Income shocks, adaptation, and temperature-related mortality: Evidence from the Mexican labor market*

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This paper examines how positive income shocks affect workers' adaptation to extreme temperatures. We use daily temperature variations alongside the implementation of a wage and fiscal policy in Mexico. Our results show that higher disposable income significantly reduces temperature-related mortality in treated areas. Exploring the mechanisms, we find that income gains increase households' adaptive capacity through higher electricity consumption and the purchase of thermoregulating appliances, like electric heaters. Our findings provide causal estimates of how income influences the marginal effect of temperature on mortality and contribute to the discussion on the effectiveness of climate-related redistribution policies.

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1. Introduction

Many countries in the Global South face the dual challenges of climate change and widespread socioeconomic inequalities. The increasing frequency of extreme temperatures is expected to raise mortality rates, especially among marginalized populations in warm climates (Carleton et al., 2022). In this context, policy interventions can influence how environmental conditions affect socioeconomic outcomes (Kahn, 2005). Even policies that do not explicitly address climate change, such as improved access to public health facilities or cash transfers, can create significant positive externalities and improve adaptation to temperature changes. Understanding how these policies can mitigate the adverse effects of climate change while identifying the most effective mechanisms for different target populations remains an understudied issue (Kala et al., 2023).

This study investigates whether a wage and fiscal policy implemented in Mexican border municipalities in 2019 reduced the impact of temperature on mortality. The policy included a differential increase in the minimum wage across Mexican regions: 114% in border areas compared to 43.5% in non-border areas, along with a 50% reduction in the Value-Added Tax (VAT) for border municipalities. The combination of the minimum wage increase and VAT reduction effectively raised disposable income in treated border areas without significant price spillovers (Calderón Cerbón et al., 2022).

To estimate the policy's effect, we leverage extensive administrative data on workers' mortality and combine them with orthogonal variations in daily average temperature and the plausibly exogenous implementation of the policy. By employing a Difference-in-Temperature (DiT) design, we identify the policy's mitigating effect through differences in the temperature-mortality relationship between border and non-border regions after the policy. Additionally, we assess the robustness of our main results using matching Difference-in-Temperature (MDiT) and Difference-in-Differences-in-Temperature (DiDiT) estimates (Colmer and Doleac, 2023). These methodologies address (un)observed cross-sectional differences between treated and non-treated municipalities that may have influenced the temperature-mortality gradient, with the DiDiT further accounting for common temporal shifts in this relationship.

Our findings align with previous research indicating a U-shaped relationship between temperature and mortality (Cohen and Dechezleprêtre, 2022; Yu et al., 2019; Deschênes and Greenstone, 2011). We further demonstrate that temperature deviations disproportionately affect primary sector workers, with 74% of all worker deaths related to temperature shocks between 1998 and 2021. These results support existing literature suggesting that temperature deviations have a more pronounced impact on mortality for outdoor laborers (Dimitrova et al., 2021).

Next, we find that the policy reform reduced mortality on both cold and hot days by as much as 3%. However, this overall reduction masks heterogeneities in the mitigating effect. The decrease in temperature-related mortality is noisier and smaller for primary sector workers. We suggest this difference may arise from the high prevalence of informal labor in this sector, which limits the benefits of the minimum wage increase. Supporting this hypothesis, we provide evidence that informality reduces the benefits of the policy.

Finally, we explore potential mechanisms driving the observed reduction in temperature-related mortality. First, we use a synthetic difference-in-difference (SDID) design and household-level data on appliances and expenditures. This analysis reveals that the policy increased electricity expenditures by 12-24% and electric heater purchases by 68-81%. We also assess the impact on air conditioning units and electric fans; however, coefficients for these appliances are statistically insignificant. Second, we collect municipality-level data on residential electricity consumption. By leveraging both DiT and DiDiT designs, we test if the policy altered electricity demand during cold and hot days. Our findings indicate that the policy reform increased electricity usage on cold days (< 10 °C) by 1.9% and by 1.4% on moderate hot days (25-30 °C). Overall, our results suggest that the policy primarily facilitated heat adaptation through increased electricity consumption rather than through the purchase of new thermoregulating appliances.

Previous literature. Our paper contributes to several strands of the existing literature. First, we contribute to the literature on the differential impact of temperature across occupational sectors (Yang et al., 2012; Heo et al., 2016; Park et al., 2019), where many studies primarily focus on labor productivity (Picchio and Van Ours, 2024; Somanathan et al., 2021). Building on this prior work, we examine the impacts of temperature on mortality—a more severe outcome than those previously studied. Our findings show that primary sector workers are especially vulnerable to temperature shocks due to their greater likelihood of working outdoors. We also estimate the heterogeneous mitigating effects of a positive income shock on the temperature-mortality gradient across occupation groups.

Second, we expand the analysis of how public policies mitigate the effects of temperature on mortality and how these policy effects vary within the target population. In a related article, Cohen and Dechezleprêtre (2022) find that the expansion of mandatory public health coverage in Mexico reduced the mortality effects of days outside thermal comfort. They also find stronger reductions for low-income individuals, who were most likely uninsured before the policy. Similarly, Helo Sarmiento (2023) provides evidence that municipalities with extensive healthcare coverage in Colombia experience reduced mortality effects from cold and hot days. In Mexico, del Valle (2024) finds that investing on infrastructure reconstruction decreases the mortality impacts of natural disasters, particularly in areas with high levels of complementary public goods, such as public healthcare. In the United States, Mullins and White (2020) find that expanding community health centers in the 1960s significantly reduced mortality due to hot weather. We build on this literature by examining the effect of a policy that, while not directly related to healthcare provision, aims to increase disposable income for the treated population.

Third, we provide new causal evidence on income's role as a mediator of environmental damage. While previous studies have documented how the effects of temperature on socioeconomic outcomes vary along the income distribution (Hsiang et al., 2019; Carleton et al., 2022), they primarily rely on cross-sectional variation to infer the role of income. Our work builds on this evidence by exploring how a quasi-exogenous shock to disposable income shapes the marginal effect of temperature. In this context, a notable mention is Garg et al. (2020), who studies the impact of a large-scale cash transfer program, *Progresa*, on the temperature-homicide gradient. losely related, Adhvaryu et al. (2024) show that progresa also mitigated the long-run negative impacts of early-life rainfall shocks on children's education and labor outcomes. Their findings emphasize how resource shocks and redistributive policies interact with vulnerability, supporting our interpretation that income plays a central role in shaping adaptation to climatic. A key difference from our work is that *Progresa* focused on a rigorously selected group of poor recipients based on geographical and socioeconomic criteria, whereas the minimum wage increase and lower VAT impacted the entire population. Despite these differences, our findings align: as income increases, the adverse effects of environmental shocks on socioeconomic outcomes diminish.

Fourth, our analysis contributes to the literature on the determinants of adaptation to environmental and temperature shocks. Socioeconomic conditions shape households' adaptive capacity, including purchasing power and access to technology (Yohe and Tol, 2002; Siders, 2019). This factor is particularly important in emerging countries like Mexico, where significant inequalities in access to adaptation persist (Davis and Gertler, 2015; Pavanello et al., 2021). Evidence from other developing-country contexts highlights similar dynamics: Dasgupta (2017) shows that India's National Rural Employment Guarantee Scheme (NREGS) mitigated the long-run adverse effects of drought shocks on child nutrition by providing income support through public works, while Premand and Stoeffler (2022) demonstrate that regular cash transfers in the Sahel increased households' resilience by fostering savings, food security, and income smoothing. Our findings align with this literature by suggesting that minimum wage and fiscal policies can improve adaptation.

Finally, our work contributes to the literature on the effects of minimum wage and income shocks on mortality and health. This body of research has produced contrasting results. Earlier studies indicated that minimum wage increases raised mortality risk by enabling workers to consume unhealthy products or engage in riskier behaviors. Adda et al. (2009) documents this pattern for several cohorts in the United Kingdom. Their findings align with evidence from the United States, where higher social security payments led to increased mortality rates (Snyder and Evans, 2006). Similarly, Evans and Moore (2011) observed that adult mortality in the United States increases shortly after unexpected income shocks from federal policies, such as social security payments or wage changes for military personnel. This initial strand of research contrasts with the recent findings of Lebihan (2023), who exploits the staggered roll-out of minimum wage legislation in European countries to identify significant increases in self-reported health measures. In a similar context, Milligan and Stabile (2011) find positive effects of child tax benefits in Canada on infant and maternal health. To our knowledge, no previous study has examined whether raising income through minimum wage increases and tax cuts improves resilience against the adverse effects of climate change.

We structure the remainder of this paper as follows. Section 2 briefly describes our policy change of interest: the simultaneous increase in minimum wage and decrease in VAT. Section 3 discusses the data sources used for our empirical analysis. Section 4 estimates the relationship between temperature and mortality for each labor group. Section 5 examines the policy's impact on workers' resilience to temperature changes. Section 6 explores the underlying mechanisms. Section 7 summarizes and concludes.

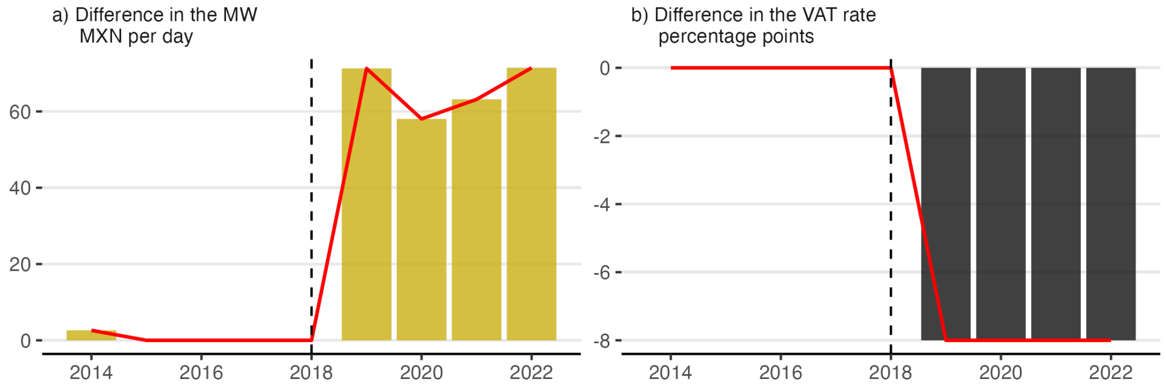
2. The 2019 tax and minimum wage reform

In January 2019, the Mexican government raised the minimum wage and reduced VAT in municipalities along the US-Mexico border (ZLFN, in Spanish: *Zona Libre de la Frontera Norte*). This initiative aimed to stimulate local economic growth by reducing cross-border consumption in the United States (Comisión Nacional de los Salarios Mínimos , CONASAMI). In border municipalities, the minimum wage doubled from 88.36 to 176.72 pesos per day. In contrast, the rest of the country saw a minimum wage increase from 88.36 to 102.68 pesos. Simultaneously, the government halved VAT in the border area, reducing it from 16% to 8%. These initiatives contributed to price stability in border municipalities, as the VAT reduction counterbalanced the effects of the minimum wage increase, leading to higher real wages in the border area (Campos-Vazquez et al., 2020; Calderón et al., 2023).

Figure 1 presents the time series of the differences in the minimum wage and VAT between border and non-border municipalities.

Following the significant increase in 2019, the National Minimum Wage Commission (CONASAMI) began implementing regular annual adjustments to align wages with inflation and improve workers' purchasing power. Since then, the national minimum wage has consistently increased at similar rates in border and non-border areas. Each year, the increase includes adjustments for inflation to address rising living costs, along with the Independent Recovery Amount (Monto Independiente de Recuperación, MIR), a fixed

Figure 1: Variation in the minimum wage and value-added tax after the policy



Notes: We obtain the average minimum wage for border and non-border areas with data from the National Minimum Wage Commission (CONSAMI). We use data from the Mexican Tax Administration Service (SAT) for the VAT rate.

sum intended to close the wage gap and improve living standards.

3. Data

3.1. Data sources

Mortality. We obtain mortality data from the Mexican National Institute of Geography and Statistics (INEGI). The dataset includes information on the municipality and date of death for all individuals who died in the country between 1998 and 2021. We also have access to sociodemographic characteristics, such as sex, age, occupation, education, and cause of death. We use population data from the Mexican Census and the National Survey of Employment and Occupation (ENOE) to convert mortality counts into rates per 10,000 people.[1]

To explore heterogeneous effects across occupations, we categorize workers into four groups: primary sector, elementary, sales, and white-collar workers. Primary sector workers work in agriculture, livestock, fishing, and hunting. Elementary workers include industrial workers, educators, and artisans. Sales workers comprise vendors, merchants, and individuals employed in personal services, such as surveillance and domestic work. White-collar workers include professionals, technicians, control personnel, and office workers. Table A.1 presents the intertemporal concordance between the INEGI labor groups and our categories.[2]

Weather. Weather data comes from the ERA5 reanalysis dataset, a leading atmospheric reanalysis product from the European Centre for Medium-Range Weather Forecasts (ECMWF). The dataset includes hourly estimates of weather variables like air temperature, precipitation, and atmospheric pressure. ERA5 combines extensive observational data from satellites, weather balloons, and ground stations with a numerical weather prediction model. This integration provides a consistent and comprehensive depiction of weather conditions across Mexico. For this study, we extracted average daily air temperature and precipitation from January 1998 to December 2021.

[1] We calculate monthly municipal mortality rates by dividing the number of deaths in a municipality on a specific period (year-month) by the population for that municipality. We use municipal population data from the 2000, 2010, and 2020 national censuses, and perform a linear interpolation of the population for the years between censuses to obtain estimates of the Mexican general population and the population per labor group in each municipality. This may introduce measurement errors in the dependent variable, a problem that reduces the efficiency of the model but not the consistency of our estimates (Cohen and Dechezleprêtre, 2022).

[2] There was a change in the classification of occupations by INEGI in 2013. We account for this change in our empirical design with fixed effects for the year of observation.

Municipality-level controls. We collect municipal data, such as categorical indicators of multidimensional poverty from the Mexican National Council for the Evaluation of Economic Policies (CONEVAL), the global relative deprivation index from the North American Space Association (GRDI), and the proportion of rural residents in each municipality from the National Census. We also extract the share of informal workers per municipality from the Mexican National Survey of Employment and Occupation (ENOE) and the number of hospitals per capita from the Information System of the Mexican Health Ministry.

Expenditures and energy appliances. We use the Mexican National Survey of Income and Expenditures (ENIGH) to evaluate policy-driven adaptation mechanisms to temperature variations. ENIGH is a biannual, representative survey of household expenditures conducted by INEGI. Relevant for our study, ENIGH gathers information on households' ownership of appliances such as electric heaters, air conditioners, and electric fans. It also collects data on electricity expenditures for the quarter preceding the interview. For our analysis, we include only households in municipalities surveyed in every wave from 2002 to 2022, which allows us to construct a balanced municipal panel with average electricity expenditures and the share of households owning each appliance.

Electricity demand. Finally, we obtain municipal electricity consumption data in kWh from the national power company, *Comisión Federal de Electricidad* (CFE). The data includes monthly electricity sales in kilowatt-hours to households, businesses, farms, and government institutions from January 2017 to December 2021.[3]

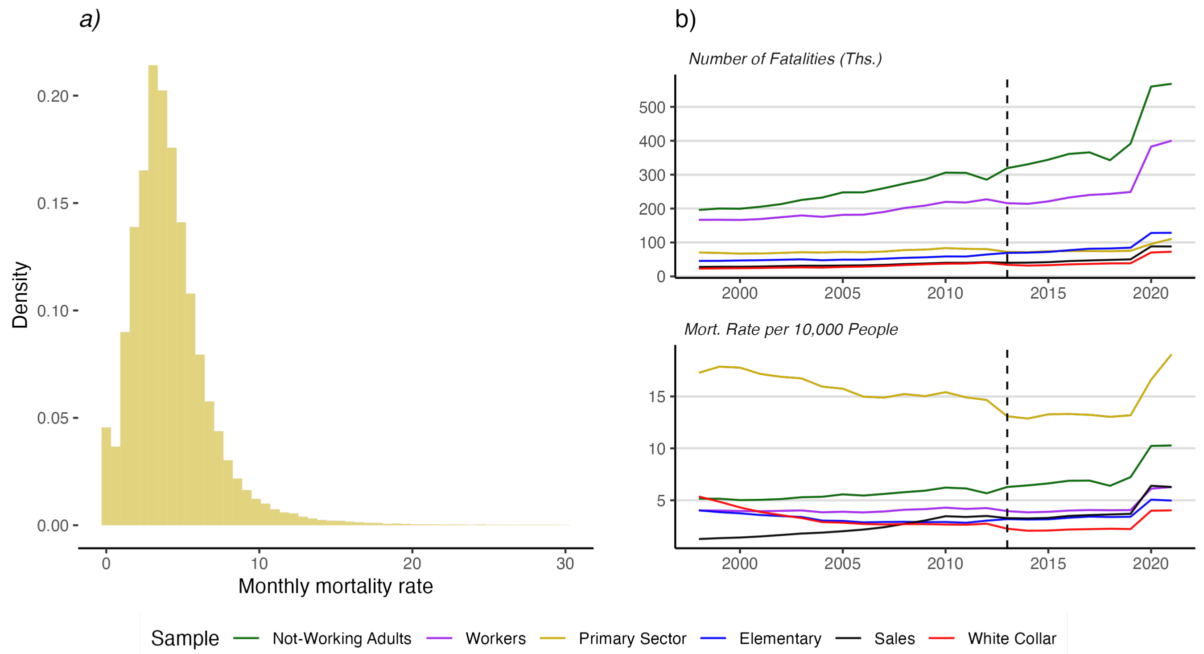
3.2. Descriptive statistics

Figure 2 depicts the distribution of monthly municipal mortality rates for workers (panel a) and the time series of annual deaths and mortality rates per 10,000 people, disaggregated by subsamples and occupations (panel b). The mortality distribution skews left, with an average of 4.19, a median of 4.01, and a standard deviation of 0.62. The time

[3]We acquire the data set through an official request via the national transparency portal, similar to *freedom of information* (FOI) requests in the USA. In Mexico, FOI requests require that the requested information exists in government records. CFE provided raw data covering monthly values from 2017 to 2021, excluding November and December of 2022 and 2023. We submitted several additional requests for the missing months but received no response.

series shows an upward trend in the absolute number of deaths from 1998 to 2021, reflecting population growth and an increasing proportion of older adults. From 1998 to 2019 (the period before COVID), deaths among nonworking individuals rose by 99%, while deaths among working individuals increased by 49%. From 1998 to 2013, most worker deaths occurred in the primary sector. However, by 2016, elementary workers recorded the highest number of fatalities. Significant disparities exist in mortality rates across occupations, particularly for primary sector workers, who exhibit much higher mortality rates than other labor groups. On average, the monthly mortality rate for primary sector workers ranges between 13 and 17 deaths per 10,000 people, nearly four times the rate observed among elementary, sales, and white-collar workers.

