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Weathering the Storm? The Effects of Natural Disasters on Households under Universal Insurance*

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Abstract

We study the indirect economic consequences of natural disasters for households using high-quality household-level data from Norway. Universal natural disaster insurance in this setting fully compensates direct physical damages, allowing us to isolate indirect effects. Linking a municipality-level measure of disaster severity to detailed transaction data measuring household expenditures, and using a matched difference-in-differences design, we find large and persistent effects on consumption: the cumulative spending fall up to four years after an event amounts to as much as 45 percent of the direct damages. Population-wide administrative records on income, wealth, housing transactions, and labor-market outcomes allow us to uncover the underlying mechanisms. First, labor income falls while unemployment increases after disasters, particularly when damages are concentrated among firms, consistent with disruptions to local labor markets. Second, housing wealth declines persistently, and homeowners cut spending more than renters despite similar income losses. Our results show that the household costs of natural disasters extend beyond the insured value of destroyed property, as households remain exposed to losses transmitted through local labor and housing markets.

JEL classification: D14, D31, E21, Q54
Keywords: Natural Disasters, Climate Risk, Disaster Insurance, Household Finance, Household Consumption

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1 Introduction

Natural disasters and extreme weather events are increasingly salient economic risks. The consequences for households extend beyond the immediate destruction of property. Disasters can disrupt firms, infrastructure, housing markets, and local amenities, with consequences that spill over to households through labor markets, asset prices, mobility, and spending. Understanding these broader consequences is important for assessing the aggregate and distributional costs of disasters and for designing policies that strengthen financial resilience.

Following Hallegatte (2015) and Botzen, Deschenes, and Sanders (2019), we distinguish between two conceptually different effects of natural disasters. Direct effects are damages to physical assets caused by the event itself. Indirect effects are higher-order losses arising from the broader local repercussions of the disaster, such as disruptions to firms, infrastructure, local amenities, housing markets, and labor demand. This paper studies the indirect effects of natural disasters on household consumption: whether spending falls even when direct physical damages are insured, and through which channels such effects arise.

Consumption is a natural outcome for assessing household welfare and adjustment, because it captures how households ultimately absorb shocks to income, wealth, and local economic conditions. Yet relatively little is known about how household consumption responds to natural disasters, largely because high-quality household spending data are rarely available. Existing studies have therefore often focused on income, employment, migration, housing markets, or aggregate local outcomes. Even when spending is observed, interpretation is difficult: if households are uninsured or only partially insured, measured consumption may combine indirect effects with the direct financial burden of repairing or replacing damaged property. In some cases, spending may even rise mechanically because households pay for reconstruction themselves. As a result, observed consumption responses depend not only on the local economic consequences of the disaster, but also on the insurance environment.

Focusing on Norway allows us to overcome these challenges. First, direct damages from natural disasters are in practice fully compensated. Natural disaster coverage is automatically bundled with standard fire insurance, which is included in property insurance policies held by virtually all Norwegian households. Damages are covered at full replacement value, and deductibles are small. As a result, subsequent changes in household consumption, income, and wealth can be interpreted as reflecting the indirect consequences of disasters rather than uninsured repair costs. Second, observed insurance claims provide a precise measure of direct physical damages at the municipality level, eliminating the need for proxies or survey-based damage assessments. We combine these insurance data with household-level administrative records on income, wealth, housing transactions, labor-market outcomes, and demographics, as well as detailed electronic transaction data that provide a high-coverage measure of household spending in a largely cashless economy (Ahn, Galaasen, and Maehlum, 2024).[1] The resulting dataset allows us to benchmark household spending responses against the direct damages caused by each disaster and to study the channels through which insured disasters affect household consumption.

