Heat, informality, and reallocation: Firm adaptation in the short and long run*

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Abstract

How do climate shocks shape resource allocation across firms? We show that rising temperatures distort resource allocation by shifting labor away from larger, more productive firms and toward smaller, informal ones. We study this question in India, an economy characterized by widespread informality, large productivity differences across firms, and extreme heat. Using census data on 42 million non-farm establishments from 1990-2013 linked to granular climate histories, we estimate the impact of heat on the firm size distribution. A 1°C temperature shock reduces firm size by 19.4%, with losses concentrated among large, formal firms. Displaced workers reallocate to smaller, informal firms, resulting in aggregate productivity losses of up to 4.3%. Over longer horizons, however, the relationship reverses: large firms adapt and absorb labor, while small firms contract. This adaptation offsets nearly half of the short-run labor demand shock. These results highlight a dynamic pattern of climate adjustment: in the short run, heat shocks shift labor toward less productive firms, while in the long run, adaptation by larger firms partially reverses aggregate losses.

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1 Introduction

A growing literature shows that climate change harms firm productivity (Heal and Park, 2016).[1] Extreme heat increases worker absenteeism (Somanathan et al., 2021) and reduces labor productivity (Adhvaryu et al., 2020; Somanathan et al., 2021). But the aggregate economic consequences of climate change depend not only on how temperature affects productivity within firms, but also on how it reshapes the allocation of resources across them. If climate vulnerability—and the capacity to adapt—varies across firms, then climate shocks may reallocate labor across firms, even within sectors.[2] In low- and middle-income countries, where numerous small, informal firms typically coexist with fewer but more productive formal firms, such within-sector reallocation can have first-order implications for aggregate productivity (Hsieh and Klenow, 2009; Hsieh and Olken, 2014; Restuccia and Rogerson, 2017; Hopenhayn, 2014).

This paper shows that heat shocks reallocate labor away from larger, more productive firms and toward smaller, informal ones, generating economically meaningful productivity losses. We establish this result using comprehensive firm census data in India, combined with high-resolution temperature data. Exploiting spatial and temporal variation in temperature anomalies, we show that heat shocks reduce average firm size, but this average effect masks stark heterogeneity: employment contracts sharply among large, formal firms while expanding among small, informal firms, shifting labor toward less productive firms. Over longer horizons, however, this pattern reverses. Larger firms adapt and reabsorb labor, highlighting climate adaptation as a dynamic process that mitigates short-run allocative costs.

We examine firm adaptation and labor reallocation in India, among the world's most climate change-exposed countries. Under a moderate (SSP2-4.5) warming scenario, the number of days with temperatures exceeding 35°C is projected to rise to 95 by 2060-79 (WorldBank, 2024). Like many other emerging economies, India also has a skewed firm-size distribution: a small number of large firms operating near the global technology frontier coexist with a multitude of small, informal firms, the latter accounting for 99 percent of firms and 75 percent of non-farm employment (Bussolo and Sharma, 2022). Firm-level misallocation costs the econ-

[1] Much of this evidence comes from the agriculture sector, where rising temperatures, changing weather patterns, and rainfall variability have reduced agricultural productivity (Burke and Emerick, 2016; Nath, 2020).

[2] Recent evidence suggests that larger firms respond differently to heat shocks than smaller firms (Goicochea and Lang, 2023; Xie, 2024a).

omy 40-60% of total productivity (Hsieh and Klenow, 2009), yet existing research has focused almost exclusively on the direct productivity effects of climate change on Indian firms.

We estimate the impact of heat shocks on firms using census data on the universe of 42 million establishments—both formal and informal—from 1990-2013, combined with spatially granular, long-term climate data. We leverage both cross-sectional and temporal variation in temperature to identify the impact of heat shocks on the moments of the firm size distribution, using a variety of spatially granular fixed effects to control for local confounders and trends.

We first estimate the relationship between firm size and local temperature anomalies.[3] In our preferred specification with village and district-by-year fixed effects, we find that a 1°C temperature anomaly reduces firm size by 19.4% on average. But this average effect conceals substantial heterogeneity across firms. To identify distributional effects, we estimate the conditional distribution of firm size using fixed effect quantile regressions. We find that the negative firm size effects of heat shocks are concentrated entirely among large, formal firms. Quantile regressions reveal that a 1°C heat shock slightly *increases* firm size at the 5th percentile, while firms at the 90th percentile contract by nearly 30%. These quantile effects hold after conditioning on disaggregated sectoral fixed effects. As such—despite the fact that we do observe some evidence of sectoral labor reallocation in response to heat shocks—our results do not simply reflect broad reallocation across sectors that vary in average firm size.

The results suggest that heat shocks cause large, formal firms to shrink and shed labor, which is then absorbed by small, informal firms. We explore this mechanism further by estimating the impact of heat shocks on sub-district level employment shares of different firm size categories—single-person microenterprises, informal firms with 1 to 7 employees, and formal firms with 8 or more employees.[4] We find that heat shocks cause a 2 p.p. reallocation in labor from the formal to informal sectors for each 1°C temperature increase. The results are consistent with the informal sector serving an absorptive role for labor displaced by negative shocks. Given the large labor productivity differentials between formal and informal firms, our estimates imply an allocative efficiency loss of 1.3-4.3% of aggregate productivity for each degree Celsius of temperature increase.

Rising informality in response to short-run temperature anomalies may serve as a transi-

[3] Anomalies are measured as annual deviations relative to long-run annual temperature averages.

[4] This local labor market approach is consistent prior research on the local labor markets effects of shocks in India, such as Jayachandran (2006).

tional adjustment mechanism to climate shocks in environments with labor market frictions. However, long-run adjustment dynamics should also depend on the differential adaptive capabilities of informal and formal firms. To examine the long-run impacts of temperatures on the firm size distribution, we estimate long-difference regressions in a panel of census villages.[5] We regress the change in the average firm size on the change in temperature over lags of 8, 15, and 23 years, corresponding to the available lags between census rounds. To estimate the differential effects across the firm size distribution, we interact the long-run temperature change with a flexible function of the average firm size in the initial year.

Based on these long-difference regressions, a 1°C increase in long-run temperature reduces average firm since by 1.6-7.5 percent, impacts notably smaller than those estimated in the short-run. This gap between short- and long-run impacts is consistent with existing literature, which interprets it as evidence of adaptation reducing the adverse impacts of climate shocks over time (Burke and Emerick, 2016; Bento et al., 2023). Under this interpretation, our estimates suggest that adaptation offsets 47% of the short-run negative labor demand shock. The long-difference results also reveal that the impact of heat on the firm size distribution reverses over time. For the 8-year lag, a 1°C increase in long-run temperature has a very small negative impact on firm size at the 5th percentile, but reduces it by roughly 15% at the 95th; instead, for the 23-year time horizon heat has no significant effect at the 95th percentile. This suggests that larger firms superior adaptive capacity can be observed only the long run.

To provide direct evidence of differential long-run adaptation, we link novel data capturing adaptation choices for nearly 1,000 Indian firms in 2024 with local temperature histories. The survey data reveal that while firms of all sizes are equally affected by heat-related business disruptions, larger firms undertake significantly more adaptation measures in response to rising temperatures. Importantly, adaptation responds most strongly to *long-term temperature change*—the impact of a long-term 1°C increase in temperature is 4.5 times larger than for the short-run anomaly. Taken together, these patterns support the interpretation that larger firms adapt more than smaller firms, particularly in the long run. In the short run, firm adaptation is limited, so large firms shed labor which is absorbed by the informal sector. In the long-run, however, large firms adapt, reversing the allocative cost of climate change.

[5]Throughout, we refer to the smallest identifiable spatial units in the firm census as "villages," although in urban areas these may represent entire towns, or neighborhoods in larger urban agglomerations.

Our interpretation is that heat-induced declines in labor productivity reduce labor demand in larger firms, leading to a short-run reallocation of workers toward smaller, less productive firms that lowers aggregate productivity, an effect that dissipates over time as larger firms adapt. However, one possibility is that our short-run results are explained by changing sample composition: large firms may contract while small firms exit, leading to observed contractions only in large firms. Throughout, we restrict our sample to exclude single-person microenterprises where the exit margin is strongest. In addition, we find no evidence of a reduction in the number of small firms in response to temperature shocks that would suggest substantial net exit.

Several possible labor supply-driven explanations may also explain firm contraction—for example, migration of working-age men in response to heat, or the expansion of India's large government workfare schemes in heat-affected areas, either of which could draw labor out of local firms. However, we find no evidence that heat shocks increase migration in our sample, nor do we find evidence that impacts are larger in areas with greater coverage by government schemes. Using sectoral indices of tradeability as a proxy for local demand exposure, we find no evidence that impacts are smaller for firms producing more tradable products, ruling out output market demand-side effects.

Finally, we test the robustness of the results to a variety of specification choices. We consider several definitions of the temperature shock, including varying lag periods, binned specifications, controlling for precipitation, and testing for nonlinear heat effects. We also test robustness of the main results to exponential models that account for the skewed, discrete nature of the firm size outcome variable. We consider robustness to numerous sample restrictions. The core results remain robust to these measurement and specification choices.

Our paper contributes to the growing literature on firm-level climate change impacts and adaptation. This literature has largely focused on the direct productivity impacts of climate shocks and firm-level adaptation mechanisms, including technology adoption (Somanathan et al., 2021), better management practices (Adhvaryu et al., 2022), and changes to supplier networks (Balboni et al., 2023).[6] Some studies find heterogeneous impacts by firm type, with agricultural, locally-selling, and smaller firms firms most affected (Nath, 2020; Gallagher et

[6]Recent reviews of the growing literature on climate change adaptation by households and firms include Goicochea and Lang (2023); Kala et al. (2023); Rexer and Sharma (2024); Carleton et al. (2024).

al., 2023; Pelli et al., 2023; Xie, 2024a; Basker and Miranda, 2017). However, there is limited evidence on reallocation across firms as an economy-wide adjustment mechanism. We provide new evidence on the allocative cost of climate change in environments with labor market frictions. In doing so, we show that heat shocks act as a distortion, reallocating labor away from larger, more productive firms. By estimating both short- and long-run responses, we provide the first evidence on the dynamic allocative costs of climate change and their partial reversal through firm adaptation.

Our findings also contribute to understanding why heat shocks have persistent economic effects. Existing work shows that temperature shocks have long-term impacts on output both nationally (Bilal and Känzig, 2024; Burke et al., 2015; Dell et al., 2012; Nath et al., 2024) and sub-nationally (Burke and Tanutama, 2019; Colacito et al., 2019), but the mechanisms underlying these effects remain less well understood. We highlight two such mechanisms that may contribute to these persistent impacts: the reallocation of labor toward less productive firms in response to short-run shocks, and the slow pace of effective firm adaptation. By linking firm-level reallocation to long-run outcomes, our results provide a micro-founded explanation for the persistence of climate impacts. More broadly, we extend the literature on allocative efficiency and economic shocks, which has largely focused on macroeconomic disruptions (Restuccia and Rogerson, 2017). As weather shocks become more frequent and intense, understanding how they affect resource allocation across firms is increasingly important.

Within this literature, Colmer (2021) is closely related, showing that labor reallocation from agriculture to manufacturing in India offsets a substantial share of the local economic losses from heat. Our results complement and extend this finding by highlighting a distinct margin of adjustment. We show that climate shocks induce reallocation across firms *within* sectors, along the firm size distribution, rather than simply across broad macro-sectors. This within-sector reallocation has markedly different implications for aggregate productivity. While Colmer (2021) emphasizes offsetting effects, we show that reallocations toward smaller, less productive firms can *amplify* the short-run productivity losses from heat. Finally, we use novel microdata to provide direct evidence of heterogeneous firm-level adaptation as a central mechanism driving the long-run reversal of aggregate productivity losses. Our results highlight that the aggregate effects of climate shocks depend critically on whether adjustment occurs across or within sectors.

Finally, we contribute to the literature on economic shocks and informality. Transitions into and out of informality are an important margin of adjustment to trade liberalization and other macroeconomic shocks, with the informal sector absorbing workers displaced from formal jobs (Ponczek and Ulyssea, 2021; Ulyssea, 2020). Because informal firms tend to be less productive than formal firms, these transitions affect aggregate productivity (Dix-Carneiro et al., 2021). We show that climate shocks generate a similar margin of adjustment: heat-induced contractions in formal firms lead to an expansion of informal employment.[7] In this literature, our paper is most closely related to Xie (2024b), who finds that workers in Brazil displaced from formal manufacturing jobs following heat shocks often transition into informal work. We build on this insight by shifting the focus from worker-level transitions to the allocation of labor across firms. This perspective allows us to characterize how climate shocks reshape the entire firm size distribution and to quantify the resulting implications for allocative efficiency. Unlike prior work, we highlight that this reallocation reflects a temporary distortion in the allocation of labor, which is partially reversed as larger firms adapt over time. In doing so, we link the literatures on informality, misallocation, and climate change.

The next section describes our data and presents descriptive statistics of temperature trends and the firm size distribution in India. Section 3 discusses our empirical strategy. Section 4 presents our results on the short-run impacts of heat shocks, and section 5 quantifies their reallocation-driven effects on aggregate labor productivity. Next, section 6 presents our estimates of the long-run impacts of heat. Section 7 presents additional evidence on differential firm-level adaptation as a key mechanism of adjustment. Section 8 discusses robustness checks and alternative explanations, and section 9 concludes.

2 Data and Measurement

2.1 Data

The primary outcome variable of interest is firm size, taken from the Indian Economic Census. The Census collects data on the universe of Indian economic establishments—both formal and informal—at an 8-year frequency. The survey contains information on firm lo-

[7] Our work is consistent with existing evidence that inflexible regulations constrain re-allocative adjustment to weather shocks within the formal sector (Adhvaryu et al., 2013; Chaurey, 2015; Colmer, 2021).

cation, the quantity and gender composition of the firm's employment, the characteristics of the firm owners (gender and caste), and the economic sector, harmonized at the 3-digit level following India's National Industrial Classification. The firm-level census data was obtained from The Socioeconomic High-resolution Rural-Urban Geographic dataset on India (SHRUG) compiled by Asher et al. (2019). We obtain four waves of the SHRUG Economic Census: 1990, 1998, 2005, and 2013.[8] SHRUG provides harmonized location keys—known as SHRIDs—to geolocate firms within specific towns and villages over time. While specific firms cannot be tracked as a panel through the census waves, the four waves allow a panel structure at the village level.[9] Though the census questionnaire collects only a small set of firm-level variables (e.g., there is no data on inputs and outputs), it covers over 100 million unique firm-year observations of private establishments in the non-farm sector. We merge firm-level census data to village-level temperature histories for approximately 510,000 villages across India.[10]

Benchmarking allocative changes requires estimates of differentials in labor productivity between the formal and informal sectors. Since there is no survey containing detailed firm outcomes that covers both formal and informal firms in India, we follow common practice in the literature and combine data from the 2015-16 National Sample Survey of the Unincorporated Enterprises (NSS), which covers informal firms, with the 2015 Annual Survey of Industries (ASI), which covers the formal manufacturing sector (Nataraj, 2011; Hsieh and Klenow, 2014; Hsieh and Olken, 2014). This combined ASI-NSS dataset enables us to measure the gap in labor productivity between firms with 8 or more employees ("formal") and smaller, informal firms in a period close to last year of our Economic Census data, 2013.[11] We measure labor productivity as value-added per worker.

Temperature histories come from the ERA5 global reanalysis data (Hersbach et al., 2020), a standard temperature dataset used in climate science on South Asia (Kumar et al., 2023) and produced by the European Union through the Copernicus Climate Change Service. We obtain daily minimum, maximum, and average temperature readings for each of our villages from

[8] SHRUG data is publicly available here.

[9] Throughout, we refer to the unique, lowest-level administrative areas—the SHRIDs—as "villages." These units, however, vary dramatically in size, from small rural hamlets to subsections of large urban centers.

[10] The merge rate varies across census years. Summary statistics on firm and village sample sizes are in Appendix Table A1. Details on the data construction pipeline are in Appendix A.

[11] The NSS Sector survey was conducted at an approximate interval of 5 years and the 2015-16 round is the closest to the most recent Economic Census round.

1980-2021. Following the climate science literature, our main independent variable of interest for analyzing short-run heat impacts measures the deviation of temperature from its long run average. We measure temperature anomalies by first averaging the daily temperature within years for each of the mean, minimum, and maximum readings. We then average these annual averages for the census year and the year prior to smooth out year to year variation in temperature. Finally, we subtract from this 2-year lagged average the long-run village-level average annual temperature from 1980-2021. This 2-year temperature anomaly calculation expresses all deviations relative to the long run mean, accounting for spatial differences in long-run temperature levels.

Finally, data on firm-level climate adaptation choices comes from the World Bank South Asia Climate Adaptation (SACA) firm survey. The SACA Survey was conducted in 2024-25 across three industrialized Indian states (Gujarat, Maharashtra, and Tamil Nadu), covering nearly 1,000 non-farm enterprises. Comprehensive details on survey design and sampling are available in Appendix A.4. In brief, the survey asked firm managers about their experiences with heatwaves, floods, storms and other extreme weather events. It also asked about adaptation actions undertaken in the past five years. Respondents selected from a list of ten action types—for example, upgrading buildings, installing air conditioning, and changing products—and then indicated whether each was taken for weather-related reasons (including, but not limited, to heatwaves). Weather-related actions were classified as adaptation measures. We create an "adaptation index" by summing the ten binary adaptation adoption variables at the firm-level and standardizing the sum to a z-score.

2.2 Sample restrictions

As shown in Appendix Table A1, more than half of firms across all census years are single-person firms. The prevalence of these microenterprises raise important issues for the interpretation of the main results on firm size, which are based on multiple rounds of cross-sectional firm-level data. In particular, these firms do not have a downward margin of labor adjustment in response to heat shocks, and can only exit to adjust to negative shocks. At the same time, since we do not observe firms in a panel, we cannot measure the exit margin directly. As such, the observed labor demand response of single-person firms is truncated from below. To mitigate these effects, we drop these single-person firms in our main estimation sample.

Our estimation sample also drops villages which do not appear in all four years of our merged temperature and economic census database. The motivation behind this is that the composition of the main sample changes substantially over time due to varying merge rates between the village-level firm census data and temperature data across census years and variation in the share of firms that can be matched precisely to harmonized spatial units in SHRUG—as discussed in Appendix A. This sample restriction helps to mitigate potentially confounding composition effects arising due to changes in the share of firms that can be matched to precise locations with temperature data across the census rounds. Importantly, we test the sensitivity of our results to these sample restrictions throughout the paper.

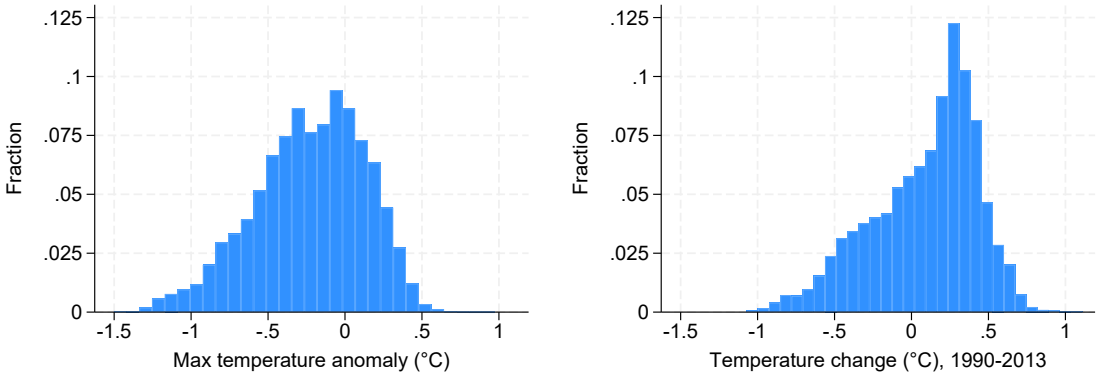
2.3 Descriptive statistics

We plot the primary identifying variation of the study in Figure A3. The left-hand panel shows maximum temperature anomalies for the 1,743,430 village-years in our full sample. This variation in unexpected deviations from average temperature drives the estimates of short-run responses to heat, where minimal adaptation is expected. The right-hand panel shows long-difference temperature change over the study period for the 575,117 unique villages in the data. This variation drives estimates of the long-run response to temperature, which includes adaptation over a nearly 25 year period. The short-run anomalies generally do not exhibit a clear pattern, distributed randomly around a slightly negative temperature shock. Meanwhile, the long-differences reflect overall warming patterns, with substantial clustering within a 0-0.5 degrees Celsius increase over the period.

Appendix B investigates warming trends across India in greater detail. Following Kumar et al. (2023), we estimate subdistrict-level linear temperature trends from 1980-2021 for each sub-season and for each district. We then collect and plot these estimates by region and season. The results reveal localized warming trends across the country that are uneven, seasonal, and spatially heterogeneous. The largest warming trends, reaching up to 0.7 degrees per decade, are concentrated in India's northern regions, in colder seasons, and observed more commonly for daily minimum and average, rather than maximum, temperatures.