Figure 2: Workers' mortality in Mexico



Notes: This figure presents the density distribution of monthly worker mortality rates in the left panel (a). We remove outliers with mortality rates above 30 deaths per 10,000 people (¡1% of observations). In the right panel (b), we also show the yearly time series of total mortality and mortality rates per 10,000 people for workers and people out of the labor force. For workers, we further subdivide the sample into primary sector, elementary, sales, and white-collar workers. The data comes from the administrative records of the Mexican National Institute of Geography and Statistics (INEGI).

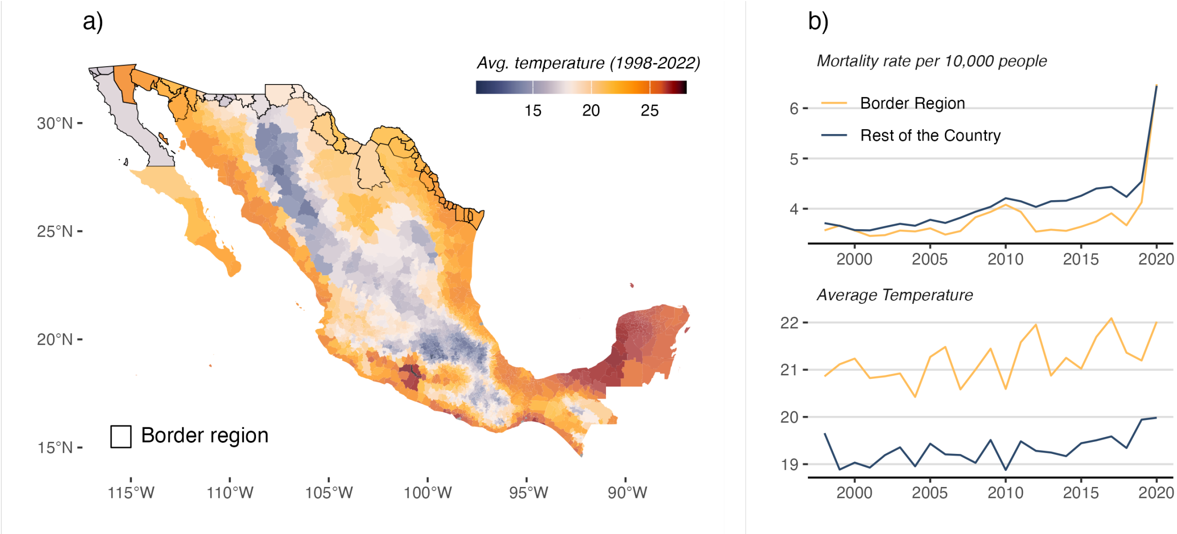
Elevated mortality rates among primary sector workers, compared to other occupational categories, primarily arise from demographic and socioeconomic differences. Appendix Table A.2 outlines the distribution of mortality across labor groups and key sociodemographic characteristics.[4] Primary sector workers tend to be older, less educated, poorer, predominantly male, and have limited access to social security. They also reside

[4]Appendix Table A.3 shows the differences in municipal characteristics.

in more marginalized, rural, and indigenous municipalities. For instance, 78% of primary sector workers fall into the lowest quartile of the income distribution, compared to only 0.98% of white-collar workers.[5]

Figure 3 presents a map of average temperatures across Mexico, distinguishing between border and non-border municipalities (a). It further includes the time series of annual mortality rates and temperatures for both groups (b).

Figure 3: Temperature and mortality in border and non-border municipalities



Notes: This figure presents a map of average temperatures in Mexico and the location of border and non-border municipalities in the left panel. In the right panel, we show the annual time series of mortality rates and temperatures for both groups. The mortality data comes from the administrative records of the Mexican National Institute of Geography and Statistics (INEGI). The weather data comes from ECMRWF.

Mexico's warmest areas are in the northwest, the Yucatan Peninsula, and along the coast. The coldest municipalities are on the Mexican Plateau, an elevated region in the center of the country. On average, border municipalities are two degrees warmer than non-border regions. From 2001 to 2007, mortality rates in both regions increased in parallel. Starting in 2007, mortality rates in border municipalities increased sharply, partly due to violence related to the war on drugs. Between 2010 and 2012, the mortality rate in border municipalities declined significantly, resulting in much lower levels than in non-border areas. After 2013, mortality rates in both regions followed similar trends until the

[5]While mortality records lack data on individual income, we estimate income using the methodology proposed by Cohen and Dechezleprêtre (2022). Specifically, we rely on data from the Mexican National Survey of Employment and Occupation, which provides average income by occupation, and apply a log-linear OLS model to predict income based on age, age squared, sex, metropolitan area, climate, education, occupation, marital status, and urbanization. The worker characteristics from the mortality records are then used to estimate predicted income. See Appendix C1 in Cohen and Dechezleprêtre (2022) for further methodological details.

onset of COVID-19, which caused a sharp rise in mortality.

4. The effect of temperature on workers' mortality

We estimate the effect of temperature variations on monthly mortality rates using *Poisson Pseudo-Maximum Likelihood* (PPML) regressions with high-dimensional fixed effects (Wooldridge, 1999). Using PPML is important as our data violates the *ordinary least squares* (OLS) assumptions of homoskedasticity and normally distributed errors (Chen and Roth, 2023). PPML accounts for the non-negative nature of count data and ensures fitted values are bounded by zero. In our context, these features effectively handle excess zeros without ad hoc transformations, reduce bias from extreme high counts using the log link, and preserve interpretability through multiplicative effects (incidence rate ratios). Moreover, under mild regularity conditions, PPML often surpasses OLS and OLS-on-logs in terms of bias and mean squared error (Aguilar-Gomez et al., 2025).

To account for non-linearities in the temperature-mortality relationship, we categorize temperatures into six exposure intervals. These intervals range from -15 °C to 40 °C. We use 20 °C-25 °C as the reference category, following previous literature such as Cohen and Dechezleprêtre (2022) who use a range between 24 °C and 28 °C in Mexico, and Helo Sarmiento (2023) who uses 23 °C-25 °C for Colombia. These thresholds align with estimates from the World Health Organization, which states that thermal comfort for healthy adults lies between 18 °C and 24 °C (64 °F to 75 °F) (Ormandy and Ezratty, 2012).

Equation 1 presents our baseline specification to identify the effect of temperature on mortality rates:

$$M_{ct} = \exp\left\{ \sum_{b=0}^{6} \lambda_b \times D_{bct} + \gamma X_{ct} + \delta_{cm} + \delta_{cy} \right\} + \varepsilon_{ct} \tag{1}$$

In this regression, M_{ct} represents the mortality rate per 10,000 persons for municipality c in period t (month-year). The variable D_{bct} indicates the number of days in period t with an average daily temperature within interval b. X_{ct} is a matrix of control variables that, in the preferred specification, includes a second-order polynomial of precipitation and an indicator variable for the COVID pandemic.[6] We account for seasonality in weather and mortality with fixed effects for the month of observation within each municipality (δ_{cm}). In addition, fixed effects for the municipality and year of observation (δ_{cy}) account for all shocks that occur in the same year within each municipality. ε_{ct} is an idiosyncratic error term, which we assume is uncorrelated with D_{bct}, conditional on our full set of controls. Our identification assumption is that, conditional on the set of controls and fixed effects, temperature is as good as random such that our coefficients of interest (λ_b) are unbiased (Hsiang, 2016). We cluster standard errors at the municipality level to account for the correlation of unobservables within municipalities and autocorrelation over time. We also weight our estimates by population to obtain representative estimates at the country level and to address heteroskedasticity.

Table 1 presents the coefficients for the general population and for workers. Our findings align with previous research indicating a U-shaped relationship between temperature and mortality (Deschênes and Greenstone, 2011; Yu et al., 2019). For the general population, we find that an additional day at or above 30 °C increases the monthly mortality rate by 0.34%, relative to an additional day between 20 °C and 25 °C. For days below 10 °C, our estimate suggests an increase of 0.90%. For workers, the coefficients are slightly smaller in magnitude, ranging from 0.22% to 0.76%. Table 1 also shows our estimates from running the same specification separately across all labor groups. For primary sector workers, we observe a U-shaped relationship between temperature and mortality. An additional day in the cold temperature intervals of (-15:10°C], (10:15°C], and (15:20°C] increases the monthly mortality rate by 1.30%, 0.64%, and 0.23%, respectively. For the warm temperature intervals of (25:30°C] and (30:40°C], mortality increases by 0.07% and 0.39%. Notably, cold temperatures affect primary sector workers more than warm temperatures. Since primary sector workers are typically older and predominantly work outdoors (Table A.2), they are more vulnerable to the adverse effects of cold. This aligns with studies that highlight the significant impacts of cold weather on respiratory and cardiovascular mortality (Zeka et al., 2014). Elementary workers also experience increased mortality rates during cold temperatures and borderline non-significant increases in the warmest interval. Sales and white-collar workers experience higher mortality rates only in the coldest temperature range. These estimates align with existing literature, suggesting

[6] The COVID indicator takes value equal to 1 after March 2020.

that temperature deviations impact mortality more significantly for outdoor laborers (e.g., Dimitrova et al., 2021). In summary, our findings indicate that the negative effects of temperature fluctuations on worker mortality in Mexico are primarily driven by primary sector and elementary workers.

Table 1: The effect of temperature on monthly mortality rates across workers

	Deaths per 10,000 people					
	General	*Workers*				
		All	*Primary Sector*	*Elementary*	*Sales*	*White Collar*
(-15:10]	0.0090***	0.0076***	0.0129***	0.0072***	0.0044**	0.0032*
	(0.0011)	(0.0011)	(0.0008)	(0.0014)	(0.0019)	(0.0017)
(10:15]	0.0034***	0.0029***	0.0064***	0.0022***	0.0005	0.0006
	(0.0006)	(0.0007)	(0.0005)	(0.0008)	(0.0010)	(0.0012)
(15:20]	0.0015***	0.0013***	0.0023***	0.0012**	-0.0005	0.0011
	(0.0004)	(0.0005)	(0.0004)	(0.0005)	(0.0009)	(0.0008)
(25:30]	0.0005	0.0002	0.0007*	0.0005	-0.0007	-0.0008
	(0.0004)	(0.0004)	(0.0004)	(0.0005)	(0.0007)	(0.0007)
(30:40]	0.0034***	0.0022**	0.0039***	0.0017	0.0019	-0.0009
	(0.0008)	(0.0009)	(0.0010)	(0.0011)	(0.0015)	(0.0012)
Fitted Stat						
Observations	702495	693816	683894	502677	412520	354306
Mean Outcome	3.943	5.628	15.180	3.418	2.762	2.838
Fixed Effects						
Municipality-Month	✓	✓	✓	✓	✓	✓
Municipality-Year	✓	✓	✓	✓	✓	✓
Controls						
Precipitation	✓	✓	✓	✓	✓	✓
COVID	✓	✓	✓	✓	✓	✓

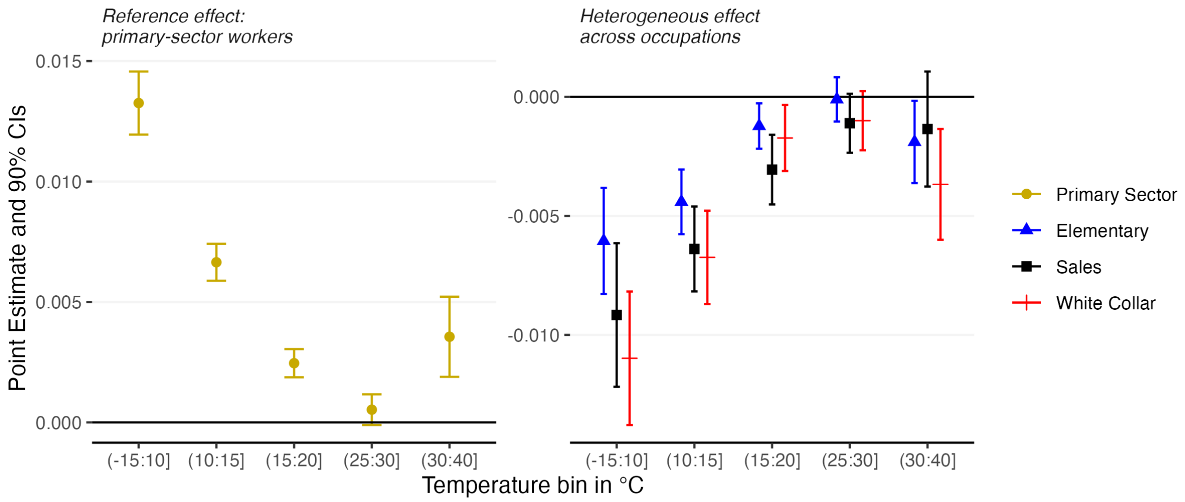
Notes: The dependent variable is mortality rates per 10,000 people. The coefficients are obtained from a Poisson Maximum Likelihood Estimator regression. The coefficients refer to variables with the number of days per month within daily temperature intervals. The reference temperature category is days within (20:25] °C. We present results separately for all mortality cases, and the sub-sample of people dying while part of the labor force. Among workers we distinguish between primary sector, elementary, sales, and white-collar workers. Controls include a second-order polynomial of precipitation, and an indicator variable for the COVID pandemic (equal to 1 after March 2020). Standard errors are clustered at the municipality level. Significance levels are indicated as: $^{***} < 0.01, ^{**} < 0.05, ^{*} < 0.1$.

Next, we use Equation 2 to test whether the heterogeneous coefficients across occupations differ significantly from the reference category (primary sector workers). In this specification, the superscript i denotes the occupation group. We augment the previous design with an interaction term that combines the matrix of temperature intervals with an indicator variable equal to one for all occupations i except the reference occupation j. The coefficient λ_b^{ij} represents the difference in the effect of an additional day in temperature bin b for occupation i compared to the reference occupation j.

$$M^i_{ct} = \exp\left\{ \sum_{b=0}^{6} \lambda^j_b \times D_{bct} + \sum_{b=0}^{6} \lambda^{ij}_b \times D_{bct} \times \mathbb{1}(i \neq j) + \right.$$
$$\left. + \gamma X_{ct} + \delta^i_{cm} + \delta^i_{cy} \right\} + \varepsilon^i_{ct} \tag{2}$$

Figure 4 presents the results of this exercise. The left panel illustrates the temperature-mortality relationship for the reference category: primary sector workers. The right panel shows the difference in the effect for the other labor groups compared to primary sector workers. A negative coefficient indicates that an additional day within that specific temperature range has a smaller impact on mortality for workers in that occupation than for their primary sector counterparts.

Figure 4: Differences in the effects of temperature on mortality between occupations



Notes: The left panel shows the relationship between temperature and mortality for primary sector workers (reference category). The right panel shows the difference for the other three occupations relative to the primary sector. We interpret the left panel as the percentage increase in the mortality rate of primary sector workers due to an additional day per month outside the thermal comfort point. We interpret the right panel as the percentage point difference in the effects of an additional day per month outside the thermal comfort point on the mortality rate of the other occupations relative to primary sector workers. The econometric model estimates the effect of temperature on mortality with Poisson Pseudo-Maximum Likelihood Estimator (PPML) while controlling for the precipitation, the COVID-19 pandemic, and fixed effects for the municipality-month and the municipality-year of observation. Standard errors clustered at the municipality level.

Our point estimates suggest that temperature changes affect all labor groups less than primary sector workers. For cold temperatures, all coefficients are negative and statistically significant, with the largest difference observed for white-collar workers. For warm temperatures, we observe differences only in the warmest interval for elementary and white-collar workers. To quantify worker vulnerability, Appendix Table A.4 reports the estimated average number of excess deaths per month nationwide for each temperature interval based on our coefficients. The highest number of deaths occurs on mildly cold days. Specifically, our estimates indicate 108, 275, 248, 18, and 10 additional monthly deaths from the coldest to the warmest temperature intervals, respectively. Most temperature-related excess mortality occurs among primary sector workers. On average, 71% of all temperature-related deaths between 1998 and 2021 occurred among primary sector workers, 26% among elementary workers, 2% among sales and personal service workers, and just 1% among white-collar workers.

Robustness checks. In our setting, a critical empirical challenge is determining whether the differential sensitivity in temperature shocks among labor groups arises from differences in exposure (indoor vs. outdoor activities) or sociodemographic risk factors (Figure A.2). Do agricultural workers face greater vulnerability to temperature changes due to their exposure to extreme conditions, or is this vulnerability related to their generally lower incomes and later retirement compared to other occupations? To address this, Appendix A.2.1 accounts for sociodemographic differences between occupations using coarsened exact matching (CEM) (Bressler et al., 2025). We leverage a complete set of death certificates to match elementary, sales, and white collar workers with primary sector workers based on similarities in age, sex, access to social security, municipal marginalization index, the share of rural households, and state of residence (Figure A.3). After matching, we apply the CEM algorithm weights to aggregate the data into the weighted number of deaths per municipality across occupations for workers most similar to primary sector workers. Estimating our specification on the weighted sample reinforces our main conclusions: primary sector workers exhibit lower resilience to temperature deviations compared to workers in other occupations, even after accounting for observed economic and demographic differences between labor groups.

5. The mitigating effect of the 2019 reform

This section explores the mitigating impact of the 2019 tax and minimum wage reform, which effectively increased the disposable income of people living in border municipalities. Conceptually, our empirical approach follows a Difference-in-Differences (DiD) design to estimate the effect of the minimum wage increase and VAT cut on the temperature-mortality relationship among workers by leveraging changes before and after the introduction of the policy in border municipalities.

