

Heat and mortality by age group in Brazil, 2014–2024: a distributed lag non-linear time-series study of 11 million deaths

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Abstract

Background. This study quantifies, in 11 million deaths from 14 Brazilian federative units, how much the acute effect of extreme heat on mortality increases with age: the cumulative excess over the first week is about three times larger at 85 years or older than below 65. Age is the most frequently cited modifier of the relationship between heat and mortality, but two questions remain poorly resolved in the Brazilian literature: the magnitude of the age gradient across multiple Brazilian federative units and the proportion of the excess that corresponds to **mortality displacement** — deaths merely brought forward by a few days, rather than additional deaths.

Objective. To estimate the risk of death from natural causes associated with heat in four age groups and to determine whether the excess observed among older adults persists after three weeks.

Methods. We analysed **11,038,656 deaths from natural causes** recorded in the Mortality Information System in 14 federative units, 2014 to 2024 (56,252 unit-days). Exposure was the daily maximum temperature measured at **373 automatic stations** of the National Institute of Meteorology (INMET), after quality control and a minimum requirement of hourly completeness. The main model was a quasi-Poisson **distributed lag non-linear model** (DLNM) for each federative unit, with lags of 0 to 21 days, a natural spline of time with 8 degrees of freedom per year, and day of the week, pooled by random effects (τ^2 by REML, Hartung–Knapp intervals). The pre-specified contrast was the 99th versus the 75th percentile of each unit's own temperature distribution. As confirmation, we repeated the analysis with a **time-stratified case-crossover** design on an expanded and partially overlapping dataset (17 federative units, all months, 2014–2025), under two exposure definitions — INMET heat-event windows and the top decile of each month's own maximum temperature.

Results. The cumulative relative risk over lags 0 to 7 rose monotonically with age: **1.119** (95% CI 1.077–1.162) below 65 years, **1.170** (1.126–1.216) at 65 to 74, **1.237** (1.173–1.303) at 75 to 84 and **1.329** (1.235–1.431) at 85 years or older. The lag curve peaked on the hot day itself, also increasing with age (1.047; 1.062; 1.062 and 1.085), and decayed slowly, remaining above unity for longer at older ages. **We found no evidence of a compensatory deficit in any age group:** the cumulative risk over lags 8 to 21 was 1.013 (0.970–1.058) below 65 years and **1.079** (1.037–1.122) at 85 or older, so that the 21-day risk exceeded the 7-day risk — 1.440 (1.301–1.594) versus 1.329 at 85 years or older. The absence of a **statistically detectable** deficit was robust to every specification tested. In the main analysis, an excess between days 8 and 21 was also observed in the groups aged 75 or older; **this finding is exploratory** and was sensitive to the specification of seasonal control, losing significance under year $\times$ month indicators. In the formal contrast between age groups, computed within each federative unit and pooled by cluster bootstrap, the ratio between the risks at 85 or older and below 65 was **1.160** (1.073–1.229) over lags 0 to 7. The case-crossover analysis reproduced the same ordering (1.011; 1.016; 1.025 and 1.035), and the INMET event windows supported it above 75 years (1.038; 1.019–1.058).

Conclusion. The primary finding is the **age gradient in the acute effect of extreme heat**, which is large and reproducible across all designs, scales and specifications tested. Second, **we found no statistically**

significant evidence of compensatory mortality reduction within the 21-day observation window, under any specification – which does not exclude partial displacement or compensation beyond the observed period. The excess between days 8 and 21 is an exploratory finding. The results support prioritising the population aged 75 years or older in extreme-heat response plans.

1. Introduction

Consolidated data show that the climate system is in a state of persistent anomaly (WMO 2024; IPCC 2023), shaping a scenario that points to a rapidly accelerating global health crisis (Romanello et al. 2025).

In this context, extreme heat episodes stand out as one of the deadliest and, paradoxically, most underestimated climate phenomena of our time. From a physical and dynamic standpoint, they are driven by synoptic-scale atmospheric processes and by land–atmosphere interactions. Among their main mechanisms is atmospheric blocking, which interrupts the zonal flow and favours the persistence of high-pressure ridges, contributing to the stagnation of warm air masses over certain regions (McGregor 2024).

The impacts of extreme heat are multidimensional. In human health, these events are associated with direct mortality, exacerbation of cardiovascular and respiratory diseases, and a range of indirect effects. The 2025 Lancet Countdown report estimates around 546,000 heat-related deaths per year and points to marked increases in the exposure of older people (Romanello et al. 2025). Clinically, prolonged exposure to extreme heat imposes a significant additional load on the cardiovascular system, while renal function is threatened by dehydration and fluid redistribution (Gagnon et al. 2026). In settings of urban poverty, continuous indoor heat exposure, including at night, can impair physiological recovery and increase the risk of potentially fatal heat exhaustion (Kim et al. 2012). Biological vulnerability is often compounded by socioeconomic inequalities, which unevenly shape the capacity for protection and adaptation in the face of extreme heat (Neves et al. 2025; Sim & Parsons 2025).

At the same time, humanity is going through a historic demographic milestone, characterised by increasing longevity and marked growth of the older population. Although this longevity is an important achievement of civilisation, it simultaneously imposes an urgent need to restructure societies and systems to accommodate population ageing in a sustainable and dignified way (WHO 2021). In this context, the vulnerability of older people to the impacts of climate change, particularly to extreme heat events, is multidimensional.

This vulnerability involves physiological aspects intrinsic to ageing, such as the progressive decline in thermoregulatory capacity and the higher prevalence of chronic comorbidities, which mark a reduction in functional reserve and in resilience to stressors (Abraham et al. 2023). Added to this is an impaired perception of thermal risk resulting from declining peripheral thermal sensitivity, which may lead older people to prefer warmer environments (Panet et al. 2020).

These physiological and clinical conditions are overlaid by social and economic vulnerability, expressed in a greater likelihood of social isolation, dependence on care and limited access to adaptation resources (WHO 2025). In Brazil this dimension is particularly relevant, and we return to it in the Discussion with national data on household infrastructure and loneliness. **It should be stated from the outset, however, that social conditions are not the object of investigation in this study:** we have no individual information on living arrangements, income or support networks, and none of the estimates reported here measures them.

The association between high temperature and mortality has been established for two decades (Basu & Samet 2002; Gasparrini et al. 2015), and older age consistently ranks among the effect modifiers (Benmarhnia et al. 2015; Son et al. 2019). What remains less settled is the **comparative magnitude** of this gradient in national datasets from middle-income countries and the behaviour of the lag curve in each age group. The classic investigation of heat-related mortality displacement, which included São Paulo, found different patterns across cities (Hajat et al. 2005), and heterogeneity across settings has been the rule ever since.

Brazil offers favourable conditions for this investigation for three reasons. First, the Mortality Information System (SIM) is the national mortality register and records the date and municipality of every notified death, which allows population-wide analysis without sampling — subject to the historical and regional differences in the system's own coverage. Second, the network of automatic stations of the National Institute of Meteorology (INMET) provides hourly instrumental observations, from which we built the daily maxima. Third, since 2024 the country has accumulated a series of **heat episodes documented in INMET technical notes**, which makes it possible to contrast an exposure definition derived from an official meteorological source with a statistical definition — a resource rarely available in the literature, where exposure is almost always defined by percentile alone.