Appendix Table A1 also provides summary statistics on the firm size distribution in the Indian economic census. The table contains 3 panels (A-C), each of which gives census round-wise statistics of the firms size distribution for all firms, services firms, and manufacturing

Figure 1: Village-level temperature: anomalies and long-differences



Note: Figure shows distribution of village-year temperature anomalies (left) and village-level long-difference temperature changes from 1990-2013, both measured in degrees celsius. Anomalies are calculated as the deviation between the average annual village-level maximum temperature in the two years before the census year and the long-run average annual maximum temperature from 1980-2021. Sample is all villages (SHRIDs) for the last four Economic Censuses of India (1990, 1998, 2005, and 2013).

firms, respectively. As is well-established, average firm size in India is small, ranging from 2.05-3.06 employees per firm across the various subsamples and census rounds. Manufacturing firms tend to be roughly 0.4-1 employee larger than services firms. In Panel A, the firm size distribution is also quite stable across years. In each round, the median firm is a single-person enterprise, indicating informal self-employment. The firm size distribution is also highly skewed. Only at the 75th, and particularly the 95th percentile do firms grow to sizes consistent with organized economic activity. The final column of the table gives the count (in millions) of surveyed firms in the census which can be linked to temperature histories.

3 Empirical strategy

Our aim is to estimate the impact of heat shocks on both the conditional expectation and conditional quantiles of the firm size distribution. Using a repeated cross section of firm-level data, the baseline linear specification for firm i in village v at census year t is:

$$y_{ivt} = \alpha + \beta A_{vt} + \delta_v + \delta_{dt} + \epsilon_{ivt} \tag{1}$$

Where y_{ivt} is firm size, A_{vt} is the 2-year village-level maximum temperature anomaly, δ_v is a village fixed effect, and δ_{dt} is an administrative unit-by-time effect, where the level of the administrative unit is higher than the village. Unit-by-time trends are important given the substantial regional heterogeneity in temperature trends, as well as potential location-specific changes in measurement in the economic census.[12] Standard errors are clustered at the village level in all specifications.

We estimate four core specifications, all of which include village fixed effects but vary in the level of the unit-by-time effects: (i) no time fixed effects, (ii) year fixed effects, (iii) state-by-year fixed effects, and (iv) district-by-year fixed effects. We note that (iv) leverages only within-district variation in temperature in each census year, which—as shown in Appendix Figure A4—accounts for a relatively small portion of the observed variation in our data. This limitation notwithstanding, the district-by-year specification is our preferred one, given the important role that districts play in implementing national and state-level policies.

Our main results are based on a balanced sample of villages and exclude single-person firms. As discussed in Section 2.2, these sample restrictions address potential concerns related to the changing composition of the sample due to imperfect matching of firms to precise locations across the census years and the left-truncated observed size response of single-person firms, respectively. We test the robustness of our results to including "unbalanced" villages and single-person firms. We also test the robustness of the results to various specifications of fixed effects and interacted trends, as well as different measurement approaches for A_{vt}.

We estimate both the conditional expectation $E[y_{it}|A_{it}]$ and the conditional quantiles $Q_\tau(y|A)$ for τ from to 5 to 95 in intervals of 5. Since the nonlinearity of quantile fixed effects models presents an incidental parameters problem, we use the location-scale estimator of Machado and Santos Silva (2019). Our identifying assumption is that, conditional on the fixed effects, residual temperature variation across villages and over census rounds is orthogonal to unobservables that determine firm size. Our results would be confounded if spatially differentiated trends in economic activity—or sample composition and measurement—are correlated with temperature shocks. These correlations would have to occur at a level *below* the district, given the inclusion of district-by-year fixed effects in our preferred specification.

[12] Note that with village fixed effects, a specification including the temperature anomaly on the right-hand side is equivalent to one in which only the temperature level is included, since the long-run average is subsumed by the village fixed effect.

4 Short-run results

4.1 Linear estimation

The estimated results of the baseline linear model are in Table 1. These results are based on the balanced village panel excluding single-person firms. The estimates here reveal the negative impact of temperature shocks on average firm size. Quantitatively, in our preferred specification with village and district-by-year fixed effects (column 4), a 1°C increase in the annual average maximum temperature reduces firm size by 0.76 employees, equivalent to 19.4% since the average firm size in our balanced sample excluding single-person firms is approximately 3.9 employees. However, this coefficient is considerably smaller in magnitude for specifications without year effects (1), with year fixed effects (2) and state-by-year fixed effects (3). Still, across all specifications, the estimated coefficient is always negative and is statistically significant at 10% or lower in 3 of four specifications.

Table 1: Heat shocks and firm size

Dependent variable	Firm size (employment)			
	(1)	(2)	(3)	(4)
Max temp anomaly	-0.264***	-0.122*	-0.138	-0.757***
	(0.04)	(0.053)	(0.09)	(0.21)
Observations	29305106	29298481	29298481	29298481
SHRID FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	No
State × Year FE	No	No	Yes	No
District × Year FE	No	No	No	Yes

Note: Standard errors in parentheses clustered at the SHRID (village) level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013) with more than 1 employee within the balanced panel of SHRIDs. Outcome is the total number of employees of the firm. Anomalies are calculated as the difference between the average annual maximum temperature of t and $t-1$ and the long-run average temperature from 1980-2021. Fixed effect specification given in table footer. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A2 displays the robustness of these results to including single-person firms and unbalanced villages in the estimation sample. The estimated coefficient on the temperature anomaly is uniformly negative across all estimation samples. However, the point estimates and their level of statistical significance are sensitive to the inclusion of unbalanced

villages (Panel B), especially for specifications that include year effects. This suggests that our estimates are sensitive to changes in sample composition due to partial matching of economic census firms to precise locations. Including single person firms reduces the estimated negative impact of temperature on firm size, a pattern that is consistent with the left-truncation of firm-size response for single person firms (Panel A, B). For example, in the balanced village panel with district-by-year fixed effects and single-person firms included, a 1°C increase in the annual average maximum temperature is estimated to reduce firm size by 0.16 employees. Given that the average firm size including single-person firms is approximately 2.2 employees, this is equivalent to a 7.2% effect.

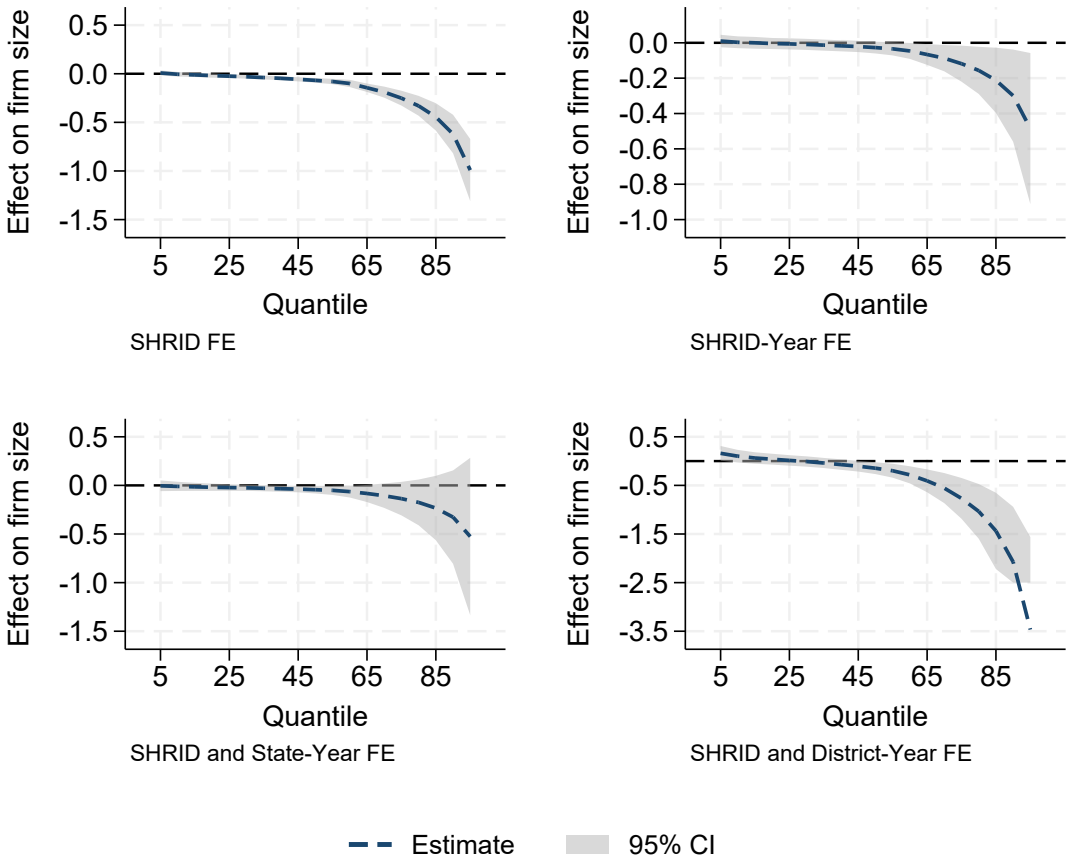
In Section 8, we discuss the robustness of these results to alternative specifications of the heat shock and the estimation model. The results are similar across specifications that use mean and minimum temperature anomalies, extend the lag period for measuring anomalies, measure heat shocks by the number of hot days, control for precipitation anomalies, and employ an exponential model with a fixed effects Poisson regression estimator to account for outliers given the skewness of the firm size distribution.

4.2 Quantile effects

One possible explanation for relatively small average estimates might be heterogeneity across the firm size distribution. If firms' sensitivity to heat is size dependent, then even a zero aggregate effect might mask substantial reallocation across firms. Figure 2 plots quantile regression coefficients and 95% confidence intervals for our main fixed effects models, estimated on the balanced panel excluding single-person firms. Each coefficient gives the marginal effect of a 1°C increase in the maximum temperature anomaly on quantile τ of firm size, conditional on fixed effects.

The quantile estimates reveal that the negative effects in Table 1 are concentrated in large firms. Depending on specification, firms below the 40th percentile see either no change or small *gains* in average firm size. In contrast, firms above 40th percentile see substantial losses. In the preferred specification, firms at the 90th percentile lose an average of roughly 2 workers for every 1°C increase in temperature. The shape of the quantile curve is robust to varying the level of the unit-by-year fixed effects, though the steepest slope obtains with the most

Figure 2: Quantile regressions of firm size on temperature anomalies



Note: Figure shows estimates and 95% confidence intervals of β_τ from quantile regressions of firm size on temperature anomalies and geographic fixed effects. Standard errors clustered at the village level. Anomalies are calculated at the village level as the deviation between the average annual temperature in the two years before the census year and the long-run average annual temperature from 1980-2021. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013) with more than 1 employee within the balanced panel of SHRIDs.

exacting specification of village fixed effects and district-by-year trends.[13]

To aid in the interpretation of magnitudes, Appendix Figure A8 shows the estimated quantile effects normalized by the quantile values to give percentage changes. In the preferred specification, firms at 5th percentile gain just over 5% of firm size following a 1°C shock. At the same time, firms at the 90th percentile shed approximately 30% of their labor, going from

[13]The shape of the quantile regressions is also generally robust to including single-person firms (Appendix Figure A6) and unbalanced villages (Appendix Figures A5 and A7) in the sample, the only exception being the state-by-year fixed effect specification, whose shape is somewhat sensitive to including unbalanced villages.

6 employees on average to 4, an economically large contraction. These results confirms that the small and sometimes insignificant average effects of Table 1 conceal meaningful distributional heterogeneity. Even in the specifications with the weakest average results, firms at the 90th percentile contract by approximately 5%. Since large firms lose and small firms gain labor from climate shocks, this plausibly suggests reallocation along the firm size distribution. Such reallocation could imply worsening aggregate productivity, given the well-established relationship between firm size and labor productivity (Van Biesebroeck, 2005).

4.3 Sector-specific effects

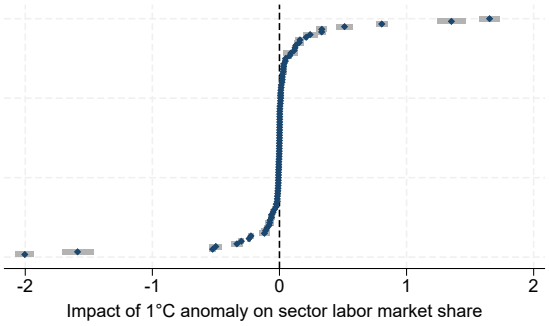
One plausible explanation for these distributional effects is reallocation across across sectors differing in average size. For example, if certain industrial process are highly heat-sensitive, then labor that was released from industrial sectors with large firms might enter self-employment and micro-enterprises across a range of sectors. To test this, we estimate reallocation effects across 90 non-farm subsectors in India's National Industrial Classification (NICS) divisions. To obtain heat-induced changes in sector shares, we estimate regressions of the sector labor market share on heat shocks at the village-year level, controlling for village and census round fixed effects. We run this model separately for each of the 90 NICS sectors and plot the resulting coefficients in Figure 3, Panel A.

The results show substantial heterogeneity in sector sensitivity to heat. The most negatively affected sectors lose roughly 2 p.p. of their labor market share, which is compensated for by gains among the winners of equal magnitude. In the distribution of sector-level t-statistics, 33 sectors gain and 32 lose with effects significant at 5%. The remaining 25 sectors do not see significant changes in their labor market share. The average sector loss for statistically significant losers is 0.21 p.p, which is quite meaningful given the relatively low level of sectoral disaggregation. Heat shocks produce substantial labor reallocation across India's non-farm sub-sectors.

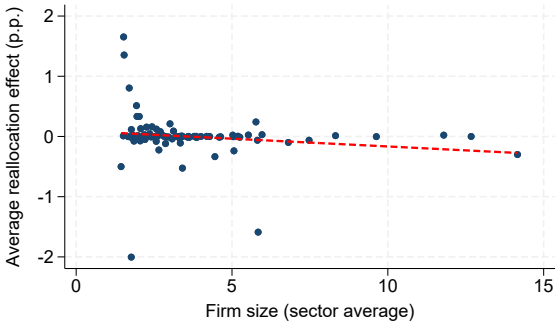
In Panel B of Figure 3, we test whether sectors that gain (lose) market share contain firms that are systematically smaller (larger), which would suggest that cross-sectoral reallocation drives the quantile effects of Figure 2. We find no evidence of a systematic relationship between average firm size and the sectoral reallocation coefficient. This suggests that, despite the fact that heat shocks generate some sectoral reallocation, this structural change does not

Figure 3: Impact of heat shocks on sector labor market shares

(a) Distribution of sector reallocation effects



(b) Correlation with sector-average firm size



Note: Panel A shows sector-level coefficient estimates and 95% confidence intervals of the impact of heat anomalies on sector employment shares at the village level. All models include village and year fixed effects. Standard errors clustered at the village level. Anomalies are calculated at the village level as the deviation between the average annual temperature in the two years before the census year and the long-run average annual temperature from 1980-2021. Panel B shows the correlation between sector-specifc reallocation coefficients plotted in Panel A and sector-level average firm size in number of employees. Sample is all villages in India for which Economic Census data is available.

systematically shift labor toward sectors with smaller firms on average. Therefore, sectoral change is unlikely to explain the quantile effects.

We consider three additional tests for sector effects. Table A3 estimates the main firm-level linear regression, splitting the sample into large macro-sectors of services and manufacturing; effect sizes are substantially larger in the manufacturing sector. It is therefore possible that the quantile effects are driven in part by larger negative responses among larger manufacturing firms relative to smaller services firms. However, Table A4 goes a step further by including fixed effects for 90 3-digit subsectors in the main model. In our preferred specification (column 5), the estimated average firm size effects hardly change, whether we allow for fixed sector heterogeneity (Panel A) or unrestricted sector-specific time trends (Panel B). Finally, Appendix Figure A9 plots quantile curves conditional on sector fixed effects. The results are nearly identical to those of Figure 2, though slightly weakened for year and state-by-year models. In sum, it is indeed highly likely that sectors are not equally exposed to, or equally able to adapt to, increasing heat. However, these sectoral heterogeneities do not appear to be driving effects across the firm size distribution. Instead, it appears that large firms contract employment most regardless of the sector in which they operate.

4.4 Formal-informal reallocation

Our quantile regressions have two possible interpretations. First, labor may be reallocating from large formal to small informal firms. This would imply an aggregate expansion in the labor share of the informal segment of the distribution. On the other hand, the quantile effects may not reflect any reallocation at all. Instead, it could be the case that all segments of the firm size distribution are shedding labor, but for large firms this shows up as a drop in firm size, whereas for small firms, it shows up as exit, without any change in average firm size—though our exclusion of single-person firms helps obviate this explanation.

To test explicitly for labor reallocation across firm size groups, we regress total employment shares by firm size category at the subdistrict level on temperature anomalies. We use the 5,883 subdistricts in India as our labor market units because there are likely to be substantial cross-village spillovers, given that labor, product, and factor markets are highly integrated within relatively narrow local areas (Jayachandran, 2006). We then calculate the temperature shock as the the average maximum temperature anomaly in the subdistrict.[14] For each subdistrict, we calculate the share of total employment in firms of the following size groups: (i) 1 employee, (ii) 2-7 employees, and (iii) 8 or more employees. The first two categories represent the self-employed and the household sector, respectively, and together comprise the informal sector. We place the second cutoff at eight employees because it is the threshold at which the Economic Census classifies firms as "directory" business establishments, rather than household enterprises.[15,16] We use employment shares instead of levels in order to focus exclusively on reallocations across segments, accounting for the fact that aggregate employment may contract for all firm size segments.

For subdistrict s at time t in firm size group g, we estimate the regression:

$$e^g_{st} = \alpha^g + \beta^g A_{st} + \delta^g_t + \delta^g_s + u^g_{st} \tag{2}$$

[14] Specifically, we take the surface area-weighted average of village-level anomalies.

[15] Directory establishments must fill in an additional module of the Census questionnaire which includes questions on their tax and business registration status and associated identification numbers. This module was used to generate a "Business Registry" from the Sixth Economic Census (CSO, 2013).

[16] We note that this cutoff yields a relatively inclusive definition of the "formal" sector in the Indian context, where a threshold of 10 employees is more commonly employed given that manufacturing establishments above this size must register under the Factories Act (1948) (Kanbur, 2017; Amirapu and Gechter, 2020). More broadly, there are multiple business registration requirements for firms that vary by industry and state, and have different size thresholds.

Where e^g_{st} is the employment share of firm size group g, A_{st} is the subdistrict average maximum temperature anomaly, and δ^g_t, δ^g_s are time and unit fixed effects, respectively. Standard errors are clustered at the subdistrict level.

Table 2: Heat shocks and employment shares, subdistrict level

Dependent variable	Share of employment				
	(1)	(2)	(3)	(4)	(5)
Panel A: Firm size $= 1$					
Max temp anomaly	-0.004	0.020***	0.004	0.020***	0.006**
	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
Panel B: $1 <$ *Firm size* < 8					
Max temp anomaly	0.016***	0.005*	0.016***	0.005**	0.016***
	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
Panel C: Firm size ≥ 8					
Max temp anomaly	-0.012***	-0.024***	-0.020***	-0.025***	-0.021***
	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
District FE	No	Yes	Yes	No	No
Year FE	No	No	Yes	No	Yes
Subdistrict FE	No	No	No	Yes	Yes
Observations	22335	22335	22335	22335	22335
R^2	0.001	0.271	0.275	0.588	0.592

Note: Standard errors in parentheses clustered at the subdistrict level. Sample is all private sector firms surveyed aggregated to the sub-district level in four Economic Census rounds (1990, 1998, 2005, and 2013). Outcome is the subdistrict-level share of employment in each firm size category. Anomalies are calculated as the difference between the average annual maximum temperature of t and $t-1$ and the long-run average temperature from 1980-2021, and then averaged to the subdistrict-level weighted by geographic area. Fixed effect specification given in table footer. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2 shows the results for a range of specifications of fixed effects. Across the specifications, the evidence is suggestive of reallocation between large, formal firms, and smaller informal ones. In our preferred two-way fixed effects specification of column (5), we estimate that a 1 degree increase in the temperature anomaly reduces the share of employment in large firms by 2.1 p.p. This reduction is offset by an increase in the subdistrict employment shares of single-person microenterprises (0.6 p.p.) and small firms below eight employees (1.6 p.p.). All estimates are significant at 5%, and the estimates are qualitatively consistent

across other specifications. For mid-sized and large firms, the estimates are always positive and negative, respectively, and significant in most specifications. For single-person firms, the estimates are somewhat less stable, ranging from precise zero estimates to a 2 p.p. reduction in employment shares, significant in 3 out of 5 specifications.

5 Aggregate productivity impact of heat shocks

What are the productivity implications of the heat-induced reallocation of labor to the informal sector in Table 2? In this section, we employ a standard macroeconomic accounting framework to quantify these effects, following the approach used by McCaig and Pavcnik (2018) to estimate the aggregate labor productivity gain from an export-induced reallocation of workers across formal and informal firms in Vietnam.[17]

5.1 Framework

In a frictionless, perfectly competitive benchmark economy, the marginal revenue product of labor (MRPL) should be the same across sectors in equilibrium. Labor market frictions or distortions may lead to departures from this condition, generating scope for changing aggregate labor productivity by shifting workers across sectors that differ in MRPL. For example, as in our paper, if MRPL is lower in the informal sector, then reallocating workers from formal to informal firms would reduce aggregate labor productivity.

