$PPT_m \times Post_t$	0.002	0.001	-0.009	-0.004	-0.011	0.033
	(0.007)	(0.006)	(0.007)	(0.007)	(0.015)	(0.023)
Observations	220	219	220	219	219	219
Panel C. Health-professional employment by nativity (GEIH, per 1,000 working-age)						
	Colombians	Venezuelans				
Health professionals (physicians, dentists, nurses)	-0.542*	0.311				
	(0.303)	(0.271)				
Mean dep. var.	8.61	4.66				
Observations	220	219				

Notes: Col. denotes Colombians and Ven. Venezuelans. Panel A reports the coefficient on $PPT_m \times Post_t$ from municipality-year regressions of log VIIRS night-light density (2016 to 2024) and log municipal tax revenue (2016 to 2023), with municipality and year fixed effects and COVID-19 severity × year controls; weighted columns use population weights; standard errors clustered by municipality. Panel B uses the GEIH household surveys (2016 to 2024), aggregated to city-year × nativity cells for the 25 main cities the survey covers. Employment is the employed share of the population aged 15 to 64, formality the formal share of the employed, and income the mean log real monthly labor income; regressions are weighted by cell survey weight, include city and year fixed effects and COVID-19 severity × year controls, and cluster standard errors by city (25 clusters, so Panel B inference is indicative). Colombians show no labor-market response on any margin, so an indirect income channel cannot account for the native mortality declines; the migrant income point estimate is positive but imprecise. Panel C reports health professionals (physicians, dentists, and nursing and midwifery personnel) per 1,000 working-age residents of each nativity group, on the same GEIH city-year cells as Panel B (25 main cities, 2016 to 2024, by nativity), identifying health professionals by occupation harmonized across the 2022 change from the CNO-70 to the CIUO-08 occupational classification. Measured on this consistent basis, the change in Venezuelan health-professional employment associated with municipal exposure is small and not statistically distinguishable from zero, and the native estimate is imprecise and not robust across alternative definitions, so these data provide no clear evidence on the health-workforce channel, inference rests on 25 city clusters and is indicative, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.4 Crime

Table D.5 sharpens the external-cause placebo into a test of the crime channel. Homicide deaths, the outcome the channel operates through, show no response to exposure (a coefficient of -0.07 on a mean of 2.2 monthly homicides per 100,000, imprecise and small).

Whatever regularization did to crime, it is not the source of the mortality declines.

Table D.5: Mechanism Test. Homicide Mortality

	Homicide deaths (per 100,000)
$PPT_m \times Post_t$	-0.068
	(0.063)
Mean dep. var.	2.215
Observations	120,960
Municipality FE	✓
Month FE	✓
COVID sev. × year	✓

Notes: Coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, (January 2022 onward) from the municipality-month specification of Table 2, with monthly homicide deaths (ICD-10 X85 to Y09) per 100,000 inhabitants as the outcome, weighted by population, standard errors clustered by municipality. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.5 The cause structure of the mortality declines

Table D.6 decomposes the all-cause mortality effect into eight causes of death. The declines concentrate in causes that timely treatment averts, influenza and pneumonia and diabetes (the cerebrovascular estimate is negative but not significant), while communicable causes beyond the reach of treatment and external causes stay flat; COVID-19 carries the largest single share. Section 6 uses this table to bound the transmission channel, and Table D.7 sharpens the same anatomy into the groups the data can support with adequate power.

D.6 Cause Decomposition and What the Data Can Support

Table D.7 reports, in levels, the cause decomposition that Figure 8 plots, for the two margins whose cause structure the data can actually resolve. We screen every candidate split by its minimum detectable effect, the smallest true effect a conventional test would detect with

Table D.6: Mechanisms. Cause-of-Death Decomposition

	(1) Influenza, pneumonia	(2) Other communicable	(3) Hypertensive	(4) Diabetes	(5) Ischemic heart	(6) Cerebrovascular	(7) COVID-19 (U07)	(8) External (placebo)
$PPT_m \times Post_t$	-0.073**	0.011	0.002	-0.043*	0.029	-0.019	-0.390***	-0.099
	(0.030)	(0.019)	(0.026)	(0.023)	(0.101)	(0.014)	(0.115)	(0.068)
	[0.064]	[0.433]	[0.543]	[0.121]	[0.493]	[0.233]	[0.006]	[0.224]
Percent of mean dep. var.	-4.6	0.9	0.1	-3.1	0.4	-0.7	-13.4	-1.9
Mean dep. var.	1.58	1.17	1.66	1.41	7.38	2.73	2.91	5.20
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓	✓	✓
COVID sev. × year	✓	✓	✓	✓	✓	✓	✓	✓
Observations	120,960	120,960	120,960	120,960	120,960	120,960	120,960	120,960

Notes: Each column reports the coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and $Post_t$ equals one from January 2022 onward, from a separate municipality-month regression of monthly deaths per 100,000 inhabitants from the indicated cause, 2016 to 2024. Causes, influenza and pneumonia (J09 to J18), the infectious deaths most responsive to timely treatment; other communicable diseases (A00 to B99); the chronic-management causes hypertensive disease (I10 to I15), diabetes (E10 to E14), ischemic heart disease (I20 to I25), and cerebrovascular disease (I60 to I69); COVID-19 (U07); and external causes (V01 to Y98), which health coverage should not affect, as a placebo. All regressions include municipality and month fixed effects and COVID-19 severity × year controls, are weighted by population, and cluster standard errors by municipality. Sharpened two-stage false-discovery-rate q-values across the eight tests in square brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

eighty percent power, and report only groupings whose components clear that screen at a magnitude comparable to the aggregate effects estimated in Table 2. This screen excludes a good deal. Cause-by-cause splits of infant deaths have minimum detectable effects between ten and one hundred twenty percent of their means, the maternal margin cannot be split at all, and the separate 1 to 4 age band carries a minimum detectable effect of ten percent of its mean, which is why the age table pools ages 1 to 19. What survives is a four-group split of deaths at ages 65 and older and a two-group split of infant deaths, and the two tell the same story.

The interpretation is in Section 6; the table adds the death counts, the mean rates, and the minimum detectable effect of each split.

D.7 Childhood vaccination

Table D.8 reports the vaccination test behind Section 6.2. Panel A is the coverage ratio the immunization program publishes, and Panels B and C split it into doses applied and the

Table D.7: Cause Decomposition of the Elderly and Infant Mortality Effects

	(1) Deaths	(2) Mean rate	(3) $PPT_m \times Post_t$	(4) Percent of mean	(5) Share of margin effect	(6) MDE, percent of mean
Panel A. Deaths at ages 65 and older, per 100,000 population aged 65 and older						
All causes	1,552,769	308.649	-8.117*** (1.646)	-2.6	100.0	1.5
Ambulatory-care sensitive	345,987	68.773	-2.515*** (0.808)	-3.7	31.0	3.3
Acute, emergency-dependent	444,819	88.418	-0.104 (1.042)	-0.1	1.3	3.3
COVID-19	99,971	19.872	-3.678*** (0.702)	-18.5	45.3	9.9
All other causes	661,992	131.586	-1.819*** (0.703)	-1.4	22.4	1.5
Panel B. Infant deaths, per 1,000 live births						
All causes	59,056	11.091	-0.225*** (0.073)	-2.0	100.0	1.8
Determined at or before birth	45,940	8.628	-0.044 (0.064)	-0.5	19.7	2.1
Amenable to postnatal care	13,116	2.463	-0.181*** (0.041)	-7.3	80.3	4.7
Infectious causes (transmission bound)	5,017	0.942	-0.012 (0.025)	-1.3	5.3	7.5

Notes: Column (3) reports the coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and $Post_t$ equals one from January 2022 onward, from a separate municipality-month regression, 2016 to 2024, with standard errors clustered by municipality in parentheses. All regressions include municipality and month fixed effects and COVID-19 severity × year controls, and are weighted by population aged 65 and older (Panel A) or births (Panel B). Column (5) expresses the estimate as a percentage of the corresponding all-causes estimate for the same margin, so the group rows sum to roughly one hundred. Column (6) reports the minimum detectable effect, the smallest true effect a two-sided test at the five percent level would detect with eighty percent power, equal to 2.802 standard errors, expressed as a percentage of the group mean. Only decompositions whose groups clear this screen are reported; cause-by-cause splits of infant and maternal deaths, and the separate 1 to 4 age band, have minimum detectable effects between ten and one hundred twenty percent of their means and are therefore uninformative and omitted. Ambulatory-care-sensitive causes are influenza and pneumonia (J09 to J18), diabetes (E10 to E14), chronic lower respiratory disease (J chapter excluding influenza and pneumonia), and hypertensive disease (I10 to I15), conditions whose fatal outcomes routine outpatient management is understood to prevent. Acute, emergency-dependent causes are ischemic heart disease (I20 to I25) and cerebrovascular disease (I60 to I69), which require intervention within hours. Infant deaths determined at or before birth are the perinatal (P) and congenital (Q) chapters; the amenable group is all remaining infant deaths. The infectious row (intestinal and other infectious chapters plus respiratory infections) fails the minimum-detectable-effect screen and is reported only because it bounds the transmission channel, its confidence interval is correspondingly wide. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

target population, so the reader can see which of the two moved.

