Firms Amplify Racial Compensation Gaps When Exposed to Climate Disasters

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Climate change is reshaping economic opportunity, yet the organizational channels through which climate shocks deepen racial inequality remain poorly understood — even though firms are central institutions of income allocation.[3,4] Linking Brazil's administrative employer–employee records for the universe of large private-sector establishments to geocoded floods, storms and landslides, we show that disaster exposure is followed by higher average establishment compensation. This premium accrues primarily to white employees, while nonwhite employees experience no comparable increase. Consequently, the within-establishment nonwhite–white compensation gap widens by about 16% relative to the sample mean and persists for at least six years. The widening occurs among continuing employment relationships and is not fully accounted for by differential exit, hiring, occupational or skill sorting, or contract types. Effects are largest in large, urban municipalities exposed to high-intensity disasters and in competitive labor markets. These findings identify firms as sites where climate shocks become durable racial compensation inequality — a mechanism largely absent from adaptation frameworks.[5]

Climate-related disasters impose economic losses that are rarely distributed evenly across society.[1–16] Existing evidence links these disasters to changes in income, employment, earnings, household finances, wealth inequality and regional economic growth.[1,7,11,17-19.] Yet this evidence still leaves firms' internal compensation largely unexamined. This gap matters because firms are major employers through which climate shocks are translated into employees' economic livelihoods. Their compensation decisions determine how post-disaster labor-market pressures are translated into worker income. If climate disasters raise the value of labor and exposed firms increase compensation to retain or attract employees, the distribution of those compensation increases may affect inequality.

Evidence on this organizational channel is important for climate adaptation. In research and public policy, firms are often studied as exposed units that passively absorb market shocks affecting their operations and performance.[20-24] But adaptation also requires internal allocation decisions. If firms systematically allocate disaster-related compensation adjustments along lines of social privilege, the inequality consequences of climate change extend beyond well-documented pathways such as human health, displacement and asset destruction, reaching into the routine operation of these labor-market institutions.

Brazil offers a particularly informative setting for studying this channel. The country combines large and well-documented racial income inequality with growing exposure to hydrometeorological disasters. Its administrative payroll records also make it possible to measure racial compensation gaps within establishments at scale. Mandatory employment reporting, racial classification in worker records and comprehensive coverage of the formal labor market allow us to observe whether compensation changes after disasters are distributed differently across racial groups working inside the same establishment. At the same time, employers retain discretion over compensation adjustments, making Brazil a setting in which post-disaster labor-market pressure can translate into unequal within-firm outcomes.[25,26]

The analysis focuses on 99,323 large private-sector establishments, where worker counts allow stable measurement of within-establishment racial compensation gaps. Exposure is defined as an establishment's first exposure to a climate disaster within 25 km. For the baseline analyses, the unit of analysis is the establishment–year, and the main outcome is the hourly compensation gap between nonwhite and white employees within the same establishment. We estimate dynamic event-study models using a staggered difference-in-differences design that exploits within-establishment variation over time, comparing newly exposed establishments with those not yet exposed (Extended Data Table 2). Fig.1 illustrates the empirical context and research design. As a robustness check, we estimate high-dimensional fixed-effects models that additionally absorb microregion-by-year and industry-by-year variation (Extended Data Table 3). Both approaches yield similar results.

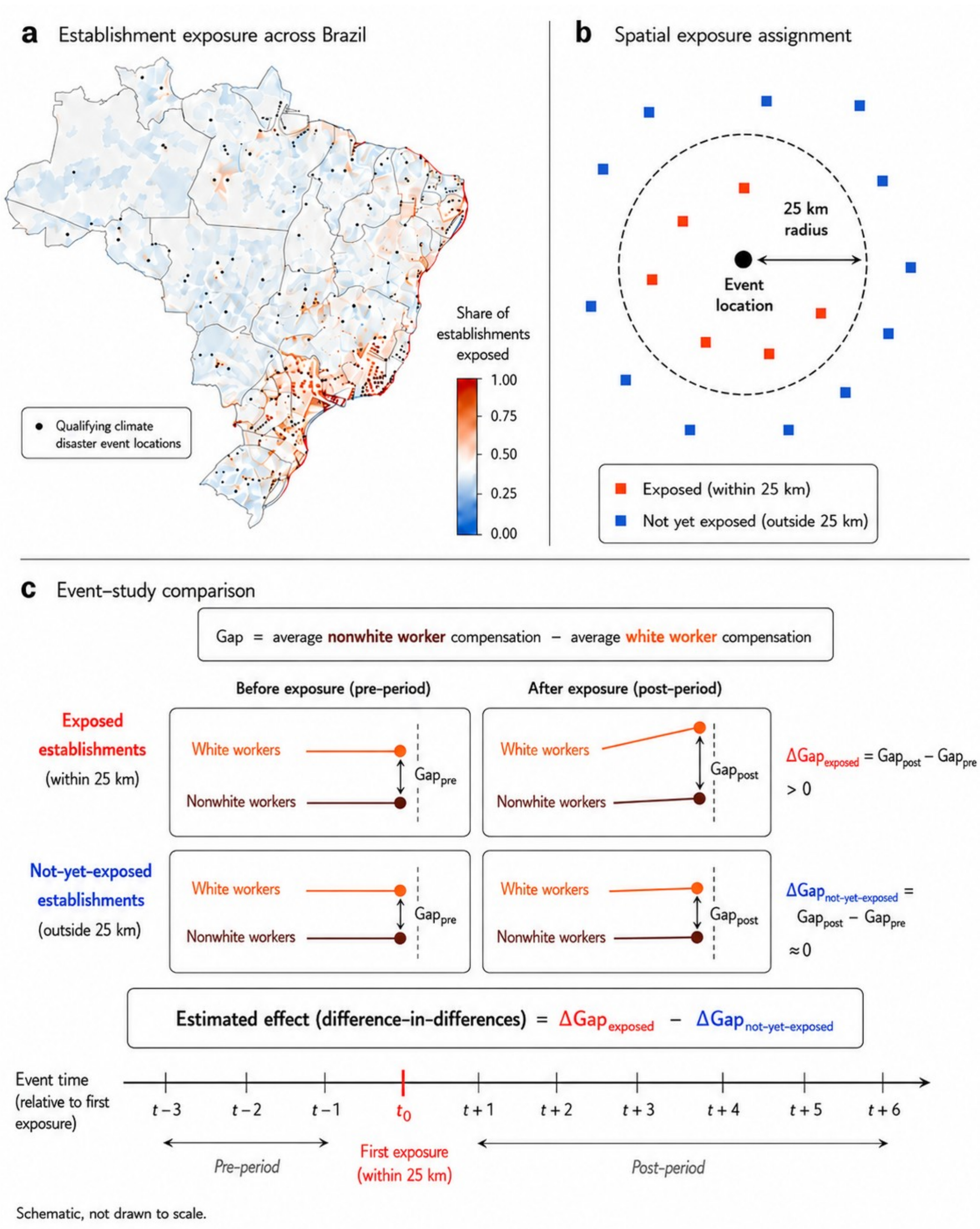

Fig. 1 | Empirical context and research design. a, Establishments are classified as exposed in the first year a qualifying climate disaster occurs within 25 km of their geocoded location; establishments not yet exposed during the same event-time window provide the comparison group. **b,** Event time is measured relative to first exposure, separating pre-exposure years from post-exposure years. **c,** The racial compensation gap is measured within establishments as average hourly compensation for nonwhite workers minus average hourly compensation for white workers. The difference-in-differences estimate compares the post-minus-pre change in this gap for exposed establishments with the corresponding change for not-yet-exposed establishments. Compensation levels and gap lengths in the schematic are illustrative rather than plotted estimates.

Compensation rises, racial gap widens

Following disaster exposure, average establishment compensation rises by 0.538 Brazilian reais (BRL) per hour, or approximately 5%, relative to pre-shock trends ($P = 0.001$), while average workforce size decreases by approximately 6% relative to pre-shock trends ($P<0.001$) (Extended Data Table 2). These patterns indicate that disaster exposure coincides with workforce reduction while average compensation rises among workers who remain. The central question is how this post-disaster premium is distributed.

White employees' compensation increases earlier and more sharply, particularly four to six years after the shock. Nonwhite employees' gains are smaller, more volatile, and not different from zero over the aggregated six post-disaster years. White workers appear to disproportionately receive this premium. Consequently, the nonwhite–white compensation gap widens by −0.437 BRL per hour over the subsequent six years ($P = 0.007$). Relative to the sample mean gap of approximately −2.715 BRL per hour, this corresponds to a widening of roughly 16%. Estimates show no evidence that exposed establishments were already on a different trajectory from not-yet-exposed establishments (Fig. 2).

This pattern points to an uneven distribution of post-disaster compensation premia. Natural disasters can operate as local labor-market shocks that change worker compensation by shifting labor demand and supply, mobility, and retention pressures. Such mechanisms can explain why average compensation rises after exposure.[9,11,22,27] They do not, by themselves, explain why this premium accrues disproportionately to white workers within the same establishments. We evaluate below this organizational mechanism against alternative internal factors that may explain why pay differences between white and nonwhite workers rise after climate disasters.

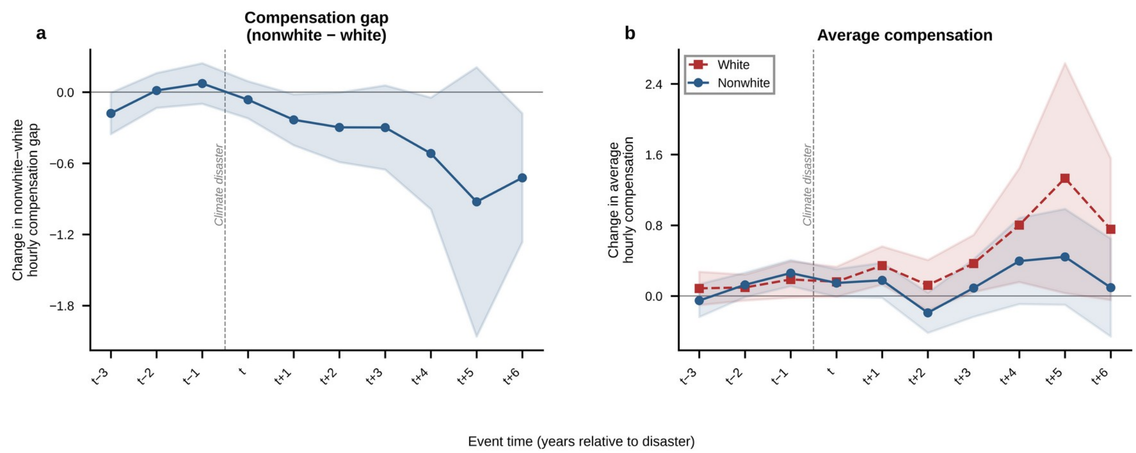

Fig. 2 | Climate disasters, racial compensation gaps and average compensation. a, Event-study estimates for the nonwhite–white hourly compensation gap within establishments. Negative values indicate a wider white compensation advantage. The average pre-exposure estimate is −0.031 BRL per hour ($P = 0.462$), and the average post-exposure estimate is −0.437 BRL per hour ($P = 0.007$). **b,** Event-study estimates for average hourly compensation among white and nonwhite workers. The average post-exposure estimate is 0.555 BRL per hour for white workers ($P = 0.004$) and 0.165 BRL per hour for nonwhite workers ($P = 0.199$). Event time is measured relative to first disaster exposure, and the vertical dashed line marks the year of exposure. Shaded bands show two-sided 95% confidence intervals based on standard errors clustered at the establishment level. *P* values are from two-sided tests of whether the reported average estimate equals zero. Full event-time estimates and joint Wald tests are reported in Extended Data Table 2.