The aim of this study is to examine how the risk of death associated with extreme heat is distributed across age groups in Brazil and, above all, **whether the excess observed in the first days is followed by a compensatory reduction in mortality in the subsequent weeks**. The distinction matters for what a response plan can promise: if the initial excess is offset by a deficit a few days later, the pattern is compatible with the advancement of deaths that would have occurred shortly anyway; if it is not, the interpretation of mere advancement loses support. The design measures the presence or absence of such compensation within the observed horizon — it does not estimate the fraction of deaths brought forward, a quantity that would require knowing, for each death, when it would otherwise have occurred. A note on scope is warranted: **the primary exposure in this study is daily maximum temperature, analysed with the DLNM using the pre-specified contrast between the 99th and 75th percentiles of each federative unit's temperature distribution, not the heatwave as a declared episode**; month-specific percentiles and the INMET event windows are used only in the confirmatory case-crossover analysis.

2. Methods

2.1 Design

We adopted a **daily time-series study** with a distributed lag non-linear model (DLNM) as the main analysis, and a **time-stratified case-crossover** design as a confirmatory analysis on an expanded dataset. The two analyses share most of the deaths and temperatures; they are distinct analytical designs applied to overlapping data, not independent samples.

The choice of the DLNM as the main analysis stems from a limitation we document in section 3.6: the case-crossover with monthly strata does not resolve lags beyond about one week, because the structure of the stratum itself limits the observable window.

2.2 Mortality

The main dataset comprises the **14 federative units whose SIM files were available for every year from 2014 to 2024** — Alagoas, Bahia, Ceará, Espírito Santo, Minas Gerais, Mato Grosso do Sul, Piauí, Paraná, Rio de Janeiro, Rio Grande do Norte, Roraima, Rio Grande do Sul, Santa Catarina and São Paulo. Paraíba, Pernambuco and Sergipe were excluded because only the 2024 file was available in the collection used — a restriction of the local collection, not of the national system. The year 2025 was excluded because its data are preliminary: under-registration at the end of the series would distort the time spline. The dataset contains **56,252 unit-days and 11,048,873 deaths from natural causes**, of which **11,038,656 enter the age-stratified analyses** (see below; the full flow of denominators is given in Supplementary Table S1).

Natural causes correspond to an underlying cause in **A00–R99**. The operational rule applied was the exclusion of underlying causes beginning with V, W, X or Y (chapter XX); we verified that the dataset contains no underlying causes beginning with U or with any letter after R outside chapter XX, so the two definitions coincide exactly.

Age was taken from the IDADE field of the death certificate, decoded according to the SIM convention (the first digit indicates the unit: 0 to 3 for minutes, hours, days and months; 4 for years; 5 for 100 years or more). The age groups — under 65, 65 to 74, 75 to 84, and 85 years or older — were fixed before the analysis and cover the entire age range, from birth onwards. **Deaths with missing or invalid age — 10,217, or 0.092% of natural deaths — were excluded** from the age-stratified analyses. Unknown age is not equivalent to age below 65 years, and in a study whose question is precisely effect modification by age, exclusion is the cleanest approach.

Each age group has its own model. Deaths are disjoint across groups, but the series share days, temperature and temporal structure, so **we did not assume zero covariance between the group-specific estimators**. The contrast between groups is estimated within each federative unit — $d = \log \text{RR}(\text{group A}) - \log \text{RR}(\text{group B})$, on the same series and the same days — and then pooled in three ways. The first is a **REML meta-analysis of the d values**, which uses $\text{Var}(d) = \text{Var}(A) + \text{Var}(B)$ and therefore assumes independence. The second is a **cluster bootstrap over the 14 federative units** (5,000 resamples of the 14

units with replacement, percentile interval – 2.5th and 97.5th –, seed 20260906), which does not require specifying the covariance between the two estimators to compute between-unit variability, although it treats the estimated unit-specific contrasts as observed. The third is **direct estimation of the covariance**: the two age series of each federative unit are stacked in a single model with full interaction between age group and all terms – which exactly reproduces the coefficients of the two separate models – and the covariance matrix is obtained with a **sandwich estimator clustered by day**, which captures the residual correlation between age groups on the same day. The three approaches are reported side by side (Table 4).

2.3 Heat exposure

From hourly readings to the station's daily maximum. The INMET hourly files label the time column as HORA (UTC), and were converted to standard local time (UTC – 4 in Mato Grosso do Sul and Roraima, UTC – 3 elsewhere). Because the period begins in 2014, when daylight saving time was still in force in part of the country, we empirically checked whether the records might be in local civil time instead of UTC: we compared the local time of the daily temperature peak between the summers of 2015–2018, when daylight saving time was in force, and those of 2021–2024, after it was abolished, at three stations – São Paulo and Porto Alegre, which observed daylight saving time, and Fortaleza, which never did. Had the records been in civil time, the first two would show a peak about one hour later in the first period and the third no difference. The observed differences were **+ 0.38 h in São Paulo, – 0.17 h in Porto Alegre and + 0.15 h in Fortaleza**: they do not show the one-hour shift that would be expected if the records represented local civil time, and are **compatible with interpreting the field as UTC throughout the series**. The full procedure is given in Appendix B. A station's daily maximum is the **highest value of the variable "maximum temperature in the previous hour"** across the valid hours of the day.

Quality control. The rule was developed by the authors after we identified, while analysing a state record, a reading of 41.9°C at an hour when the dry-bulb temperature was 26.6°C and the previous hour's minimum was 25.2°C – a sensor spike with no echo in any neighbouring hour. The resulting pre-specified rule: **an hourly maximum reading is accepted only if it does not exceed by more than 6°C the highest dry-bulb value between that hour and the previous one**. The 6°C threshold was chosen empirically, as it exceeds the largest plausible hourly difference between maximum and dry-bulb temperature under intense radiation; no normative reference supports it. Within the scope of the study, the rule **rejected 667 of 28,976,055 hourly readings (0.0023%)**, changing the state mean on 154 of the 56,252 unit-days, with a maximum difference of 1.08°C. We ran the analysis with and without the filter (Table 10).

Intraday completeness requirement. A station-day enters the calculation only if it has **at least 18 of the 24 hourly observations**. Within the scope of the study – 373 stations, 14 federative units, 2014 to 2024 – there are **1,397,979 observed station-days**, 93.3% of the theoretical maximum of 1,498,714, and **1,182,593 (84.6%) meet the 18-hour criterion**.

The intermediate station file contains 3,831 repeated station–date pairs, all on 31 December: they are an artefact of the conversion from UTC to local time, which carries the first hours of the following year's file into the 31st. They are inert in the analysis — in none of the 3,831 cases do both rows pass the 18-hour filter (3,218 have one passing, 613 none), so the state mean never counted the same station twice.

State aggregation and missing data. The daily exposure of each federative unit is the **unweighted mean of the maxima of its valid stations on that day**. Each station receives equal weight, regardless of the population it represents — a choice we discuss as a limitation in section 4.2. Two counts must be distinguished: **unique stations in operation during the period** and the **median number of valid stations per day**. The first totals 373 across the 14 units and ranges from 1 (Roraima) to 67 (Minas Gerais); the second, which is what actually makes up each day's mean, ranges from 1 to 60 (Table 1). Under either reading, "state exposure" does not denote the same construct in every unit.

