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The impact of hydrogeological events on firms: Evidence from Italy

Stefano Clò [a,*], Francesco David [b], Samuele Segoni [c]

[a] *Department of Economics and Management, University of Florence, Italy*
[b] *Bank of Italy, Regional Economic Research Division, Palermo Branch, Italy*
[c] *Department of Earth Sciences, University of Florence, Italy*

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ABSTRACT

Using a novel dataset of natural disasters affecting Italy from 2010 onward, we investigate the impact of hundreds of hydrogeological events on firms' survival and performance. Despite being less extreme, these events are increasingly frequent and geographically widespread, this constituting a relevant but unexplored topic in the natural disasters literature. In order to assess the impact of multiple events occurred over several years, we implement a staggered difference-in-differences design that exploits the variation in the timing of the treatment. Our results show that hit firms have a 7.3% higher probability of exiting the market. Conditional on surviving, in the three years after the calamity, firms experience an average decline in their revenues and employment by −4.9% and −2.2%, respectively. These impacts are highest for micro-small, younger and low-tech firms.

1. Introduction

The ongoing climate crisis has been influencing both the frequency and the intensity of rainfall, leading to a rise in the occurrence of harmful hydrogeological events (HG events henceforth, namely floods and landslides; Hoeppe 2016). These events exhibit diverse trends and impacts globally (Gariano and Guzzetti 2016; Hacque et al., 2016). The Emergency Events Database (EM-DAT) managed by the Centre for Research on the Epidemiology of Disasters (CRED) documents an increase in the number of climate-related HG events and other natural catastrophes, contrasting with a comparatively insignificant rise in geophysical events such as earthquakes, tsunamis, and volcanic eruptions. Climate-related disasters, with floods being the most prevalent, constitute over 90% of calamities recorded worldwide between 1998 and 2017. These disasters resulted in more than 160,000 deaths and economic losses totaling approximately USD 660 billion (UNISDR-CRED 2018).

The economic impact of natural disasters extends beyond immediate losses and damages to physical capital (direct effects) and may persist in the medium to long run through interactions among economic agents (indirect or high-order effects). From a theoretical standpoint, the long-term impact of natural disasters is unclear: economies may or may not recover to the previous trend or might even grow faster if, for example, the damaged productive capital is replaced and upgraded.

A substantial body of empirical literature has explored this pertinent question, but a consensus has not yet emerged. Earlier

* Corresponding author.
E-mail addresses: stefano.clo@unifi.it (S. Clò), francesco.david@bancaditalia.it (F. David), samuele.segoni@unifi.it (S. Segoni).

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contributions focused on cross-country comparisons of aggregate economic outcomes, such as GDP. Many studies utilized the EM-DAT database, which, however, underreports events characterized by minimal loss of life or those not requiring international aid, mainly in developed countries (Kousky 2014; Botzen et al., 2019).

These studies have yielded mixed evidence regarding the sign and intensity of the impact of natural disasters, ranging from null to slightly negative and temporary to highly negative and long-lasting.

Recent analyses focus on regional or county-level outcomes, while others analyze firms or households micro-level data. In many cases, these studies adopt a difference-in-differences empirical strategy that exploits a single natural catastrophe as a quasi-natural experiment. Results are more homogeneous, emphasizing the negative effects of singular and exceptionally impactful event.

The role of multiple and less severe events has received less attention so far, despite their increasing frequency and potential for non-negligible economic costs.

Our research builds on this literature to investigate the causal impact of hydrogeological disasters on Italian firms. To gain a comprehensive picture of the effects of natural disasters on business activities, we analyze both firms' survival probability (the extensive margin) and their performance conditional on survival (the intensive margin).

For this purpose, we exploit a novel municipal-level nationwide dataset of more than 800 HG events that hit the whole Italian territory over the period 2010–2018. This was compiled through an automatized web scraping algorithm that collects and geotags (at the municipal level) internet news referring to landslides and floods.

Our research contributes to the existing literature in several ways. While the literature has mainly focused on single, isolated and particularly extreme events, such as the Katrina hurricane (Basker and Miranda 2018) or the Ise Bay typhoon in Japan (Okubo and Strobl 2021), our research looks at the impact of hundreds of HG events that took place in Italy for almost a decade. Despite being less extreme, these events are increasingly frequent and geographically widespread, this representing a relevant but unexplored topic in the natural disasters literature.

Second, in order to assess the impact of multiple events occurred over several years, we implement a quasi-experimental design that departs from the standard two-way fixed effects difference-in-differences approach. Building on recent developments in the difference-in-differences literature (Deshpande and Li, 2019; Fadlon and Nielsen 2021; see Roth et al., 2022 for a review), we implement a staggered difference-in-differences design that exploits the variation in the timing of the treatment, i.e. the different years in which Italian municipalities were hit by a natural disaster. This approach is more robust to time heterogeneity in the treatment.

Third, we analyze a geographically accurate database; indeed, we identify municipalities hit by a HG event, while most of the previous multi-hazard analysis is conducted at regional or even country level. Moreover, data collected through an automatic procedure are less prone to self-reporting bias.[1]

Owing to its high exposure to hydrogeological hazards, Italy constitutes a relevant, though underexplored, setting to investigate the impact of multiple HG events.[2]

Our results show a negative impact of floods and landslides both on firms' survival probability and the performance of (surviving) businesses. Compared to firms located in municipalities unaffected by natural disasters, hit firms have a 7.3% higher probability of exiting the market; conditional on surviving, in the three years after the calamity, firms experience an average decline in their revenues and employment by −4.9% and −2.2%, respectively. We also observe a 3% increase in the share of intangible assets, which, compared to tangible ones, are less exposed to the risk of physical damage. The heterogeneity analysis clarifies that impacts are highest for micro-small, younger and low-technology firms.

Our estimates should be interpreted as an average impact of HG events on both directly and non-directly hit firms. Dealing with hundreds of events, we cannot precisely delimit municipal areas affected by the catastrophe (as done in some other paper focusing on a single major event); therefore, we are unable to disentangle the impact on the two sets of firms. This minor loss is compensated by the gain in external validity coming from analyzing multiple events. Moreover, since we are interested in both direct and high-order effects of hydrogeological events on firms, we believe it reasonable to assume that businesses located in hit municipalities are all, in some way, affected. Indeed, as recognized by Kousky (2014), business continuity may suffer damages to suppliers, evacuation of workers or loss of electricity and water; households and firms may have to adopt costly measure in order to cope with loss of infrastructure and firms may be affected by reduction of local demand via income effects (see also Johar et al., 2022).

Moreover, our results measure the effects of HG events net of financial aids that eventually were disbursed to affected areas. Indeed,

[1] As acknowledged by Hsiang and Jina (2014) this is a major shortcoming in existing dataset and one potentially affecting the quality of estimates based on them, raising endogeneity issue since "The quality and completeness of these self-reported measures are known to depend heavily on the economic and political conditions in a country, factors which also affect growth and thus might confound these results".

[2] From a geological point of view, the Italian territory is still in a juvenile geomorphological stage, meaning that HG events are naturally more intense and frequent than in more mature territories. Italy is characterized by hillslopes and mountains (42% and 35% of the national territory, respectively), which are naturally affected by erosion and mass-wasting processes, such as landslides. During the last few decades, an increase in landslide activity has been acknowledged, partly due to climate change and its effects on rainfalls' frequency and intensity (Gariano and Guzzetti, 2016). Moreover, except for a few relevant main rivers, Italy is characterized by steep riverbeds and narrow valleys that are susceptible to flash floods. In turn, the largest alluvial plains have always been naturally affected by large flood events caused mainly by prolonged rainfall events. The problem is exacerbated by urban expansion: at least since the Middle Ages, the areas of natural expansion of rivers were subtracted from their physiological dynamics to expand cities, industrial districts or crops. More recently, foothills and mountainsides have been highly urbanized as well, resulting in a relevant exposure of assets, properties and infrastructures to either landslides or floods. According to the Italian Ministry of the Environment, 91% of municipalities cover some areas exposed to the maximum level of landslide or flood hazard and 18.3% of Italian firms are located in such areas (Iadanza et al., 2021).

focusing on a plurality of events spanning over a decade and working with the universe of incorporated firms, we are unable to collect comprehensive and granular information on the financial aids that some firms might have received from national, regional or local authorities in the post-disaster recovery process. Notwithstanding, due to data limitations, we are unable to disentangle the role of aid from the effects of calamities, a shortcoming that is widespread in the literature on natural disaster, even in the case of devastating events and developed countries.[3]

Our results are validated by a rich set of tests designed to address several potential issues. First, placebo tests provide evidence in favour of the parallel trend assumption that must hold for the staggered difference-in-differences design to provide unbiased estimated. Second, we show that our results hold when we apply more demanding conditions to address potential endogeneity bias that might arise from the potential non-random distribution of natural events or in the case of firms' endogenous localization choices.[4] Third, we verify that our results are not driven by those firms that experienced multiple HG events. Fourth, we analyze potential spatial spillovers among municipalities, finding that the indirect effects of natural disasters involve mainly adjacent areas, while leaving relatively unaffected non-bordering municipalities. Fifth, we confirm the validity of our results under alternative definition of the event of interest. Sixth, we exploit the heterogeneity in the municipalities' dimension to address potential measurement errors that can raise by the adoption of municipality-level treatments. Finally, following recent advances in the econometric literature, we test the sensitivity of our results by using the new estimator proposed by de Chaisemartin and D'Haultfoeuille (2020), which relies on slightly different assumptions with respect to our baseline.

The rest of the paper is organized as follows. After reviewing the relevant literature in Section 2, we describe the data used in Section 3 and the empirical strategy adopted in Section 4. Sections 5 and 6 report our results and robustness checks, respectively, while Section 7 concludes.

2. Literature review

The economic literature on the impact of natural disasters distinguishes between direct and indirect (or high-order) effects. The former are generally negative and concern mainly casualties and loss of physical assets, such as damaged infrastructures, buildings and firms' capital stock and inventories (Myung and Jang, 2011; Wirtz et al., 2014; Badoux et al., 2016; Hacque et al., 2016; Rossi et al., 2019). The size of direct costs is related to both the nature and the intensity of the disaster, as well as to societal resilience, which in turn is driven by a wide range of factors, as for example the effectiveness of early warning systems and evacuation plans, building codes, prevention measures, and the quality of government institutions (Kahn 2005).

The indirect effects may include both costs, such as those arising from the interruption of firms' activity or value chains, and benefits, for example in terms of a demand surge for reconstruction expenditures or for firms not directly hit by the disaster that may take over the reduced supply from impaired ones. Theoretically, the net indirect impact of natural disasters on economic activity is ambiguous. In a standard neoclassical model with exogenous technological progress, following the destruction of physical capital, more rapid capital accumulation will sustain higher growth rates until a new steady state is reached. In endogenous growth models with increasing returns to scale, technological change is increasing in the stock of capital, and the losses produced by natural disasters result in lower growth (Zhou and Botzen, 2021). Moreover, according to the creative destruction hypothesis, natural disasters may rise long-run growth if damaged capital is replaced and upgraded; firms may also raise investments in human capital as a substitute for the damaged physical one because the former is less exposed to the risk of physical deterioration and this may contribute to support economic growth (Skidmore and Toya 2002).

The empirical literature has found mixed evidence on the sign and intensity of the indirect effects of natural disasters. In a review of 64 studies published between 2000 and 2013, Lazzaroni and van Bergeijk (2014) show that the macroeconomic impact is null on average, whereas the meta-regression analysis of Klomp and Valckx (2014) finds a negative impact on growth rates, especially in developing countries. Hsiang and Jina (2014) exploits the within-country random variation in cyclone strikes, by means of granular satellite and other observational data, and finds a long-lasting impact of disasters on national income in both rich and poor countries. However, some of these macroeconomic studies are based on cross-country panel regressions, which can suffer from institutional and geographic differences that are not properly accounted for and that may be correlated with the probability of a disaster and with the intensity of its effects.

Sub-national analyses show more homogeneous findings, highlighting negative impacts on local growth and population (Strobl 2011), households' income and real estate values (Boustan et al., 2017), households' expenditures and their composition (with a reduction in health and education; Anttila-Hughes and Hsiang, 2013).

Recent research has assessed the impact of natural disasters on a plurality of firms outcomes mainly in a difference-in-differences

[3] See for example Deryugina (2022): "Finally, natural disasters almost always generate at least some aid response from governments, non-governmental organizations, and individuals. Such disaster aid can play an important role in the disaster-recovery process, as can private insurance. Because such responses are complex functions of disaster severity, it is essentially impossible to control for such aid administered in the aftermath of a disaster. Thus, the appropriate way to interpret the estimated impacts of a disaster assumes that they represent net impacts, including the effects of any policy response. Unfortunately, there are no reliable estimates of the causal effects of disaster aid on victims' outcomes. Thus, while aid is certainly helpful, its exact effectiveness is currently unknown."