Gap widens among continuing workers

The racial compensation gap could widen if disaster exposure changes who works at affected establishments rather than how continuing workers are compensated.[28-30] Nonwhite workers might disproportionately leave, white workers might be more likely to remain, or post-disaster hiring might alter the racial composition of the workforce. We therefore decompose the workforce relative to each establishment's pre-exposure anchor year, separating workers already employed before the disaster from those hired afterward.

The gap widens most clearly among incumbent workers whose employment relationship predates the disaster. Among anchor-year workers who remain at the establishment after exposure, the nonwhite–white compensation gap widens by −0.113 BRL per hour through four years after exposure (P = 0.019), with no evidence of differential pre-exposure trends. Over the full six-year post-exposure window, the estimate remains negative and similar in magnitude, although less precise (−0.121 BRL per hour, P = 0.052; Fig. 3a). This pattern is central because entrants cannot mechanically generate a widening gap among workers who were already employed by the establishment before the disaster.

Workforce adjustment nevertheless can occur after exposure, so we also test whether the incumbent result reflects differential racial exit. Retention patterns do not indicate sustained disproportionate nonwhite exit. The nonwhite-minus-white difference in the retained share of anchor workers remains close to zero throughout the full post-exposure window (P = 0.540; Fig. 3b). Non-switcher estimates, which restrict the sample further to workers observed only at the same establishment throughout the panel, are directionally consistent but less precise (Extended Data Table 4). Results for newly hired workers are examined separately and do not explain the widening gap observed among workers who were already employed before the disaster. Together, these results indicate that turnover and hiring composition are not sufficient to explain the main pattern.

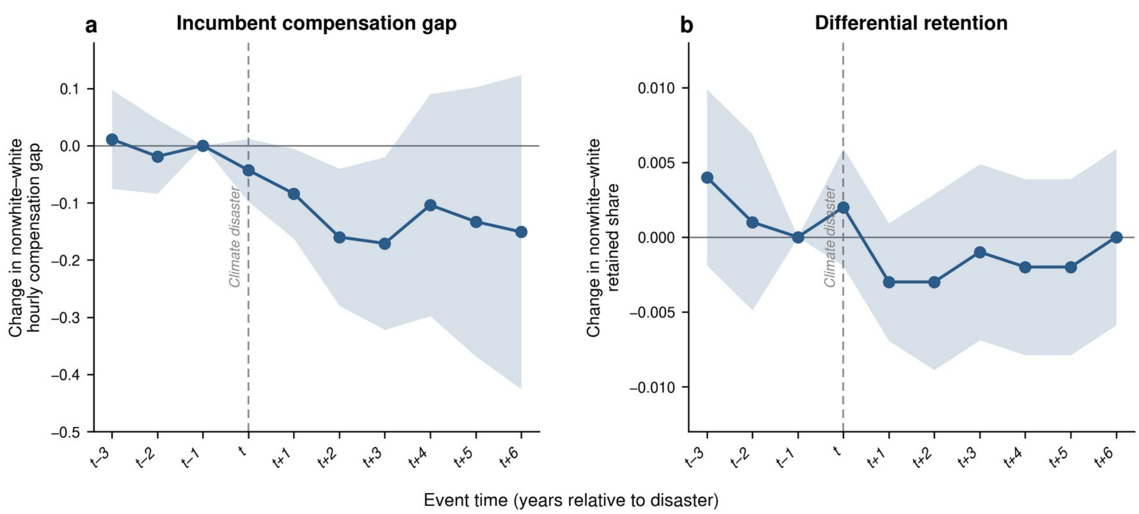

Fig. 3 | The compensation gap widens among existing workers without sustained differential exit. a, Event-study estimates for the nonwhite–white hourly compensation gap among incumbent workers, defined as anchor-year employees who remain employed at the same establishment after first disaster exposure. The average pre-exposure estimate is −0.003 BRL per hour (P = 0.908), the average estimate through four years after exposure is −0.113 BRL per hour (P = 0.019), and the average estimate over the full six-year post-exposure window is −0.121 BRL per hour (P = 0.052). **b,** Event-study estimates for differential retention, defined as the retained share of nonwhite anchor workers minus the retained share of white anchor workers. The average post-exposure estimate is −0.001 over the full six-year window (P = 0.540), indicating no sustained disproportionate nonwhite exit. Negative values in a indicate a wider white compensation advantage. Negative values in b indicate lower retention of nonwhite anchor workers relative to white anchor workers. Event time is measured relative to first disaster exposure, and the vertical dashed line marks the year of exposure. Shaded bands show two-sided 95% confidence intervals based on standard errors clustered at the establishment level. P values are from two-sided tests of whether the reported average estimate equals zero. Full event-time coefficients, non-switcher estimates, entrant estimates and joint Wald tests are reported in Extended Data Table 4.

Gap persists after accounting for occupational and skill composition

One alternative explanation is that disasters widen racial compensation gaps by changing the occupational or skill structure of work inside establishments. Nonwhite workers could be shifted toward lower-paid occupations, white workers could move into higher-paid occupations, or disaster recovery could raise returns in occupations or skill segments disproportionately occupied by white workers at the time of exposure. We test these possibilities in three ways.

First, we construct occupation-weighted racial compensation gaps that hold broad occupational composition constant. The adjusted gap widens by −0.380 BRL per hour after exposure, compared with −0.437 BRL per hour in the baseline estimate, meaning that approximately 87% of the widening remains after accounting for occupational composition (Fig. 4a and Extended Data Table 2). Second, we test whether disaster exposure changes where racial groups sit within the establishment's occupational hierarchy. The estimates do not indicate that nonwhite workers become more concentrated at the bottom of the hierarchy or that white workers show a corresponding movement out of those positions

(Fig. 4b). Additional skill-composition estimates also point away from a downward-sorting explanation: the nonwhite–white difference in lower-skill employment shares does not increase after disaster exposure, lower-skill contracts decline at similar rates for nonwhite and white workers, and higher-skill contracts decline less for nonwhite workers than for white workers (Extended Data Fig. 1). Finally, the racial compensation gap widens within the lower-skill segment itself, with a directionally similar but less precisely estimated pattern in higher-skill work (Fig. 4c and Extended Data Table 2).

Together, these findings suggest that occupational composition accounts for only a limited share of the effect, and that broad occupation- or skill-segment price changes are unlikely to be the primary explanation. The evidence instead points to a widening gap within comparable occupational and skill structures, consistent with the unequal allocation of post-disaster compensation premia inside establishments.

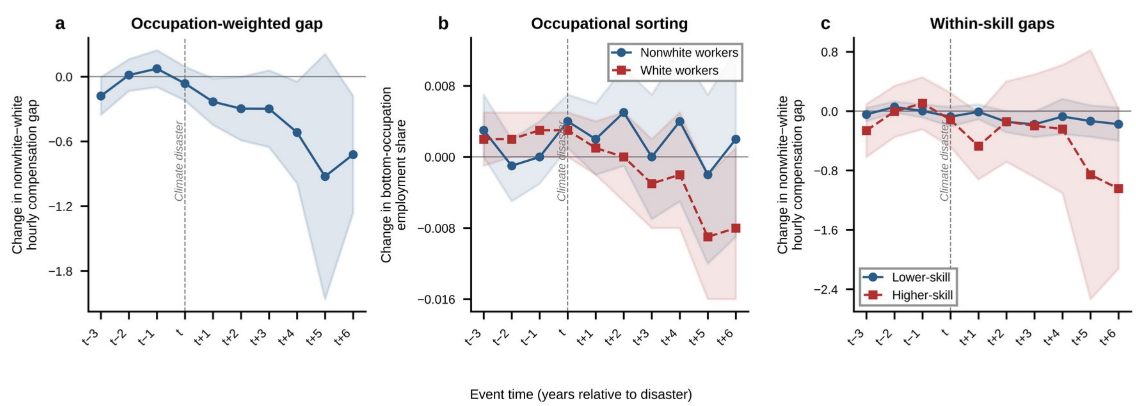

Fig. 4 | The widening compensation gap persists after accounting for occupational and skill composition. a, Event-study estimates for the nonwhite–white hourly compensation gap using an occupation-weighted measure that adjusts for three-digit occupational composition. The average pre-exposure estimate is −0.016 BRL per hour (P = 0.678), and the average post-exposure estimate is −0.380 BRL per hour (P = 0.015). **b,** Event-study estimates for the share of nonwhite and white workers employed in bottomranked occupations. For nonwhite workers, the average pre-exposure estimate is 0.001 (P = 0.357), and the average post-exposure estimate is 0.002 (P = 0.465). For white workers, the average pre-exposure estimate is 0.002 (P = 0.001), and the average post-exposure estimate is −0.003 (P = 0.224). **c,** Event-study estimates for the nonwhite–white hourly compensation gap within lower-skill and higher-skill occupations. The average post-exposure estimate is −0.113 BRL per hour in lower-skill occupations (P = 0.050) and −0.439 BRL per hour in higher-skill occupations (P = 0.117). Negative values in a and c indicate a wider white compensation advantage. Event time is measured relative to first disaster exposure, and the vertical dashed line marks the year of exposure. Shaded bands show two-sided 95% confidence intervals based on standard errors clustered at the establishment level. P values are from two-sided tests of whether the reported average estimate equals zero. Full event-time estimates and joint Wald tests are reported in Extended Data Table 2.

Contract changes do not drive the gap

A remaining possibility is that the widening reflects changes in employment arrangements rather than unequal compensation allocation. Exposed establishments, for example, could shift nonwhite workers disproportionately into temporary or part-time contracts. We find little evidence for this account. The post-exposure changes in nonwhite–white temporary-contract and part-time-contract share gaps are economically small and indistinguishable from zero (Extended Data Fig. 2).

Together, these results locate the widening within ongoing employment relationships and comparable occupational, skill and contract structures. The evidence points away from turnover, hiring composition, occupational sorting and contract-form changes as primary explanations. The remaining pattern is one in which establishments respond to climate shocks with higher average compensation but distribute those increases unequally across racial groups. If this interpretation is correct, the widening should be largest where post-disaster compensation pressure is strongest, which we evaluate next.

Where the gap is largest

The widening is concentrated in large, urban labor markets. In the most populous municipalities, the post-exposure average racial compensation gap widens by −0.459 BRL per hour (P = 0.009) (Extended Data Table 2). By contrast, lower-population municipalities do not show a wider white compensation advantage, and the estimate moves in the opposite direction (Fig. 5a). Similarly, the gap widens in highly urbanized municipalities and moves in the opposite direction in low-urbanization areas (Extended Data Fig. 3).

Market concentration is consistent with this interpretation. We split municipalities by pre-exposure local market concentration, measured by the HHI of employers' employment shares. If the widening mainly reflected workers having few outside options, the effect should be strongest in high-HHI markets. Instead, the largest point estimate appears in low-HHI, more competitive markets, while the middle-HHI estimate is close to zero and the high-HHI estimate is directionally similar but less precise (Fig. 5b and Extended Data Table 2). This pattern does not support a simple outside-options explanation. It is more consistent with the view that competitive labor markets create larger compensation adjustments, within which unequal allocation can operate.

