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Raided by the storm: How three decades of thunderstorms shaped U.S. incomes and wages

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ABSTRACT

Climate change and weather events are increasingly affecting the macroeconomic performance of countries and regions. However, their effects on income inequality are less understood. We estimate the dynamic impact of thunderstorms on income and wages and reveal a robust asymmetric effect. We leverage a comprehensive dataset covering more than 200,000 events affecting contiguous U.S. counties across three decades. Storms have caused the highest number of billion-dollar disaster events since the eighties, but they have the lowest average event cost. They are short-lived, locally confined, relatively frequent, difficult-to-predict, and hazardous albeit not fully destructive events. While such features are convenient for the identification of impacts, previous studies mostly focused on more extreme events. We document a robust negative association between storm activity, income and wages growth. While income tends to recover in the long run, wages exhibit a significantly more stubborn decline, suggesting persistent and adverse impacts on (functional) income inequality. A one standard deviation increase in wind exposure generates a loss of 0.15% (0.21%) in wages after three (nine) years; incomes fall by a larger extent initially (0.19% after three years) while fully recovering in the longer run. In addition to their notable asymmetry, such estimates are non-negligible—especially given the downward rigidity of U.S. wages. Our analyses also highlight a lack of effective adaptation and stronger negative impacts in economically disadvantaged regions. Finally, we find evidence for a sizable shock-absorbing role of federal assistance.

1. Introduction

The effects of climate and weather on economic activities have been obvious to mankind for thousands of years. In agricultural societies, where a dry season or a strong hailstorm could induce devastating losses, weather has always been a daily concern for peasants and farmers. Nowadays, with climate change reshaping the landscape of losses and potential damages, a thorough understanding of the impacts of weather events is essential for designing appropriate adaptation and mitigation policies. This is the goal of the rapidly growing "New Weather-Economy Literature" (Dell et al., 2014), which attempts to characterize the effects of temperature, precipitation and extreme weather events on economic outcomes.

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Much empirical research focuses on the analysis of temperature and precipitation anomalies (Burke et al., 2015; Palagi et al., 2022; Kotz et al., 2022) which, borrowing from statistical nomenclature, are often called "sufficient statistics". Part of their attractiveness lies in their simplicity, as well as their ease of use in projections, which allows researchers to effectively explore, e.g., alternative future climatic scenarios. However, climate change has been convincingly shown to entail more complex dynamics (Pörtner et al., 2022), as it causes changes along the state of the atmosphere, ocean and freshwater systems (Hsiang and Kopp, 2018), as well as changes in the frequency and strength of extreme events. Focusing on sufficient statistics may hide a substantial amount of heterogeneity, including physical specificities of distinct hazards (e.g., hurricanes, extreme temperatures, floods), different behavioral attitudes and anticipatory actions, and geographical and sectoral asymmetries.

Numerous studies (e.g., Cavallo et al., 2011) have thus focused on the economic repercussions of distinct hazards, considering time frames that extend beyond the immediate aftermath. There are several hypotheses in the literature about how output and other economic variables might respond to extreme events in the long-run. The *creative destruction* and *build back better* hypotheses postulate that natural hazards foster long-run economic growth by replacing damaged assets with newer and more efficient ones (Skidmore and Toya, 2002; Ahlerup, 2013; Hallegatte and Dumas, 2009). On the contrary, the *recovery to trend* hypothesis posits the lack of any permanent change to economic activity. Finally, the *no recovery* hypothesis conjectures a permanent negative impact on long-run economic growth (Hsiang and Jina, 2014; Anttila-Hughes and Hsiang, 2013). Overall, empirical studies have reached conflicting conclusions, depending on the hazard type, geographical scope, aggregation level and statistical methodology employed (Cavallo et al., 2013; Klomp and Valckx, 2014; Skidmore and Toya, 2002).

In this paper, we contribute to the debate by studying the long-run economic effects of thunderstorms in the U.S.. While more extreme phenomena have been thoroughly investigated (e.g., hurricanes and cyclones, Hsiang and Jina, 2014) the analysis of thunderstorms has received considerably less attention. However, according NOAA data, severe storms have caused the highest number of billion-dollar disaster events in the U.S. between 1980 and 2022 (163), though they have the lowest average event cost. Overall, storms cover up to 15% of total disaster costs in U.S. since the eighties. For comparison, droughts total up to 7% and wildfires to 5%.[1] Further, storms offer a valuable design. They are much more frequent and geographically dispersed than other natural disasters, which increases sample size and variability in the data; they are very difficult to predict and vanish rapidly, which eliminates anticipation effects and makes them resemble on-off shocks; they can interrupt electricity supply, impair traffic routes, uncover roofs, flood buildings and destroy cultivated fields, cars and trucks, but they do not level entire blocks or cities as hurricanes and tornadoes do. Relatedly, storms do not typically induce migratory phenomena of people and firms (Deryugina et al., 2018), which allows avoiding major confounding factors in the identification of the impacts. This, however, does not prevent such events from being highly damaging at the macroeconomic level. Indeed, severe storms account, every year, for 45% of all weather-related insured property losses in the United States (Kunkel et al., 1999).

In our analyses we employ National Oceanic and Atmospheric Administration (NOAA) data on more than *200,000 severe thunderstorms* that hit the continental United States between 1991 and 2019, which we integrate with *county-level economic data* from the Regional Economic Accounts (Bureau of Economic Analysis) and the Quarterly Census of Employment and Wages (U.S. Bureau of Labor Statistics), and *disaster declaration data* from the Federal Emergency Management Agency (FEMA). Through *distributed lag models* (Greene, 2003), traditionally employed in the analysis of exogenous meteorological events (Dell et al., 2012; Barrios et al., 2010; Hsiang and Jina, 2014; Callahan and Mankin, 2022), we identify the response of income and wages to storm exposure.

We find significant statistical effects of storm exposure on both income and wages. However, while the impact on income shrinks and eventually vanishes over time, in line with the *recovery to trend* hypothesis, the impact on wages appears to persist, in line with the *no recovery* hypothesis. Given the downward wage rigidity of U.S. wages, the wage loss induced by storms is non-negligible: counties hit by two storm events in a decade—ceteris paribus—experience a loss in real wages corresponding to about half of the wage contraction suffered in the aftermath of the 2007 financial crisis. The combination of recovering income and persistence of the wage loss suggests that severe storms may lead to an increase in functional income inequality. Such dynamics could be due to a hazard-induced accelerated depreciation of capital, which leads firms to invest in new, labor-saving technologies. This is supported by the evidence at the sectoral level of economic activity: while all industries show a negative and persistent statistical effect on wages, such effect is more marked in sectors characterized by a higher intensity of physical capital (good producing industries). Further, capital intensive sectors exhibit job losses that are not visible in services. Our results are robust to a battery of control exercises, and we find similar patterns repeating our analyses on a separate set of nearly 200,000 hailstorms occurred between 1991 and 2019.

Next, we analyze the ability of specific localities to mitigate the negative impacts of storms. We find evidence that poorer counties systematically display larger long-run impacts on income, consistent with a notion of local *adaptation deficit*, or *gap* (Fankhauser and McDermott, 2014). At the same time, counties historically more exposed to storms do not display significantly smaller losses—suggesting that, like for hurricanes (Bakkensen and Mendelsohn, 2016), repeated exposure to a natural hazard does not necessarily lead to some forms of successful adaptation. Finally, and importantly, we find evidence of a critical role for public interventions in containing the impacts of storms: our results show that areas which benefit from federal aid in the aftermath of a storm do not experience significant losses in income and wages.

To sum up, our results support two main conclusions. First, (relatively) non-extreme but rather common natural hazards such as thunderstorms can differentially impact economic sectors and income classes. While the negative statistical effects on income taper in the long run, those on wages persist, exacerbating the existing trend of rising income inequality (Piketty and Saez, 2014). Second,

[1] See https://www.ncei.noaa.gov/access/billions/.

we find that poorer areas show evidence of an adaptation deficit relative to richer ones and, remarkably, public interventions (in the form of federal aid) appear to effectively counteract negative economic impacts. As climate change could increase both the frequency and the magnitude of severe storms (Diffenbaugh et al., 2013), mitigation and, especially, adaptation policies need to be strengthened in order to enhance local resilience and reduce vulnerability to this type of hazards.

The remainder of the paper is organized as follows. Section 2 reviews the literature on storms, climate change and the impact of natural disasters on the economy. Section 3 describes data and methodology employed in our analyses. Section 4 details our results. Finally, Section 5 provides conclusions and remarks on future work.

2. What do we know about the long-run economic impacts of natural hazards?

Natural hazards can significantly affect physical and social infrastructure, property, environmental conditions, and living standards (Ibarrarán et al., 2009). Macroeconomic studies consistently show immediate declines in economic output, deteriorated trade balances, fiscal imbalances, increased poverty rates and heightened income inequality measures (Rasmussen, 2004). These findings are corroborated by micro-level studies on economic and socio-demographic indicators such as productivity, life expectancy, mortality and crime rates (Hsiang et al., 2017). However, signs and magnitude of aggregate economic effects in the medium and long-run are still openly debated (Noy et al., 2018).[2]

Overall, scholars have proposed four different—and by and large mutually exclusive—hypotheses on the long-term consequences of natural hazards, as summarized in Hsiang and Jina (2014).[3] First, according to the *creative destruction* hypothesis, the occurrence of hazards can temporarily stimulate economic growth by increasing demand for goods and services as communities replace lost capital, fostering the influx of aid and assistance, and triggering innovations (Skidmore and Toya, 2002; Ahlerup, 2013). However, only in exceptional cases (e.g., small nations receiving sizable international aid) economies are able to avoid a short-run decline in output. The *"build back better"* hypothesis suggests that natural disasters are immediately followed by a slowdown in economic growth due to loss of life and productive capital, as well as lengthy and onerous reconstruction processes, but that long-term economic growth is stimulated by the replacement of damaged assets with newer and more efficient units (Hallegatte and Dumas, 2009; Sawada et al., 2011; Akao and Sakamoto, 2018). Similarly, the *recovery to trend* hypothesis postulates that an economy hit by a hazard experiences a short-term contraction, but eventually returns to its pre-disaster growth trajectory. In contrast, the *no recovery* hypothesis suggests that the negative effects of a hazard on an economy persist beyond the initial contraction phase, preventing the return to the pre-hazard growth trajectory (as found for hurricanes in Hsiang and Jina, 2014). Various mechanisms have been proposed to explain such hysteresis dynamics (Cerra et al., 2023), including the diversion of resources from productive investment to meet urgent consumption needs (Anttila-Hughes and Hsiang, 2013).

Empirical analyses often produce conflicting findings—in support of one or the other hypothesis—which could be due to a variety of factors. First, results can vary with the statistical methods employed. Most of the initial studies used cross-sectional regressions, which can suffer from omitted variable bias (Hino and Burke, 2021). This concern has been mitigated through an increasing use of methods that exploit panel data (Dell et al., 2014; Burke et al., 2015), or at least repeated observations of the phenomenon under analysis (Hino and Burke, 2021; Bernstein et al., 2019). An additional challenge is posed by the simple fact that economies are constantly changing; pinpointing the effects of specific events requires appropriate "counterfactuals" to compare against observed data. This is typically pursued either through the introduction of composite fixed effects (Zivin et al., 2023), or by creating artificial control groups through, e.g., propensity score matching algorithms (Deryugina et al., 2018). Second, results can vary with the geographical scope of an analysis, because different countries and areas have distinct socio-economic structures, are at different development stages and are characterized by heterogeneous levels of exposure and resilience to natural hazards. Not surprisingly, most of the studies reporting evidence in favor of the *build back better* hypothesis focus on high-income countries (Crespo Cuaresma et al., 2008; Lackner, 2018), endowed with enough resources and technical know-how to pursue effective adaptation and efficient capital replacement. Third, results can vary with geographical resolution. Natural hazards are highly localized—even tough their effects can extend well beyond the affected area (Hallegatte, 2019). Broad, aggregate studies might therefore either fail to capture confined impacts, or confound them with spatial spillover effects—which may have very different signs and magnitudes across locations. Regional studies have indeed produced more clear-cut results (Xiao and Feser, 2014; Hornbeck, 2012; Vu and Noy, 2018).

Increased geographical resolution may also enrich and help disambuiguate analyses in terms of income levels. Growing evidence from aggregate studies based on sufficient statistics is pointing towards asymmetric consequences of climate anomalies, with poorer populations carrying a heavier burden from climate anomalies (Palagi et al., 2022). For what concern specific natural hazards, the micro-econometric literature has often concentrated on event studies (Elliott and Pais, 2006), particularly in highly-exposed developing countries (Carter et al., 2007; Mottaleb et al., 2013; Sakai et al., 2017).[4] Recent studies examining a larger set of events have provided additional evidence supporting a connection between natural hazards and income inequality, both through macro- (Cappelli et al., 2021) and micro-econometric approaches (Howell and Elliott, 2019). However, little is still known about long-run effects of specific natural hazards on income inequality at a sub-national level.