[4] We test the sensitivity of our model by i) restricting the control group only to not-yet treated firms or ii) developing a conditional staggered difference-in-differences strategy where we select, through a propensity score matching (PSM) technique, a sub-sample of control units with pre-treatment characteristics similar to the treated units.

setting, exploiting natural disasters as quasi-natural experiments. Leiter et al. (2009) use the EM-DAT database to identify European regions (NUTS-II) hit by floods in the year 2000 and use a difference-in-differences approach to analyze the effect of floods on capital stock, employment, and productivity. Their results show a higher growth in capital accumulation and employment for firms in hit areas and a negative impact on productivity. Boehm et al. (2019) find that natural disasters decrease firms' productivity by disrupting networks, destroying infrastructures, increasing the costs of day-to-day operations. Zhou and Botzen (2021) analyze several flooding and storm events in Vietnam during 2000–2014; by using a GMM model, they achieve consistent evidence of negative effects on firms' growth. Arrighi et al. (2022) quantify the indirect costs caused by a localized flood in a wide area of Tuscany (Italy) due to the disruption of linear infrastructures such as roads and water pipes. Coelli and Manasse (2014) estimate the impact of the 2010 flooding in the Veneto region (Italy) on the short-run performance of manufacturing firms, through a difference-in-differences model. They find a positive impact on value added growth, partially attributable to public recovery funds. Some recent studies jointly examine the survival probability of firms and the performance of survivors. Basker and Miranda (2018) exploit Hurricane Katrina in 2005 as a natural experiment and find very low survival rates for damaged businesses, especially small and less-productive ones, while conditional on survival, larger and more productive firms experienced higher employment growth in the following years. Studying the impact of the Vera Typhoon, which hit Ise Bay in Japan in 1959, Okubo and Strobl (2021) highlight the heterogeneity of the effects of the storm among sectors (negative for retail and wholesale, positive or negligible for manufacturing and construction), both on survival probability and on performance.

Natural disasters are not only relevant in terms of firms' economic activity. Various researches focus on households and document a negative short-term impact of natural disasters either income or employment. Gröger and Zylberberg (2016) focus on the Typhoon Ketsana, which inundated parts of Vietnam in 2009, finding a negative short-term impact in flooded households' income, mainly due to a disruption of agricultural production. Deryugina et al. (2018) and Groen et al. (2020) both focus on the Hurricane Katrina which severely affected the city of New Orleans in 2005, finding that residing individuals experienced a decline in their short-term income and faced a notable decrease in the likelihood of employment. Kocornik-Mina et al. (2020) focus on 54 major floods which affected mainly poor countries and find that their short-run negative effects on economic activity, proxied by satellite-detected night light intensity, are quickly recovered. Boustan et al. (2020) analyzed 90 years of US disaster data and estimate that disasters negatively affect local income and housing prices causing a rise in poverty and net migration.

Other studies focus on individuals well-being and social welfare. An extensive body of literature has identified numerous long-term negative effects of natural disasters on various socio-economic variables, including physical and mental health, education, crime rates, social cohesion, environmental degradation, inequality, absolute and relative poverty, and overall well-being (Noy, 2009; Hallegatte and Przyluski 2010; Cavallo et al., 2013; Felbermayr and Gröschl, 2014; Kousky, 2014; Botzen et al., 2019).

Joahr et al. (2022) analyze annual data from the Household, Income and Labour Dynamics in Australia (HILDA) Survey over a ten-year panel window (2009–18) and find that households whose home is damaged or destroyed by a natural disaster may in financial hardships and increased risk-aversion. Ho et al. (2023) find that adverse events cause an increase in financial distress and mortgage arrears.

Rehdanz et al. (2015) focus on the Fukushima nuclear disaster and document that people living in a place affected by the tsunami experienced a drop in life happiness. Conversely, Gunby and Coupé (2023) employ a staggered difference-in-differences design to assess the impact of a plurality of natural disasters which occurred in Australia for the period 2009 to 2019, and find little evidence of a statistically significant or sizable negative effect of weather-related home damage on subjective well-being.

Disasters can also increase inequality and poverty (Kahn 2005; Stromberg 2007). Keerthiratne and Tol (2018) find that natural disasters in Sri Lanka mainly affect the lowest-income households. Rodriguez-Oreggia et al. (2013) estimate a negative impact of natural disasters on the Human Development Index (HDI) and Poverty Index.

By affecting a broad range of physical and psychological health outcomes (Deryugina 2022), disasters may also reduce the accumulation of human capital, especially when occurring during the academic year (Spencer et al., 2016). Toya and Skidmore (2007) highlight a negative relationship between the number of deaths and education in the event of a natural disaster, highlighting a significant impact on educational outcomes. Paudel and Ryu (2018) find that, after the 1988 earthquake which occurred in Nepal, students were less likely to complete middle and high school. Natural disasters cause long-term negative effects on children well-being, particularly in developing countries by damaging their physical health and by causing mental health problems (Kousky 2016). Other studies highlight that natural disasters bring to an interruption of children's education due to the increased children participation in the labor market (Jacoby and Skoufias 1997; Beegle et al., 2006; Duryea et al., 2007).

Dell et al. (2014) argue that natural disasters can trigger social conflicts while other researches focus on the potential role of social cohesion and institutional quality in mitigating the negative impact of natural disasters on several socio-economic variables (Yamamura 2010; Barone and Mocetti 2014; Rodríguez-Pose 2020). De Juan et al. (2020) shows that local cooperation and social cohesion can increase in the aftermath of a natural disaster, while they can worsen as a consequence of irregular distribution of reconstruction aid which exacerbate pre-existing social inequalities. Boudreaux et al. (2023) find that the country's quality of governance moderates the short-term and long-term negative impact of natural disasters on entrepreneurship and start-up activity.

3. Data and descriptive statistics

The first step of the research consists of the establishment of a complete nationwide dataset of hydrogeological events (floods and landslides) with detailed and reliable spatial and temporal accuracy. Existing open datasets either collect information on negative events on a self-reporting basis or mainly account for the most significant events that brought to some form of public refunding compensation. While informative, these databases present some drawbacks in terms of geographical resolution, temporal extension,

Table 1
Number of events recorded by SECAGN per year.

Year	total	severe
2010	756	63
2011	718	91
2012	601	62
2013	1035	79
2014	1148	111
2015	952	100
2016	916	82
2017	994	112
2018	1559	169
Total	8679	869

and degree of representativeness and completeness.

To address these issues, we resort to a novel database developed by the Department of Earth Sciences of the University of Florence. The database is based on the so-called SECAGN (Semantic Engine to Classify And Geotag News), a semantic search engine that constantly scans the internet (at 30-min intervals) searching for news connected to landslides, floods, or similar phenomena (Battistini et al., 2013, 2017). The semantic algorithm is quite complex, as it has been constantly updated and upgraded for more than ten years to reduce errors, increase accuracy and adapt to changes in the standards used to release news through the internet (Franceschini et al., 2022). SECAGN performs a semantic analysis of internet news, searching for a series of keywords and connecting them until an internal system of weights assigns a score that is high enough to consider that news related to a target event (landslide or flood) occurred in the recent past. Subsequently, the news is geotagged (i.e., assigned to a specific spatial location, typically the municipality) based on the toponyms contained in the news, and finally, the information is stored in a geodatabase.

With respect to existing HG databases, SECAGN allows us to map recent events at a finer spatial resolution. Moreover, SECAGN is constantly updated based on a completely automated process, without the need for reporting from national or local authorities, insurance companies or single interested persons. This research uses the SECAGN dataset from January 01, 2010 to December 31, 2018 as input data. During the period of investigation, SECAGN recorded 8679 HG events (approximately 66% of which are landslides) hitting more than 2600 municipalities (33% of the total, accounting for approximately 50% of Italian territory). For each event, the following information is provided: type of event (flood or landslide), geographical localization at the municipal level, date of occurrence, and number of news from different sources connected to the same event.

In recent regional- and national-scale studies, the datasets derived by SECAGN have proven to be in accordance with observed reality (Segoni and Caleca 2021; Caleca et al., 2022; Franceschini et al., 2022). However, SECAGN suffers from some setbacks, which need to be properly accounted for during research development. In particular, it does not account for event intensity or for resulting impacts (e.g., damages or rebuilding costs). Given the high number of reported events, the SECAGN process might lead to an over-detection of relevant HG events. To address this issue, we turn to the number of news reported for each detected event as a potential measure of their severity. The main intuition is that a minor event is likely to generate news only on local websites, while major events are more likely to be reported by regional and national webpages as well. A potential shortcoming is that major cities are likely to be associated with wider mass media coverage, implying that a higher number of news might reflect the municipality relevance rather than the event severity *per se*. In light of these considerations, we proxy the event severity by the number of reported news, scaled by the population living in the hit municipality.[5]

A careful analysis of SECAGN news reveals that approximately 75% of the reported HG events recorded 3 news or less (2 for landslides and 5 for floods). Given the risk of misspecification, the disproportion between hit and non-hit municipalities, and considering that minor events are less likely to cause structural economic losses, we restrict our analysis only to the severe HG events, where an event is classified as "severe" when it belongs to the top decile of the distribution of the per capita number of news (computed separately for landslides and for floods). Table 1 reports the yearly distribution of total and severe (top 10%) events, while Fig. 1 reports their geographical distribution.

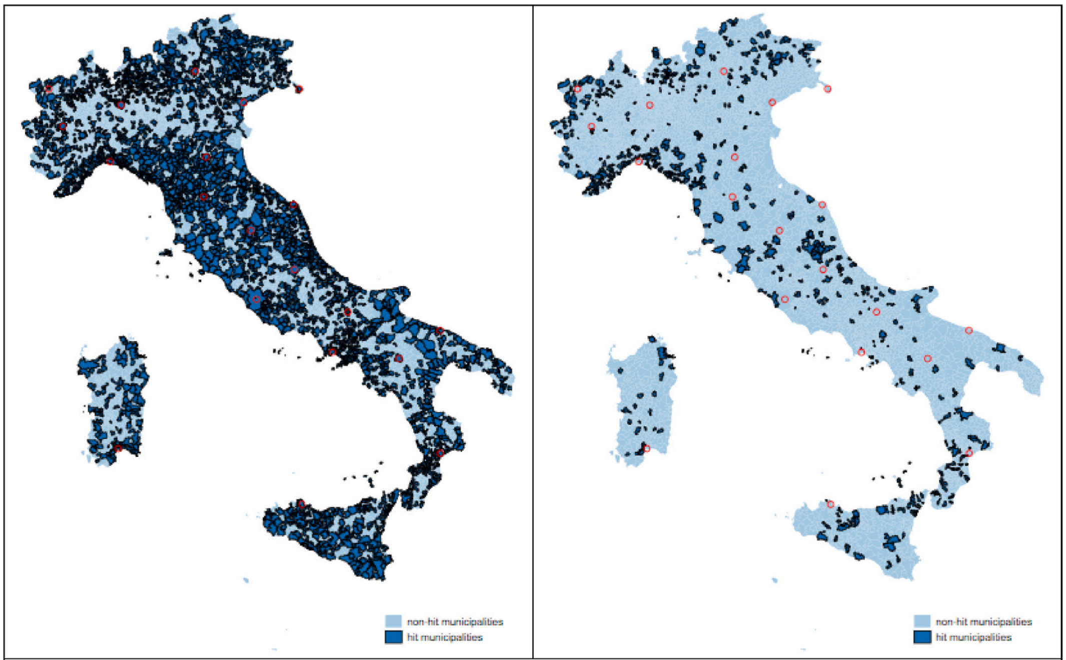
Next, we extract from the CERVED[6] registry yearly balance sheets' data and information on the universe of Italian incorporated non-financial firms,[7] including their NACE rev. 2 sector of activity, their geographical localization and year of incorporation. These data are merged with the number of people employed, which are released by the Italian National Social Security Institute (INPS). This allows us to estimate the firm-level Total Factor Productivity (TFP).[8] The firm-level dataset is augmented with some municipal-level data (population and degree of urbanization), which are published by the Italian National Statistical Institute (ISTAT). All data used and their sources are summarized in Table 2.

[5] The main analysis utilizes severe HG events, while the whole sample of events is used in the robustness checks.

[6] CERVED is an Italian provider of financial information about businesses.