Table D.8: Childhood Vaccination and PPT Exposure

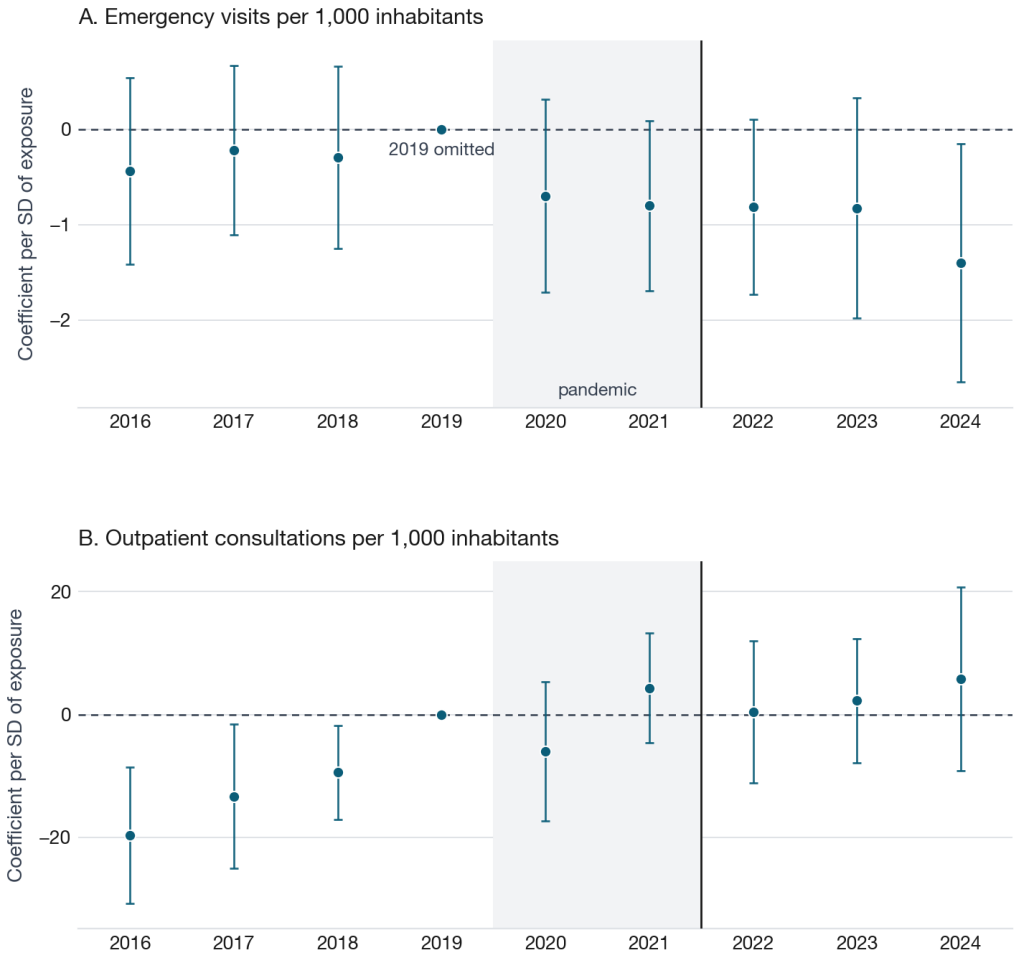
	(1) Pentavalent	(2) Polio	(3) MMR	(4) BCG	(5) Pneumococcal	(6) Rotavirus
Panel A. Coverage ratio, doses applied per 100 children in the program target						
$PPT_m \times Post_t$	-1.279***	-1.026***	-1.273***	0.070	-1.242***	-1.026***
	(0.269)	(0.273)	(0.343)	(0.715)	(0.327)	(0.268)
Mean dep. var.	90.6	89.9	91.5	86.0	91.1	88.2
Panel B. Log doses applied						
$PPT_m \times Post_t$	0.0044	0.0070	0.0034	0.0472***	0.0054	0.0066
	(0.0073)	(0.0081)	(0.0098)	(0.0103)	(0.0071)	(0.0064)
Panel C. Log program target population						
$PPT_m \times Post_t$	0.0178***	0.0178***	0.0177***	0.0178***	0.0178***	0.0178***
	(0.0067)	(0.0067)	(0.0067)	(0.0067)	(0.0067)	(0.0067)
Observations	10,038	10,038	10,038	10,038	10,038	10,038

Notes: Each cell reports the coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and $Post_t$ equals one from 2022 onward, from a separate municipality-year regression, 2016 to 2024, of the indicated outcome for one of the six core childhood vaccines. Panel A is the coverage ratio the immunization program reports, doses applied per 100 children in its target population, censored at 200. Panels B and C separate the two parts of that ratio, the log of doses applied and the log of the target population, so that a change in the ratio can be attributed to the numerator or to the denominator. All regressions include municipality and year fixed effects and COVID-19 severity × year controls, are weighted by population, and cluster standard errors by municipality. Source. Ministry of Health immunization program (PAI) records via SISPRO; clean municipal panel. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.8 Congestion relief and capacity, dynamics

Figure D.2 shows the event studies behind Panel B of Figure 7 and Table D.2. Colombian emergency visits are statistically indistinguishable from flat relative to exposure before the pandemic (joint pre-trend $p = 0.24$), and they decline after mass delivery, reaching -1.4 per 1,000 inhabitants by 2024. The outpatient-consultations panel is less clean, as it displays a pronounced upward pre-trend that continues into the post-period, so we do not read the post-2022 increase as cleanly attributable to the program, and we rest the congestion-relief argument on the fall in the emergency share of Colombian care, which is estimated precisely, rather than on the level decline, which is not.

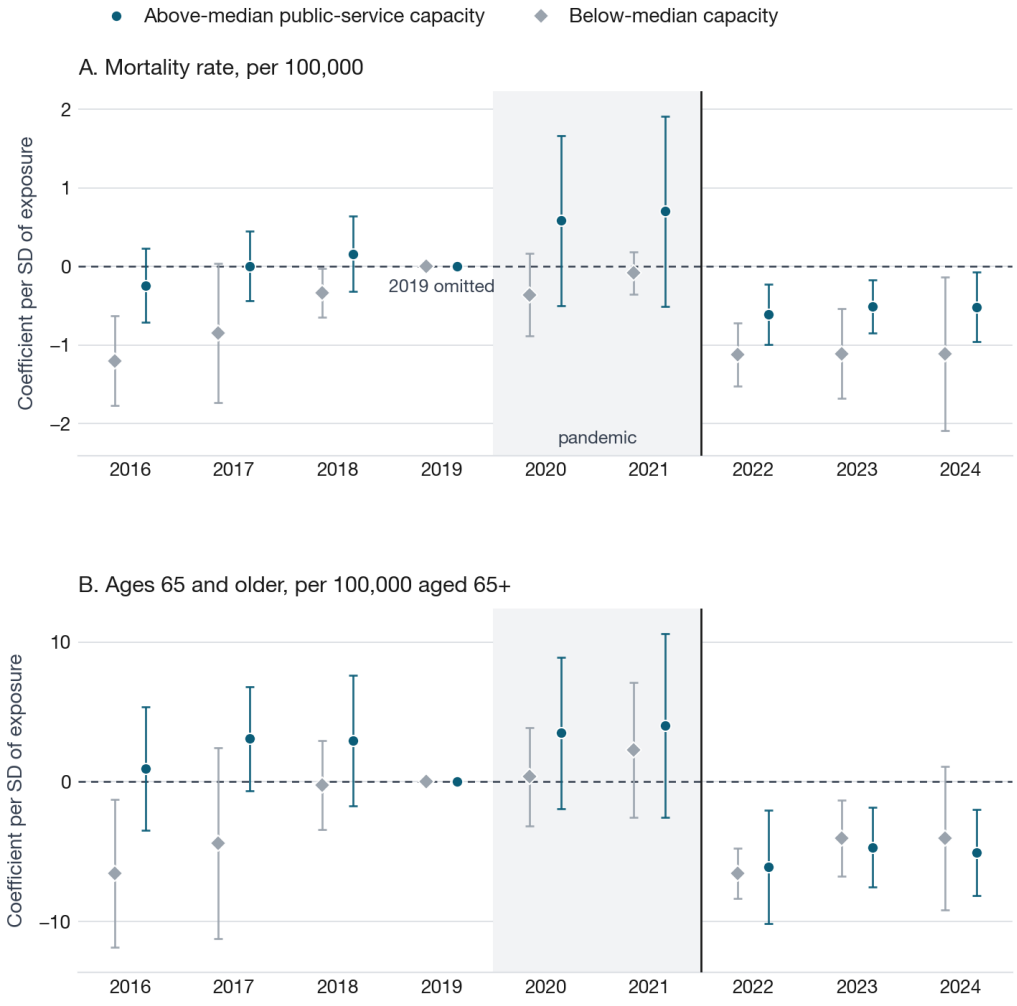
Figure D.2: Event Studies. Colombian Emergency and Outpatient Care and PPT Exposure



Notes: Yearly coefficients on $PPT_m \times$ year indicators, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, from municipality-month regressions of Colombian citizens' encounters (document types CC, TI, RC) per 1,000 inhabitants, by type of care; 2019 omitted. Municipality and month fixed effects and COVID-19 severity $\times$ year controls; weighted by population; standard errors clustered by municipality; bars are 95% confidence intervals. Source. RIPS (SISPRO) Colombian utilization panel and clean municipal panel.