[2] Most of the empirical results which are currently contributing to the debate originate from the so-called New Weather-Economy Literature (Dell et al., 2014), to which this work also contribute.

[3] Focusing on hurricanes, Bakkensen and Barrage (2018) proposed a stochastic endogenous growth model that tries to reconcile these contradictory hypotheses within a unified framework.

[4] Sociological research has generated numerous studies demonstrating their disproportionate effects on vulnerable population segments (Baez and Santos, 2007; Klein, 2007).

By the same token, results can vary based on the "economic resolution" of the data, as impacts are very heterogeneous across sectors and economic activities, and are generally greater in agriculture (Xiao, 2011) and manufacturing (duPont IV and Noy, 2015). When examining labor market outcomes, research that emphasizes sufficient statistics indicates adverse effects on both wages and unemployment, primarily attributed to reduced labor productivity (Leduc and Wilson, 2023). Conversely, investigations centered on catastrophic events such as hurricanes tend to reveal positive impacts on labor compensation (Belasen and Polachek, 2009; Kirchberger, 2017; Zhu et al., 2021), although there are studies that report contrary findings (Mueller and Quisumbing, 2009). These positive effects are frequently attributed to mechanisms that characterize major events, and typically do not apply to thunderstorms, including: (i) significant migration (Groen and Polivka, 2008; McIntosh, 2008), leading to increased unemployment and reduced labor supply (McComb et al., 2011); and (ii) considerable disruptions, coupled with the influx of federal and international aid (Zhu et al., 2021), which bolsters specific sectors of the economy—e.g. construction (Belasen and Polachek, 2008).

Finally, a distinct body of literature delves into the pivotal role of international or federal assistance in facilitating recovery following hazardous events. Typically, these studies illustrate how aid helps mitigate adverse macroeconomic consequences. While the majority concentrate on country-level analysis, primarily examining aggregate income (Yang, 2008; Hochrainer, 2009), there are also studies focused on the United States that employ county-level analysis (Davlasheridze and Miao, 2021; Deryugina, 2017) or that specifically focus on wages (Zhu et al., 2021).

In this paper we contribute to the debate by focusing on a specific and understudied type of hazard: severe thunderstorms. While less extreme than other, more broadly studied hazards (e.g., floods, hurricanes, tornadoes) storms are more common and can be highly damaging. From a meteorological standpoint, they are low-pressure areas, even if the term storm is widely used also in a broader sense to indicate heavy winds or hailstorms.[5] Growing evidence suggests that climate change is likely to increase both the frequency and the strength of both thunderstorms (Diffenbaugh et al., 2013) and extra-tropical storms in the Northern Hemisphere (Vose et al., 2014). Here, we focus on the United States, where storms are a fundamental part of the nation's climate, producing between 15% (West Coast) and 70% (high plains) of the average precipitation across the nation. Storm-related damages are a fairly frequent occurrence nation-wide, accounting for 45% of all weather-related insured property losses (Kunkel et al., 1999).[6] These events are typically characterized by wind speeds (measured on the Beaufort wind force scale) ranging from 75 km/h to hurricane-like forces ($\geq$ 118 km/h, cf. Barua, 2005); the associated impacts can range from slight structural damages (severe gales) to devastation (hurricane-like wind forces).

Studying the U.S. allows us to consider a large geographical area, with data of reliable quality and reasonably high resolution (the counties). Thus, our analyses can leverage highly diversified information, both in terms of hazard exposure and in terms of economic activities. On such rich data, we employ an empirical strategy akin to that in Hsiang and Jina (2014) and, more recently, Callahan and Mankin (2022). Another point of strength of our study is the ability to consider different aggregate economic outputs, namely income and wages, both overall and by economic sector. This helps further elucidate the transmission channels through which hazards (storms in our case) affect the economy (Hsiang et al., 2017), and the asymmetric dynamics effects these events may induce.

3. Data and methods

In this Section, we describe in detail the data on storms and economic variables used in our analyses (Sections 3.1 and 3.2), the measures of hazard exposure we calculate from the storm data (Section 3.3), and our empirical strategy (Section 3.4).

3.1. Storm data

We employ data from the Storm Events Database (SED), which is maintained by the National Oceanic and Atmospheric Administration (NOAA), and informed by the National Weather Service (NWS). SED documents a variety of weather-related events capable of causing significant losses to property or life (see Table A.1). In our study, we only consider severe storm events involving damaging winds, i.e. those labeled as "thunderstorm wind".[7] Among these, we further restrict the analysis to those with wind speeds higher than 75 km/h (or 21 m/s)—the wind speed at which structural damage occurs[8]—taking place between 1991 and 2019, for a total of 307,289 events.[9] After extensive data cleaning and grouping of storm events, our final dataset consists of 204,319 distinct storm events—details are provided in Appendix A. As already mentioned above, a crucial feature of these events is that they extend well beyond the typical hurricane landfall basins (see Fig. 1A).

Severe storms are typically highly-localized, short-lived phenomena (Changnon, 1980; Fujita, 1985; Caracena et al., 1989). The average storm span in our dataset is just 6.91 km, and the 95th percentile just 33.61 km.[10] Even restricting attention to events with

[5] In Section 4.4 we also consider hailstorms.

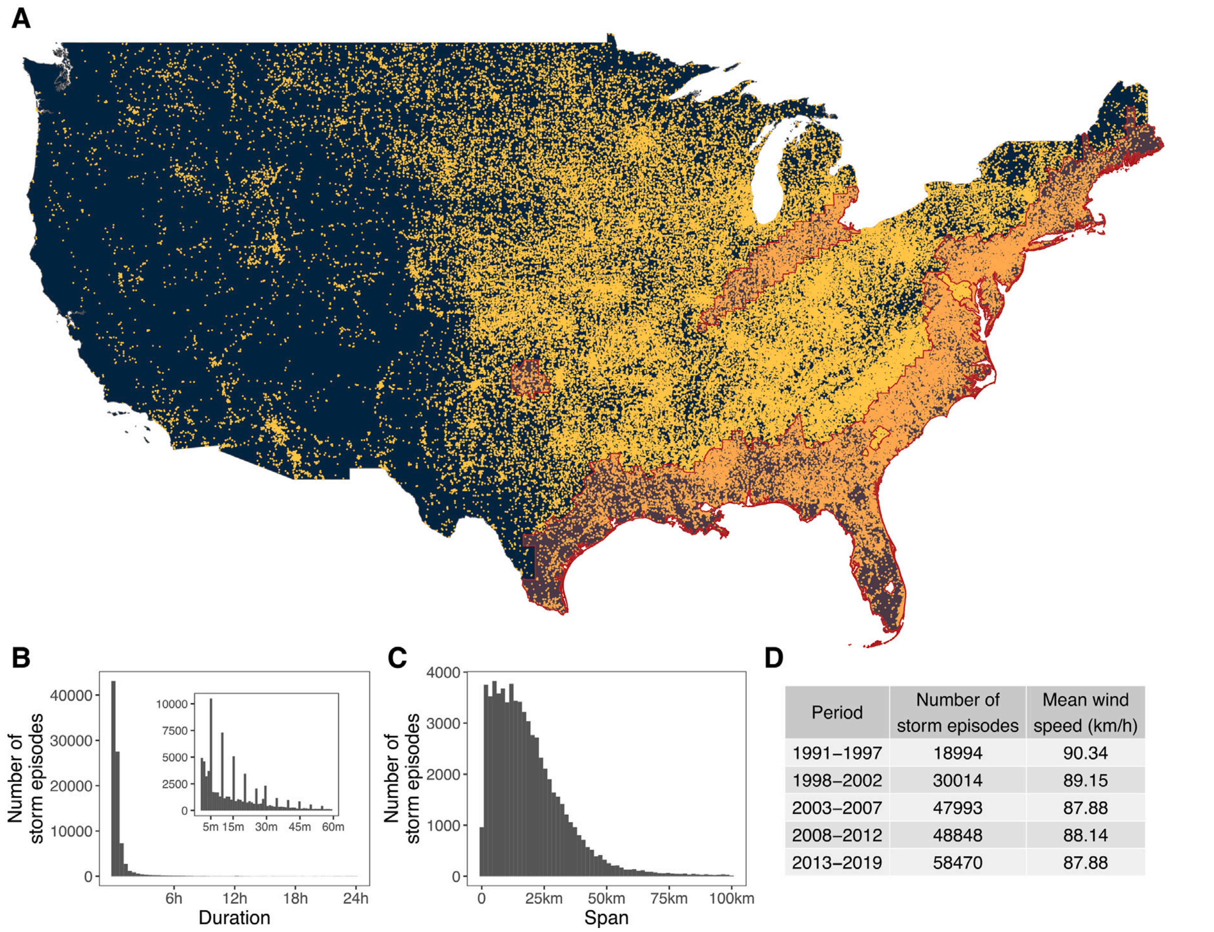
[6] Storms are typically better covered by insurance policies than other types of hazards (Jahn, 2015).

[7] SED also contains other types of events that involve damaging winds; namely, events labeled as "high wind" and "strong wind". We did not consider these in our study, as they may include non-convective events (Knox et al., 2011), and are collected by SED within a time span different from that used for thunderstorms. Nevertheless, including them does not sensibly alter our main results (estimates available upon request).

[8] See https://www.weather.gov/media/pqr/wind/wind.pdf.

[9] Excluding events with wind speed lower than the designed threshold does not alter our main findings, see Figure B.4.

[10] For approximately 97% of the events comprised in our final dataset SED provides start and end coordinates; the span, or radius, of these events is computed as the distance between start and end coordinates. Events were attributed to counties not by relying on coordinates but rather by utilizing the FIPS codes provided by SED, which SED itself indicates as the primary geolocation information—see Appendix A.



Period	Number of storm episodes	Mean wind speed (km/h)
1991–1997	18994	90.34
1998–2002	30014	89.15
2003–2007	47993	87.88
2008–2012	48848	88.14
2013–2019	58470	87.88

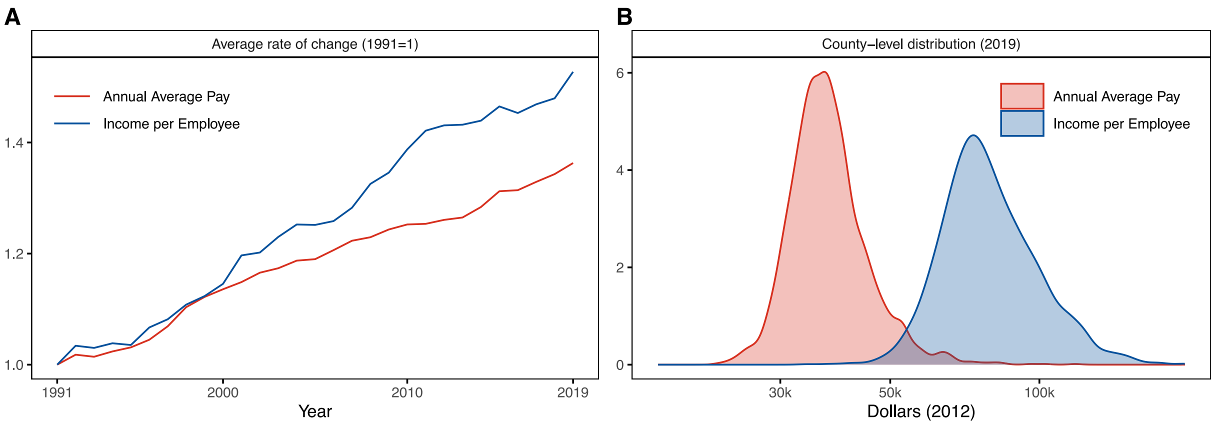
Fig. 1. Panel A: Yellow points mark the locations of the 204,319 thunderstorm events occurred between 1991 and 2019 within the continental U.S., with reported initial and final coordinates. In contrast to the broad geographic spread of these locations, the red shading indicates counties that suffered at least one Atlantic basin tropical storm (with sustained wind above 21 m/s) between 1991 and 2019—source: modeled winds (Willoughby et al., 2006) from hurricane best tracks data (Anderson et al., 2020). Panel B: Number of observed thunderstorm events by duration, measured as the difference between start and end time of storms (only events with strictly positive duration). The inset shows a detail of the left tail of the distribution. Panel C: Number of observed thunderstorm events by span, measured as the distance between the start and end location of storms (only events with strictly positive span). Panel D: Number of observed thunderstorm events and mean wind speed (in kilometers per hour) in various sub-periods of the 1991–2019 time interval covered by the study.

strictly positive span,[11] the distribution remains right skewed, with a mean of 20.33 km and a 95th percentile equal to 49.30 km (see Fig. 1B). In terms of duration, most events last less than one hour (see Fig. 1C), and almost all (99.1%) begin and end on the same day. Further, thunderstorms are very difficult to predict, even a few days ahead (Clark et al., 2009; Lawson, 2019), which reduces the chances of effectively anticipating their arrival. Notwithstanding their limited span and duration, these events can cause large damages to property and people, with effects ranging from large branches breaking off trees, to constructions and barricades blowing over, to flash flooding.