Empirical framework. Equation 3 presents our baseline specification, known as the Difference-in-Temperature (DiT) design (Colmer and Doleac, 2023). In this specification, λ_b^p identifies the effect of the policy change on temperature-related mortality by interacting the exogenous interannual variation in the number of days within each temperature interval with an indicator variable equal to one for municipalities classified as part of the border region after 2019. Negative coefficients for at least one temperature bin indicate that the policy change reduced temperature-related mortality.

$$M_{ct} = \exp\left\{ \sum_{b=0}^{6} \lambda_b \times D_{bct} + \sum_{b=0}^{6} \lambda_b^p \Big[D_{bct} \times \mathbb{1}(Border)_{ct} \Big] + \right. \\ \left. + \gamma X_{ct} + \delta^{\tau}_{cy} + \delta^{\tau}_{cm} + \delta^{\tau}_{my} \right\} + \varepsilon_{ct} \tag{3}$$

Building on previous papers, such as Barreca et al. (2016) and Cohen and Dechezleprêtre (2022), we argue that the exogenous interannual variation in temperatures allows us to identify λ_b^p, as any omitted variable bias would need to correlate with both temperatures and policy implementation after accounting for the fixed effects. Critically, λ_b^p does not identify the effect of the policy on total mortality; instead, it isolates the effect on temperature-related mortality. For instance, one could argue that municipalities with more (or less) extreme temperatures may also be where the government implements policies to increase income. However, our fixed effects will capture these cross-sectional differences.

We further saturate our fixed effects by interacting them with the treatment indicator (e.g., $\delta^{\tau}_{cy} = \delta_{cy} \times \mathbb{1}(Border)_{ct}$). These fixed effects absorb all direct differences due to treatment assignment, and control for potential changes in the direct effect of the policy on our outcomes of interest—e.g. changes in the mortality's seasonality in treated units. As in Colmer and Doleac (2023), our design employs a specialized Difference-in-Differences approach, requiring an additional exogenous continuous variable to reveal heterogeneous effects. This allows us to consistently estimate a treatment $\times$ exposure interaction as the continuous exposure varies within those fixed-effect units, yielding the necessary within-unit variation. This method is suitable in contexts where directly identifying a treatment effect is challenging due to potential confounders, yet the interaction effect can be clearly identified and interpreted.

A threat to our baseline identification is the presence of unobservable cross-sectional differences in temperature-related mortality between treated and control units and across time. This is particularly problematic if these unobservable factors correlate with the policy. To address this concern, we control for these potential confounders using a Difference-in-Differences-in-Temperature (DiDiT) research design (Mullins and White, 2020; Colmer and Doleac, 2023). In this approach, we augment Equation 3 by interacting temperature bins with fixed effects for the year of observation (δ_y) and a constant indicator variable for treated municipalities ($Treated_c$).

$$\begin{aligned} M_{ct} = \exp\Bigg\{ & \sum_{b=0}^{6} \lambda_b^p \Big[D_{bct} \times \mathbb{1}(Border)_{ct} \Big] + \\ & + \sum_{b=0}^{6} \lambda_b^c \Big[D_{bct} \times Treated_c \Big] + \sum_{b=0}^{6} \lambda_b^y \Big[D_{bct} \times \delta_y \Big] + \\ & + \gamma X_{ct} + \delta_{cy}^{\tau} + \delta_{cm}^{\tau} + \delta_{my}^{\tau} \Bigg\} + \varepsilon_{ct} \end{aligned} \tag{4}$$

Conceptually, Equation 4 mimics a triple-difference research design, keeping any unobserved differences in the temperature-mortality relationship fixed. The interaction between temperature and annual dummies captures time-related differences that affect the temperature-mortality relationship across all municipalities (e.g., other national policies like *Seguro Popular*). Similarly, the interaction between temperature and the treated indicator allows us to control for cross-sectional differences between border and non-border regions that influence the temperature-mortality relationship over time (e.g., climatic conditions).

Interpretation. We interpret the coefficients λ_b^p as the impact of the policy on the first partial derivative of the logarithm of the mortality rate with respect to temperature bins—i.e., temperature-related mortality. Formally, for each temperature bin D_b we can take the logarithm of Equation 3 and differentiate it with respect to temperature interval $D_b = D_{b^*}$, yielding:

$$\frac{\partial \ln M_{ct}}{\partial D_{b^*ct}} = \lambda_b^* + \lambda_{b^*}^p \mathbb{1}(Border)_{ct}$$

To interpret the policy's effect, we can consider two scenarios: a) before treatment, $\mathbb{1}(Border)_{ct} = 0$, and b) after treatment, $\mathbb{1}(Border)_{ct} = 1$. This can be summarized as follows:

$$\left.\frac{\partial \ln M_{ct}}{\partial D_{b^*ct}}\right|_{\mathbb{1}(\text{Border})_{ct}=d} = \begin{cases} a) \quad \lambda_{b^*} + \lambda_{b^*}^p, & \text{d} = 1, \\ b) \quad \lambda_{b^*}, & \text{d} = 0. \end{cases}$$

By subtracting a) from b), we can determine the policy's effect on the derivative of $\ln(M_{ct})$ with respect to temperature bin b. For instance, if $\lambda_b^p = -0.02$, this implies a decrease of approximately 2% in $\frac{\partial ln M_{ct}}{\partial D_{bct}}$. This reduction specifically relates to temperature-related mortality, not total mortality. For instance, if an additional day at temperatures of b^* raises the mortality rate by 10%, a value of 0.2 for λ_b^p would suggest a reduction of 2% on temperature-related mortality and 0.2% in total mortality (10% $\times$ 2%).

Results. Table 2 presents our estimates. The DiT results (columns 1 and 3) indicate that the higher minimum wage and reduced VAT in the border region lowered temperature-related mortality. We observe negative coefficients for all temperature intervals, with statistically significant effects in the (10-15] °C, (25-30] °C, and (30-40] °C bins. For overall mortality, significant coefficients indicate a reduction of 0.9% for days between 10 °C and 15 °C and a decrease of 2.31% for days warmer than 30 °C. This finding suggests that higher disposable incomes mitigate the impact of warm temperatures more effectively. When focusing on workers, the effect is slightly greater for warm temperatures and not statistically significant at conventional levels for the two coldest intervals. The slightly larger coefficients for workers align with the minimum wage policy targeting this group. For example, Campos-Vazquez and Esquivel (2023) documents that the policy did not improve the economic conditions of families without labor income.

Our DiDiT estimates (columns 2 and 4) align with our baseline estimates. Furthermore, they suggest that after accounting for unobserved cross-sectional differences between treated and control units, as well as changes in the temperature-mortality relationship over time, the 2019 reform also reduced cold-related mortality among workers.

Table 2: The 2019 tax and minimum wage reform on temperature-related mortality

	Deaths per 10,000 people			
	General		*Workers*	
	DiT	DiDiT	DiT	DiDiT
$\mathbb{1}$(Border) ×				
(-15:10]	-0.0147	-0.0191*	-0.0173	-0.0251**
	(0.0119)	(0.0106)	(0.0115)	(0.0116)
(10:15]	-0.0092**	-0.0107***	-0.0082	-0.0091**
	(0.0044)	(0.0032)	(0.0051)	(0.0046)
(15:20]	-0.0018	-0.0043	-0.0017	-0.0054
	(0.0056)	(0.0051)	(0.0048)	(0.0045)
(25:30]	-0.0156***	-0.0138***	-0.0204***	-0.0184***
	(0.0020)	(0.0023)	(0.0046)	(0.0044)
(30:40]	-0.0234***	-0.0186*	-0.0279***	-0.0226***
	(0.0079)	(0.0085)	(0.0078)	(0.0077)
Fitted Stat				
Observations	702444	702444	693672	693672
Mean Outcome (pre-policy)	3.740	3.740	5.422	5.422
Fixed Effects				
Treatment-Municipality-Month	✓	✓	✓	✓
Treatment-Municipality-Year	✓	✓	✓	✓
Treatment-Month-Year	✓	✓	✓	✓
Controls				
Precipitation	✓	✓	✓	✓
COVID	✓	✓	✓	✓
Bins × Year		✓		✓
Bins × Treated		✓		✓

Notes: The dependent variable is mortality rates per 10,000 people. The coefficients are obtained from a Poisson Maximum Likelihood Estimator regression. The coefficients refer to the effect of the difference in minimum wages between border and non-border municipalities since 2019 on the consequences of an additional day with average daily temperatures within the specified temperature bin relative to the reference temperature category (20-25] °C. We present results for all mortality cases and the subsample of people who died while part of the labor force. Controls include a second-order polynomial of precipitation and an indicator variable for the COVID pandemic (equal to 1 after March 2020). Standard errors are clustered at the municipality level. Significance levels are indicated as: $^{***} < 0.01, ^{**} < 0.05, ^{*} < 0.1$.

To estimate the potential number of avoided annual temperature-related deaths from the nationwide implementation of this policy, we can perform a simple back-of-the-envelope calculation. From Table 1 we find that the coefficient for days with daily average temperatures above 30 °C is 0.0022, or 0.22%. This implies that 185 workers die annually in Mexico due to temperatures exceeding 30 °C.[7] Multiplying this by λ_b^p for b = (30:40],

[7]For each temperature bin we compute the number of related deaths as follows: $\hat{m}_b = \frac{\partial log(M)}{\partial D_b} \times$

which equals -0.0275 or -2.7%, implies that a nationwide policy could save 5.08 worker lives annually on days with average temperatures above 30 °C.

Table 3 displays results by labor group. Using the DiT specification, we observe statistically significant reductions in temperature-related mortality across all occupations during warm temperature intervals. In colder intervals, significant effects are limited to elementary workers within the 10 °C and 15 °C range. Interestingly, although the coefficients are not statistically different from each other, they appear larger and more precisely estimated for sales and white-collar workers. That is, we estimate a reduction in temperature-related mortality of 2.2% for primary sector workers associated with the warmest temperature range. For white-collar workers, this coefficient nearly doubles to 3.6%. The relatively lower coefficient for primary sector workers may relate to a higher share of informality, which limits the benefits of the minimum wage legislation. Data from the National Survey of Employment and Occupation suggest that while 20% of white-collar workers operate in the informal sector, this share rises to 91.2% for primary sector workers. This high level of informality can diminish the direct benefits of a minimum wage increase, which may explain the smaller and less precise estimates. Supporting this perspective, Pérez (2020) finds much larger responses to an unexpected increase in the minimum wage level for workers in the formal sector compared to those in the informal sector.[8] DiDiT coefficients for each labor group reinforce our main results. Elementary, sales, and white-collar workers clearly benefited from the policy, especially regarding heat-related mortality. In contrast, primary-sector workers show significant reductions only in extreme cold mortality.

To explain the smaller effect on primary sector workers, we hypothesize that as the share of informal workers increases, the positive effect of the minimum wage policy on temperature-related mortality decreases. To test this, we extract data on the share of informal workers per municipality from the ENOE and interact this share with the num-

$\left\{[(M/10000) \times pop] \times [D_b \times N \times 12]\right\}$. Here, M represents the mortality rate per 10,000 inhabitants, pop indicates the average population per municipality, D_b reflects the average number of monthly days in the interval b, and N shows the number of municipalities in the country. Finally, 12 aggregates everything to the annual level. For instance, for $b = (30 : 40]$ we substitute: $D_b = 0.4$; M = 5.62; pop = 17,440; N = 2,457.

[8] Although minimum wage increases can spill over to informal workers, because of increased bargaining power after the minimum wage increase (Khamis, 2013), we believe that such spillover effects are significantly smaller than the direct effects on formal workers.

Table 3: The policy's mitigating effect by occupation

	Deaths per 10,000 people							
	Primary Sector		*Elementary*		*Sales*		*White Collar*	
	DiT	DiDiT	DiT	DiDiT	DiT	DiDiT	DiT	DiDiT
$\mathbb{1}$(Border) ×								
(-15:10]	-0.0182	-0.0304**	-0.0166	-0.0226*	-0.0199	-0.0245*	-0.0064	-0.0149
	(0.0124)	(0.0127)	(0.0118)	(0.0127)	(0.0127)	(0.0127)	(0.0140)	(0.0134)
(10:15]	-0.0020	-0.0076	-0.0110**	-0.0100**	-0.0079	-0.0093	0.0007	-0.0025
	(0.0100)	(0.0105)	(0.0047)	(0.0049)	(0.0086)	(0.0080)	(0.0067)	(0.0054)
(15:20]	0.0137	0.0097	-0.0027	-0.0054	0.0005	-0.0061	-0.0019	-0.0057
	(0.0088)	(0.0090)	(0.0045)	(0.0048)	(0.0066)	(0.0065)	(0.0085)	(0.0076)
(25:30]	-0.0174*	-0.0162*	-0.0181***	-0.0143***	-0.0210***	-0.0213***	-0.0249***	-0.0232***
	(0.0103)	(0.095)	(0.0054)	(0.0048)	(0.0056)	(0.0056)	(0.0068)	(0.0078)
(30:40]	-0.0220*	-0.0130	-0.0213***	-0.0149**	-0.0303***	-0.0275**	-0.0363***	-0.0344***
	(0.0121)	(0.0112)	(0.0072)	(0.0069)	(0.0114)	(0.0112)	(0.0091)	(0.0111)
Fitted Stat								
Observations	683574	683574	502434	502434	412274	412274	353981	353981
Mean Outcome (pre-policy)	14.998	14.998	3.205	3.205	2.409	2.409	2.709	2.709
Fixed Effects								
Treatment-Municipality-Month	✓	✓	✓	✓	✓	✓	✓	✓
Treatment-Municipality-Year	✓	✓	✓	✓	✓	✓	✓	✓
Treatment-Month-Year	✓	✓	✓	✓	✓	✓	✓	✓
Controls								
Precipitation	✓	✓	✓	✓	✓	✓	✓	✓
COVID	✓	✓	✓	✓	✓	✓	✓	✓
Bins × Year		✓		✓		✓		✓
Bins × Treated		✓		✓		✓		✓

Notes: The dependent variable is mortality rates per 10,000 people. The coefficients are obtained from a Poisson Maximum Likelihood Estimator regression. The coefficients refer to the effect of the difference in minimum wages between border and non-border municipalities since 2019 on the consequences of an additional day with average daily temperatures within the specified temperature bin relative to the reference temperature category (20-25] °C. We estimate the effect independently for each labor group. Controls include a second-order polynomial of precipitation and an indicator variable for the COVID pandemic (equal to 1 after March 2020). Standard errors clustered at the municipality level. Significance levels are indicated as: $^{***} < 0.01, ^{**} < 0.05, ^{*} < 0.1$.

ber of days in each temperature range, along with the treatment indicator for border municipalities.[9] To reduce the number of coefficients, we condense the temperature intervals into a single vector of days outside the reference category (20-25] °C. Specifically, we include the following additional term to Equation 3:

$$\lambda^{inf}\Big[D_{ct} \times Inf_{ct} \times \mathbb{1}(Border)_{ct}\Big]$$

[9]Note that because we only have data on informality from 2005 onward and for a sub-sample of municipalities, our sample in Table A.5 changes. We decrease the number of missing observations for small municipalities by imputing their values with the 75th percentile of informality in their state, effectively assuming that small and remote municipalities are in the upper part of the informality distribution in a state. However, the results are robust to excluding these municipalities or imputing the value with the median.

where D_{ct} represents the number of days outside thermal comfort, while Inf_{ct} indicates the pre-treatment share of informal workers. We interpret λ^{inf} as the effect of informality on the benefits of higher minimum wages related to temperature-related mortality. A positive λ^{inf} suggests that informality reduces the effectiveness of higher minimum wages in lowering temperature-related mortality. Table 4 presents our estimates. The coefficient for informality is positive and statistically significant at the 10% level, indicating that for each percentage point increase in the share of informal workers, the positive effect of the policy decreases by 0.11%.[10]

Table 4: The mitigating effect of informality

	Estimate	Std. Error	**p-value**	**Observations**	**N Treated / N Control**
Treatment	-0.0517**	(0.0235)	0.0277	462068	43 / 2414
Informality (λ^{inf})	0.0011*	(0.0006)	0.0930	462068	43 / 2414

Notes: The dependent variable is mortality rates per 10,000 people. The coefficients are obtained from a Poisson Maximum Likelihood Estimator regression. The coefficients refer to the mitigating effect of the share of informal workers on the consequences of an additional day with average daily air temperatures outside thermal comfort after the policy implementation. We present results for the subsample of people who died while part of the labor force. The specification includes treatment-municipality-month, treatment-municipality-year, and treatment-month-year of observation. Controls include a second-order polynomial of precipitation and an indicator variable for the COVID pandemic (equal to 1 after March 2020). Standard errors clustered at the municipality level. Significance levels are indicated as: $^{***} < 0.01, ^{**} < 0.05, ^{*} < 0.1$.

Threats to identification and robustness checks. A potential concern for identification is that income increases in border municipalities may trigger migration from other regions. If these potential migrants significantly differ from the general population, some of the observed effects could arise from migration. To address this issue, we use data from the ENOE and follow Minton and Wheaton (2022) to estimate if the policy increased migration from other states or municipalities within the same state, as detailed in Appendix A.3. We find no evidence of workers migrating from other states; however, we do observe migration from municipalities within the same state. This supports the idea that migration is costly and typically feasible only for workers in areas close to the border. Based on these results, we test whether our coefficients remain robust when excluding all untreated municipalities in border states from the control group (Figure A.5).[11] The

[10] Table A.5 presents the effect for each occupation. Although the coefficients are positive for the primary sector and white-collar workers, they are not statistically different from zero. On the other hand, the results for sales and elementary workers are positive and statistically significant, mainly due to the greater variation in the share of informality between municipalities for these labor groups.

[11] Note that our estimates for treated municipalities are not affected by the migration from the control group as we do not record migration in our population measure. It comes from imputed annual census

point estimates remain quantitatively similar when excluding these municipalities.