Figure D.3 estimates the mortality event studies separately for municipalities above and below the median of the public-service capacity index. Whether the effect is larger where local services had the capacity to absorb it is a natural question, and we tested it three ways before concluding that we cannot answer it. Comparing the two groups' post-period event-study coefficients is not invariant to the omitted year, and reverses sign when the reference moves. Giving each group its own difference-in-differences is invariant, but the difference it produces comes from the pre-period rather than from what happens after the program, since the two groups reach nearly the same post-2022 level and differ in where they started. The test that settles the question is the event-study triple difference, exposure interacted with the capacity index and with year indicators, with capacity's own trajectory absorbed. We apply it to all three moderators. Capacity fails, since for the overall rate the interaction is significantly positive before the program (joint $p = 0.02$) and indistinguishable from zero after it ($p = 0.57$), and for the elderly rate neither period is jointly significant ($p = 0.19$ and $p = 0.24$). The vulnerable-demand index fails in the same way, with pre-period $p = 0.001$ on the overall rate and $p = 0.006$ on the elderly. Supply-side funding passes, its interaction being flat before mass delivery ($p = 0.14$ and $p = 0.83$) and significant afterwards on the overall rate ($p = 0.003$), which is what a genuine moderator produces. Supply-side transfers enter the capacity index as one of its four components, so it is worth saying why the index can fail while the component passes. Funding is nearly orthogonal to the other three, correlating -0.02 with aqueduct coverage, 0.01 with sewerage coverage and 0.08 with the municipal health results score, while the two utility measures correlate 0.63 with each other. The index therefore measures utility infrastructure more than health financing, and averaging funding in with the rest loses the margin that survives on its own. The moderation therefore reflects a differential pre-trend by capacity rather than a break at mass delivery, and we make no claim about a capacity channel. Table 3 reports no capacity panel for the same reason. The congestion-relief reading rests instead on the care-switch evidence, on the fall in the emergency share of Colombian care, and on the decongestion event study, none of which depends on this comparison.

Figure D.3: Mortality Event Studies by Baseline Service Capacity



Notes: Yearly coefficients on $PPT_m \times$ year indicators, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, estimated separately for municipalities above (blue) and below (gray) the median of the pre-shock public-service capacity index, the standardized mean of water and sewerage coverage, the municipal health results score, and supply-side health transfers per capita; 2019 omitted. Specifications as in Figure 6, with municipality and month fixed effects and COVID-19 severity $\times$ year controls, weighted by the relevant population, standard errors clustered by municipality, bars are 95% confidence intervals. Source. clean municipal panel and CEDE.

D.9 The composition of migrant emergency care

Table D.9 examines the congestion mechanism inside migrant emergency care itself. Levels of migrant emergency visits are not precisely estimated because regularization also brought previously unrecorded migrants into the administrative data, so the informative margins are compositional. The share of migrant emergency visits accounted for by avoidable, ambulatory-care-sensitive conditions (chronic, respiratory, vaccine-preventable, and waterborne diagnoses) falls significantly with exposure, the admission rate conditional on an emergency contact is imprecise, consistent with milder cases leaving the emergency queue for outpatient settings, and obstetric emergency visits, which track births rather than access, do not respond. Nationally, migrant emergency visits rise only modestly after mass delivery while the covered population grew to nearly two million, implying a sharp decline in emergency use per covered migrant.

Table D.9: Within-Emergency Composition of Migrant Care

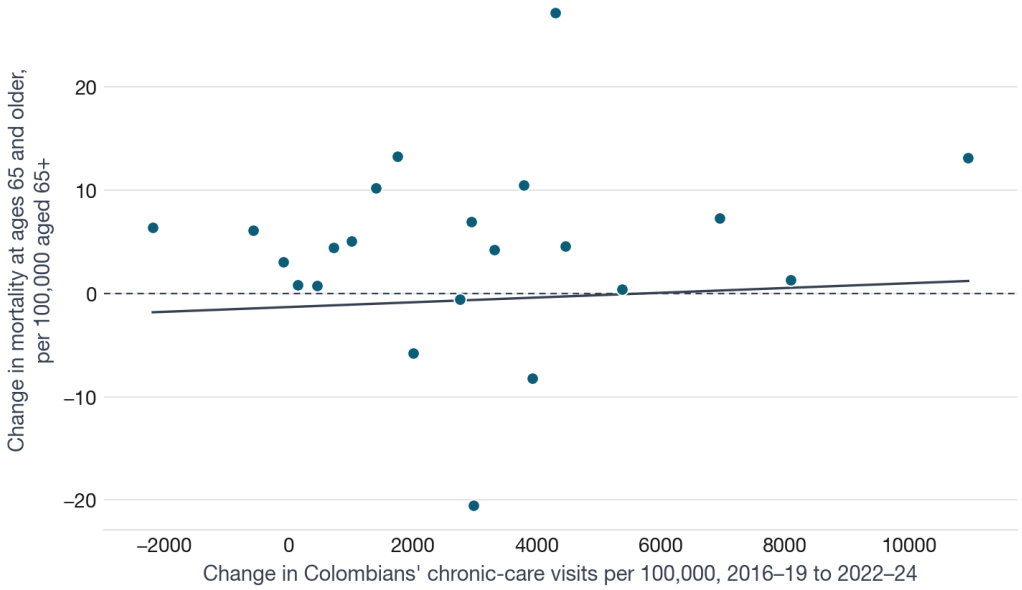
	Avoidable ER	Non-avoid. ER	Avoidable share	Admission rate	Obstetric ER
	(1)	(2)	(3)	(4)	(5)
	(per 1,000)	(per 1,000)	(%)	(%)	(per 1,000)
Exposure × post	0.005	0.042*	-0.353**	0.917	0.001
	(0.004)	(0.025)	(0.175)	(0.638)	(0.004)
% of mean	45.5	53.2	-3.0	1.8	6.3
Mean dep. var.	0.01	0.08	11.78	51.06	0.01
Observations	120,960	120,960	34,910	49,824	120,960

Notes: Each column reports the coefficient on $PPT_m \times Post_t$, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, and $Post_t$ equals one from January 2022 onward, from a municipality-month regression, 2016 to 2024, for migrant (non-Colombian document) users. Avoidable emergency visits are those with chronic, respiratory, vaccine-preventable, or waterborne diagnoses in the RIPS unified disease panel; non-avoidable are all remaining non-obstetric emergency visits; the avoidable share is avoidable visits as a percentage of all migrant emergency visits; the admission rate is hospitalized patients as a percentage of emergency plus hospitalization patients; obstetric visits carry maternal diagnoses. All regressions include municipality and month fixed effects and COVID-19 severity × year controls, are weighted by population, and cluster standard errors by municipality. Because regularization increases the recording of migrant care, level increases combine real care and improved registration; the compositional columns (3) and (4) difference this visibility margin out. Source. RIPS (SISPRO) unified disease panel and migrant care panel. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.10 A mediation test through chronic care

If the congestion-relief channel operated through Colombians' chronic-disease management specifically, municipalities where Colombians' chronic-care visits rose most should be where elderly mortality fell most. Figure D.4 examines this directly, plotting the municipal change in elderly mortality between 2016 to 2019 and 2022 to 2024 against the change in Colombians' chronic-care visits. The relationship is null, a standardized slope of 0.07 (standard error 0.05) conditional on exposure. We read this as evidence that the mortality gains do not travel through the volume of chronic consultations in particular, they are more consistent with the timeliness margins of care, emergency access and early treatment, that visit counts do not capture. The test also cautions against interpreting the rise in Colombians' outpatient consultations reported in Table D.2 as the mediating pathway.

Figure D.4: Municipal Changes in Chronic-Care Visits and Elderly Mortality



Notes: Binned scatter (25 population-weighted bins) of the municipal change in monthly deaths per 100,000 aged 65 and older against the change in Colombians' chronic-disease visits per 100,000 inhabitants, comparing the 2022 to 2024 average with the 2016 to 2019 average. The standardized slope conditional on PPT exposure is 0.07 (standard error 0.05). Source. clean municipal panel and RIPS unified disease panel.

D.11 Institutional Maternity Care

Table D.10: Mechanisms. The Institutional Maternity-Care Gap (Suggestive Margin)

	Maternal
$PPT_m \times Post_t$ [β_1]	-6.136**
	(2.706)
$PPT_m \times Post_t \times$ Baseline home-birth share (2016–19) [β_2]	-13.399*
	(6.898)
	[0.055]

Notes: The outcome is maternal mortality per 100,000 live births (weighted by births); the estimate reports β_1 and β_2 from the specification of Table 3, where X_m is the baseline home-birth share (births outside health facilities, 2016 to 2019), the marker of missing institutional maternity care. Negative β_2 means larger mortality reductions where the baseline home-birth share is higher. The maternal margin carries a significant pre-trend and is treated as suggestive throughout the paper; this heterogeneity is reported on the same terms. Sharpened false-discovery q-value in square brackets; standard errors clustered by municipality in parentheses. Estimation sample matches Table 2, column (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