3.2. Economic data

We retrieve county-level data from the Quarterly Census of Employment and Wages (QCEW), maintained by the Bureau of Labor Statistics (BLS). QCEW publishes quarterly measurements covering more than 95% of U.S. jobs, disaggregated by the economic sectors comprised in the North American Industry Classification System (NAICS). In our main analyses, we employ Annual Average Pay (i.e. per capita annual wage) as our primary measure for wages. It includes the base wage but also bonuses, stock options, severance pay, the cash value of meals and lodging, tips and other gratuities. Employment, as captured by the Annual Average

[11] Approximately 63% of the events in our dataset are recorded as starting and ending in the same location, and thus have a radius of 0 km. Likewise, approximately 57% of observations report identical start and end date. These instances are likely to signal very short and geographically confined storm spells.



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Income source	Percentiles of wealth					Total
	< 25th	25th–50th	50th–75th	75th–90th	> 90th	
Wages	77.93%	80.1%	70.37%	63.79%	44.91%	60.91%
Interest or dividends	< 0.05%	0.05%	0.29%	1.5%	7.72%	3.45%
Business, farm, self–employment	3.24%	4.51%	7.74%	8.53%	24.43%	13.77%
Capital gains	< 0.05%	0.78%	0.37%	1.93%	12.01%	5.37%
Social Security or retirement	11.03%	12.14%	18.94%	17.89%	9.6%	13.35%
Transfers or other	7.76%	2.42%	2.28%	6.36%	1.33%	3.15%

Fig. 2. Panel A: Average rate of change of Annual Average Pay (wages) and Income per capita, 1991 = 1. Panel B: Distribution of Annual Average Pay and Income per capita at the county level, 2019. Panel C: relative percentages of before-tax family income, distributed by income sources, by percentile of net worth, in 2019 (source: Survey of Consumer Finances, Board of Governors of the Federal Reserve System). Panels A and B use variables computed with same denominator to ease comparison.

Employment measure, is used in our robustness checks. In the QCEW, wages are registered as reported by employers. As such, they are imputed to the county where the employer is located (Feyrer et al., 2017).[12]

Separately, we retrieve county-level data on personal income from the Regional Economic Account (REA), published by the Bureau of Economic Analysis (BEA). REA measures total personal income as the total of all the revenues arising from wages, proprietors' income, dividends, interest, rents, and government benefits. It is calculated as the sum of wages and salaries, supplements to wages and salaries, proprietors' income with inventory valuation and capital consumption adjustments, rental income of persons with capital consumption adjustment, personal dividend income, personal interest income, and personal current transfer receipts, less contributions for government social insurance plus the adjustment for residence. An individual's income is registered in the county where she lives, even if the income originated elsewhere.

Fig. 2 depicts the behavior of average income and wages in our sample. It provides a tale of increasing functional income inequality. In 2019 incomes are way larger than wages (panel B), as a result of a considerably more sustained process of growth spanning at least three decades (panel A). In an average county, wages account for about 60% of total incomes, with considerable differences along the income distribution (panel C). Our study attempts to assess whether weather events—particularly storms (which are relatively widespread and frequent)—contributed to exacerbating this macro-trend.

3.3. *Storm exposure measures*

To analyze the association between thunderstorms and economic variables observed at the county level, we produce a yearly, county level measurement of exposure aggregating storm events. Formally, the aggregation is performed through a generic metric function

$$M_{i,t} = \mu(\eta_{1,i,t}, \eta_{2,i,t}, \dots, \eta_{n_{i,t},i,t}) \tag{1}$$

[12] In QCEW, average annual figures per employee are computed by dividing total annual wages by annual average employment. In some states, QCEW wages also include states' employer contributions to certain deferred compensation plans, such as 401(k) plans. Further, note that individuals employed by state and local governments on a temporary basis following a declared emergency, e.g., due to a storm, fire, snow, earthquake, flood, etc., and individuals employed under a Federal relief program are excluded from QCEW data. For details, see https://www.bls.gov/cew/publications/employment-and-wages-annual-averages/2022/home.htm.

where $M_{i,t}$ is the exposure measure for county i in year t, $\mu(\cdot)$ is the metric function, and $\eta_{j,i,t}$, with $j = 1, \dots, n_{i,t}$ are the $n_{i,t}$ events affecting county i in year t. The literature on hazard impacts provides several choices for $\mu(\cdot)$, with different emphasis on event intensity and/or frequency. We consider four alternative functions and test the robustness of our results to the resulting exposure measures (see Figure B.5).

Nordhaus (2010) focused on frequency as the primary factor for the analysis of the economic impacts of hurricanes. The sheer number of events can indeed be a simple and effective way to capture activity also in the case of storms, resulting in the first exposure measure:

$$M^{(1)}_{i,t} = \mu(\eta_{1,i,t}, \eta_{2,i,t}, \dots, \eta_{n_{i,t},i,t}) = \#(\eta_{1,i,t}, \eta_{2,i,t}, \dots, \eta_{n_{i,t},i,t}) = n_{i,t} \quad , \tag{2}$$

where $\#(\cdot)$ is the counting function that enumerates the elements of a set.[13] However, this approach disregards information about wind speed, which can be captured through metrics derived from climate physics. This is what we do with the second exposure measure; namely, the maximum wind speed experienced throughout all storms in a given year (as in Hsiang and Narita, 2012):

$$M^{(2)}_{i,t} = \mu(\eta_{1,i,t}, \eta_{2,i,t}, \dots, \eta_{n_{i,t},i,t}) = \max_{j=1,\dots,n_{i,j}} s_{j,i,t} \quad , \tag{3}$$

where $s_{j,i,t}$ is the wind speed recorded during event $\eta_{j,i,t}$. Measuring exposure through maximum wind speed is appealing, as most rigid materials used to construct durable capital fail above a critical level of stress. On the other hand, this approach does not consider the cumulative effect which may derive from the occurrence of multiple storms in the same time period. In order to account for both magnitude and frequency, metrics are often based on empirically derived damage functions which relate a physical stressor (e.g., wind speed) to experienced damages. Damage functions are highly heterogeneous, as they are typically scale and location-dependent and can change over time (due, e.g., to adaptation). As such, they cannot be directly inferred from a few physic principles, although they are often concave and upwardly curved. Hence, the general approach is to model exposure as either the square (Pielke and Landsea, 1999) or the cube (Emanuel, 2005) of wind speed, although higher powers have also been suggested (Münchener, 2002; Nordhaus, 2010). Since damages accumulate, aggregation can be achieved through summation (Henry et al., 2020). This leads to the third and fourth exposure measures, using squares and cubes, respectively:

$$M^{(3)}_{i,t} = \mu(\eta_{1,i,t}, \eta_{2,i,t}, \dots, \eta_{n_{i,t},i,t}) = \sum_{j=1}^{n_{i,t}} s^3_{j,i,t} \quad , \tag{4}$$

$$M^{(4)}_{i,t} = \mu(\eta_{1,i,t}, \eta_{2,i,t}, \dots, \eta_{n_{i,t},i,t}) = \sum_{j=1}^{n_{i,t}} s^2_{j,i,t} \quad . \tag{5}$$

Another important consideration is that, while storms in our dataset have relatively homogeneous sizes, county sizes range from 62 km^2 (Bristol County, Rhode Island) to 51,947 km^2 (San Bernardino County, California). Since exposure could simply increase with county size, in line with the literature (Hsiang, 2016) we normalize the measures in Eqs. (2), (3), (4) and (5) as

$$S^{(k)}_{i,t} = \frac{\bar{a}}{a_i} M^{(k)}_{i,t} \quad , \quad k = 1, 2, 3, 4 \tag{6}$$

where a_i is the area of county i, and $\bar{a}$ the average county area. Because of this normalization, estimated impacts (Section 4) should be interpreted as impacts on a county of average size.[14]

Using the cumulative, square-based measure $S^{(4)}$, Fig. 3A shows the percentage of years in which each county registered an exposure in excess of one (pooled) standard deviation above the (pooled) mean.[15] This demonstrates again the much broader geographical spread of the storms considered here relative to that of typical hurricanes and tropical storms. The vast majority of storm over-exposure occurs east of the Rocky Mountains (though storms extend to other parts of the country; see Fig. 1 A). In addition, the distribution of exposure measures (pooled over counties and years; Fig. 3B) presents a large spread and a strong skew—suggesting a marked heterogeneity over time and space. Fig. 3C reports correlations for the four exposure measures considered, which range between 0.7 and 0.98—suggesting that all measures capture the same salient aspects of storm exposure.[16]

3.4. Empirical strategy

We model our panel data, which comprises $i = 1, \dots, 2,408$ counties and $t = 1, \dots, 29$ years, with the general equation

$$y_{i,t} = \sum_{\ell=0}^{p} \beta_\ell S_{i,t-\ell} + \gamma X_{i,t} + \alpha_i + \omega_{i,t} + \epsilon_{i,t}$$

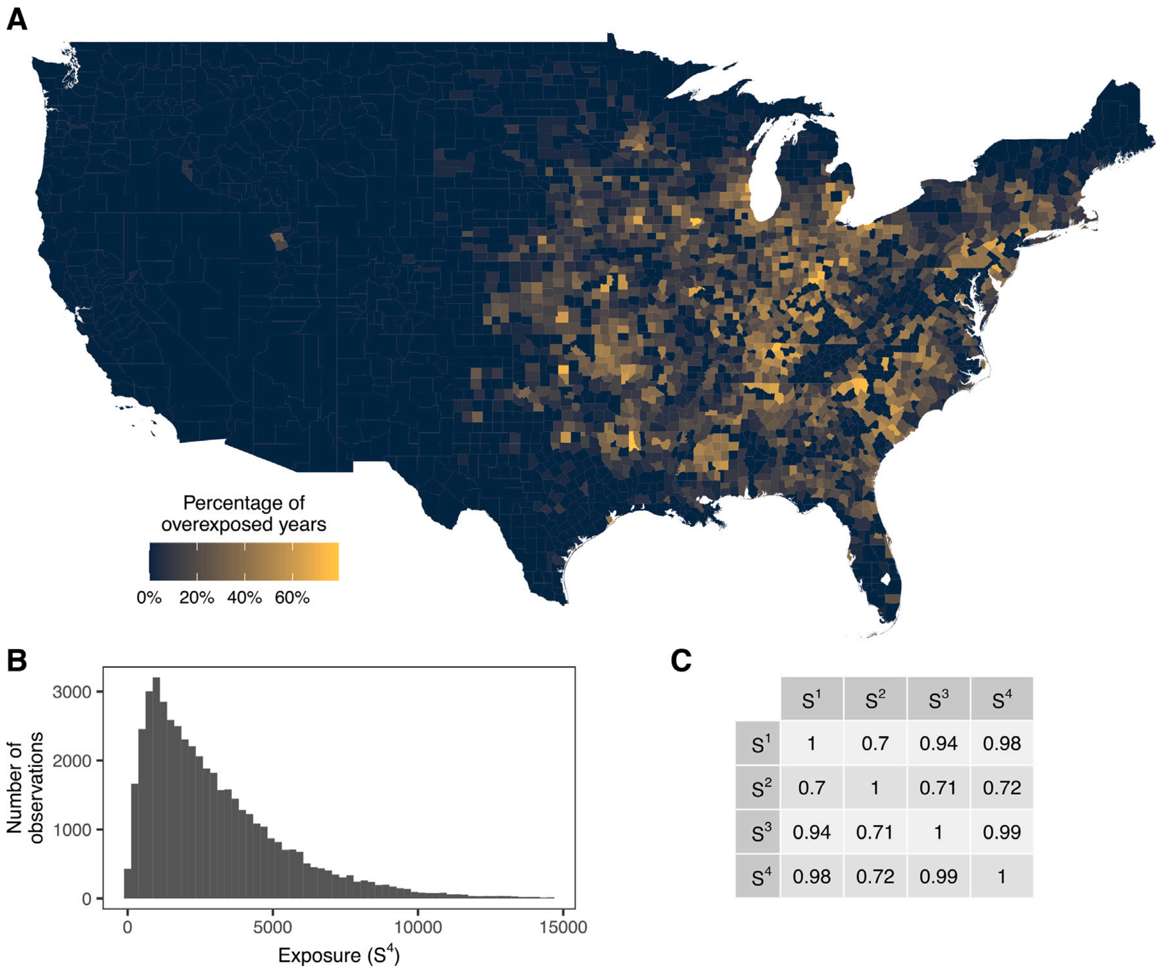
where $y_{i,t}$ is the annual growth rate of our dependent variable (i.e., annual average pay or, separately, income per capita), $S_{i,t}$ is an exposure measure (as defined in Eq. (6)), which enters the model with p lags ($\ell = 0, \dots, p$), and $X_{i,t}$ is a set of control variables. α_i is a county-level fixed effect introduced to control for observed and unobserved characteristics, such as differences in climate

13 Even simpler metrics can be employed, as in Deryugina (2017), which only considers a dichotomous variable indicating whether a county has been affected by at least one event in that year.