The aggregate labor productivity impact of labor reallocation between formal and informal firms depends on their MRPL gap. This gap can be estimated from the observed gap in their average productivity of labor. Suppose output in sector $s \in \{informal, formal\}$ is given by a Cobb-Douglas production function of the form $Y_s = A_s K_s^{\alpha_s} L_s^{1-\alpha_s}$. If markets are competitive, each sector will employ labor until its marginal revenue product equals the wage rate:

$$w_s = MRPL_s = (1 - \alpha_s) ARPL_s \tag{3}$$

where $ARPL_s$ is the average revenue product of labor in sector s. Assuming that the output

[17] McCaig and Pavcnik (2018) build their approach on papers such as Gollin et al. (2014). Note that in their case, the trade shock causes a reallocation from lower-productivity informal firms to higher-productivity formal firms, leading to an increase in allocative efficiency. In our paper, the reallocation occurs in the opposite direction.

elasticity of labor α is the same in each sector, this implies that

$$\frac{w_{informal}}{w_{formal}} = \frac{MRPL_{informal}}{MRPL_{formal}} = \frac{ARPL_{informal}}{ARPL_{formal}} = ARPL^{ratio} \tag{4}$$

In other words, the ratio of the marginal revenue product of labor equals the ratio of the average revenue product of labor. Given this ARPL ratio, we can estimate the percentage change in aggregate labor productivity due to reallocation by applying the following accounting formula:

$$\Delta P_L = \frac{\Delta s(ARPL^{ratio} - 1)ARPL_{formal}}{s_{informal}ARPL_{informal} + (1 - s_{informal})ARPL_{formal}} \tag{5}$$

Where ΔP_L is the change in aggregate $ARPL$, Δs is the percentage of total employment reallocated from the formal to the informal sector, and s_s is the employment share of sector s.

5.2 Parameter estimates

The calibration of ΔP_L requires empirical estimates for two parameters: Δs and $ARPL^{ratio}$. Our estimate of Δs, the percentage of employment reallocated to informal firms, comes from the preferred subdistrict-level regression of employment shares in various firm size categories on temperature anomalies in Table 2, column (5): a 1°C temperature anomaly reduces the share of employment in formal firms by 2.1 percentage points. This share of total employment is reallocated to the informal sector, which we have defined as firms employing fewer than 8 individuals, including the self-employed. We also obtain the initial employment shares s_s for each sector from the firm census data.

Next, we estimate $ARPL^{ratio}$ using the combined NSS-ASI dataset by regressing firm-level value-added per worker on an indicator variable for formality, defined as firm size above 8 employees. The productivity regression results are presented in Appendix Table A5, with estimation details in Appendix D. One concern is that part of the gap in observed labor productivity could be due to greater human capital in formal firms; this component should be excluded when assessing the change in aggregate productivity resulting from worker reallocation. Ideally, we would control for worker level attributes such as years of education, as in McCaig and Pavcnik (2018). However, since ASI and NSS do not collect worker-level data, we estimate gaps in productivity conditional on state and 5-digit industry fixed effects.

The first column of Table 3 presents our estimates of the ratio of labor productivity in informal and formal firms for various specifications. The first row presents our baseline or "raw" $ARPL^{ratio}$, which shows that labor productivity in informal firms is just 38% that of formal firms. The second and third rows present the $ARPL^{ratio}$ adjusted for 5-digit industry fixed effects and state fixed effects. As expected, using within-industry variation industry reduces the estimated labor productivity gap between formal and informal firms, while state fixed effects do not make a meaningful difference.[18]

The last two rows present two additional estimates of $ARPL^{ratio}$ for robustness. de Mel et al. (2009) estimate that micro-enterprises in low- and middle-income countries may under-report revenue by about 30 percent. As this would tend to bias the productivity gap upwards, we present an estimate where reported revenue of informal firms has been adjusted for 30 percent under-reporting. Lastly, we present the $ARPL$ gap using 10 workers as the formality threshold. Our baseline 8-worker cutoff—which follows the Economic Census's categorization scheme—is a relatively inclusive definition of formal firms in the Indian context (Kanbur, 2017). By including borderline informal firms in the formal segment, the 8-worker cutoff may lead to an underestimation of the latter's productivity advantage. Indeed, the $ARPL^{ratio}$ based on the 10-worker cutoff is nearly half of that based on the baseline 8-worker cutoff.[19]

5.3 Aggregate productivity impacts

Column (2) of Table 3 presents the main estimates of the change in aggregate labor productivity obtained by applying the accounting formula in equation (5) to the $ARPL$ ratios in column (1).[20] The estimated decrease in aggregate labor productivity due to reallocation caused by a 1 1°C increase in the temperature anomaly ranges from 4.3% in the 10-worker cutoff specification to 1.3% in the 8-worker cutoff with industry and state fixed effects. Even the bottom end of this range represents a sizable negative productivity impact, considering that the temperature shock is only 1°C. Estimates from a moderate-high climate scenario (SSP3-

[18] However, these adjustments will be overly conservative, since a substantial share of the cross-sector variance in labor productivity will be due to firm rather than worker effects.

[19] This may explain why our raw productivity differentials are low compared to the range of estimates reported in McCaig and Pavcnik (2018) and previous papers such as Nataraj (2011) and La Porta and Shleifer (2014). La Porta and Shleifer (2014) report that the median $ARPL^{ratio}$ across a sample of low-and middle-income countries is 0.15. Their estimate for India, 0.18, is close to our 10-worker cutoff estimate.

[20] Appendix Table A6 presents detailed calculations.

7.0) forecast that by 2075, India's temperature could rise by 2.17°C relative to the final year of our sample period. This implies an aggregate productivity loss of 2.8-9.3% due only to reallocation effects.

Table 3: Labor reallocation and change in aggregate labor productivity

Method	$ARPL^{ratio}$	ΔP_L
	(1)	(2)
Raw	0.38	-2.47
Industry FE	0.55	-1.44
Industry + State FE	0.58	-1.33
Underreporting-adjusted	0.55	-1.46
10-worker cutoff	0.20	-4.32

Note: Estimates based on National Sample Survey of Unincorporated Non-agricultural Enterprises (2015-16) and the Annual Survey of Industries (2015). The ARPL ratio is the ratio of value added per worker in informal and formal establishments. The "raw" or baseline ARPL ratio is estimated by regressing value added per worker on a dummy for informality of the establishment. The "industry FE" and "industry + state FE" $ARPL$ ratios are based on including 5-digit industry and state fixed effects in the baseline regression. The "underreporting-adjusted" estimate is based on correcting value added per worker in informal establishments for an assumed under-reporting by 30%. The "10-worker cutoff" estimate is based on defining the formal sector using a minimum size of 10 workers, as compared to a baseline cutoff of 8 workers. Regression estimates are in Appendix Table A5.

We acknowledge that our estimates are subject to significant methodological caveats. A key limitation of our approach is that we cannot control adequately for differences in worker skills across formal and informal firms due to a lack of worker-level data. However, our estimates with sector fixed effects are only sensitive to differences in skills *within* narrow 5-digit industries. Another limitation is that the since the ASI dataset only covers manufacturing firms, we are assuming the estimated $ARPL^{ratio}$ applies to services firms as well.

Other caveats, which also apply to McCaig and Pavcnik (2018) and other papers sharing this approach, concern the validity of using the ratio of the average revenue product of labor to measure the ratio of marginal labor productivity in informal and formal firms. Differences in the output elasticity of labor between formal and informal firms would drive a wedge between the $ARPL$ and $MRPL$ ratios (equation 3). Furthermore, revenue-based ratios may be biased measures of physical productivity ratios if there are systematic differences in price markups by firm size due to market power.

While data limitations prevent further robustness checks to address these concerns, it is worth reiterating that our estimates of the productivity gap between formal and informal firms are already at the more conservative end of the range reported in the literature (Nataraj, 2011; La Porta and Shleifer, 2014; McCaig and Pavcnik, 2018), potentially due to our conservative size cutoff for defining formality. More broadly, our binary treatment of heat-induced reallocation—formal versus informal—likely understates the productivity impact by ignoring reallocation within the formal sector. As Figure 2 suggests, heat shocks cause a reallocation from larger to smaller firms even within the formal sector, with the largest contractions at the far right tail. The two-sector productivity accounting framework, while tractable and grounded in prior literature, does not account for the productivity impacts of such a continuous reallocation.

6 Long-run results

The empirical result that smaller firms are less affected by heat shocks—and even absorb labor from larger firms—may appear counterintuitive. After all, larger formal firms in India have greater access to capital and management expertise, both of which are strongly correlated with climate adaptation investments (Lang et al., eds, 2025). Evidence from a global sample of firms also show that small and medium enterprises tend to be less resilient to temperature shocks (Berg et al., 2025).

However, it could be the case that in the short-run, the informal sector—particularly household-based enterprises—serves as a safety net for labor released from larger, formal firms affected by shocks. Large firms shed labor because they face adjustment costs to implementing adaptations that mitigate the impact of heat on labor productivity. For instance, new machinery takes time to install, while more heat-resilient production processes must be designed and implemented. It is therefore plausible that in the long-run, larger firms eventually adapt, the labor market adjusts, and labor is re-absorbed into larger firms. In this case, the expansion of the informal sector is a transition dynamic that represents an economy-wide adjustment cost in adapting to heat. In this section we use long difference specifications to compare the impact of heat on the firm size distribution in the short and long run.

6.1 Long difference specification

We estimate the long-run average effects of using the following specification for village v

$$\Delta y_{vkt} = \lambda_k + \beta_k \Delta T_{vkt} + v_{vkt} \tag{6}$$

Where the outcome y, is the average firm size in village v and the independent variable is average annual maximum temperature T, averaged over the year of the census and the year prior. The long difference for variable X is defined as $\Delta X_{vkt} = X_{vt} - X_{v,t-k}$ for a given lag k. The longest lag in our data is 23 years, from the initial round of the economic census in 1990 to the final round in 2013. However, we also calculate differences for all intermediate lag periods—the 8-year lag (1990-1998, 1998-2005, and 2005-2013) and the 15 year lag (1990-2005, 1998-2013). We then stack each t, k long-difference cross-section into a single dataset, estimate the model separately for each lag length k, and cluster standard errors at the village level.

By differencing, the specification removes village fixed effects; by including the constant λ, it removes time trends within a long-difference period. The specification is therefore similar to our earlier specification (1), except for two key differences: i) we exploit longer-term shocks rather than two-year lagged transitory anomalies for identification, and ii) the specification is estimated at the village rather than the firm level, given the absence of firm panel data.

To estimate the differential effects of long-run temperature change across the firm size distribution, a quantile regression is no longer appropriate. This is because equation (6) is specified in changes, so the quantile regression will estimate the conditional quantiles of the distribution of firm size *changes* Δy rather than y, as desired. To solve this problem, we estimate the following specification:

$$\Delta y_{vkt} = \alpha_k + \Delta T_{vkt} \times \theta_k(y_{v,t-k}) + \varepsilon_{vkt} \tag{7}$$

Where the long difference change in temperature ΔT_{vkt} is interacted with the flexible function θ_k of average firm size in the initial period of the difference $y_{v,t-k}$. The function θ_k evaluated at a given initial firm size $y_{v,t-k}$ therefore gives the impact of a 1°C long-run increase in temperature at a given point in the firm size distribution. We estimate the θ functions using a flexible, kernel-weighted local linear regression (Hainmueller et al., 2019). Because the

firm size distribution is highly skewed, we also use a variable kernel estimation procedure in which the bandwidth varies along y and is inversely proportional to the density at a given point in the distribution (Terrell and Scott, 1992).[21]

6.2 Long-run average effects

The estimates of equation (6) are presented in Table 4. Columns (1)-(2) provide estimates for $k = 23$, columns (3)-(4) for $k = 15$, and columns (5)-(6) for $k = 8$. Across all specifications, a 1°C increase in the long-run temperature is associated with a 0.02 to 0.13-employee reduction in firm size. All estimates are significant at the 5 or 1% levels. The inclusion of state fixed effects—-which is equivalent to a state-by-time trend given the long differencing—increases the relationship from 0.016 to 0.069 for $k = 15$, but weakens it from 0.13 to 0.073 for $k = 8$. Overall, it appears that effects are slightly larger for the longest difference, but remain robustly negative and significant across all specifications. In the state fixed effects specifications, the $k = 23$ estimates are statistically significantly larger than the other two differences, though the magnitudes are moderate, while $k = 15$ and $k = 8$ coefficients are not statistically different from each other.

The results are robust to using log differences in Appendix Table A7. In these models, a 1°C increase in long-run temperature is estimated to reduce average firm since by between 1 and 7.5%. Figure A10 visualizes the long difference correlations with binned scatterplots.

The results in Table 4 use a village-level dataset that includes all firms—including single-person firms—when computing village-level average firm size. This differs from our baseline short-run results, which exclude single-person firms because of concerns about interpreting firm-level regressions when a plurality of firms are single-person and have no downward margin of labor adjustment. At the village level, the concern is less acute, as we are considering the mean of the firm size distribution, and single-person firms are a major component of that distribution. Nonetheless, Appendix Table A8 shows that the long-run results are generally robust to excluding single-person firms when computing village-level averages.[22] The results are also robust to replacing state fixed effects with district fixed effects—the equivalent

[21] Specifically, we set $h(y) = \frac{h}{p(y)}$, where $p(y)$ is the density function of initial firm size and h is a constant equal to the desired bandwidth at the modal point of the distribution ($p(y) = 1$).

[22] The sample size drops because some villages contain only single-person firms at the beginning or end of the long-difference period.

Table 4: Heat shocks and firm size: long difference estimates

Dependent variable	Change in firm size (employment)					
Difference	23-year		15-year		8-year	
	(1)	(2)	(3)	(4)	(5)	(6)
Temperature change (C)	-0.102***	-0.110***	-0.016**	-0.069***	-0.130***	-0.073***
	(0.008)	(0.012)	(0.007)	(0.009)	(0.003)	(0.007)
State × Period FE	No	Yes	No	Yes	No	Yes
Observations	331998	331998	697146	697146	1097327	1097327
R^2	0.001	0.010	0.000	0.022	0.003	0.023

Note: Standard errors in parentheses clustered at the SHRID (village) level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013), collapsed to the village level. Outcome is the change in average number of employees of private firms in a village over the long difference period t, k. Temperature shocks are calculated as the change in the 2-year lagged average annual maximum temperature over the long difference period t, k. Fixed effect specification given in table footer. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

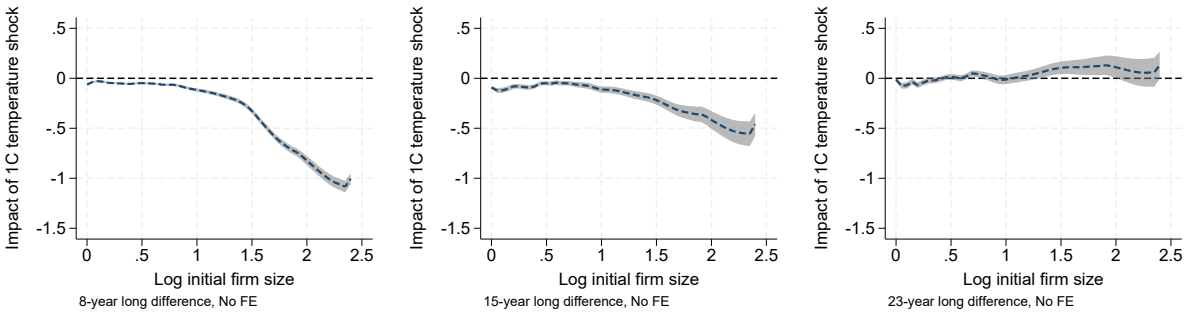
of a district-by-time trend given the long differencing (column 3). Across all specifications, long-run temperature increase has a negative association with firm size. The only exception is the 23-year panel with no fixed effects, where long-run temperature increase has a positive association with firm size, significant at the 1% level. This estimate reverts to the correct sign upon the inclusion of state or district fixed effects.

Notably, these effects are generally smaller in magnitude than our short-run estimates: the estimate in the preferred short-run specification in Table 1 is 0.76, while the 23-year long-run estimate from the comparable district fixed effects specification excluding single-person firms is 0.40 (Panel C, column 3 of Table A8). This is consistent with a large literature measuring the differential effects of long and short-run heat shocks on agricultural productivity and other economic outcomes. In the literature, this difference is typically interpreted as a reduced-form estimate of the magnitude of climate adaptation, since adaptation should be expected to reduce the costs of climate shocks over time (Burke and Emerick, 2016; Bento et al., 2023). Rexer and Sharma (2024) define the "adaptation ratio" as the share of the economic losses due to weather shocks that are recovered by climate adaptation. If we interpret the difference between the short-run estimate of 0.76 and long-run estimate of 0.40 as the effect of adaptation, then the adaptation ratio in this setting is about 47%.

6.3 Long-run distributional effects

The long-difference estimates are suggestive of firms' long-run adaptation to heat. If large firms are better able to adapt in the long-run, they may be able to reverse the short-run distributional effects observed in Figure 2. To test this hypothesis, we estimate equation (7), allowing the impact of temperature change in the long-differences equation to vary flexibly by initial average firm size. In practice, we use local kernel-weighted regressions to estimate the θ_k functions, with an adaptive bandwidth that varies along the the distribution of initial firm size depending on the density of the data.

Figure 4: Firm size impacts across the distribution: long difference kernel regressions



Note: Figure shows estimates and 95% confidence intervals of the impact of a long-run one degree Celsius increase in temperature along the initial firm size distribution. Estimates are presented for each long difference lag k. Standard errors clustered at the village level. Long differences are calculated at the village level as the difference between t and $t-k$ in $i)$ the average annual temperature in the two years before the census year and $ii)$ the average firm size. θ_k functions are estimated using kernel-weighted local linear regression with adaptive bandwidth selection. Sample is village-level data of the last four Economic Censuses of India (1990, 1998, 2005, and 2013).

The results are in Figure 4. Each panel plots the impact of a 1°C long-run temperature change on the change in average firm size, θ_k, estimated nonparametrically at each point along the (logged) distribution of initial firm size $y_{v,t-k}$. These effect curves are plotted separately for each long-difference lag k (8, 15, and 23 years), and are estimated without fixed effects. 95% confidence bands are derived from standard errors clustered at the SHRID level.

The results provide clear evidence of a long-run reversal of the distributional consequences of heat, likely driven by large firms' superior adaptive capacities. In the 8-year estimates, the effect curve mirrors the quantile results of Figure 2. The (villages with) larger firms tend to see larger contractions in firm size: for villages at the 95th percentile of the firm size distribution

(see Table A1) the effect of a 1°C increase in temperature is to reduce employment by an average of roughly 0.75 worker per firm, or 15%. For villages at the median, this effect is almost zero, though large samples yield a small and precisely estimated negative effect. While all experience somewhat negative effects, as with the quantile results, the effect curve remains relatively flat until around the 75th percentile, after which it slopes downward dramatically.

However, these trends moderate, and ultimately reverse, with longer lags in the differences. For $k = 15$, plotted in the middle panel of Figure 4, the slope flattens. The impact of a 1°C long-run increase in temperature at the 95th percentile of $y_{v,t-k}$ is now less than 0.5, while the estimates below the median firm size are similar to the 8-year estimates. This suggests that as time passes, larger firms begin making the necessary adaptive investments, and as a consequence narrow the gap in firm size effects across the distribution.

Finally, the right-hand panel shows estimates of θ_k for $k = 23$, the longest difference available in our data. Remarkably, the effect curve inverts completely—now the negative impacts of heat on firm size are largest for the smallest firms. As in other lags, a 1°C increase in long-run temperature reduces firm size by a small amount at the 5th percentile of the firm size distribution, and the effect remains negative and significant until just above the median firm size. However, by the 95th percentile of the distribution, the effect of a 1°C temperature rise is to *increase* firm size, though this is not statistically different from zero. Taken together, the results suggest that it takes several decades for large firms to adapt and the labor market to adjust to a heat shock. This implies potentially large aggregate adjustment costs entailing long periods of heightened misallocation, as the informal sector swells to absorb labor from large firms. These costs are likely to be nontrivial, and since temperature increases continuously, these costs will be spread out over time, rather than just experienced in the aftermath of an individual shock.

7 Adaptation in the short and long run

In Section 6, we interpreted the gap between long and short-run estimates—and the reversal of the large-firm contraction—as suggestive of superior long-run adaptation by larger firms. Here, we present direct firm-level evidence on adaptation from the 2024 World Bank South Asia Climate Adaptation (SACA) Survey (Lang et al., eds, 2025), which contains infor-

mation on self-reported adaptation measures. We estimate the following regressions to examine how firms' self-reported heat vulnerability and adaptation choices relate to long-term temperature change:

$$Y_i = \alpha + \beta \Delta \text{Temp}_i + \theta_s + \theta_j + \epsilon_i \tag{8}$$

and

$$Y_i = \alpha + \gamma \Delta \text{Temp}_i \times \text{Size}_i + \beta_1 \Delta \text{Temp}_i + \beta_2 \text{Size}_i + \theta_s + \theta_j + \epsilon_i \tag{9}$$

Here, Y_i is the outcome of interest for firm i (e.g., the adaptation index); ΔTemp_i is a measure of heat exposure in firm i's location, Size_i is firm i's total employment (a measure of its size); and θ_s and θ_j are state and sector fixed effects, respectively. The main coefficients of interest are β, which measures the relationship between heat exposure and the outcome of interest, and γ, which measures how that relationship varies with firm size. To capture both long and short-run adaptation responses, we compare specifications with two different definitions of ΔTemp_i: i) the increase in the mean maximum temperature at the firm's location between 1989-91 and 2019-21, and ii) the temperature anomaly in 2020-21, the start of the survey's 5-year recall window. The key outcome—the firm-level adaptation index—is measured as the total number of individual climate adaptation actions reported by the firm.