The racial compensation gap widens after disaster exposure in tradable goods and resources (−0.281 BRL per hour, P = 0.034) and is directionally similar in market services (−0.347 BRL per hour, P = 0.069; Extended Data Fig. 4). Infrastructure, network industries and construction—sectors in which disaster recovery can sustain or raise labor demand—show no clear post-exposure widening. These differences suggest that the widening of the racial compensation gap is not an automatic consequence of disaster exposure, but is concentrated where post-disaster labor-market disruption appears to generate larger compensation adjustments.

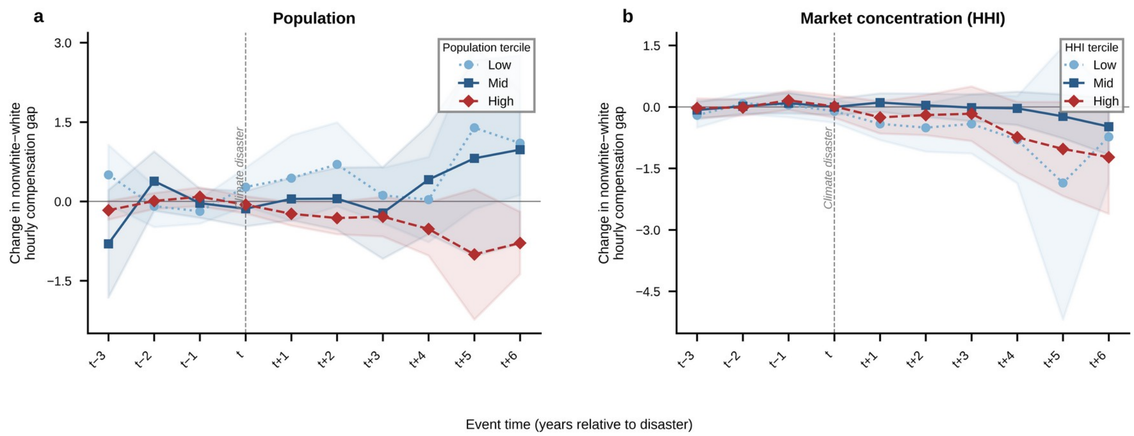

Fig. 5 | Compensation-gap changes by municipality population and market concentration. a, Event-study estimates for the nonwhite–white hourly compensation gap by terciles of municipality population. The average post-exposure estimate is 0.579 BRL per hour in the low-population tercile (P = 0.032), 0.278 BRL per hour in the middle-population tercile (P = 0.475), and −0.459 BRL per hour in the high-population tercile (P = 0.009). b, Event-study estimates for the same compensation gap by terciles of market concentration. Concentration is measured using the Herfindahl–Hirschman Index (HHI), computed from employers' employment shares within each municipality and averaged over the three years before the municipality's first observed disaster exposure; lower HHI values indicate more competitive local labor markets. The average post-exposure estimate is −0.690 BRL per hour in the low-HHI tercile (P = 0.070), −0.089 BRL per hour in the middle-HHI tercile (P = 0.555), and −0.515 BRL per hour in the high-HHI tercile (P =

0.126). Negative values indicate a wider white compensation advantage. Event time is measured relative to first disaster exposure, and the vertical dashed line marks the year of exposure. Shaded bands show two-sided 95% confidence intervals based on standard errors clustered at the establishment level. P values are from two-sided tests of whether the reported average estimate equals zero. Full event-time estimates and joint Wald tests are reported in Extended Data Table 2.

Stronger shocks, larger gaps

The effect is also larger after more severe disasters. When disasters are stratified by terciles of reported fatalities, establishments exposed to the highest-fatality events show the largest increase in the racial compensation gap, whereas establishments in the lowest-fatality tercile show little post-exposure movement (Fig. 6 and Extended Data Table 2). This gradient is consistent with the interpretation that the increase in the compensation gap is related to the intensity of the underlying shock rather than only to average differences around disaster timing.

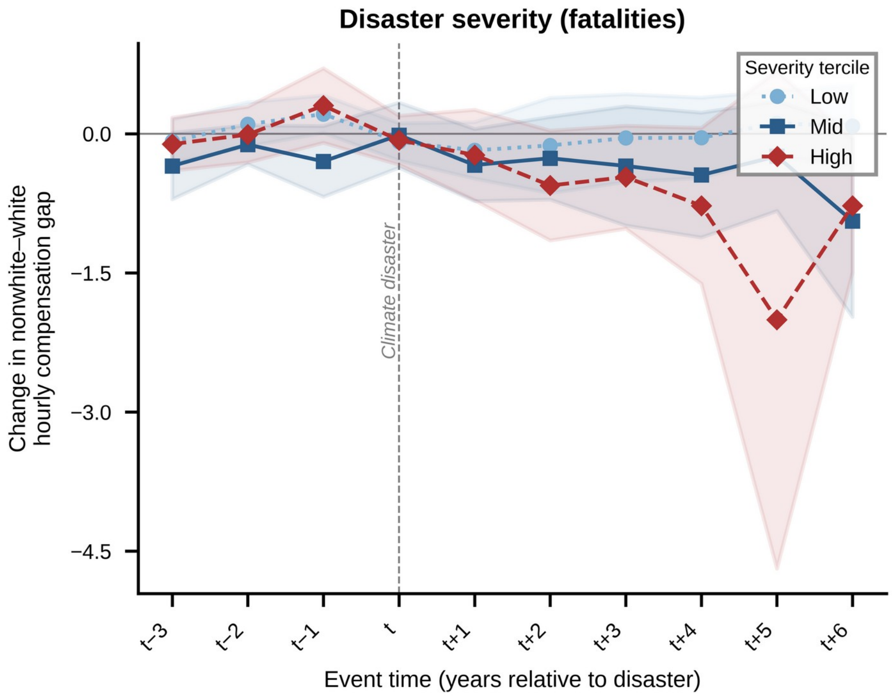

Fig. 6 | Compensation-gap changes by disaster severity. Event-study estimates for the nonwhite–white hourly compensation gap, stratified by terciles of disaster severity measured using reported disaster-related fatalities among treated establishments. The average post-exposure estimate is −0.041 BRL per hour in the low-fatality tercile ($P = 0.729$), −0.370 BRL per hour in the mid-fatality tercile ($P = 0.072$), and −0.697 BRL per hour in the high-fatality tercile ($P = 0.060$). Negative values indicate a wider white compensation advantage. Event time is measured relative to first disaster exposure, and the vertical dashed line marks the year of exposure. Shaded bands show two-sided 95% confidence intervals based on standard errors clustered at the establishment level. P values are from two-sided tests of whether the reported average estimate equals zero. Full event-time estimates and joint Wald tests are reported in Extended Data Table 2.

Discussion

Climate disasters can deepen inequality through the way firms distribute post-disaster compensation increases. Exposed establishments pay higher compensation on average, but those premia accrue unevenly across racial groups within the same workplace. The widening is concentrated among continuing workers, persists within comparable occupational and skill structures, and is more pronounced after the most severe disasters and in dense, competitive local labor markets. Together these patterns identify firms as organizational sites where climate recovery can produce racial stratification, and they point away from explanations based on who enters or exits, occupational reshuffling, or limited outside options alone.

These results shift the interpretation of climate adaptation and disaster resilience from aggregate recovery to distributional recovery. Exposed establishments continue operating and raise average compensation, yet these aggregate signals coexist with worsening inequality inside the same organizations, even as the incumbent workforce contracts. A positive average effect is therefore not sufficient evidence of inclusive recovery. In this setting, the compensation growth that accompanies post-disaster adjustment conceals a widening racial gap among comparable workers within affected establishments.

The setting matters. These estimates come from large formal-sector establishments in Brazil and should not be read as universal. Their broader relevance lies in the channel they identify. Large employers organize substantial shares of labor-market opportunity in many economies, and climate shocks can require firms to adjust compensation, staffing, and retention. Where those adjustments occur amid pre-existing social inequality, the ordinary operation of compensation systems can convert environmental shocks into racially unequal compensation outcomes.

At the same time, the evidence cannot reveal the precise compensation channel through which this divergence emerges. The adjustment could reflect differential access to raises, overtime, bonuses, promotion-linked pay, bargaining outcomes, or other compensation practices that are not separately observed in the administrative records. Nor can we directly observe the motives, routines, or discretionary judgments behind these internal decisions. These limitations define the boundary of the study. By showing that the gap widens within ongoing employment relationships and comparable occupational structures, the results narrow the set of viable explanations and point to compensation allocation inside establishments as the channel through which inequality emerges. Future research could examine those internal factors directly, using personnel records, compensation audits, or organizational case studies to trace where the divergence operates.

Average recovery can obscure unequal recovery. When post-disaster compensation gains are distributed unevenly inside firms, adaptation can reproduce racial inequality through the ordinary institutions of work. The distribution of those gains should therefore be treated as central to understanding the social effects of climate change.

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Methods

Data and sample construction

We construct the analysis dataset from two complementary files in RAIS (*Relação Anual de Informações Sociais*), a matched employer–employee database compiled by the Brazilian Ministry of Labor that provides a near-census of the formal labor market. The establishment-level file records employment counts, addresses, industry classification (CNAE codes), organizational form and establishment identifiers. The labor-contract-level file records workers' demographic characteristics (gender, race, disability status, education), occupation (CBO codes), as well as contract-specific information, including contract type, contracted weekly hours, and nominal remuneration.

Sample restrictions. The sample covers 2003–2017. Starting from the raw RAIS data, we code basic categorical variables, harmonize variable definitions and labels across years, and normalize the data at the worker-establishment-city-year level. We then retain establishments classified as private organizations with at least 100 employees in at least one year; below this threshold, employment information is more prone to misreporting. We exclude establishments associated with multiple addresses because our identification strategy relies on geographic exposure to climate shocks. From these establishments, we extract every labor contract over the full sample period and keep those active at year end. When a worker holds multiple contracts with the same employer in a given year, we retain the highest-compensation contract, breaking ties by duration, then working hours, then at random. Establishment-level contract counts include every active year-end contract that satisfies these restrictions; compensation and compensation-gap measures additionally require valid, non-zero end-of-year compensation. The final panel comprises 99,323 unique establishments and 965,007 establishment–year observations.

Measures

Race classification. Race is reported by the employer in RAIS. We classify workers reported as *branca* as white and workers reported as *preta*, *parda*, *amarela* or *indígena* as nonwhite. Workers whose race is unidentified enter establishment-level contract counts but are excluded from race-specific compensation and compensation-gap measures.

Average Compensation. We measure hourly compensation as December nominal remuneration divided by estimated monthly contracted hours, where monthly hours equal contracted weekly hours multiplied by 4.34, the average number of weeks per month. The resulting measure, expressed in BRL per hour, underlies our establishment-level, race-specific and worker-category outcomes. We set hourly compensation to missing when remuneration or contracted hours are missing, or when contracted hours are non-positive; contracts reporting zero December remuneration are retained, consistent with their inclusion in the establishment head-count variables. For the worker-category compensation outcomes, we winsorize hourly compensation within calendar year at the 1st and 99th percentiles, using common thresholds across racial groups. Because the establishment-level specifications use unwinsorized compensation while the worker-category specifications winsorize it, the fact that both yield a significant post-exposure widening indicates the estimate is not driven by zero-payroll establishment-years or extreme compensation values.