14 In the case of very small counties, very large scaling factors generate outliers. We trimmed our data to mitigate their effect. Additional details are provided in Appendix A.

15 The square of a wind speed is often used as it proxies the energy carried by the storm, see Nordhaus (2010).

16 Similar correlations can be observed among non-normalized exposure measures, i.e. among $M^{(1)}$, $M^{(2)}$, $M^{(3)}$ and $M^{(4)}$, see Table A.2.



	S^1	S^2	S^3	S^4
S^1	1	0.7	0.94	0.98
S^2	0.7	1	0.71	0.72
S^3	0.94	0.71	1	0.99
S^4	0.98	0.72	0.99	1

Fig. 3. Panel A: Percentage of years (between 1991 and 2019) in which each county experienced an exposure ($S^{(4)}$) in excess of one pooled standard deviation above the pooled mean. Panel B: Distribution of exposures ($S^{(4)}$) for all counties and years (1991–2019). Panel C: Correlations for normalized exposure measures ($S^{(1)}$, $S^{(2)}$, $S^{(3)}$ and $S^{(4)}$; see Eqs. (2), (3), (4), (5), and (6)).

and in long-run trajectories of economic development across counties.[17] $\omega_{i,t}$ is a term that, in various specifications of our model, comprises alternative formulations of composite time fixed effects. In its most comprehensive formulation, $\omega_{i,t}$ represents time-varying individual effects entering the model as heterogeneous time trends (Bai, 2009), generated by multiple common time-varying factors. For instance, when estimating cyclone impact on country-level growth, Hsiang and Jina (2014) consider year-fixed effects and country-specific trends, thus controlling for country-specific changes in economic policies and curvatures of income growth trajectories, as well as trends in climate variables. In our data, we do not find evidence of time trends in the county-level growth rates of the dependent variables (cf. Figure B.1); most common statistical tests exclude the existence of trends for an overwhelming majority of counties. Nevertheless, we include *state*-specific linear time trends in our baseline specification in order to flexibly capture macroeconomic dynamics and, in a conservative spirit, any possible true or artifactual trends in climate variables.[18] Our baseline model is thus

$$y_{i,t} = \sum_{\ell=0}^{p} \beta_\ell S_{i,t-\ell} + \gamma X_{i,t} + \alpha_i + \phi_t + \lambda_{\mathcal{S}_i} t + \epsilon_{i,t} \tag{7}$$

where $\lambda_{\mathcal{S}_i} t$ represents a state-specific linear time trend ($\mathcal{S}_i$ is the state to which county i belongs).

Considering $S_{i,t}$ with p lags allows us to capture its effects dynamically; for an exposure occurring in year t_0, we track the response of the dependent variable throughout the period from t_0 to $t_0 + p$. Since we are interested in examining dynamic responses to storm exposure over a long time span, we typically set $p = 10$. This is as large as we can afford while still guaranteeing reliable

17 Since our dependent variable is expressed in growth rates, α_i captures the time-trends along which each county moves over time.

18 As shown in Table B.5 results are robust to the exclusion of state-specific trends, as well as to the inclusion of more restrictive time fixed effects.

estimates; the panel comprises 29 years of annual exposure measures, so $p = 10$ already excludes over one-third of the observations. Information criteria and Wald tests have been used to justify our lag selection (see Appendix B Section "Lag selection"). Notably, though, results are robust to different choices of p (cf. Table B.6). Following Hsiang and Jina (2014) and, more recently, Callahan and Mankin (2022), for each intermediate duration $\tau \leq p$ we can express the cumulative effect of storm exposure as

$$\hat{C}_\tau = \sum_{\ell=0}^{\tau} \hat{\beta}_\ell \quad . \tag{8}$$

While our models estimate the dependent variables as growth rates, the results presented in Figures and Tables throughout the manuscript are consistently expressed as cumulative effects ($\hat{C}_\tau$) standardized relative to a pooled standard deviation of exposure, along with corresponding confidence intervals. Therefore, each specific value of $\hat{C}_\tau$ should be interpreted as the percentage difference in the dependent variable (annual average pay or income per capita) in levels, up to τ years after exposure, holding all else constant.

Our panel methodology exploits both cross-sectional and temporal variations in storm exposure measures. We note that the timing, location and intensity of storms "shocks" are unpredictable and stochastic across years, conditional on each county's typical climate. Our exposure measures may be seen as locally exogenous variables to the dynamics of U.S. incomes and wages. Further, the timespan of our sample and its geographical coverage make any influence of the economy on the occurrence and strength of weather events implausible.[19] As a consequence, in tune with other studies employing analogous measures (e.g. Hsiang and Jina, 2014), the reported values of $\hat{C}_\tau$ may reflect a causal effect of the weather. However, we notice that our model does not estimate the effect of being hit by a single storm; rather, it identifies the local (county-level) dynamic impact of suffering, in a given year, an exposure to thunderstorm winds that is one standard deviation higher than the historical average in that location, all else being equal. In the Appendix, we restrict our attention to empirical settings that are closer to event-studies to check the robustness of our results (see also Section 4.2, Figures B.8 B.9 and Table B.8). To account for spatial clustering of the variables used in our empirical setting (see e.g. Fig. 1), all uncertainty measures related to our estimates are based on spatially robust Conley standard errors (Conley, 1999) allowing for arbitrary serial correlation (Colella et al., 2023) - see also Section 4.2 and Figures B.10, B.11, B.12).

In Section 4.3, we model local spatial spillovers in order to disentangle direct and indirect (to neighboring counties) effects. We do so by estimating a Spatial Lag of X (SLX) model (Halleck Vega and Elhorst, 2015) which explicitly includes p lags of the weighted mean of the exposures of neighboring counties (S^N) as covariates, with the weighting scheme being determined by the spatial weighting matrix $\mathcal{W}$:

$$y_{i,t} = \sum_{\ell=0}^{p} \beta_\ell S_{i,t-\ell} + \sum_{\ell=0}^{p} \rho_\ell \mathcal{W}_i S^N_{i,t-\ell} + \gamma X_{i,t} + \alpha_i + \phi_t + \lambda_{S_i} t + \epsilon_{i,t} \tag{9}$$

Finally, we consider an alternative specification of the model in which exposure, at all lags, interacts with a categorical variable that labels counties ($\mathcal{D}_i$), or counties and years in the most general formulation ($\mathcal{D}_{i,t}$), as belonging to different groups:

$$y_{i,t} = \sum_{\ell=0}^{p} \beta_{\ell,\mathcal{D}_i} (S_{i,t-\ell} \times \mathcal{D}_{i,t-\ell}) + \gamma X_{i,t} + \alpha_i + \phi_t + \lambda_{S_i} t + \epsilon_{i,t} \quad . \tag{10}$$

We utilize Model (10) with county labels to differentiate impacts based on income and historical exposure levels (Section 4.5), and with county-and-year labels to differentiate impacts based on whether FEMA disaster declarations were issued (Section 4.6).

4. Results

In this Section, we report our main results on the impact of thunderstorms on wages and income per capita at the county level (Section 4.1), followed by a battery of additional analyses which confirm their robustness (Section 4.2)—including a parallel analysis of the economic impacts of hailstorms (Section 4.4). Finally, we report results that offer important insights on the ability of different counties to adapt to storm exposure (Section 4.5), and on the role of public relief policies (Section 4.6).

4.1. The impact of storm exposure on income and wages

Our findings reveal a significant negative association between storm exposure and both wages and income per capita, although with notable differences. Estimates arising from our baseline model (Eq. (7)) employing our preferred measure of exposure $S^{(4)}$ (Eqs. (5) and (6)) are summarized in Fig. 4 and Table 1. Following a storm exposure of one pooled standard deviation beyond the historical average, wages exhibit a steady and nearly monotonic decline over time (Fig. 4). More specifically, wages are estimated to undergo a 0.14% reduction below pre-exposure levels in the medium-run (after 3 years), eventually stabilizing at a plateau of 0.21% below pre-exposure levels in the long term (after 10 years). The estimated negative impacts exhibit high statistical significance in all years, except the one in which the exposure occurs ($\hat{C}_0$; Table 1). Remarkably, the estimated dynamic effect of storm exposure on per capita income follows a different pattern. The same-year impact on income is statistically significant and substantially larger than that on wages (a decrease of 0.1% compared to 0.027%). This is likely explained by wage stickiness; revising labor contracts—whether upward or downward—is typically a time-consuming process, while damages to capital stock (e.g., buildings

[19] Tables B.10 and B.11 in Appendix B provide additional robustness checks testing the lack of predictive power of past exposure on present exposure, conditional on local climatic conditions; in addition, we provide evidence that storm exposure is not related to key socio-economic confounders that can influence wages or income, see Table B.12.

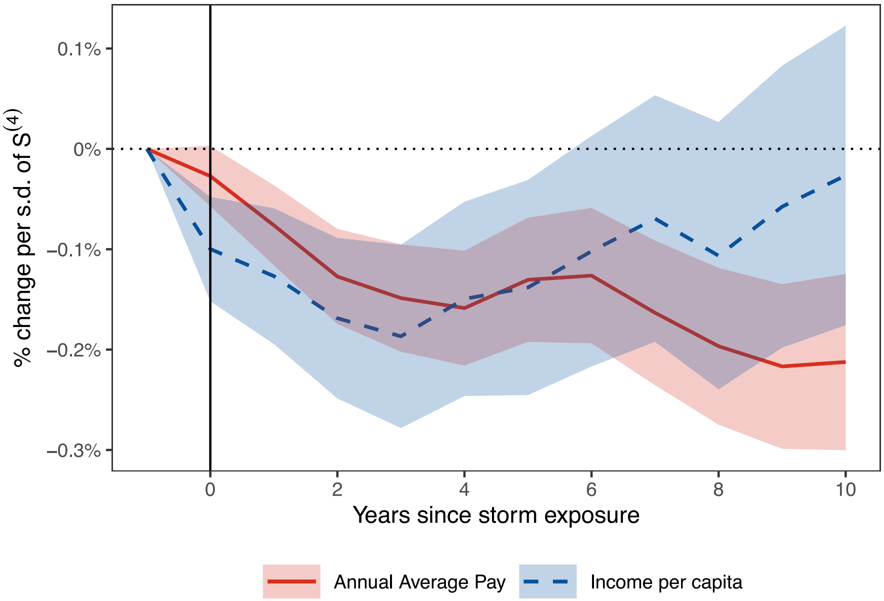


Fig. 4. Cumulative effects of severe thunderstorms ($\hat{C}_\tau$) for one pooled standard deviation of the exposure measure, on Annual Average Pay (wages; red) and Income per capita (blue), as estimated by Model (7). The exposure measure is $S^{(4)}$ (Eqs. (5) and (6)). The number of lags considered is $p = 10$. The climatic controls included are population-weighted county-level yearly total precipitations and average temperatures. The shaded areas represent spatial and serial correlation robust Conley 95% confidence intervals (spatial cutoff 50 km, temporal cutoff 1 lag, Colella et al., 2023). The horizontal black dotted line at 0 represents the baseline trend for each county (a 0 effect indicates that a county follows its baseline trajectory after storm exposure). Full details on estimates can be found in Table 1.

and infrastructure) are immediately reflected in reduced incomes. In the short term, income follows a declining trajectory until reaching its minimum (−0.18% after 3 years). Subsequently, it gradually recovers, eventually approaching pre-exposure levels in the long run; the estimated impact after 10 years is small (−0.026%) and no longer statistically significant. Thus, the dynamic behavior of wages is consistent with the *no recovery* hypothesis, while that of income aligns with the *return to trend* hypothesis.[20] Since income comprises both labor and capital components (e.g., rents, profits, etc.; see also Fig. 2, panel C), this divergent behavior points towards a concurrent deterioration of functional income inequality. Though our estimates are not as large as those associated with more destructive events (e.g. Hsiang and Jina, 2014), the frequency of storms and the trend in their occurrence suggest a non-negligible economic effect, especially considering the downward U.S. wage rigidity. More specifically, in the 1136 counties (corresponding to 47% of the sample) in which we register at least 2 events corresponding to a one-standard-deviation variation in exposure (or more) in a 10 years period, the loss in real wages—ceteris paribus—is about half of the wage contraction suffered in the aftermath of the 2007 financial crisis.[21]

The findings described above do not depend on the choice of exposure measure $S^{(4)}$. Using the other measures introduced in Section 3.3; namely, $S^{(1)}$, $S^{(2)}$ and $S^{(3)}$ (Eqs. (2), (3), (4) and (6), respectively), yields very similar results for both wages and income (Figure B.5). More specifically, estimated impacts are slightly milder with $S^{(2)}$, almost identical with $S^{(3)}$, and slightly stronger with $S^{(1)}$. This suggests that the observed dynamics are likely triggered by a combination of hazard severity (primarily captured by $S^{(2)}$) and hazard frequency (primarily captured by $S^{(1)}$).