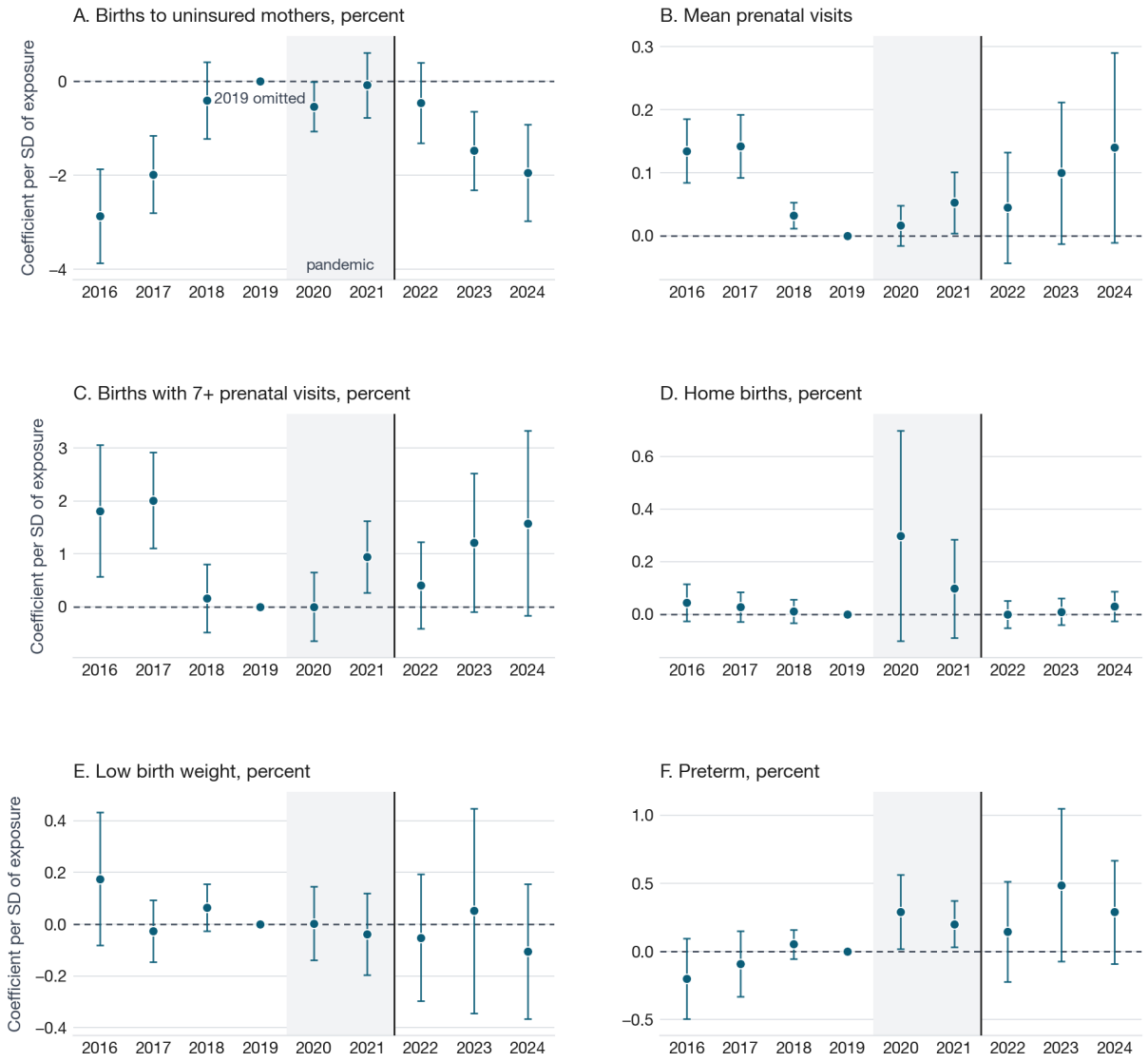
E Insurance at Birth, Prenatal Care, and Birth Outcomes

The 2024 birth-certificate microdata, the first to record the mother's country of birth, sharpen two facts used in the text. First, Venezuelan-born and Colombian-born mothers have nearly identical outcome levels (low birth weight 11.5 versus 11.2 percent; preterm birth 10.7 versus 11.2 percent), so the treatment-correlated entry of migrant mothers into the birth pool cannot materially move municipal birth-outcome rates, the bound cited in Section 5. Second, 38.7 percent of Venezuelan-born mothers were still uninsured at delivery in 2024, against 0.1 percent of Colombian-born mothers, so the direct coverage margin for migrant mothers remains far from exhausted.

This appendix uses the municipality-month panel of birth records to trace the PPT's coverage transition at the point of delivery and its consequences for prenatal care and birth outcomes. The birth certificate records the mother's insurance regime, so births to *uninsured*

mothers, before 2022, essentially the status of undocumented migrants, provide a direct, nationality-free measure of the coverage margin. Figure E.1 and Table E.1 show the results. The share of births to uninsured mothers rises with exposure through the 2018 to 2021 arrival wave and then falls monotonically after mass permit delivery, by 2024 it is 1.9 percentage points below its 2019 level in high-exposure municipalities (column 1), roughly thirty percent of the sample mean, with a partial, marginally significant shift into the subsidized regime (column 2). Prenatal-care indicators recover in parallel, mean prenatal visits and the share of births with seven or more visits dip during the arrival wave, when newly arrived migrant mothers with little prenatal contact enter the birth registry, and return to or above baseline by 2023 to 2024. Low birth weight shows no significant movement, and the preterm coefficient is positive and marginally significant in 2023 alone, which we do not read as an effect; because these pool Colombian and migrant births, compositional change works against detecting improvements, as newly regularized migrant mothers with higher-risk pregnancies remain in the registry, so mother-nationality-specific estimates (available from 2024 birthplace records, and retrospectively once additional years accumulate) are the natural next step.

Figure E.1: Event Studies. Insurance at Birth, Prenatal Care, and Birth Outcomes



Notes: Yearly coefficients on $PPT_m \times$ year indicators, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized,; 2019 omitted. Municipality-month births panel, 2016 to 2024, all births; municipality and month fixed effects, COVID-19 severity $\times$ year controls, weighted by births, standard errors clustered by municipality; bars are 95% confidence intervals. The pre-program dynamics reflect the migrant arrival wave (uninsured births accumulating in high-exposure municipalities); the post-2022 decline in Panel A is the PPT coverage transition at the point of delivery. Source. DANE vital statistics births panel and clean municipal panel.

Table E.1: PPT Exposure, Insurance at Birth, Prenatal Care, and Birth Outcomes

	(1) Unins. (%)	(2) Subsid. (%)	(3) Prenatal visits	(4) 0 visits (%)	(5) 7+ visits (%)	(6) Home birth (%)	(7) Low bw (%)	(8) Preterm (%)
$PPT_m \times$ 2022	-0.459	0.396	0.045	-0.423	0.402	-0.001	-0.052	0.146
	(0.437)	(0.387)	(0.045)	(0.340)	(0.420)	(0.026)	(0.125)	(0.187)
$PPT_m \times$ 2023	-1.478***	0.639	0.100*	-0.641**	1.208*	0.010	0.052	0.488*
	(0.428)	(0.466)	(0.057)	(0.321)	(0.670)	(0.025)	(0.202)	(0.286)
$PPT_m \times$ 2024	-1.946***	1.093*	0.140*	-0.590	1.582*	0.030	-0.105	0.290
	(0.521)	(0.560)	(0.077)	(0.405)	(0.893)	(0.028)	(0.133)	(0.193)
Mean dep. var.	6.8	27.9	6.6	3.5	53.9	0.3	12.5	9.8
Observations	116,702	116,702	116,702	116,702	116,702	116,702	116,702	116,702
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓	✓	✓
COVID sev. × year	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Municipality-month births panel (DANE vital statistics), 2016 to 2024, all births regardless of the mother's nationality. Coefficients on $PPT_m \times$ year indicators, where PPT_m is PPT permits delivered per 1,000 inhabitants (2022 population), standardized, with 2021 (the last pre-mass-delivery year) omitted, from one regression per column with municipality and month fixed effects and COVID-19 severity × year controls, weighted by births, standard errors clustered by municipality. Outcomes, share of births to mothers with no health insurance (the pre-ETPV status of undocumented migrants); share in the subsidized regime; mean prenatal visits; shares with zero and with seven or more prenatal visits; share of home births; share below 2,500g; share before 37 weeks. Year-specific coefficients are reported because the pre-period reflects the 2018 to 2021 arrival wave (uninsured migrant births accumulating in high-exposure municipalities), which a pooled post coefficient would average against. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

F Fiscal Accounting Details

This appendix documents every input, source, and formula behind the fiscal exercise of Section 7 and Table 4, in enough detail to replicate it from public sources. All computations are performed in the replication file `64_fiscal.do`, which also rebuilds the table. Quantities are annual and expressed in 2024 United States dollars at the average 2024 exchange rate of 4,070 pesos per dollar (Banco de la República).

F.1 Inputs

- **Covered population.** $C = 1{,}960{,}000$ migrants with a delivered PPT, the administrative snapshot of Migración Colombia registrations used to build the treatment in Section 3.
- **Capitation payment.** The 2024 subsidized-regime UPC is 1,256,076 pesos per affiliate per year (Ministerio de Salud y Protección Social, 2023), equal to $u = 308.62$ dollars. Newly regularized migrants overwhelmingly enter the subsidized regime, 79.4 percent of migrant affiliates at the end of 2023 (Appendix G), so the subsidized UPC, about a fifth below the contributory value, is the relevant price.
- **Pre-program emergency-regime cost.** Before regularization, undocumented migrants were entitled to emergency care only, which providers must deliver regardless of status. The fiscal accounting of Ibáñez et al. (2025) prices health spending on a representative non-regularized migrant household at 898 dollars of 2020 per year, built from the emergency-room tariff (*Manual Tarifario*), observed visit frequencies, and vaccination costs from Ministry of Health administrative sources. The representative household has four members (head, partner, one child aged 0 to 5, one child aged 6 to 18), so the per-person cost is $898/4 = 224.50$ dollars of 2020. Adjusting by cumulative United States consumer price inflation between 2020 and 2024 (a factor of 1.212) gives $e = 272.09$ dollars of 2024. For reference, the same accounting prices the full-access regime under the PEP at 550 dollars per household, 137.50 per person, or 167 dollars

of 2024 after the same inflation adjustment, which is why regularization *lowers* the per-migrant health cost, emergency care is more expensive per contact than insured routine care.