To identify these effects, we treat the most severe weather events as natural experiments. We construct a systematic classification of such events using municipality-level insurance claims. For each municipality and year, we measure disaster severity as total insurance payouts related to natural damages divided by local labor income. Because the measure is based on realized insurance payouts, it reflects the economic magnitude of damages in each local area rather than meteorological intensity alone. Our main analysis defines a natural disaster as a municipality-year in which payouts exceed five percent of local labor income. This produces a set of large realized shocks, while still capturing events that are more frequent and less extreme than the single-disaster episodes studied in much of the existing literature.[2] Because these shocks are local rather than national in scope, they leave many otherwise comparable municipalities

[1]We use "consumption" as shorthand for consumption expenditures measured from the transaction data. Similarly to most measures of consumption used in the literature, our data measure spending flows rather than the flow of consumption services. This distinction matters especially for durable goods, where lower spending may reflect postponed purchases (see Section 4).

[2]Our methodology is similar to Roth Tran and Wilson (2025) and Boustan, Kahn, Rhode, and Yanguas (2020), but differs from studies that focus on individual large events, such as Hurricane Katrina in 2005 (Deryugina, Kawano, and Levitt, 2018) or the 2004 Indian Ocean Tsunami (Frankenberg, Sumantri, and Thomas, 2023).

unaffected in the same years. This feature is central to our identification strategy, as it allows us to compare households in affected municipalities with similar households in unaffected areas. It also shapes the interpretation of our estimates: they capture the household consequences of local disaster shocks, mostly abstracting from national general-equilibrium effects that may arise after very large events, such as changes in fiscal policy, commodity prices, or monetary policy. At the same time, localized disasters are themselves an important margin of climate risk: climate projections point to more frequent and intense extreme weather events that affect particular local areas (IPCC, 2023).

We link the identified events to household-level administrative and transaction data for the period 2006–2018. We define treated households as those living in a municipality affected by a natural disaster at the time of the event, rather than only households that file an insurance claim. This reflects our focus on the indirect effects of disasters: disruptions to local labor markets, housing markets, infrastructure, and amenities may affect all households in the exposed area, not only those whose own property is physically damaged. For each treated household, we construct a matched comparison group of never-treated households, defined as households that have never lived in a municipality exposed to a natural disaster during the sample period. The matching procedure combines exact and interval matching on pre-disaster household and municipality characteristics, including home ownership, financial positions, income, consumption, age, education, family structure, and municipality size. We also exclude potential controls from the same county as the treated municipality, to limit contamination from spatial spillovers. The resulting control group is therefore similar to affected households along key pre-disaster characteristics, while remaining outside the local area affected by the disaster.

We estimate the effects of disasters using a matched event-study difference-in-differences design. Because disasters occur in different municipalities in different years, treatment timing is staggered. However, identification comes from comparisons between treated households and matched never-treated controls assigned the same event year, rather than from comparisons between earlier- and later-treated cohorts. The event-study specification traces the difference between treated and matched control households in each year relative to the disaster, normalized to the year immediately before the event. This allows us to test for differential pre-trends and to estimate the dynamic response of consumption and other household outcomes after the disaster.

We find that natural disasters lead to a large and persistent decline in household consumption. Affected households reduce cumulative spending by about $1,460 over the four years following the disaster, corresponding to 32% of the average direct damages caused by the event. Because some households receive insurance transfers directly and may use them to repair or replace damaged property, our baseline consumption measure may partly include insurance-financed repair spending. We therefore also construct an adjusted consumption measure that subtracts excess insurance transfers received by treated households relative to controls. Using this measure, the cumulative consumption decline amounts to 45% of direct damages. There is little evidence of recovery for at least three years after the disaster, underscoring lasting economic consequences that extend well beyond direct physical damages. Furthermore, using the detailed consumption categories in the transaction data, we show that households reduce spending especially on more adjustable categories. Durable consumption falls more than non-durable consumption, and non-essential spending falls more than essential spending. This pattern suggests that households respond to the disaster by actively adjusting their consumption basket in order to potentially smooth the welfare impact of the disasters.