To gain a deeper understanding, we repeat the analysis disaggregating wages by economic sector. We fit Model (7) separately for the primary sector (agriculture, forestry, fishing, hunting and mining), the service sector, and the goods-producing sector further partitioned into Construction and Manufacturing—which are characterized by distinct dynamics following a climatic shock (Belasen and Polachek, 2008; Hsiang, 2010).[22] As shown in Fig. 5, Services, Construction and Manufacturing exhibit a wage dynamics similar to that observed at the aggregate level, with an initial decline followed by a stabilization below pre-exposure levels. The primary sector differs from the others; estimated effects do not reach statistical significance in both the short and the long run, but the estimated contemporaneous effect is significant and notably larger than in other sectors. This aligns with the notion that the agricultural sector may experience more immediate and pronounced impacts from weather shocks (Xiao, 2011). Additionally, the primary sector—and in particular agriculture—is typically characterized by seasonal work contracts; this reduces wage stickiness and

[20] Notice that an almost identical dynamics of the response of wages is obtained using BEA rather than QCEW data on wages; see Figure B.3.

[21] According to FRED data, the real median weekly wage of full time workers in the five years following the 2007 financial crisis moved from 335 USD (Q1 2008) to 331 (Q1 2013), totaling a 1.2% reduction before restarting positive year-on-year growth.

[22] While sectoral data is available at finer digits resolution in the QCEW dataset, we focus on this coarse partition because capturing the effects of highly volatile county-level sub-sectoral dynamics would require more targeted studies and is beyond the scope of this study.

Table 1
Cumulative effects of severe thunderstorms ($\hat{C}_\tau$), in percentage, for one pooled standard deviation of the exposure measure, on Annual Average Pay (wages) and Income per capita, as estimated by Model (7). The exposure measure is $S^{(4)}$ (Eqs. (5) and (6)). The number of lags considered is $p = 10$. The controls included in the model are population-weighted county-level yearly total precipitations and average temperatures. Spatial and serial correlation robust Conley standard errors (spatial cutoff 50 km, temporal cutoff 1 lag, Colella et al., 2023) are reported between parentheses. See also Fig. 4.

Years since storm exposure	Annual average pay Cumulative effect $\hat{C}_\tau$	Income per capita Cumulative effect $\hat{C}_\tau$
0	−0.027*(0.015)	−0.100**** (0.027)
1	−0.076**** (0.020)	−0.127**** (0.035)
2	−0.127**** (0.024)	−0.169**** (0.041)
3	−0.149**** (0.027)	−0.187**** (0.047)
4	−0.159**** (0.029)	−0.149*** (0.049)
5	−0.130**** (0.032)	−0.138** (0.055)
6	−0.126**** (0.034)	−0.102* (0.059)
7	−0.163**** (0.037)	−0.070 (0.063)
8	−0.197**** (0.040)	−0.106 (0.068)
9	−0.217**** (0.042)	−0.058 (0.072)
10	−0.212**** (0.045)	−0.026 (0.076)
County fixed effects	✓	✓
Year fixed effects	✓	✓
State-level trends	✓	✓
Climatic controls	✓	✓
Adjusted R^2	0.045	0.089
Observations	45752	45752
Wald (χ^2) test $\lambda_{S_i} = 0$ (p-value)	< 0.001	< 0.001

Note:
**** $p < 0.001$.
*** $p < 0.01$.
** $p < 0.05$.
* $p < 0.1$.

may contribute to its ability to quickly respond to exogenous shocks. The goods-producing sector exhibits larger negative impacts than the service sector. This is especially true for Manufacturing, in line with previous results (duPont IV and Noy, 2015), while Construction exhibits a medium-term temporary rebound, potentially influenced by public aid flows and reconstruction efforts. In all, our findings are consistent with the notion that industries in which physical capital plays a larger role experience more pronounced disruptions due to storms.

While the short-term effects can be attributed to a storm-induced decline in productivity, we interpret our findings on the long-run impacts of storm exposure as the outcome of firms replacing damaged physical capital before its natural obsolescence—i.e., hazard induced depreciation (Hsiang and Jina, 2015). This process of capital substitution is not technology neutral, as it often entails the adoption of labor-saving technologies, which can lead to a decrease in labor demand and to a permanent reduction in wages. At the same time, capital replacement drives income towards its pre-exposure levels, potentially resulting in a net increase in income inequality. This conjecture is corroborated by the fact that the effect is stronger in higher capital intensive sectors. To further investigate this mechanism we also tested the effect of storms on employment dynamics across different sectors. Results show significant job losses in constructions, agriculture and manufacturing (albeit at 10% significance level in the latter case). Differently, employment in services are unaffected. This reinforce our interpretation of our results as driven by hazard-induced depreciation and capital substitution towards labor-saving technologies. The impacts of severe storms appear then to exacerbate pre-existing trends of increasing income inequality (see, e.g., Piketty and Saez, 2014, among a large literature), as wages exhibit a lower average rate of growth with respect to income—together with a lower variance, see Figure B.1.

Finally, we find that storm exposure has a negligible impact on housing prices (see Figure C.1). If anything, housing values tend to increase slightly about five years after the event. This pattern aligns with the idea that storms necessitate maintenance or accelerate the replacement of elements like roofs and windows, which may ultimately boost the selling price. Such an evidence supports the view that storms exacerbate economic disparities by increasing the number of salary-months required to afford a home.

4.2. Robustness

We challenge the results in Section 4.1 with a series of robustness checks involving additional control variables, the specification of fixed-effects included in the model, the number of lags of the exposure variable, alternative time periods, as well as a "placebo" analysis based on different types of randomization, and a county sub-sampling exercise. Overall, our findings remain robust.

Control variables. Estimated impacts of storm exposure vary negligibly when additional control variables (i.e. other terms in $X_{i,t}$ of Eq. (7)) are included alongside population-weighted annual total precipitations and average temperatures (Tables B.3 and B.4). Specifically, we added squares of the precipitation and temperature controls, to capture potential non-linearities in climate impacts (Burke et al., 2015; Palagi et al., 2022); county-level growth rates for employment and population, to account for labor

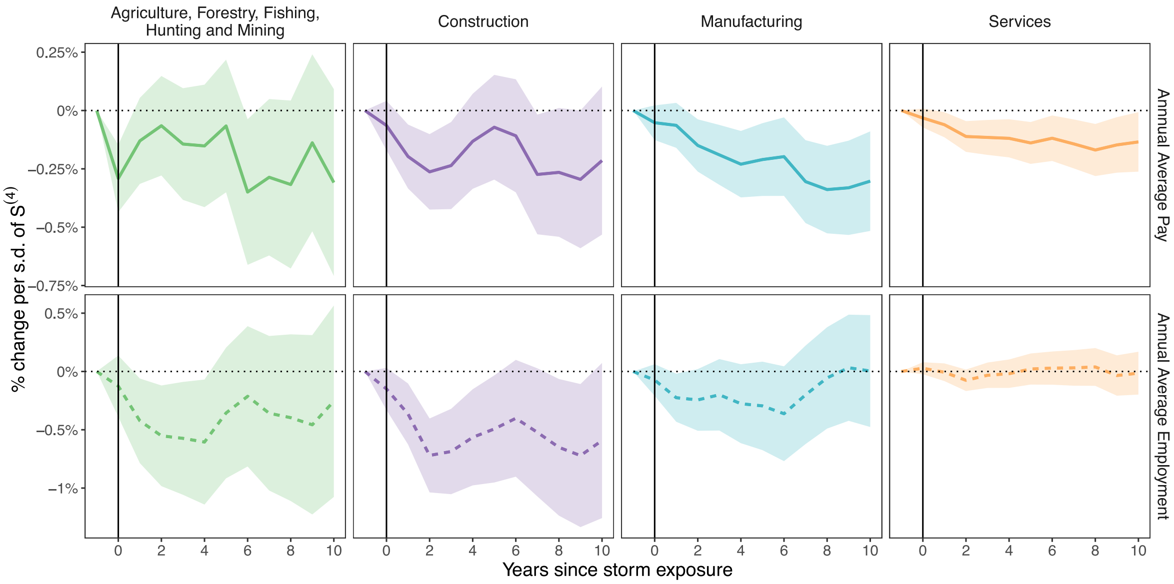


Fig. 5. Cumulative effects of severe thunderstorms ($\hat{C}_\tau$) for one pooled standard deviation of the exposure measure, on Annual Average Pay (wages) and Annual Average Employment disaggregated by economic sectors, as estimated by Model (7). The exposure measure is $S^{(4)}$ (Eqs. (5) and (6)). The number of lags considered is $p = 10$. The climatic controls included are population-weighted county-level yearly total precipitations and average temperatures. The shaded areas represent spatial and serial correlation robust Conley 95% confidence intervals (spatial cutoff 50 km, temporal cutoff 1 lag, Colella et al., 2023). In each panel, the horizontal black dotted line at 0 represents the baseline trend for each county (a 0 effect indicates that a county follows its baseline trajectory after storm exposure).

market dynamics; as well as lagged state-level active population (i.e. employed workforce over total population). Furthermore, even when introducing the state-level lagged growth rate of the dependent variable (Annual Average Pay or Income per Capita) as an extra control, results remain very similar to those obtained with the baseline model. This suggests that the consequences of storm exposure primarily stem from highly localized (county-level) effects.

Fixed effects. The use of panel methodology is a crucial component of our empirical strategy. As discussed in Section 3.4, our baseline model (Eq. (7)) includes county-level fixed effects, year fixed effects, and state-level trends. Concerning the latter, we run Wald and F tests to assess the null hypothesis of state-level trends being inconsequential to our regression analysis ($\lambda = 0$). As shown in Table 1, both tests strongly reject the null for both wages and income regressions. However, removing state-level trends from our model does not substantially alter impact estimates for the wage regression, and only modestly modifies impact estimates for the income regression, resulting in a positive long-term effect in line with the *build back better* hypothesis (Table B.5). Finally, our results remain qualitatively unchanged even using a highly restrictive model incorporating state-year fixed effects, although with such a model the estimates for the income regression are, understandably, less significant (Table B.5).

Time lags. Also altering the number of lags with which the exposure variable is included in our model (p in Eq. (7)) does not fundamentally change our key findings (Table B.6). While increasing the number of lags is often recommended to mitigate the risk of omitted variable bias (Greene, 2003; Hsiang and Jina, 2014), it also introduces greater statistical uncertainty and leads to a larger number of dropped observations, thereby amplifying statistical noise. Impact estimates for the wage regression are largely unchanged when using $p = 8$ or $p = 12$. On the other hand, while remaining consistent with the *return to trend* hypothesis, impact estimates for the income regression exhibit a gradual reduction in size and statistical significance as the number of lags increases.

Temporal window. In a conservative spirit, we conducted additional checks by restricting our model fits to different temporal windows within the 1991–2019 interval. While this reduces the number of observations and diminishes statistical power, it allows us to assess whether our results are driven by dynamics specific to a particular time period. Table B.7 shows results obtained across four different sub-samples (those outlined in Fig. 1C). Specifically, we consider two panels beginning in 1998 and in 2003, respectively (to remove the first years of the sample containing fewer events); one dataset ending in 2008 (to remove the years of the global financial crisis and its aftermath); and one dataset ending in 2012 (to exclude most recent years in our sample). Despite the decrease in statistical significance due to the substantially smaller sample sizes, the results remain consistent with our baseline estimates. Additionally, we notice that the different responses of wages and income are starker in samples ending in 2008 and 2012, suggesting that more recent years have witnessed a lower asymmetry.