Compensation gap. Our primary outcome is the average hourly compensation for nonwhite workers minus average hourly compensation for white workers within the same establishment–year. Because nonwhite workers earn less on average, this gap is typically negative. A negative exposure-effect coefficient therefore indicates a wider white compensation advantage and a deterioration in relative compensation for nonwhite workers. The baseline establishment-year gap is calculated whenever valid compensation means are observed for both white and nonwhite workers. For the worker-category compensation-gap analyses, we additionally require at least three valid-compensation workers in each racial group within the relevant establishment–cohort–year worker-category cell. We relax to five and ten in robustness tests.

Employment. We measure establishment employment as the log of n_{it}, the number of active year-end contracts at establishment *i* in year *t*.

Worker categories

We define worker categories relative to first disaster exposure. We index establishments by *i*, the exposure cohort by g (the calendar year of first disaster exposure) and calendar years by *t*. Event time is $k = t - g$ and the anchor year is $g - 1$. For a worker category *c* and race r $\in$ {NW, W} (nonwhite, white), let n_c^r and w_c^r denote the number of active year-end contracts and the average hourly compensation of *race*-*r* workers in category *c* at establishment *i* in cohort *g* and year *t*.

The categories are defined as follows.

All workers. The set W_{it} of all active year-end contracts at establishment i in year t. This category is not cohort-specific and yields the primary establishment-level outcomes.

Anchor workers (A). The anchor set A_{ig} comprises every worker employed at establishment i in the anchor year $g - 1$. Its counts and average compensation are fixed within a cohort and describe the pre-exposure workforce.

Retained anchor workers—incumbents (R). R_{igt} = {j ∈ A_{ig}: j is employed at establishment i in year t}: anchor workers still present in year t. Their number declines over time as anchor workers leave. Unlike the anchor set, whose values are fixed at $g – 1$, their compensation is measured in the current year t, capturing how the compensation of workers who remain evolves after exposure.

Non-switchers (NS). $NS_{igt} \subseteq R_{igt}$ restricts incumbents to workers who appear only at establishment i whenever they are observed in RAIS over 2003–2017. We define this status globally rather than within a cohort.

Post-anchor entrants (E). E_{igt} = {j employed at establishment i in year t : $j \notin A_{ig}$}: workers observed at the establishment in year t who were not in the anchor-year workforce. In post-exposure years, this category captures workforce renewal. Because entrants by construction have no pre-anchor presence, entrant outcomes are evaluated only in post-anchor years ($t \geq g$); an individual is counted as an entrant from the first year they are observed at the establishment after the anchor year, so pre-anchor entrant cells are empty and do not enter any estimate.

Race-normalized selection measures. To test whether post-exposure selection differs by race, we compare race-specific shares relative to the anchor workforce. The retained-share gap is NR,NW/NA,NW − NR,W/NA,W, and the non-switcher-share gap is

NNS,NW/NA,NW − NNS,W/NA,W, where Ncr is the number of race-*r* workers ($r \in$ {NW, W}) in category *c* at establishment *i* in year *t*, with A denoting the anchor workforce (*g* − *1*), R retained incumbents and NS non-switchers. Because each race is normalized by its own anchor stock, these gaps isolate differential retention from differences in baseline composition. For entrants, the hiring-composition gap is the nonwhite share among post-anchor entrants minus the nonwhite share of the anchor workforce, NE,NW/(NE,W + NE,NW) − NA,NW/(NA,W + NA,NW).

Occupation-weighted compensation gap. To account for compensation variation across occupations, we construct occupation-weighted compensation-gap measures. We define occupations at the three-digit CBO level (192 categories). For each occupation–establishment–year cell in which both racial groups are present, we calculate the within-occupation compensation gap and aggregate to the establishment level, weighting by relative occupation frequency. We additionally define high-skill occupations as those requiring tertiary education and classify the remainder as low-skill.

Compensation-distribution shares. We construct variables measuring the share of each racial group at different points of the within-establishment compensation distribution. We rank occupations by mean hourly compensation within each establishment using only workers observed in the establishment's first sample year, normalize ranks to (0, 1], and freeze them at that baseline. For each racial group we then compute the share of its workers employed in lower-ranked occupations—those with normalized rank ≤ 0.5.

Industry and microregion. We define industry at the two-digit CNAE division level (87 divisions in the CNAE 2.0 classification). Microregions are sub-state statistical units defined by IBGE (the Brazilian Institute of Geography and Statistics) prior to 2017, which group contiguous municipalities with shared economic and spatial characteristics (558 nationwide). For the worker-category analysis both are fixed at the anchor year. Descriptive statistics for all main variables appear in Extended Data Table 1.

Climate disasters. We identify climate disasters using the Emergency Events Database (EM-DAT), maintained by the Centre for Research on the Epidemiology of Disasters, and the Global Dataset of Geocoded Disaster Locations (GDIS).[31,32] EM-DAT records event-level information including disaster type, timing, reported fatalities and reported economic damages. GDIS geocodes EM-DAT disaster locations, enabling us to assign exposure below the national or state level. The analysis focuses on acute hydrometeorological disasters whose timing and spatial footprint can be assigned precisely to establishments: floods, storms and landslides. We exclude slower-onset events such as droughts, because their exposure boundaries and onset timing are too diffuse for an establishment-level event-study design. The final linked disaster file contains 248 geocoded disaster locations from 63 unique EM-DAT events during the 2003–2017 analysis window.

Geocoding and exposure definition

We geocode establishments using address information reported in RAIS. We standardize address strings and submit them to the Google Maps Platform Geocoding API to obtain latitude and longitude. We retain a geocoded establishment only when the returned location can be reconciled with the RAIS municipality and state identifiers. We take disaster locations from GDIS coordinates and project all coordinates in a common latitude–longitude system.

For each establishment–year, we compute the Haversine distance to every qualifying disaster location. An establishment is classified as treated in the first year in which a flood, storm or landslide occurs within 25 km. This radius balances local precision against attenuation from geocoding and event-location error. Narrower radii risk missing establishments affected by disasters whose recorded locations are approximate, while wider radii increase the probability of classifying unaffected establishments as exposed. The distance-based definition also avoids reliance on large administrative units, which can misclassify establishments near municipal or regional boundaries. Under this rule, 10,733 establishments experience at least one qualifying exposure during the sample period; the remaining 88,590 are never exposed during the sample period.

Identification and estimation

Baseline staggered difference-in-differences. The baseline design compares within-establishment changes at newly exposed establishments to changes at establishments not yet exposed in the same calendar years. We estimate dynamic exposure effects using the Callaway and Sant'Anna estimator for settings with staggered treatment.[33] In practice, we fit the estimator with the establishment as the panel unit, calendar year as the time variable and the first-exposure year as the cohort (group) variable, using both never-treated and not-yet-treated establishments as the comparison group:

$$\mathrm{ATT}(g,t) = \mathbf{E}[Y_{it}(g) - Y_{it}(0) \mid G_i = g] \tag{1}$$

Equation (1) is the group–time average treatment effect on the treated (ATT) for establishments first exposed in cohort year g, evaluated in year t. Here i indexes establishments and t calendar years; Y denotes the outcome (racial compensation gap, average compensation or occupation-weighted compensation gap); and $G_i = g$ indicates first exposure in year g. We aggregate the ATT(g,t) into an event-study profile by event time k.

The identifying assumption is that, absent disaster exposure, newly exposed establishments would have followed the same outcome path as the comparison establishments used in the same calendar years. We cluster standard errors at the establishment level, assess pre-exposure trends with joint Wald tests over pre-exposure coefficients, and use joint Wald tests over post-exposure coefficients to evaluate post-exposure changes.

We report estimates for three pre-exposure years ($k = -3$ to $k = -1$) and six post-exposure years ($k = 0$ to $k = +6$). In the Callaway–Sant'Anna specification, pre-exposure ATT estimates are retained for diagnostic testing rather than normalized away; in the high-dimensional fixed-effects specification, $k = -1$ is the omitted reference period. The reference is the not-yet-treated comparison group rather than a fixed pre-period, so the pre-exposure lead at $k = -1$ is itself estimated rather than set to zero. To keep treatment interpretable as the response to a first disaster shock, we censor observations at and after a second qualifying exposure for the same establishment. RAIS does not provide valid year-end employment-contract data for the state of Pernambuco in 2017, so we omit the Pernambuco–2017 observations.

Alternative estimation: high-dimensional fixed effects. We also estimate a high-dimensional fixed-effects event-study model:

$$y_{\mathrm{it}} = \sum_k \delta_k 1\{t - g_i = k\} + \alpha_i + \alpha_t + \alpha_{jt} + \alpha_{rt} + \varepsilon_{\mathrm{it}} \quad (2)$$

where k indexes event years before and after first climate-disaster exposure. Event-time coefficients span $k = -3$ to $k = +6$ relative to first exposure, with $k = -1$ omitted so that $\delta_{-1} = 0$. The model absorbs establishment (α_i), calendar-year (α_t), microregion-by-year (α_{rt}; 558 IBGE microregions) and industry-by-year (α_{jt}; CNAE two-digit divisions) fixed effects, with standard errors clustered at the establishment level. We use joint F-tests over the post-exposure coefficients ($k = 0$ to $k = +6$) to assess whether the racial compensation gap widens significantly; pre-trend F-tests cover $k = -3$ and $k = -2$.

Worker-category estimation. Because anchor, incumbent, non-switcher and entrant status are defined relative to the cohort anchor year $g - 1$, we evaluate these outcomes on a cohort-stacked panel with one observation per establishment, cohort and year (i, g, t). Within each cohort slice, establishments first exposed in year g enter as treated. Establishments serve as comparisons for cohort g only if they are never exposed during the sample or are first exposed more than six years after g, after cohort g's event window has closed. Establishments exposed within that event window are excluded from cohort g's comparison group rather than contributing until their own exposure year.

We estimate stacked event-study models for worker-category outcomes using high-dimensional fixed effects, with standard errors clustered at the establishment level to account for repeated observations of the same establishment across years and cohort stacks. For incumbent and non-switcher workers, compensation is determined primarily by pre-existing employment relationships and adjusts slowly due to contractual and managerial considerations. The preferred specification absorbs only establishment-by-cohort and cohort-by-year fixed effects, which already account for most relevant time-invariant and cohort-specific variation. Adding micro-region-by-year or industry-by-year fixed effects on top of these would leave little remaining variation to identify the compensation response to the disaster and risks absorbing part of the very effect we want to estimate. For the employment-count and race-normalized selection diagnostics, by contrast, hiring and separation decisions are considerably more responsive to local labor market conditions and sector-wide trends unrelated to the disaster itself. We therefore include micro-region-by-year and industry-by-year fixed effects in these specifications to net out these confounding aggregate local and sectoral dynamics, allowing us to better isolate the race-differential margin in the firm-level employment response attributable to the shock.

Entrant outcomes require separate specifications because entrants do not exist as a pre-exposure category and therefore have no within-establishment pre-period and no pre-exposure reference period. The entrant compensation-gap column is a within-post diagnostic absorbing establishment-by-cohort, cohort-by-year, microregion-by-year and industry-by-year fixed effects, with event time 0 as the omitted reference period. This column shows whether the entrant racial compensation gap changes across the post-exposure window. The entrant employment-count column is a post-anchor level contrast absorbing cohort-by-year, microregion-by-year and industry-by-year fixed effects with anchor-year controls but no establishment-by-cohort fixed effect, because establishment-by-cohort fixed effects would absorb the between-establishment hiring-volume difference the column is intended to estimate. The entrant hiring-composition column is a post-anchor composition contrast relative to the anchor workforce, absorbing cohort-by-year fixed effects only.