Table 5 presents the estimation results. In columns (1)-(4,) the main regressor is the increase in average maximum temperature at the firm's location between 1989-91 and 2019-21.[23] The outcome in columns (1)-(2), the "first-stage regression," is a binary variable that equals one if the firm reports having experienced at least one episode of extreme heat that affected its business operations in the past five years. Column (1) shows that firms in areas with larger temperature increases are significantly more likely to report heat-related business disruption. Column (2) adds an interaction with firm size, showing this relationship does not vary by firm size. The outcome in columns (3)-(4) is the adaptation index. Column (3) shows that a 1°C temperature increase is associated with a 0.9 standard deviation increase in the adaptation index. Column (4) then reveals that this relationship is significantly stronger for larger

[23] As in the rest of the paper, the temperature data used in the SACA analysis are the ERA5 global reanalysis data. To measure long-term temperature increase, we first average the daily maximum temperature within years, then compute 3-year moving averages centered on 1990 and 2020 to smooth out year-to-year variation, and then take the difference between the 2020- and 1990-centered averages.

firms. These results suggest that while heat-related business disruption rises equally across firm sizes as temperatures increase, larger firms respond with more adaptation measures.

Columns (5)-(8) in Table 5 estimate short-run effects by using the temperature anomaly in 2020-21, the start of the survey's 5-year recall window. The patterns largely mirror columns (1)-(4): recent temperature shocks are significantly associated with both heat-related business disruption and adaptation actions, with the adaptation relationship stronger for larger firms. However, comparing coefficients in columns (3) and (7) suggests that adaptation responds much more strongly to long-term temperature change than to recent shocks: a 1°C long-term increase is associated with a 0.95 standard deviation rise in the adaptation index nearly five times larger than the effect size for a 1°C anomaly. This suggests that firm adaptation to heat intensifies over time, with larger firms adapting more in both the short and long run.

Table 5: Heat exposure and adaptation: survey data regressions

Temp. var.	Δ Temp				Anomaly			
Outcome	Experienced Heat		Adaptation Index		Experienced Heat		Adaptation Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Δ Temp.	0.752***	0.712***	0.949***	0.776***				
	(0.066)	(0.079)	(0.128)	(0.152)				
Size		0.000		-0.001***		0.000		-0.000
		(0.000)		(0.000)		(0.000)		(0.000)
Δ Temp. × Size		0.001		0.003**				
		(0.001)		(0.001)				
Anomaly					0.523***	0.461***	0.195*	-0.012
					(0.056)	(0.072)	(0.100)	(0.120)
Anomaly × Size						0.001		0.003***
						(0.001)		(0.001)
N	959	959	959	959	959	959	959	959
Adj. R^2	0.248	0.248	0.421	0.432	0.211	0.211	0.387	0.401

Note: Robust standard errors in parentheses. All regressions include state and sector fixed effects. Δ *Temp* is the increase in mean maximum temperature between 1989-91 and 2019-21. *Anomaly* is the deviation of the 2020-2021 mean maximum temperature from the long-run average (1980-2021). *Experienced Heat* is a dummy variable equal to one for firms that report that their business was affected by an episode of extreme heat in the last five years. *Adaptation Index* is an index measuring the total number of adaptation measures undertaken by the firm in the last five years, normalized by its standard deviation for ease of interpretation. *Size* is total employment. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We next examine which types of adaptation actions account for the observed differences between large and small firms. We regress binary indicators for each of the ten potential

adaptation actions on long-term temperature change and its interaction with firm size. The results are presented in Appendix Table A9. They show that long-term temperature change correlates most prominently with the following adaptation measures: building upgrades, air conditioner installation, sales strategy changes, product changes, energy-efficient equipment installation, labor composition adjustments, and heat plan development. The correlation with long-term temperature change increases significantly with firm size for three of these measures: upgrading buildings, changing sales strategy and changing products. This suggests that large and small firms differ not only in the intensity of their adaptive responses but also in the nature of their adaptation strategies.

In sum, two patterns emerge from this analysis: adaptation measures intensify with longer heat exposure, and larger firms undertake more adaptation. While these findings should be interpreted with caution given the cross-sectional design and reliance on self-reported measures, they point to differences in adaptation patterns as potential mechanisms underlying the main results. In particular, the long-run adaptation patterns align well with the results of Table 4 and Figure 4 suggesting that larger firms become more resilient to heat over time. Still, the short-run results somewhat less clearly aligned: although larger firms report more adaptation efforts in the SACA survey, the Economic Census suggests that heat shocks cause labor reallocation toward *smaller* firms. One potential explanation is that adaptation measures have adjustment costs and therefore have a lagged impact on heat resilience. Consequently, even firms actively undertaking adaptation may shed labor in the immediate aftermath of heat shocks. The short-run expansion of the informal sector may thus reflect this transitional period during which adaptation measures are being implemented but have not yet generated their full protective effects.

8 Robustness and alternative explanations

Throughout, our core argument is that declining labor productivity reduces labor demand, leading large firms to contract. Displaced labor reallocates to small, unproductive, informal firms, reducing aggregate productivity in the short-run. This allocative effect dissipates in long-run, as larger firms adapt to heat. However, our results could be driven by inappropriate specification choices, measurement error, firm exit, labor supply responses such

as migration and government assistance, or product demand effects. In this section, we consider each of these explanations in turn. However, we note that none of these alternative hypotheses can fully explain both the short and long-run effects observed in our data.

Heat shock specification choice: Our results may be sensitive to specification choices for the heat shock. We consider three robustness to specifying the heat shock. In Table A10, we find similar results when defining the temperature anomaly using average or minimum daily temperatures. In Table A11, we calculate the anomaly over lags of one and three years instead of two. We find that the effect holds only for three-year lags but not only the census-year, consistent with increased noise from year-to-year temperature fluctuation and limited response time for firms when the shock is measured contemporaneously. Finally, in Table A12 we estimate a model in which temperature enters semi-parametrically as the number of days in each temperature interval. The estimates reveal that firm size remains a downward sloping function of temperature, consistent with the main results. The largest and most significant effects on firm size occur in the ≥ 40 degree bin.

Nonlinear heat effects: We also test whether effects of temperature vary by initial temperature, since differential impacts of heat shocks between hotter vs. colder places can be interpreted as evidence of long-run adaptation (Barreca et al., 2015). In Table A13, we interact the temperature anomaly in our main specification with an indicator variable equaling one if the long-term average daily maximum temperature is 30°C or greater. We find mixed evidence of systematic differences in firm size effects for hotter and cooler places—in the most rigorous specification (column 4), these differences shrink to zero.

Model specification choices. Table A14 tests the robustness of the main short-run effects result to model specification choices. In Panel A, we control for precipitation anomalies, constructed identically to our temperature anomalies. The results are essentially unchanged. Next, Panel B estimates an exponential model with a fixed effects Poisson regression estimator, to account for outliers given the skewness of the firm size distribution in India, with a large mass at 1, as well as the discrete count nature of the outcome. Estimates from the Poisson regression, interpreted as percent changes, are similar in relative magnitude to the main estimates, and again significant in the same three out of five specifications.

Urban heat islands: Our results may also be driven by the urban heat island (UHI) effect. Because of the UHI, urban areas tend to be hotter, and large firms cluster in urban

areas. In a naive specification, this would lead us to underestimate the negative effect of heat; however, our use of temperature anomalies and fixed effect strategy address this concern. A more subtle issue is that a measured 1°C anomaly may feel hotter in urban areas, which may not be captured in the relatively coarse temperature data, which can underestimate temperature extremes. This correlated measurement error could generate the illusion of larger negative effects for large firms, when in fact these large urban firms simply experience larger unmeasured heat shocks. We test this hypothesis by splitting the sample into urban and rural SHRIDs and re-running both the fixed effects (Table A15) and quantile regressions (Figure A11) on each subsample. The main results hold in both subsamples.

Entry and exit: As discussed in Section 4.1, our baseline specification for the short-run effects excludes single-person firms because they have no downward margin of labor adjustment—their only possible response to a heat shock is exit, which is unobserved in our data. A related concern remains: small and large firms may respond identically to heat, but small firms exit the market while large firms contract but remain active. If so, the short-run quantile effects reported in Section 4.2 could reflect differential survival rather than differential size adjustment among continuing firms. To rule this out, Table A16 contains estimates of the total number of firms in each firm size bin as a function of the heat shock at the the subdistrict level. Using an exponential PPML estimator to account for the highly right-skewed count outcome, we observe small and statistically insignificant effects for all firm size groups.[24]

Migration: It is possible that heat shocks causes migration (Mueller et al., 2014; Baez et al., 2017), which forces firms to contract due to labor shortage. In this case, contraction is driven by a labor supply rather than labor demand effect. To test this hypothesis, we use census data to estimate the relationship between annual migration rates and temperature anomalies at the district level. Figure A12 displays the results, which show no relationship between heat shocks on migration.

Government workfare programs: Another labor supply explanation is that government targets welfare programs to villages most affected by heat, increasing the opportunity cost of private sector work. Evidence shows that participation in NREGA, an employment guarantee

[24]The log-linear specification in Panel A shows a small but statistically significant 4% reduction in the number of 1 person firms, though this is accompanied by an even larger 6-7% reduction in the number of large firms, suggesting exit across the board rather than differentially by size. We also note that the PPML estimator is likely to outperform the log-linear if the functional form is exponential (Wooldridge, 1999).

workfare scheme, increases in heat-affected areas Garg et al. (2020). We replicate this result in Table A17, with evidence of meaningful increases in NREGA activity in response to heat shocks. However, in Table A18, we interact the log of district-level NREGA participation with the temperature anomaly in our main regression specification. There is no evidence that villages with higher NREGA enrollment observe larger firm size effects, suggesting that labor supply does not drive our results.

Demand-side effects: If household consumption of the products made by large firms is particularly sensitive to heat, then our quantile effects may be driven by a product rather than labor demand channel. Following Caggese et al. (2024), we test for demand effects by leveraging differences across sectors in tradeability, based on the logic that traded sectors are less sensitive to local demand shocks. Table A19 interacts the heat anomaly with a sectoral export exposure index. The results show no evidence that firm size effects are smaller for firms in tradeable sectors.

9 Conclusion

Firms in lower- and middle-income countries are increasingly vulnerable to climate shocks. These countries are also characterized by highly skewed firm size distributions, where a large mass of unproductive small firms coexist with a small number of large firms operating at the international productivity frontier. While the negative effects of climate change on firm-level productivity are by now well-studied, the implications for firm-level resource allocation are not well understood, particularly beyond reallocation between agriculture and industry.

This paper represents an initial attempt to understand this fundamental problem in the context of India, a country facing severe climate risk and exhibiting a high degree of misallocation. Using fixed effects models, we show that a 1°C temperature shock reduces non-agricultural firm size by 19.4% on average. However, this effect is highly heterogeneous, and disproportionately affects the largest firms. While temperature shocks do induce reallocation across sectors, this does not drive the observed firm size heterogeneity. Rather, large firms contract, and small firms expand, conditional on highly disaggregated sector fixed effects. Aggregate labor market results suggest reallocation of labor from the formal to informal sector as large firms shed workers in the face of rising heat. This short-run reallocation from

formal to informal firms accounts for a 1.3-4.3 percent aggregate productivity loss for each degree Celsius of warming. However, in the long-run, we show evidence that larger firms adapt to rising temperatures, labor markets adjust accordingly, and ultimately the disproportionate incidence of climate change on large firms reverses.

The results are consistent with the role of the informal sector as a safety net in the face of negative labor demand shocks. We also provide new evidence on the transitional allocative efficiency costs of climate change when the largest, most productive firms fail to swiftly adapt. But the results suggest that more research is needed on how firms adapt and change input choices, and the frictions that cause substantial lags in adaptation. In addition, the role of size-dependent regulation and credit constraints in impeding both firm-level adaptive responses as well as efficiency-enhancing reallocation deserve more exploration. We view these as promising avenues for future research.

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ONLINE APPENDIX

A Data appendix

A.1 Temperature data

A.1.1 Data Source

The core unit of observation in our climate data is the village, or SHRID, derived from unique spatial identifiers at the lowest possible level (in both rural and urban areas) from the Socioeconomic High-resolution Rural-Urban Geographic Platform for India (SHRUG Asher et al. 2019). We compile daily weather data for 576,153 villages in India spanning 1980–2021. Villages are uniquely identified by the SHRID, a code constructed from state, district, sub-district, and village identifiers. Temperature readings are obtained obtained from the ERA5 reanalysis dataset, which provides maximum, mean, and minimum surface air temperature at a spatial resolution of $0.25° \times 0.25°$ (Hersbach et al., 2020). In addition, daily mean precipitation data from ERA5 are used. ERA5 is widely employed in historical climate reconstruction and provides consistent global coverage. Each village-level SHRID is mapped to the nearest ERA5 grid cell using village-level shapefiles from SHRUG.

A.1.2 Data Processing and measurement

For each daily temperature series (min, mean, and max), we construct annual aggregates for each SHRID. In addition, to capture the frequency distribution of temperature beyond averages, we classified each daily temperature observation into predefined bins ($< 0C, 0-10C, 10\tilde{}20C, 20-30C, 30-40C, > 40C$). For each SHRID and year, we then calculated the number of days falling into each bin. These bin-based measures provide a discrete characterization of thermal exposure, complementing the continuous averages and allowing for non-linear and semi-parametric models, following Burgess et al. (2017), among others.

Our main variable of interest—the temperature anomalies—measures deviation from a long-run baseline climate. To establish reference conditions, we calculated long-term climatological means for each SHRID by computing the 1980–2021 averages (across years) of annual maximum, mean, and minimum temperatures. These serve as the baseline climatological

norms against which deviations and anomalies are measured. Villages are classified as "hot locations" if their long-term average maximum temperature exceeds 30°C.

We define temperature anomalies relative to the long-term climatology. Let T_{it}^{max} be annual average maximum temperature for village i at year t. Then the anomaly A_{it} is given by:

$$\mathrm{A}_{it} = \frac{1}{2}\left(T_{it}^{max} + T_{i,t-1}^{max}\right) - \overline{T^{max}}_{i,\text{1980-2021}}$$

Note that we use the two-year average of T^{max} for t and $t-1$ in calculating the anomaly. This is done both to smooth out year-to-year variation in temperature, which is substantial (see Figure A1), but also to account for the fact that firm labor demand responses to heat shocks may occur with a lag. In Appendix G.1, we test the robustness of our results to numerous measurement assumptions. The final dataset is a balanced panel at the SHRID level covering 42 years (1980–2021). For each village-year, the dataset includes the max temperature anomalies and number of days in different temperature bins for each village in India, with the unique SHRID used to merge with other data sets.

A.2 Economic census data

A.2.1 Data Source

We utilize establishment-level records from the Indian Economic Census (EC) for the 1990, 1998, 2005, and 2013 rounds. The Economic Census, conducted at 8-year intervals by the Ministry of Statistics and Program Implementation, collects basic data on the universe of commercial establishments (what we call "firms") in India. Each record corresponds to an individual commercial establishment and contains information on employment size, industry classification, ownership type, and owner demographics. The raw data contains information on 21,806,546 firms in 1990, 26,772,187 firms in 1998, 35,795,897 firms in 2005 and 46,462,994 firms in 2013. We obtain the raw, firm-level census data from SHRUG, who also produce weighted aggregates at varying spatial levels. Note that firms are not uniquely identified or followed with a panel identifier throughout the EC, which we therefore treat as a repeated cross section.

A.2.2 Data processing and merging

The raw EC files contain geographic identifiers for each establishment, including state, district, subdistrict, block, town, ward and village codes, along with the firm responses to census questions. In order to map these geographic identifiers to the unique SHRID code, SHRUG provides a set of keys for both rural and urban areas that match firms to SHRIDs on the combination of state, district, subdistrict, block, town, ward and village codes. This procedure allows us to match firm-level observations with village-level weather data.
Because the EC provides separate lookup keys for rural (village-based) and urban (town-based) SHRIDs, we perform two merges for each establishment record in the EC: (i) a merge on {state, district, subdistrict, town} with the urban key to obtain a candidate urban identifier SHRID2^U, and (ii) a merge on {state, district, subdistrict, village} with the rural key to obtain a candidate rural identifier SHRID2^R. We then coalesce these candidates to a single spatial identifier according to the following rule for firm observation i:

$$\text{SHRID}_i = \begin{cases} \text{SHRID}_i^U & \text{if SHRID}_i^U \text{ is non-missing and SHRID}_i^R \text{ is missing,} \\ \text{SHRID}_i^R & \text{if SHRID}_i^R \text{ is non-missing and SHRID}_i^U \text{ is missing,} \\ \text{missing} & \text{if both are missing.} \end{cases}$$

In the rare case where both identifiers are present (e.g., along rural–urban administrative boundaries), we merge the weather data using both SHRIDs and take the average of the resulting measures.

After coalescing, we drop records with missing SHRID$_i$ and retain a harmonized set of variables—including ownership type (with a private-sector indicator), power source, caste, sex, total and female employment, financing mode, and census year, with consistent coding and value labels across census rounds. Finally, using the SHRID mapping, firm-level records are merged with the SHRID-level weather data. This linkage assigns to each firm the climate anomalies, long-term climatological averages, and temperature-bin measures corresponding to its village of location. The firm-level dataset thus provides, for each establishment and census round, both microeconomic characteristics (employment size, sector of main output, and ownership type) and local climatic conditions.

A.2.3 Sample selection

For the purposes of our analysis, we focus exclusively on non-agricultural, private-sector firms. First, we drop all records with missing SHRID_i values, as this spatial identifier is required to merge establishments with the weather data. Second, we exclude agricultural firms using SHRIC industry codes. The SHRIC (SHRUG Industrial Classification) codes were constructed by SHRUG to harmonize India's National Industrial Classification (NIC) codes across different NIC revisions. The SHRIC system harmonizes using 3-digit NIC08 codes to facilitate concordance with NIC 2004 and NIC 1987. There are 90 3-digit SHRIC codes.

Finally, we further restrict the sample to private-sector establishments. The definition of "private" varies across census rounds. In 1990, private firms include all establishments categorized as privately owned or organized under cooperative models. In 1998, the definition expands to include all non-government establishments. The 2005 and 2013 rounds follow a similar classification, where all non-government establishments are treated as private firms. This harmonization ensures consistency in our definition of the private sector across time.

After merging with climate data and restricting the sample to non-agricultural establishments with valid SHRIC industry codes and to private-sector firms as defined for each census round, the final dataset contains 110.6 million unique establishment-year-level records across the four rounds of the Economic Census. Specifically, there are 15.1 million firms in 1990, 22.5 million firms in 1998, 31 million firms in 2005, and 42 million firms in 2013 (Table A1, Panel A). Note that given varying merge rates and sample restrictions, not all of the original 576,153 villages are matched to firms in our final sample. Final sample sizes for villages—which represents the number of clusters for our main identifying variation—are in Table A1, Panel D.

A.3 ASI-NSS firm-level data

A.3.1 Data sources

Apart from the Economic Census, which does not collect revenue and expenditure data, there is no firm-level survey that collects representative data on both informal micro-enterprises and larger, more formalized firms in India. As a result, past studies such as (Nataraj, 2011; Hsieh and Klenow, 2014; Hsieh and Olken, 2014) have relied on a combination of two surveys

to estimate labor productivity differentials between informal and formal firms, an approach that we also follow. Our data source for the formal sector is the 2015 round of the Annual Survey of Industries (ASI). The ASI, conducted annually by India's Central Statistical Organization, is a detailed plant-level survey of manufacturing establishments registered under The Factory Act (1948). The ASI consists of a census of plants with employment above a threshold of 300 and a random sample of smaller plants.[25]

Our data source for the informal sector is the 2015-16 (73rd Round) Survey of Non-Agricultural Unincorporated Enterprises conducted by the National Sample Survey Organization. Until 2020, this survey was conducted about once every five years. The NSS survey covers manufacturing establishments not registered under the Factories Act (1948), as well as service sector establishments, but excludes construction firms.

A.3.2 Sample selection

From each dataset, we obtain firm-level $ARPL$, calculated as total value added divided by the number of employees. We define "formal" firms as those with 8 or more workers. For consistency with ASI, we drop non-manufacturing firms from the NSS dataset before appending the two datasets. We also drop establishments with employment greater than 9 from the NSS dataset and less than 10 from the ASI dataset.[26] Lastly, we winsorize value added at the top and bottom 0.1 percentile level in both ASI and NSS data to mitigate the influence of extreme outliers in value added.[27] After combining the ASI and NSS datasets, we obtain 134,480 firm-level observations for which we can observe value-added per worker and employment size.