[7] Incorporated firms are those that must fill a balance sheet every year. We exclude from the analysis firms in financial and insurance sectors (NACE codes between 64 and 66) and a small number of multi-plant firms (less than 100). The latter are identified through a complementary source to CERVED data, which is called CEBIL, a proprietor database that supplement balance sheet data with information collected by reading financial disclosures.

[8] The estimate is produced according to the method of Wooldridge (2009) and using the Stata routine *prodest*. See Appendix V.



Red circles indicate regional capital cities

Fig. 1. Italian municipalities hit by HG events (left panel) and severe HG events (number of per capita news in the top 10% of the distribution) during the 2010–2018 period.

Table 2
Main Variables and Data sources.

Variables	Years	Source
Floods and Landslides	2010–2018	SECAGN
Firms balance sheet	2010–2018	CERVED
Firms' employees	2010–2018	INPS
Total factor productivity	2010–2018	Own elaboration
Degree of urbanization	2010–2018	ISTAT
Population	2010–2018	ISTAT

Table 3 reports some relevant descriptive statistics on hit and non-hit municipalities and firms. Hit municipalities are smaller in terms of population (even if the difference with respect to other municipalities is not statistically significant) and have a lower share of cities and towns. Firms in hit municipalities are slightly younger, smaller (in terms of both revenues and employees), less productive and have a lower share of intangible over total assets. Moreover, the sectoral composition highlights some differences between the two groups, with higher shares of high-technology or knowledge intensive enterprises and industrial firms among the non-hit ones.[9]

Finally, firm demography appears to be more intense in non-hit municipalities (Fig. 2, panel a). Focusing only on firms existing at the beginning of our period, firm survival is slightly worse in hit municipalities (Fig. 2, panel b).

[9] Firms in hit and non-hit municipalities differ in several characteristics. Whether firms sort by ex-ante HG event risk is potentially an interesting and complementary research question. By exploiting information provided by the Italian Ministry of Agriculture we can test whether firms' characteristics correlate with municipality ex-ante HG risk. We have regressed a dummy variable indicating whether firm location is high-risk on a set of firm' variables, defining high-risk municipalities as those having a share of surface at high or very high HG risk above the median or the 75th percentile of the distribution. Apart from very small differences in age that emerge in some specification, only revenues seem to be negatively correlated with being in a high-risk location, but the coefficient magnitude is very small. Moreover, our empirical strategy aims at reducing potential endogeneity issues: descriptive statistics in appendix I shows how similar treated and control firms are in our estimation sample.

[10] The low/high tech definition is based on the Eurostat/OCSE classification: the high-tech sector encompasses both the high and medium-high manufacturing sectors and the knowledge intensive services. Medium-high manufacturing sectors are NACE codes 25.4, 27.1-27-3, 27.9, 28, 30.1–30.2, 30.4, 33, 20, 27.5, 29, 30.9, 27.4 while high-tech ones are 21, 26, 30.3 and 32.5. Knowledge intensive sectors are further classified in technology services (53, 58, 60–63 and 72), market services (50, 51, 68, 69–71, 73–74, 77–78, 80–82) and financial services (64, 65 and 66).

[11] We distinguish micro-small firms from medium and large ones. According to European Commission Recommendation of 6 May 2003, a microenterprise employs fewer than 10 persons and its turnover and/or annual balance sheet total does not exceed EUR 2 million; small enterprises are defined as those which employ fewer than 50 persons and whose annual turnover and/or annual balance sheet total does not exceed EUR 10 million.

Table 3
Descriptive statistics for firms located in hit and non-hit municipalities.

Variables	non-hit (a)	Hit (b)	difference (a) - (b)	SE
	Firms			
Age	12.747	12.312	0.435***	0.068
Total assets	4334.812	2573.536	1761.276	1178.930
Intangible/Total Assets	0.334	0.312	0.021***	0.002
Revenues	3421.290	2010.405	1410.885**	491.892
Employees	13.467	10.725	2.742*	1.352
TFP	15.571	14.111	1.459***	0.337
Share high-tech/knowledge intensive[10]	0.180	0.144	0.037***	0.002
Share industrial	0.192	0.150	0.042***	0.002
Share services	0.603	0.609	−0.007*	0.003
Share micro-small firms[11]	0.915	0.915	−0.000	0.002
N	575062	29137		
	Municipalities			
Population	7764.264	5661.661	2102.603	1786.909
Share of cities and towns	0.331	0.229	0.102***	0.020
N	7312	590		

***p < 0.01, **p < 0.05, *p < 0.1; Note: pre.treatment 2009 data. Hit municipalities are those experiencing a hydrogeological event with a number of associated news (per capita) in the top 10% of the distribution. The differences in column 3 are evaluated according to t tests on the equality of means.

4. Empirical strategy

4.1. Survival analysis

A survival analysis is adopted to assess whether the likelihood of firm survival is invariant to the occurrence of natural disasters (Cleves et al., 2016). Let T be a non-negative random variable denoting the time to a failure event,[12] with probability density function $f(t)$ and cumulative distribution function (cdf) $F(t) = \Pr(T \leq t)$. The survivor function $S(t)$ is the reverse of the cdf:

$$S(t) = 1 - F(t) = \Pr(T > t) \tag{1}$$

The survivor function reports the probability of surviving beyond time t, i.e., the probability that there is no failure event prior to t. The function is equal to one at $t = 0$ and then decreases toward zero as t goes to infinity. The density function can be obtained from $S(t)$:

$$f(t) = \frac{dF(t)}{dt} = \frac{d\{1 - S(t)\}}{dt} = -S^{'}(t) \tag{2}$$

The hazard function $h(t)$ is the instantaneous rate of failure, i.e., the (limiting) probability that the failure event occurs in a given time interval, conditional upon the subject having survived to the beginning of that interval, divided by the width of the interval:

$$h(t) = \lim_{\Delta t \to 0} \frac{Pr(t + \Delta t > T > t | T > t)}{\Delta t} = \frac{f(t)}{S(t)} \tag{3}$$

The hazard function can vary from zero (meaning no risk at all) to infinity (meaning the certainty of failure at that instant). In survival analysis, the hazard function is modeled as a function of time and some predictors:

$$h_i(t) = g(t, \beta_0 + \beta_x X_i) \tag{4}$$

The assumptions made on $g(.)$ determine the class of models used. In accordance with much of the literature on firm survival (Manjon-Antolin and Arauzo-Carod 2008; Okubo and Strobl, 2021), to measure whether natural disasters affect firm survival probability, we estimate $h(.)$ using the following semiparametric Cox proportional hazard regression model[13]

[12] The term "failure event" refers to the event we are interested in; it can be the time until a person finds an employment, or the time until a firm exit from the market.

[13] Proportional hazard (PH) models can be classified as parametric or semiparametric: parametric models are those making a specific assumption on the functional form for $h_o(t)$, examples are the exponential and the Weibull function, while semiparametric models don't make any assumption on $h_o(t)$ and leave it unspecified. The most popular semiparametric model is the Cox PH model.

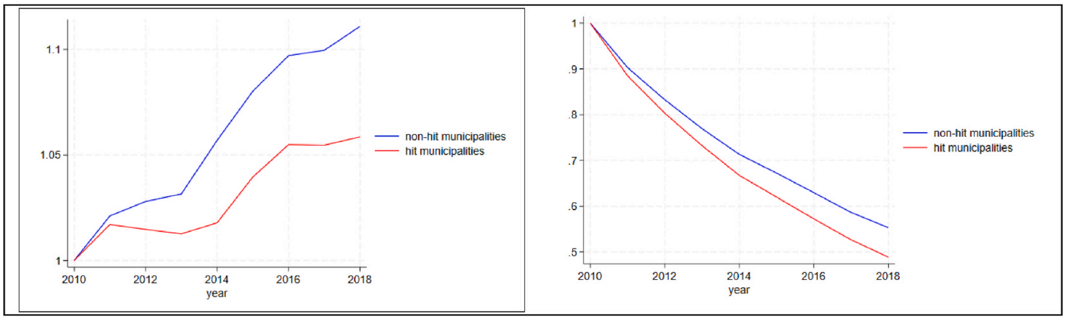


Fig. 2. Total number of firms (left panel) and firms exiting the market (right panel) (index 2010 = 1).
Note: Hit municipalities are those that have experienced a hydrogeological event in the 2010–2018 period, with a number of associated news (per capita) in the top 10% of the distribution.

$$h_i(t) = h_0(t)\ exp(\alpha + \beta POST_{it} + \gamma X_{it-1} + REG_i + YEAR_t + SECT_i + \varepsilon_{it}) \tag{5}$$

where $h_i(t)$ is the hazard of firm *i* exiting the market in year *t* after a natural disaster occurs.[14] $h_i(t)$ is assumed to be multiplicatively proportional to a baseline hazard faced by every firm $h_0(t)$, which represents the probability of failure conditional on the fact that the firm has survived until time t. The proportionality is assumed to depend on some firms' characteristics, which are parameterized in the $\exp(.)$ part of equation (5). Our main variable of interest is $POST_{it}$, which equals one after the municipality in which the firm is located is hit by a severe event. β is our main parameter of interest, which we expect to be positive, implying that the hazard increases with the occurrence of a severe event and the corresponding probability of survival declines. X_{it-1} is a vector of the firm's characteristics (age, revenues, people employed and total factor productivity) measured in $t-1$; in some specifications, the model is augmented for regional, year and sector-specific fixed effects ($REG_i, YEAR_t, SECT_i$). Standard errors are clustered at the municipality level.

To assess whether the impact is heterogeneous for different types of firms, we estimate our preferred model for subsamples of units sharing some important feature (class size, sector of activity, age, etc.).

In the robustness check section we test whether our baseline results are stable to variations in the model underlying assumption (by estimating a parametric version of it) and to different definitions of the threshold that identify sever events; we look for spillover effects and explore the impact of repeated HG events.

4.2. *Firms' performance*

The second part of the empirical analysis quantifies the impact of HG events on firms' performance, conditional on firms' survival. Natural disasters hit firms in different years over the 2010–2018 period, leading us to a staggered difference-in-differences design. For similar settings, previous research has usually adopted a two-way fixed effects (TWFE) estimator. However, recent advances in the econometric literature highlighted that the application of a standard difference-in-differences design to a setting with multiple and time-heterogeneous treatments is likely to raise some estimation bias (Goodman-Bacon, 2018; Abraham and Sun, 2018 Deshpande and Li, 2019; Callaway and Sant'Anna, 2018; de Chaisemartin and D'Haultfœuille, 2020).[15]

We address these potential biases by adopting an empirical strategy that allows us to causally assess the impact of an adverse event on a variable of interest by exploiting the heterogeneity in the treatment time (Deshpande and Yue, 2019; Fadlon and Nielsen 2021). First, treated units are categorized into cohorts depending on the year they first received the treatment (occurrence of a severe event). Then, we rearrange our data as follows. For each treatment year t (with t = 1, …, G) of the period 2013–2016,[16] we construct a subsample composed of (i) a treated group (firms located in the municipality hit by the severe event in that year *t*) and (ii) a control group (firms that in a given time span[17] do not change their treatment status). This implies that the control group is in principle composed of both never treated firms (never hit during the whole period of observation, 2010–2018) and firms receiving the treatment (being located in a hit municipality) in a year outside the window: notably, both early treated and late treated units (treatment year

[14] We classify a firm as exiting in year *t* if it does not exhibit a balance sheet from *t* onward. We exclude from the analysis a small set of firms (less than 100) that we know having more than one plant, since we are not able to map single establishments, and firms that move their head-quarter in the period of analysis, since those moves can in principle be related to natural disasters, but the survival analysis would be complicated by a so called "competing risk" (we left this for future analysis). We only focus on the exit side of the market since we have no microdata on firms that would potentially enter the market.

[15] In the case of multiple treatments, the "*TWFE regressions make both "clean" comparisons between treated, never treated and not-yet treated units as well as "forbidden comparisons" between units who are both already-treated*" and "*when treatment effects are heterogeneous, these "forbidden" comparisons potentially lead to severe drawbacks such as TWFE coefficients having the opposite sign of all individual-level treatment effects*" (Roth et al., 2022).

[16] We restrict the treatment period to the central part of our time span because we want a sufficient pre- and post-treatment period. We also decide to avoid pre-2013 events since we would not be able to identify which municipalities were treated before 2010.

[17] For each cohort *g* the time window considered goes from $(g - t_0)$ to $(g + 2)$

after *t+2*). To avoid potential biases, early treated firms are excluded from the control group, which therefore is composed of both never treated and not-yet treated units. Applying this approach recursively to each treatment year, we end up with G = 4 groups, one for each year t within the period 2013–2016, that we stack into a unique dataset.