For each outcome we report a pooled post-exposure average and a shorter-run post-exposure average (k = 0 to +4), together with pre-exposure averages and joint Wald tests for the categories defined before exposure. The fixed effects, anchor-year controls, sample sizes and robustness checks for every worker-category specification are reported in Extended Data Table 4. All P values reported in the study are from two-sided tests unless otherwise stated.

Heterogeneity analyses

We stratify the sample along five dimensions:

Population and urbanization. We compute population and urbanization terciles from city-level Brazilian Census data, averaged across the 2000 and 2010 waves.

Employer concentration. To proxy for workers' outside options, we construct a city-by-year employer concentration index defined as the Herfindahl–Hirschman Index (HHI) of employers' employment shares, averaged over three pre-exposure years. We define employer-concentration terciles using the pre-exposure city-level HHI.

Disaster severity. We compute disaster-severity terciles using reported fatalities associated with each climate disaster, collected from EM-DAT.

Industry sector. We classify establishments into four sectors on the basis of CNAE two-digit codes: tradable goods and resources (agriculture, mining and manufacturing; CNAE 2.0 sections A–C), infrastructure and construction (utilities and construction; sections D–F), market services (trade, transport, hospitality, information, finance, professional and administrative services; sections G–N, R–S) and public and social services (public administration, education, health and domestic services; sections O–Q, T–U).

Data availability

This study uses restricted-access identified RAIS employer–employee microdata obtained under institutional authorization and confidentiality requirements administered by the Brazilian Ministry of Labor. The authors are not permitted to redistribute the identified RAIS records, establishment identifiers, geocoded establishment coordinates or the establishment-level linked RAIS–disaster panel, because these data could permit reidentification of firms, establishments or workers. Researchers seeking access to the RAIS microdata must apply directly to the relevant Brazilian government authority and comply with applicable confidentiality and data-use requirements. Climate-disaster data from EM-DAT are publicly available at https://www.emdat.be/. Upon reasonable request, we will make available replication code, variable-construction documentation and non-identifying aggregate outputs sufficient to reproduce the reported tables and figures for researchers with authorized access to the restricted RAIS data, subject to the data-use restrictions described above.

Code availability

Replication code used to construct the analysis files and reproduce the reported estimates will be made available to editors and reviewers during peer review and deposited in a public repository upon publication, excluding any restricted identifiers, geocoded establishment coordinates or confidential data extracts.

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Author contributions

L.B., X.L. and L.N. contributed to the conceptual development of the study and the interpretation of the results. L.N. assembled and processed the administrative labor data and constructed the worker-level analytic variables, including the worker-category, retention, and compensation measures. L.B. geolocated the establishments, assembled the disaster data, constructed the disaster-exposure measures, and conducted the main analyses. L.B. wrote the submitted manuscript, with earlier drafting from X.L. X.L. and L.N. reviewed and edited the manuscript. All authors approved the final manuscript.

Competing interests

The authors declare no competing interests.

Additional information

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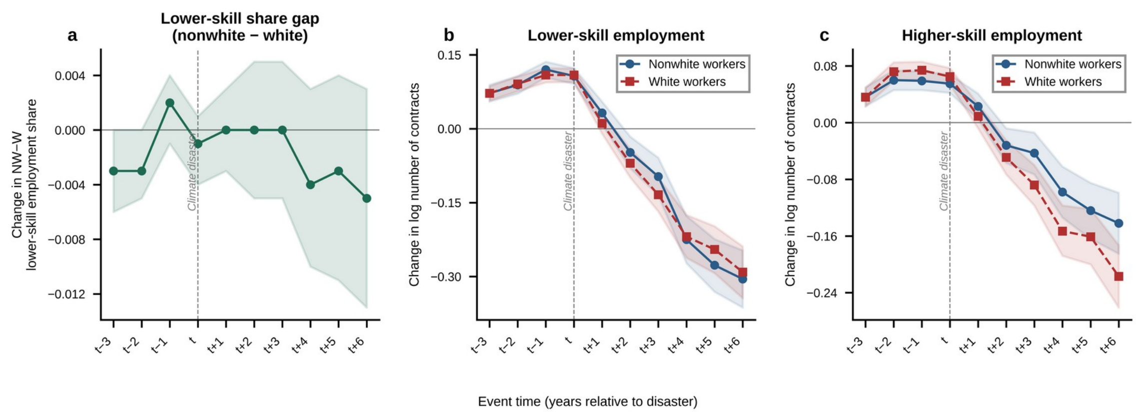
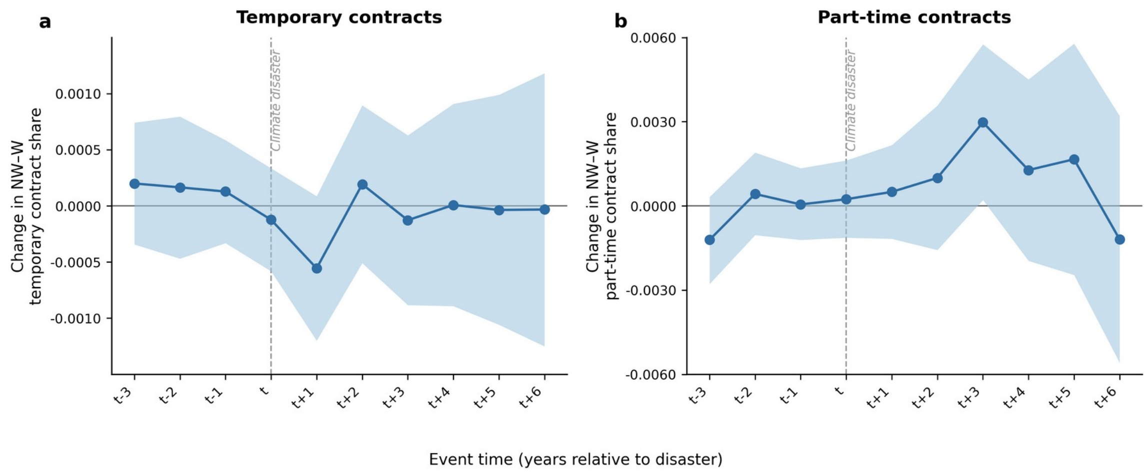
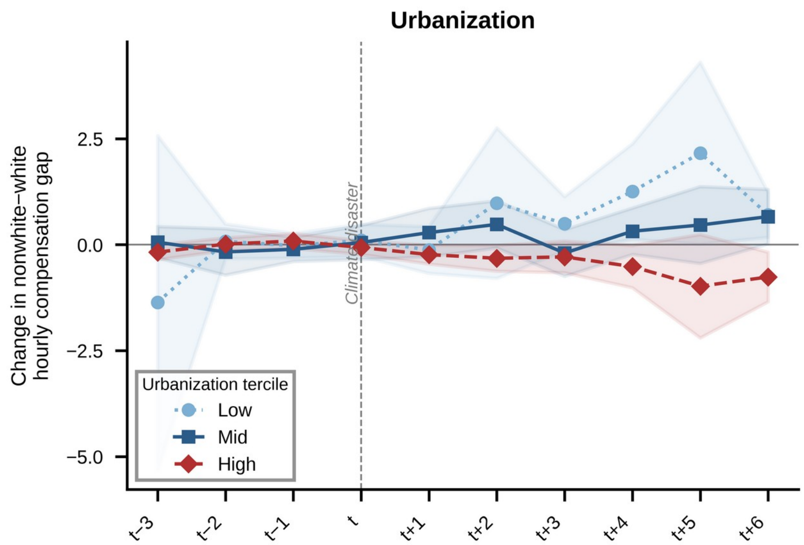


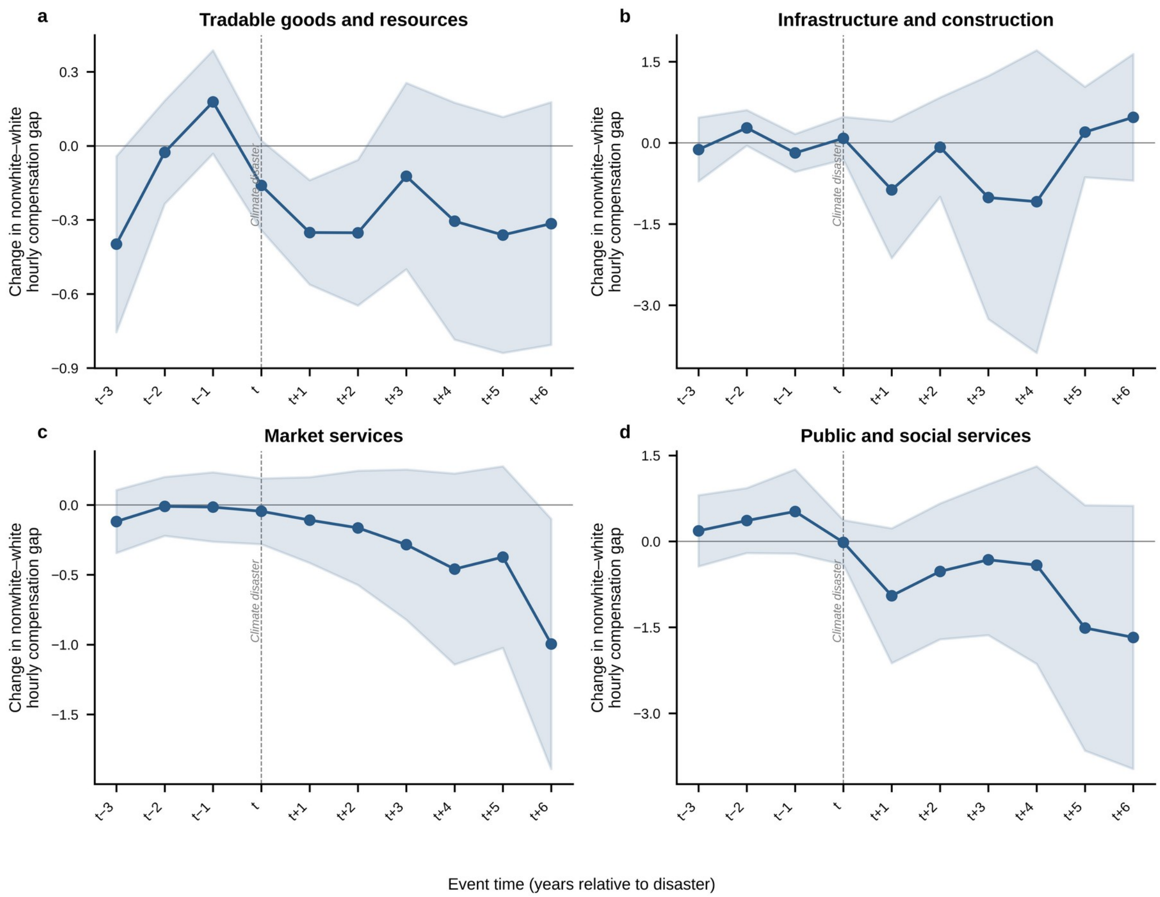
Extended Data Fig. 1 | Skill composition and employment adjustment after climate disasters. a, Event-study estimates for the nonwhite–white difference in the share of workers employed in lower-skill occupations. Positive values indicate that nonwhite workers became more concentrated than white workers in lower-skill employment. The average pre-exposure difference is −0.001 (P = 0.045), and the average post-exposure difference is −0.002 (P = 0.335). **b,c**, Event-study estimates for the log number of contracts among nonwhite and white workers in lower-skill occupations (**b**) and higher-skill occupations (**c**). In lower-skill employment, the average post-exposure nonwhite–white difference is 0.004 (P = 0.843). In higher-skill employment, the average post-exposure nonwhite–white difference is 0.033 (P = 0.040). Estimates are normalized to the year before first exposure, and the vertical dashed line marks the first year of exposure. Shaded bands show two-sided 95% confidence intervals based on standard errors clustered at the establishment level. P values are from two-sided tests of whether the reported average estimate equals zero.