A.4 South Asia Climate Adaptation Surveys

The 2024 World Bank South Asia Climate Adaptation (SACA) Survey collected data on firm-level experiences and expectations of weather shocks in Bangladesh, India, and Pakistan (Lang et al., eds, 2025); this paper uses only the India data, which covers 975 firms. Using

[25]The cutoff for the "census" segment of the ASI has varied over the years.

[26]By design, since all manufacturing establishments with 10 or more workers (and using electricity, which almost all firms do) are required to register under the Factories Act, there should be no overlap between the NSS and ASI beyond the employment cutoff of 10. In practice, outdated sampling frames and potential regulatory non-compliance lead to overlaps (Kanbur, 2017).

[27]Our estimates are not sensitive to varying the winsorization cutoff between 0.01 and 1 percentile.

a sample frame based on the 6th Economic Census (2013–2014), the sample covered three of the country's most industrialized states: Gujarat, Maharashtra, and Tamil Nadu. Districts within these states were selected based on having the largest concentrations of firms and ensuring coverage of the different types of weather shocks that firms in these states may experience. Eight districts were included: Ahmedabad, Chennai, Coimbatore, Mumbai, Pune, Surat, Thane, and Tirupur. Within each district, the sample was stratified by broad sector (manufacturing versus services) and firm size (under 20 employees, 20–100 employees, and over 100 employees). The survey interviewed firm managers—defined as the person primarily responsible for making day-to-day decisions about the firm's operations—and covered past and expected exposure to weather shocks (extreme heat, floods, waterlogging, storms, and droughts), estimated damages, adaptive actions taken or planned, management practices, access to finance, and firm performance. Since the SACA survey records firm coordinates, we were able to identify the SHRID to which each firm belongs, allowing us to merge daily ERA5 weather data at the SHRID level and construct measures of temperature exposure at each firm's location.

A.5 Other data sources

A.5.1 District-Level NREGA Data

We incorporate district-level data on the National Rural Employment Guarantee Act (NREGA) from Garg et al. (2020). These data provide annual measures of NREGA enrollment at the district level, as well as a corresponding harmonized district shapefile which fix the geographic boundaries of districts at one moment in time. Each village in our dataset is assigned to a district using the NREGA district shapefile. This mapping allows us to merge firm-level Economic Census records with district-level NREGA statistics through the SHRID village identifier. The NREGA data contains the following district-year participation variables: the number of households, person-days, labor expenditure, and materials expenditure of the program.

Since NREGA enrollment is reported annually from 2006-2014, the Economic Census rounds that overlap this period are 2005 and 2013. We construct smoothed measures by taking the three-year average of NREGA enrollment surrounding each census year. Thus, the NREGA measure for 2005 reflects the average over 2004–2006, and the measure for 2013 reflects the av-

erage over 2012–2014. In total, there are 75,268,119 firm-years observed in the 2005 and 2013 Economic Census rounds. Of these, 44,825,100 firm-level observations are located in villages that can be mapped to NREGA districts using the shapefile. This reflects the fact that NREGA was not rolled out nationwide simultaneously, and certain urban and non-program districts fall outside its coverage.

A.5.2 Trade Exposure Data

We construct sector-level trade exposure measures using the Asian Development Bank Multi-Regional Input–Output Tables (ADB MRIOT). The ADB MRIOT provides trade-to-output ratios for India across detailed sectors, including imports-to-gross output (MtGO), exports-to-gross output (XtGO), and total trade-to-gross output (XMtGO). These indicators quantify the extent to which industries are integrated into international markets.

To integrate the ADB MRIOT trade exposure measures with the Economic Census, we first map MRIOT industry codes to ISIC Rev. 3 (three-digit) industries using the concordance provided in the MRIOT documentation. We then translate ISIC Rev. 3 codes into India's National Industrial Classification (NIC-08), which is the industry classification system used for the EC SHRIC. During this process, certain industry codes were consolidated (for example, NIC 2711–2719 were recoded to 2710) to ensure consistency. Once the MRIOT sectors are mapped into SHRIC, we assign to each NIC code its corresponding values of MtGO, XtGO, and XMtGO. These SHRIC-level exposure measures are then merged directly with the firm-level Economic Census using each establishment's reported industry code. Thus, every firm inherits the trade exposure values of its industry. Importantly, trade exposure is not constructed at the SHRID (village) level; rather, it remains at the SHRIC (industry) level and is common across all firms in the same industry.

A.5.3 Migration Data

We use migration statistics from the Population Census of India for the years 2001 and 2011. The census reports origin district-level tabulations of migrants by gender, age, reason for migration, and years since migration. For the purposes of this study, we focus exclusively on individuals who reported work or business as their reason for migration. For both males

and females, the census distinguishes work/business migrants by years since migration into three groups: migrated within the last 0–5 years, migrated 6–10 years ago, and migrated more than 10 years ago. We construct total male and female work-related migrants by summing across all age groups and genders for each migration period.

To merge migration outcomes with climate exposures, we aggregate the SHRID-level weather data to the district level annually for the period 1980–2021. For each district and year, we collapse maximum temperature anomalies, weighting by SHRID population. Migration is reported only in 2001 and 2011, while weather data are annual. However, since each migration census contains a count of migrants who migrated in two distinct periods (within 5 years, and 5-10 years), we construct four periods in a pseudo-panel: 1992-1996, 1997-2001, 2002-2006, 2007-2011. We then merge to weather data at the district-period level, taking the time-averaged temperature anomaly in each period.

All district identifiers are harmonized to the 2011 census codes. This involves standardizing district names, correcting spelling inconsistencies, and mapping the 2001 district codes to 2011 boundaries. Adjustments are made for districts that were split or renamed between the two rounds. After harmonization, the dataset covers a total of 640 districts in a total of 26800 observation from 1980 to 2021. For 26800 observations, 12290 observations are matched with district-level information and migration data, such that one row is one district at one year.

B Estimating temperature trends

We begin by assessing aggregate warming trends across India over our sample period, in part replicating work done by Kumar et al. (2023). Figure A1 plots trends in average annual and seasonal surface temperature anomalies in degrees celsius. We disaggregate annual anomalies into four seasons corresponding with standard seasonal demarcations in India: pre-monsoon (March–April–May or MAM), monsoon (June–July–August–September or JJAS), post-monsoon (October–November or ON), and winter (December–January–February or DJF). Seasonal anomalies are calculated relative to the long run mean for that season. We find evidence for a clear aggregate warming trend in the post-monsoon period of October and November, but not in other seasons.

However, this aggregate trend masks substantial spatial heterogeneity across India. We

Table A1: Summary statistics

Census year	Mean	SD	P25	P50	P75	P95	P99	Count
Panel A: Firm size, all firms								
1990	2.28	45.11	1	1	2	5	14	15.1
1998	2.46	30.08	1	1	2	6	15	22.5
2005	2.21	11.45	1	1	2	5	10	31.0
2013	2.27	24.35	1	1	2	5	11	42.0
Panel B: Firm size, services firms								
1990	2.05	43.73	1	1	2	5	12	11.6
1998	2.25	29.41	1	1	2	5	13	18.0
2005	2.08	10.01	1	1	2	5	9	24.8
2013	2.19	24.34	1	1	2	5	11	33.0
Panel C: Firm size, manufacturing firms								
1990	3.06	50.54	1	2	2	7	23	3.4
1998	3.32	33.22	1	2	3	8	25	4.2
2005	2.74	16.31	1	1	2	6	15	5.9
2013	2.59	24.17	1	1	2	6	14	8.0
Panel D: Maximum temperature anomaly								
1990	-0.31	0.17	-0.43	-0.31	-0.20	-0.003	0.09	352146
1998	-0.55	0.38	-0.82	-0.59	-0.31	0.05	0.16	402907
2005	0.10	0.20	-0.02	0.13	0.24	0.39	0.40	478115
2013	-0.22	0.29	-0.41	-0.17	-0.03	0.16	0.45	510262

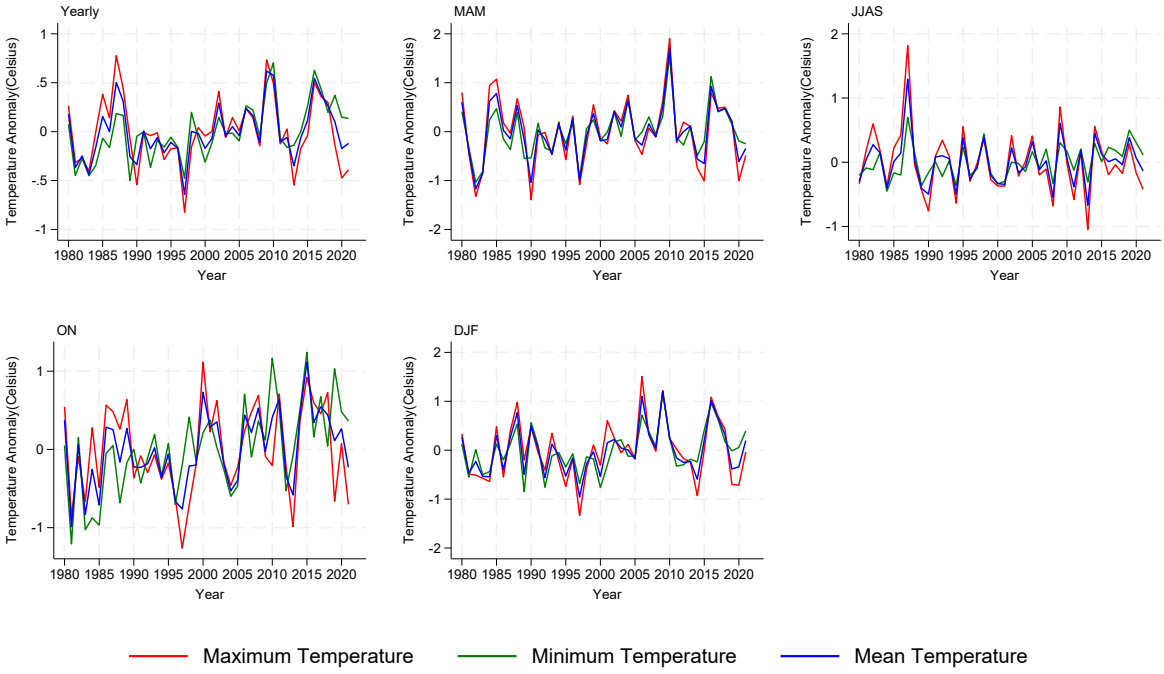
Note: Figure shows summary statistics for firm-level economic census variables (Panel A-C), as well as village-level climate shocks (Panel D), by census year. Count is in millions of firms for Panels A-C, and in number of villages for Panel D.

estimate long-run annual trends in temperature change across Indian districts using the following time trend regression

$$y_{it} = \alpha_i + \beta_i t + \epsilon_{it}$$

where y_{it} is average annual temperature in district i at year t. We estimate location-specific linear trends for each of the 5868 subdistricts in India for annual min, mean, and max temperature readings. Figure A2 then plots the distribution district-specific coefficients across India by temperature reading and region. The map indicates substantial spatial variation across India in warming trends. In some seasons and locations, warming trends are actually negative,

Figure A1: Seasonal temperature trends

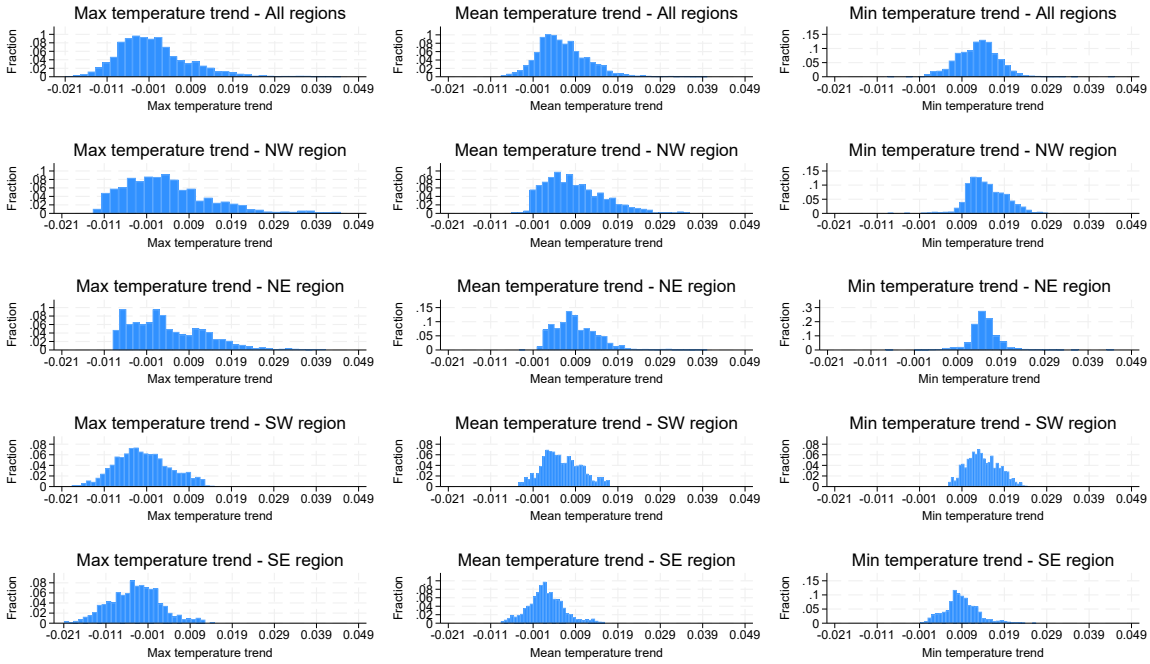


Note: Figure shows estimated annual surface temperature anomalies in degrees celsius relative to the 1980-2021 temperature average, for all of India. Anomalies are estimated for both annual and seasonal temperature averages. MAM - March, April, May; JJAS - June, July, August, September; ON - October, November; DJF - December, January, February.

whereas in others they are as large as 0.5 degrees every decade. Generally most warming is concentrated in the northern part of the country, where trends are consistently positive. More universal warming trends are observed for minimum and mean than for maximum temperature. The post-monsoon and winter seasons also see the most uniform warming trends, consistent with the results of Kumar et al. (2023). Warming in India is uneven, seasonal, and spatially heterogeneous.

This substantial heterogeneity in heat shocks over time and space will form the basis of our identifying variation. Appendix Figure A3 plots histograms of temperature anomalies at the village level for each census year. There is substantial cross-sectional variation across villages in census-year anomalies, which drive identification in models without village fixed effects. As the census round becomes more recent, there is also a rightward shift in the distribution of annual anomalies, consistent with moderate warming, though this shift is far from universal.

Figure A2: Histograms of district-level temperature trends



Note: Figure shows histograms of estimated annual subdistrict-level surface temperature trends in degrees Celsius of average temperature change per year, by temperature reading and region. Sample is all 5868 subdistricts in India from 1980-2021. Trends are estimated from a regression of temperature on a linear time trend separately for each subdistrict.

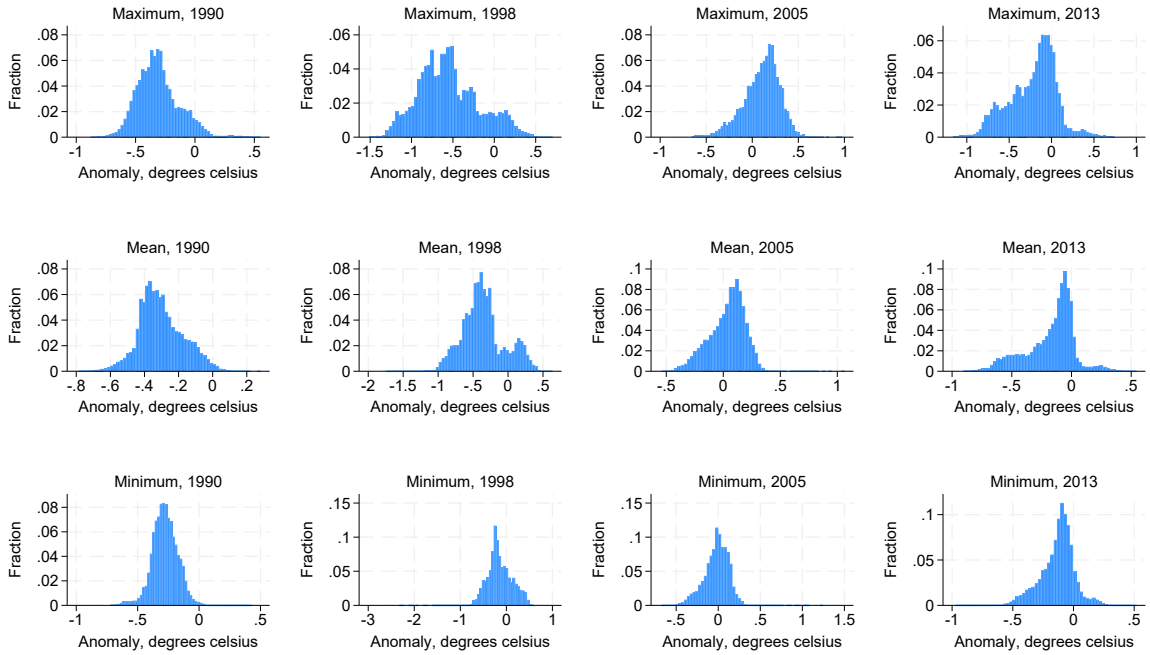
In models with village fixed effects, differential within-village shifts in anomalies over time will be the primary identifying variation. The key statistics of the temperature distribution are in Panel D of Appendix Table A1.

B.1 Residual variation in anomalies

Our core short-run firm-level regressions comprise four fixed effect specifications: (i) village fixed effects, (ii) village and year effects, (iii) village and state-by-year effects, and (iv) village and district-by-year effects. Each specification identifies the impact of heat from the residual variation in temperature anomalies that remains after conditioning on the respective set of fixed effects.

We display this residual variation in Figure A4. There is substantial within-village variation in temperature anomalies. This variation diminishes somewhat after conditioning on

Figure A3: Histograms of village-level temperature anomalies



Note: Figure shows histograms of estimated annual village-level surface temperature anomalies in degrees celsius relative to the 1980-2021 temperature average, for each round of the Economic Census of India (1990, 1998, 2005, and 2013). Anomalies are calculated at the village level as the deviation between the average annual temperature in the two years before the census year and the long-run average annual temperature from 1980-2021. Sample is all villages in India for which Economic Census data is available.

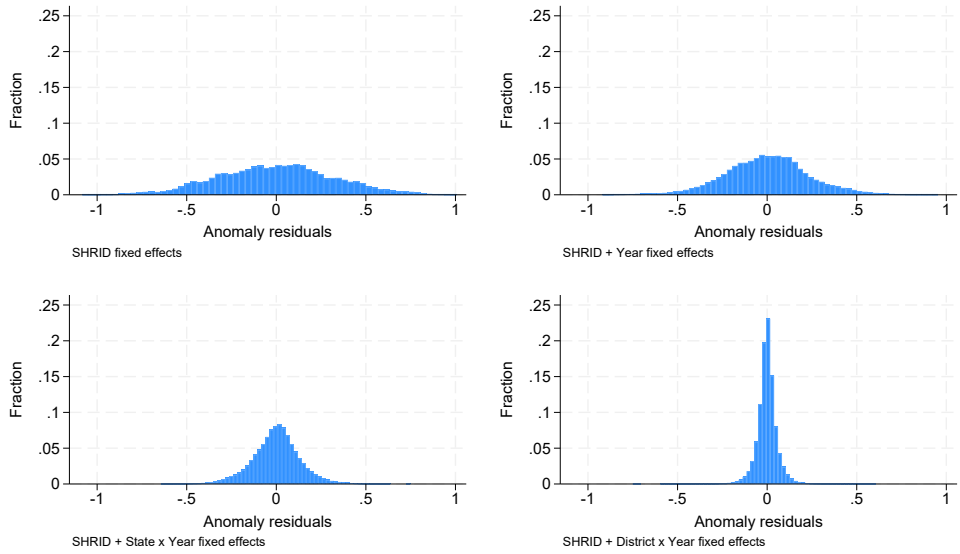
year or state-by-year effects, and more notably after conditioning on district-by-year effects. In other words, the most saturated specification exploits a relatively small portion of the overall variation in temperature shocks in our dataset.

C Robustness tests: Sample restrictions and sector effects

C.1 Robustness to sample restrictions

In this section, we consider the robustness of the main results to various different sample restrictions: *i*) the full sample of all firms and all SHRIDs, *ii*) the full sample of all firms in a balanced panel of SHRIDs, and *iii*) the sample of firms excluding one-person microenterprises but including all SHRIDs.

Figure A4: Residual variation in anomalies



Note: Figure shows estimates and 95% confidence intervals of β_τ from quantile regressions of firm size on temperature anomalies and geographic fixed effects. The estimates are normalized by quantiles of firm size to obtain percent changes. Standard errors clustered at the village level. Anomalies are calculated at the village level as the deviation between the average annual temperature in the two years before the census year and the long-run average annual temperature from 1980-2021. Sample is the unit level data of the last four Economic Censuses of India (1990, 1998, 2005, and 2013).

The results of re-estimating the regressions in Table 1 for these three different samples are in Appendix Table A2. Broadly, the effects are negative and significant in columns (1) and (5) for all samples. However, the effects are statistically insignificant for columns (2) and (3), the models with year and state-by-year fixed effects. In Panel B in particular (the full sample), the point estimates for these specifications are close to zero.