Placebo analysis. To further investigate potential misspecifications in our baseline model and validate our estimates, we conduct a "placebo" analysis along the lines of Hsiang and Jina (2014). Specifically, we re-fit our baseline specification (Eq. (7)) after different types of data randomizations. This approach allows us to gauge whether our findings could arise from spurious relationships or biases due to the specification itself. In the first exercise we randomize storm exposures while keeping the dependent variables (wages and income growth rates) fixed. Conversely, in the second exercise we keep storm exposures fixed and randomize the

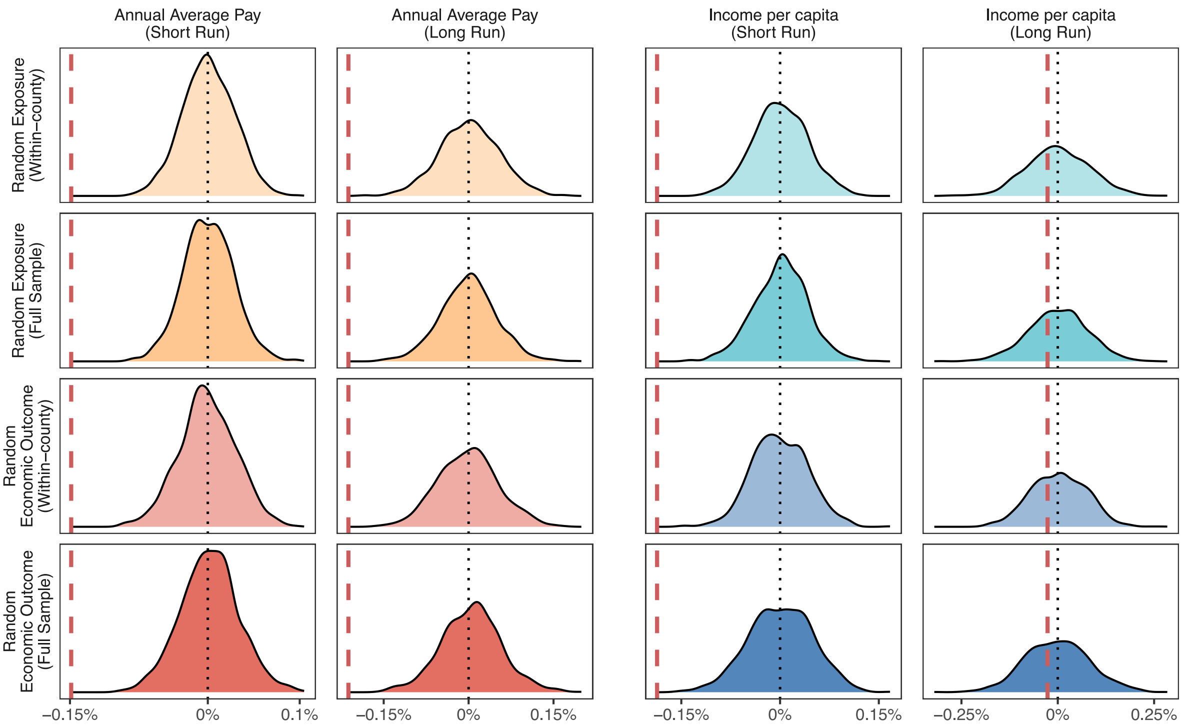


Fig. 6. Distributions of cumulative effects of severe thunderstorms in the short ($\hat{C}_3$) and long ($\hat{C}_{10}$) run for one pooled standard deviation of the exposure measure, on Annual Average Pay (wages) and Income per capita, obtained re-estimating Model (7) on randomized datasets. Randomization is carried out with two different schemes: i) randomly re-assigning observations within each county (Within-county), or ii) randomly re-assigning observations across the entire panel (Full Sample). Each randomization scheme is carried out on the exposure measure (Random Exposure) and, separately, on the dependent variable (Random Economic Outcome), repeating randomization and subsequent estimation 1000 times. The exposure measure is $S^{(4)}$ (Eqs. (5) and (6)). The number of lags considered is $p = 10$. The climatic controls included are population-weighted county-level yearly total precipitations and average temperatures. Vertical dotted black lines mark 0 and vertical dashed red lines mark the original estimates obtained fitting Model (7) without randomizing the data (Fig. 4 and Table 1). T-tests comparing the means of the estimates obtained through randomization to the original estimates all yield p-values < 0.001.

dependent variables. Furthermore, for each exercise, we implement two different randomization schemes. In the *Full Sample* randomization scheme, we scramble the whole set of values (exposures, or dependent variables) thereby introducing randomness in both timing and location of the observations. This allows us to assess potential biases stemming from both temporal trends (local or aggregate) and cross-sectional patterns (across counties). In the *Within-county* randomization scheme, we scramble the time series (exposures, or dependent variables) separately within each county. This specifically targets potential biases stemming from cross-sectional patterns (across counties). Fig. 6 displays the distribution of point estimates obtained through repeated re-fits of Model (7) on datasets generated through the four combinations of variables being randomized (exposures, or dependent variables) and randomization schemes being applied full sample, or within-county). Randomization and re-estimation are repeated 1000 times for each configuration. Specifically, for each of the four combinations and for each dependent variable, we show estimates of the cumulative effects of storm exposure in the short-run ($\hat{C}_3$) and in the long-run ($\hat{C}_{10}$). The averages of all distributions are around 0, and the original estimates (red dashed lines in Fig. 6) fall well to the left of the range of the randomized estimates, except for the long-run effect on income (the original estimates for this effect where consistently non-significant in our multiple analyses). In all, the "placebo" results demonstrate that our original estimates are highly unlikely to stem from the omission of time or location-time specific terms.

Random sub-samples. To ascertain whether our results may be driven by a limited number of counties, we perform a sub-sampling exercise, progressively excluding from the analysis increasing fractions of counties (10%, 20%, 30%, and 40%) selected at random. For each fraction, we repeat random sub-sampling and the re-estimation of Model (7) 1000 times. As shown in Figure B.6, the larger the fraction of counties excluded, the more uncertainty one has in effect estimates. Notwithstanding such an obvious effect related to sample size, the averages of the distributions of effect estimates remain significantly different from 0, demonstrating that our findings are not driven by a restricted subset of counties.

Pre-exposure coefficients. Although our model only relies on conditional independence, and our dependent variables (county-level income and wage growth rates) are trendless, we enrich our distributed lag model with exposure leads to exclude pre-existing diverging patterns between counties that are differently hit by storms, conditional on our controls (Schmidheiny et al., 2019). As shown in Figure B.7, pre-exposure coefficients are not significant for both wages and income per capita, while post-exposure estimates are very similar to those reported in Fig. 4, which hints to the robustness of our baseline estimates.

Repeated storms. Due to the frequent occurrence of thunderstorms, counties may experience additional exposure to storm winds while still recovering from the previous one. This could introduce bias if the dynamics induced by both exposure events do not represent a linear combination of their individual effects (Zivin et al., 2023). To control for such a bias, we re-estimate Model (7) on subsamples that specifically consider counties where significant exposures are both preceded and followed by periods of low or absent exposures. We do so experimenting with different thresholds.[23] Following this strategy we attempt at aligning our approach to an event study, though imperfectly. Indeed, storms are frequent and relatively mild events in the majority of contiguous US (as shown in Fig. 1). Hence, contrary to the case of hurricanes (Zivin et al., 2023; Hsiang and Jina, 2014), it is impossible to find in our dataset a large number of locations facing a single large exposure within a decade, which is what we would have needed to closely mimic an event study or a diff-in-diff design.[24] Given such limitation imposed by our data, the experiments we conduct confirm our baseline results. As depicted in Figure B.8, while income tends to recover slightly faster than observed in Fig. 4, wage estimates are largely uncharged, although the considerably smaller sample sizes lead to broadened confidence intervals. Overall, repeated years of large exposure to storms induce effects that can be assimilated to a linear combination of single exposures.

Event-study difference-in-difference. As discussed above, our framework does not represent an ideal setup for an event-study difference-in-difference approach (DiD). While related methods have progressed allowing for repeated (non-absorbing, De Chaisemartin and d'Haultfoeuille, 2024), staggered (Callaway and Sant'Anna, 2021; Borusyak et al., 2024), or continuous (Callaway et al., 2024) treatments, these approaches typically presuppose scenarios where treatments can be distinctly identified across units and time. However, they can be extremely useful to test pre-existing trends. We attempt to approximate a DID setup by discretizing our observations into treated and non-treated groups based on whether the county-year storm exposure (as measured by S^4) is simultaneously above the pooled median (thereby selecting relatively severe events) and above the within-county median (ensuring the selection of events that exceed average local climatic conditions). We then apply the recent event-study difference-in-difference estimator proposed by De Chaisemartin and d'Haultfoeuille (2024), which accounts for non-absorbing and staggered treatments, making the setting more comparable to our distributed lag model. Results are presented in Figure B.9 and Table B.8. The patterns observed in wages and income per capita closely mirror those obtained using the distributed lag model, both qualitatively and in terms of magnitude. Moreover, all pre-event coefficients are statistically non-significant, whether assessed individually or jointly, which confirms that the parallel trends assumption holds, consistent with the results obtained testing pre-event coefficients in the distributed lag model (Figure B.7).

Spatial and temporal clustering. Standard errors for our baseline estimates are computed using Conley standard errors (Conley, 1999), with a spatial cutoff of 50 km (about the average diameter of a U.S. county), and allowing for arbitrary serial correlation (Colella et al., 2023), with a temporal cutoff of one lag. This choice is based on the observation that only a small fraction of counties show statistically significant auto-correlation beyond one period for both wage and income growth, as well as for storm exposure $S^{(4)}$ (see Table B.9). In Figure B.10, we present results for our baseline estimates allowing for a broader spatial clustering (spatial cutoffs of 100 km and 150 km), Figure B.11 shows results allowing for a slower decay in the auto-correlation structure (temporal cutoffs extending up to 5 lags), while Figure B.12 shows results varying both spatial and temporal cutoffs. In all these scenarios, findings remain qualitatively unchanged.

Auto-correlation. We conclude this Section with an additional analysis addressing auto-correlations. Estimates from distributed lag models may exhibit some degree of bias due to the presence of short-run serial correlation in growth rates. A common robustness check involves introducing autoregressive terms in the estimated model. Following Cerra and Saxena (2008) and Hsiang and Jina (2014), we augment our baseline Model (7) with additional autoregressive terms, up to the 4th degree. As shown in Table B.13, this does not qualitatively change our results.

4.3. Spatial spillovers

Even highly localized events such as storm spells can induce economic effects that extend beyond the county where they occurred. Indeed, climate anomalies have been shown to produce impacts that propagate along supply chains and adversely affect trade, spreading to neighboring areas (Kotz et al., 2024). In this Section, we test the presence of relevant spillovers in neighboring counties. We do so by estimating the spatial SLX model specified in Eq. (9). Such model allows us to disentangle direct impacts (pertaining to the county where the event occurred) and local indirect impacts (extending to neighboring counties) (Halleck Vega and Elhorst, 2015).

Results shown in Fig. 7 reveal that storms trigger non-negligible indirect effects, whose qualitative behavior closely resembles that of direct effects. Spatial spillovers are less pronounced and statistically significant than direct effect for wages. On the other hand, they are approximately equal in magnitude to direct effects for income. This asymmetry is likely due to income being recorded by place of residence, while wages are recorded by place of work. Consequently, storms may have more spatially clustered effects on income than on wages due to commuting patterns. Notably, the direct effects for both wages and income are remarkably similar in magnitude to impacts obtained in the baseline model (Fig. 4). This finding implies that our baseline estimates, which neglect spatial spillovers, may be overly conservative. In addition, it reassures us that our baseline estimates did not capture geographically spurious effects. Furthermore, Tables C.2 and C.3 show that when expanding the distance criterion used to define neighboring counties (from less than 50 km to less than 100 km between centroids), the results remain substantially unchanged. This suggests that spatial spillovers are confined and do not extend much beyond the most immediate neighboring areas. Finally, results remain unaltered also with alternative weighting schemes (see Tables C.2 and C.3).

[23] See the caption of Figure B.8 for details.

[24] Our framework is much more similar to the one used to investigate the effects of heatwaves and El Nino in on economic activities in Callahan and Mankin (2022) and Callahan and Mankin (2023).