Extended Data Fig. 2 | Changes in contract-form shares after climate disasters. a, Event-study estimates for the change in the nonwhite–white difference in the share of temporary contracts within establishments. The average pre-exposure estimate is 0.0002 (P = 0.130), and the average post-exposure estimate is −0.0001 (P = 0.741). b, Event-study estimates for the change in the nonwhite–white difference in the share of part-time contracts within establishments. The average pre-exposure estimate is −0.0002 (P = 0.491), and the average post-exposure estimate is 0.0009 (P = 0.384). Positive values indicate an increase, relative to the year before exposure, in the nonwhite-minus-white difference in the relevant contract-share outcome. Event time is measured relative to first disaster exposure, and the vertical dashed line marks the year of exposure. Shaded bands show two-sided 95% confidence intervals based on standard errors clustered at the establishment level. P values are from two-sided tests of whether the reported average estimate equals zero.



Extended Data Fig. 3 | Compensation-gap changes by urbanization. Event-study estimates for the nonwhite–white hourly compensation gap, stratified by terciles of local urbanization. The average post-exposure estimate is 0.796 BRL per hour in the low-urbanization tercile (P = 0.049), 0.295 BRL per hour in the middle-urbanization tercile (P = 0.151), and −0.452 BRL per hour in the high-urbanization tercile (P = 0.009). Negative values indicate a wider white compensation advantage. Event time is measured relative to first disaster exposure, and the vertical dashed line marks the year of exposure. Shaded bands show two-sided 95% confidence intervals based on standard errors clustered at the establishment level. P values are from two-sided tests of whether the reported average post-exposure estimate equals zero.



Extended Data Fig. 4 | Racial compensation-gap changes by industry group after climate disasters. Event-study estimates for the nonwhite–white hourly compensation gap within establishments by industry group: **a**, tradable goods and resources; **b**, infrastructure, network industries and construction; **c**, market services; and **d**, public and social services. The average pre-exposure estimate is −0.082 BRL per hour in tradable goods and resources (P = 0.270), −0.011 in infrastructure, network industries and construction (P = 0.865), −0.004 in market services (P = 0.916), and 0.356 in public and social services (P = 0.138). The average post-exposure estimate is −0.281 BRL per hour in tradable goods and resources (P = 0.034), −0.327 in infrastructure, network industries and construction (P = 0.463), −0.347 in market services (P = 0.069), and −0.773 in public and social services (P = 0.240). Negative values indicate a wider white compensation advantage. Event time is measured relative to first disaster exposure, and the vertical dashed line marks the year of exposure. Shaded bands show two-sided. 95% confidence intervals based on standard errors clustered at the establishment level. P values are from two-sided tests of whether the reported average estimate equals zero.

Variable	Unit	N	Mean	S.D.	Min.	Max.
Average compensation	i, t	965,007	10.769	18.127	0.000	5,765.663
Average compensation – White	i, t	907,704	12.010	20.121	0.000	5,765.663
Average compensation – Nonwhite	i, t	862,660	9.403	14.329	0.000	1,547.105
Racial compensation gap	i, t	814,124	−2.715	11.479	−1,543.498	832.383
Racial compensation gap – occupation-weighted	i, t	792,008	−0.612	9.232	−889.050	928.899
Racial compensation gap – low-skill	i, t	785,705	−0.995	4.911	−1,367.396	627.696
Racial compensation gap – high-skill	i, t	529,560	−4.431	19.067	−2,290.151	1,633.432
Number of workers	i, t	965,007	209.840	456.521	1.000	28,896
Number of white workers	i, t	965,007	116.110	277.730	0.000	24,216
Number of nonwhite workers	i, t	965,007	85.811	321.546	0.000	85,477
Number of temporary workers	i, t	965,007	2.104	39.159	0.000	9,116
Number of part-time workers	i, t	965,007	8.339	144.440	0.000	46,439
Number of lower-skill workers	i, t	965,007	168.571	391.630	0.000	25,902
Average compensation – incumbent	i, g, t	5,313,442	13.107	23.411	0.411	7,811.165
Average compensation – non-switcher	i, g, t	4,734,427	12.587	22.372	0.396	7,716.732
Average compensation – entrant	i, g, t	4,228,804	10.676	18.583	0.423	6,014.706
Racial compensation gap – incumbent	i, g, t	4,133,641	−2.946	15.334	−4,602.829	2,051.990
Racial compensation gap – non-switcher	i, g, t	2,900,722	−3.010	18.767	−4,603.139	1,914.956
Racial compensation gap – entrant	i, g, t	3,484,155	−2.436	11.928	−1,543.731	1,105.937
Number of incumbent workers	i, g, t	6,437,188	96.090	378.663	0.000	92,954
Number of non-switcher workers	i, g, t	6,437,188	26.520	109.136	0.000	15,714
Number of entrant workers	i, g, t	6,437,188	95.913	319.565	0.000	92,954

Extended Data Table 1 | Summary statistics. Descriptive statistics for the variables used in the analysis. Compensation is measured in Brazilian reais (BRL) per hour. The racial compensation gap is the nonwhite−white difference in average hourly compensation within an establishment; negative coefficients indicate a widening white compensation advantage. Establishment-level variables are measured at the establishment–year level (*i*, *t*); worker-category variables (incumbent, non-switcher and entrant) are measured at the establishment–cohort–year level (*i*, *g*, *t*), where *i* indexes establishments, g the anchor-year cohort and t the calendar year. Entrant counts are zero before the anchor year. Zero values for the compensation variables correspond to establishment-years with zero reported December payroll, which are retained in the sample (see Methods).