1,306 of the 56,252 unit-days (2.32%) have no valid station, and the missingness is concentrated in two units: Roraima (961 days, 24% of its series, with a single station) and Rio Grande do Norte (345 days). These days were filled by **linear interpolation** within the unit before fitting, because the cross-basis propagates any missing value to the following 21 days. For these two combined reasons, Roraima is the least reliable unit in the series.

The effect contrast, declared before the analysis, is the **99th versus the 75th percentile of each unit's own temperature distribution**. The reference at the 75th percentile was chosen to estimate the increment associated with extreme heat **within the warm part of the distribution**, not to compare against the minimum mortality temperature: the relative risks reported here should not be read as "the total effect of extreme heat versus optimal thermal conditions".

This is a **percentile-based** contrast, not an absolute one: in degrees Celsius it ranges from 2.1°C in Ceará to 6.1°C in Rio Grande do Sul (Table 1), so the pooled estimate combines different thermal doses. We therefore repeated the analysis with a **common absolute contrast — 3°C above each unit's 75th percentile** (Table 11).

2.4 Main model

For each federative unit and each age group we fitted a **quasi-Poisson** generalised linear model, which estimates overdispersion freely:

deaths ~ cross-basis(Tmax, lag 0–21) + ns(time, 8 df/year) + factor(day of week)

The degrees of freedom of the time spline are computed as $8 \times$ the number of years in the series, that is, **88 degrees of freedom** over the 11 years of each unit.

The **cross-basis** is the tensor product of a natural cubic spline of temperature, with knots at the 50th, 75th and 90th percentiles of each unit, and a natural cubic spline of lag, with three internal knots at logarithmically equal spacing (Gasparrini et al. 2010). The time spline controls for seasonality and long-

term trend, while the cross-basis estimates the temperature–mortality association distributed across lags; day of the week enters as a factor.

Reduction and pooling. From each model we extracted the vector of cross-basis coefficients and its covariance matrix. For a lag window L , the log relative risk is obtained as the linear combination $u'\beta$, in which u sums, over the lags in L , the product of the difference between the temperature bases at P99 and P75 and the lag basis; the variance is $u'Vu$. There was no multivariate reduction of the cross-basis: each quantity of interest – cumulative risk over 0–7, 8–21, 18–21 and 0–21, and the risk at each individual lag – was computed by federative unit and pooled **univariately** by age group. Pooling uses **random effects with τ^2 estimated by REML**, and we report the confidence interval by the **Hartung–Knapp** method, which is more conservative with a small number of units; the DerSimonian–Laird estimate enters as a sensitivity analysis (Table 4).

Validation of the implementation. The cross-basis was built with the base R package splines, reproducing the algebra of the dlnm package. Our own implementation was **validated against the *dlnm* package, version 2.4.10**, by fitting the 56 models (14 units $\times$ 4 age groups) in both versions and comparing coefficients, cumulative risks and lag-specific risks: the largest relative difference between relative risks was 1.3×10^{-14} , that is, machine precision.

The full record of the validation procedure is provided in the supplementary material.

2.5 Confirmatory analysis: case-crossover

The case-crossover analysis uses an expanded dataset: **17 federative units, all months from 2014 to 2025, 10,160,394 natural deaths** within the strata.

In the **time-stratified case-crossover** design, each death is a case and the day of death is the case day; the referent days are the other days of the same month, in the same federative unit and on the same day of the week. The exposure on each day – case or referent – is the binary hot-day indicator. The relative risk compares, within each stratum, the odds of the death falling on an exposed versus an unexposed day. It is not the hot day that defines the case; it is the death.

Estimator. Because all deaths on the same day share the same exposure and the same stratum, we fitted the **conditional Poisson likelihood on daily counts**, which is algebraically equivalent to conditional logistic regression on individual cases in this design (Armstrong et al. 2014) and computationally much lighter. The stratum is the combination **federative unit $\times$ year $\times$ month $\times$ day of the week**; only strata with at least one exposed day, one unexposed day and one death contribute. Confidence intervals come from the Fisher information of the conditional likelihood, and the p value from a score test under the null hypothesis.

Definition of an exposed day. Exposure is a **binary indicator**: a day is exposed if it is a hot day or one of the three days following a hot day. "Lag 0 to 3" therefore denotes the extent of the window, not four covariates nor a moving average. We ran two definitions of a hot day:

- **INMET event windows:** built from the institute's technical notes for the

eight episodes between January 2024 and March 2025. Where the note declares an interval, the interval is used; where it only names a record day, a symmetric window of ± 2 days around it is applied (Appendix). Some of the days so classified, therefore, **were not formally declared by the institute** – the window is derived from the notes, not identical to them;

- **Meteorological window:** top decile of each unit's own maximum temperature in each month, applied to the entire

series.

Municipal scale. We repeated the analysis assigning each municipality the INMET station nearest to its municipal seat. The coordinates of the seats came from the IBGE register of localities (BR_Localidades_2010_v1) and those of the stations from the headers of the INMET files themselves; distances were computed with the Haversine formula. The median distance to the nearest station is **33 km** (P75 = 48 km; 78% of municipalities within 50 km, 99% within 100 km). With a 100 km radius, **4,512 municipalities and 98.6% of deaths** are included; municipalities with no station within the radius are left out. The same station may represent several municipalities, and the stratum becomes **municipality $\times$ year $\times$ month $\times$ day of the week**, with the top decile computed on the municipality's own series.

2.6 Internal controls and bias tests

Three safeguards, declared before running the analyses:

1. **Negative control outcome.** Congenital malformation (ICD-10 Q00–Q99) as the underlying cause is an outcome whose trajectory is established before birth and for which **we expect a substantially smaller or null acute association** with temperature. We do not assume biological impossibility: a severe congenital heart defect may, in principle, have its acute risk of death altered by heat.
2. **Placebo test.** We drew 200 sets of **placebo exposure windows**, preserving the number and temporal structure of the real exposure blocks, positioned at random within the month and without reference to temperature, to measure the positional bias of the case-crossover design. The real estimate is compared with the distribution of the 200 placebo estimates by a z score based on the empirical standard deviation of that distribution.
3. **Temperature at each lag.** We report the mean temperature percentile of the exposed day at each lag, because a lagged estimate cannot be read without knowing how hot that day was.

Data extraction used Python 3.10 (datasus_dbc, dbfread) and modelling used R 4.6.0 with the splines package from the base distribution. Code and aggregated data are available for reproduction on Zenodo (doi:10.5281/zenodo.23421154).

3. Results

3.1 Dataset and exposure

Table 1. Federative units, stations, coverage and temperature contrast, 2014–2024.