Another potential concern is that we compare municipalities with large baseline differences (Figure 3), which raises the possibility that, even with the exogenous nature of temperature deviations, we may not accurately identify the effect of the policy. To address this issue, we have implemented the DiDiT approach. However, as a robustness, we also leverage a matching DiT design using genetic matching in Appendix A.4. This approach selects a subsample of the control group that closely resembles border municipalities in terms of weather, poverty, urbanization, and pre-treatment mortality rates. After constructing the counterfactual, we estimate our preferred specification using this selected subset of control municipalities. Table A.8 presents the estimates from this exercise. The matching DiT is qualitatively and quantitatively consistent with our preferred specification. Similar to the DiDiT approach, it increases the statistical significance of our coefficients, yielding statistically significant reductions in cold temperatures. This suggests that our DiT model is likely conservative regarding the statistical significance of the policy's effect on cold-temperature mortality.

Finally, other potential concerns regarding our identification relate to isolating the policy's impact due to the timing of its implementation. The minimum wage and VAT reform occurred one year before COVID-19 and one year prior to the introduction of INSABI, which restructured Mexico's healthcare system in 2019. INSABI replaced Seguro Popular, creating new gaps in healthcare access, particularly during 2020-2021. Both COVID-19 and INSABI could have differentially impacted treated and control municipalities during our post-period, influencing weather-related mortality. We cannot fully rule out this possibility. However, our DiDiT design accounts for common changes in the mortality-temperature relationship across municipalities, which effectively accounts for changes in the gradient that occurred after 2020 due for unobserved factors. Thus, the DiDiT estimates should isolate the policy's impact. Additionally, we conduct a set of robustness checks to assess the consistency of our estimates in Appendix A.5, Table A.9. To account for the potential effects of COVID-19 in our estimates, we allow the effect of temperature on mortality to vary based on the number of COVID cases at the munici-

values. In addition, if young workers move to treated municipalities, the total number of recorded deaths should remain the same as long as migrating workers are not more likely to die. If they are more likely to die, the point estimates would be lower bounds of the true treatment effect.

pality level. We achieve this by interacting the temperature indicators with a continuous measure of COVID cases per municipality. This approach allow us to spatially control for changes in the temperature-mortality relationship due to COVID. To consider the effects of the shift from Seguro Popular to INSABI, we interact temperature with the state-level number of emergency room visits from uninsured and low-income populations, as well as state-level per capita healthcare expenditure. In all specifications, our main results remain consistent, addressing some of the concerns.

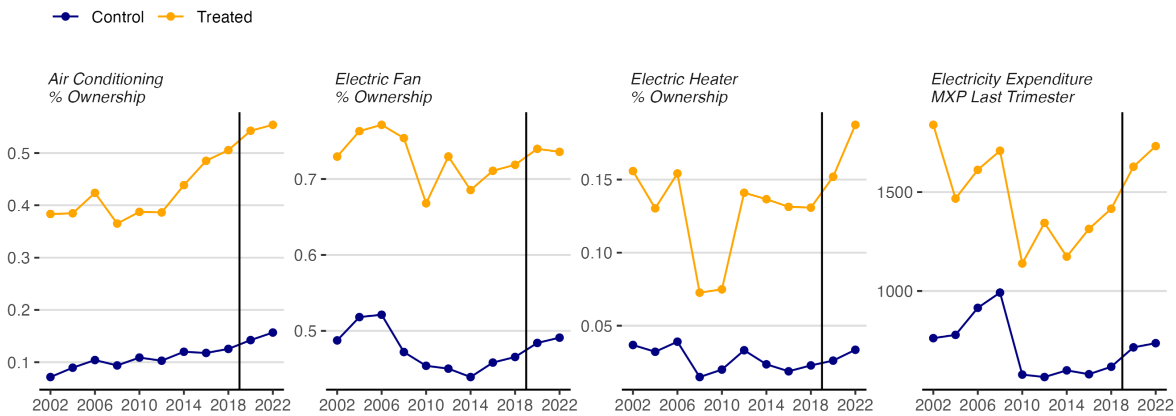
Finally, Appendix A.7 replicates the study by Cohen and Dechezleprêtre (2022) on the impact of the introduction of Seguro Popular on temperature-related mortality. The goals are to (i) strengthen the validity of our empirical strategy by testing if we can replicate the results of Cohen and Dechezleprêtre (2022), and (ii) assess whether the estimates of Cohen and Dechezleprêtre (2022) apply for our sub-sample of workers. Our results confirm Cohen and Dechezleprêtre (2022); the introduction of Seguro Popular increased workers' resilience to temperature shocks. We also contribute to Cohen and Dechezleprêtre (2022) by showing that the pre-treatment share of uninsured individuals and access to health facilities can influence Seguro Popular's effect on temperature-related mortality.

6. Mechanisms

The previous section provides evidence that increased income in border municipalities results in a reduction in temperature-related mortality. In this section, we identify potential mechanisms underlying this increased resilience. To achieve this, we use data from the ENIGH to examine whether residents in border municipalities have adopted protective behaviors. Specifically, we assess whether they have increased electricity expenditures or purchased electric heaters, air conditioners, and electric fans. Additionally, we leverage municipality-level electricity demand data to test if the policy led to increased electricity consumption in response to temperature shocks. We interpret higher electricity expenditures and consumption as adaptations along the intensive margin, particularly among those who already possess thermoregulating appliances.

Household-level energy appliances and electricity expenditure. Our econometric strategy resembles the effect of the policy on temperature-related mortality, with the caveat that instead of leveraging the exogenous interannual variations in temperatures to identify the effects, we need to rely on the traditional assumptions of DiD. To estimate β in a DiD-type setting, the common trend assumption (CTA) must hold. This means that, without the 2019 reform, electricity expenditure and ownership of energy appliances in border municipalities would have followed the same trajectory as for the rest of the country. Although we cannot directly test the validity of the CTA, Figure 5 shows the pre-treatment trajectories of treated and control units in our data.

Figure 5: Trends in ownership rates of appliances and electricity expenditures



Notes: Each panel shows the average evolution of the outcome in treated and control municipalities, before and after treatment.

Electric fans and heaters exhibit similar trajectories, but significant disparities exist regarding air conditioners.[12] Border municipalities consume more electricity and have higher ownership rates of air conditioners (an average of +30 percentage points) and fans (an average of +15 percentage points). Differences in municipality characteristics, like climatic conditions and average income, may explain the variations in levels and trajectories. For instance, border municipalities tend to be wealthier and located in warmer areas (Davis and Gertler, 2015; Pavanello et al., 2021). If this is the case, controlling for covariates $\mathbf{X}_{it}$ in our analysis is essential. Furthermore, as we cannot rule out a violation of the CTA in our setting, a TWFE-DiD estimator may not consistently identify the true average treatment effect.

[12]Some of the volatility in the trend of these appliances is likely related to the annual change in the sample of interviewed households by the ENIGH.

A traditional strategy to reduce CTA dependency is the synthetic control method (SCM) (Abadie, 2021). This methodology decreases reliance on the parallel trend assumption by re-weighting control units to match the average outcome of the treated group during pre-exposure periods. However, significant level differences between treated and control units complicate the identification of an appropriate combination of controls to accurately replicate both the levels and trends of the treated group. Therefore, we apply the synthetic difference-in-difference (SDID) method introduced by Arkhangelsky et al. (2021).

The SDID design is appealing because, like standard difference-in-differences, it considers unit-specific and time-specific fixed effects, allowing treated and control units to trend at different levels before policy implementation. Additionally, like SCM, SDID reduces reliance on the parallel trend assumption by optimally generating a matched control unit. However, unlike SCM, SDID employs unit weights to ensure that the average outcome for treated units is parallel to the weighted average of control units rather than directly matching their levels. As a result, SDID addresses common issues associated with TWFE-DiD and SCM: biased average treatment effects from non-parallel trends in TWFE-DiD, and difficulties in creating the convex combination of control units to match the average pre-treatment outcome of treated units in SCM.

Equation 5 presents the mathematical representation of the SDID design:

$$(\hat{\beta}^{sdid}, \hat{\gamma}, \hat{\mu}, \hat{\delta}) = \underset{\beta,\gamma,\mu,\delta}{\arg\min} \left\{ \sum_{c=1}^{N} \sum_{t=1}^{T} \big(Y_{ct} - \beta^{sdid} \mathbb{1}(Border)_{ct} \right. \\ \left. - \mu_c - \delta_t - \mathbf{X}_{ct}\gamma\big)^2 \hat{\omega}_c^{sdid} \hat{\lambda}_t^{sdid} \right\} \tag{5}$$

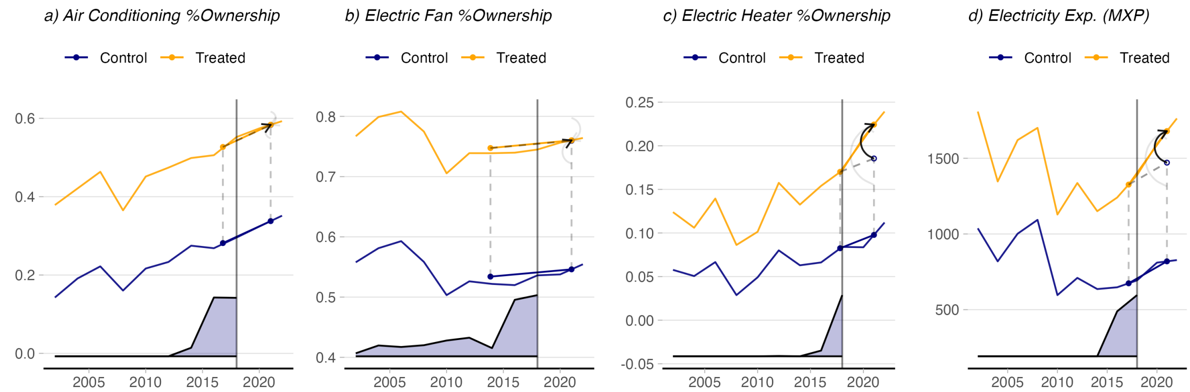
In this equation, Y_{ct} represents the outcome of interest in municipality c at time t (year of observation). $\mathbb{1}(Border)_{ct}$ is an indicator equal to 1 for border municipalities after 2019. $\mathbf{X}_{ct}$ is a vector of municipality-level covariates.[13] μ_c and δ_t denote fixed effects for municipality and year of observation. ε_{ct} is the error term. The ATT $\hat{\beta}^{sdid}$ results

[13] Like TWFE-DiD, SDID also allows to control for time-varying covariates $\mathbf{X}_{ct}$. To do so, we follow Arkhangelsky et al. (2021) by applying the SDID design to the residuals obtained from regressing Y_{it} on $\mathbf{X}_{ct}$.

from a two-way fixed effect regression with optimally chosen weights for units ($\hat{\omega}_c^{sdid}$) and observation periods ($\hat{\lambda}_t^{sdid}$). Unit weights ensure that we compare treated and control groups with approximately parallel trends before policy implementation. Time weights $\hat{\lambda}_t^{sdid}$ assign greater weight to pretreatment periods that closely resemble posttreatment periods. This maintains a consistent difference between the average posttreatment outcome for control units and their weighted average pretreatment values. We compute standard errors using the jackknife algorithm proposed by Arkhangelsky et al. (2021).[14]

Figure 6 plots the average outcome path for treated and control units in the SDID design. The SDID design better fits the pretreatment periods. At the bottom of the figure, we display pretreatment time weights in light blue. Positive time weights span multiple periods, with greater weights assigned to waves closer to the intervention.

Figure 6: Trends in ownership rates of appliances and electricity expenditures — SDiD



Notes: Each panel shows the average evolution of the outcome in treated and synthetic municipalities, before and after treatment. The shaded area at the bottom of each panel shows the distribution of optimal pre-treatment weights across time periods.

Table 5 presents our estimates for each outcome of interest using the TWFE-DiD and SDiD designs. The TWFE-DiD design suggests that the policy significantly increases electric heater ownership by 7.4-9.1 percentage points. We observe no significant effects on air conditioning or electric fan ownership. In the SDiD estimates, we confirm that the policy is significantly associated with an increase in the electric heater ownership rate of 3.9-4.0 percentage points, representing an 81% increase relative to the pre-policy mean. Additionally, we find a significant increase in electricity expenditure of 172-208

[14]This procedure consists of iterating over all units in the data, in each iteration removing the given unit, and recalculating $\hat{\beta}^{sdid}$, denoted $\hat{\beta}^{sdid}_{(-i)}$, maintaining fixed the optimal weights obtained in the original SDID estimate. The variance of the jackknife, $\hat{V}^{j}_{\beta}$, is then calculated using the variance of all $\hat{\beta}^{sdid}_{(-i)}$ estimates. We also show robustness results for this choice.

pesos per quarter, or 20-24% relative to the pre-policy mean. The effects on fans and air conditioning remain small and statistically insignificant. A plausible explanation for the lack of investment in expensive appliances like air conditioning is the permanent nature of the income shock. Previous literature suggests that permanent increases in income are mainly allocated to the consumption of nondurable goods (Alonso, 2022; Dautović et al., 2024). In contrast, transitory income sources, such as remittances, usually encourage precautionary savings and support investment in durable goods, such as air conditioners (Randazzo et al., 2023).

Table 5: The effect of minimum wage and tax reform on energy adaptation (ENIGH)

	Air conditioning		Electric fan		Electric heater		Electricity exp.	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Panel A: TWFE-DiD								
$\hat{\beta}$	0.064*	0.019	0.004	-0.020	0.091**	0.074*	238.214*	131.308
	(0.035)	(0.037)	(0.027)	(0.032)	(0.038)	(0.039)	(124.261)	(116.324)
Panel B: SDiD								
$\hat{\beta}$	0.001	-0.015	0.001	-0.007	0.039**	0.040**	208.256***	171.800**
	(0.017)	(0.021)	(0.019)	(0.018)	(0.015)	(0.016)	(79.783)	(84.240)
Controls		✓		✓		✓		✓
Municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	1496	1496	1496	1496	1496	1496	1496	1496
Mean Outcome	0.167	0.167	0.535	0.535	0.048	0.048	878.984	878.984

Notes: The ENIGH sample is restricted to municipalities that are surveyed in all waves. Controls include household income, 24-degree cooling degree days, 15-degree heating degree days, share of households owning a house, share of households living in an urban area, share of female household heads, shares of household heads having completed primary, secondary and post-secondary education, and average age of the household head. Standard errors are computed using the jackknife algorithm proposed by Arkhangelsky et al. (2021). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Our results suggest that households in border municipalities improved their ability to cope with temperature changes by purchasing electric heaters (extensive margin) and increasing their electricity expenditure (intensive margin). The increase in the number of electric heaters suggests adaptation to cold weather. In contrast, the rise in electricity expenditure could reflect adaptation to both heat and cold, as households might have increased usage of both air-conditioning units and electric heaters.

Our results remain consistent across various specification tests, including different calculations of standard errors (Table A.10) and a narrower pretreatment period (Table A.11). We also conduct a placebo test for each dependent variable by: (i) excluding the treated municipalities, (ii) randomly selecting nine control units and assigning them to the treatment group, and (iii) estimating the SDID estimator. Figure (Figure A.7) presents the distribution of p-values from 1,000 replications. For all outcomes, the mean and median p-values exceed 0.35. Finally, we analyze a TWFE-DID design using households as the observation unit. Although we must interpret these estimates cautiously, they provide an interesting comparison to the municipality-level analysis. We report results in Table A.12 for the entire sample and in Table A.13 for the sample of balanced municipalities. The coefficients align with our baseline estimates.

Municipality-level electricity demand. One caveat regarding the estimates from Table 5 is that our measure of electricity expenditures is self-reported, making them susceptible to measurement error and exaggeration bias (Bound et al., 2001; Meyer and Sullivan, 2003). Another concern is that differential changes in electricity prices may bias these results; however, this is unlikely as household electricity tariffs in the country are heavily regulated and subsidized (Davis et al., 2014; Komives et al., 2009). We address these concerns by conducting an additional exercise using municipal electricity consumption (in kWh) for the residential sector, obtained from the national power company. To estimate the policy effect on power demand, we employ a DiDiT design similar to Equation 3:

$$\begin{aligned} ln(E_{ct}) = & \sum_{b=0}^{6} \lambda_b^p \Big[D_{bct} \times \mathbb{1}(Border)_{ct} \Big] + \\ & \sum_{b=0}^{6} \lambda_b^c \Big[D_{bct} \times Treated_c \Big] + \sum_{b=0}^{6} \lambda_b^y \Big[D_{bct} \times \delta_y \Big] + \\ & \gamma X_{ct} + \delta_{cy}^{\tau} + \delta_{cm}^{\tau} + \delta_{my}^{\tau} + \varepsilon_{ct} \end{aligned} \tag{6}$$

In Equation 6, the dependent variable is the logarithm of electricity sales per user (kWh). We estimate the effect using OLS rather than PPML due to the log-normal distribution of electricity demand (see Appendix Figure A.8). The primary specification includes fixed effects for year-by-month, municipality-by-year, and municipality-by-month.

One concern with these estimates is that confounding factors, like COVID-19, may bias our results, even though inter-annual temperature changes likely remain independent. If confounding factors exist, they likely affect sectors beyond residential power demand. An economic shock, for instance, would affect electricity demand for both households and industries. Fortunately, CFE provided data on power demand per user across the industrial, public, agricultural, and residential sectors, which we use as a placebo test for our analysis.

Table 6 presents estimates from the interaction between the policy and temperature intervals separately for each sector. Consistent with our findings from the ENIGH data, we observe increased temperature-related residential electricity demand following the treatment (column 1). For cold temperatures, the increase ranges from 1.9% for temperatures below 10 °C to 0.62% for temperatures between 15 and 20 °C. For warm temperatures, electricity demand rises by 1.4% for each additional day per month between 25 and 30°C, and by an statistically insignificant 0.75% for each day above 30°C. Given the average power demand per household in our data (112 kWh), the 1,9% and 1.4% coefficients imply an increase of between 1.5 and 2.1 kWh. 2 kWh is roughly equivalent to 41 hours of a 50 Watt fan, 2.1 hours of a standard (1 kW) AC unit, and 1.4 hours of a (1.5 kW) electric heater. Overall, these estimates confirm insights from Table 5, indicating that some benefits from the policy arise from adaptation through new thermoregulating appliances and increased power demand. In contrast, we find no statistically significant effects on power demand in the industrial, public, or agricultural sectors (columns 2-4). These estimates reduce the likelihood of correlated unobservables biasing our coefficients, as such biases would need to apply exclusively to household power demand. However, as the policy could also affect these sectors in unexpected ways, these exercises serve only as robustness checks.[15]

[15]For instance, on the one hand, higher wages could increase costs and reduce demand for intermediary inputs; on the other, the reduction in VAT could lower these costs.