Appendix Figures A5, A6, and A7 estimate the quantile regression plots for the full sample, the balanced SHRID panel, and the full sample without one-person firms, respectively.

C.2 Normalized quantile effects

The estimates in Figure 2 are not adjusted for firm size. In Figure A8 we present *normalized* quantile effects. We normalize the quantile coefficients by dividing by the average firm size in each quantile. The axis now represents percent changes in firm size. The downward slope

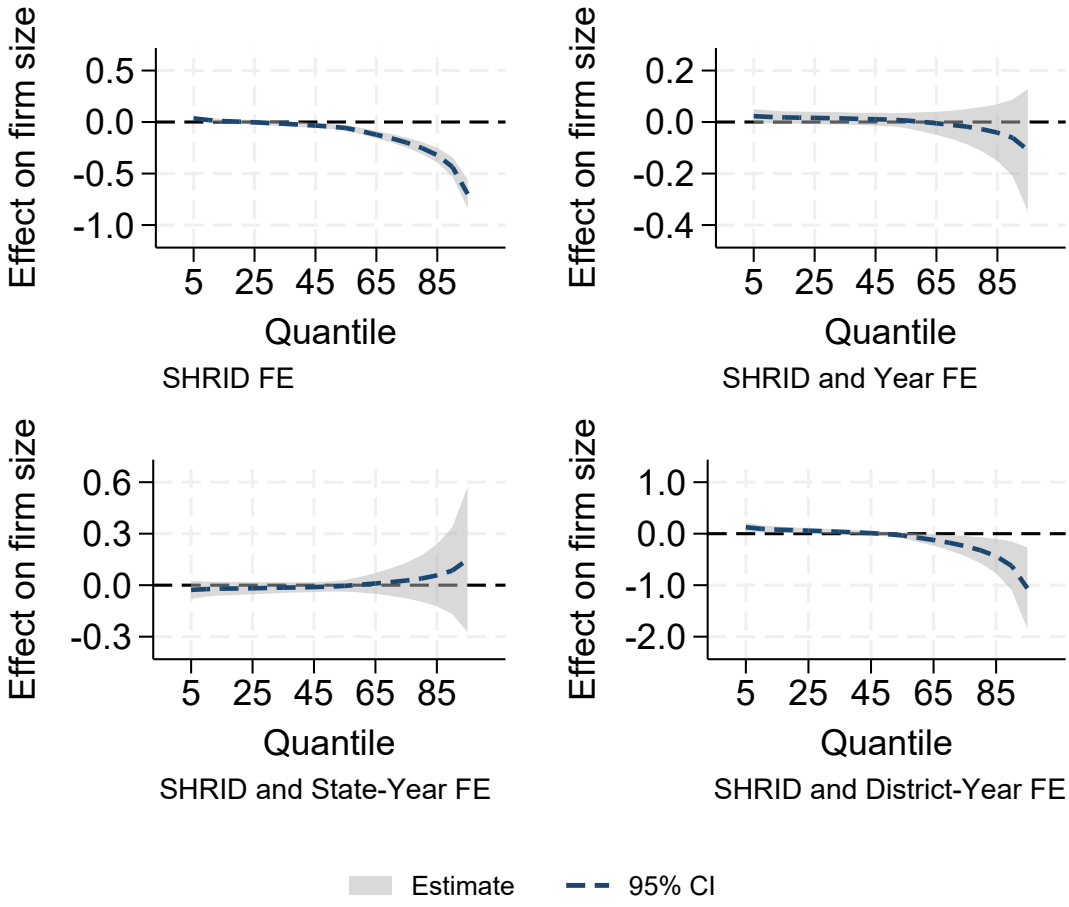
Table A2: Heat shocks and firm size: sample restrictions

Dependent variable	Firm size (employment)			
	(1)	(2)	(3)	(4)
Panel A: Balanced SHRID panel, all firms				
Max temp anomaly	-0.178***	-0.053*	-0.046	-0.163*
	(0.017)	(0.024)	(0.049)	(0.081)
Observations	69810363	69810363	69810363	69810363
Panel B: Full SHRID sample, all firms				
Max temp anomaly	-0.161***	-0.007	-0.01	-0.263**
	(0.026)	(0.034)	(0.045)	(0.085)
Observations	110600000	110600000	110600000	110600000
Panel C: Full SHRID sample, excluding one person firms				
Max temp anomaly	-0.278***	-0.102	-0.059	-0.825***
	(0.045)	(0.062)	(0.087)	(0.192)
Observations	47725693	47725693	47725693	47725693
SHRID FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	No
State $\times$ Year FE	No	No	Yes	No
District $\times$ Year FE	No	No	No	Yes

Note: Standard errors in parentheses clustered at the SHRID (village) level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013). Outcome is the total number of employees of the firm. Anomalies are calculated as the difference between the average annual maximum temperature of t and $t-1$ and the long-run average temperature from 1980-2021. Fixed effect specification given in table footer. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

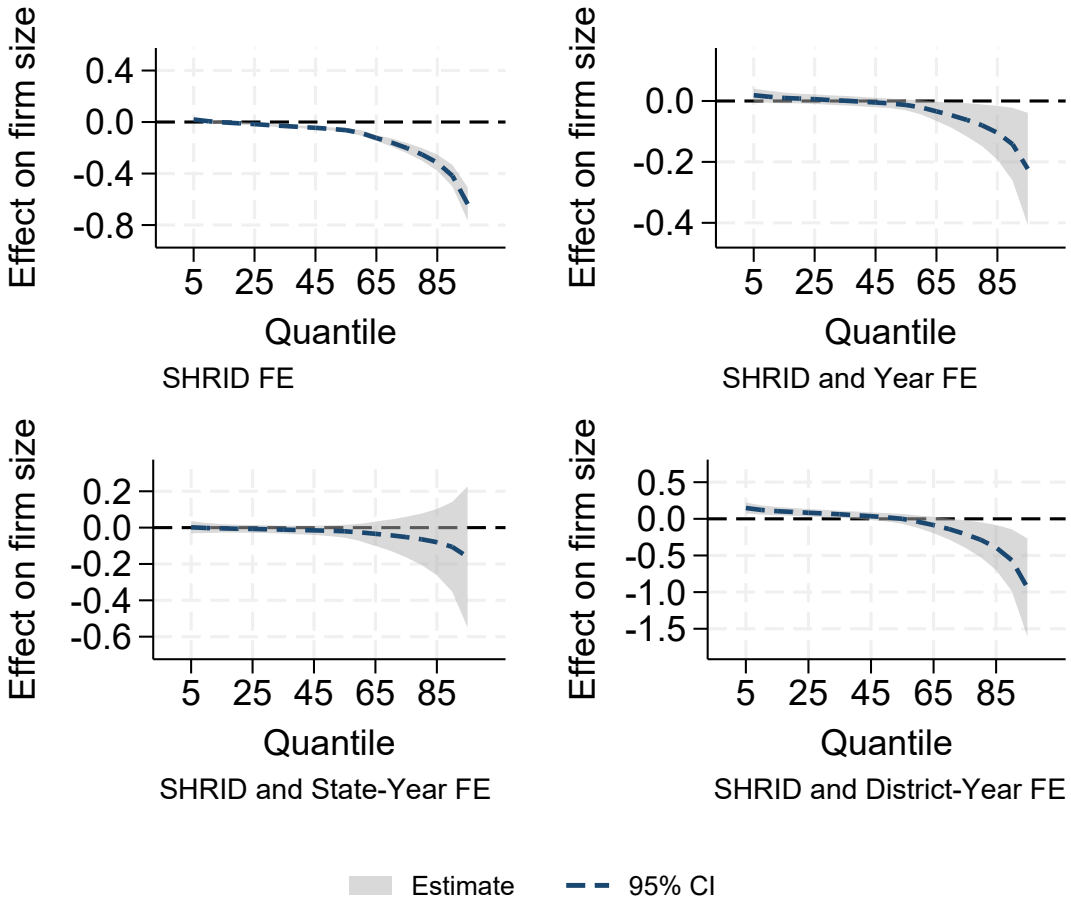
of the quantile curve becomes even more pronounced, particularly in the SHRID and district-year fixed effects specification.

Figure A5: Quantile regressions: full panel, all firms



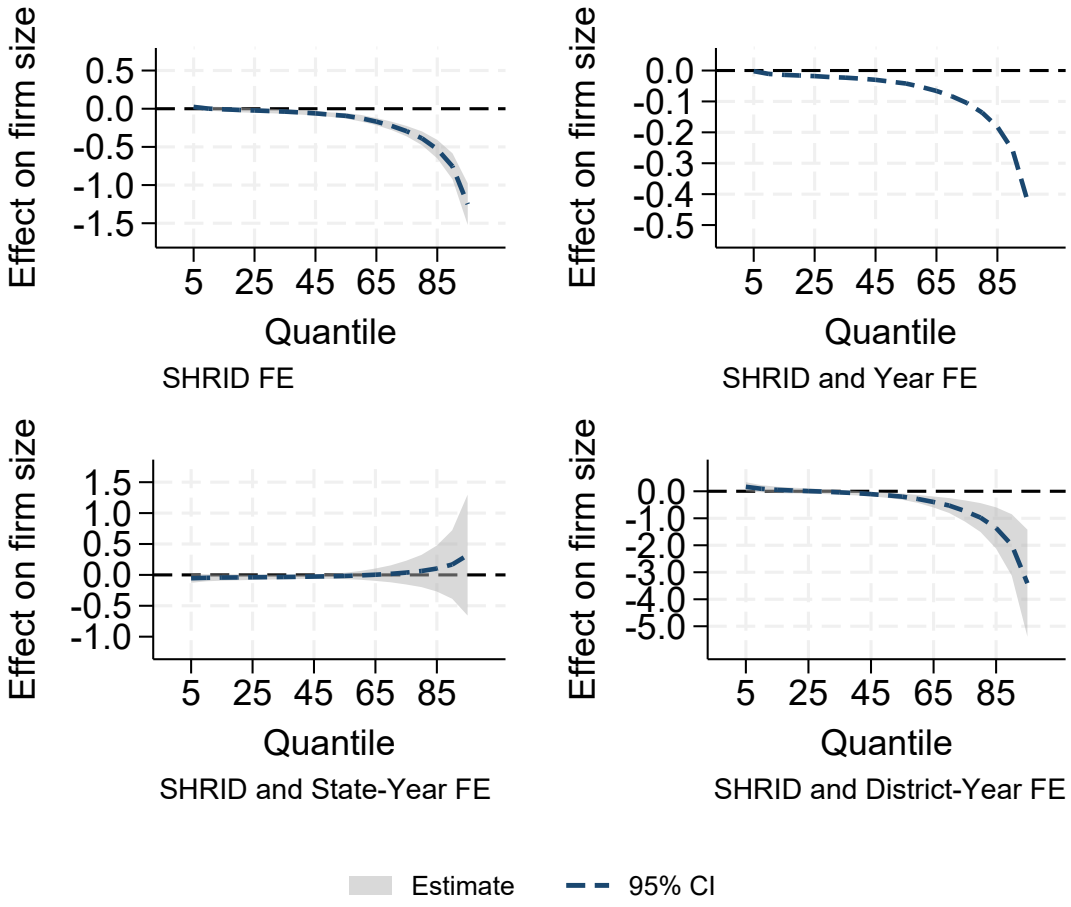
Note: Figure shows estimates and 95% confidence intervals of β_τ from quantile regressions of firm size on temperature anomalies and geographic fixed effects. The estimates are normalized by quantiles of firm size to obtain percent changes. Standard errors clustered at the village level. Anomalies are calculated at the village level as the deviation between the average annual temperature in the two years before the census year and the long-run average annual temperature from 1980-2021. Sample is the unit level data of the last four Economic Censuses of India (1990, 1998, 2005, and 2013).

Figure A6: Quantile regressions: balanced panel, all firms



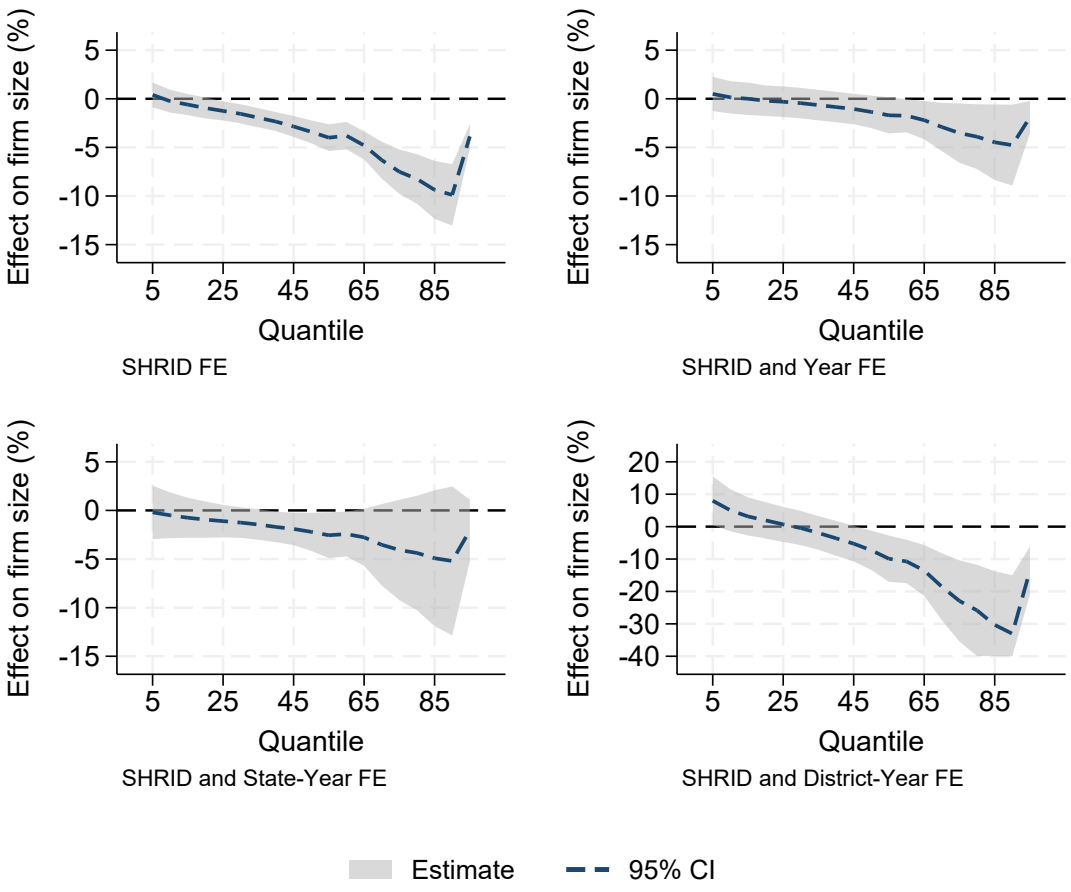
Note: Figure shows estimates and 95% confidence intervals of β_τ from quantile regressions of firm size on temperature anomalies and geographic fixed effects. The estimates are normalized by quantiles of firm size to obtain percent changes. Standard errors clustered at the village level. Anomalies are calculated at the village level as the deviation between the average annual temperature in the two years before the census year and the long-run average annual temperature from 1980-2021. Sample is the unit level data of the last four Economic Censuses of India (1990, 1998, 2005, and 2013).

Figure A7: Quantile regressions: full panel excluding single person firms



Note: Figure shows estimates and 95% confidence intervals of β_τ from quantile regressions of firm size on temperature anomalies and geographic fixed effects. The estimates are normalized by quantiles of firm size to obtain percent changes. Standard errors clustered at the village level. Anomalies are calculated at the village level as the deviation between the average annual temperature in the two years before the census year and the long-run average annual temperature from 1980-2021. Sample is the unit level data of the last four Economic Censuses of India (1990, 1998, 2005, and 2013).

Figure A8: Quantile regressions: normalized effects



Note: Figure shows estimates and 95% confidence intervals of β_τ from quantile regressions of firm size on temperature anomalies and geographic fixed effects. The estimates are normalized by quantiles of firm size to obtain percent changes. Standard errors clustered at the village level. Anomalies are calculated at the village level as the deviation between the average annual temperature in the two years before the census year and the long-run average annual temperature from 1980-2021. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013) with more than 1 employee within the balanced panel of SHRIDs.

C.3 Sector effects

Section 4.3 presents an analysis of sector-specific reallocation effects. Here, we expand on specific robustness tests. Appendix Table A3 estimates the main firm-level linear regression, splitting the sample into large macro-sectors of services and manufacturing. For services, in Panel B, the estimated effect again remains significant in three out of five specifications, with magnitudes similar to those in Table 1. Instead, Panel A reveals that the effect sizes are substantially larger in absolute magnitudes and more significant in the manufacturing sector. Combining these estimates with the subsample means in Appendix Table A1 reveals that in our preferred specification (column 5), a 1 degree increase in maximum temperature reduces firm size by 23.5% in manufacturing and 9.7% in services.

Table A3: Heat shocks and firm size by macro-sector

Dependent variable	Firm size (employment)			
	(1)	(2)	(3)	(4)
Panel A: Manufacturing				
Max temp anomaly	-0.463***	-0.072	-0.044	-1.539***
	(0.083)	(0.121)	(0.165)	(0.416)
Observations	6825406	6825406	6825406	6825406
Panel B: Services				
Max temp anomaly	-0.169***	-0.098	-0.078	-0.521*
	(0.039)	(0.056)	(0.096)	(0.217)
Observations	22101761	22101761	22101761	22101761
SHRID FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	No
State × Year FE	No	No	Yes	No
District × Year FE	No	No	No	Yes

Note: Standard errors in parentheses clustered at the SHRID (village) level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013) with more than 1 employee within the balanced panel of SHRIDs. Outcome is the total number of employees of the firm. Anomalies are calculated as the difference between the average annual maximum temperature of t and $t-1$ and the long-run average temperature from 1980-2021. Fixed effect specification given in table footer. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The most rigorous test of whether sector effects are driving the firm size response to heat shocks is to include disaggregated sector fixed effects in the main model (Appendix Table

A4). Panel A reveals that, after conditioning on 90 3-digit subsectors, the estimated average firm size effects hardly change, remaining significant at 1% in the same specifications. Panel B interacts the sector fixed effects with year indicators to account for not just sector-specific effects, but also potential sectoral trends in firm size and exposure to climate shocks. The results in columns (1)-(4) seem to reveal that the average firm size effect vanishes conditional on sector-specific trends. However, this is because these specifications do not control sufficiently for heterogeneous spatial trends. Instead, in our preferred specification of column (5), the estimate is nearly identical to that of Table 1 1.

Table A4: Heat shocks and firm size, sectoral fixed effects

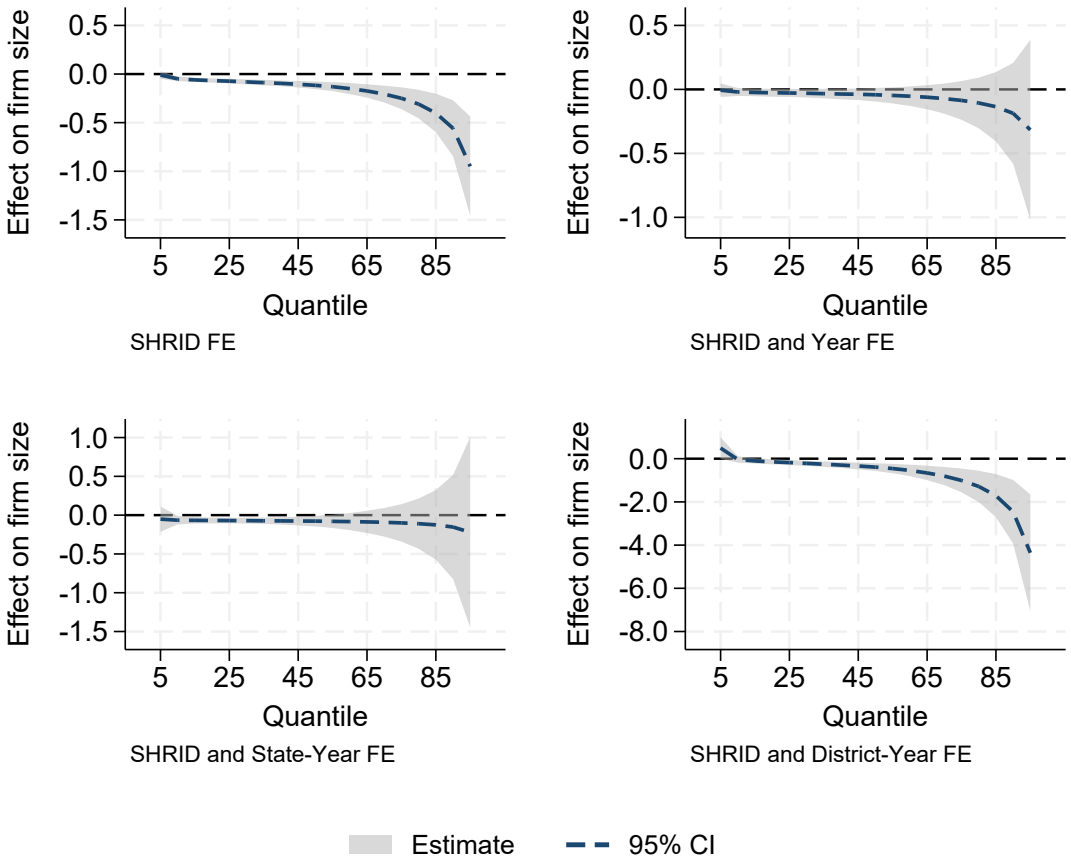
Dependent variable	Firm size (employment)			
	(1)	(2)	(3)	(4)
Panel A: Sector fixed effects				
Max temp anomaly	-0.198***	-0.069	-0.092	-0.778***
	(0.041)	(0.057)	(0.091)	(0.208)
Panel B: Sector-by-year fixed effects				
Max temp anomaly	-0.083	-0.083	-0.093	-0.740***
	(0.056)	(0.056)	(0.089)	(0.209)
SHRID FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	No
State $\times$ Year FE	No	No	Yes	No
District $\times$ Year FE	No	No	No	Yes

Note: Standard errors in parentheses clustered at the SHRID (village) level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013) with more than 1 employee within the balanced panel of SHRIDs. Outcome is the total number of employees of the firm. Anomalies are calculated as the difference between the average annual maximum temperature of t and $t-1$ and the long-run average temperature from 1980-2021. Fixed effect specification given in table footer. Sector fixed effects refer to 90 NIC87 3-digit subsectors of economic activity. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Lastly, Appendix Figure A9 plots quantile curves conditional on sector fixed effects. The results are nearly identical to, and if anything somewhat more pronounced than, those of Figure 2, implying nearly a 2-employee reduction in firm size at the right tail of the distribution. In sum, we find evidence of some between-sector reallocation, as well as quantitatively mean-

ingful heterogeneity in effects across manufacturing and services. It is indeed highly likely that sectors are not equally exposed to, or equally able to adapt to, increasing heat shocks. However, the results of the sectoral fixed effects specification suggest that these sectoral heterogeneities do not appear to be driving effects across the firm size distribution. Instead, it appears that large firms contract employment most regardless of the sector in which they operate.