Descriptive statistics on the samples used in the performance analysis are provided in Appendix I.

The stacked datasets is utilized to estimate the following regression:

$$ln(Y_{igt}) = \alpha_{ig} + \delta_{gt} + \beta_1 Post_{gt} + \beta_2 Treated_{ig} \times Post_{gt} + \varepsilon_{igt} \quad (6)$$

where Y_{igt} can be one of the following economic outcomes of interest for firm *i*, belonging to group *g*, in year *t*: revenues, number of employees, share of intangible assets (Intangible over Total Assets) and TFP. α_{ig} are firm-level fixed effects for firm *i* within group *g* (note that the same firm can appear as a control or a treated unit in different groups *g*), and δ_{gt} are time fixed effects referring to year *t* within group *g*. $Treated_{ig}$ is a dummy identifying treated firms in group *g* and $Post_{gt}$ identifies post-treatment years for firms in group *g*. β_2 is the coefficient of the interaction term, which captures the differential effect of the treatment on treated firm *i* compared to the control units in the post-treatment period. ε_{igt} are random standard errors clustered at the municipal level.

Our identifying assumption is similar to Callaway and Sant'Anna (2018) and Roth et al. (2022), as it rests upon a "parallel trends assumption for staggered settings": cohort *g* treated firms' performance would have evolved as in the control group in absence of a natural disaster. In the baseline model, the control group is made up by both never-treated and not-yet treated firms,[18] while in the robustness check, we relax the above assumption by excluding the never treated units from the control group, in line with Callaway and Sant'Anna (2018) approach (Parallel trends based on not-yet treated groups).

We further estimate regression (6) for subsamples of firms to assess whether the impact of natural disasters is heterogeneous along some firm-level or geographical characteristics: firm size, main sector of activity, firms' age and degree of technological intensity.

We also extend the baseline model by looking at its dynamic specification:

$$ln(Y_{igt}) = \alpha_{ig} + \delta_{gt} + \sum_{\tau} D^{\tau}_{gt} + \sum_{\tau} \beta_{\tau} \left(Treated_{ig} \times D^{\tau}_{gt} \right) + \varepsilon_{igt} \quad (7)$$

where D^{τ}_{gt} are indicators equal to 1 if year *t* is τ years before/after the year of treatment for group *g*. This dynamic specification allows us to investigate eventual pre-trends in the outcome variables and to observe how the impact of natural disasters evolves over time.

In the robustness check section we test the sensitivity of our results to different control groups (which may include only the "not-yet treated firms" or be selected through matching techniques) and to different definitions of the threshold that identify sever events; we look for spillover effect, explore the impact of repeated HG events and perform some placebo test. We also exploit the alternative estimator proposed by de Chaisemartin and d'Haultfoeuille (2020), that relies on a slightly different common trend assumption, stating that the evolution of the outcome without treatment would be the same over time in every group. Provided a "stable group" exists, meaning a set of units whose state (treated or untreated) does not change during an interval period, in a staggered setting the proposed estimator compares the evolution of the mean outcome between *t-1* and *t* of the "joiners" (i.e. those receiving the treatment) with those remaining untreated.

5. Results

5.1. Survival analysis

Table 4 reports the results from our Cox PH model (equation (5)). Column (3) includes geographical, year and sectorial fixed effects, while the last column includes geographical and time fixed effects and stratifies the baseline hazard according to sector of economic activity: in this way, the assumption that every firm faces the same baseline risk of failure is replaced by a baseline hazard that differs across sectors. In all the estimated specifications, being located in a municipality hit by a severe natural disaster increases the probability of exiting in a statistically significant way. According to our preferred specification of column 3 – and converting the estimated coefficient of interest to hazard ratio – firms in those locations face a hazard 7.3% greater than non-hit firms. Conditional on the estimated parameters and evaluating the baseline hazard rate at average values of the Xs, the hazard rate is slightly below 6% for the control group and increases by around 40 basis points for the treated one, a non-trivial increase.

If the sample is split according to firm characteristics (size, sector of activity and age), the impact of natural disasters is concentrated on smaller firms, in the manufacturing, construction and low-technology sectors and amongst younger firms (Table 5).

5.2. Performance analysis

Estimation of equation (6) shows that, conditional on survival, in the three years after the event, firms hit by severe events experience a statistically significant decline in revenues and employment by 4.9% and 2.2%, respectively, while their productivity is

[18] In the notation of assumption 4.a in Roth et al. (2022), the parallel trends assumption for staggered setting (on post-treatment only) is $E[Y_{i,t}(\infty) - Y_{i,t'}(\infty)|G_i = g] = E[Y_{i,t}(\infty) - Y_{i,t'}(\infty)|G_i = g']$, for all $t, t' \geq g_{min} - 1$, where g_{min} is the first period where a unit is treated and $Y_{i,t}(\infty)$ is the potential outcome of non-treated units.

not significantly affected (Table 6). Since survivor firms may in principle be more resilient with respect to firms in the control group, these estimates may be considered a lower bound of the impact. In addition, we found that adverse natural events are associated with an increase in the share of intangible assets. This evidence suggests that natural disasters cause a greater loss of tangible assets compared to intangible ones, as the former are more exposed to the risk of physical deterioration than the latter.

The dynamic impact of the treatment on our variables of interest is shown in Fig. 3. The post-treatment variation is significant at a 5% level only for revenues and the number of employees, which exhibit a declining trend. The same significance level is not met for the share of intangible assets. Moreover, in the pre-treatment period, none of the considered variables shows coefficients with statistically significant differences among the treated and the control groups. This evidence supports the pre-treatment parallel trend assumption that is required for the difference-in-differences analysis to provide unbiased estimates.

Note: For each variable of interest, the graph plots the estimated coefficient of the $Treated_{ig} \times D^{\tau}_{gt}$ variables of equation (7). The omitted period is t-6. Time and firm-level fixed effects are included. Robust SE, clustered at municipal level, with 95% confidence interval.

We further investigate some potential heterogeneous effects across firms (Table 7). The results show that natural disasters impact only micro and small enterprises, while medium-large enterprises are not significantly affected. At the sectoral level, firms in construction and services, as well as other activities (among which there is also a small number of agricultural firms) are impaired. This may be a consequence of their business typology, which is non-tradable and rests upon the physical accessibility of customers. Conversely, the impact on the manufacturing sector is slightly less significant on the revenue side. Younger and low-technology firms are more affected than older and high-tech ones.

6. Robustness checks

In this section we present and discuss a battery of analyses developed to test the robustness of our results to alternative identifications of the treated and control groups, alternative estimators or alternative definition of the treatment. These analyses allow to highlight and address some potential issues related to our baseline specification.

6.1. *Placebo test*

The dynamic analysis shows that, in the period preceding the treatment, treated units are not statistically different from the control units (Fig. 3). Also the results of the t-tests (table a1; Appendix I), show virtually no differences in pre-treatment observables across firms located in hit and non-hit municipalities, except for TFP which is ex-ante slightly higher among non-treated firms.

On top of this evidence, we developed the following placebo tests: we restricted our analysis to the pre-treatment period [t-6; t-1] and we assumed a hypothetical false treatment taking placing in t-4. This splits the pre-treatment period into a false pre-treatment [t-6; t-5] and into a false post-treatment period [t-4; t-1]. Results reported in Table 8 show that, for all the variables of interest, the coefficient of the interaction term is not statistically different from zero, implying that in the real pre-treatment period [t-4; t-1] treated units are not statistically different from control ones. The same conclusion holds when the false hypothetical treatment is assumed to take place in t-3 (Table a2; Appendix I). Together, these results provide further evidence supporting the parallel trend assumption that must hold for the staggered difference-in-differences design to provide unbiased estimated.

6.2. *Non-random distribution of natural events*

In this sub-section we show that our results hold when we apply more demanding conditions to address potential endogeneity bias that might arise from the potential non-random distribution of natural events or in the case of firms' endogenous localization choices.

First we replicate our baseline model removing the never treated municipalities from the control group. In this way, we mimic the Deshpande and Yue (2019) design, who estimated the effect of endogenous closings of Social Security Administration field offices on the number of disability recipients by exploiting the variation in the timing of closure, with treated units being compared only to not-yet treated ones. This approach also allows us to tackle possible biases related to a potential non-randomness of the treatment since it does not require the natural event to be an exogenous event but only its timing to be random.

Second, we adopt a conditional staggered difference-in-differences design to address potential self-selection biases stemming from firms' localization choices. In particular, we adopt a propensity score matching (PSM) technique to select from the control group a subgroup of firms that, before the treatment, was not statistically different from the treated one along a variety of observable dimensions (details of the PSM procedure are reported in Appendix II).

Our estimates show that our main findings are robust to these alternative specifications, thus highlighting that the results are not significantly affected by endogeneity issues (Table 9). When excluding never-treated units from the control group, the magnitude of the effect of natural disasters on revenues and on the share of intangible assets is higher compared to the baseline model, while no significant effect is detected for the number of employees. The results are also consistent when the PSM technique is used to define a smaller control group that is not statistically different from the treated group in the pre-treatment period. These outcomes prove once more the significantly negative effect of HG events on firms' revenues and employees.

Moreover, to mitigate the risk of our results being biased due to the omission of relevant localized time-varying covariates, we expanded our baseline model (equation (6)) by including time fixed effects respectively at a macro-area (North-East, North-West, Centre, South and Islands), and regional level, which are well suitable to capture the territorial heterogeneity of the Italian

Table 4
Impact of severe HG events on firm survival – Cox proportional hazard model.

	(1)	(2)	(3)	(4)
	Hazard rate	Hazard rate	Hazard rate	Hazard rate
Post	0.0818**	0.0811**	0.0701**	0.0701**
	(0.0370)	(0.0375)	(0.0340)	(0.0340)
Age		0.0278***	0.0182***	0.0184***
		(0.00526)	(0.00528)	(0.00514)
Revenues		−1.36e-05**	−2.39e-06	−2.31e-06
		(6.50e-06)	(1.51e-06)	(1.46e-06)
Employees		−0.00506***	−8.93e-05	−0.000108
		(0.00123)	(0.000130)	(0.000133)
TFP			−0.00519***	−0.00518***
			(0.00103)	(0.00103)
Observations	4,571,789	4,283,147	2,871,061	2,871,061
Region FE	NO	NO	YES	YES
Sector FE	NO	NO	YES	NO
Time FE	NO	NO	YES	YES

Robust standard errors, clustered at the municipal level, in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1. Note: In column (4) the baseline hazard rate is stratified by sector. Severe HG events are those with a number of associated news (per capita) in the top 10% of the distribution.

economy. Our main findings are confirmed also in this setting effects (Table a3; Appendix I).

6.3. Repeated events

In the baseline model, we defined the treatment period according to the first year of an event, without considering whether a geographic unit was treated more than once in the study period. However, while 486 municipalities were hit by a single severe event,

Table 5
Impact of natural disasters on firms' survival – Heterogeneity Analysis.

Subsamples	Hazard rate
	Firm size (a)
Micro-small firms	0.0573*
	(0.0335)
Observations	2,700,360
Medium-Large firms	0.0363
	(0.0900)
Observations	170,701
	Sector (b)
Manufacturing	0.107*
	(0.0633)
Observations	655,840
Construction	0.114***
	(0.0395)
Observations	428,665
Services & other activities	0.0499
	(0.0359)
Observations	1,786,556
	Firm's Age (c)
Old Firms (year of incorporation < median)	0.0657
	(0.0424)
Observations	1,516,889
Young Firms (year of incorporation > median)	0.0712**
	(0.0346)
Observations	1,354,172
	Technology Intensity (d)
Low-tech firms	0.0709**
	(0.0334)
Observations	2,440,261
High-tech firms	0.0300
	(0.0717)
Observations	430,800

Robust standard errors, clustered at the municipal level, in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1. The table reports the coefficients of the Post variable of equation (5). Time and firm-level fixed effects are included.

Table 6
Impact of natural disasters on firms.