FU	Unique stations in the period	Median valid per day	FU-days without a station	P50 (°C)	P75 (°C)	P99 (°C)	P99–P75
AL	7	6	0	31.8	33.5	36.5	3.0
BA	48	38	0	31.1	32.5	35.4	2.9
CE	16	12	0	33.3	34.9	37.0	2.1
ES	13	11	0	29.4	31.8	35.4	3.6
MG	67	60	0	29.2	30.9	35.1	4.2
MS	28	25	0	31.6	33.5	38.7	5.2
PI	23	17	0	34.0	35.9	39.2	3.3
PR	24	19	0	27.2	29.8	34.6	4.8
RJ	27	22	0	28.0	30.6	35.0	4.4
RN	9	6	345	33.1	34.0	37.6	3.7
RR	1	1	961	33.9	35.4	37.9	2.9
RS	44	40	0	24.9	28.4	34.4	6.1
SC	24	20	0	24.1	27.0	31.8	4.8
SP	42	31	0	29.0	31.2	35.9	4.7

The 14 federative units total **373 unique stations** in operation between 2014 and 2024; the daily median column shows how many actually make up each day's mean, after quality control and the requirement of 18 valid hours. The P99–P75 contrast varies by a factor of almost three across units. The pooled estimate therefore describes the effect of the **same percentile jump**, not of the same jump in degrees — a distinction that probably contributes to the observed heterogeneity.

3.2 The age gradient is large over lags 0 to 7

Table 2. Cumulative relative risk of death from natural causes, 99th versus 75th percentile of each unit's own temperature, 14 federative units, 2014–2024. Pooling by REML; Hartung–Knapp 95% CI.

Age group	Deaths	RR (lag 0–7)	RR (lag 0–21)
Under 65 years	3,740,800	1.119 (1.077–1.162)	1.132 (1.061–1.209)
65 to 74 years	2,364,511	1.170 (1.126–1.216)	1.182 (1.123–1.244)
75 to 84 years	2,623,248	1.237 (1.173–1.303)	1.309 (1.243–1.378)
85 years or older	2,310,097	1.329 (1.235–1.431)	1.440 (1.301–1.594)

Over lags 0 to 7, the excess mortality at 85 years or older is 32.9%, compared with 11.9% below 65 — **approximately three times** as large, under the same percentile contrast. These values correspond to the cumulative relative risk over lags 0 to 7 associated with a single day at the 99th percentile compared with a day at the 75th percentile — not to an increase in observed mortality over an entire week of extreme heat. Over 21 days, the ratio between the excesses rises to **3.3** (44.0% versus 13.2%), because risk continues to accumulate in the older groups and levels off in the younger ones (Figure 1B).

The gradient is monotonic in the pooled estimate, **but not in every unit taken individually**: 7 of the 14 federative units show strictly increasing ordering across the four age groups in the 21-day cumulative risk, and **13 of the 14 show higher risk at 85 or older than below 65** (Table 3). Unit-level estimates are noisy — ranging from 0.786 to 2.173 —, which was expected for a three-week cumulative risk in individual state series, and is why the result rests on the pooled estimate rather than on unit-by-unit replication.

Table 3. Cumulative 21-day relative risk by federative unit and age group.

FU	<65	65–74	75–84	85+	Monotonic
AL	1.065	1.104	1.098	1.269	no
BA	1.021	1.038	1.212	1.318	yes
CE	1.354	1.442	1.403	1.606	no
ES	0.907	0.997	1.281	1.121	no
MG	1.124	1.163	1.244	1.430	yes
MS	1.198	1.205	1.450	2.173	yes
PI	1.084	1.153	1.176	1.370	yes
PR	1.121	1.132	1.345	1.552	yes
RJ	1.092	1.273	1.378	1.495	yes
RN	1.193	1.212	1.246	0.937	no
RR	1.066	1.128	0.786	1.300	no
RS	1.436	1.359	1.470	1.637	no
SC	1.173	1.157	1.240	1.470	no
SP	1.087	1.164	1.387	1.555	yes

Table 4. Formal contrast between age groups. Each estimate is the ratio between the relative risks of two age groups, computed **within each federative unit** and pooled in three ways: REML meta-analysis of the contrasts assuming zero covariance; **direct estimation of the covariance by a sandwich estimator clustered by day**; and cluster bootstrap over the 14 federative units.

Contrast	Window	REML (sum of variances)	Estimated covariance (sandwich by day)	FU bootstrap
85+ / <65	lags 0–7	1.209 (1.156–1.265)	1.208 (1.154–1.264)	1.160 (1.073–1.229)
85+ / <65	lags 0–21	1.286 (1.191–1.389)	1.286 (1.192–1.386)	1.256 (1.143–1.367)
75–84 / <65	lags 0–7	1.115 (1.079–1.151)	1.115 (1.080–1.151)	1.094 (1.059–1.128)
75–84 / <65	lags 0–21	1.153 (1.088–1.222)	1.151 (1.086–1.221)	1.108 (1.018–1.192)
65–74 / <65	lags 0–7	1.049 (1.027–1.071)	1.048 (1.026–1.071)	1.049 (1.024–1.076)
65–74 / <65	lags 0–21	1.050 (1.018–1.083)	1.051 (1.019–1.084)	1.040 (1.014–1.066)
85+ / 75–84	lags 0–7	1.089 (1.060–1.119)	1.089 (1.058–1.120)	1.060 (1.006–1.107)
85+ / 75–84	lags 0–21	1.122 (1.074–1.171)	1.121 (1.076–1.169)	1.134 (1.029–1.248)

The formal contrasts provide consistent evidence of effect modification by age, including when the comparison is based on the bootstrap over federative units. All eight contrasts exclude unity under all three approaches, including the most demanding one — 85 or older versus 75 to 84, adjacent groups that are both older adults. All three approaches support the presence of the gradient, **although its magnitude is smaller in the unweighted bootstrap**: 1.160 versus 1.208 and 1.209 in the comparison between extreme groups over lags 0 to 7. The smaller magnitude in the bootstrap is compatible with the difference between an unweighted synthesis of the state contrasts and precision-weighted pooling.

The decisive result is the third column. **Direct estimation of the covariance, by a sandwich estimator clustered by day, yields intervals virtually identical to those obtained assuming independence** — 1.208 (1.154–1.264) versus 1.209 (1.156–1.265) in the comparison between extreme groups. Incorporating the estimated covariance produced minimal changes in the estimates and confidence intervals. The bootstrap remains a robustness analysis of the between-unit gradient, not an equivalent inferential route.

3.3 The delayed component: no detectable compensation, with an excess sensitive to specification

Table 5. Cumulative relative risk by lag window, same contrast. Pooling by REML; Hartung–Knapp 95% CI.

Age group	Lags 0–7	Lags 8–21	Lags 18–21
Under 65 years	1.119 (1.077–1.162)	1.013 (0.970–1.058)	1.005 (0.993–1.017)
65 to 74 years	1.170 (1.126–1.216)	1.002 (0.983–1.021)	0.999 (0.988–1.010)
75 to 84 years	1.237 (1.173–1.303)	1.044 (1.020–1.070)	1.006 (0.996–1.015)
85 years or older	1.329 (1.235–1.431)	1.079 (1.037–1.122)	1.003 (0.992–1.014)

This is the formal estimate used to investigate the presence of delayed compensation.

First reading. Over lags 8 to 21 **we found no evidence of a deficit in any age group**. Below 75 years the estimates are compatible with no effect (1.013 and 1.002); above 75, the association remains positive and statistically detectable – 1.044 (1.020–1.070) at 75 to 84 years and 1.079 (1.037–1.122) at 85 or older. No statistically significant compensatory deficit was detected. The intervals still allow for some small compensation – below 65 years, for example, the lower limit is 0.970 –, so the result is absence of evidence of compensation, not a demonstration that no compensation exists.