	Average compensation			Number of workers	Racial compensation gap												
	All	White	Nonwhite		All	Occupation-weighted	Skill		Municipality population			Market concentration			Disaster intensity		
							Low	High	Low	Mid	High	Low	Mid	High	Low	Mid	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
	0.129	0.123	0.111	9.621	−0.031	−0.016	0.004	−0.054	0.075	−0.151	−0.024	−0.024	0.011	0.039	0.078	−0.254	0.061
Pre-exposure avg. (−3, −1)	(0.041)	(0.045)	(0.043)	(1.063)	(0.042)	(0.038)	(0.021)	(0.086)	(0.147)	(0.160)	(0.043)	(0.074)	(0.053)	(0.072)	(0.046)	(0.098)	(0.090)
	[0.002]	[0.006]	[0.011]	[<0.001]	[0.462]	[0.678]	[0.832]	[0.532]	[0.610]	[0.347]	[0.573]	[0.741]	[0.842]	[0.587]	[0.094]	[0.010]	[0.494]
	0.538	0.555	0.165	−12.469	−0.437	−0.380	−0.113	−0.439	0.579	0.278	−0.459	−0.690	−0.089	−0.515	−0.041	−0.370	−0.697
Post-exposure avg. (0, 6)	(0.155)	(0.191)	(0.129)	(3.533)	(0.163)	(0.156)	(0.058)	(0.280)	(0.270)	(0.389)	(0.175)	(0.381)	(0.150)	(0.336)	(0.118)	(0.206)	(0.371)
	[0.001]	[0.004]	[0.199]	[<0.001]	[0.007]	[0.015]	[0.050]	[0.117]	[0.032]	[0.475]	[0.009]	[0.070]	[0.555]	[0.126]	[0.729]	[0.072]	[0.060]
	0.067	0.086	−0.052	5.988	−0.179	−0.179	−0.045	−0.262	0.503	−0.802	−0.168	−0.204	−0.076	−0.029	−0.081	−0.347	−0.112
t = −3	(0.088)	(0.096)	(0.095)	(1.686)	(0.089)	(0.069)	(0.046)	(0.181)	(0.288)	(0.518)	(0.090)	(0.154)	(0.116)	(0.121)	(0.121)	(0.183)	(0.146)
	[0.441]	[0.375]	[0.583]	[<0.001]	[0.044]	[0.009]	[0.324]	[0.149]	[0.081]	[0.122]	[0.064]	[0.184]	[0.510]	[0.808]	[0.502]	[0.058]	[0.443]
	0.056	0.096	0.126	11.383	0.014	0.090	0.056	−0.005	−0.091	0.383	0.008	0.081	0.017	−0.013	0.102	−0.117	−0.009
t = −2	(0.061)	(0.075)	(0.071)	(1.662)	(0.075)	(0.087)	(0.038)	(0.176)	(0.202)	(0.286)	(0.077)	(0.134)	(0.094)	(0.102)	(0.121)	(0.107)	(0.153)
	[0.360]	[0.201]	[0.075]	[<0.001]	[0.852]	[0.300]	[0.143]	[0.978]	[0.651]	[0.181]	[0.914]	[0.543]	[0.859]	[0.902]	[0.399]	[0.275]	[0.955]
	0.265	0.187	0.259	11.492	0.073	0.042	0.002	0.106	−0.187	−0.034	0.087	0.050	0.091	0.160	0.212	−0.298	0.304
t = −1	(0.092)	(0.104)	(0.076)	(1.766)	(0.087)	(0.079)	(0.045)	(0.178)	(0.120)	(0.141)	(0.091)	(0.154)	(0.127)	(0.120)	(0.103)	(0.194)	(0.205)
	[0.004]	[0.073]	[0.001]	[<0.001]	[0.405]	[0.598]	[0.958]	[0.553]	[0.118]	[0.810]	[0.339]	[0.747]	[0.473]	[0.183]	[0.040]	[0.125]	[0.137]
	0.103	0.161	0.147	11.269	−0.064	0.024	−0.075	−0.117	0.271	−0.139	−0.064	−0.105	−0.001	0.011	−0.087	−0.015	−0.070
t = 0	(0.074)	(0.086)	(0.080)	(2.035)	(0.080)	(0.092)	(0.068)	(0.185)	(0.187)	(0.170)	(0.083)	(0.144)	(0.099)	(0.139)	(0.103)	(0.179)	(0.134)
	[0.163]	[0.062]	[0.065]	[<0.001]	[0.421]	[0.792]	[0.266]	[0.527]	[0.146]	[0.415]	[0.439]	[0.465]	[0.990]	[0.935]	[0.397]	[0.935]	[0.604]
	0.268	0.345	0.177	0.982	−0.233	−0.175	−0.010	−0.472	0.440	0.047	−0.235	−0.414	0.107	−0.255	−0.179	−0.337	−0.229
t = 1	(0.095)	(0.110)	(0.101)	(2.765)	(0.110)	(0.115)	(0.051)	(0.229)	(0.412)	(0.206)	(0.114)	(0.201)	(0.115)	(0.202)	(0.155)	(0.198)	(0.248)
	[0.005]	[0.002]	[0.080]	[0.722]	[0.034]	[0.128]	[0.848]	[0.039]	[0.285]	[0.820]	[0.038]	[0.039]	[0.354]	[0.206]	[0.249]	[0.090]	[0.356]
	0.052	0.122	−0.190	−4.896	−0.297	−0.251	−0.145	−0.141	0.700	0.051	−0.315	−0.507	0.040	−0.199	−0.126	−0.264	−0.558
t = 2	(0.118)	(0.145)	(0.116)	(3.751)	(0.150)	(0.127)	(0.072)	(0.277)	(0.405)	(0.300)	(0.156)	(0.299)	(0.147)	(0.250)	(0.262)	(0.227)	(0.302)
	[0.657]	[0.402]	[0.102]	[0.192]	[0.047]	[0.048]	[0.045]	[0.611]	[0.084]	[0.864]	[0.044]	[0.089]	[0.785]	[0.426]	[0.631]	[0.246]	[0.065]
	0.404	0.367	0.090	−11.946	−0.298	−0.228	−0.178	−0.198	0.113	−0.218	−0.288	−0.415	−0.018	−0.167	−0.046	−0.347	−0.467
t = 3	(0.148)	(0.165)	(0.166)	(4.526)	(0.181)	(0.215)	(0.087)	(0.353)	(0.266)	(0.442)	(0.191)	(0.368)	(0.167)	(0.340)	(0.241)	(0.328)	(0.283)
	[0.006]	[0.026]	[0.590]	[0.008]	[0.099]	[0.287]	[0.042]	[0.575]	[0.671]	[0.622]	[0.131]	[0.260]	[0.912]	[0.624]	[0.850]	[0.290]	[0.099]
	0.822	0.802	0.395	−23.882	−0.517	−0.555	−0.073	−0.241	0.034	0.412	−0.522	−0.801	−0.035	−0.740	−0.043	−0.445	−0.776
t = 4	(0.301)	(0.328)	(0.249)	(5.352)	(0.240)	(0.248)	(0.121)	(0.441)	(0.411)	(0.526)	(0.256)	(0.543)	(0.208)	(0.440)	(0.221)	(0.343)	(0.428)
	[0.006]	[0.015]	[0.112]	[<0.001]	[0.031]	[0.025]	[0.546]	[0.585]	[0.934]	[0.433]	[0.041]	[0.140]	[0.867]	[0.093]	[0.845]	[0.195]	[0.070]
	1.147	1.333	0.443	−26.340	−0.925	−0.659	−0.134	−0.857	1.391	0.813	−1.002	−1.857	−0.231	−1.027	0.113	−0.242	−2.006
t = 5	(0.354)	(0.664)	(0.277)	(6.099)	(0.579)	(0.333)	(0.108)	(0.856)	(0.785)	(0.944)	(0.629)	(1.701)	(0.274)	(0.586)	(0.179)	(0.301)	(1.369)
	[0.001]	[0.045]	[0.110]	[<0.001]	[0.110]	[0.048]	[0.213]	[0.317]	[0.076]	[0.389]	[0.111]	[0.275]	[0.398]	[0.080]	[0.528]	[0.421]	[0.143]
	0.968	0.755	0.095	−32.468	−0.722	−0.814	−0.176	−1.046	1.101	0.978	−0.786	−0.729	−0.481	−1.226	0.082	−0.942	−0.777
t = 6	(0.381)	(0.409)	(0.283)	(6.843)	(0.277)	(0.345)	(0.114)	(0.550)	(0.501)	(0.907)	(0.301)	(0.574)	(0.352)	(0.705)	(0.222)	(0.527)	(0.367)
	[0.011]	[0.065]	[0.736]	[<0.001]	[0.009]	[0.018]	[0.124]	[0.057]	[0.028]	[0.281]	[0.009]	[0.204]	[0.171]	[0.082]	[0.714]	[0.074]	[0.034]
Pre-trend test, $\chi^2(3)$	13.537	9.076	18.693	92.293	4.895	8.517	3.105	2.492	10.712	2.853	4.489	2.032	1.064	2.701	7.070	6.990	3.025
	[0.004]	[0.028]	[<0.001]	[<0.001]	[0.180]	[0.036]	[0.376]	[0.477]	[0.013]	[0.415]	[0.213]	[0.566]	[0.786]	[0.440]	[0.070]	[0.072]	[0.388]
Post 0–2 test, $\chi^2(3)$	9.441	10.632	10.617	57.143	5.520	7.021	5.488	4.599	6.941	2.012	5.438	4.871	1.045	2.174	1.480	3.440	5.074
	[0.024]	[0.014]	[0.014]	[<0.001]	[0.137]	[0.071]	[0.139]	[0.204]	[0.074]	[0.570]	[0.142]	[0.182]	[0.790]	[0.537]	[0.687]	[0.329]	[0.166]
Post 3, 4 test, $\chi^2(2)$	8.669	6.511	2.622	20.874	4.782	5.134	4.355	0.392	0.196	8.194	4.250	2.293	0.029	5.225	0.061	1.764	3.514
	[0.013]	[0.039]	[0.270]	[<0.001]	[0.092]	[0.077]	[0.113]	[0.822]	[0.907]	[0.017]	[0.119]	[0.318]	[0.986]	[0.073]	[0.970]	[0.414]	[0.173]
Post 5, 6 test, $\chi^2(2)$	10.759	5.563	3.515	22.600	7.396	5.665	2.507	3.693	5.009	1.222	7.414	2.290	2.123	3.207	0.440	3.949	5.175
	[0.005]	[0.062]	[0.173]	[<0.001]	[0.025]	[0.059]	[0.286]	[0.158]	[0.082]	[0.543]	[0.025]	[0.318]	[0.346]	[0.201]	[0.802]	[0.139]	[0.075]
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	965,007	907,704	862,660	965,007	814,124	792,008	785,705	529,560	11,442	34,311	886,498	326,458	318,442	320,087	900,100	879,237	886,862

Extended Data Table 2 | Callaway–Sant'Anna difference-in-differences estimates. Group–time average treatment effects on the treated, estimated with the Callaway and Sant'Anna (2021) staggered difference-in-differences estimator comparing newly exposed establishments with not-yet-exposed establishments; the unit of analysis is the establishment–year (*i*, *t*). The racial compensation gap is the nonwhite−white difference in average hourly compensation within an establishment; negative coefficients indicate a widening white compensation advantage. Average compensation and the racial compensation gap are measured in BRL per hour. Columns 4 reports the establishment's count of active contracts. Each cell reports the coefficient, its establishment-clustered standard error in parentheses, and the exact two-sided P value in brackets. Event time t is measured relative to first exposure, and pre-exposure coefficients are retained for diagnostic testing; the pre- and post-exposure averages aggregate the corresponding event-time coefficients. Joint tests report the χ^2 statistic and its P value for the pre-exposure leads and for the short- (0–2), medium- (3, 4) and long-run (5, 6) post-exposure coefficients. Standard errors are clustered at the establishment level.

	Average compensation				Racial compensation gap											
				Number of workers		Skill		Municipality population			Market concentration			Disaster intensity		
	All	White	Nonwhite		All	Low	High	Low	Mid	High	Low	Mid	High	Low	Mid	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	0.031	0.049	−0.169	−38.036	−0.095	−0.066	−0.053	0.297	0.537	−0.105	−0.121	0.343	−0.335	−0.301	0.475	−0.567
t = −3	(0.163)	(0.180)	(0.135)	(3.885)	(0.144)	(0.070)	(0.275)	(0.638)	(0.410)	(0.146)	(0.231)	(0.206)	(0.267)	(0.165)	(0.342)	(0.274)
	[0.849]	[0.785]	[0.211]	[<0.001]	[0.509]	[0.346]	[0.847]	[0.642]	[0.190]	[0.472]	[0.600]	[0.096]	[0.210]	[0.068]	[0.165]	[0.039]
	−0.258	−0.258	−0.167	−10.940	−0.047	−0.059	−0.179	0.636	0.299	−0.049	−0.001	0.131	−0.329	−0.186	0.340	−0.286
t = −2	(0.156)	(0.170)	(0.098)	(2.285)	(0.110)	(0.063)	(0.216)	(0.654)	(0.250)	(0.114)	(0.186)	(0.158)	(0.196)	(0.149)	(0.216)	(0.265)
	[0.098]	[0.129]	[0.088]	[<0.001]	[0.669]	[0.349]	[0.407]	[0.331]	[0.232]	[0.667]	[0.996]	[0.407]	[0.093]	[0.212]	[0.115]	[0.280]
t = −1 (ref.)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	−0.026	0.061	−0.002	12.940	−0.116	−0.120	−0.128	−0.215	0.007	−0.129	−0.255	0.093	−0.046	−0.128	0.037	−0.466
t = 0	(0.101)	(0.112)	(0.100)	(2.800)	(0.105)	(0.074)	(0.225)	(0.584)	(0.263)	(0.108)	(0.169)	(0.146)	(0.227)	(0.147)	(0.197)	(0.293)
	[0.797]	[0.586]	[0.984]	[<0.001]	[0.269]	[0.105]	[0.569]	[0.713]	[0.979]	[0.232]	[0.131]	[0.524]	[0.839]	[0.384]	[0.851]	[0.112]
	−0.131	−0.084	−0.172	0.848	−0.147	−0.057	−0.425	−0.417	0.521	−0.159	−0.381	0.280	−0.186	−0.203	−0.128	−0.332
t = 1	(0.126)	(0.143)	(0.119)	(3.330)	(0.129)	(0.068)	(0.258)	(0.749)	(0.384)	(0.133)	(0.214)	(0.157)	(0.263)	(0.183)	(0.223)	(0.337)
	[0.298]	[0.557]	[0.148]	[0.799]	[0.254]	[0.402]	[0.099]	[0.578]	[0.175]	[0.232]	[0.075]	[0.075]	[0.479]	[0.267]	[0.566]	[0.325]
	−0.264	−0.277	−0.322	−11.815	−0.144	−0.137	−0.116	0.348	0.037	−0.171	−0.401	0.209	−0.288	0.068	−0.014	−0.477
t = 2	(0.161)	(0.186)	(0.130)	(4.273)	(0.164)	(0.079)	(0.303)	(0.501)	(0.419)	(0.170)	(0.306)	(0.178)	(0.249)	(0.337)	(0.255)	(0.352)
	[0.101]	[0.136]	[0.013]	[0.006]	[0.380]	[0.083]	[0.702]	[0.487]	[0.930]	[0.314]	[0.190]	[0.240]	[0.247]	[0.840]	[0.956]	[0.175]
	−0.203	−0.244	−0.384	−16.961	−0.122	−0.199	−0.178	0.274	−0.441	−0.126	−0.287	−0.175	−0.226	−0.425	0.144	−0.406
t = 3	(0.199)	(0.221)	(0.177)	(4.809)	(0.191)	(0.087)	(0.352)	(0.793)	(0.537)	(0.199)	(0.350)	(0.211)	(0.306)	(0.302)	(0.347)	(0.340)
	[0.308]	[0.270]	[0.030]	[<0.001]	[0.523]	[0.022]	[0.613]	[0.730]	[0.412]	[0.527]	[0.412]	[0.407]	[0.460]	[0.159]	[0.678]	[0.232]
	0.067	0.040	−0.178	−24.482	−0.386	−0.124	−0.384	−0.680	0.177	−0.399	−0.792	−0.078	−0.310	−0.350	−0.113	−0.729
t = 4	(0.314)	(0.341)	(0.226)	(5.075)	(0.234)	(0.100)	(0.395)	(1.057)	(0.636)	(0.244)	(0.461)	(0.207)	(0.334)	(0.264)	(0.350)	(0.391)
	[0.831]	[0.907]	[0.431]	[<0.001]	[0.099]	[0.215]	[0.331]	[0.520]	[0.781]	[0.102]	[0.086]	[0.706]	[0.353]	[0.185]	[0.747]	[0.062]
	0.653	0.879	0.013	−29.800	−0.935	−0.157	−1.310	0.402	1.000	−0.981	−1.676	−0.167	−0.531	−0.498	0.113	−1.686
t = 5	(0.299)	(0.446)	(0.222)	(5.749)	(0.350)	(0.093)	(0.506)	(1.200)	(1.036)	(0.367)	(0.711)	(0.229)	(0.423)	(0.235)	(0.315)	(0.687)
	[0.029]	[0.049]	[0.953]	[<0.001]	[0.008]	[0.091]	[0.010]	[0.738]	[0.334]	[0.008]	[0.018]	[0.466]	[0.209]	[0.034]	[0.720]	[0.014]
	0.923	1.177	0.075	−51.896	−1.402	−0.366	−1.816	−1.552	0.274	−1.453	−1.996	−0.745	−1.389	−0.895	−0.967	−2.026
t = 6	(0.399)	(0.438)	(0.259)	(7.996)	(0.306)	(0.093)	(0.552)	(0.943)	(0.686)	(0.328)	(0.582)	(0.328)	(0.683)	(0.303)	(0.596)	(0.481)
	[0.021]	[0.007]	[0.772]	[<0.001]	[<0.001]	[<0.001]	[0.001]	[0.100]	[0.690]	[<0.001]	[<0.001]	[0.023]	[0.042]	[0.003]	[0.105]	[<0.001]
Pre-trend test, F	1.937	1.730	1.511	47.983	0.217	0.525	0.373	0.491	1.095	0.257	0.254	1.418	1.690	1.801	1.265	2.197
	[0.144]	[0.177]	[0.221]	[<0.001]	[0.805]	[0.591]	[0.689]	[0.612]	[0.335]	[0.774]	[0.776]	[0.242]	[0.184]	[0.165]	[0.282]	[0.111]
Post 0–6 test, F	1.933	2.393	1.991	14.184	3.490	3.022	2.302	0.863	1.558	3.343	2.403	2.074	0.987	1.719	1.365	3.351
	[0.060]	[0.019]	[0.052]	[<0.001]	[0.001]	[0.004]	[0.024]	[0.536]	[0.143]	[0.001]	[0.019]	[0.043]	[0.438]	[0.100]	[0.215]	[0.001]
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry×year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Microregion×year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	965,007	907,704	862,660	965,007	814,124	785,705	529,560	11,342	34,578	891,139	329,242	314,954	320,794	900,100	879,237	886,862