	(1) Revenues	(2) Employees	(3) TFP	(4) Intangible/Total Assets
Post	−0.020***	0.022***	0.015***	−0.025**
	(0.005)	(0.001)	(0.001)	(0.012)
Treated x Post	−0.049***	−0.022***	−0.010	0.039*
	(0.014)	(0.007)	(0.006)	(0.021)
Constant	6.517***	1.759***	2.573***	−1.254***
	(0.003)	(0.002)	(0.001)	(0.003)
Observations	8,058,466	7,791,133	5,390,527	8,058,466

***p < 0.01,**p < 0.05,*p < 0.1. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included.

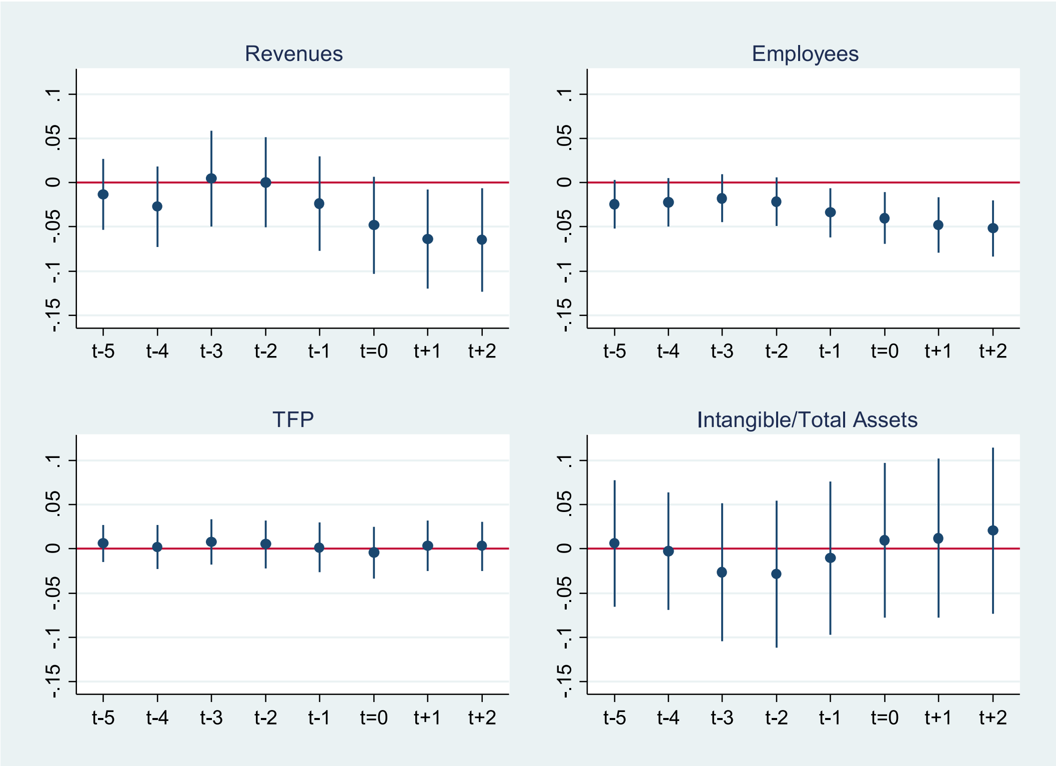


Fig. 3. Impact of natural disasters on firms performance: dynamic analysis.

104 municipalities where hit more than once over the considered period 2010–2018. Therefore, we are interested in verifying whether our results are driven by those firms which experienced multiple and repeated events. We first verified that our main results hold when we exclude those firms from treated group, which now includes only firms facing one single event (Table 10). In particular, in the post-treatment period, firms hit only once experience a statistically significant decline in revenues and employment by 4.4% and 1.8% respectively. Interestingly, the size of the estimated coefficients is lower compared to the baseline (−4.9% and −2.2%), suggesting that repeated events have an incremental impact on the exposed firms.

To further explore the impact of repeated events, the control group of untreated firms may not represent an appropriate counterfactual. Indeed, when focusing on firms hit over several years, the first event is likely to put the treated units on a diverging trajectory compared to the control units. In this case, the pre-parallel trend assumption is not likely to hold in the period between two consecutive events. To address this issue, we exclude the control (both never treated and not-yet treated) units from our sample and we re-build our treated and control groups in the following way: we consider a time window [t-n; t+3] composed by a pre-treatment period [t-n; t] and a post-treatment period [t; t+3]. We include in control group those firms located by municipalities which were hit only once in the year *n*, while we include in the treated group those firms located by municipalities which were hit both in the year *n* and in the year *t*. By construction, treated firms facing repeated events are compared to firms which experienced one single event. Both of them were exposed to an adverse event in the pre-treatment period, while only treated units are exposed to a second event in the post-treatment phase. Like in the previous cases, we apply this approach recursively to each treatment year within the period

Table 7
Impact of natural disasters on firms: Heterogeneity analysis.

Subsamples	Revenues	Employees	TFP	Intangible/Total Assets
	Firm size (a)			
Micro-small firms	−0.053***	−0.028***	−0.011	0.037*
	(0.014)	(0.008)	(0.007)	(0.021)
Observations	7,543,994	7,277,465	4,932,396	7,543,994
Medium-Large firms	0.007	−0.006	−0.008	0.083
	(0.038)	(0.022)	(0.017)	(0.116)
Observations	514,472	513,668	458,131	514,472
	Sector (b)			
Manufacturing	−0.031	−0.020	−0.015	0.023
	(0.021)	(0.017)	(0.009)	(0.042)
Observations	1,754,589	1,730,504	1,459,221	1,754,589
Construction	−0.079*	0.005	−0.019	0.057
	(0.041)	(0.014)	(0.019)	(0.041)
Observations	1,107,736	1,075,304	729,096	1,107,736
Services & other activities	−0.032***	−0.026***	−0.002	0.033
	(0.010)	(0.008)	(0.007)	(0.026)
Observations	5,196,141	4,985,325	3,202,210	5,196,141
	Firm's Age (c)			
Old Firms (year of incorp. < median)	−0.047**	−0.018*	0.001	0.032*
	(0.019)	(0.010)	(0.009)	(0.018)
Observations	4,032,535	3,936,530	2,697,560	4,032,535
Young Firms (year of incorp. > median)	−0.059***	−0.034***	−0.017**	0.031
	(0.016)	(0.009)	(0.007)	(0.024)
Observations	4,025,931	3,854,603	2,692,967	4,025,931
	Tech Intensity (d)			
Low-tech firms	−0.027**	−0.022***	−0.005	0.035*
	(0.011)	(0.007)	(0.006)	(0.018)
Observations	5,100,791	4,971,541	3,645,514	5,100,791
High-tech firms	−0.051***	−0.031**	−0.025	−0.001
	(0.018)	(0.014)	(0.016)	(0.026)
Observations	1,547,136	1,504,584	933,467	1,547,136

***$p < 0.01$,**$p < 0.05$,*$p < 0.1$. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included. Note: The table reports the coefficients of the Treated x Post variable of equation (6).

2013–2016 and then we stack each group into a unique dataset.

Results reported in Table 11 show that treated firms facing multiple events experienced a statistically significant decline in their employees and an increase in the share of intangible assets compared to the control group of firms experiencing only one event, while we do not find any significant difference in terms of revenues and TFP. On one side, the decline in the number of employees and changes in the asset composition suggest that repeated events may cause structural negative consequences on hit firms. On the other side, the lack of any significant difference in revenues among treated and control firms is consistent with the possibility that, after facing one event, firms implement some defensing strategies aimed at mitigating the negative effect of potential future events.

In a similar exercise for the survival analysis, we compare firms hit only once with untreated firms and with those subject to repeated events. We first show that our baseline is confirmed when we restrict our analysis to firms treated only once (Column 1, Table 12). Moreover, we show that, after a second event, repeatedly treated firms do not show an increase in exit probability with respect to firms treated only once (Column 2, Table 12). This suggests that firms repeatedly hit may learn or adapt to natural disasters and become more resilient.

Table 8
Placebo test with a false hypothetical treatment in *t-4*.

	(1)	(2)	(3)	(4)
	Revenues	Employees	TFP	Intangible/Total Assets
False Post	−0.020***	0.027***	0.009***	0.139***
	(0.004)	(0.004)	(0.002)	(0.004)
Treated x Flase Post	−0.009	−0.013	0.002	−0.029
	(0.015)	(0.009)	(0.006)	(0.022)
Constant	6.480***	1.674***	2.533***	−2.000***
	(0.002)	(0.003)	(0.002)	(0.002)
Observations	4,783,948	4,619,117	3,214,930	4,783,948
False Treatment	t-4	t-4	t-4	t-4
Time period	[t-6; t-1]	[t-6; t-1]	[t-6; t-1]	[t-6; t-1]

***$p < 0.01$,**$p < 0.05$,*$p < 0.1$. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included.

Table 9
Impact of natural disasters on firms: alternative specification of the Control group.

	(1) Revenues	(2) Employees	(3) TFP	(4) Intangible/Total Assets
	Control group: not yet treated units			
Treated x Post	−0.061***	−0.009	−0.011	0.056**
	(0.018)	(0.012)	(0.008)	(0.027)
Observations	105,633	100,637	68,728	105,633
	Control group selected through PSM technique			
Treated x Post	−0.037***	−0.013*	−0.003	0.003
	(0.014)	(0.007)	(0.005)	(0.023)
Observations	332,256	325,229	213,925	332,256

***p < 0.01,**p < 0.05,*p < 0.1. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included.

Table 10
Impact of natural disasters on firms: focus on unrepeated events.

	(1) Revenue	(2) Empl.	(3) TFP	(4) Intangible/Total Assets
Post	−0.020***	0.022***	0.015***	−0.038***
	(0.005)	(0.001)	(0.001)	(0.008)
Treated x Post	−0.044***	−0.018***	−0.011	0.026
	(0.013)	(0.006)	(0.007)	(0.018)
Constant	6.517***	1.759***	2.573***	−1.871***
	(0.003)	(0.002)	(0.001)	(0.002)
Obs.	8,050,840	7,783,766	5,385,597	8,050,840

***p < 0.01,**p < 0.05,*p < 0.1. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included. Note: firms facing repeated events are excluded from the treated group.

6.4. Spatial spillovers

Working on hundreds of municipal-level events, we are unable to identify for each one the precise perimeter of the affected areas. However, the event's indirect effect can potentially spread beyond the administrative boundaries of the treated municipalities, affecting firms located in adjacent municipalities included in the control group. If the effect of treatment crosses over borders, then the difference-in-differences design produces biased estimates. We address potential spillover biases in alternative ways. We first focus on first-order adjacent municipalities. For this purpose, we remove the treated units from the sample and compare the first-order contiguous municipalities with non-adjacent ones. Second, we remove the first-order contiguous municipalities from the control group and we compare treated units with non-adjacent ones.

Concerning the survival analysis, neighboring municipalities experience similar effects to the hit firms but with a smaller magnitude (Table 13, Column 2). A consistent outcome is that the direct negative impact increases when neighboring municipalities are excluded and twice that of adjacent neighboring ones (Table 13, Column 3).

Concerning the performance analysis, both specifications provide consistent evidence on the existence of negative spillover effects of natural disasters from the hit municipalities to the adjacent ones (Table 14). We first show that firms located in neighboring municipalities adjacent to the hit ones experience a decline in revenues and an increase in the share of intangible assets (compared to the control group), and the magnitude of this indirect effect is smaller compared to the direct effect on the firms located in the hit municipalities (baseline model). Conversely, no direct effect is found when looking at the number of employees. The second specification shows that the magnitude of the direct negative impact of natural disasters on firms' revenues is higher when neighboring municipalities are excluded from the control group, suggesting that firms located adjacent to hit municipalities may also be affected and slightly attenuate our baseline estimates.

This evidence brings us to question whether the spillover effects persist on larger spatial scales. In particular, we focus on the second-order spatial contiguity: those municipalities not directly contiguous to the treated ones, but which are contiguous to the first-order contiguous units. We thus remove treated units and first-order contiguous municipalities from the sample. The new treated group includes only second-order adjacent units which are now compared to the remaining non-adjacent units. Results reported in Table 15 point to the lack of any statistically significant difference in the post-treatment period between second-order adjacent units and other control units with respect to all the considered variables. These results suggest that the indirect effects of the natural events involve mainly the "first-order" adjacent municipalities, while they do not affect significantly those municipalities that are not directly bordering the treated ones.

In a further robustness check, we remove both the first-order and the second-order contiguous municipalities from the control group and we run again our baseline regression. Estimates indicate that the size of the negative effect of natural disasters further increases with the distance between treated and control units (Table a6; Appendix III). Finally, we assess spatial spillovers by

Table 11
Impact of natural disasters on firms: impact of repeated events.

	(1)	(2)	(3)	(4)
	Revenues	Employees	TFP	Intangible/Total Assets
Post	0.018	0.006	0.011	−0.018
	(0.031)	(0.035)	(0.011)	(0.029)
Treated x Post	0.033	−0.038***	−0.018	0.256***
	(0.035)	(0.013)	(0.012)	(0.023)
Constant	5.986***	1.519***	2.499***	−1.820***
	(0.027)	(0.007)	(0.009)	(0.028)
Observations	42,595	41,091	26,344	42,595

***p < 0.01,**p < 0.05,*p < 0.1. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included.
Note: firms facing repeated events are compared to firms hit only once.