- **Baseline users (Scenario B).** The RIPS migrant panel (Section 3) records 1,696,805 migrant patient-months in 2021, summing patients across the twelve months over all non-Colombian document categories including the undocumented. Assuming a typical baseline user appears in $k = 2$ months of the year, the number of distinct baseline users is $U_B = \lfloor 1{,}696{,}805/2 \rfloor = 848{,}402$, or 43.3 percent of the covered population. Under $k = 1$ the share would be 86.6 percent, approaching Scenario A; under $k = 4$ it would be 21.6 percent, and net new spending in Scenario B would rise from 374 to 489 million dollars, leaving every conclusion unchanged.

- **Deaths averted.** $L = 6{,}500$ Colombian deaths per year, the illustrative aggregation of Section 5, namely the monthly effect of -0.83 Colombian deaths per 100,000 inhabitants per standard deviation (Table 2, Panel B), annualized ($\times 12 = 9.93$ per 100,000), divided by the 29.4 permits per 1,000 that constitute one standard deviation, and applied linearly to the 1.96 million permits delivered ($9.93/100{,}000/29.4 \times 1{,}000 \times 1{,}960{,}000 \approx 6{,}620$, which we round down to 6,500).

- **Value of a statistical life.** $V = 1.0$ million dollars for Colombia, obtained conservatively by transferring the United States guidance value of 13.2 million dollars (U.S. Department of Transportation, 2024) at an income elasticity of one, the value Viscusi and Masterman (2017) argue is appropriate for transfers to lower-income countries in place of the smaller elasticities conventional in international guidance. Colombian income per capita is about a twelfth of the United States level, so the transfer gives close to 1.1 million dollars, which we round down. The bottom line is insensitive to this choice, at one tenth the value the mortality benefit still exceeds net health spending by a factor of between 1.7 and 9.1 depending on the scenario.

F.2 Formulas

Scenario $s \in \{A, B\}$ differs only in the population assumed to have generated emergency-regime costs before the program, $U_A = C$ and $U_B = 848{,}402$. The three quantities in Table 4 are

$$\begin{aligned} \text{Net new health spending}_s &= C \cdot u - U_s \cdot e, \\ \text{Mortality benefit} &= L \cdot V, \\ \text{Net annual cost}_s &= C \cdot u - U_s \cdot e - L \cdot V, \end{aligned}$$

and the net fiscal cost per life saved is $(C \cdot u - U_s \cdot e)/L$. Substituting the inputs gives $C \cdot u = 604.9$ million, $U_A \cdot e = 533.3$ million, and $U_B \cdot e = 230.8$ million, hence net new spending of 71.6 and 374.0 million dollars, a mortality benefit of 6,500 million, net annual costs of $-6{,}428.4$ and $-6{,}126.0$ million (negative values are social gains), and 11,013 and 57,546 dollars per Colombian life saved.

F.3 Omissions and their direction

Four omissions make the exercise conservative. It assigns no value to migrants' own health gains, the direct benefit the program was designed to deliver. It ignores fiscal revenue from regularized migrants, which reduces the net fiscal cost of hosting a migrant household by roughly 42 percent in the short run (Ibáñez et al., 2025), with the share of migrants saving formally rising from about 40 to 70 percent in the medium run (Rozo, 2026). It ignores averted emergency spending on Colombians themselves, the congestion margin of Section 6. And it charges the full capitation for every covered migrant in every year, although affiliation ramped up gradually through 2022 and 2023. The one assumption working in the opposite direction is linearity of the mortality aggregation; halving the deaths-averted figure doubles the cost per life saved and leaves the net balance a gain of roughly three billion dollars per year.

G Qualitative and Documentary Evidence from Other Settings

This appendix situates the paper's quantitative estimates in the documentary record on health-coverage exclusion and integration in other countries. The mechanism we estimate for Colombia has two observable signatures that contemporaneous reporting can capture even where causal identification is unavailable. Under emergency-only regimes, excluded populations do not exit the health system; their demand is displaced to the most expensive point of care, where it appears in the record as uncompensated emergency utilization, hospital debt, late presentation, and, in some settings, preventable deaths and outbreaks. Under integration episodes, the same record shows enrollment, financing that follows the patient, and normalization of hospital billing. We assemble this record from peer-reviewed studies, government and multilateral documents, and primary press reporting across the Americas, Europe, the Middle East, and Asia. The evidence collected here is descriptive and selected rather than causal and representative; its role is to show that the accounting categories our estimates quantify for Colombia, unpaid emergency care under exclusion, congestion relief and financing under coverage, are the same categories that hospitals, ministries, and journalists independently document wherever the emergency-only model operates. Two global reviews frame the record: the WHO's first world report on refugee and migrant health concludes that inclusive coverage permits conditions to be treated before they become acute,[6] and the UCL–Lancet Commission on Migration and Health finds no systematic evidence for the propositions, common in policy debates, that migrants import disease or that covering them burdens health services.[7]

[6] World Health Organization, *World Report on the Health of Refugees and Migrants*, 20 July 2022, https://www.who.int/publications/i/item/9789240054462.

[7] I. Abubakar et al., "The UCL–Lancet Commission on Migration and Health: the health of a world on the move," *The Lancet* 392(10164), December 2018; University College London press summary, 5 December 2018, https://www.ucl.ac.uk/news/2018/dec/harmful-myths-about-migration-and-health-used-justify-exclusion-policies.

G.1 Colombia before and after the ETPV: the documentary record

The pre-program equilibrium our estimates exploit is extensively documented in Colombian official and press sources. Under the emergency-only regime, care delivered to irregular migrants was legally mandatory and financed ex post by the central government only partially and with delay, so the shortfall accumulated as hospital debt concentrated in receiving regions. A Banco de la República study published in May 2020 put the accumulated cost of health services delivered to Venezuelans at COP 616,235 million for 2017–2019, of which COP 557,423 million fell on the public hospital network.[8] Under Article 168 of Ley 100 of 1993, as affirmed by the Constitutional Court in T-210 of 2018, irregular migrants were entitled only to initial emergency care; Médecins Sans Frontières described the resulting regime in 2019 as one in which the public system "only attends vital emergencies, births and vaccination," performing more than fifty thousand consultations of its own in La Guajira, Norte de Santander and Arauca to fill the gap.[9] The border hospitals carried the largest balances, and the arrears compounded year after year. At the Hospital Universitario Erasmo Meoz in Cúcuta, the main public hospital on the Norte de Santander border, reported migrant-care debt rose from COP 22,000 million in 2018 (15,040 patients over three years) to COP 56,294 million in 2019, COP 73,481 million by March 2021, and COP 98,000 million by May 2022, by which point migrants accounted for a third of all services the hospital provided.[10] Nationally, the Contraloría General reported in October 2021 that debt to public hospitals for emergency care provided to Venezuelan citizens over the preceding three years exceeded

[8] Banco de la República, regional economics working paper on Venezuelan migration and health services, May 2020, as reported by ColombiaCheck, "Es inchequeable la cifra de Alejandro Gaviria sobre el gasto de salud para atender a migrantes venezolanos," https://colombiacheck.com/index.php/chequeos/es-inchequeable-la-cifra-de-alejandro-gaviria-sobre-el-gasto-de-salud-para-atender.

[9] Corte Constitucional, Sentencia T-210 de 2018, https://www.corteconstitucional.gov.co/relatoria/2018/T-210-18.htm; Médicos Sin Fronteras, "Venezolanos en Colombia: una crisis desatendida," 17 December 2019, https://www.msf.org.co/actualidad/venezolanos-colombia-una-crisis-desatendida.

[10] El Tiempo, "En Cúcuta ahora nacen más bebés venezolanos que colombianos," 18 August 2018, https://www.eltiempo.com/colombia/otras-ciudades/en-cucuta-ahora-nacen-mas-bebes-venezolanos-que-colombianos-257574; El Tiempo, "Partos de venezolanas están disparados en Cúcuta," 14 August 2019, https://www.eltiempo.com/colombia/otras-ciudades/partos-de-venezolanas-estan-disparados-en-cucuta-400838; La Opinión (Cúcuta), "El 33% de las atenciones en el Meoz son para migrantes," 23 June 2022, https://www.laopinion.co/cucuta/el-33-de-las-atenciones-en-el-meoz-son-para-migrantes. The 2019 report notes that the government had reimbursed only COP 8,231 million of the accumulated balance over three fiscal years.

COP 407,000 million.[11] The March 2021 balance at Erasmo Meoz, struck weeks after the ETPV was announced, was COP 73,481 million.[12] The maternity margin, our least precisely estimated, is the one the reporting documents most closely. Births to Venezuelan mothers at the Erasmo Meoz rose from 212 in 2015 to 734 in 2017, and in June and July of 2018 they were 469 of 788 deliveries, 59 percent; by 2019 the hospital reported seven of every ten newborns per day to Venezuelan mothers, most arriving with no prenatal care and classified as high risk.[13] In Barranquilla and Atlántico the combined cost of attending deliveries of Venezuelan women was put at COP 27,000 million by May 2019.[14] The consequences appear in the vital statistics: maternal deaths among foreign women in Colombia, overwhelmingly Venezuelan, rose from 4 in 2018 to 43 in 2020, with hemorrhage the leading cause.[15] In December 2021 the national government assigned more than COP 77,000 million to twenty-two providers in La Guajira for migrant-care arrears, of which the Riohacha hospital received over COP 16,000 million.[16] The infectious-disease record of the pre-program period is likewise documented: Colombia confirmed its first measles case imported from Venezuela in March 2018, and the Instituto Nacional de Salud counted 26 confirmed cases by June 2018, among them 17 imported and 7 in recently arrived Venezuelans, an episode Colombia contained

[11] Contraloría General de la República, "Deuda de hospitales por atención a venezolanos superó los 400 mil millones de pesos," 7 October 2021, https://www.contraloria.gov.co/es/w/noticiasrptv.com-deuda-de-hospitales-por-atencio%CC%81n-a-venezolanos-supero%CC%81-los-400-mil-millones-de-pesos.