Table 6: Effect of the policy on temperature-related electricity demand

	Log(Electricity per capita)			
	Residential (1)	*Industrial* (2)	*Public Light.* (3)	*Agricultural* (4)
$\mathbb{1}$(Border) ×				
(-15:10]	0.0191***	0.0178	-0.0203	-0.0546
	(0.0060)	(0.0131)	(0.0173)	(0.0383)
(10:15]	0.0014	0.0107	-0.0068	-0.0321
	(0.0068)	(0.0115)	(0.0164)	(0.0272)
(15:20]	0.0062**	-0.0005	0.0140	–0.0302
	(0.0031)	(0.0053)	(0.0155)	(0.0295)
(25:30]	0.0144**	0.0054	0.0050	0.0327
	(0.0060)	(0.0049)	(0.0067)	(0.0238)
(30:40]	0.0075	0.0066	0.0072	0.0622
	(0.0052)	(0.0062)	(0.0098)	(0.0737)
Fitted Stat				
Observations	126611	124802	122359	82185
Mean Outcome (pre-policy)	143.3	1631.9	21348.9	7863.3
Fixed Effects				
Treatment-Municipality-Month	✓	✓	✓	✓
Treatment-Municipality-Year	✓	✓	✓	✓
Treatment-Month-Year	✓	✓	✓	✓
Controls				
Precipitation	✓	✓	✓	✓
COVID	✓	✓	✓	✓
Bins × Year	✓	✓	✓	✓
Bins × Treated	✓	✓	✓	✓

Notes: The dependent variable is the logarithm of average electricity demand (in kWh). The coefficients are obtained from an Ordinary Least Squares regression. The coefficients refer to variables with the number of days per month within the daily temperature intervals. The reference temperature category is days within (20-25] °C. Controls include a second-order polynomial of precipitation and an indicator variable for the COVID pandemic (equal to 1 after March 2020). Standard errors clustered at the municipality level. Significance levels are indicated as: $^{***} < 0.01,^{**} < 0.05,^{*} < 0.1$.

7. Conclusion

This paper provides new evidence on how public policies can improve workers' resilience to temperature-related mortality. For this, we leverage a 2019 reform in Mexican border municipalities that increased wages and reduced value-added taxes to demonstrate that higher disposable income mitigates the negative effects of temperature on mortality. Our analysis also shows that income-enhancing policies can stimulate adaptive responses through higher residential electricity consumption and households' adoption of electric heaters, highlighting the role of increased income in facilitating adaptation.

This study has significant implications for policy design on climate change adaptation and socioeconomic development.

First, our results show that income can mitigate the adverse effects of temperature on socioeconomic outcomes, highlighting the potential for integrating socioeconomic and environmental policies. Addressing income constraints can enhance the resilience of vulnerable populations to climatic shocks while tackling climate impacts and socioeconomic inequalities. The literature on climate justice and adaptive capacities has extensively examined the relationship between income and climate resilience (e.g., Bistline et al., 2024). The Intergovernmental Panel on Climate Change (IPCC) emphasizes the importance of equity and social justice in climate action, noting that adaptation efforts become more effective when aligned with sustainable development and poverty eradication goals (IPCC, 2014).

Second, our results suggest that redistributive programs funded by carbon taxes can effectively address the complex challenges of climate change and socioeconomic inequality. This aligns with proposals to redistribute carbon tax revenues in support of sustainable development objectives, mitigating immediate temperature impacts and promoting long-term socioeconomic resilience. Therefore, policies combining environmental taxation with income redistribution can serve as a dual strategy for tackling climate change and reducing inequality.

Finally, this study opens new avenues for future investigations and policy developments aimed at protecting workers' health and well-being in the face of environmental challenges. Future research should explore the heterogeneous effects of temperature on worker productivity and compare the impact of similar policies across different contexts.

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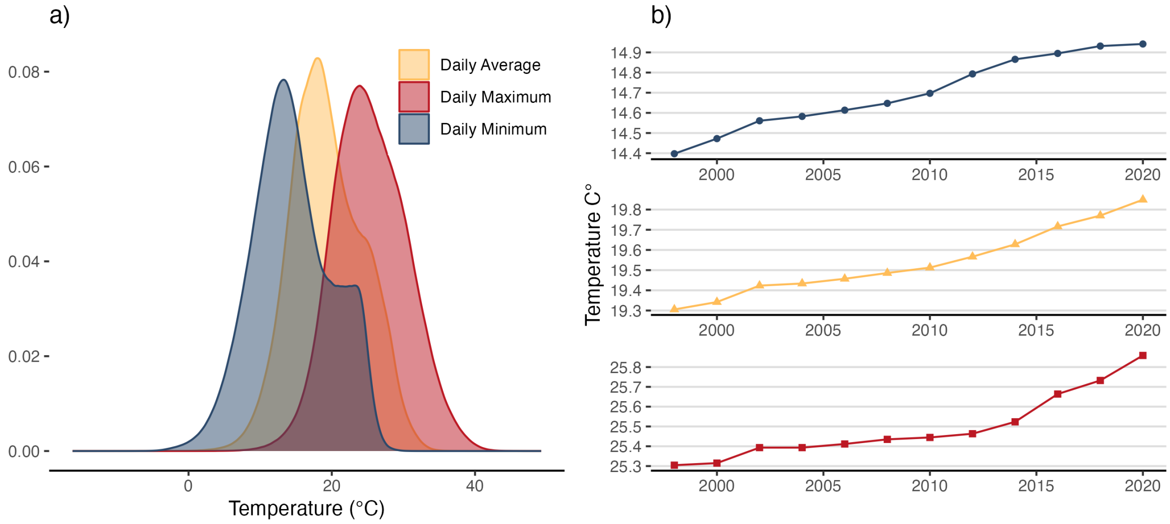
Appendix A

A.1 Additional descriptive statistics

Figure A.1: Temperatures in Mexico



Notes: This figure shows the density distribution of average daily mean, maximum, and minimum temperatures across the country in the left panel. In the right panel, we present the annual four-year rolling average of the municipal minimum, mean, and maximum temperatures. Self-constructed statistics using data from ERA5.

Table A.1: Classification of workers

Occupation (INEGI)	**From**	**To**	**Group**
Workers in the Primary Sector			
Workers in agriculture, livestock, hunting, and fishing activities	1998	2021	Primary Sector
Elementary Activities and Artisans			
Education workers	1998	2012	Elementary
Workers in the processing industry	1998	2012	Elementary
Fixed machinery operators	1998	2012	Elementary
Assistants in the industrial and artisan production process	1998	2012	Elementary
Drivers of mobile machinery and means of transport	1998	2012	Elementary
Artisans	2013	2021	Elementary
Industrial machinery operators, assemblers, and transport drivers	2013	2021	Elementary
Workers in elementary and support activities	2013	2021	Elementary
Sales and Personal Services			
Merchants, trade clerks, and sales agents	1998	2012	Sales
Vendors	1998	2012	Sales
Workers in personal services in establishments	1998	2012	Sales
Domestic Workers	1998	2012	Sales
Armed Forces, Protection and Surveillance Workers	1998	2012	Sales
Merchants, sales clerks, and sales agents	2013	2021	Sales
Workers in personal services and surveillance	2013	2021	Sales
White Collar Workers			
Professionals	1998	2012	White Collar
Technicians	1998	2012	White Collar
Art, sports and entertainment workers	1998	2012	White Collar
Officers and managers	1998	2012	White Collar
Control personnel in the industrial production process	1998	2012	White Collar
Middle-level administrative workers	1998	2012	White-collar
Lower-level administrative workers	1998	2012	White Collar
Officers, directors and heads	2013	2021	White Collar
Professionals and technicians	2013	2021	White Collar
Auxiliary workers in administrative activities	2013	2021	White Collar

Notes: This table presents the concordance between the classification of occupations in Mexican death certificates and the categories we use in this study. The Mexican National Institute of Geography and Statistics (INEGI) is in charge of determining the different occupations. In particular, there is a break in 2013 for the classification of occupations by INEGI.

Table A.2: Sociodemographic differences in mortality rates between labor groups

	No Work	Work	Primary Sector	Elementary	Sales	White Collar
Sex						
Male (%)	28.24	89.25	98.76	94.04	85.32	78.86
Female (%)	71.76	10.75	1.24	5.96	14.68	21.14
Age						
[13, 20) (%)	1.97	1.22	1.03	1.60	0.82	1.44
[20, 40) (%)	5.26	16.22	8.30	19.62	15.75	21.20
[40, 60) (%)	15.09	29.93	16.90	34.28	31.71	36.84
[60, 120] (%)	77.69	52.63	73.77	44.50	51.72	40.52
Education						
Unknown Education (%)	3.43	2.61	2.84	2.71	2.80	2.06
Professional (%)	5.02	14.26	0.42	4.63	6.17	45.82
High School (%)	15.25	23.31	5.59	25.84	32.71	29.10
No Education (%)	25.45	14.95	36.59	11.31	9.72	2.20
Primary School (%)	50.85	44.87	54.56	55.52	48.59	20.81
Income (Estimated)						
[467, 1976) (%)	20.85	23.21	77.77	6.76	7.34	0.98
[1976, 2727) (%)	32.11	13.74	20.30	13.72	17.79	3.15
[2727, 3404) (%)	31.19	17.69	1.72	26.07	36.88	6.08
[3404, 12844] (%)	15.85	45.36	0.21	53.45	37.99	89.79
Social Security						
Unmatchified (%)	8.38	10.57	13.38	9.57	9.60	9.71
No Social Security (%)	25.52	33.56	47.40	31.65	34.50	20.69
Social Security (%)	66.10	55.87	39.22	58.77	55.90	69.61

Notes: This table presents the percentage of deaths grouped by different sociodemographic characteristics and labor groups. Interpret each number as the percentage share of deaths within the specific group. For sex, age, and access to social security, we obtain data from mortality records. For income, we estimate the average household income for each category using data from the National Survey of Employment and Occupation alongside a simple log-linear model similar to Cohen and Dechezleprêtre (2022).

Table A.3: Macro characteristics of death certificates across labor groups

	No Work	Work	Primary Sector	Elementary	Sales	White Collar
Poverty (CONEVAL)						
Very High (%)	1.93	2.29	7.26	0.85	0.6	0.45
High (%)	7.48	8.89	23.67	5.29	3.87	2.72
Average (%)	7.77	9.28	19.45	7.74	5.88	4.03
Low (%)	13.88	15.48	21.36	15.85	14.04	10.66
Very Low (%)	68.94	64.07	28.26	70.26	75.61	82.14
Other Macro Controls						
NASA-GRDI	45.53	47.26	60.73	45.63	42.42	40.24
	(19.82)	(17.68)	(12.78)	(18.85)	(19.41)	(19.68)
Share of Indigenous Persons	7.33	8.13	16.18	6.25	5.3	4.79
	(16.65)	(15.56)	(27.67)	(13.51)	(11.22)	(9.85)
Share of Rural Households	16.95	19.33	39.56	15.54	12.28	9.93
	(23.56)	(21.41)	(28.5)	(21.55)	(18.77)	(16.81)

Notes: This table presents the percentage share of mortality records for different municipal characteristics and labor groups. The poverty indicator for each municipality comes from the National Commission for the Evaluation of Economic Policies (CONEVAL). The NASA-GRDI comes from NASA's Socio-Economic Data and Application Center. The share of indigenous people and rural households comes from the 2020 Mexican Census.

A.2 Robustness: Additional results

Table A.4: Monthly excess mortality due to temperature deviations

	(-15:10]	(10:15]	(15:20]	(25:30]	(30:40]	$\sum_b$ (p.value $<$ 0.1)	$\sum_b$ (Total)
Primary Sector	61.07***	212.67***	171.27***	17.68*	9.97***	472.66	472.66
	(3.78)	(15.44)	(26.43)	(10.26)	(2.58)		
Elementary	29.16***	63.16***	77.07**	11.18	3.83	169.39	184.40
	(5.52)	(22.52)	(34.62)	(11.81)	(2.39)		
Sales	11.34**	8.85	–18.69	–10.09	2.68	11.34	–5.91
	(4.78)	(18.58)	(34.48)	(10.79)	(2.02)		
White Collar	6.95*	8.89	37.91	–10.13	–1.10	6.95	42.52
	(3.71)	(1.78)	(28.64)	(8.68)	(1.47)		
$\sum_i$ (p.value ¡ 0.1)	108.52	275.83	248.34	17.68	9.97	675.03	
$\sum_i$ (Total)	108.52	301.26	274.51	8.64	15.38		708.31

Notes: We estimate the average effect of each temperature interval on monthly mortality rates by calculating $\partial M_i/\partial D_b$ for each occupation using the coefficients estimated in Equation 2. After estimating $\partial M_i/\partial D_b$, we compute the monthly number of deaths attributable to each temperature bin, $\hat{m_i}$, as follows: $\hat{m_i} = \partial M_i/\partial D_b \times \left\{[(M_i/10000) \times pop_i] \times D_b \times N\right\}$, where M_i is the mortality rate per 10,000 inhabitants of occupation i, pop_i is the average population per municipality in occupation i, D_b is the average number of monthly days in the bin b, and N is the number of municipalities in the country. To estimate the standard errors for $\hat{m_i}$, we calculate bootstrapped 95% confidence intervals across 1,000 iterations. Standard errors clustered at the municipality level in parentheses.

Table A.5: The mitigating effect of informality on minimum wage benefits

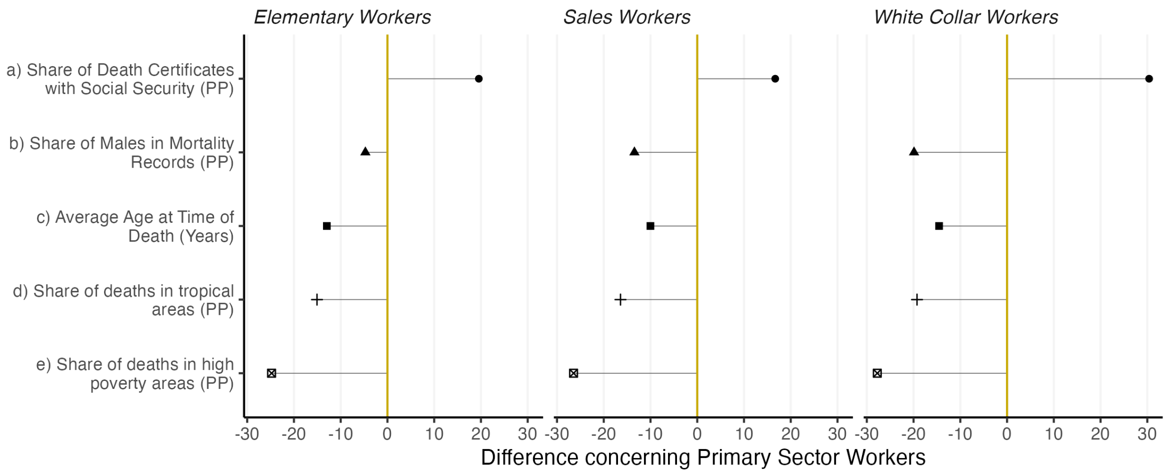
	Primary Sector (1)	Elementary (2)	Sales (3)	White Collar (4)
λ_{mw}^{inf}	0.0001	0.0012*	0.0018*	0.0012
	(0.0009)	(0.0006)	(0.0010)	(0.0012)
Fitted Stats				
Observations	454553	333612	272428	231618
Average D_{ct}	22.27	22.27	22.27	22.27
# Control Mun.	2414	2414	2414	2414
# Treated Mun.	43	43	43	43
# Control Periods	288	288	288	288
# Treated Periods	36	36	36	36

Notes: The dependent variable is mortality rates per 10,000 people. The coefficients are obtained from a Poisson Maximum Likelihood Estimator regression. Interpret the coefficients as the effect of increasing the share of informality by ten percentage points before treatment on the reduction of temperature-related mortality related to the increase in disposable income due to the increase in minimum wages and lower VAT between border and non-border municipalities. The specification includes treatment-municipality-month, treatment-municipality-year, and treatment-month-year of observation fixed effects. Controls include a second-order polynomial of precipitation and an indicator variable for the COVID pandemic (equal to 1 after March 2020). Standard errors clustered at the municipality level. Significance levels are indicated as: $^{***} < 0.01,^{**} < 0.05,^{*} < 0.1$.

A.2.1 Accounting for socioeconomic differences

Considering that temperature effects vary between occupations due to socioeconomic and demographic factors beyond occupational attributes, it is crucial to understand the role of these observable differences on workers' mortality responses to temperature changes. For instance, if workers in occupation i significantly differ from those in occupation j, the varying impact of temperature on mortality may partly arise from these differences. Formally, assume that the difference in the effect of temperature b between two occupations is λ_b^{ji}. Considering a set of observable socioeconomic differences (v), we can estimate a new coefficient equivalent to $\lambda_b^{ji}|v_j$. This coefficient represents the difference in the effects of temperature between occupations, net of the observed variations between i and j. Figure A.2 illustrates the differences in key variables between workers in the primary sector and other labor groups.