Table 12
Impact of natural disasters on firms' survival: impact of unrepeated and repeated events.

	(1)	(2)
	Treated units without repeated events	repeatedly vs non-repeatedly treated
Post	0.101**	−0.0641
	(0.0396)	(0.0635)
Age	0.0178***	0.131**
	(0.00526)	(0.0625)
Revenues	−2.37e-06	−4.00e-06
	(1.48e-06)	(8.73e-06)
Employees	−8.33e-05	−4.99e-05
	(0.000126)	(0.000203)
TFP	−0.00517***	−0.0109***
	(0.00103)	(0.00244)
Observations	2,846,282	137,572

***p < 0.01, **p < 0.05, *p < 0.1. Robust standard errors, clustered at the municipality level, in parentheses. Time, Region and Sector fixed effects are included.

considering different control groups whose size varies depending on their distance – respectively 5, 20, 50 and 100 km – from the nearest treated municipalities. Consistently with our expectations, we found that both the statistical significance and the size of the coefficient capturing the post-treatment differential impact of the natural disasters among treated and control units increases with control group's distance from the treated one (Table a7; Appendix III).

By exploiting the first-order and second-order spatial proximity among municipalities we brought some evidence of spatial spillover effects, which however attenuate and lose relevance as the distance between treated and control units increases. Our results suggest that the indirect effects of the natural events involve mainly the first-order adjacent municipalities, while they do not affect significantly those municipalities that are not directly adjacent to the treated ones.

6.5. *Alternative events' identification strategies*

In this section we run some regressions that confirm the validity of our results under alternative strategies to identify the treatment. Concerning the performance analysis, we first check whether our results depend on the definition of severe events and on the decision to include those municipalities hit by a non-severe event in the control group. To address this issue, we consider all the municipalities which experienced one event as treated. Results reported in Table 16 show that the effect of natural disasters on firms' revenues is diluted when including non-severe events within the treated group, while no significant effect is found when looking at the other variables. These results suggest that non-severe HG events affect firms' performance as well, but less than severe ones. This result is consistent with our definition of severity.

We reply the same exercise for the survival analysis and show that our baseline results (Cox proportional hazard model) are broadly confirmed repeated including in the treated group all the events, irrespective of their severity (Table 17, Column 2). Our baseline result is robust also to a parametric proportional hazard model with an exponential specification (Table 17, Column 1).

For both the survival and the performance analysis, our main findings are robust to further alternative definitions of severity. We first use an absolute cutoff to the number of news articles to identify sever events. Next, we use of Google Search trends to classify the severe events (Appendix IV).

6.6. *Measurement errors*

In this section, we developed an analysis that exploits the heterogeneity in the municipalities' dimension to address potential measurement errors that can raise by the adoption of municipality-level treatments. Being unable to localize the precise perimeter of

Table 13
Impact of natural disasters on firms' survival: First-order spatial spillovers.

	(1)	(2)	(3)
	Baseline model	Treated Group: adjacent municipalities only	Control Group: adjacent municipalities excluded
Post	0.0701**	0.0336*	0.0882*
	(0.0340)	(0.0187)	(0.0483)
Age	0.0182***	0.0159***	0.0240***
	(0.00528)	(0.00486)	(0.00651)
Revenues	−2.39e-06	−2.36e-06	−3.95e-06*
	(1.51e-06)	(1.48e-06)	(2.31e-06)
Employees	−8.93e-05	−8.56e-05	5.99e-06
	(0.000130)	(0.000129)	(9.48e-05)
TFP	−0.00519***	−0.00515***	−0.00509***
	(0.00103)	(0.00103)	(0.00156)
Observations	2,871,061	2,848,445	1,862,652

***p < 0.01, **p < 0.05, *p < 0.1. Robust standard errors, clustered at the municipal level, in parentheses. Time, Region and Sector fixed effects are included.

Table 14
Impact of natural disasters on firms: First-order spatial spillovers.

	(1)	(2)	(3)	(4)
	Revenues	Employees	TFP	Intangible/Total Assets
	Treated Group: adjacent neighboring municipalities only			
TreatedxPost	−0.026***	−0.002	−0.008**	0.042**
	(0.008)	(0.005)	(0.004)	(0.017)
Observations	8,018,639	7,753,381	5,365,613	8,018,639
	Control Group: adjacent neighboring municipalities excluded			
TreatedxPost	−0.058***	−0.023***	−0.013**	0.052**
	(0.013)	(0.007)	(0.006)	(0.021)
Observations	5,348,138	5,179,890	3,653,308	5,348,138

***p < 0.01,**p < 0.05,*p < 0.1. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included.

Table 15
Impact of natural disasters on firms: Second-order spatial spillovers.

	(1)	(2)	(3)	(4)
	Revenues	Employees	TFP	Intangible/Total Assets
Post	−0.008**	0.026***	0.017***	−0.054***
	(0.004)	(0.002)	(0.002)	(0.004)
Treated x Post	−0.003	−0.005	−0.001	0.004
	(0.004)	(0.005)	(0.002)	(0.006)
Constant	6.611***	1.801***	2.574***	−1.929***
	(0.003)	(0.003)	(0.001)	(0.002)
Observations	5,308,311	5,142,138	3,628,394	5,308,311

***p < 0.01,**p < 0.05,*p < 0.1. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included.
Note: the Treated Group includes "second-order" contiguous municipalities, while the Control group includes non-adjacent treated units.

each disaster, we cannot unambiguously distinguish those firms located in areas that were directly involved in the natural disaster from those firms that, in spite of being located in the hit municipality, were not directly affected by the event. Being interested in both direct and indirect (or high-order) effects of hydrogeological events, we believe it reasonable to consider all firms located in the hit municipality as treated.[19] However, we can assess only an average effect which mediates the direct effects on companies directly involved in the disaster with the indirect effects on companies which, although not suffering a direct loss of physical capital, may suffer indirect losses due to their proximity with the affected area.

In light of this limitation, we develop an analysis aimed at lowering the risk of wrongly assigning the treatment status to firms that, in spite of being located in hit municipalities, were not affected directly by the natural disasters. Our intuition is that the risk of a

[19] As recognized by Kousky (2014), disasters cause economic losses which are not exclusive to firms that sustained direct physical damages. Business continuity can be affected by damages to suppliers, evacuation of workers or loss of electricity and water. Businesses may be affected by reduction of local demand via income effects (see also Johar et al., 2022).

Table 16
Impact of natural disasters on firms: Focus on non-severe events.

	(1)	(2)	(3)	(4)
	Revenues	Employees	TFP	Intangible/Total Assets
	Treated group: all events included			
TreatedxPost	−0.015***	−0.003	−0.004**	0.002
	(0.004)	(0.003)	(0.002)	(0.006)
Observations	3,359,697	3,243,956	2,383,651	3,359,697

***p < 0.01,**p < 0.05,*p < 0.1. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included.

wrongful assignment of the treated status increases with the dimension of the hit municipality. For instance, it is plausible to assume that a negative event in a big municipality as Rome, Milan or Naples will involve only a small share of the firms located in this city. Conversely, if a natural disaster affects a municipality with a limited geographical dimension, then it is more plausible to expect that those firms located within the administrative border of that municipality will be effectively affected by the event.

To test this hypothesis, we replicated our baseline model for different sub-samples, which vary depending on the considered municipality surface area.[20]Results reported in Table 18 are consistent with our expectations: the size of the estimated coefficient decreases for increasing dimensions of the municipal surface areas. Our interpretation is that, in case of smaller municipalities, the estimated coefficient would still capture a weighted average of direct and indirect effects but one in which the weight of the former is higher, while the opposite is true in case of bigger municipalities.

6.7. *Alternative estimator*

Finally, following recent advances in the econometric literature, we test the sensitivity of our results by using the new estimator proposed by de Chaisemartin and D'Haultfoeuille (2020), which relies on a slightly different common trend assumption, stating that the evolution of the outcome without treatment would be the same over time in every group. In particular de Chaisemartin and D'Haultfœuille (2020) set up an alternative approach to estimate the dynamic treatment effect by using estimators that are robust to arbitrary treatment effect heterogeneity and can provide a causal interpretation of the treatment effect.[21]

Table 19 shows that our main findings are robust to this alternative specification: the effects of disasters on firms revenues and employees are negative and statistically significant at least one and two years after the event,[22] and the coefficient magnitude is similar to our baseline specification. To provide evidence of the parallel trend assumption, the authors suggest placebo tests that compare the two group between t-2 and t-1 (hence before the switch). According to Table 19, there are no pre-trends among treated and control units.

7. Conclusions

Climate change is increasing the frequency and magnitude of hydrogeological disasters, demanding a heavy toll in casualties and economic losses worldwide. Italy is no exception, as it has long been exposed to floods and landslides, due to peculiar geological, geographical, and human factors. Using a novel dataset for Italy that maps HG events at the municipality level over the period 2010–2018, we adopt the perspective of firms and study the impact of a plurality of natural disasters on the survival and performance of (survivor) firms.

Our results show that firms in hit municipalities face on average a 7.3% increase in the probability of exiting the market with respect to non-impaired firms, with effects concentrated in smaller firms, on the manufacturing, construction and the low-technology sectors and on younger firms. Conditional on surviving, firms' performance is negatively affected by HG events, especially concerning revenues and, to a lesser extent, employment. After being exposed to a flood or landslide, revenues decrease on average by 4.9% and employment by 2.2% in our preferred specification. These effects are evident for micro and small businesses and for those active in the services and construction sectors. Severe events are also associated with an increase in the share of intangible assets over total assets, suggesting that they are less affected compared to tangible ones. We show that these results are robust to a variety of tests and analyses where alternative identification strategies, alternative estimators or alternative definition of the treatment are adopted to address some potential issues related to our baseline specification.

We believe that our results contribute to the existing literature is some interesting ways. First, rather than focusing on major

[20] we have grouped municipalities in different classes according to the following surface areas dimensions: 1) First class: surface area <6.1 squared kilometers (cumulative distribution: 10% of the sample); 2) Second Class: 6.1 squared kilometers < surface area <11.4 squared kilometers (cumulative distribution: 25% of the sample); 3) Third Class: 11.4 squared kilometers < surface area <22.2 squared kilometers (cumulative distribution: 50% of the sample); 4) Fourth Class: 22.2 squared kilometers < surface area <44 squared kilometers (cumulative distribution: 75% of the sample); 5) Fifth Class: surface area >44 squared kilometers (cumulative distribution: 100% of the sample).

[21] The empirical strategy amounts to compare changes in outcome for units whose status switches in period *t* with respect to *t-1* ("switchers") to those of non-switchers ones. The estimates are produced using the Stata routine difference-in-differences_multiplegt.

[22] As for employment, also the contemporaneous coefficient is statistically significant at the 95% level of confidence.

Table 17
Impact of natural disasters on firms' survival: Focus on non-severe events.

Variables	(1) Exponential PH model Severe events	(2) Cox PH model - all SECAGN events
Post	0.0675*	0.0845***
	(0.0350)	(0.0147)
Age	−0.0367***	0.0181***
	(0.000478)	(0.00528)
Revenues	−2.14e-06	−2.40e-06
	(1.40e-06)	(1.49e-06)
Employees	−8.36e-05	−9.94e-05
	(0.000121)	(0.000132)
TFP	−0.00518***	−0.00530***
	(0.00104)	(0.00102)
Observations	2,871,061	2,871,061

***p < 0.01, **p < 0.05, *p < 0.1. Robust standard errors, clustered at the municipal level, in parentheses. Time, Region and Sector fixed effects are included.

adverse events, our research considers the impact of less extreme, thought increasingly frequent and geographically widespread, events. To inspect this topic, we rely on a novel dataset on municipal-level events compiled through an automatized web scraping algorithm that collects and geotags internet news referring to landslides and floods. Data collected through an automatic procedure are less prone to self-reporting bias and allows a higher geographical accuracy compared to previous multi-hazard studies which usually rely on regional or even country level data. Moreover, we adopt a novel empirical strategy. While previous micro-econometric literature mainly relied on the traditional difference-in-differences design, building on recent econometric contributions, we assess the impact of multiple treatments by means of a staggered difference-in-differences design, which addresses potential biases associated with the heterogeneity of treatment effects.