Second reading. Consequently, the 21-day cumulative risk **exceeds** the 7-day risk in the older groups: 1.440 versus 1.329 at 85 or older, and 1.309 versus 1.237 at 75 to 84. In the younger groups the cumulative risk levels off (Figure 1B, right panel). A sufficiently large displacement occurring within the observed window would predict the opposite pattern – an initial excess followed by a deficit and a declining cumulative risk.

Third reading. At the final lags, 18 to 21 days, all estimates lie around unity (0.999 to 1.006) – **with no compensatory phase below it**. What happens after day 21 this design cannot see.

An algebraic caveat is in order. Because each window was pooled separately in a random-effects meta-analysis, **the pooled estimates for the sub-windows are not algebraically decomposable into the pooled 0–21-day risk**: the product of 1.329 and 1.079 is 1.434, close to but not identical with the 1.440 in Table 2. We therefore describe the relationship between windows qualitatively, without converting it into fractions of the excess.

Table 6 shows the values of the figure at selected lags. The peak occurs on the day itself and increases with age; the decay is slower the older the group – below 65 years the risk reaches unity around the twelfth day, whereas at 85 or older it remains significantly elevated until the sixteenth.

Table 6. Relative risk at selected lags, same contrast. REML; Hartung–Knapp 95% CI.

Lag	Under 65	65 to 74	75 to 84	85 or older
0	1.047 (1.035–1.059)	1.062 (1.047–1.077)	1.062 (1.043–1.082)	1.085 (1.067–1.103)
1	1.024 (1.015–1.033)	1.035 (1.028–1.043)	1.045 (1.031–1.058)	1.062 (1.049–1.076)
2	1.012 (1.006–1.018)	1.021 (1.015–1.026)	1.034 (1.021–1.046)	1.046 (1.034–1.058)
3	1.008 (1.004–1.012)	1.013 (1.008–1.018)	1.025 (1.016–1.034)	1.035 (1.025–1.044)
5	1.006 (1.002–1.009)	1.008 (1.003–1.014)	1.015 (1.010–1.020)	1.022 (1.016–1.028)
7	1.004 (1.001–1.007)	1.005 (0.999–1.011)	1.010 (1.006–1.013)	1.016 (1.012–1.020)
10	1.001 (0.997–1.005)	1.002 (0.998–1.006)	1.005 (1.003–1.008)	1.010 (1.005–1.014)
14	1.000 (0.996–1.004)	1.000 (0.997–1.002)	1.003 (1.000–1.005)	1.006 (1.002–1.009)
18	1.001 (0.998–1.003)	0.999 (0.998–1.001)	1.002 (0.999–1.004)	1.002 (0.999–1.004)
21	1.002 (0.998–1.005)	1.000 (0.995–1.004)	1.001 (0.998–1.004)	1.000 (0.996–1.003)

3.4 Consistency with a different design

The case-crossover analysis, applied to an expanded dataset with a different design, reproduces the ordering (Table 7). The two designs do not estimate the same parameter — binary exposure, a 0–3-day window and monthly stratification, versus continuous exposure, the P99 × P75 contrast and the 0–7-day cumulative risk in the DLNM —, so the agreement is in the ordering across age groups, not in magnitude.

Table 7. Time-stratified case-crossover, 17 federative units, all months, 2014–2025. Exposed day: a day in the top decile of that month's own maximum temperature, or one of the three following days.

Age group	Deaths	RR	95% CI
Under 65 years	3,428,256	1.011	1.008–1.013
65 to 74 years	2,180,273	1.016	1.013–1.019
75 to 84 years	2,416,426	1.025	1.022–1.028
85 years or older	2,135,439	1.035	1.032–1.038
75 years or older	4,551,865	1.030	1.028–1.032
Congenital malformation (negative control)	75,289	1.004	0.988–1.021

The trend is **1.0056 per decade of age** (1.0047–1.0065; $p = 6 \times 10^{-35}$), and the ratio between those aged 85 or older and those under 65 is **1.024** (1.020–1.028; $p = 1.2 \times 10^{-34}$). The relative risks are much smaller than those from the DLNM because the contrast is different — "a day in the top decile or up to

three days after" versus "the other days of the same month", rather than the 99th versus the 75th percentile — but the ordering and the ratios between age groups agree.

The **INMET heat-event windows**, analysed separately on 110,013 natural deaths in 27 unit-months, support the age-related ordering where it matters: 1.040 (1.014–1.068; $p = 0.0028$) at 75 to 84 years, 1.036 (1.008–1.064; $p = 0.012$) at 85 or older, and 1.038 (1.019–1.058; $p = 1.0 \times 10^{-4}$) for those aged 75 or older combined. Below 65 years, the same design detects no effect: 1.004 (0.981–1.028). The agreement between exposure definitions constructed in different ways reinforces the consistency of the age gradient. Note that **the confirmatory case-crossover analysis applies to the acute gradient, not to the delayed component**: as we show in section 3.6, this design lacks resolution for long lags, so the finding for lags 8 to 21 rests entirely on the DLNM.

At the **municipal scale** — station nearest to the seat, 100 km radius, 4,512 municipalities —, the gradient holds: 1.012, 1.018, 1.024 and **1.038** (1.035–1.041), based on **10,247,387 natural deaths** actually included in the municipal model.

3.5 Stability and sensitivity

Exclusion of units with interpolated temperature. Roraima has a single station and 24% of days without observations; Rio Grande do Norte has 345 missing days. Because linear interpolation could, in principle, erase a real heat episode between two observed days — and the study is precisely about extremes —, we repeated the main analysis without these units (Table 8).

Table 8. Sensitivity to the exclusion of units with interpolated temperature days. REML; Hartung–Knapp 95% CI.

Set	Age group	Lags 0–7	Lags 0–21
14 FUs (main)	<65	1.119 (1.077–1.162)	1.132 (1.061–1.209)
	65–74	1.170 (1.126–1.216)	1.182 (1.123–1.244)
	75–84	1.237 (1.173–1.303)	1.309 (1.243–1.378)
	85+	1.329 (1.235–1.431)	1.440 (1.301–1.594)
13 FUs (without RR)	<65	1.116 (1.074–1.161)	1.134 (1.058–1.215)
	65–74	1.169 (1.123–1.216)	1.182 (1.120–1.248)
	75–84	1.237 (1.170–1.308)	1.312 (1.248–1.379)
	85+	1.335 (1.239–1.439)	1.441 (1.294–1.605)
12 FUs (without RR and RN)	<65	1.118 (1.072–1.166)	1.131 (1.050–1.218)
	65–74	1.166 (1.118–1.216)	1.181 (1.114–1.251)
	75–84	1.244 (1.174–1.318)	1.314 (1.246–1.385)
	85+	1.362 (1.282–1.448)	1.478 (1.356–1.611)

Removing the units with partly modelled exposure **does not reduce the estimates; if anything, it increases them** – at 85 or older, the 21-day cumulative risk goes from 1.440 to 1.478 when Roraima and Rio Grande do Norte are removed. The direction of the change is compatible with attenuation due to exposure misclassification; the sensitivity analysis does not suggest that interpolation explains the observed associations.

Degrees of freedom of the time spline (Table 9). This is the decisive sensitivity analysis for the result in the delayed window, because the time spline and the distant lags compete for the same slow variation: an insufficiently flexible spline could let residual seasonality be absorbed by the long lags, creating a spurious excess over lags 8 to 21.