Figure A9: Quantile regressions with sector fixed effects



Note: Figure shows estimates and 95% confidence intervals of β_τ from quantile regressions of firm size on temperature anomalies and geographic fixed effects. Standard errors clustered at the village level. Anomalies are calculated at the village level as the deviation between the average annual temperature in the two years before the census year and the long-run average annual temperature from 1980-2021. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013) with more than 1 employee within the balanced panel of SHRIDs.

D Estimating the productivity impact of labor reallocation

Using the pooled ASI and NSS firm-level data, we estimate the $ARPL$ gap between formal and informal firms through the following OLS regression:

$$ARPL_i = \alpha + \beta Informal_i + X_i + \epsilon_i \tag{10}$$

where $ARPL$ is value added per worker and $Informal$ is a dummy for firms with less than 8 workers. The baseline version does not include any controls; thus, the constant term of the baseline specification measures the mean ARPL in formal firms. The robustness variants of this specification include X_i controls: 5-digit industry group and state fixed effects. The regressions are weighted using the inverse sampling probabilities included in the ASI and NSS datasets.

Table A5 shows the estimated labor productivity differentials between firm types, in USD. Column (1) shows the raw differential, as well as the constant term (the mean ARPL in formal formal firms). Columns (2) and (3) include industry and state fixed effects to help adjust for unobserved differences in workers' human capital across firms. The underlying concern is that part of the gap in observed labor productivity between formal and informal firms could be due to a higher average human capital level of workers in formal firms; this component of the raw gap should be excluded when assessing the change in aggregate productivity from worker reallocation.

As expected, using within-industry variation industry reduces the estimated labor productivity gap between formal and informal firms. The state fixed effects do not make a substantial additional difference. However, these adjustments will be overly conservative, since a substantial share of the cross-sector variance in labor productivity will be due to firm rather than worker effects.

Columns (4) and (5) present two additional estimates of the $ARPL$ differential. It has been estimated by de Mel et al. (2009) that micro-enterprises in low- and middle-income countries may under-report revenue by about 30 percent. As this would tend to bias the productivity gap upwards, we present an estimate where the reported value added of informal firms has been adjusted for 30 percent under-reporting. Lastly, we present the ARPL gap estimated by using 10 workers as the threshold for categorizing firms as formal. This is because our

Table A5: Productivity gaps between formal and informal sectors: NSS-ASI Survey Estimates

Outcome variable	Value-added per worker				
	(1)	(2)	(3)	(4)	(5)
Informal	-2172.315***	-1576.889***	-1494.859***	-1594.732***	-5283.343***
	(111.472)	(108.057)	(107.113)	(112.280)	(257.116)
Formal average	3520.006***				6636.827***
	(110.691)				(256.779)
Sector FE	No	Yes	Yes	No	No
State FE	No	No	Yes	No	No
Observations	134480	134449	134449	134480	134480
R^2	0.005	0.139	0.157	0.002	0.007

Note: Estimates based on combined National Sample Survey of Unincorporated Non-agricultural Enterprises (2015-16) and the Annual Survey of Industries (2015) establishment-level dataset. Value added per worker is expressed in 2015 USD terms. The employment size cutoff used to define the formal and informal sectors is 8 in columns 1-4 and 10 in column 5. Column 4 uses an alternative version of the outcome variable based on adjusting the reported value added of informal firms for 30 percent under-reporting. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

baseline 8-worker cutoff—which follows the Economic Census's categorization scheme—is a relatively inclusive definition of formal firms in the Indian context. For example, 10 workers is a commonly used cutoff for formal manufacturing firms (Kanbur, 2017). By including borderline informal firms in the formal segment, the 8-worker cutoff may lead to an underestimation of the latter's productivity advantage. Indeed, the $ARPL$ gap in column (5) is the largest of all specifications.

Table A6: Labor reallocation and aggregate productivity change accounting

		Raw	Industry FE	Industry & State FE	Underreporting-adjusted	10-worker cutoff
		(1)	(2)	(3)	(4)	(5)
NSS-ASI	Informal ARPL	1347.69	1943.12	2025.15	1925.27	1353.48
	Formal ARPL	3520.01	3520.01	3520.01	3520.01	6636.83
	ARPL ratio	0.38	0.55	0.58	0.55	0.20
Economic Census	Baseline informal share (%)	77	77	77	77	77
	Baseline formal share (%)	23	23	23	23	23
	Δ informal employment share (%)	2.10	2.10	2.10	2.10	2.10
	Δ ARPL	-2.47	-1.44	-1.33	-1.46	-4.32

Note: Estimates based on the combined National Sample Survey of Unincorporated Non-agricultural Enterprises (2015-16) and Annual Survey of Industries (2015) dataset (NSS-ASI) and the 6th Economic Census of India. ARPL is value added per worker, expressed in USD terms using the 2015 exchange rate. The ARPL ratio is the ratio of value added per worker in informal and formal establishments. The "raw" or baseline ARPL ratio is estimated by regressing value added per worker on a dummy for informality of the establishment. The "industry FE" and "industry + state FE" ARPL ratios are based on including 5-digit industry and state fixed effects in the baseline regression. The "underreporting-adjusted" estimate is based on correcting value added per worker in informal establishments for an assumed under-reporting by 30%. The "10-worker cutoff" estimate is based on defining the formal sector using a minimum size of 10 workers, as compared to a baseline cutoff of 8 workers. The baseline shares of formal and informal firms in total non-agricultural employment in India are estimated using the 2013 Economic Census data. The change in these shares due to a 1 degree heat anomaly increase is based on the regression estimate reported in column 5 of Table 2.

Finally, Table A6 shows the calculation details that translate productivity differentials and the formal-informal reallocation effect into changes in aggregate $ARPL$, following the formula in equation (5). The first section of the table reprints all the estimates of Table A5, which are based on the NSS-ASI data. The second section takes the estimates from Table 2, column (5), as well as estimates of the share of formal and informal firms, both from the Economic Census.

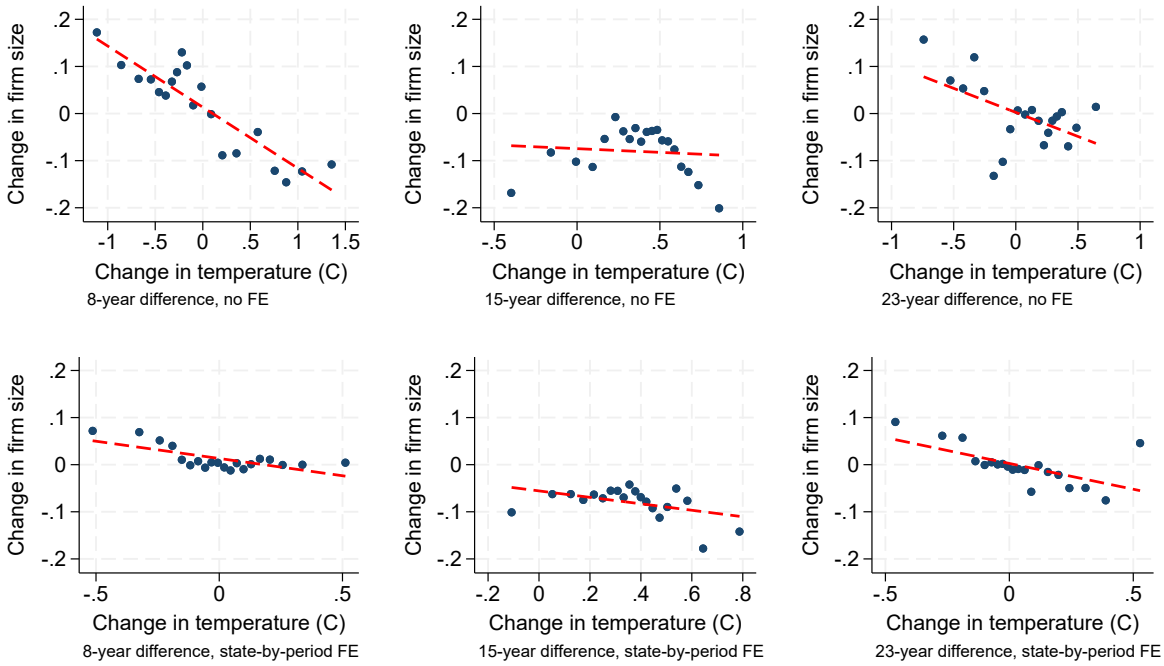
E Robustness tests: Long differences

Section 6 estimates the long-run impacts of rising temperature on the distribution of firm size. Here, we present several auxiliary results to Table 4. First, in Figure A10, we present binned scatterplots of the relationship between long-run change in firm size and temperature across 8, 15, and 23-year periods. The top panel shows the unconditional long-difference relationship, while the bottom panel shows the relationship after controlling for state-by-period fixed effects. Overall, the results reveal robust negative relationships, which tend to be weakest for the 15-year period but relatively consistent overall. The relationships are not meaningfully sensitive to the inclusion of fixed effects.

Next, we test the robustness of measurement choices in Table 4 by using the long-log-differences in Table A7. In this case, the outcome variable is the log-difference of firm size instead of its level, so the coefficients can be interpreted as percent changes for small changes. Overall, the results are highly consistent with the main estimates.

Table A8 presents robustness checks for the long-difference estimates in Table 4. It shows that the long-run results are generally robust to excluding single-person firms when computing village-level averages. The results are also robust to replacing state-by-period fixed effects with district-by-period fixed effects (column 3), which is equivalent to conditioning on district-specific trends given the long-differencing approach. Across most specifications, long-run temperature increase has a negative association with firm size. The one exception is the 23-year panel with no fixed effects (Panel C, column 1), where the association is positive and significant at the 1% level; however, this sign becomes negative once state or district fixed effects are included.

Figure A10: Heat and firm size: long differences



Note: Figure shows binned scatterplots of the relationship between the change in firm size and change in temperature over periods of 8 (1990-1998, 1998-2005, 2005-2013), 15 (1990-2005, 1998-2013), and 23 (1990-2013) years. Temperature at each endpoint of a long difference period is calculated as a 3-year lagged average. Bottom panel includes state-by-period fixed effects, where period refers to the long difference period.

Table A7: Heat shocks and firm size: long difference estimates in logs

Dependent variable	Change in firm size (employment)					
Difference	23-year		15-year		8-year	
	(1)	(2)	(3)	(4)	(5)	(6)
Temperature change (C)	-0.075***	-0.060***	-0.010***	-0.027***	-0.046***	-0.026***
	(0.003)	(0.005)	(0.002)	(0.003)	(0.001)	(0.003)
State × Period FE	No	Yes	No	Yes	No	Yes
Observations	331998	331998	697146	697146	1097327	1097327
R^2	0.002	0.015	0.000	0.026	0.003	0.024

Note: Standard errors in parentheses clustered at the SHRID (village) level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013), collapsed to the village level. Outcome is the log change in average number of employees of private firms in a village over the long difference period t, k. Temperature shocks are calculated as the change in the 2-year lagged average annual maximum temperature over the long difference period t, k. Fixed effect specification given in table footer. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A8: Heat shocks and firm size: Long difference estimates excluding single-person firms and including district FEs

Dependent variable	Change in firm size (employment)		
	(1)	(2)	(3)
Panel A: 8-year			
Temperature change (C)	-0.284***	-0.196***	-0.139**
	(0.010)	(0.025)	(0.060)
Observations	677453	677453	677453
R^2	0.002	0.013	0.037
Panel B: 15-year			
Temperature change (C)	-0.082***	-0.137***	-0.067
	(0.021)	(0.031)	(0.070)
Observations	431919	431919	431919
R^2	0.000	0.017	0.051
Panel C: 23-year			
Temperature change (C)	0.201***	-0.065	-0.403***
	(0.026)	(0.040)	(0.106)
Observations	212554	212554	212554
R^2	0.000	0.012	0.047
State × Period FE	No	Yes	No
District × Period FE	No	No	Yes

Note: Standard errors in parentheses clustered at the SHRID (village) level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013), collapsed to the village level. Outcome is the change in average number of employees of private firms in a village over the long difference period t, k, where single-person firms have been excluded before computing village level averages. Temperature shocks are calculated as the change in the 2-year lagged average annual maximum temperature over the long difference period t, k. Fixed effect specification given in table footer. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

F Types of adaptation actions: SACA survey

We use the SACA survey data to examine which types of adaptation actions account for the observed differences between large and small firms. We regress binary indicators for each of the ten potential adaptation actions on long-term temperature change and its interaction with firm size. The results are presented in Table A9. Panel A shows that long-term temperature change has a statistically significant positive relationship with all major adap-

Table A9: Types of adaptation measures: South Asia Climate Adaptation Survey regressions

	Long-run temperature increase									
	Bldg upgrade	Relocate	Fans	AC	Change supplier	Change sales strategy	Change prod.	EEA	Labor	Heat Plan
Δ Temp.	0.346***	0.108*	0.125*	0.319***	0.123**	0.282***	0.267***	0.454***	0.519***	0.585***
	(0.065)	(0.058)	(0.076)	(0.065)	(0.058)	(0.061)	(0.058)	(0.065)	(0.057)	(0.054)
N	959	959	959	959	959	959	959	959	959	959
	Long-run temperature increase × Firm Size									
	Bldg upgrade	Relocate	Fans	AC	Change supplier	Change sales strategy	Change prod.	EEA	Labor	Heat Plan
Δ Temp.	0.204***	0.017	0.130	0.273***	0.036	0.215***	0.177***	0.443***	0.491***	0.575***
	(0.075)	(0.066)	(0.086)	(0.078)	(0.066)	(0.069)	(0.066)	(0.074)	(0.067)	(0.067)
Δ Temp. × Size	0.002***	0.001***	-0.000	0.001	0.001***	0.001**	0.001***	0.000	0.000	0.000
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)
N	959	959	959	959	959	959	959	959	959	959

Note: All regressions include state and sector fixed effects. Robust SEs. * p<0.10, ** p<0.05, *** p<0.01. Δ *Temp* is the increase in mean maximum temperature between 1989-91 and 2019-21. *Anomaly* is the deviation of the 2020-2021 mean maximum temperature from the long-run average (1980-2021). *Experienced Heat* is a dummy variable equal to one for firms that report that their business was affected by an episode of extreme heat in the last five years. *Size* is total employment. The outcome variables are dummies for types of adaptation measures undertaken in the past five years: building upgrading; relocation; fan installation; air conditioner installation; change of suppliers; change in sales strategy; change in product; energy efficient equipment installation (EEA); change in labor mix; and preparation of a heat plan.

tation measures. The effect size are markedly larger for building upgrades, air conditioner installation, sales strategy changes, product changes, energy-efficient equipment installation, labor composition adjustments, and heat plan development. Panel B introduces an interaction of long-term temperature change with firm size. It shows that among these prominent adaptation measures, the relationship with long-term temperature increase is significantly larger among larger firms for the following: upgrading buildings, changing sales strategy and changing products.

G Additional robustness tests

G.1 Heat shock measurement

Our results may be sensitive to specification choices for the heat shock. We consider robustness to several different measurements of heat. First, it is possible that the results are sensitive to use of the average daily maximum temperature for the calculation of anomalies. In Table A10, we re-estimate the main models of Table 1, calculating the temperature anomaly

as the deviation of the average daily mean or minimum temperature from its long-run value. We find similar results when defining the temperature anomaly using average or minimum daily temperatures, though they are somewhat less pronounced for the minimum temperature specification.

Table A10: Heat shocks and firm size, robustness to temperature measurement

Dependent variable	Firm size (employment)			
	(1)	(2)	(3)	(4)
Panel A: Mean temperature				
Mean temp anomaly (2-year lag)	-0.350***	-0.132*	-0.156	-1.055***
	(0.057)	(0.066)	(0.098)	(0.269)
Panel B: Min temperature				
Min temp anomaly (2-year lag)	-0.352***	-0.093	-0.094	-1.245***
	(0.082)	(0.091)	(0.141)	(0.306)
Observations	29305106	29298481	29298481	29298481
SHRID FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	No
State × Year FE	No	No	Yes	No
District × Year FE	No	No	No	Yes

Note: Standard errors in parentheses clustered at the SHRID (village) level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013). Outcome is the total number of employees of the firm. Anomalies are calculated as the difference between the average annual maximum temperature of the given lag period and the long-run average temperature from 1980-2021. Fixed effect specification given in table footer. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Throughout, we use the average of temperature anomalies in t and $t-1$ as our main independent variable, to reduce the influence of year-to-year noise in the temperature series. We test whether the results are sensitive to the choice of lag period. In Table A11, we calculate the anomaly over lags of one and three years instead of two. We find that the effect holds for three-year lags but not for only the census-year (that is, the one-year lag), consistent with increased noise from year-to-year temperature fluctuation and limited response time for firms when the shock is measured in the contemporaneous year.

Finally, we consider an entirely different specification of temperature that does not use anomalies. Rather, we implement a model following Burgess et al. (2017) and Caggese et al. (2024), among many others, in which temperature enters semi-parametrically as the number of days in specific temperature interval. Specifically, for firm i in village v in census year t, we

Table A11: Heat shocks and firm size, robustness to lag

Dependent variable	Firm size (employment)			
	(1)	(2)	(3)	(4)
Panel A: Current year				
Max temp anomaly (census year)	-0.059	0.096	-0.100	-0.315
	(0.043)	(0.058)	(0.076)	(0.162)
Panel B: 3-year lag				
Max temp anomaly (3-year lag)	-0.179***	-0.163*	-0.283*	-0.754**
	(0.051)	(0.070)	(0.123)	(0.248)
Observations	29305106	29298481	29298481	29298481
SHRID FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	No
State × Year FE	No	No	Yes	No
District × Year FE	No	No	No	Yes

Note: Standard errors in parentheses clustered at the SHRID (village) level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013). Outcome is the total number of employees of the firm. Anomalies are calculated as the difference between the average annual maximum temperature of the given lag period and the long-run average temperature from 1980-2021. Fixed effect specification given in table footer. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

estimate the model

$$y_{ivt} = \alpha + \sum_j \beta_j days_{jvt} + \delta_v + \delta_t + \delta_{vt} + \epsilon_{ivt}$$

Where j are bins of the distribution of temperatures, and $days_{jvt}$ indicates the number of days that village v experienced in temperature bin j in round t. As before, we average this number over t and $t-1$. We set bins in intervals of 10C from 0 to 40, with endpoints at < 0 and ≥ 40, and average the number of days in each temperature bin in the census year and $t-1$.[28] The estimates in Table A12 reveal that firm size remains a downward sloping function of temperature, consistent with the main results. The largest and most significant effects on firm size occur in the ≥ 40 degree bin. These results are robust to most fixed effect specifications and the use of an exponential Poisson pseudo maximum likelihood (PPML) model to account for the right-skewness and count nature of the firm size outcome (Wooldridge, 1999).

Finally, it is possible that our linear specification of temperature anomalies is unable to

[28] All estimates omit the bin between 10 and 20C.