Nevertheless, we are aware that our analysis presents some limitations that should be kept in mind when interpreting the related results. Dealing with hundreds of events, we cannot precisely trace the perimeter of the areas affected by each event. This implies that, a priori, we cannot exclude the possibility that the events have effects beyond the administrative boundaries of the municipalities. By exploiting the first-order and second-order spatial proximity among municipalities we bring some evidence of spatial spillover effects, which however tend to be confined to first-order adjacent municipalities and become less relevant as the distance between treated and control units increases. A related concern is that, within the treated municipalities, we cannot distinguish the directly damaged firms from those firms that, in spite of being located in the hit municipality, were not directly affected by the event. Therefore, our estimates should be interpreted as an average impact of HG events on both directly and non-directly hit firms.

Another limit of our analysis is that, working with hundreds of HG events and with the universe of Italian incorporated firms, we have no access to granular information on any form of private or public financial aids that were eventually disbursed to damaged firms. Due to its proneness to almost any kind of natural disasters, Italy has developed a system of immediate response to climate-related disaster, which is managed by a national governmental agency. While minor events are normally managed by local and regional authorities, which can implement recovery measures with a certain level of discretion consistently with the juridical subsidiarity principle, national intervention by civil protection agency is foreseen only for major severe and widespread events which cannot be managed by local entities with ordinary measures, and require the promulgation of extraordinary measures at a national level.

During the emergency, the government allocates national funds to the damaged regions, which can be used for any kind of action connected with the emergency, including first aid, assistance, recovery, restoration, preliminary economic refunds, intervention for residual risk reduction, survey of impacts and needs, delocalization of buildings and people settled in high-risk areas, and so on. Unfortunately, public available data do not allow to determine which firms received public financial support.

While from a social perspective, the aid represents simply a transfer among taxpayers which does not lower the overall impact of a disaster (Kousky 2014), these supporting measures are likely to mitigate the impact of natural disasters on firms' activity and can play a crucial role in the post-disaster recovery process. In spite of the importance of controlling for the financial aids, as underlined from various scholars, to the best of our knowledge, so far no one has provided causal evidence on that.[23] In light of this limit, our results

[23] According to Deryugina (2022): "*Finally, natural disasters almost always generate at least some aid response from governments, non-governmental organizations, and individuals (…) Unfortunately, there are no reliable estimates of the causal effects of disaster aid on victims' outcomes. Thus, while aid is certainly helpful, its exact effectiveness is currently unknown.*" Deryugina (2011) states that: "Unfortunately, annual county data on disaster spending over time is not available, so I cannot incorporate disaster spending into my main empirical framework". As for Hurricane Katrina in both Deryugina et al. (2018) and Gallagher and Hartley (2017) we found no more than aggregate data on financial aid (no granular data neither empirical evidence on their effect). Johar et al. (2022) interact their treatment variables with an indicator of severe disasters and, in finding no effect in those areas, they argue that a possible explanation is that "special financial support and services from governments and community organizations are concentrated in more sever disaster areas", but no data on aids are exploited in the analysis. In other cited papers, no information on aids are exploited in the empirical analysis. Basker and Miranda (2018) state that; "Unfortunately, we are unable to link information about loans to the LBD because the loans were issued in the name of the owner, not the business; moreover, many owners provided out-of-state addresses to the SBA".

Table 18
Impact of natural disasters on firms: Focus on alternative the municipal surface areas.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Revenues				Employees			
Post	−0.004	0.004	−0.007**	0.025***	−0.001	0.063***	0.040***	0.064***
	(0.008)	(0.006)	(0.003)	(0.003)	(0.004)	(0.005)	(0.003)	(0.002)
Treated x Post	−0.149*	−0.097*	−0.056**	−0.058***	−0.100***	−0.076***	−0.037**	−0.022***
	(0.081)	(0.051)	(0.029)	(0.016)	(0.037)	(0.025)	(0.017)	(0.006)
Constant	6.705***	6.697***	6.683***	6.611***	1.883***	1.805***	1.829***	1.785***
	(0.006)	(0.003)	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)
Obs.	293,222	763,095	1,774,803	3,096,978	285,572	742,482	1,725,244	3,004,745
Size Class	≤1	≤2	≤3	≤4	≤1	≤2	≤3	≤4

***$p < 0.01$,**$p < 0.05$,*$p < 0.1$. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included.

Table 19
De Chaisemartin e d'Haultfœuille (2020) estimator.

	Revenues					
	Estimate	SE	Lower Bound CI	Upper Bound CI	N	Switchers
Effect at t = 0	−0.018	0.009	−0.036	0	2718987	13798
Effect at t = 1	−0.035	0.012	−0.058	−0.012	2382562	11610
Effect at t = 2	−0.036	0.016	−0.067	−0.005	2026203	9851
Effect at t = 3	−0.029	0.018	−0.064	0.006	1672497	8521
Effect at t = 4	−0.006	0.017	−0.038	0.027	1320677	6282
Effect at t = 5	−0.005	0.023	−0.05	0.04	971183	4869
Effect at t = 6	0.041	0.032	−0.022	0.104	624406	2670
Placebo at t = 1	−0.002	0.011	−0.023	0.02	2382939	11987
Placebo at t = 2	0.002	0.008	−0.013	0.018	2027333	10981
Placebo at t = 3	0.011	0.01	−0.009	0.031	1672563	8587
Placebo at t = 4	0.002	0.009	−0.016	0.02	1321700	7305
Placebo at t = 5	0.003	0.008	−0.014	0.019	971247	4933
Placebo at t = 6	0.003	0.015	−0.026	0.033	625401	3665
	Employees					
	Estimate	SE	Lower Bound CI	Upper Bound CI	N	Switchers
Effect at t = 0	−0.010	0.004	−0.018	−0.002	2622457	13117
Effect at t = 1	−0.013	0.006	−0.025	−0.001	2299014	11017
Effect at t = 2	−0.024	0.009	−0.041	−0.007	1956411	9355
Effect at t = 3	−0.020	0.012	−0.042	0.003	1616268	8182
Effect at t = 4	−0.008	0.013	−0.034	0.017	1277628	6076
Effect at t = 5	−0.008	0.017	−0.042	0.025	939992	4702
Effect at t = 6	0.008	0.023	−0.038	0.054	604780	2573
Placebo at t = 1	−0.002	0.004	−0.010	0.006	2299363	11366
Placebo at t = 2	0.000	0.003	−0.006	0.007	1957463	10407
Placebo at t = 3	0.001	0.005	−0.010	0.012	1616213	8127
Placebo at t = 4	−0.004	0.005	−0.013	0.006	1278452	6900
Placebo at t = 5	−0.005	0.005	−0.014	0.005	939928	4638
Placebo at t = 6	−0.003	0.010	−0.022	0.016	605733	3526

should be interpreted as the effects of HG events net of any mitigating effect of any policy response, including the provision of private or public financial aids.

Although representing an obvious limit, we believe that in our case its severity should not be overstated. Given our interest in less extreme and striking events that are increasingly frequent due to climate change, our research analyzes a large number of localized events, and it is likely that most of them did not receive any form of public funding from the national government. Moreover, it shall be noted that government aids take time to materialize.[24] Since our analysis focus on the short-run effects of natural disasters (e.g. performance in the following three years) we believe aids could be a less severe confounding factor.

We finally believe that our research has some relevant policy implications. Effective action to mitigate climate change has been hindered by the temporal and geographical nature of its effects. Local policymakers might be reluctant to support during their term of office certain mitigation costs against uncertain long-term benefits, with a significant present value only at a very low discount rate. Moreover, the transboundary nature of climate change effects can induce free-riding behavior, limiting effective cooperation in the international arena. Our findings contribute to the literature, which is providing increasing evidence on the short-run and localized

[24] If we consider the recent major flood (May 2023) which affected around 30% of the Emilia-Romagna regional surface, by November 2023 households and firms did not have the possibility to ask for any refund.

negative economic effects of climate change. This evidence suggests the need to intensify land protection and maintenance policies. Indeed, contrasting the intensification of hydrogeological phenomena through prevention, mitigation and adaptation policies would produce tangible localized and short-term benefits.

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CRediT authorship contribution statement

Stefano Clò: Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Software, Supervision, Writing – original draft, Writing – review & editing. **Francesco David:** Conceptualization, Data curation, Formal analysis, Investigation, Methodology, Supervision, Writing – original draft, Writing – review & editing. **Samuele Segoni:** Conceptualization, Data curation, Writing – original draft, Writing – review & editing.

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Appendix I

Descriptive statistics of the "stacked" samples

For the performance analysis the samples used in the baseline and other models are constructed following the procedure described in the empirical strategy section and derived from Deshpande and Yue (2019). Here we provide descriptive statistics for those samples. We first compare treated and untreated units in the pre-treatment period, and then we compare treated and not-yet treated units (in the pre-treatment period). We focus on the main firm level variables. The results of the t-tests show that, after the construction of the final stacked sample, in the pre-treatment period treated and untreated firms only have a statistically significant difference in their TFP. Nevertheless, such a difference is no more significant when we compare treated and not-yet treated units.

Table a1
Stacked Final Sample. T-tests on the pre-treatment period characteristics for different sample types

	non-hit	hit	difference (a) - (b)	std error
	1. treated vs untreated - Full sample			
Intangible Assets	1409.889	470.286	939.604	1905.815
Total Assets	3018.104	1418.501	1599.603	2578.956
Revenues	5044.537	2426.474	2618.063	1896.171
Employees	17.531	13.634	3.896	4.561
TFP	17.879	16.057	1.822*	0.809
	2. treated vs not-yet treated - Full sample			
Intangible Assets	535.53	470.286	65.244	217.777
Total Assets	1582.581	1418.501	164.08	333.311
Revenues	3281.846	2426.474	855.372*	404.484
Employees	15.446	13.634	1.812	2.03
TFP	17.087	16.057	1.03	1.25

Note: in panel 1 the control group includes both never treated and not yet treated firms.

Placebo test

The placebo analysis is repeated assigning a placebo treatment in t-3; the absence of differences between treated and control units is confirmed.

Table a2
Placebo test: Staggered Difference-in-Difference with a false hypothetical treatment in *t-3*

	(1)	(2)	(3)	(4)
	Revenues	Employees	TFP	Intangible/Total Assets
False Post	0.020***	0.034***	0.009***	0.129***
	(0.005)	(0.004)	(0.002)	(0.004)
Treated x False Post	0.004	−0.010	0.002	−0.033
	(0.013)	(0.007)	(0.007)	(0.020)
Constant	6.470***	1.680***	2.538***	−1.997***
	(0.002)	(0.002)	(0.001)	(0.002)
Observations	4,783,948	4,619,117	3,214,930	4,783,948
False Treatment	t-3	t-3	t-3	t-3
Time period	[t-6; t-1]	[t-6; t-1]	[t-6; t-1]	[t-6; t-1]

***p < 0.01,**p < 0.05,*p < 0.1. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included.

Regional-time FE

Due to the heterogeneity in the Italian economy, there may be different economic trends across space. Hence we augment our baseline model on firms' performance with (alternatively) two set of time trends: at the macro-area (North, North-Centre, South-Centre, South and Islands), or at the regional level. The table reported below shows that our main findings are confirmed when we further control for regional-year fixed effects.

Table a3
Impact of natural disasters on firms with Macro-area and regional time FE

	(1)		(2)		(3)		(4)	
	Revenues		Employees		TFP		Revenues	
Post	−0.022***	−0.022***	−0.008***	−0.022***	−0.022***	−0.008***	−0.001**	−0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Treated x Post	−0.035**	−0.044**	−0.015**	−0.035**	−0.044**	−0.015**	0.016	0.014
	(0.016)	(0.018)	(0.007)	(0.016)	(0.018)	(0.007)	(0.015)	(0.013)
Constant	6.431***	6.427***	1.744***	6.431***	6.427***	1.744***	−1.878***	−1.891***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.004)
Observations	8,058,466	8,058,466	7,791,133	8,058,466	8,058,466	7,791,133	8,058,466	8,058,466
Regional Time FE	YES	NO	YES	NO	YES	NO	YES	NO
Macro-Area Time FE	NO	YES	NO	YES	NO	YES	NO	YES

***p < 0.01,**p < 0.05,*p < 0.1. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included.

Appendix II. Propensity Score Matching

We selected through a propensity score matching (PSM) a control group that, before the treatment, was not statistically different from the treated group with respect to a variety of observable dimensions.[25] Treated and untreated units were matched on the estimated propensity scores (on the estimated probability of being treated given a set of observable characteristics of the treated and control units). This process was developed recursively for each treated year.[26] We first estimated through a Logit model to what extent the probability of being treated was explained by the following covariates observed in the pre-treatment period: tangible and intangible assets, revenues, number of employees, regional localization, sector of activity, and size class. The results, reported in Table a4, show that the probability of being treated is positively related to tangibles and negatively related to the number of

[25] Rosenbaum and Rubin (1983) propose this method stating that propensity score refers to the conditional probability $P(Xi)$ that individual i enters the treatment group given a set of covariates (Xi). The procedure uses a Logit regression model, *Probit* and other probability models to estimate the propensity score. The idea is to find a control group that is as similar as possible to the treatment group to reduce selection bias and remove confounding bias of observed variables and other observable factors (Rosenbaum and Rubin, 1983). The PSM made the covariates of the treatment and control groups balanced and comparable to control the effect of the treatment.