[12] Infobae (Colombia), "Por concepto de atención médica a migrantes, Gobierno nacional adeuda 73.481 millones al Hospital Universitario Erasmo Meoz," 4 March 2021, https://www.infobae.com/america/colombia/2021/03/04/por-concepto-de-atencion-medica-a-migrantes-gobierno-nacional-adeuda-73481-millones-al-hospital-universitario-erasmo-meoz/; see also La Opinión (Cúcuta), "Se agudiza la crisis en Hospital Erasmo Meoz," https://laopinion.com.co/cucuta/se-agudiza-la-crisis-en-hospital-erasmo-meoz-172972.

[13] El Tiempo, 18 August 2018 and 14 August 2019, cited above.

[14] El Heraldo (Barranquilla), "$27 mil millones, el costo por atender partos de venezolanas," 27 May 2019, https://www.elheraldo.co/barranquilla/27-mil-millones-el-costo-por-atender-partos-de-venezolanas-635980.

[15] Universidad de los Andes, Observatorio de Salud Pública, "Con la pandemia aumentó la mortalidad materna en Colombia," https://www.uniandes.edu.co/es/noticias/salud-y-medicina/con-la-pandemia-aumento-la-mortalidad-materna-en-colombia; Instituto Nacional de Salud, *Mortalidad materna, informe de evento 2022*, https://www.ins.gov.co/buscador-eventos/Informesdeevento/MORTALIDAD%20MATERNA%20INFORME%202022.pdf. The 2020 figure is not attributable to migration alone, since it coincides with the pandemic.

[16] El Tiempo, "Más de 77 mil millones a IPS guajiras por atención a migrantes," 1 December 2021, https://www.eltiempo.com/colombia/otras-ciudades/mas-de-77-mil-millones-a-ips-guajiras-por-atencion-a-migrantes-636175.

while maintaining its measles-elimination status.[17]

The post-program record tracks the mechanism in the direction our estimates imply. Affiliation of Venezuelan migrants to the health system rose from 6,441 in November 2017 to 1,291,900 at the end of 2023, of which 79.4 percent in the subsidized regime, and to roughly 1.48 million by mid-2024, with public investment in this coverage of COP 2.7 trillion between 2017 and September 2024.[18] By October 2024 the national statistical agency's migration survey (Pulso de la Migración) reported that 66.6 percent of Venezuelan migrants were affiliated to the health system, with lack of documents the main reason among the remainder (DANE, Encuesta Pulso de la Migración, October 2024). The residual, and instructive, feature of the post-program record is that uncompensated-care debt persists precisely for the population that remains outside the permit system: in September 2024 the Ministry of Health and ADRES transferred COP 327,000 million to 659 public hospitals to cover roughly half of a reported COP 675,311 million debt for emergency care delivered to *uninsured* migrants, with La Guajira, Norte de Santander, and Arauca among the largest recipients.[19] The transition was partial rather than clean. Legacy balances were settled through discrete national transfers rather than disappearing, and debt kept accruing for those still outside the system: in January 2024 the Erasmo Meoz reported COP 126,000 million accumulated over three years and a promised transfer that had not arrived, and a 2024 review found effective coverage of permit holders below half in Bogotá, Norte de Santander, Vichada and Chocó.[20] What the record does show is a change in the accounting:

[17]CNN en Español, "Ya son tres los casos de sarampión importado desde Venezuela en Colombia," 22 March 2018, https://cnnespanol.cnn.com/2018/03/22/sarampion-importado-colombia-venezuela-tercer-caso-confirmado-instituto-nacional-colombia; Pan American Health Organization, Actualización Epidemiológica Sarampión, 8 June 2018, https://www.paho.org/sites/default/files/2018-junio-8-phe-actualizacion-epidemiologica-sarampion.pdf.

[18]Ministerio de Salud y Protección Social figures, as reported by Consultorsalud, "MinSalud presenta logros en atención sanitaria a más de 1.5 millones de migrantes," https://consultorsalud.com/minsalud-logros-atencion-sanitaria-a-migrantes/.

[19]ADRES, "ADRES inició giros por cerca de $327 mil millones a 659 clínicas y hospitales que han atendido población migrante," September 2024, https://www.adres.gov.co/sala-de-prensa/noticias/adres-inicio-giros-por-327-mil-millones-a-clinicas-y-hospitales-que-han-atendido-migrantes; Ministerio de Salud, Resolución 1769 de 2024, reported in El Tiempo, "Gobierno destina $327.000 millones para cubrir déficit de hospitales y clínicas por atención médica a migrantes en Colombia," https://www.eltiempo.com/salud/gobierno-destina-327-000-millones-para-cubrir-deficit-de-hospitales-y-clinicas-por-atencion-medica-a-migrantes-3385797.

[20]El Tiempo, "Gobierno no ha girado dinero que le prometió a hospital Erasmo Meoz de Cúcuta," 18 Jan-

care that had been delivered and then billed to no one increasingly became care attached to an insurer, which is the margin our estimates operate on.

G.2 United States: the emergency-only model priced at the patient level

The United States provides the most granular pricing of the emergency-only model because federal law (EMTALA, 1986) mandates emergency treatment without mandating its financing. The American Hospital Association reports that hospitals delivered USD 41.6 billion in uncompensated care in 2019 and USD 42.7 billion in 2020, and almost USD 745 billion cumulatively since 2000, measured at cost as the sum of bad debt and financial assistance and excluding public-payer shortfalls.[21] Emergency Medicaid, the federal channel that pays for emergency care for people who meet the income test but not the immigration-status test, spent USD 3.8 billion in fiscal 2023, four tenths of one percent of Medicaid spending.[22] The sharpest within-country comparison comes from end-stage renal disease, where state Medicaid programs differ in whether undocumented patients receive scheduled outpatient dialysis or must instead present to the emergency department in life-threatening condition ("emergency-only hemodialysis"). Cervantes and coauthors, following 211 undocumented patients at three hospitals between 2007 and 2014, find that emergency-only patients faced roughly five-fold higher mortality at three years and fourteen-fold at five years than observably similar patients on standard thrice-weekly dialysis, with 9.8 times the acute-care days.[23]

uary 2024, https://www.eltiempo.com/colombia/otras-ciudades/gobierno-no-ha-girado-dinero-que-le-prometio-a-hospital-erasmo-meoz-de-cucuta-845985; UNHCR and Observatorio Nacional de Migración y Salud, "Boletín de aseguramiento y acceso a la atención en salud de población refugiada y migrante venezolana en Colombia, primer semestre 2024," https://data.unhcr.org/en/documents/details/114748.

[21] American Hospital Association, "Fact Sheet: Uncompensated Hospital Care Cost," February 2022, https://www.aha.org/fact-sheets/2020-01-06-fact-sheet-uncompensated-hospital-care-cost; American Hospital Association, "Hospitals provided $41.6 billion in uncompensated care in 2019," 21 January 2021, https://www.aha.org/news/headline/2021-01-21-aha-hospitals-provided-416-billion-uncompensated-care-2019.

[22] KFF, "Less than 1% of total Medicaid spending goes to emergency care for noncitizen immigrants," 4 October 2024, https://www.kff.org/quick-insights/less-than-1-of-total-medicaid-spending-goes-to-emergency-care-for-noncitizen-immigrants/.

[23] L. Cervantes et al., "Association of Emergency-Only vs Standard Hemodialysis With Mortality and Health Care Use Among Undocumented Immigrants With End-Stage Renal Disease," *JAMA Internal*

The restricted regime is also more expensive, not less: a Houston (Harris Health) comparison found annual costs of USD 284,655 per emergency-only patient versus USD 76,906 under scheduled dialysis, with 162 versus 10 hospital days and 26.3 versus 1.4 emergency-department visits per patient-year. [24] Colorado's 2019 administrative decision to reclassify outpatient dialysis as emergency care for Medicaid purposes, ending emergency-only dialysis there, followed the publication of this evidence. The dialysis literature is a single-disease microcosm of our aggregate result: the emergency-only regime produced worse survival at roughly 3.7 times the cost, and the difference was borne by the shared hospital system.

G.3 Peru and Brazil: two receiving countries, two access regimes

Peru and Brazil received the second- and fourth-largest Venezuelan populations under sharply different access rules. In Peru, access to the public insurer (SIS) required regular migratory status and social-registry eligibility, so coverage among Venezuelans remained minimal; Peru's national survey of the Venezuelan population found 8.5 percent insured in 2018, rising to 27 percent in 2022 as regularization advanced, with 20 percent covered by SIS; in the same 2022 survey 58 percent of those with a health problem did not seek care. A companion study of 2,144 migrants in 2018 put insurance at about 11 percent and reported cost (64.5 percent) and lack of insurance (24.3 percent) as the main reasons for not using services.[25][26] Peru's

Medicine 178(2), February 2018; summary at https://www.healio.com/news/nephrology/20180227/emergencyonly-hemodialysis-yielded-greater-mortality-rate-in-undocumented-immigrants-with-esrd.

[24] O. K. Nguyen et al., "Association of Scheduled vs Emergency-Only Dialysis With Health Outcomes and Costs in Undocumented Immigrants With End-Stage Renal Disease," *JAMA Internal Medicine* 179(2), 2019, record at https://www.rand.org/pubs/external_publications/EP68336.html; on hospital cost and resource use see also https://westjem.com/articles/the-impact-of-emergency-only-hemodialysis-on-hospital-cost-and-resource-utilization.html.