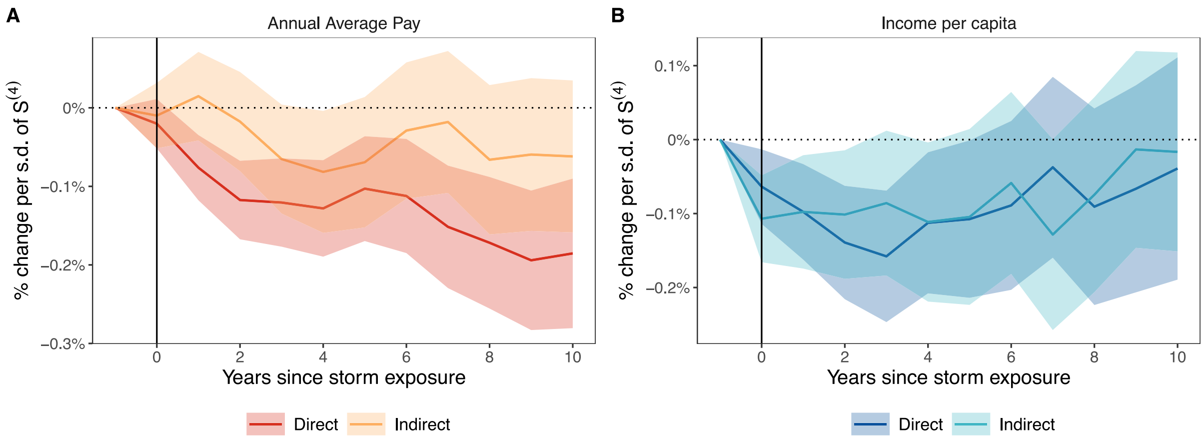


Fig. 7. Cumulative effects of severe thunderstorms ($\hat{C}_\tau$) for one pooled standard deviation of the exposure measure, on Annual Average Pay (wages; Panel A) and Income per capita (Panel B), as estimated by Model (9), capturing direct and indirect (to neighboring counties) effects. Neighbors are defined as those counties with distance D among centroids lower than 50 km; spatial weighting matrix $\mathcal{W}$ scheme based on inverse distance among centroids D^{-1}, globally standardized. Counties with no neighbors are removed from the dataset. The exposure measure is $S^{(4)}$ (Eqs. (5) and (6)). The number of lags considered is $p = 10$. The climatic controls included are population-weighted county-level yearly total precipitations and average temperatures. The shaded areas represent spatial and serial correlation robust Conley 95% confidence intervals (spatial cutoff 50 km, temporal cutoff 1 lag, Colella et al., 2023). The horizontal black dotted line at 0 represents the baseline trend for each county (a 0 effect indicates that a county follows its baseline trajectory after storm exposure). Full details on estimates can be found in Tables C.2 and C.3.

4.4. Hailstorms

As an additional way to validate our findings, we perform a parallel analysis exploring the impacts of a distinct yet analogous weather event recorded in the SED dataset: hailstorms. Although damages occur differently (thunderstorms cause damage mainly through strong wind gusts and potential flash floods), this parallel analysis provides an opportunity to scrutinize the effects of a detrimental meteorological phenomenon that bears some resemblance to thunderstorms—in terms of potential for property damage, predominant type of harm and highly localized nature. Consequently, we anticipate that hailstorms will exhibit economic impacts similar to those of thunderstorms.

Like thunderstorms, hailstorms are geographically confined and short-lived weather events. On average, they have a duration of approximately one hour, and the distance between their starting and ending locations is typically between 20 and 30 km (Figure A.1C). However, they differ from thunderstorms in terms of geographical distribution (Figure A.1A), as they primarily occur in the Central part of the continental U.S.—while thunderstorm exposure is highest in the Eastern part of the country (Fig. 3). Notably, we are thus investigating a comparable event whose strongest impacts are expected to affect areas characterized by a different socio-economic context.

The magnitude of hailstorms is measured in terms of hailstone diameter. Accordingly, we construct an exposure measure that serves as a proxy for the energy carried by the hailstorm. Specifically, we calculate the sum of the cubes of hailstone diameters recorded in hailstorms that occurred in each county and year, as described in Eq. (4), thereby producing a measure directly related to the volume of hail cubes. To ensure cross-county comparability, we then normalize using Eq. (6), resulting in a hail exposure measure $H^{(3)}$ which is analogous to $S^{(3)}$ for thunderstorms—see Appendix A for additional details. In fact, the distribution of $H^{(3)}$, likewise that used for thunderstorms, exhibits a pronounced right skew (Figure A.1B).

Results for the fits of Model (7) with $H^{(3)}$ as the exposure measure are shown in Fig. 8. Like for thunderstorms, wages tend to decline in the first three years following exposure, and then stabilize on a plateau. Effect estimates are generally statistically significant (except for $\hat{C}_{10}$), but the magnitude of the short-run decline is less pronounced with respect to thunderstorms ($\hat{C}_3 = -0.069$). For income, effect estimates are consistently positive but not statistically significant. Nevertheless, patterns for both wages and income are qualitatively consistent with those observed for thunderstorms, and show that also hailstorms induce different impacts on the two dependent variables.

4.5. Do income levels and long-term exposure matter?

After assessing the impacts of severe storms on economic activity, we investigate whether such impacts vary in significant and meaningful ways across the counties in our panel—and in particular, whether counties present different degrees of resilience related to income levels and long-term exposure—as these could be interpreted as evidence of differential response and adaptation ability.[25] To this end, we employ Model (10), where exposures at all lags interact with a categorical variable representing groups of counties.

[25] Here, we broadly refer to adaptation as the array of measures that a specific community can implement to mitigate the impacts of weather events.

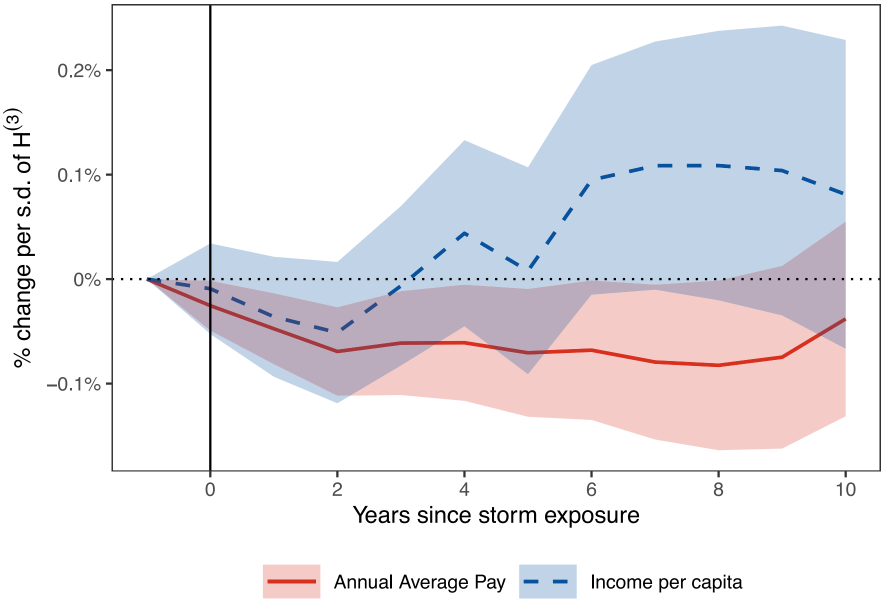


Fig. 8. Cumulative effects of hailstorms ($\hat{C}_\tau$) for one pooled standard deviation of the exposure measure, on Annual Average Pay (wages; red) and Income per capita (blue), as estimated by Model (7). The exposure measure, based on hail size, is $H^{(3)}$ (Eqs. (4) and (6)). The number of lags considered is $p = 10$. The climatic controls included are population-weighted county-level yearly total precipitations and average temperatures. The shaded areas represent spatial and serial correlation robust Conley 95% confidence intervals (spatial cutoff 50 km, temporal cutoff 1 lag, Colella et al., 2023). The horizontal black dotted line at 0 represents the baseline trend for each county (a 0 effect indicates that a county follows its baseline trajectory after storm exposure). Full details on estimates can be found in Table C.8.

A broad literature emphasizes the role of income in shaping both resilience and adaptation to natural hazards. Indeed, richer areas tend to absorb weather-related impacts more efficiently (e.g., due to more efficient institutions, higher savings, or spending capacity), and economically disadvantaged areas can suffer from adaptation deficits or gaps (Fankhauser and McDermott, 2014). Moreover, empirical evidence suggests a positive relationship between income and the demand for climate security (Bakkensen and Mendelsohn, 2016). To examine this in the context of thunderstorms, we partition the counties in our panel into three distinct groups based on their initial income level (below, between, or above the inter-quartile range of the income distribution in 1991; Fig. 9C) and estimate Model (10). Our results do *not* display marked differences for what concerns impacts on wages (Fig. 9A). Counties in the low, middle and upper range of the income distribution exhibit indistinguishable responses to storm exposure, essentially identical to the pooled results shown in Fig. 4. However, there are notable differences in estimated impacts on income (Fig. 9B). The poorest counties exhibit much larger contemporaneous impacts ($\hat{C}_0 = -0.272\%$, compared to -0.056% for medium-income counties and a statistically not significant -0.038% for high-income counties), as well as short-run impacts ($\hat{C}_3 = -0.365\%$, compared to -0.136% for medium-income counties and a statistically not significant -0.116% for high-income counties). Moreover, while medium- and high-income counties do exhibit a return-to-trend behavior in the long run, poor counties remain significantly below their pre-exposure levels, with a negative estimated long-run impact as large as $\hat{C}_0 = -0.404\%$.

Empirical studies have also provided evidence suggesting that historical exposure to natural hazards can stimulate adaptation efforts to reduce losses. Fankhauser and McDermott (2014), Hsiang and Narita (2012), Neumayer et al. (2014), Schumacher and Strobl (2011) and Plumper et al. (2010). However, other works (sse e.g., Bakkensen and Mendelsohn, 2016) have reported no significant hazard-driven adaptation to hurricane-related damages in the United States. In the context of thunderstorms, counties that frequently experience damaging winds could have invested more in preventive measures to contain damages and the negative impact of severe weather events. To examine this hypothesis, we partition the counties in our panel into three distinct groups based on their average historical exposure (below, between, or above the inter-quartile range of the distribution of average exposure, where averages are taken over the period 1991–2019).[26] Evidence of hazard-driven adaptation would be reflected in lower impact estimates in classes with higher risk, i.e., higher average exposure. Our findings, consistent with Bakkensen and Mendelsohn (2016), do *not* support such hypothesis, see Table C.5. Counties in the middle and upper range of the risk distribution exhibit estimates very similar to the pooled results shown in Fig. 4, and counties in the low range of the risk distribution exhibit virtually no effect.[27]

[26] To avoid possible issues of under-reporting of weather events and taking into account that year-on-year exposure to storms may vary significantly due to intrinsic weather stochasticity, we decided to partition the data according to the historical average exposure within our full sample, and not according to the intensity of storm activity in a given year.

[27] Note that these counties are rarely affected by thunderstorms and essentially serve as a control group here; their minimal exposure does not generate statistically detectable impacts.

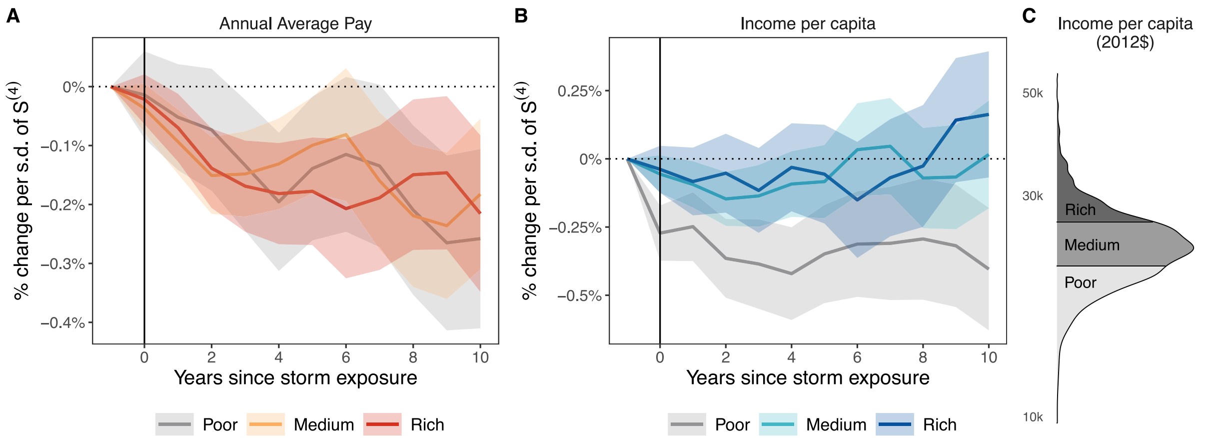


Fig. 9. Cumulative effects ($\hat{C}_\tau$) of severe thunderstorms for one pooled standard deviation of the exposure measure, on Annual Average Pay (wages; Panel A) and Income per capita (Panel B), as estimated by Model (10) with dummies $\mathcal{D}_{i\ell} = \mathcal{D}_i$ capturing three income groups (counties with 1991 income below, in and above the inter-quartile range; Panel C shows the 1991 income distribution in 2012$ and on the logarithmic scale). The exposure measure is $S^{(4)}$, as defined in Eqs. (5) and (6). The number of lags considered in $p = 10$. The climatic controls included are population-weighted county-level yearly total precipitations and average temperatures. The shaded areas represent spatial and serial correlation robust Conley 95% confidence intervals (spatial cutoff 50 km, temporal cutoff 1 lag, Colella et al., 2023). Horizontal black dotted lines at 0 in Panels A and B represent the baseline trend for each county (a 0 effect indicates that a county follows its baseline trajectory after storm exposure). Full details on estimates can be found in Table C.4.