Table 9. Sensitivity to the degrees of freedom of the time spline. REML; Hartung–Knapp 95% CI.

df/year	Age group	Lags 0–7	Lags 8–21	Lags 0–21
6	<65	1.113 (1.068–1.159)	1.000 (0.950–1.052)	1.109 (1.027–1.199)
	65–74	1.144 (1.103–1.188)	0.985 (0.953–1.017)	1.124 (1.058–1.193)
	75–84	1.221 (1.151–1.296)	1.028 (0.983–1.076)	1.250 (1.155–1.352)
	85+	1.322 (1.229–1.422)	1.071 (1.025–1.118)	1.412 (1.276–1.563)
8 (main)	<65	1.119 (1.077–1.162)	1.013 (0.970–1.058)	1.132 (1.061–1.209)
	65–74	1.170 (1.126–1.216)	1.002 (0.983–1.021)	1.182 (1.123–1.244)
	75–84	1.237 (1.173–1.303)	1.044 (1.020–1.070)	1.309 (1.243–1.378)
	85+	1.329 (1.235–1.431)	1.079 (1.037–1.122)	1.440 (1.301–1.594)
10	<65	1.108 (1.065–1.152)	0.986 (0.934–1.040)	1.091 (1.011–1.178)
	65–74	1.157 (1.117–1.198)	0.974 (0.938–1.011)	1.129 (1.065–1.198)
	75–84	1.219 (1.147–1.295)	1.015 (0.984–1.048)	1.262 (1.184–1.344)
	85+	1.311 (1.225–1.403)	1.047 (1.019–1.076)	1.388 (1.278–1.507)

The 0–7-day cumulative risk is remarkably stable: at 85 or older it ranges between 1.311 and 1.329. The 21-day cumulative risk varies more, between 1.388 and 1.440. And, most importantly, **the excess over lags 8 to 21 at 85 or older survives all three specifications** – 1.071, 1.079 and 1.047, with the interval excluding unity in all of them –, including when the spline is made more flexible, which is the direction that would remove residual seasonality. At 75 to 84 years the delayed excess loses significance with 6 and with 10 degrees of freedom, so the evidence there is weaker.

Temperature quality control and alternative seasonal control (Table 10). Two additional sensitivity analyses, both actually run rather than assumed.

Table 10. Sensitivity to the temperature quality filter and to the form of seasonal control. REML; Hartung–Knapp 95% CI.

Specification	Age group	Lags 0–7	Lags 8–21	Lags 0–21
With QC	75–84	1.237 (1.173–1.303)	1.045 (1.020–1.070)	1.309 (1.243–1.378)
	85+	1.329 (1.235–1.431)	1.079 (1.037–1.122)	1.440 (1.301–1.594)
Without QC (raw Tmax)	75–84	1.237 (1.174–1.304)	1.045 (1.020–1.070)	1.310 (1.244–1.379)
	85+	1.330 (1.235–1.432)	1.079 (1.037–1.122)	1.441 (1.301–1.595)
Time spline, 88 df (main)	75–84	1.237 (1.173–1.303)	1.044 (1.020–1.070)	1.309 (1.243–1.378)
	85+	1.329 (1.235–1.431)	1.079 (1.037–1.122)	1.440 (1.301–1.594)
Year × month strata (132 indicators)	<65	1.101 (1.052–1.152)	0.989 (0.948–1.032)	1.084 (1.009–1.164)
	65–74	1.150 (1.107–1.195)	0.967 (0.929–1.007)	1.106 (1.033–1.184)
	75–84	1.214 (1.138–1.296)	1.014 (0.989–1.039)	1.227 (1.142–1.319)
	85+	1.293 (1.181–1.415)	1.040 (0.985–1.098)	1.335 (1.173–1.520)

Note: the two QC rows were computed from series reconstructed from the station file, whose values are rounded to one decimal place; hence 1.045 where the main analysis, which uses the full-precision series, gives 1.0444. The difference is due to rounding in the intermediate file, not to the method.

The quality filter is irrelevant to the result: removing the 6 °C rule changes the estimates in the fourth decimal place (85+ over 21 days goes from 1.440 to 1.441), as expected given that it reclassifies 154 of the 56,252 unit-days.

Seasonal control, by contrast, matters – and qualifies the second finding. When the time spline is replaced by 132 year × month indicators, which absorb all variation that is constant within the month, the 0–7-day excess persists (85+: 1.293; 1.181–1.415), but **the excess over lags 8 to 21 falls by half and loses significance** – 1.040 (0.985–1.098) at 85 or older and 1.014 (0.989–1.039) at 75 to 84.

This specification controls more strictly for between-month variation, at the cost of also removing exposure components shared within each month; the trade-off between the two options is discussed in section 4.2. The result delimits what can be claimed: **no specification produces a statistically detectable deficit in any age group, whereas the presence of a sustained excess between days 8 and 21 depends on the form of seasonal control.**

Absolute thermal contrast (Table 11). Because the percentile contrast corresponds to different thermal doses across units — from 2.1 °C to 6.1 °C —, we repeated the analysis with a common increment of 3 °C above each unit's 75th percentile.

Table 11. Cumulative relative risk over lags 0 to 7 under the percentile contrast and under a common absolute contrast. REML; Hartung–Knapp 95% CI.

Age group	Percentile (P99 × P75)	Absolute (P75 + 3 °C)
Under 65 years	1.119 (1.077–1.162)	1.057 (1.045–1.068)
65 to 74 years	1.170 (1.126–1.216)	1.077 (1.056–1.098)
75 to 84 years	1.237 (1.173–1.303)	1.108 (1.083–1.134)
85 years or older	1.329 (1.235–1.431)	1.176 (1.115–1.240)

Under an identical thermal dose in all units, the risks are naturally smaller — 3 °C is less than the average P99–P75 —, but **the monotonic ordering across age groups is fully preserved**. The age gradient therefore remained present when all units were compared under the same absolute temperature increment.

Pooling method (Table 12). The Hartung–Knapp interval is the most conservative with only 14 units and was adopted as the main one.

Table 12. 21-day cumulative risk by interval method.

Age group	REML (Wald)	REML + Hartung–Knapp	I ²
<65	1.132 (1.066–1.203)	1.132 (1.061–1.209)	76%
65–74	1.182 (1.126–1.240)	1.182 (1.123–1.244)	44%
75–84	1.309 (1.249–1.371)	1.309 (1.243–1.378)	53%
85+	1.440 (1.322–1.568)	1.440 (1.301–1.594)	72%

Period. In the case-crossover analysis, among those aged 85 or older, the relative risk is 1.038 (2014–2019), 1.034 (2020–2021) and 1.032 (2022–2025), with the same exposure and the same window as in Table 7. The three intervals overlap widely; there is no evidence that the gradient depends on the pandemic period, although we did not formally test the interaction with period.

Taken together, the sensitivity analyses show **high stability of the age gradient and of the 0–7-day excess**. No specification produces a statistically detectable deficit in any age group. In contrast, **the evidence of a positive excess between days 8 and 21 is sensitive to the form of seasonal control, including at 85 or older**. The 21-day cumulative risk should be read together with the observed variation: 1.335 to 1.440 at 85 or older.