Why does consumption fall so much when direct damages are almost fully insured? We argue that disasters affect households through two indirect channels. First, disposable income falls after the disaster, driven by lower labor income. The income response is concentrated in events where direct damages are borne primarily by firms, and these events are accompanied by a short-run increase in unemployment. This points to a labor-market channel: disasters disrupt firms and local production, reducing household earnings. However, applying a standard marginal propensity to consume out of an income shock implies that lower income can explain only around one fifth of the consumption decline.[3] Second, disasters reduce household wealth, with the

[3] An MPC of around 0.4 is within the range of estimates for relatively persistent shocks such as unemployment, as explained in Section 4.

decline concentrated almost entirely in housing wealth. Such losses may arise even when rebuilding costs are insured, because disasters can update perceptions of future risk and reduce demand for housing in affected areas (Gallagher, 2014; Gibson and Mullins, 2020; Iversen and Aanesen, 2026). Homeowners, who are directly exposed to local housing-price declines, reduce consumption substantially more than renters, even though the income response is similar across the two groups. Homeowners also reduce debt and become less likely to purchase new housing, while renters are more likely to move. A simple accounting exercise shows that the remaining consumption decline is consistent with standard literature estimates of marginal propensities to consume out of housing wealth of about $3-5$ percent.

The labor-market and housing-market channels are likely to reinforce each other. Disruptions to firms and local production may reduce household earnings directly, but may also weaken the attractiveness of affected areas, lowering housing demand and property values. Consistent with this interpretation, housing-wealth losses are larger following events in which damages are concentrated among firms. Thus, the indirect effects of natural disasters propagate through interconnected local labor and housing markets.

Our results show that comprehensive insurance against direct physical damages does not fully protect households from the economic consequences of natural disasters. Although damaged property and other physical assets are largely compensated, insurance does not cover reductions in labor income, declines in local housing wealth, or the broader weakening of local economic conditions. As emphasized in the household finance literature, liquidity constraints, precautionary saving motives, and borrowing limits can prevent households from fully smoothing consumption in response to adverse shocks (Blundell, Pistaferri, and Preston, 2008; Carroll, 1997). Consistent with this, we find persistent consumption declines even in a setting where direct damages are almost fully insured. The welfare costs of natural disasters therefore extend beyond the insured value of destroyed property.

The remainder of the paper is organized as follows. Section 1.1 relates the paper to the literature, and Section 1.2 describes the institutional setting, including Norway's universal natural perils insurance scheme. Section 2 presents the data sources. Section 3 outlines the empirical strategy and research design. Section 4 reports our estimates for how natural disasters affect household consumption spending, and in Section 5 we explain how the large fall in spending is consistent with weaker labor markets and housing markets. Section 6 concludes.

1.1 Related literature

This paper relates to a large and growing body of literature studying the economic impact of natural disasters on the economy (for reviews, see Botzen, Deschenes, and Sanders, 2019; Cavallo and Noy, 2010; Klomp and Valckx, 2014). Our contribution to this literature is threefold. First and foremost, our setting allows us to provide clean estimates of the indirect effects of natural disasters. Since insurance provides full compensation for the direct damages caused by a disaster, any change in consumption, income, wealth, or other variables observed after the disaster is an indirect consequence of the event. We find that natural disasters lead to a decrease in economic activity three years after the event, suggesting there is limited to no recovery after a natural disaster.[4]

Second, we rely on detailed administrative individual-level data that allow us to follow households affected by heterogeneous natural disasters over time for a large number of disasters. Due to data availability, previous studies analyzing the economic effects of natural disasters either relied on administrative data to follow individuals after a single large event (Deryugina, Kawano, and Levitt, 2018; Gallagher, 2014), or used county-level aggregated data as outcome variables to analyze the impact of disasters of different magnitudes (Anttila-Hughes and Hsiang, 2013; Boustan et al., 2020). Our