Figure A.2: Differences in observables between occupations

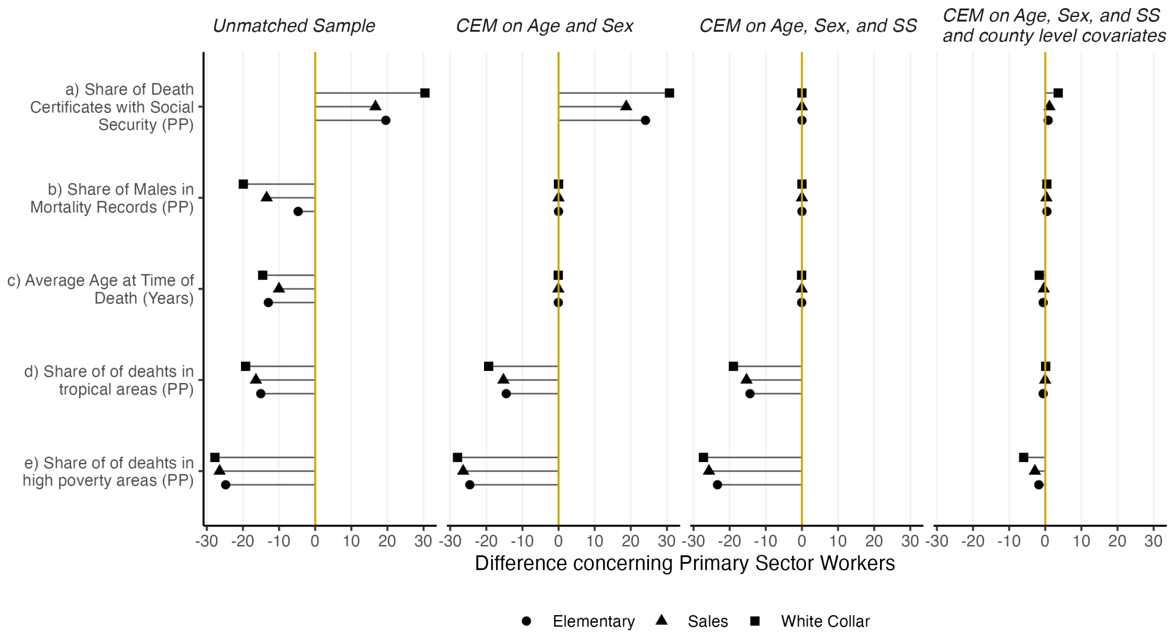


Notes: This figure shows the difference between primary sector workers and elementary, sales, and white-collar workers for selected sociodemographic characteristics at their time of death. The data comes from the administrative data of the Mexican National Institute of Geography and Statistics, The Mexican Water Commission, and the National Council for the Evaluation of Poverty. PP referees to percentage points.

Primary sector workers have a significantly lower percentage of death certificates with social security compared to other occupations. Other occupations also have fewer males in mortality records. The difference in social security coverage varies from 3.7 percentage points for elementary workers to 19.9 percentage points for white-collar workers. A similar trend appears concerning age. Elementary, sales, and white-collar workers typically die 10 to 15 years earlier than primary sector workers. Regarding climate and marginalization, deaths related to agriculture are more prevalent in tropical and marginalized areas.

Following the methodology outlined in Bressler et al. (2025), Figure A.3 shows the differences in sociodemographic characteristics between labor groups after matching. We present results for the unmatched sample and three Coarsened Exact Matching (CEM) specifications: matching on age and sex; matching on age, sex, and access to social security; and matching on age, sex, access to social security, and municipal-level covariates, which include municipal poverty, the share of rural households, and the state of observation. The CEM algorithm improves balance between occupations for all selected covariates, thanks to the large number of observations in the raw data (over 14 million).

Figure A.3: Differences in observables between occupations (CEM balance)

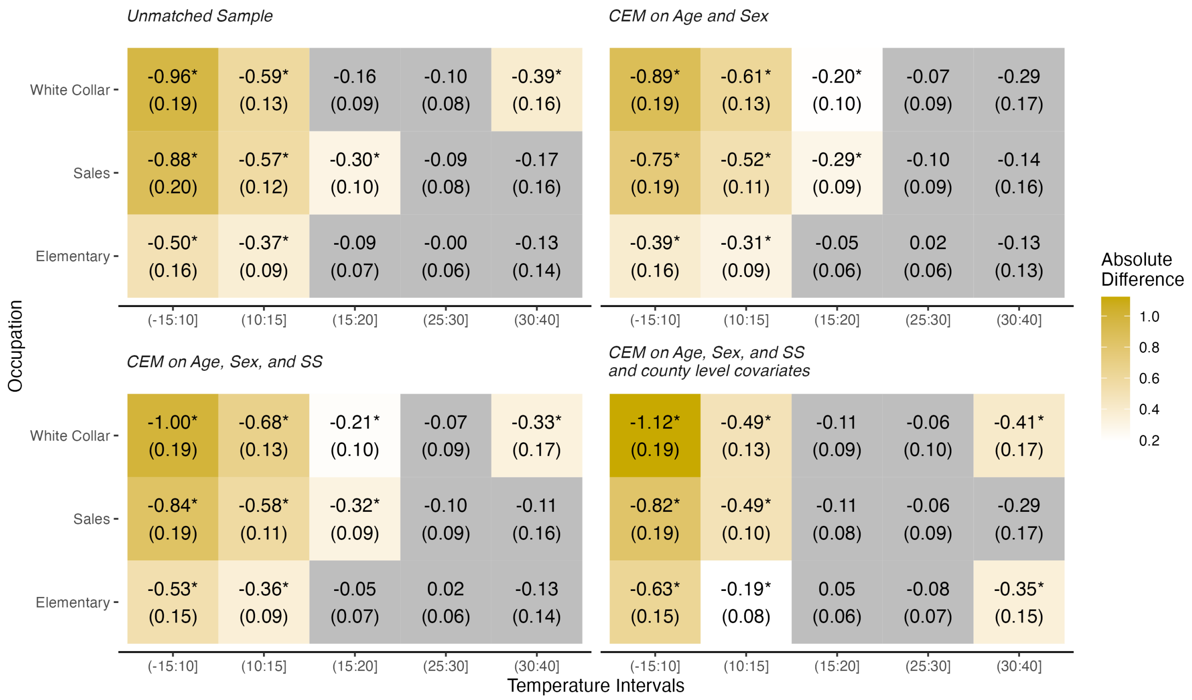


Notes: Matched difference in observables across occupations using primary sector workers as the reference category. The unmatched sample presents the results without matching. CEM on age and sex matches on age and sex by forcing the distribution of all occupations to mimic the distribution for primary sector workers. CEM on age, sex, and SS matches on age, sex, and access to social security. CEM on age, sex, SS, and municipal-level covariates matches age, sex, access to social security, the marginalization index, the share of rural households, and the state of observation.

Figure A.4 illustrates the differences in the effects of various temperature intervals across different matched specifications and the unmatched sample. These coefficients reveal the heterogeneous effects of temperature on mortality, assuming all occupations have a similar distribution of matched covariates. While quantitative estimates vary slightly with different socioeconomic controls, a consistent pattern emerges. All coefficients are negative, indicating that primary sector workers are more sensitive to temperature changes.

Our matching results confirm the key findings about the varying impacts of temperature deviations on mortality across different occupations. Workers in the primary sector exhibit lower resilience to days outside thermal comfort compared to those in the other occupations. These differences are statistically significant when matching on age, sex, and our comprehensive set of municipality-level covariates for cold and hot days (except for

Figure A.4: Difference in the effect of temperatures between occupations with CEM



Notes: Matched difference in the effect of temperatures on mortality across occupations using primary sector workers as the reference category. We present results for three occupations, four matching specifications, and five temperature intervals. All econometric models estimate the effect of temperature on mortality with PPML panel models containing fixed effects for the municipality-month and municipality-year of observation with standard errors clustered at the municipality level. The unmatched sample presents the results without matching. The CEM on age and sex matches age and sex by forcing the distribution of all occupations to mimic the distribution for primary sector workers. The CEM on age, sex, and social security matches on age, sex, and access to social security. The CEM on age, sex, social security, and municipal covariates matches on age, sex, access to social security, the marginalization index, the share of rural households, and the state of observation. Standard errors clustered at the municipality level. Significance levels are indicated as: $^* < 0.1$.

days above 30°C relative to sales workers). Since these differences persist when matching with our complete set of covariates, it appears that the heterogeneous impacts of temperature on mortality across occupations hold after accounting for observable differences in socioeconomic vulnerabilities. At the same time, it is important to stress that we can only account for the set of observable covariates. Therefore, any difference in vulnerability caused by unobservable factors will remain unaccounted for in our econometric framework.

A.3 Robustness: minimum wage changes and migration

We address concerns that the policy may significantly affect internal migration patterns, potentially invalidating some identifying assumptions of the empirical strategy. We model the effect of changes in minimum wage policies on migration after Minton and Wheaton (2022). Equation 7 presents our empirical design. In this design, Δm_{ct} represents the number of workers migrating to municipality c at time t. $\mathbb{1}(Border)_{ct}$ is our indicator variable, set to one after the increase in minimum wages and the reduction in VAT for border municipalities. β is our parameter of interest, reflecting the effects of the policy on the logarithmic relative change in migration flows. We identify treatment effects by comparing worker migration between treated and control municipalities after the changes in minimum wages and VAT. We assume that, following the policy change, migration patterns would have evolved in parallel between border and non-border municipalities. We estimate the effect using PMLE because of the high percentage of observations with zero values (years with zero migrants between municipalities).

$$\Delta m_{ct} = exp\left[\beta\,\mathbb{1}(Border)_{ct} + \lambda_c + \lambda_t\right] + \epsilon_{ct} \tag{7}$$

We obtain migration data from the National Survey of Employment and Occupation (ENOE), a quarterly rotating panel survey of Mexico's labor market. The dataset indicates whether a new household member migrated from another state or within the same state each quarter. We estimate the effect of the policy change on the total number of workers reporting migration in treated municipalities, regardless of whether they migrated from another state or the same state. To focus on workers, we restrict our sample to individuals actively participating in the labor market, excluding children, retirees, and economically inactive persons. We specifically include unemployed workers and those earning up to two minimum wages to target labor force participants more likely to be affected by the policy change.

In Table A.6, we present the results from Equation 7. We do not find evidence of cross-state migration induced by the policy reform. However, we observe a 20% increase in low-wage worker migration across municipalities within the same state. The results in columns 1 and 3 of Table A.6 align with those of Minton and Wheaton (2022) who find no evidence of migration from control to treated states in the US. The significance in columns 2 and 4 arises because, while Minton and Wheaton (2022) can only examine the average effect at the state level, our source of variation allows us to analyze migration within Mexican states. All else being equal, we can assume that migration costs for low-income workers are lower when moving to other municipalities within their state than when relocating across states.

Table A.6: Effect of the policy change on worker migration (Low income workers)

	(1)	(2)	(3)	(4)
β	8.00	-20.1**	7.62	-20.16**
	(24.68)	(10.79)	(24.7)	(10.79)
Fitted Stats				
N.Mun	823	1476	820	1472
N.Obs	10090	15597	9992	15436
N.Periods	16	16	15	15
R2	0.4	0.6	0.4	0.6
Migration (Pre-Treat)	212.3	1335.4	212.3	1335.4
Outcome Variable				
Between States	Yes	No	Yes	No
Within States	No	Yes	No	Yes
Sample				
With 2020	Yes	Yes	No	No
Without 2020	No	No	Yes	Yes

Notes: This table presents the effect of the change in minimum wages between border and non-border municipalities on worker migration patterns. To estimate the effect, we used data from the National Survey of Employment and Occupations. The dependent variable is the total number of newly reported persons in surveyed households who participate in the labor force and earn up-to two minimum wages. The causal variable is an indicator variable equal to one if municipality c is a border municipality after 2018. Standard errors in parentheses. We control for county and year fixed effects and cluster standard errors at the county level. We present the results for two samples and two outcome variables. (1) looks at the impact on migration between states. (2) looks at migration within the state. (3) and (4) perform the same analysis for migration between and within states while restricting the sample to years before 2020. Significance codes: *** <0.01, ** < 0.05, * < 0.1.

Before discussing the implications of these results for our point estimates, we conduct a final exercise to account for potential bias in our raw difference-in-differences. As discussed in Minton and Wheaton (2022), a simple difference-in-differences design can introduce bias if policies or changes in migration determinants correlate with the policy. To address this concern, we estimate a triple difference-in-differences of the following form:

$$log(\Delta m_{ct}) = \beta\, \mathbb{1}(Border)_{ct} \times \mathbb{1}(g = MW) + \lambda_{gc} + \lambda_{gt} + \lambda_{ct} + \epsilon_{ct} \tag{8}$$

In this equation, $\mathbb{1}(g = MW)$ serves as an indicator variable, equal to one for the migration of workers with incomes near the minimum wage threshold. The triple difference-in-differences compares the within-county migration patterns of near-minimum wage workers to the migration patterns of higher wage workers, who are less likely to be affected by the policy. In our case, we estimate the difference between workers earning between two and three minimum wages and those earning less than two minimum wages. As Minton and Wheaton (2022) notes, the new assumption is that, in the absence of the policy, the migration patterns of near-minimum wage workers in treated counties would have mirrored the migration patterns of higher income earners to treated counties.

Table A.7 presents the results of the triple DiD exercise. Consistent with Table A.6, the results suggest an increase in migration to treated municipalities from control units within the same state.

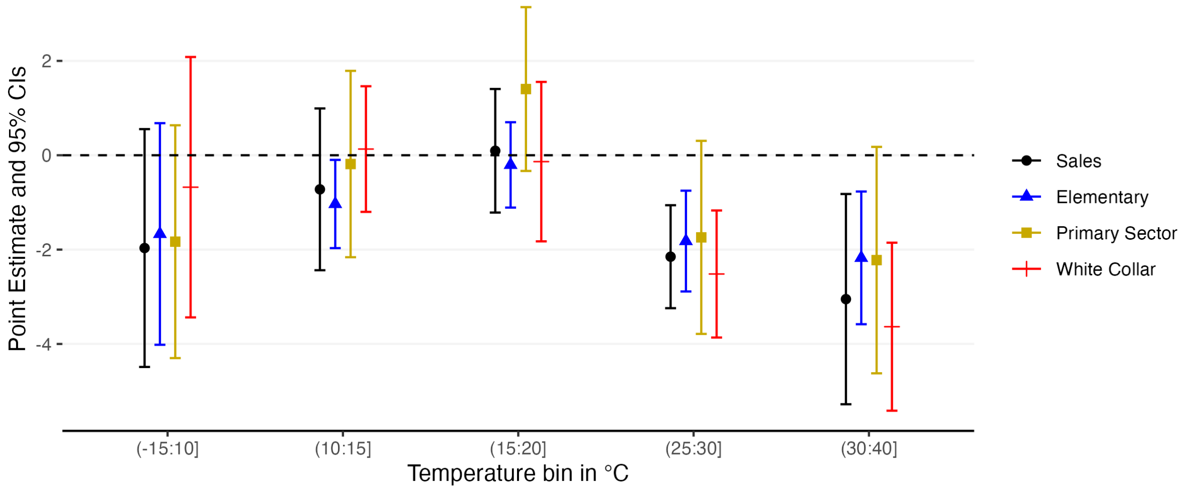
Table A.7: Effect of the policy change on worker migration (Triple DiD)

	(1)	(2)	(3)	(4)
β	2.51	30.32***	-2.85	29.17***
	(24.72)	(5.89)	(41.92)	(7.53)
Fitted Stats				
N.Mun	1260	2500	1244	2481
N.Obs	3828	12471	3694	12064
N.Periods	32	32	30	30
R2	0.98	0.98	0.98	0.98
Migration (Pre-Treat)	1066.8	2800.9	1066.8	2800.9
Outcome Variable				
Between States	Yes	No	Yes	No
Within States	No	Yes	No	Yes
Sample				
With 2020	Yes	Yes	No	No
Without 2020	No	No	Yes	Yes

Notes: This table presents the effect of the change in minimum wages between border and non-border municipalities on worker migration patterns. To estimate the effect, we use data from the National Survey of Employment and Occupations. The dependent variable is the logged value of the total number of new reported persons in surveyed households participating in the labor force. The causal variable is an indicator variable equal to one if municipality c is a border municipality after 2018. Standard errors in parentheses. We control for county and year fixed effects and cluster standard errors at the county level. We present the results for two samples and two outcome variables. (1) looks at the impact on migration between states. (2) looks at migration within the state. (3) and (4) perform the same analysis for migration between and within states while restricting the sample to years before 2020. Significance codes: *** <0.01, ** <0.05, * <0.1.

Given evidence of migration within municipalities in border states, we analyze the robustness of our estimates on the policy effect by excluding potentially contaminated controls. Figure A.5 illustrates the mitigating effect of the policy change when excluding municipalities in border states from the control group. The results are not statistically different from our preferred specification.

Figure A.5: Effects of higher minimum wages and lower VAT on temperature-related mortality between labor groups (restricted sample without control municipalities in border states)



Notes: Effect of the difference in minimum wages and VAT between border and non-border municipalities since 2019 on the consequences of an additional day with average daily air temperatures within the specified temperature bin concerning days between 20° and 15°. We estimate the effect independently for each labor group. To simplify the interpretation of the coefficients, we transform the value of λ_b^{mw} into $[exp(\lambda_b^{mw}) - 1] \times 100$. Interpret λ_b^{mw} as the percentage change in the temperature-related mortality rate due to the difference in minimum wages between border and non-border municipalities after an additional day per month outside the thermal comfort point. We restrict the sample to control municipalities outside of border states. The econometric model estimates the effect of differences in minimum wages on temperature-related mortality with a PPMLE panel model with municipality-month, municipality-year, and month-year fixed effects along with fixed effects for precipitation and COVID. Standard errors in parentheses clustered at the municipality level.

A.4 Robustness: Matching DiT

Although interannual temperature variations should be quasi-exogenous, unobservable factors may still correlate with both temperatures and the selection of our treatment group. Moreover, significant differences in mortality rates between treated and control groups, or random shocks affecting either group, could inflate standard errors and diminish the statistical confidence of our estimates.