3.6 What the case-crossover cannot see

One methodological result deserves to be recorded, because it guided the choice of the main model.

The **placebo test** did not identify relevant positional bias within the short window: with 200 randomly drawn exposure windows, the pooled relative risk returns to 1.000 with an empirical standard deviation of 0.0016, and the negative control outcome is indistinguishable from that distribution ($z = 0.42$; $p = 0.68$). The test showed no spurious effect under the randomisation used – which does not demonstrate absence of bias through other mechanisms.

It stops working, however, as the window lengthens. When individual lags are extended to 14 days, all age groups drift downwards and those under 65 accumulate **-13.6%** – an implausible magnitude, inconsistent with the pattern of the main analyses, in which the same group shows an excess over lags 0 to 7 and a delayed estimate close to unity. The number of usable strata falls from 1,910 to 1,465. The cause is positional selection: at long lags, only the hot days at the beginning of the month contribute, and the corresponding exposed day always falls in the second half of the month. Checking the temperature at each lag completes the diagnosis – at lags of 4 to 7 days, exposed days lie at percentile 0.444 to 0.483, practically identical to that of the referent days (0.441), so the apparent deficit did not come from temperature.

The practical conclusion, restricted to what we observed: **in this dataset, the case-crossover with monthly stratification proved reliable for short lags, but showed evidence of positional selection as the window was extended.** We did not test whether this is a general property of the design.

4. Discussion

4.1 What the findings support

The results form a hierarchy of robustness that should be made explicit before discussing them.

1. **Primary finding – age gradient in the acute effect.** It survives everything we tested: two designs (DLNM and case-crossover), two exposure definitions (INMET event windows and meteorological decile), two geographical scales (state and municipal), percentile contrast and an absolute 3°C contrast, exclusion of the units with interpolated temperature, three specifications of the time spline, and seasonal control by year × month indicators.
2. **Secondary finding – no statistically detectable deficit was observed between days 8 and 21,** under any of the specifications tested. This does not demonstrate absence of displacement: the study is not an equivalence test, and a non-significant result does not formally establish absence of effect.
3. **Exploratory finding – possible persistent excess between days 8 and 21, mainly at 85 or older.** Present in the main analysis and in the three variations of the time spline, absent under year × month control. It is not established.

Heat increases mortality from natural causes in all age groups, with a steep, monotonic gradient in the pooled estimate: the first-week excess is approximately three times larger at 85 or older than below 65. The finding is consistent with the international literature on effect modification by age (Benmarhnia et al. 2015; Son et al. 2019) and adds to it in three respects: it quantifies the gradient in a large multi-state Brazilian dataset – 14 of the 27 federative units, with 11 million deaths; it reproduces the ordering in two distinct analytical designs; and it supports it under an exposure definition set by a meteorological authority, not only by a statistical percentile.

Beyond the physiological vulnerability associated with ageing, the observed age gradient should be interpreted in the context of the social and housing conditions of the older Brazilian population. The 2022 Demographic Census counted 32.1 million people aged 60 or older in the country, corresponding to 15.8% of the population (IBGE 2024). Part of this population is exposed to conditions that may limit the capacity for protection during extreme heat episodes: among people aged 60 or older, 1.9% lived in households without piped water and 0.4% in households without a bathroom, toilet or pit latrine (IBGE 2025a). In addition, 6.6% of residents of Favelas and Urban Communities were aged 65 or older in 2022 – a lower proportion than in the Brazilian population as a whole (10.9%) – which, applied to the 16,390,790 inhabitants of these territories, corresponds to approximately 1.08 million people in this age group (calculation derived from IBGE 2025b). **These conditions were not measured individually in this study and therefore cannot be regarded as explanatory mechanisms for the estimated risks;** they do indicate, however, that the biological vulnerability associated with age may coexist with material barriers to heat adaptation.

The social dimension may represent another layer of vulnerability. In a nationally representative sample of Brazilians aged 50 or older from ELSI-Brazil, 16.8% reported always feeling lonely and 31.7% sometimes; living alone was associated with a 2.44 times higher prevalence of persistent loneliness (Sandy Júnior et al. 2023). Although loneliness, social isolation and living alone are distinct constructs, these indicators are relevant to heat response, in which early recognition of symptoms, access to hydration and to cooler environments, and contact with family, neighbours or health services may determine whether intervention is possible. **The present study has no individual information on living arrangements or social networks, so this hypothesis cannot be tested directly.**

The second finding has the greatest practical consequence, and must be stated with the precision the intervals allow. **We found no statistically significant evidence of a compensatory deficit in any age group within 21 days, under any of the specifications tested.** This is not the same as demonstrating absence of compensation: under seasonal control by year × month indicators, the point estimate at 65 to 74 years is 0.967, with a lower limit of 0.929 – that is, a deficit of up to about 7% remains compatible with the data in this group. What can be stated is that **compensation, if it exists, was not large enough to be detected statistically in this design.**

A third finding, exploratory and sensitive to specification, is the persistent excess between days 8 and 21 in the groups aged 75 or older – 1.044 (1.020–1.070) and 1.079 (1.037–1.122) in the main analysis.

This finding must be weighed against the most plausible alternative explanation: a spurious excess at long lags due to residual seasonality not absorbed by the time spline. The two relevant sensitivity analyses point in different directions and must be read together. Making the spline **more** flexible — 10 degrees of freedom per year, which is the direction that would remove remaining seasonal variation — does not eliminate the delayed excess at 85 or older (1.047; 1.019–1.076). The alternative specification with year × month indicators, however, attenuates this component, and its interval comes to include unity (1.040; 0.985–1.098). The excess between days 8 and 21 is therefore not an established finding.

Previous investigations of heat-related displacement found heterogeneous results across cities, including São Paulo (Hajat et al. 2005). The data from this study provide no evidence of substantial compensation within the three weeks observed.

The reading for policy must respect what the design measures. The study did not evaluate any intervention; it observed an association and the absence of detectable compensation within three weeks. What follows from this is more modest, yet still relevant: **economic evaluations of prevention measures should not assume, without evidence, that a large part of the excess mortality associated with heat merely represents an advancement of a few days.**

4.2 What the findings do not support

Six limitations must be made explicit, lest the text claim more than the design tested.

First, association is not heat-specific causation. Air pollution, humidity and wildfire smoke accompany temperature, and this model does not separate them. What we estimate is the effect of a hot day together with everything that comes with it.

Second, 21 days is the observed window, not the full horizon. A compensatory deficit beyond day 21 remains possible, and the design does not allow us to state that the deaths would not have occurred over a longer period. Nor did we estimate years or months of life lost, a quantity that would require individual survival data.

Third, the delayed excess is the fragile part of the finding. The 0–7-day cumulative risk is stable (85+: 1.293 to 1.330 across all specifications). The excess between days 8 and 21, by contrast, survives the three variations of the time spline at 85 or older, loses significance at 75 to 84 years in two of them, and **loses significance in both age groups under seasonal control by year × month indicators** (Table 10). This last specification controls more strictly for between-month variation and therefore also removes exposure components shared within each month; what is robust is the absence of a statistically detectable deficit; the presence of a sustained excess is not.