[25] Instituto Nacional de Estadística e Informática, *Encuesta dirigida a la población venezolana que reside en el país* (ENPOVE), 2018 and 2022 rounds, https://www.inei.gob.pe/media/MenuRecursivo/publicaciones_digitales/Est/Lib1886/libro.pdf; L. F. Pachacama Ramirez et al., "Características de los migrantes extranjeros afiliados al Seguro Integral de Salud del Perú durante el 2022," *Anales de la Facultad de Medicina* 85(3), 2024, which reports 838,148 foreign SIS affiliates in 2022, about 85 percent Venezuelan, and notes that the carné de extranjería was an indispensable requirement for affiliation, so documentation rather than entitlement was the binding constraint.

[26] A. Hernández-Vásquez et al., "Factores asociados a la no utilización de servicios de salud en inmigrantes venezolanos en Perú," *Revista Peruana de Medicina Experimental y Salud Pública* 36(4), 2019, https://www.scielosp.org/article/rpmesp/2019.v36n4/583-591/.

subsequent, disease-specific correction is itself evidence on the fiscal logic: in October 2024 Peru legislated SIS coverage for migrants with HIV and tuberculosis regardless of status, on the argument that treating these conditions early was cheaper and safer than absorbing them through emergency and inpatient care.[27] Brazil, by contrast, grants full access to the universal system (SUS) regardless of migratory status, so the Venezuelan inflow appears in the record as documented congestion of a funded system rather than as unpayable debt: Human Rights Watch reported that admissions of Venezuelan patients at the Hospital Geral de Roraima in Boa Vista rose 253 percent in the first half of 2018 year-on-year and emergency visits 366 percent, and that 61 percent of measles cases in Roraima state occurred among Venezuelans, prompting federal reinforcement of the state system (Operação Acolhida) rather than a parallel humanitarian channel. Births to Venezuelan women at the Boa Vista maternity hospital rose from 288 in 2016 to 556 in 2017 and 1,603 in 2018, and a hospital study found the neonatal intensive care unit, built for forty, running with fifty-three patients, forty percent of them children of Venezuelan mothers; the federal government transferred R$187 million for health infrastructure and a further R$160 million to the state health services. The authors of that study conclude that the inflow intensified deficiencies the system already had rather than creating them, which is the same reading our funding heterogeneity supports.[28][29] The Peru–Brazil contrast reproduces, across countries, the within-country contrast our design exploits: where financing followed the patient the strain was visible but fundable; where it did not, care was forgone or unfunded.

[27] UNAIDS, "Peru approves groundbreaking law to extend health coverage for migrants with HIV and TB," 21 October 2024, https://www.unaids.org/en/resources/presscentre/featurestories/2024/october/20241021_peru.

[28] L. Arruda-Barbosa, A. F. G. Sales and M. E. M. Torres, "Impacto da migração venezuelana na rotina de um hospital de referência em Roraima, Brasil," *Interface: Comunicação, Saúde, Educação* 24, 2020, http://www.scielo.br/j/icse/a/wyyZ7pD8kJbCDWLq3X3xMLC/?lang=pt.

[29] Human Rights Watch, "Venezuela's Humanitarian Emergency: Large-Scale UN Response Needed to Address Health and Food Crises," 4 April 2019, https://www.hrw.org/report/2019/04/04/venezuelas-humanitarian-emergency/large-scale-un-response-needed-to-address.

G.4 Spain: exclusion by decree, measured mortality, and reversal

Spain is the cleanest European episode because exclusion arrived as a single national decree. Royal Decree-Law 16/2012 removed undocumented migrants from primary and specialist care as of September 2012, leaving emergency care only. Exploiting this change, Juanmarti Mestres, López-Casasnovas, and Vall Castelló estimate that the restriction raised mortality among the undocumented by about 15 percent over its first three years, roughly 82 additional deaths per year.[30] The case reporting matches the estimates. The death of Alpha Pam, a 28-year-old Senegalese man who died of untreated tuberculosis in Mallorca in April 2013 after being turned away and threatened with billing at a public hospital, became the emblematic documented case, notable because tuberculosis is both fatal untreated and communicable.[31] Civil-society monitoring networks (REDER, Médicos del Mundo) systematically documented denial-of-care cases during 2012–2018, and several regional governments declined to implement the exclusion in practice.[32] Royal Decree-Law 7/2018 reversed the exclusion in July 2018, restoring universal access. Spain thus supplies, in miniature, both directions of the Colombian comparison: a coverage withdrawal followed by measured excess mortality among the excluded, and a restoration justified publicly on health and efficiency grounds.

G.5 United Kingdom: charging, deterrence, and documented harm

The UK did not remove entitlement outright but introduced upfront charging for non-urgent hospital care for patients unable to document eligibility (2015 and 2017 regulations, upfront charging from October 2017). The documented consequences run through deterrence. Maternity Action's September 2018 report found pregnant migrant women withdrawing from

[30] A. Juanmarti Mestres, G. López-Casasnovas, and J. Vall Castelló, "The deadly effects of losing health insurance," *European Economic Review* 131, 2021, https://www.sciencedirect.com/science/article/abs/pii/S0014292120302385.

[31] eldiario.es, "El forense diagnosticó tuberculosis: la muerte de Alpha Pam," https://www.eldiario.es/desalambre/forense-diagnostico-tuberculosis-alpha-pam_1_5742788.html.

[32] H. Legido-Quigley et al., "Solidarity against healthcare access restrictions on undocumented immigrants in Spain: the REDER case study," *International Journal for Equity in Health* 18, 2019, https://link.springer.com/article/10.1186/s12939-019-0971-9; on the design and effects of the exclusion see also *Health Policy* (2018), https://www.sciencedirect.com/science/article/pii/S0168851018304160.

antenatal and postnatal care for fear of unpayable debts and of data-sharing with the Home Office, and the Royal College of Obstetricians and Gynaecologists now formally acknowledges that the threat of charging deters women from time-critical maternity care.[33] The individual cases documented during the Windrush scandal show the same mechanism applied to long-settled legal residents unable to produce papers: Sylvester Marshall (reported as Albert Thompson), resident in Britain for 44 years, was asked for £54,000 upfront for prostate-cancer radiotherapy in November 2017 and received treatment only in April 2018 after the case was raised in Parliament.[34] The delay itself has been measured, in a design whose logic mirrors our own. At Barts Health in East London, which handles about a tenth of England's tuberculosis caseload, the median time from symptom onset to treatment among 2,237 adult patients rose from 69 to 89 days for non-UK-born patients after the cost-recovery program began, and the share experiencing diagnostic delay rose from 44.6 to 55.2 percent, an adjusted odds ratio of 1.37. For UK-born patients, who were never subject to charging, the same period shows no significant change.[35] That contrast between an affected and an unaffected group within the same hospitals is the closest analogue in this literature to the falsification logic of Section 5, and it points the same way: the cost of exclusion appears as delay, and delay appears later and more expensively.

What the charging regime raised is also documented. The National Audit Office reported that trusts charged overseas visitors £289 million in 2015-16 against a government ambition of £500 million a year by 2017-18, a target it judged would not be met, with half of all non-EEA charging concentrated in ten London trusts.[36]

[33]Maternity Action, *What Price Safe Motherhood? Charging for NHS Maternity Care in England and its Impact on Migrant Women*, September 2018, https://maternityaction.org.uk/2018/09/what-price-safe-motherhood/; Maternity Action, RCOG and RCM updated guidance, May 2024, https://maternityaction.org.uk/2024/05/maternity-action-rcog-and-rcm-publish-updated-guidance-on-supporting-access-to-maternity-care-for-women-affected-by-nhs-charging/.

[34]The Guardian broke the case in March 2018; BBC News, "Windrush: Albert Thompson gets date for cancer treatment," 24 April 2018, https://www.bbc.co.uk/news/uk-43883198.

[35]J. L. Potter, M. Burman, C. D. Tweed et al., "The NHS visitor and migrant cost recovery program: a threat to health?" *BMC Public Health* 20:407, 2020, https://pmc.ncbi.nlm.nih.gov/articles/PMC7169002.

[36]National Audit Office, *Recovering the Cost of NHS Treatment for Overseas Visitors*, HC 728, October 2016, https://www.nao.org.uk/reports/recovering-the-cost-of-nhs-treatment-for-overseas-patients/.

G.6 Germany: restricted entitlements that cost more, measured over twenty years

Germany's asylum-seeker benefit law (Asylbewerberleistungsgesetz) restricts newly arrived asylum seekers to acute and emergency care for a waiting period before regular entitlements begin, which makes Germany the setting with the longest quasi-experimental cost comparison of the two regimes. Analyzing 1994–2013, 4.16 million person-years under restricted access and 1.53 million under regular access, Bozorgmehr and Razum find per-capita health spending was EUR 375.80 (39 percent) *higher* under restricted access, an estimated EUR 1.56 billion in excess spending over two decades, and that the 1997 reform lengthening the waiting period widened the gap; they conclude that "the cost of excluding [asylum seekers and refugees] from health care appears ultimately higher than granting regular access to care."[37] Germany nonetheless moved in the restrictive direction in February 2024, doubling the waiting period from 18 to 36 months, the same reform margin the 1997 evidence identifies as cost-increasing. The German case is the closest documentary analogue to our fiscal accounting: a within-country comparison in which the emergency-only regime is directly measured to be the more expensive one.