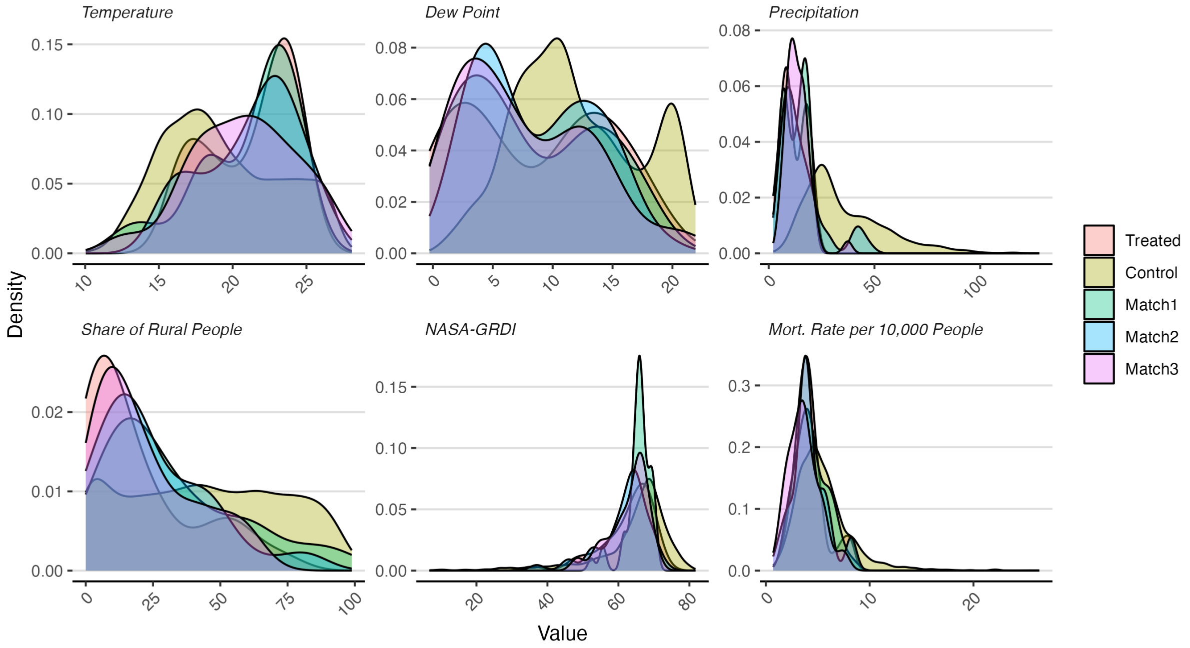
To address these concerns, we estimate the treatment effect using a matched subsample of control municipalities that closely resemble our treated group in the border region. To do so, we employ a genetic matching (GM) algorithm. Genetic matching is a multivariate method that achieves balance across covariates in observational studies (Diamond and Sekhon, 2013). The algorithm combines principles from genetic optimization with propensity score matching to reduce bias in estimating treatment effects. It iteratively searches for the optimal weights for each covariate, reducing both the mean and maximum imbalance across covariates between treated and control groups. Genetic matching minimizes potential confounders more effectively than traditional methods by improving balance without relying on a specified functional form.

The use of genetic matching constructs a valid counterfactual composed of municipalities that are, on average, similar to border municipalities before the minimum wage reform. We apply the GM algorithm to the subset of 2,124 non-border municipalities. To avoid SUTVA violations, we also apply the matching algorithm to municipalities outside border states. We match on three sets of variables. First, we use weather variables to ensure that the climates are as similar as possible between treated and control groups. For this, we consider average mean, maximum, and minimum temperatures, along with precipitation and dew point temperature to account for humidity. Next, we include the proportion of people living in rural communities as a proxy for urbanization, the NASA global relative deprivation index for poverty, and indicator variables for marginalization constructed by CONEVAL. Finally, we incorporate the average mortality rate before treatment.

Figure A.6 displays the distribution of the main variables for the matched samples. The densities of temperatures, dew points, and precipitation in treated and control municipalities differ significantly because the treated group mainly comprises arid and semiarid municipalities. In contrast, most control municipalities are temperate, tropical, and subtropical, leading to lower temperatures, higher humidity, and increased precipitation in these regions. After applying the GM algorithm, the distribution of temperature variables becomes more similar. The CEM also improves the fit between the densities concerning the share of rural population, NASA GRDI, and the mortality rate.

Table A.8 presents point estimates for all workers and each occupation across the unmatched sample and for the three previously discussed matching specifications. The fourth matching specification mirrors the third, differing only by restricting controls to municipalities outside border states. Point estimates for the pooled model across specifications remain qualitatively similar to the unmatched specification, but they show slightly higher coefficients and greater statistical significance. The matching specifications now reveal statistically significant reductions for temperatures between -15 °C and 10 °C, as well as between 10 °C and 15 °C for all workers. For differences among specifications

Figure A.6: Density distribution of key variables between treated, control, and matched sub-samples



across occupations, the coldest temperature interval is not statistically significant for primary sector workers, elementary workers, and sales workers. These differences range from a decrease of 2.1% and 2.6%.

Table A.8: Regression results by occupation and matching specifications

Occupation	-15 to 10	10 to 15	15 to 20	25 to 30	30 to 40	N Obs	R2	Cor2	MRate
All Workers									
unmatched	-0.017	-0.008	-0.002	-0.020***	-0.028***	693672	1.049	0.249	5.421
	(0.012)	(0.005)	(0.005)	(0.005)	(0.008)				
match 1	-0.022*	-0.010*	-0.003	-0.017***	-0.028***	24408	0.998	0.281	5.421
	(0.012)	(0.005)	(0.005)	(0.005)	(0.007)				
match 2	-0.020*	-0.010*	-0.003	-0.019***	-0.028***	24276	0.998	0.258	5.421
	(0.012)	(0.005)	(0.005)	(0.005)	(0.007)				
match 3	-0.022*	-0.009*	-0.003	-0.017***	-0.027***	24300	0.998	0.272	5.421
	(0.011)	(0.005)	(0.005)	(0.005)	(0.007)				
match 4	-0.022*	-0.009*	-0.003	-0.018***	-0.026***	24448	0.997	0.314	5.421
	(0.011)	(0.005)	(0.005)	(0.005)	(0.008)				
Primary Sector									
unmatched	-0.018	-0.002	0.014	-0.017*	-0.022*	683574	0.860	0.534	13.620
	(0.012)	(0.010)	(0.009)	(0.010)	(0.012)				
match 1	-0.023*	-0.006	0.012	-0.016	-0.020*	22480	1.000	0.502	13.620
	(0.013)	(0.010)	(0.009)	(0.010)	(0.011)				
match 2	-0.023*	-0.006	0.013	-0.017*	-0.021*	22482	1.000	0.409	13.620
	(0.013)	(0.010)	(0.010)	(0.010)	(0.011)				
match 3	-0.023*	-0.005	0.013	-0.016	-0.020*	22423	1.000	0.620	13.620
	(0.013)	(0.010)	(0.010)	(0.010)	(0.011)				
match 4	-0.026**	-0.006	0.012	-0.016	-0.019*	23487	1.000	0.409	13.620
	(0.013)	(0.010)	(0.009)	(0.010)	(0.011)				
Elementary and Artisanal Workers									
Unmatched	-0.017	-0.011**	-0.003	-0.018***	-0.021***	502434	1.082	0.367	3.282
	(0.012)	(0.005)	(0.005)	(0.005)	(0.007)				
Match 1	-0.023*	-0.013***	-0.004	-0.015***	-0.022***	22040	0.998	0.266	3.282
	(0.012)	(0.005)	(0.005)	(0.006)	(0.007)				
Match 2	-0.020	-0.012**	-0.004	-0.016***	-0.022***	21111	0.998	0.258	3.282
	(0.013)	(0.005)	(0.005)	(0.006)	(0.007)				
Match 3	-0.023*	-0.011**	-0.004	-0.015***	-0.022***	21928	0.998	0.263	3.282
	(0.012)	(0.005)	(0.005)	(0.006)	(0.007)				
Match 4	-0.022*	-0.012**	-0.004	-0.016***	-0.021***	21857	0.997	0.313	3.282
	(0.012)	(0.005)	(0.005)	(0.006)	(0.007)				
Sales and Personal Services									
unmatched	-0.020	-0.008	0.000	-0.021***	-0.030***	412274	1.102	0.467	2.582
	(0.013)	(0.009)	(0.007)	(0.006)	(0.011)				
match 1	-0.024**	-0.010	-0.003	-0.017***	-0.030***	18090	0.996	0.299	2.582
	(0.012)	(0.009)	(0.007)	(0.006)	(0.010)				
match 2	-0.023*	-0.012	-0.004	-0.019***	-0.031***	17816	0.996	0.295	2.582
	(0.013)	(0.009)	(0.007)	(0.006)	(0.011)				
match 3	-0.023*	-0.011	-0.003	-0.017***	-0.029***	18584	0.996	0.258	2.582
	(0.013)	(0.009)	(0.007)	(0.006)	(0.010)				
match 4	-0.021*	-0.009	-0.002	-0.020***	-0.028**	18572	0.995	0.339	2.582
	(0.012)	(0.009)	(0.007)	(0.006)	(0.011)				
White Collar									
unmatched	-0.006	0.001	-0.002	-0.025***	-0.036***	353981	1.103	0.508	3.108
	(0.014)	(0.007)	(0.009)	(0.007)	(0.009)				
match 1	-0.010	0.001	-0.001	-0.021***	-0.036***	17915	0.998	0.383	3.108
	(0.014)	(0.006)	(0.009)	(0.007)	(0.009)				
match 2	-0.008	0.000	-0.003	-0.023***	-0.037***	17654	0.998	0.368	3.108
	(0.014)	(0.006)	(0.008)	(0.008)	(0.009)				
match 3	-0.012	0.001	-0.002	-0.019***	-0.034***	17952	0.998	0.374	3.108
	(0.014)	(0.006)	(0.008)	(0.007)	(0.009)				
match 4	-0.011	0.001	-0.002	-0.020***	-0.033***	17786	0.998	0.386	3.108
	(0.014)	(0.007)	(0.009)	(0.007)	(0.009)				

A.5 Robustness: COVID19 and INSABI

Table A.9: The 2019 tax and minimum wage reform on workers' temperature-related mortality (Controlling for potential confounders — DiDiT)

	Deaths per 10,000 people			
	Covid cases	Exposed uninsured population	Exposed uninsured population (per capita)	Social security ER
	(1)	(2)	(3)	(4)
$\mathbb{1}$(Border) ×				
(-15:10]	-0.0199**	-0.0190*	-0.0192*	-0.0177*
	(0.0099)	(0.0106)	(0.0106)	(0.0101)
(10:15]	-0.0103***	-0.0119***	-0.0102***	-0.0110***
	(0.0031)	(0.0032)	(0.0032)	(0.0030)
(15:20]	-0.0037	-0.0052	-0.0036	-0.0037
	(0.0044)	(0.0051)	(0.0050)	(0.0051)
(25:30]	-0.0111***	-0.0140***	-0.0132***	-0.0144***
	(0.0020)	(0.0022)	(0.0022)	(0.0023)
(30:40]	-0.0128*	-0.0192**	-0.0177**	-0.0188**
	(0.0075)	(0.0085)	(0.0085)	(0.0085)
Fitted Stat				
Observations	702,444	702,444	702,444	405,768
Mean Outcome	39.618	39.618	39.618	42.956
Fixed Effects				
Treatment-Municipality-Month	✓	✓	✓	✓
Treatment-Month-Year	✓	✓	✓	✓
Treatment-Municipality-Year	✓	✓	✓	✓
Controls				
Precipitation	✓	✓	✓	✓
COVID	✓	✓	✓	✓
Bins × Treated	✓	✓	✓	✓
Bins × Year	✓	✓	✓	✓
Bins × Additional Control	✓	✓	✓	✓

Notes: This table presents the point estimates from a Poisson Maximum Likelihood Estimator panel model of mortality rates per 10,000 people as a function of monthly temperature deviations. The coefficients refer to to the effect of one additional day per month within the pre-specified temperature interval. The reference temperature category is days within (20:25°C]. We present results for all mortality cases and the sub-sample of people dying while employed. Standard errors are clustered at the municipality level. Significance codes: $^{***} < 0.01, ^{**} < 0.05, ^{*} < 0.1$.

A.6 Robustness: mechanisms

Table A.10: Synthetic difference-in-differences (Alternative standard errors)

	SDID					
	Placebo (1)	Jackknife (2)	Bootstrap (3)	Placebo (4)	Jackknife (5)	Bootstrap (6)
Panel A: Air conditioning						
$\hat{\beta}$	0.001 (0.018)	0.001 (0.017)	0.001 (0.015)	-0.019 (0.017)	-0.019 (0.025)	-0.019 (0.015)
Mean Outcome	0.167	0.167	0.167	0.167	0.167	0.167
Panel B: Electric fan						
$\hat{\beta}$	0.001 (0.018)	0.001 (0.019)	0.001 (0.018)	-0.011 (0.021)	-0.011 (0.025)	-0.011 (0.023)
Mean Outcome	0.535	0.535	0.535	0.535	0.535	0.535
Panel C: Electric heater						
$\hat{\beta}$	0.039*** (0.007)	0.039** (0.015)	0.039*** (0.013)	0.033*** (0.010)	0.033* (0.018)	0.033** (0.017)
Mean Outcome	0.048	0.048	0.048	0.048	0.048	0.048
Panel D: Avg. Household electricity expenditure (pesos)						
$\hat{\beta}$	208.256*** (60.802)	208.256* (79.783)	208.256** (73.800)	143.987** (63.159)	143.987 (91.507)	143.987* (82.628)
Mean Outcome	878.984	878.984	878.984	878.984	878.984	878.984
Controls	No	No	No	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1496	1496	1496	1496	1496	1496

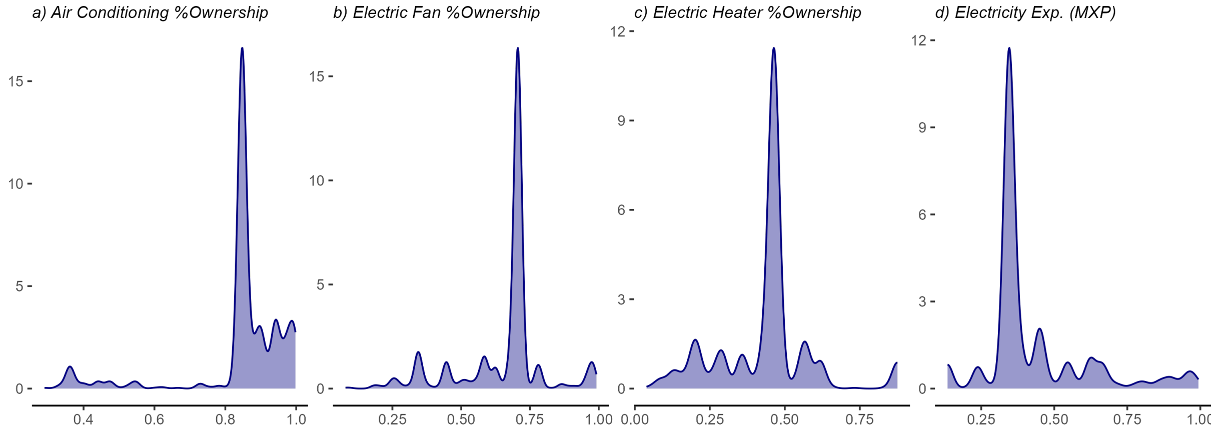
Notes: Jackknife is our preferred algorithm for computing standard errors. Bootstrap standard errors are obtained using 200 replications. The ENIGH sample is restricted to municipalities that are interviewed in all waves. Controls include household income, 24-deg cooling degree days, 15-degree heating degree days, share of households owning a house, share of households living in an urban area, share of female household head that are female, shares of household heads having completed primary, secondary and post-secondary education, and average age of the household head. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Synthetic difference-in-differences (Alternative time span)

	SDID			
	2010-2022	2016-2022	2010-2022	2016-2022
	(1)	(2)	(3)	(4)
Panel A: Air conditioning				
$\hat{\beta}$	0.010	-0.011	0.008	-0.009
	(0.017)	(0.025)	(0.017)	(0.028)
Mean Outcome	0.183	0.183	0.198	0.198
Panel B: Electric fan				
$\hat{\beta}$	0.003	0.001	-0.002	0.008
	(0.018)	(0.025)	(0.016)	(0.023)
Mean Outcome	0.511	0.511	0.520	0.520
Panel C: Electric heater				
$\hat{\beta}$	0.044***	0.041**	0.047***	0.044**
	(0.015)	(0.017)	(0.015)	(0.018)
Mean Outcome	0.045	0.045	0.043	0.043
Panel D: Avg. Household electricity expenditure (pesos)				
$\hat{\beta}$	190.385**	140.720*	206.911**	133.100*
	(80.970)	(88.637)	(82.292)	(82.962)
Mean Outcome	754.547	754.547	773.408	773.408
Controls	No	Yes	No	Yes
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1496	1496	1496	1496

Notes: The ENIGH sample is restricted to municipalities that are interviewed in all waves. Controls include household income, 24-deg cooling degree days, 15-degree heating degree days, share of households owning a house, share of households living in an urban area, share of female household head that are female, shares of household heads having completed primary, secondary and post-secondary education, and average age of the household head. Standard errors are computed using the jackniffe algorithm proposed by Arkhangelsky et al. (2021). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.7: Synthetic difference-in-differences (Placebo test)



Notes: Each panel shows the distribution of p-values from 1,000 replications of a placebo test conducted: (i) dropping the treated municipalities, (ii) randomly selecting nine control units and assigning them to the treatment group, and (iii) estimating the SDID estimator.