Fourth, exposure is the unweighted mean of the state's stations. Each station carries equal weight, regardless of the population it represents, so the exposure assigned to a metropolitan region may carry the same weight as that of a station in a sparsely populated area. In addition, the number of unique stations ranges from 1 to 67 across units, and the percentile contrast corresponds to thermal jumps of

2.1°C to 6.1°C — heterogeneity of construction that probably contributes to the I^2 of 72% at 85 or older. **Roraima deserves specific mention:** a single station and 24% of days without observations, which makes it the least reliable unit in the series. The age gradient was preserved in the municipal-scale analysis using the nearest station; because the two scales use different exposure constructions and different subsets of deaths, the difference in magnitude between them does not directly identify the direction of the measurement error. Excluding Roraima and Rio Grande do Norte, the two units with interpolated temperature days, slightly increased the estimates (Table 8), a finding compatible with attenuation due to exposure misclassification, although differences between federative units may also contribute. Although the municipal-scale analysis supported the age gradient, spatial exposure misclassification remains a relevant limitation of the state-level estimates, particularly in geographically extensive federative units.

Fifth, the gradient does not fully replicate unit by unit. Seven of the 14 federative units show strictly monotonic ordering across the four age groups in the 21-day cumulative risk, although 13 of the 14 show higher risk at 85 or older than below 65. Individual state estimates are noisy; the finding rests on pooling.

Sixth, stratification was by age only. At the individual level used in this analysis, SIM does not allow characterisation of housing conditions, living arrangements, social isolation, access to air conditioning or other determinants of the capacity to adapt to heat. The observed age gradient therefore represents the average vulnerability associated with age in the population studied, and does not allow the relative contribution of physiological and social mechanisms to be distinguished.

In addition, age comes from the IDADE field of the death certificate, without external verification.

4.3 Methodological contribution

Using a placebo test and a lag-by-lag temperature diagnostic, we documented the **temporal resolution limit of the case-crossover with monthly stratification**. We found no relevant positional bias within the short window assessed — the placebo returns 1.000 with a standard deviation of 0.0016 —, but the design produces spurious deficits when the window is extended. Studies investigating mortality displacement with this design would do well to check the stability of the estimates as the window lengthens, and to consider time-series models when the question requires long lags. The check is not trivial: naive extension of the lag window would have led, in this dataset, to the false conclusion that heat produces a net mortality deficit among the youngest.

5. Conclusion

The risk of death associated with heat increases monotonically with age in Brazil: in the first week after a day of extreme heat, an excess of 32.9% at 85 or older versus 11.9% below 65; over 21 days, 44.0% versus 13.2%. **No age group shows a statistically detectable deficit between days 8 and 21 under any of the specifications tested, and we found no statistically significant evidence of compensatory mortality reduction within the 21-day observation window, which does not exclude partial displacement or**

compensation beyond that period. Beyond that, the design cannot see. The persistent excess in this window is suggestive, not established: detectable at 85 or older in the main analysis and in the time-spline variations, absent under seasonal control by year $\times$ month indicators.

Three recommendations follow from the study:

1. **Prioritise the population aged 75 years or older** in municipal and state extreme-heat response plans, with attention to the day of the event itself and the two following days, when the excess is concentrated. Within this population, in light of the literature and the social indicators discussed in section 4.1, active case-finding strategies may consider older people who live alone, have reduced mobility or live in housing conditions that hinder adequate hydration and cooling.
2. **Evaluate response plans without presupposing compensation.** Within the three-week horizon we detected no compensatory deficit in any age group, under any specification. The possibility of prolonged vulnerability after extreme heat episodes should be considered, especially among the oldest, bearing in mind that the evidence of excess between days 8 and 21 was sensitive to the specification of seasonal control.
3. **Check the stability of the lag window** in case-crossover studies with monthly stratification, and adopt time-series models when the question requires long lags.

Two questions remain open, and this design does not answer them. The first is the separation of heat from the pollutants that accompany it, which requires coupling the mortality and temperature series with particulate matter and ozone series for the same units. The second is the horizon of displacement beyond three weeks, which requires longer lag windows and, with them, more careful handling of the competition between the long-term trend and the distant lags.

Declarations

Ethics approval and consent to participate. This study used only secondary, de-identified data from public-access national databases: death records from the Brazilian Mortality Information System (SIM/DATASUS) and meteorological data from the National Institute of Meteorology (INMET). No individual was contacted, and the analysis used daily death counts aggregated by federative unit and municipality, with no possibility of individual identification. Under Brazilian National Health Council Resolution No. 510/2016 (Article 1, sole paragraph, items II, III and V), research using publicly accessible information, information in the public domain, or databases with aggregated information without individual identification is not registered with or reviewed by the Research Ethics Committee system (CEP/CONEP). Informed consent was therefore not applicable.

Data and code availability. The aggregated data and the code that reproduce all analyses are available on Zenodo (doi:10.5281/zenodo.23421154). Individual SIM microdata are public and can be obtained directly from DATASUS; historical automatic-station data, from the INMET portal.

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Figures

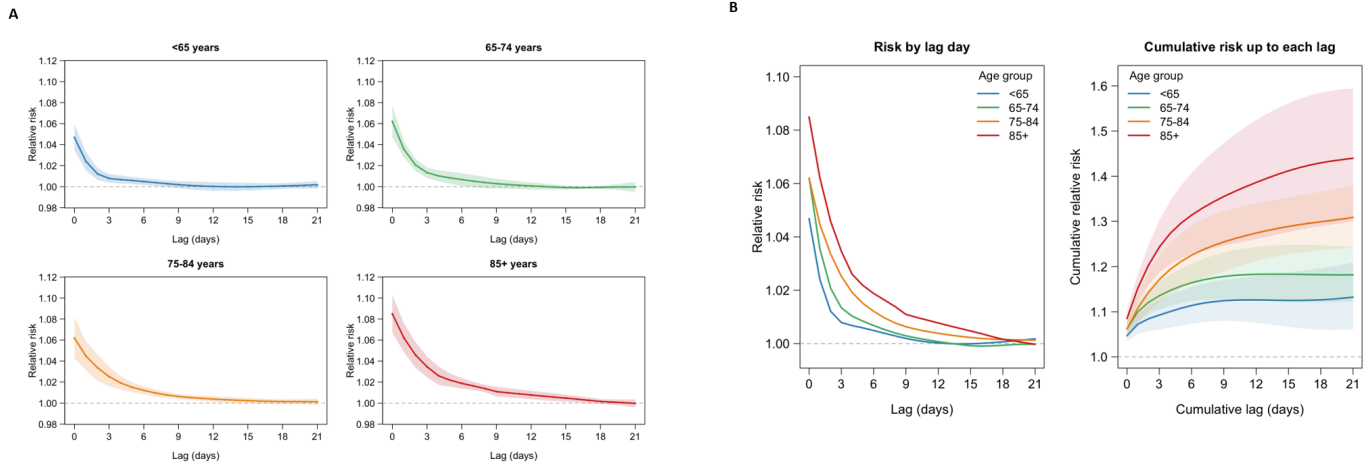


Figure 1

Lag curve by age group, 99th versus 75th percentile of each unit's own temperature, 14 federative units, 2014–2024. (A) Relative risk at each lag day, by age group, with Hartung–Knapp 95% CI. (B) Left, risk by lag day; right, cumulative risk up to each lag, with 95% CI — at 7 and 21 days the cumulative risk matches Table 2.

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