Table A12: Firm size and heat: number of days

Dependent variable	Firm size (employment)			
	(1)	(2)	(3)	(4)
Panel A: Linear regression				
Average number of days $Temp < 0$	0.000115	-0.0146	-0.00623	0.0704
	(0.0223)	(0.0235)	(0.0320)	(0.0560)
Average number of days $0 \leq Temp < 10$	0.0192	0.00970	0.0160	0.0374
	(0.0213)	(0.0224)	(0.0252)	(0.0339)
Average number of days $10 \leq Temp < 20$				
Average number of days $20 \leq Temp < 30$	-0.0181***	-0.0279***	-0.00546	-0.0231
	(0.00696)	(0.00863)	(0.00920)	(0.0245)
Average number of days $30 \leq Temp < 40$	-0.0219***	-0.0309***	-0.00591	-0.0297
	(0.00695)	(0.00853)	(0.00929)	(0.0247)
Average number of days $Temp \geq 40$	-0.0330***	-0.0338***	-0.0169*	-0.0447*
	(0.00688)	(0.00838)	(0.00993)	(0.0258)
Panel B: Poisson regression				
Average number of days $Temp < 0$	0.000766	-0.00281	0.000225	0.0184
	(0.00623)	(0.00661)	(0.00850)	(0.0127)
Average number of days $0 \leq Temp < 10$	0.00591	0.00369	0.00609	0.00980
	(0.00604)	(0.00637)	(0.00719)	(0.00785)
Average number of days $10 \leq Temp < 20$				
Average number of days $20 \leq Temp < 30$	-0.00419**	-0.00643***	-0.00123	-0.00618
	(0.00175)	(0.00207)	(0.00226)	(0.00467)
Average number of days $30 \leq Temp < 40$	-0.00514***	-0.00719***	-0.00146	-0.00808*
	(0.00171)	(0.00204)	(0.00228)	(0.00471)
Average number of days $Temp \geq 40$	-0.00828***	-0.00833***	-0.00382	-0.0113**
	(0.00184)	(0.00211)	(0.00247)	(0.00497)
Observations	24443274	24443274	24443274	24443274
SHRID FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	No
State $\times$ Year FE	No	No	Yes	No
District $\times$ Year FE	No	No	No	Yes

Note: Standard errors in parentheses clustered at the SHRID (village) level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013). Temperature bins are calculated as the number of days in which the maximum temperature fell within the indicate range, averaged between the census year and $t-1$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

capture more complex relationships between firm outcomes and heat. This has important economic implications, as Barreca et al. (2015) argue that differential impacts of heat shocks between initially hotter vs. colder places can be interpreted as evidence of long-run adapta-

tion. We therefore test whether effects of temperature vary by initial temperature. We interact the temperature anomaly in our main specification with an indicator variable equaling one if the long-term average daily maximum temperature is 30C or greater. Table A13 finds mixed evidence of systematic differences in firm size effects for hotter and cooler places. In some specifications (columns 2 and 3), hotter locations have significantly smaller firm size reductions in response to heat, consistent with adaptation. However, in the most rigorous specification (column 5), these differences shrink to zero, suggesting limited long-run adaptation on average.

Table A13: Heat shocks and firm size: heterogeneity by long-run temperature

Dependent variable	Firm size (employment)			
	(1)	(2)	(3)	(4)
Max temp anomaly	-0.374***	-0.237**	-0.165	-0.834***
	0.062	0.078	0.105	0.253
Max temp anomaly × Hot location	0.242*	0.159	0.073	-0.072
	0.123	0.137	0.129	0.104
Observations	24443274	24443274	24443274	24443274
SHRID FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	No
State × Year FE	No	No	Yes	No
District × Year FE	No	No	No	Yes

Note: Standard errors in parentheses clustered at the SHRID (village) level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013). Anomalies are calculated as the difference between the average annual maximum temperature of the given lag period and the long-run average temperature from 1980-2021. A hot location is defined as a village whose average annual maximum temperature exceeds 30C. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

G.2 Model specification

We consider robustness to two key model specification choices—the use of controls, and the choice of estimation method. Table A14 tests the robustness of the main result to several specification choices. In Panel A, we control for precipitation anomalies, constructed identically to our temperature anomalies. The results are essentially unchanged. Next, Panel B estimates an exponential model with a fixed effects Poisson regression estimator, to account for outliers given the skewness of the firm size distribution in India, with a large mass at 1, as well as the discrete count nature of the outcome. Estimates from the Poisson regression,

interpreted as percent changes, are similar in relative magnitude to the main estimates, and again significant in the same three out of five specifications.

Table A14: Heat shocks and firm size, robustness to specification

Dependent variable	Firm size (employment)			
	(1)	(2)	(3)	(4)
Panel A: Controlling for precipitation				
Max temp anomaly (2-year lag)	-0.424***	-0.348***	-0.214	-0.803***
	(0.050)	(0.068)	(0.112)	(0.211)
Panel B: Poisson regression				
Max temp anomaly (2-year lag)	-0.067***	-0.033*	-0.038	-0.177***
	(0.010)	(0.013)	(0.020)	(0.045)
Observations	29298481	29298481	29298481	29298481
SHRID FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	No
State $\times$ Year FE	No	No	Yes	No
District $\times$ Year FE	No	No	No	Yes

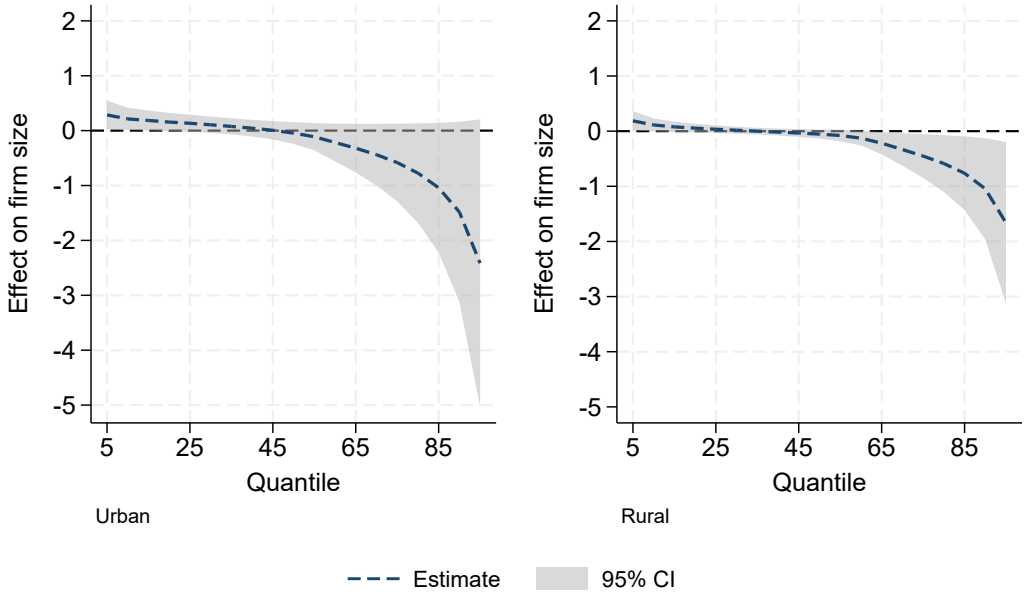
Note: Standard errors in parentheses clustered at the SHRID (village) level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013). Outcome is the total number of employees of the firm. Anomalies are calculated as the difference between the average annual maximum temperature of the given lag period and the long-run average temperature from 1980-2021. Panel A includes a control for the 2-year lag annual precipitation anomaly-the difference between the average annual precipitation of years t and $t-1$ and the long-run average annual precipitation for 1980-2021. Fixed effect specification given in table footer. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

G.3 Urban heat islands

Our results may also be driven by the urban heat island effect. Because of the UHI, urban areas tend to be hotter, and large firms cluster in urban areas. In a naive specification, this would lead us to underestimate the negative effect of heat; however, our use of temperature anomalies and fixed effect strategy address this concern. A more subtle issue is that a measured 1°C anomaly may feel hotter in urban areas, which may not be captured in the relatively coarse temperature data, which can underestimate temperature extremes. This correlated measurement error could generate the illusion of larger negative effects for large firms,

when in fact these large urban firms simply experience larger unmeasured heat shocks. We test this hypothesis by splitting the sample into urban and rural SHRIDs and re-running both the fixed effects (Table A15) and quantile regressions (Figure A11) on each subsample. The main results hold in both subsamples.

Figure A11: Heat and firm size: quantile regressions by urban location



Note: Figure shows estimates and 95% confidence intervals of β_τ from quantile regressions of firm size on temperature anomalies and geographic fixed effects for subsamples of urban and rural villages. Standard errors clustered at the village level. Anomalies are calculated at the village level as the deviation between the average annual temperature in the two years before the census year and the long-run average annual temperature from 1980-2021. Sample is the unit level data of the last four Economic Censuses of India (1990, 1998, 2005, and 2013).

Table A15: Heat shocks and firm size: urban and rural

Dependent variable	Firm size (employment)			
	(1)	(2)	(3)	(4)
Panel A: Urban				
Max temp anomaly	-0.414***	-0.250**	-0.197	-1.094**
	(0.081)	(0.094)	(0.151)	(0.391)
Observations	14951242	14949716	14949715	14949716
Panel B: Rural				
Max temp anomaly	-0.136**	-0.108	-0.107	-0.534
	(0.047)	(0.067)	(0.106)	(0.276)
Observations	9498657	9493558	9493558	9493558
SHRID FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	No
State × Year FE	No	No	Yes	No
District × Year FE	No	No	No	Yes

Note: Standard errors in parentheses clustered at the SHRID (village) level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013). Anomalies are calculated as the difference between the average annual maximum temperature and the long-run average temperature from 1980-2021, averaged between t and $t-1$. Relevant sample of villages given in panel headers. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

H Alternative explanations

H.1 Firm exit

A plausible explanation for the short-run results in Section 4.2 is that small and large respond identically to heat, but small firms exit the market while large firms contract but remain active. This explanation is mechanical, rather than behavioral given the large density of one-person firms in the data. To test this hypothesis, we estimate subdistrict-level regressions over firm size bins similar to equation (2). However, instead of employment shares in each firm size bin as the outcomes, we use the total number of firms. The logic behind this test is that if our results are driven by small firm exit, we should expect that the number of small firms reduces substantially in response to heat shocks.

Table A16 contains the results. We estimate the model using two different methods—fist, a log-linear regression (Panel A), and second, an exponential Poisson pseudo-maximum likelihood (PPML) model to account for the highly right-skewed count outcome (Panel B).

Table A16: Heat shocks and firm counts, subdistrict level

Dependent variable	Number of firms					
Firm size	1		2-7		8 and above	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Log-linear regression						
Max temp anomaly	-0.036**	-0.040**	0.066***	0.059***	-0.059***	-0.073***
	(0.018)	(0.017)	(0.015)	(0.014)	(0.021)	(0.020)
District FE	Yes	No	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Subdistrict FE	No	Yes	No	Yes	No	Yes
Observations	22072	22072	22032	22032	21176	21176
Panel B: Poisson regression						
Max temp anomaly	-0.013	-0.025*	0.031	0.013	0.015	-0.015
	(0.017)	(0.015)	(0.024)	(0.020)	(0.036)	(0.035)
District FE	Yes	No	Yes	No	Yes	No
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Subdistrict FE	No	Yes	No	Yes	No	Yes
Observations	22095	22093	22095	22093	22095	22095

Note: Standard errors in parentheses clustered at the subdistrict level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013). Outcome is the number of firms of a given size in the subdistrict, indicated in table header. Anomalies are calculated as the difference between the average annual maximum temperature and the long-run average temperature from 1980-2021, averaged between t and $t-1$. Panel B uses an exponential Poisson pseudo maximum likelihood (PPML) estimator. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In Panel A, there is a small but significant 4% reduction in the number of firms, but this is accompanied by an even larger 6-7% reduction in the number of large firms, as well as an *increase* in the number of mid-sized firms. Panel B reconciles these conflicting results by re-estimating the regressions with a PPML model, using an exponential functional form to account for the highly right-skewed count outcome. This model yields small and statistically insignificant effects for all firm size groups. The PPML approach is likely to yield a more reliable estimator of the model if the true relationship is exponential (Wooldridge, 1999).

H.2 Migration

It is well-established in the literature that rising temperatures spur migration (Mueller et al., 2014; Baez et al., 2017; Lang et al., eds, 2025). Out-migration of working-age men might force firms to contract due to labor shortage. This is a labor supply explanation of contraction, rather than the labor demand story we favor.[29] We test this explanation by estimating the relationship between temperature anomalies and migration in data from the Census of India. Specifically, we take district-level migration census data for 2001 and 2011, where migrant origin districts are collected. The data are presented as origin district-level counts of migrants, who are categorized based on the recency of their migration in bins of 0-5 years and 6-11 years. We then calculate the average temperature anomaly for the origin district in the period in question, yielding a pseudo panel of four periods—two census rounds ten years apart, each with two five-year periods. Finally, we regress the log of total district migrants in a given period on the average temperature anomaly for that period.

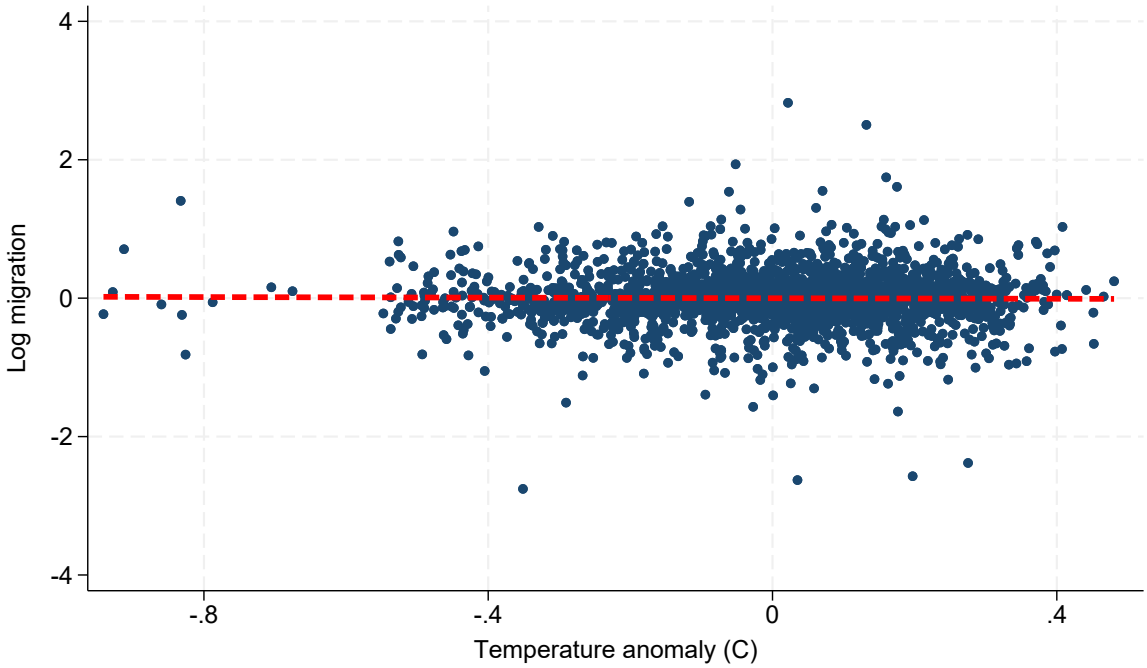
The results are in Figure A12, which shows a residualized scatterplot after controlling for district and period fixed effects. The results reveal a precisely estimated zero relationship between heat shocks and migration levels. This zero relationship may appear inconsistent with the existing literature that identifies migration as a common response to heat (Rexer and Sharma, 2024). However, recall that our regression is identified based on short-run shocks, while climate-induced migration is likely to be long run phenomenon. This perspective is consistent with other results throughout the paper that show limited adaptation in the short run. Overall, the results suggest that migration is unlikely to be the mechanism by which large, formal firms shed labor.

H.3 NREGA

Another labor supply explanation for our results is that the Indian state targets welfare programs to villages most affected by heat, increasing the opportunity cost of private sector work. One such program in India is NREGA, an employment guarantee workfare scheme. Garg et al. (2020) show that NREGA participation increases in heat-affected areas, potentially explaining our results. We address this potential confounder in two ways. First, we use

[29] Although presumably declining labor demand and wages would serve as a cause of climate-related migration.

Figure A12: Heat and migration



Note: Figure shows residualized scatterplot relationship between average temperature anomaly and migration at the district-period level. Sample is 640 migrant origin districts from the Census of India, observed over four 5-year periods in a pseudo panel from 1991-2011. Migration is defined as the log of the number of migrants who left the origin district for work over a given 5-year period. Temperature anomaly defined as the average difference between the annual max temperature and its long-run mean over the same period. Both variables are residualized of district and period fixed effects.

district-level data to replicate the results of Garg et al. (2020) on the relationship between our temperature anomalies and uptake of NREGA employment at the district level. Second, we estimate a version of our main specification in which we interact the temperature anomaly with a measure of NREGA uptake at the district level to show that firm size effects are not larger for areas with NREGA.

Table A17 estimates the impact of heat on NREGA uptake. To do so, we construct a district-level panel from 2006-2014. We then regress various NREGA take-up outcomes on a population-weighted district-level average of our maximum temperature anomaly, controlling for district and year fixed effects. The results confirm the findings of Garg et al. (2020), with evidence of meaningful increases in NREGA activity—particularly labor and material expenditure—in response to heat shocks.

Table A17: Heat shocks and NREGA uptake

Dependent variable	Log households	Log person-days	Log labor exp	Log material exp
	(1)	(2)	(3)	(4)
Max temp anomaly	0.115	0.040	0.101**	0.174**
	(0.099)	(0.041)	(0.046)	(0.087)
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	3511	3511	3511	3511
R^2	0.631	0.946	0.785	0.641

Note: Standard errors in parentheses clustered at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A18: Heat shocks and firm size: NREGA uptake

Dependent variable	Firm size (employment)				
	(1)	(2)	(3)	(4)	(5)
Max temp anomaly	-2.407	0.355	0.284	0.774	1.231
	(1.419)	(2.253)	(1.960)	(0.771)	(0.860)
NREGA 3 year average	-0.028	-0.014	-0.019	-0.110	0.024
	(0.043)	(0.016)	(0.014)	(0.057)	(0.053)
Max temp anomaly × NREGA 3 year average	0.162	-0.032	-0.032	-0.077	-0.071
	(0.101)	(0.156)	(0.134)	(0.056)	(0.062)
Observations	44665890	44641946	44665887	44665885	44641944
District FE	No	No	Yes	No	No
District × Year FE	No	No	No	Yes	Yes
SHRID FE	No	Yes	No	No	Yes

Note: Standard errors in parentheses clustered at the SHRID (village) level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In Table A18, we interact the district-level log of NREGA person-days with the temperature anomaly in our main regression specification. Since the data do not perfectly overlap with the Economic Census, we use only the latter two rounds and assign the NREGA value of 2006 for the 2005 round and the average of 2012-2014 for the 2013 round. The interaction between NREGA participation and the temperature anomaly is generally small and insignificant. There is no evidence that villages with higher NREGA enrollment observe larger firm size effects, suggesting that labor supply effect does not drive our results. An important caveat is the limited overlap between the Economic Census and the advent of NREGA, which began in 2006, severely limiting the sample for Table A18. That being said, the lack of NREGA in roughly half our sample should imply, prima facie, that this is unlikely to drive the results.

H.4 Demand-side effects

One concern is that output market demand drive the quantile effects of heat shocks, rather than labor demand effects. This would be the case if, for example, demand for the products made by large firms is differentially sensitive to heat. Caggese et al. (2024) find that demand effects only explain a small amount of the total firm-level effect of heat for Italian firms. Following the approach in Caggese et al. (2024), we test for demand effects by leveraging differences across sectors in tradeability, based on the logic that traded sectors are less sensitive to local demand shocks. Therefore, if demand effects are driving the contraction, rather than labor productivity effects, we should expect to see larger effects in the non-traded sectors. We test this hypothesis by interacting the heat anomaly with the log of sectoral export exposure. Export exposure is measured at the sector level as the share of total domestic sectoral output that is exported, averaged from 2000-2013, accounting for intermediate imported inputs. Data on sectoral exports and output comes from the regional ADB input-output tables is transformed into 3-digit NIC08 industry codes using available crosswalks.

Table A19: Heat shocks and firm size: heterogeneity by sectoral export exposure

Dependent variable	Firm size (employment)				
	(1)	(2)	(3)	(4)	(5)
Max temp anomaly	-0.079	-0.120	-0.133*	-0.018	-0.156
	(0.048)	(0.066)	(0.066)	(0.098)	(0.083)
Log export exposure	-0.134***	-0.137***	-0.135***	-0.137***	-0.138***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)
Max temp anomaly × Log export exposure	-0.020	-0.029	-0.023	-0.025	-0.029
	(0.024)	(0.026)	(0.027)	(0.024)	(0.024)
Observations	110537617	110523664	110537617	110537617	110523664
District FE	No	No	Yes	No	No
District × Year FE	No	No	No	Yes	Yes
SHRID FE	No	Yes	No	No	Yes

Note: Standard errors in parentheses clustered at the SHRID (village) level. Sample is all private sector firms surveyed in four Economic Census rounds (1990, 1998, 2005, and 2013). Anomalies are calculated as the difference between the average annual maximum temperature and the long-run average temperature from 1980-2021, averaged between t and $t-1$. Export exposure is calculated as the share of domestic production that is exported, defined at the sector-level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The results are in Table A19. The results show no evidence of differential firm size effects based on tradeability, with interaction effects being uniformly negative and insignificant across all specifications. This suggests that the employment effects operate through diminished labor productivity, consistent with Somanathan et al. (2021).