Figure A.8: Density distribution of power demand per household

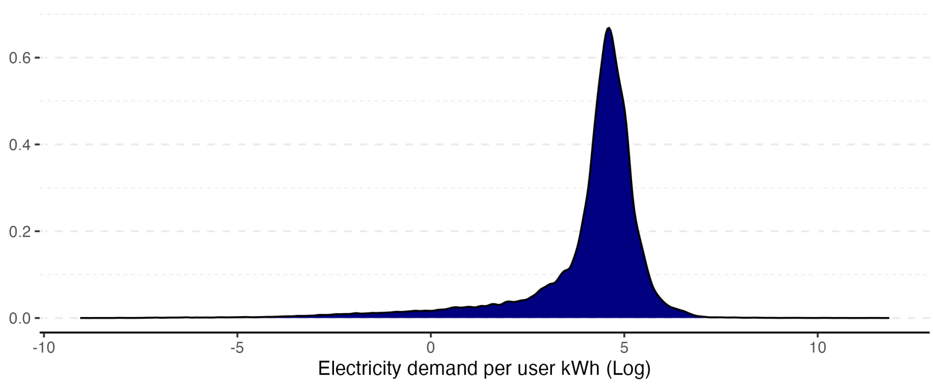


Table A.12: DiD on the ownership rates of energy appliances (Household, Full)

	DID					
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Panel A: Air conditioning						
$\hat{\beta}$	0.416***	0.138***	0.059*	0.055	0.118**	0.044
	(0.125)	(0.042)	(0.034)	(0.034)	(0.052)	(0.033)
Mean Outcome	0.138	0.138	0.138	0.138	0.138	0.138
R^2	0.022	0.316	0.407	0.416	0.350	0.428
Panel B: Electric Fan						
$\hat{\beta}$	0.249***	0.003	−0.036	−0.062**	0.007	−0.026
	(0.035)	(0.024)	(0.038)	(0.029)	(0.055)	(0.020)
Mean Outcome	0.493	0.493	0.493	0.493	0.493	0.493
R^2	0.004	0.275	0.353	0.355	0.327	0.370
Panel C: Electric heater						
$\hat{\beta}$	0.138***	0.079***	0.072**	0.096***	0.093***	0.083***
	(0.035)	(0.030)	(0.031)	(0.021)	(0.029)	(0.019)
Mean Outcome	0.035	0.035	0.035	0.035	0.035	0.035
R^2	0.009	0.105	0.144	0.154	0.110	0.159
Panel D: Electricity expenditure (pesos)						
$\hat{\beta}$	950.501***	304.534**	164.628	136.087	421.785**	124.390
	(317.592)	(141.607)	(121.561)	(119.571)	(177.569)	(117.710)
Mean Outcome	748.433	748.433	748.433	748.433	748.433	748.433
R^2	0.012	0.142	0.232	0.236	0.205	0.242
Controls			✓	✓	✓	✓
State FE		✓	✓	✓		
Year FE		✓	✓	✓	✓	✓
Municipality FE					✓	✓
State × Linear Trend				✓		✓

Notes: Controls include household income, 24-deg cooling and 15-deg heating degree days, dummy for urban household, age of the household head, gender of the household head, education level of the household head, and household size. Standard errors are clustered at the municipality level in parentheses. Regressions are conducted using survey weights. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table A.13: DiD on the ownership rates of energy appliances (Household, Balanced)

	DID					
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Panel A: Air conditioning						
$\hat{\beta}$	0.368**	0.117***	−0.001	−0.002	0.062	0.023
	(0.150)	(0.042)	(0.031)	(0.030)	(0.048)	(0.022)
Mean Outcome	0.180	0.180	0.180	0.180	0.180	0.180
R^2	0.021	0.343	0.445	0.454	0.416	0.468
Panel B: Electric fan						
$\hat{\beta}$	0.199***	0.015	−0.003	−0.040	0.021	−0.012
	(0.047)	(0.030)	(0.034)	(0.025)	(0.054)	(0.013)
Mean Outcome	0.537	0.537	0.537	0.537	0.537	0.537
R^2	0.004	0.308	0.339	0.340	0.315	0.353
Panel C: Electric heater						
$\hat{\beta}$	0.121***	0.056	0.041	0.068**	0.057	0.057**
	(0.040)	(0.037)	(0.036)	(0.027)	(0.036)	(0.024)
Mean Outcome	0.050	0.050	0.050	0.050	0.050	0.050
R^2	0.007	0.111	0.157	0.168	0.135	0.172
Panel D: Electricity expenditure (pesos)						
$\hat{\beta}$	730.414*	246.535	26.779	−12.333	275.816	4.669
	(368.528)	(154.222)	(104.197)	(128.517)	(189.407)	(118.134)
Mean Outcome	886.657	886.657	886.657	886.657	886.657	886.657
R^2	0.009	0.129	0.230	0.234	0.214	0.244
Controls			✓	✓	✓	✓
State FE		✓	✓	✓		
Year FE		✓	✓	✓	✓	✓
Municipality FE					✓	✓
State × Linear Trend				✓		✓

Notes: The sample is restricted to households living in municipalities that are sampled in all waves. Controls include household income, 24-deg cooling and 15-deg heating degree days, dummy for urban household, age of the household head, gender of the household head, education level of the household head, and household size. Standard errors are clustered at the municipality level in parentheses. Regressions are conducted using survey weights. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

A.7 Seguro Popular

The Mexican government introduced *Seguro Popular* in 2004 under the General Health Law to provide health insurance coverage to the uninsured. Policymakers designed the program to extend health services to those excluded from existing social security systems. *Seguro Popular* began as a pilot program between 2001 and 2003 and expanded throughout the country in 2004. In this section, we replicate the estimates of Cohen and Dechezleprêtre (2022) with a greater focus on occupational differences. We hypothesize that the introduction of *Seguro Popular* reduced the negative effects of temperature changes on worker mortality.

Like Cohen and Dechezleprêtre (2022), we define treatment using mortality records. After the first death certificate reports affiliation to *Seguro Popular*, we assign treatment status to the municipality. *Seguro Popular* rose from a handful of municipalities in 2004 to almost 90% of municipalities in 2008 (2,131 out of 2,457). The aggressive rollout of the program also significantly increased the number of insured persons. For example, despite age and population growth, the number of people without social security at the time of death decreased by 43 percentage points between 1998 and 2020.

We estimate the effect of *Seguro Popular* using a difference-in-difference design that exploits exogenous temperature variation within municipalities to identify the impact. Equation 9 presents our empirical strategy. In this context, M_{ct} denotes the mortality rate in municipality c at time t. λ_b^{sp} reflects the change in mortality for the temperature interval b resulting from the introduction of *Seguro Popular*. We derive λ_b^{sp} by interacting each temperature interval with a vector indicating if *Seguro Popular* is available for municipality c at time t. We control for unobservable factors with municipality-by-year, municipality-by-month, and month-by-year fixed effects. We hypothesize that λ_b^{sp} is negative for at least one temperature interval b, indicating that the introduction of *Seguro Popular* reduced temperature-related mortality for workers.

$$M_{ct} = \exp\left\{\sum_{b=0}^{6} \lambda_b D_{bct} + \sum_{b=0}^{6} \lambda_b^{sp}\Big[D_{bct} \times \mathbb{1}(SP=1)_{ct}\Big] + \gamma X_{ct} + \delta_{cy} + \delta_{cm} + \delta_{ym}\right\} + \epsilon_{ct} \tag{9}$$

Table A.14 presents the results on the effect of *Seguro Popular* on temperature-related worker mortality. Following Cohen and Dechezleprêtre (2022), we restricted the sample to workers without traditional social security who died from diseases covered by *Seguro Popular*. The results show that *Seguro Popular* decreased mortality. This reduction is particularly significant at the extremes of the temperature distribution. For instance, our econometric design suggests a 0.723% reduction in the mortality rate for days between -15 °C and 10 °C compared to days without *Seguro Popular*. This finding implies that *Seguro Popular* reduced temperature-related mortality. The results are negative and statistically significant in other temperature intervals, except for days between 25 °C and 30 °C.

Table A.14: The effect of *Seguro Popular* on temperature-related mortality

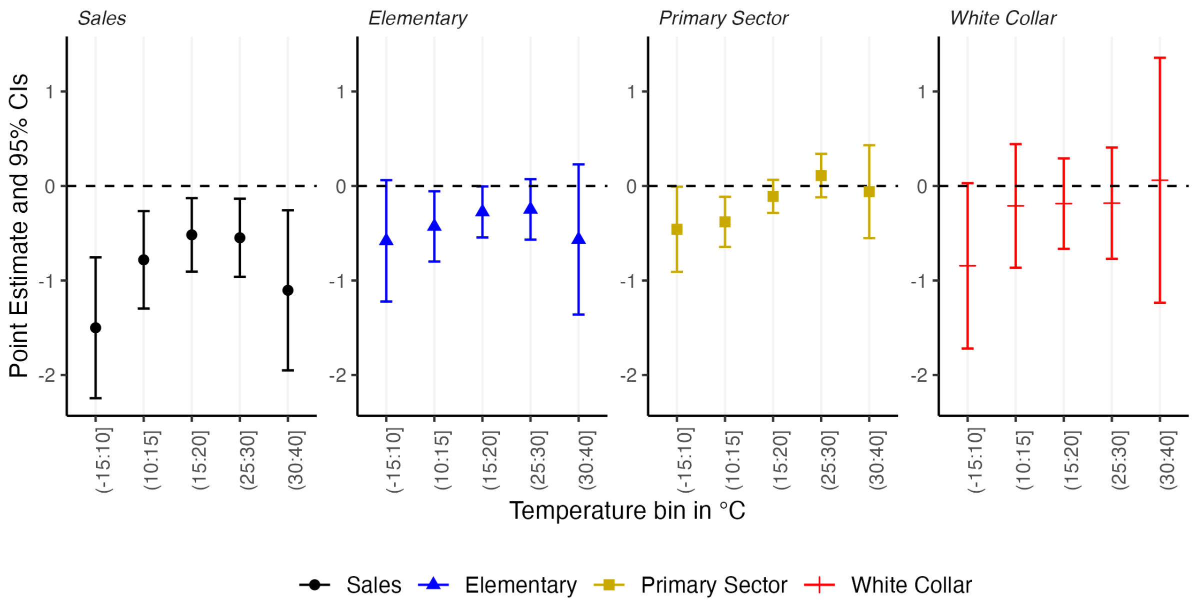
Temperature bins	(-15:10]	(10:15]	(15:20]	(25:30]	(30:40]
λ_b^{sp}	-0.723***	-0.531***	-0.182***	0.0390	-0.430***
	(0.152)	(0.079)	(0.055)	(0.062)	(0.134)
Fitted Stats					
R^2	0.826	0.826	0.826	0.826	0.826
# Obs	687389	687389	687389	687389	687389
# Counties	2457	2457	2457	2457	2457
# Periods	288	288	288	288	288
Interpretation of Results					
Mort. Rate (Pre-Treatment)	2.5	2.5	2.5	2.5	2.5
Average Days in each Interval	0.71	5.07	11.54	4.36	0.43

Notes: This table displays the effect of the introduction of *Seguro Popular* on the consequences of an additional day with average daily air temperatures within the specified temperature range related to the reference category (20-25] °C for workers without access to traditional social security. To simplify the interpretation of the coefficients, we transform the value of λ_b^{sp} into $[exp(\lambda_b^{sp}) - 1] \times 100$. Interpret λ_b^{sp} as the percentage change in the mortality rate because of the introduction of *Seguro Popular* due to an additional day per month outside of the thermal comfort point. The econometric model estimates the effect of temperature on mortality with a PPMLE panel model with fixed effects for the municipality-month, municipality-year, and month-year of observation along with controls for precipitation and COVID. Standard errors clustered at the municipality level. Significance codes: *** <0.01, ** < 0.05, *<0.1.

These results align with Cohen and Dechezleprêtre (2022), but show higher statistical significance. Specifically, Seguro Popular reduced mortality among treated workers across all temperature intervals, except for the 25-30 °C bin. In contrast, Cohen and Dechezleprêtre (2022) find that reduction mainly occurred in the 12-16 °C bin. These differences may arise from using two distinct estimators, our focus on the working population, or our larger sample size.

Figure A.9 presents the results of the effect of *Seguro Popular* on each labor group. Point estimates indicate that the policy significantly reduced temperature-related mortality in at least one temperature interval for each labor group. For sales workers, the coefficients are statistically significant across the temperature distribution. They range from a highly relevant 1.5% reduction for days between -15 °C and 10 °C to a 0.25% decrease on days between 15 °C and 20 °C. For elementary workers, the coefficients are smaller than for sales workers but still significant or borderline significant for cold temperatures. For primary sector workers, we observe statistically significant reductions in mortality only for temperatures below 15 °C. For white-collar workers, we find borderline significance for days below 10 °C.

Figure A.9: Effects of *Seguro Popular* on temperature-related mortality across occupations



Notes: Effect of the introduction of *Seguro Popular* on the mortality consequences of an additional day with average daily air temperatures within the matchified temperature range concerning days between 20° and 15°. To simplify the interpretation of the coefficients, we transform the value of λ_b^{sp} into $[exp(\lambda_b^{sp}) - 1] \times 100$. Interpret λ_b^{sp} as the percentage change in the mortality rate due to the introduction of *Seguro Popular* because of an additional day per month outside of the thermal comfort point. We estimate the effect separately for each occupation and for the samples of workers without access to traditional social security before the introduction of *Seguro Popular*. The econometric model estimates the effect of temperature on mortality with a PPMLE panel model with fixed effects for the municipality-month, municipality-year, and month-year of observation along with controls for precipitation and COVID. Standard errors are clustered at the municipality level.

Although we do not find statistical evidence of differing effects across occupations, we explore how the share of insured individuals before treatment and access to health facilities may reduce the benefits of *Seguro Popular*, potentially leading to heterogeneous treatment effects. The premise is that increased access to healthcare provides fewer benefits if the share of insured persons before treatment is already high, or if workers lack access to health facilities even after the the program's implementation.

We test the mitigating effect of access to healthcare facilities before treatment by adding an interaction term that incorporates the number of days outside thermal comfort, the share of insured individuals before treatment in county c, and labor group i, along with the treatment indicator. That is, $\lambda^{sp}_{Share}\Big[D_{ct} \times Share_{ci} \times \mathbb{1}(SP = 1)_{ct}\Big]$. To reduce the number of coefficients, we consolidate all temperature intervals into a single variable (D_{ct}), that captures the number of days outside thermal comfort. A positive coefficient for λ^{sp}_{share} indicates that the pretreatment share of insured persons reduces the mortality rate due to *Seguro Popular*.

Table A.15 presents the results of the estimate of λ^{sp}_{share} for each labor group. While we cannot find statistically significant coefficients for primary sector workers, probably because there is too little variation in the pretreatment share of insured persons, we do find statistically significant coefficients when looking at elementary, sales, and white-collar workers. We can interpret the coefficients as the effect of increasing the share of insured persons before treatment by ten units on the reduction in mortality associated with the introduction of *Seguro Popular*. These estimates suggest that increasing the pretreatment share of insured persons leads to a decrease in the mortality reduction related to *Seguro Popular* for elementary, sales, and white collar workers. These findings align with Cohen and Dechezleprêtre (2022) who find that the expansion of mandatory health coverage in Mexico reduced the mortality effects of days outside the thermal comfort range, particularly impacting low-income individuals more likely to be uninsured before the policy. In our framework, the point estimates are higher for sales and elementary workers than for white-collar workers, as they were also more likely to be uninsured before the policy.

Table A.15: The mitigating effect of pretreatment access to social security on the effects of Seguro Popular on temperature-related mortality

	Primary Sector	Elementary	Sales	White Collar
λ^{sp}_{Share}	0.010	0.15**	0.26**	0.30**
	(0.04)	(0.06)	(0.1)	(0.14)
Fitted Stats				
R^2	0.96	0.94	0.96	0.96
Average D_{ct}	22.27	22.27	22.27	22.27
# Obs	675359	470129	378815	319088
# Municipalities	2457	2457	2457	2457
# Periods	288	288	288	288

Notes: Mitigating effect of the pre-treatment share of insured persons on the consequences of an additional day with average daily air temperatures outside thermal comfort after the introduction of *Seguro Popular*. To simplify the interpretation of the coefficients, we transform the value of λ^{sp}_{Share} into $[exp(\lambda^{sp}_{Share}) - 1] \times 100 \times 10$. Interpret the coefficients as the effect of increasing the share of insured persons before treatment by ten units on the reduction of temperature-related mortality related to the introduction of *Seguro Popular*. The econometric model estimates the effect of temperature on mortality with a PPMLE panel model with fixed effects for the municipality-month, municipality-year, and month-year of observation along with controls for precipitation and COVID.. Standard errors clustered at the municipality level. Significance codes: *** <0.01, ** < 0.05, * < 0.1.

To examine the mitigating effects of accessibility to healthcare facilities, we use data from the Mexican Health Secretariat (Secretaria de Salud) on the location of all hospitals and clinics in the country in 2020. We calculate the number of hospital beds per capita within a 20 km radius around the reported place of death. Using these data, we analyze whether differences in accessibility between occupations can diminish the positive effects of *Seguro Popular* on temperature-related mortality. As expected, the number of beds per capita near primary sector workers is lower than for other occupations. Sales, elementary, and white-collar workers have access to 0.66, 1.2, and 1.4 more beds per 10,000 inhabitants than workers in the primary sector.

In Table A.16, we examine how access to health facilities influences the effect of *Seguro Popular* by specifying an interaction term that incorporates the average number of beds per capita in each county and occupation, the number of days outside thermal comfort, and the treatment indicator. That is, $\lambda^{sp}_{access}[D_{ct} \times 1(SP = 1)_{ct} \times Beds_{ct}]$. The results indicate that the reduction in mortality due to the introduction of *Seguro Popular* increases with the number of beds per capita. This finding holds for elementary, sales, and white-collar workers. However, we cannot infer statistically significant effects at conventional levels for primary sector workers due to minimal variation in the number of beds per capita within this labor group.

Table A.16: The consequences of access to health facilities on the effects of Seguro Popular on temperature-related mortality

	Primary Sector	Elementary	Sales	White Collar
λ^{sp}_{access}	0.04	-0.12*	-0.25***	-0.19**
	(0.03)	(0.06)	(0.07)	(0.08)
Fitted Stats				
R^2	0.52	0.79	0.77	0.79
Average D_{ct}	22.27	22.27	22.27	22.27
# Obs	649038	456812	371011	313985
# Municipalities	2457	2457	2457	2457
# Periods	288	288	288	288

Notes: Mitigating effect of access to health facilities on the consequences of an additional day with average daily air temperatures outside thermal comfort after the introduction of *Seguro Popular*. To simplify the interpretation of the coefficients, we transform the value of λ^{sp}_{access} into $[exp(\lambda^{sp}_{access}) - 1] \times 100 \times 10$. Interpret the coefficients as the effect of increasing the share of insured persons before treatment by ten units on the reduction of temperature-related mortality related to the introduction of *Seguro Popular*. The econometric model estimates the effect of temperature on mortality with a PPMLE panel model with fixed effects for the municipality-month, municipality-year, and month-year of observation along with controls for precipitation and COVID. Standard errors clustered at the municipality level. Significance codes: *** <0.01, ** < 0.05, * < 0.1.