G.7 Greece: exclusion by unemployment, with outbreak signatures

Greece's austerity period shows what happens when coverage is withdrawn at scale from citizens rather than migrants, which is useful because it isolates the exclusion mechanism from migration itself. Because Greek insurance was tied to employment, prolonged unemployment left large populations uninsured, about 800,000 people were estimated to be without coverage or access by 2014, while public hospital budgets were cut roughly a quarter between 2009 and 2011.[38] The infectious-disease record deteriorated in ways contemporaneous

[37] K. Bozorgmehr and O. Razum, "Effect of Restricting Access to Health Care on Health Expenditures among Asylum-Seekers and Refugees: A Quasi-Experimental Study in Germany, 1994–2013," *PLOS ONE* 10(7), 22 July 2015, https://journals.plos.org/plosone/article?id=10.1371/journal.pone.0131483.

[38] A. Kentikelenis, M. Karanikolos, A. Reeves, M. McKee, and D. Stuckler, "Greece's health crisis: from austerity to denialism," *The Lancet* 383(9918), 22 February 2014, https://pubmed.ncbi.nlm.nih.gov/

surveillance documented precisely: reported HIV infections among people who inject drugs in Athens rose from fewer than twenty a year through 2010 to 260 in 2011 and more than 500 in 2012, 1,113 cases over 2011 to 2013 in all, with prevalence in this group rising from 0.8 percent in 2010 to 16.5 percent in 2013, as prevention and outreach programs were cut,[39] By 2015 roughly 2.5 million people, about a quarter of the population, held no health coverage, an exclusion Greece reversed by statute in 2016 when Law 4368 restored free access to public services for the uninsured.[40] Locally transmitted *P. vivax* malaria returned in 2011–2012 in Evrotas (Laconia), 46 locally acquired cases among seasonal agricultural communities with poor access to care, the first sustained local transmission in about four decades.[41] Greece restored access for the uninsured by law in 2016. Attribution is confounded, exclusion arrived bundled with budget cuts and a depression, but the Greek episode documents the outbreak-and-late-presentation signature of exclusion in a European surveillance environment where the counts are reliable.

G.8 Turkey, Lebanon, and Jordan: the largest displacement, three financing regimes

The Syrian displacement produced three adjacent regimes whose documentary records diverge along financing lines. **Turkey** integrated registered Syrians under temporary protection into state-financed health care from 2013–2014, building approximately 190 dedicated Migrant Health Centres in 32 provinces staffed by about 4,000 workers including 787 Syrian physicians and 1,149 Syrian nurses retrained and licensed with WHO support, an explicit

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[39]D. Paraskevis et al., "Economic Recession and Emergence of an HIV-1 Outbreak among Drug Injectors in Athens Metropolitan Area," *PLOS ONE* 8(11), 2013, https://journals.plos.org/plosone/article?id=10.1371/journal.pone.0078941.

[40]European Observatory on Health Systems and Policies, Health Systems and Policy Monitor, Greece 2017, "Health coverage for the uninsured population improved," https://eurohealthobservatory.who.int/monitors/health-systems-monitor/updates/hspm/greece-2017/health-coverage-for-the-uninsured-population-improved.

[41]ECDC, "Epidemiological update: Malaria in Greece," October 2011, https://www.ecdc.europa.eu/en/node/9083; V. Tseroni et al., *PLOS Neglected Tropical Diseases* / PMC4652894, 2015, https://pmc.ncbi.nlm.nih.gov/articles/PMC4652894/.

supply-side expansion accompanying coverage of a population that reached 3.5 million.[42] That regime ended on 1 January 2026, when a presidential decree tied Syrians' access to social-security contributions, private insurance, or means-tested hardship status, with contribution fees at the point of service; press analysis at the change noted that formal employment became the main route back to coverage.[43] **Lebanon** financed refugee hospital care through UNHCR rather than the national system; when donor funding collapsed in 2025 (the agency was 20 percent funded in April 2025), UNHCR announced on 28 May 2025 that it would end hospitalization coverage, including deliveries and acute emergency admissions, for the roughly 700,000 registered Syrian refugees by 1 November 2025, and phased out primary-care support in July 2025.[44] The parallel-financing model thus proved reversible at the speed of a donor budget cycle, and the withdrawal shifts an unfunded emergency caseload onto Lebanese hospitals, the configuration whose costs the rest of this appendix documents. **Jordan** moved along the price margin: free public care for registered Syrians (from March 2012) was rescinded on 20 November 2014, when refugees were moved to uninsured-Jordanian rates, and in early 2018 to 80 percent of the foreigner rate. UNHCR's health-access surveys and Amnesty International documented the utilization response, with the share of refugees citing cost as the immediate barrier to care rising from 46 percent in 2017 to 55 percent in 2018, alongside reports of forgone chronic-disease care and unattended home births.[45]

[42]UNHCR Global Compact on Refugees good-practice records, https://globalcompactrefugees.org/good-practices/migrant-health-centers and https://globalcompactrefugees.org/good-practices/employment-syrian-health-workers; AIDA/ECRE country report, Türkiye: health care, https://asylumineurope.org/reports/country/turkiye/content-temporary-protection/health-care/; WHO, "Ensuring equitable access to health care for refugees and migrants in Turkey," https://www.who.int/europe/news-room/photo-stories/item/ensuring-equitable-access-to-health-care-for-refugees-and-migrants-in-turkey.

[43]The Syrian Observer, "After Losing Free Healthcare: What Awaits Syrians in Turkey?," January 2026, https://syrianobserver.com/refugees/after-losing-free-healthcare-what-awaits-syrians-in-turkey.html.

[44]L'Orient Today, "UNHCR announces end of healthcare support for Syrian refugees in Lebanon," 28 May 2025, https://today.lorientlejour.com/article/1462142/unhcr-announces-end-of-healthcare-support-for-syrian-refugees-in-lebanon.html; UNHCR Lebanon, "Access to Healthcare," https://help.unhcr.org/lebanon/en/welcome/access-to-healthcare/; UNHCR Lebanon Fact Sheet, August 2025, https://www.unhcr.org/sites/default/files/2025-09/UNHCR%20Lebanon%20FactSheet%20Aug%202025.pdf.

[45]UNHCR, *Health Access and Utilization Survey: Access to Healthcare Services Among Syrian Refugees in Jordan*, December 2018, https://reliefweb.int/report/jordan/health-access-and-utilization-survey-access-healthcare-services-among-syrian-refugees; Amnesty International, "Jordan: Syrian refugees blocked from accessing critical health services," 23 March 2016, https://www.amnesty.org/en/latest/news/2016/03/jordan-syrian-refugees-blocked-from-

G.9 Thailand: insuring the undocumented to protect hospital finances

Thailand is the standing counterexample to the claim that low- and middle-income hosts cannot afford to insure irregular migrants, and its stated rationale is the one our estimates quantify. Since 2001, and at scale since a 2013 cabinet resolution, the Ministry of Public Health has sold an annual Health Insurance Card to migrants from Myanmar, Cambodia, and Laos *including the undocumented*, explicitly to convert unpayable hospital losses into prepaid revenue and to protect public health; Thailand has been described as the only country selling universal coverage to undocumented migrants.[46] The border documentary record shows why: studies of maternal care on the Thailand–Myanmar border (Tak province, Mae Sot) identify institutional debt from unpaid obstetric bills and late arrival of emergency cases as the defining problems of serving uninsured migrants, and document how a dedicated insurance fund for costly emergency interventions increased coverage and financial protection at Thai government hospitals.[47] Coverage remains incomplete, with roughly half of Thailand's migrant population uninsured,[48] but the policy's design premise, that hospitals bear the cost of exclusion either way and prepayment dominates bad debt, is the fiscal argument of Section 7 stated by a ministry of health two decades earlier.

G.10 What this record can and cannot establish

None of the episodes assembled here identifies a causal effect of coverage on host-population mortality; that is precisely the term the documentary record cannot supply and this paper

accessing-critical-health-services/.

[46] NPR, "Only One Country Offers Universal Health Care To Undocumented Migrants," 31 March 2016, https://www.npr.org/sections/goatsandsoda/2016/03/31/469608931/only-one-country-offers-universal-health-care-to-undocumented-migrants; V. Tangcharoensathien, A. Thwin, and W. Patcharanarumol, "Implementing health insurance for migrants, Thailand," *Bulletin of the World Health Organization* 95(2), 2017.

[47] "Including undocumented migrants in universal health coverage: a maternal health case study from the Thailand–Myanmar border," *BMC Health Services Research* 21, December 2021, https://link.springer.com/article/10.1186/s12913-021-07325-z.

[48] Think Global Health, "In Thailand, Noncitizen Health Matters," https://www.thinkglobalhealth.org/article/thailand-noncitizen-health-matters.

estimates. The press and administrative sources are selected, exclusion episodes often arrive bundled with austerity or displacement shocks, and outcomes for excluded populations are systematically under-recorded, a bias our own migrant-mortality null shares. What the record does establish is the consistency and ubiquity of the mechanism's observable signatures. Wherever routine coverage is withheld from a resident population, the same items recur independently across continents, languages, and income levels: emergency departments as the default point of care; late presentation in more severe and more expensive states; hospital debt concentrated in receiving regions; deterrence of maternity and chronic care; and occasional outbreak or excess-mortality events among the excluded that surveillance systems record where they function. Where routine coverage is extended, Turkey in 2014, Thailand's card scheme, Colombia in 2021, the record shows enrollment, financing that follows patients, and residual debt confined to those still outside the system. And in the two settings where the two regimes have been costed directly against each other, Germany's twenty-year quasi-experiment and the US dialysis comparisons, the emergency-only regime is measured to be the more expensive one, by 39 percent and by a factor of roughly 3.7 respectively. The documentary record therefore does not prove our result, but it shows that every element of the accounting our estimates formalize, unfunded emergency care under exclusion and fundable routine care under coverage, has been observed and priced, piecemeal, wherever the choice Colombia faced has been made in either direction.