Extended Data Table 3 | High-dimensional fixed-effects difference-in-differences estimates. Dynamic event-study estimates from a high-dimensional fixed-effects difference-in-differences specification that absorbs establishment, calendar-year, industry×year and microregion×year fixed effects, comparing newly exposed establishments with not-yet-exposed establishments; the unit of analysis is the establishment–year (i, t). This two-way fixed-effects model is a secondary robustness check, and event time $t = -1$ is the omitted reference. The racial compensation gap is the nonwhite−white difference in average hourly compensation within an establishment; negative coefficients indicate a widening white compensation advantage. Average compensation and the racial compensation gap are measured in BRL per hour. Columns 4 reports the establishment's count of active contracts. Each cell reports the coefficient, its establishment-clustered standard error in parentheses, and the exact two-sided P value in brackets. Joint tests report the F statistic and its P value for the pre-exposure leads (−3, −2) and the post-exposure coefficients (0–6). Standard errors are clustered at the establishment level.

	Racial compensation gap			Employment count			Racial selection and composition		
	Incumbent (1)	Non-switcher (2)	Entrant (3)	Incumbent (4)	Non-switcher (5)	Entrant (6)	Incumbent (7)	Non-switcher (8)	Entrant (9)
Pre-exposure avg. (−3, −1)	−0.003	−0.008	—	−24.711	−2.092	—	0.002	0.001	—
	(0.023)	(0.033)		(1.802)	(0.317)		(0.002)	(0.001)	
	[0.908]	[0.796]		[<0.001]	[<0.001]		[0.374]	[0.078]	
Post-exposure avg. (0, 4)	−0.113	−0.061	−0.084	−45.273	−5.570	10.406	−0.001	0.002	0.002
	(0.048)	(0.067)	(0.073)	(3.377)	(0.557)	(7.484)	(0.002)	(0.001)	(0.003)
	[0.019]	[0.363]	[0.249]	[<0.001]	[<0.001]	[0.164]	[0.498]	[0.110]	[0.513]
Post-exposure avg. (0, 6)	−0.121	−0.075	−0.069	−53.454	−7.325	11.295	−0.001	0.002	0.000
	(0.062)	(0.080)	(0.077)	(3.805)	(0.673)	(7.794)	(0.002)	(0.001)	(0.003)
	[0.052]	[0.348]	[0.373]	[<0.001]	[<0.001]	[0.147]	[0.540]	[0.074]	[0.949]
t = −3	0.011	−0.006	—	−46.382	−3.978	—	0.004	0.002	—
	(0.044)	(0.060)		(3.215)	(0.574)		(0.003)	(0.001)	
	[0.801]	[0.925]		[<0.001]	[<0.001]		[0.258]	[0.023]	
t = −2	−0.019	−0.020	—	−27.751	−2.298	—	0.001	0.001	—
	(0.033)	(0.048)		(2.281)	(0.390)		(0.003)	(0.001)	
	[0.560]	[0.680]		[<0.001]	[<0.001]		[0.622]	[0.334]	
t = −1 (reference)	0.000 (ref.)	0.000 (ref.)	—	0.000 (ref.)	0.000 (ref.)	—	0.000 (ref.)	0.000 (ref.)	—
t = 0	−0.043	−0.033	0.000 (ref.)	−22.183	−2.564	4.422	0.002	0.001	0.005
	(0.028)	(0.045)		(2.328)	(0.370)	(7.779)	(0.002)	(0.001)	(0.002)
	[0.117]	[0.456]		[<0.001]	[<0.001]	[0.570]	[0.221]	[0.065]	[0.025]
t = 1	−0.084	−0.096	−0.034	−38.201	−4.524	9.109	−0.003	0.001	0.005
	(0.040)	(0.064)	(0.068)	(3.166)	(0.526)	(7.634)	(0.002)	(0.001)	(0.003)
	[0.032]	[0.136]	[0.616]	[<0.001]	[<0.001]	[0.233]	[0.132]	[0.331]	[0.027]
t = 2	−0.160	−0.115	−0.182	−48.601	−5.704	11.575	−0.003	0.000	0.001
	(0.061)	(0.091)	(0.088)	(3.651)	(0.568)	(7.845)	(0.003)	(0.001)	(0.003)
	[0.009]	[0.204]	[0.038]	[<0.001]	[<0.001]	[0.140]	[0.254]	[0.737]	[0.709]
t = 3	−0.171	−0.005	−0.150	−55.868	−7.116	15.077	−0.001	0.002	0.001
	(0.077)	(0.100)	(0.095)	(4.032)	(0.695)	(7.950)	(0.003)	(0.001)	(0.004)
	[0.026]	[0.963]	[0.117]	[<0.001]	[<0.001]	[0.058]	[0.611]	[0.106]	[0.886]
t = 4	−0.104	−0.055	0.031	−61.513	−7.941	11.848	−0.002	0.003	−0.004
	(0.099)	(0.126)	(0.099)	(4.518)	(0.820)	(7.821)	(0.003)	(0.001)	(0.004)
	[0.294]	[0.659]	[0.751]	[<0.001]	[<0.001]	[0.130]	[0.556]	[0.034]	[0.372]
t = 5	−0.133	−0.059	0.006	−70.831	−10.787	13.339	−0.002	0.003	−0.003
	(0.120)	(0.145)	(0.110)	(5.021)	(1.011)	(9.169)	(0.003)	(0.002)	(0.005)
	[0.265]	[0.682]	[0.955]	[<0.001]	[<0.001]	[0.146]	[0.555]	[0.040]	[0.478]
t = 6	−0.151	−0.161	−0.086	−76.980	−12.638	13.697	0.000	0.003	−0.006
	(0.140)	(0.161)	(0.117)	(5.441)	(1.103)	(9.386)	(0.003)	(0.002)	(0.005)
	[0.278]	[0.317]	[0.463]	[<0.001]	[<0.001]	[0.144]	[0.922]	[0.146]	[0.170]
Pre-trend test, F	0.436	0.111	—	110.676	26.347	—	0.790	3.553	—
	[0.647]	[0.895]		[<0.001]	[<0.001]		[0.454]	[0.029]	
Post 0–2 test, F	2.527	0.868	2.503	63.730	38.383	2.779	4.267	1.417	2.650
	[0.055]	[0.457]	[0.082]	[<0.001]	[<0.001]	[0.040]	[0.005]	[0.236]	[0.047]
Post 3, 4 test, F	2.718	0.162	3.132	96.791	52.648	2.037	0.176	2.287	1.189
	[0.066]	[0.850]	[0.044]	[<0.001]	[<0.001]	[0.130]	[0.839]	[0.102]	[0.304]
Post 5, 6 test, F	0.675	0.637	0.692	100.550	68.286	1.076	0.425	2.425	1.302
	[0.509]	[0.529]	[0.501]	[<0.001]	[<0.001]	[0.341]	[0.654]	[0.089]	[0.272]
Establishment–cohort FE	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	No
Cohort–year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Microregion–year FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No
Industry–year FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes	No
Anchor controls	No	No	No	No	No	Yes	No	No	No
N	3,337,451	1,754,398	2,827,088	6,375,460	6,375,460	3,523,219	4,713,236	4,713,236	3,426,334

Extended Data Table 4 | Worker-category gaps, employment, and racial selection after disaster exposure. The table reports stacked difference-in-differences event-study estimates on an establishment–cohort–year panel; worker categories are defined relative to the pre-exposure anchor year; categories defined before exposure use event time −1 as the omitted reference, whereas the entrant gap column omits event time 0 and the entrant count and composition columns have no omitted reference. Each cell reports the coefficient, its establishment-clustered standard error in parentheses, and the exact two-sided P value in brackets. Incumbent and non-switcher gap columns (1, 2) use a minimum-fixed-effects total-effect specification (establishment–cohort and cohort–year fixed effects). The entrant gap column (3) is a within-post specification with event time 0 omitted. Count and selection columns (4, 5, 7, 8) additionally absorb microregion–year and industry–year fixed effects. Entrant columns (6, 9) are post-anchor specifications estimated without establishment fixed effects and have no pre-trend test, because entrants do not exist before the anchor. Column 7 is the nonwhite-minus-white retained-share gap among anchor workers; column 8 is the corresponding non-switcher-share gap; column 9 is the entrant hiring-composition gap, defined as the nonwhite share among post-anchor entrants minus the nonwhite share of the pre-disaster anchor workforce (negative values indicate that new hires are whiter than the existing workforce). Joint tests report the F statistic and its P value for the pre-exposure leads and for the short- (event times 0–2), medium- (3, 4) and long-run (5, 6) post-exposure coefficients. Standard errors are clustered at the establishment level.