[26] Hereby we report the results of the PSM for the year 2013, although the results are consistent also for the following treatment years. The results are available upon request.

employees, and the size of the estimated coefficients is non-neglectable.

Table a4
Propensity score estimates

	(1)
Tangibles	0.076***
	(0.013)
Intangibles	0.006
	(0.013)
Revenues	0.015
	(0.023)
Employees	−0.192***
	(0.036)
Constant	−6.964***
	(0.431)
Observations	305,452

Logit estimator. Dependent variable: treatment. Standard errors in parentheses. Regressors include geographical and sector dummies. The results refer to the PSM on the pre-treatment period.

Based on the estimated propensity scores, we matched each treated unit to a maximum of eight nearest neighbors non-treated units (in terms of estimated propensity score). Non-treated units lying out of the common support of the estimated propensity score were excluded from the analysis.

A preliminary visual inspection of the density distribution of the propensity scores in both groups before and after matching confirms the common support between the treatment and comparison groups and the goodness of the PSM procedure (see Figure A1).

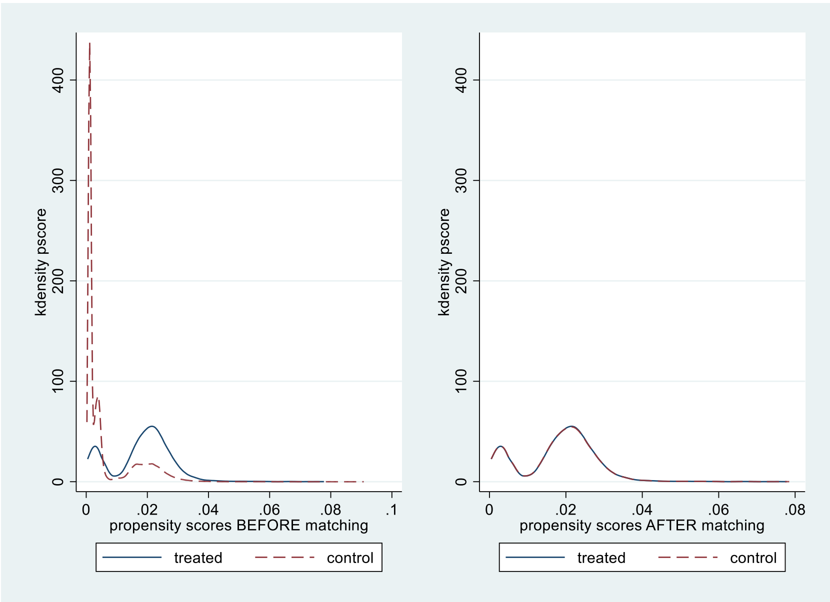

Fig. A1. Probability of receiving the treatment before and after matching.

In addition, we report the PSM balancing test. They show that, along several dimensions, the differences between treated and untreated units are significant only before the matching procedure. Conversely, matched treated and untreated units do not show any statistically significant difference, thus allowing us to reject the null hypothesis (Table a5).

Table a5
Balance test

	—	Mean		t-test	
		Treated	Control	T	p > |t|
Tangibles	U	4.1295	4.1182	0.220	0.826
	M	4.1295	4.1415	−0.160	0.870
Intangibles	U	2.5413	2.7864	−5.010	0.000
	M	2.5413	2.5416	0.000	0.997
Revenues	U	6.0032	6.4103	−10.460	0.000
	M	6.0032	6.0053	−0.040	0.969
Employees	U	1.4692	1.6371	−6.130	0.000
	M	1.4692	1.4778	−0.240	0.810

Appendix III. Spillover effects

Table a6
Impact of natural disasters on firms: first-order and second-order contiguous municipalities excluded from the Control Group

	(1)	(2)	(3)	(4)
	Revenues	Employees	TFP	Intangible/Total Assets
Post	−0.006	0.024***	0.019***	−0.053***
	(0.004)	(0.003)	(0.002)	(0.007)
Treated x Post	−0.055***	−0.019***	−0.012**	0.035**
	(0.014)	(0.007)	(0.006)	(0.016)
Constant	6.524***	1.786***	2.530***	−1.971***
	(0.003)	(0.002)	(0.002)	(0.004)
Observations	1,884,152	1,817,666	1,304,140	1,884,152

***p < 0.01,**p < 0.05,*p < 0.1. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included.

Table a7
Impact of natural disasters on firms: distance from the nearest treated group

control group: distance from the nearest treated group (km)	<5	<20	<50	<100
	Intangible/Total Assets			
Treated x Post	0.019	0.012	0.024	0.031**
	(0.014)	(0.016)	(0.015)	(0.016)
Observations	701,456	2,280,483	5,118,099	7,194,060
	Revenues			
Treated x Post	−0.031**	−0.037**	−0.049***	−0.050***
	(0.014)	(0.016)	(0.015)	(0.014)
Observations	701,456	2,280,483	5,118,099	7,194,060
	Employees			
Treated x Post	−0.023***	−0.025***	−0.026***	−0.025***
	(0.007)	(0.008)	(0.008)	(0.008)
Observations	675,811	2,200,775	4,952,810	6,960,265
	TFP			
Treated x Post	0.004	0.001	−0.003	−0.003
	(0.005)	(0.006)	(0.005)	(0.005)
Observations	451,949	1,489,910	3,423,491	4,816,172

***p < 0.01,**p < 0.05,*p < 0.1. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included.

Appendix IV

Absolute threshold

As an alternative strategy to identify severe events we resort to an absolute cutoff of the number of news articles (instead of that in per capita terms). In the following tables we replicate our baseline results by adopting two different thresholds: 2 or 5 news articles. All the results are qualitatively very similar to our baseline. The size of the estimated impact increases when we adopt a stricter absolute cutoff – 5 article-news – to identify the treatment.

Table a9
Impact of natural disasters on firms' survival: absolute cutoff as measure of event severity

	(1)	(2)
	Treated group: absolute cutoff = 2	Treated group: absolute cutoff = 5
Post	0.0898***	0.0932***
	(0.0149)	(0.0157)
Age	0.0180***	0.0180***
	(0.00528)	(0.00529)
Revenues	−2.40e-06	−2.41e-06
	(1.49e-06)	(1.49e-06)
Employees	−0.000102	−0.000102
	(0.000132)	(0.000132)
TFP	−0.00533***	−0.00535***
	(0.00102)	(0.00102)
Observations	2,871,061	2,871,061
Region Sector Time FE	YES	YES

***p < 0.01, **p < 0.05, *p < 0.1. Robust standard errors, clustered at the municipality level, in parentheses.

Table a10
Impact of natural disasters on firms: absolute cutoff as measure of event severity

	(1)	(2)	(3)	(4)
	Intangible/Total Assets	Revenues	Employees	TFP
	2-news absolute cutoff			
Treated x Post	0.003	−0.024***	−0.003	−0.005***
	(0.007)	(0.005)	(0.003)	(0.002)
Observations	3,904,216	3,904,216	3,768,805	2,759,733
	5-news absolute cutoff			
Treated x Post	0.018**	−0.033***	−0.007*	−0.005**
	(0.008)	(0.007)	(0.004)	(0.002)
Observations	4,358,390	4,358,390	4,206,467	3,066,328

***p < 0.01,**p < 0.05,*p < 0.1. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included.

Google trends

To further check the robustness of our results, we adopted the Google Search trends (e.g., how many people searched for a localized HG event) as an alternative measure of severity. For each Italian municipality, we collected monthly data on the amount of google search for several keywords over the same-period: flood, landslide, inundation, natural disaster. Unfortunately, this procedure provides missing data for several small municipalities. In this case, missing municipal data were replaced by the same information collected at the corresponding provincial level. We classify as severe those events identified by the SECAGN that belonged to the top decile distribution of the google search data. Our previous findings on the negative impact on widespread natural disasters on firms' survival and their economic activity is largely confirmed when we identify severe events using an alternative strategy. In this latter case, however, both the size and the statistically significance of coefficient decrease when we focus on the firms' number of employees.

Table a11
Impact of events identified through Google Search Trend on firms – Survival analysis

VARIABLES	(1)	(2)
	baseline	Treated group: google trend
Post	0.0701**	0.0645***
	(0.0340)	(0.0208)
Age	0.0182***	0.0181***
	(0.00528)	(0.00528)
Revenues	−2.39e-06	−2.41e-06
	(1.51e-06)	(1.50e-06)
Employees	−8.93e-05	−9.68e-05
	(0.000130)	(0.000131)
TFP	−0.00519***	−0.00529***
	(0.00103)	(0.00101)
Observations	2,871,061	2,871,061
Region, Sector and Time FE	YES	YES

***p < 0.01, **p < 0.05, *p < 0.1. Robust standard errors, clustered at the municipality level, in parentheses.

Table a12
Impact of severe events identified through Google Search Trend on firms – Staggered difference-in-differences

	Revenues	Employees	TFP	Intangible/Total Assets
Post	0.022***	0.064***	0.042***	−0.073***
	(0.003)	(0.002)	(0.001)	(0.005)
Treated x Post	−0.031***	−0.005	−0.007***	0.004
	(0.007)	(0.004)	(0.002)	(0.008)
Constant	6.502***	1.747***	2.494***	−2.041***
	(0.001)	(0.001)	(0.001)	(0.002)
Observations	4,359,215	4,208,910	3,056,125	4,359,215

***p < 0.01,**p < 0.05,*p < 0.1. Robust SE clustered at the municipal level in parentheses. Time and firm-level FE included.

Appendix V

Methodological Note on TFP Estimation

We assume a Cobb-Douglas production function:

$$Y_{it} = A_{it} * K_{it}^{\beta_K} * L_{it}^{\beta_L} * M_{it}^{\beta_M} \tag{1a}$$

where the output of firm i at time t, Y_{it}, depends on the quantity of capital K_{it}, labor L_{it}, and intermediate inputs M_{it}. It also includes a multiplicative factor A_{it} expressing the efficiency with which the factors are combined in the company's production processes. A_{it} represents the so-called Total Factor Productivity or "TFP."

The Cobb-Douglas production function can also be expressed in logarithms:

$$\ln(Y_{it}) = \ln(A_{it}) + \ln\left(K_{it}^{\beta_K}\right) + \ln\left(L_{it}^{\beta_L}\right) + \ln\left(M_{it}^{\beta_M}\right) = \ln(A_{it}) + \beta_K \ln(K_{it}) + \beta_L \ln(L_{it}) + \beta_M \ln(M_{it}) \tag{2a}$$

The value of production Y_{it} is expressed in terms of net revenues obtained from balance sheet data, deflated with the production deflator implicit in Istat's sector-level data; the labor input L_{it} is measured using the number of employees from INPS (the Italian Social Security Institute); the value of intermediate goods M_{it} is approximated by the sum of balance sheet data related to net purchases of goods and services, deflated with the production deflator from Istat. The capital stock K_{it} is reconstructed using the perpetual inventory method.

The parameters of equation (2) are estimated using the Stata command "prodest." The estimation is conducted over the period from 2002 to the most recent data, at the Ateco 2007 section level, using Wooldridge's (2009) method with a second-degree polynomial. This method efficiently accounts for the simultaneity between TFP realization and the choice of production inputs.

From parameter estimation, companies with values of $\ln\left(\frac{K_{it}}{Y_{it}}\right)$, $\ln\left(\frac{M_{it}}{Y_{it}}\right)$ or $\ln\left(\frac{L_{it}}{Y_{it}}\right)$ below the 5th percentile or above the 95th percentile of their respective distributions are excluded.

Given the estimated parameters $\widehat{\beta_K}$, $\widehat{\beta_L}$ e $\widehat{\beta_M}$, TFP is calculated for all companies as the exponentiated difference between the logarithm of output and its estimated value:

$$\widehat{A_{it}} = e^{\ln(Y_{it}) - \ln(\hat{Y}_{it})} = e^{\ln(Y_{it}) - \hat{\beta_K}\ln(K_{it}) - \hat{\beta_L}\ln(L_{it}) - \hat{\beta_M}\ln(M_{it})} \tag{3a}$$

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