Taken together, our results suggest that severe storms may exacerbate income inequality not only through different impact patterns on labor and capital incomes, but also through asymmetric effects across counties. While middle- and high-income counties do return to trend in terms of income, poor counties—which may lack the capacity to effectively update their capital in response to adverse events—do not. Poor counties may in fact lack the capacity to effectively update their capital in response to adverse events, leading to a greater persistence of negative impacts on income. The observed differences in impacts may be influenced not only by greater recovery capabilities, but also by higher adaptation efforts in wealthier counties, which are less income-constrained. Nonetheless, our results do suggest that these differing efforts are not related to hazard risk.

4.6. The role of relief policies

Results shown in the previous Section suggest a possible link between local spending capacity and a form of resilience to the impacts of thunderstorms. Relatedly, we next investigate whether public aid provided in the aftermath of storm events can affect counties' adaptation capabilities, and, more generally, our impact estimates.

A significant body of research has found a positive role for the aid provided by governments and international organizations in limiting the impacts of major natural hazards (Davlasheridze and Miao, 2021; Deryugina, 2017; Hochrainer, 2009; Yang, 2008). Given their localized and generally non-extreme nature, U.S. thunderstorms do not activate international assistance—but the most disruptive ones can overwhelm the resources of local and state authorities, thus prompting federal intervention. Federal responses to emergencies and disasters in the U.S. are typically the domain of the Federal Emergency Management Agency (FEMA). Upon issuance of a formal disaster declaration, FEMA can activate its assistance programs[28] and provide financial means, resources and expertise to support affected areas in their response and recovery efforts.

We thus employ the FEMA Disaster Declaration Summary (DDS) to identify events for which FEMA issued a disaster declaration. To determine which storms were covered by FEMA, for each county we use a matching procedure between storm dates in the SED database and disaster declaration dates in the FEMA record. Conservatively, we establish a match between a storm and a disaster declaration only if the timeframes (starting and ending dates) reported for the hazard pertaining to the declaration by DDS fall within those reported for the storm by SED (Figure A.2 provides a visual representation of the matching procedure). The disaster entries selected for the matching procedure include only those categorized as explicitly labeled as storms (see Appendix A for more information on data treatment).

We identify a total of 8436 storm events for which FEMA issued a disaster declaration, 4.13% of all storms in our dataset. Next, since FEMA assistance is typically deployed for an extended period of time (often several months) and our panel data is aggregated at the county-year level rather than at the storm level, we label any county-year in which at least one storm received FEMA support

[28] These programs include Individual Assistance, Public Assistance, and Hazard Mitigation. The Individual Assistance program provides financial support to affected individuals and households, while the Public Assistance program offers funding to local government and nonprofit organizations for recovery efforts (e.g., covering facilities reparation costs). The Hazard Mitigation program focuses on reducing future disaster risks. In addition, Small Business Administration (SBA) loans are often made available in conjunction with FEMA assistance programs. The SBA offers low-interest loans to homeowners, renters, and businesses located in the affected region.

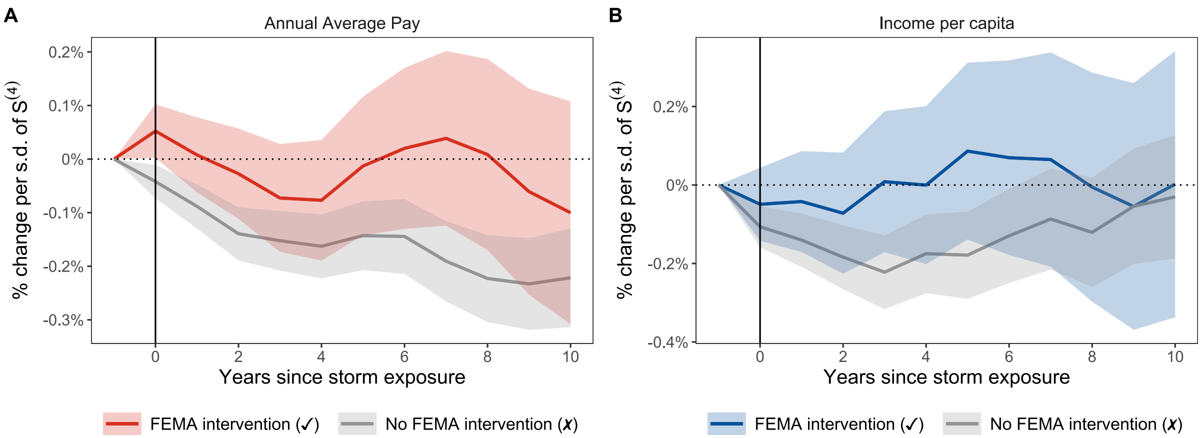


Fig. 10. Cumulative effects ($\hat{C}_\tau$) of severe thunderstorms for one pooled standard deviation of the exposure measure, on Annual Average Pay (wages; Panel A) and Income per capita (Panel B), as estimated by Model (10) with dummies $\mathcal{D}_{it}$ capturing county-year pairs with FEMA interventions. The exposure measure is $S^{(4)}$ (Eqs. (5) and (6)). The number of lags considered is $p = 10$. The climatic controls included are population-weighted county-level yearly total precipitations and average temperatures. The shaded areas represent spatial and serial correlation robust Conley 95% confidence intervals (spatial cutoff 50 km, temporal cutoff 1 lag, Colella et al., 2023). Horizontal black dotted lines at 0 in Panels A and B represent the baseline trend for each county (a 0 effect indicates that a county follows its baseline trajectory after storm exposure). Full details on estimates can be found in Tables C.6 and C.7. More information on the identification of county-year pairs associated with FEMA interventions can be found in Appendix A and Figure A.2.

as a "FEMA intervention" datum. There are 3641 county-year pairs, 5.21% of all pairs in our panel. We then employ a model akin to Model (10) utilizing the categorical variable to separate county-years into "intervention" and "non-intervention" (see caption of Fig. 10).

Fig. 10 suggests a remarkable effect of federal assistance on the estimated storm impact patterns: restricting attention to counties and years where FEMA issued a disaster declaration, the impacts of storm exposure on both wages and income are close to 0 and statistically non-significant across almost the entire impact period considered. The only exception is the contemporaneous impact on wages ($\hat{C}_0$), which is significantly positive. This suggests that FEMA intervention quickly prompts an influx of resources which sustain wage growth over the medium run. Reassuringly, for counties and years where FEMA did not issue a disaster declaration, impact patterns are indistinguishable from the overall ones in Fig. 4—confirming that our main conclusions are not driven by a small portion of extreme cases (i.e., counties-years with thunderstorms triggering disaster declarations).

We also tested whether our results could be influenced by spurious effects, such as counties-years experiencing other simultaneous disasters that prompted FEMA intervention. We accordingly augmented our model controlling for FEMA interventions related to non-wind-water-related events. Results (see Tables C.6 and C.7) are essentially indistinguishable from those in Fig. 10. As previously mentioned, these results rely on a conservative matching procedure that identifies storm-disaster pairs only when disasters are explicitly categorized as storms in DDS. However, when we expand our matching procedure to include disaster declarations involving secondary hazards (e.g., floods or landslides that may occur as a consequence of a storm) and other more hazardous wind-water related events, the estimates in the "FEMA intervention" group get closer to those in the "No FEMA intervention" group (see Tables C.6 and C.7).

In summary, our findings show that FEMA interventions can play a crucial role in mitigating the adverse impacts of storms on wages and income dynamics, potentially preventing any associated increases in income inequality. However, estimates obtained when considering more hazardous water-wind related disaster declarations suggest that the effectiveness of interventions may diminish as the severity of events increases—indicating that federal assistance might not fully offset the largest impacts.[29]

5. Conclusions

In this study, we examine the economic impacts of severe thunderstorms. While previous research has extensively analyzed the impacts of extreme events such as hurricanes and tropical storms, our study is the first to provide a comprehensive analysis of these less extreme, yet much more common and still pernicious weather events. We employ detailed information on over 200,000 severe storms occurred in the continental United States from 1991 to 2019, including wind speed and geolocation data, to create physically-grounded storm exposure measures, and use these to fit distributed-lag models within a panel framework.

[29] Due to the distinct categorization of events across SED and SSD datasets, we cannot rule out the possibility that disasters declaration not explicitly labeled as storms—which we indeed excluded from our baseline estimates in Fig. 10—may refer to separate events occurring concurrently with a storm episode. Consequently, our results related to larger events might underestimate the positive impact of FEMA intervention. See Appendix A for more information on SSD labeling.

Our analyses reveal significant negative economic effects associated with storm exposure. Specifically, we find that severe storms considerably affect income, which then recovers over time consistent with a *return to trend* hypothesis. However, we also find that severe storms lead to persistent declines in wages, exhibiting hysteresis and supporting a *no recovery* hypothesis. Jointly, these results suggest that storm exposure can contribute to an increase in functional income inequality—likely by accelerating capital obsolescence and the subsequent adoption of labor-saving technologies, which may explain the diverging trajectories observed for wages and income. This is supported by our analyses disaggregated by economic sectors. In addition to a broad battery of robustness checks, our main findings are also confirmed by a separate analysis of the impacts of hailstorms occurred in the U.S. between 1991 and 2019.

We also run analyses to investigate whether some communities may be better equipped than others to withstand the impacts of storms. In particular, we find that economically disadvantaged counties experience larger and more enduring income losses, suggesting that local spending capacity (and perhaps attitudes correlated with affluence) can help mitigate negative economic impacts. In contrast, we do not find evidence of hazard-driven adaptation, i.e. of a reduction in economic impacts associated with long-term exposure levels.

Finally, we investigate whether federal interventions can reduce the severity of economic impacts and foster more equitable recoveries. Remarkably, the issuance of FEMA disaster declarations and the provision of aid do dampen the negative consequences of severe storms on both income and wages, although such positive effects may not apply to the largest events.

Our work can be extended in several ways. Specifically, we intend to delve deeper into the impact of federal aid and related support programs on shaping recovery trajectories post-hazardous events, with a particular focus on their role in fostering equitable outcomes. Prior studies have indeed reached conflicting conclusions. While micro-econometric approaches reported evidence of FEMA intervention worsening wealth inequality along lines of race, education and home ownership (Howell and Elliott, 2019), other studies suggested instead a generally equitable allocation of FEMA relief funds (Domingue and Emrich, 2019). Our own observations only pertain to the domain of functional income inequality, and are limited to a specific subset of relatively moderate weather events. Further studies employing more disaggregated data will be needed to validate our findings and elucidate to what extent federal interventions may promote equitable outcomes. Relatedly, we believe that extending the present analysis to the effects on the full distribution of personal income is a promising avenue that could complement with finer-grained information a consolidating body of evidence relying on country level data (Cappelli et al., 2021; Castells-Quintana and McDermott, 2023; Paglialunga et al., 2022; Méjean et al., 2024; Gilli et al., 2024).

CRediT authorship contribution statement

Matteo Coronese: Writing – review & editing, Writing – original draft, Visualization, Methodology, Investigation, Data curation, Conceptualization. **Federico Crippa:** Writing – review & editing, Writing – original draft, Methodology, Investigation, Data curation, Conceptualization. **Francesco Lamperti:** Writing – review & editing, Writing – original draft, Supervision, Methodology, Investigation, Funding acquisition, Data curation, Conceptualization. **Francesca Chiaromonte:** Writing – review & editing, Writing – original draft, Supervision, Methodology, Investigation, Funding acquisition, Conceptualization. **Andrea Roventini:** Writing – review & editing, Writing – original draft, Supervision, Methodology, Investigation, Funding acquisition, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jeem.2024.103074.

Data availability

All the data used in this study are publicly available. All code and data supporting the findings of this study can be accessed here.

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