[4]Hsiang and Jina (2014) present four competing hypotheses about the long-term impact of natural disasters on economic output. The "creative destruction" hypothesis suggests that disasters may temporarily boost economic growth through increased demand for goods and services, international aid, and innovation. The "build back better" hypothesis posits that while initial growth may suffer due to the loss of lives and capital, the replacement of outdated assets with modern units can lead to long-term growth. The "recovery to trend" hypothesis argues that growth should initially decline but eventually rebound to pre-disaster levels. Finally, the "no recovery" hypothesis asserts that disasters permanently lower economic growth by destroying productive capital and durable goods. Our results are consistent with the no recovery hypothesis, in levels if not in growth rates.

access to detailed administrative data over several natural events allows us to exploit heterogeneity across disasters and household characteristics to explore which events are most economically damaging, and which segments of the population are more severely affected. This helps us identify the mechanisms driving our results. In addition, our access to the insurance payouts allows us to construct precise estimates of the economic magnitudes of each disaster in a municipality. Most of the studies in this literature rely on cost estimates of the damages provided by the local authorities (such as those provided by the EM-DAT, FEMA or SHELDUS databases), which can be biased for political reasons (e.g., to access emergency funds, see Botzen et al., 2019; Garrett and Sobel, 2003).

Third, through the detailed Norwegian administrative data we are able to analyze several outcome variables that to the best of our knowledge have not been explored previously in the literature. This includes sub-components of income such as labor income and self-employment income, housing wealth, housing transactions, and within-county relocations. These variables allow us to shed new light on the mechanisms through which households weather the effects of a natural disaster. For instance, we find that in the wake of a natural disaster, unemployment increases temporarily when the damages are concentrated among firms, suggesting that some of the indirect effects are due to disruptions in the local labor markets. We also build on the results by Boustan et al. (2020), who find that severe disasters increase county out-migration rates in affected counties, by showing that fully insured individuals tend to relocate within the affected municipality after a natural disaster. In related and complementary work, Kivedal (2023) relies on the same insurance payments data as we do to show that some types of natural disasters depress regional house prices. We complement his study by showing that these local housing-market effects translate into lower household housing wealth, reduced housing purchases, and weaker consumption among homeowners. This links our results to the broader literature on housing wealth effects and household spending (Aastveit, Böjeryd, Gulbrandsen, Juelsrud, and Roszbach, 2025; Guren, McKay, Nakamura, and Steinsson, 2021).

Of particular interest is our access to population-wide electronic transaction data, which provide a high-coverage measure of household consumption expenditures over the period 2006–2018. Because Norway was already a largely cashless economy during these years, the data capture a broad range of household spending and allow us to trace consumption dynamics before and after multiple disaster events at the household level. Previous studies of household consumption responses to natural disasters have typically relied on survey-based expenditure measures (Bui, Dungey, Nguyen, and Pham, 2014; Sawada and Shimizutani, 2008). Relative to this literature, our data therefore provide substantially broader coverage and a longer panel dimension. We find large and persistent declines in household spending even in a setting where direct physical damages are almost fully insured.

The granularity of the transaction data also allows us to study how households adjust the composition of spending after a disaster. This contributes to a related literature showing that environmental shocks can alter household demand across categories. For example, Benmir, Jaccard, and Vermandel (2021) propose a model in which environmental externalities increase households' willingness to consume certain goods, while empirical studies find that pollution and climate-related risks raise spending on defensive goods such as electricity, air conditioning, air purifiers, and medicine (e.g. Abel, Holloway, Harkey, Meier, Ahl, Limaye, and Patz, 2018; Deschenes, Greenstone, and Shapiro, 2017; Ito and Zhang, 2020). We complement this literature by separating durable from non-durable spending and essential from non-essential expenditures. We find that the decline in spending is concentrated in categories that are more adjustable and easier to postpone, especially durable and non-essential consumption.

Our setting also speaks to a growing literature on disaster insurance, household financial resilience, and credit markets. In settings with incomplete or risk-based insurance coverage, disaster exposure affects households through insurance take-up, the cost and availability of coverage, mortgage performance and credit constraints (Boomhower, Fowlie, Gellman, and Plantinga, 2024; Del Valle, Scharlemann, and Shore, 2024; Ge, Johnson, and Tzur-Ilan, 2025; Keys and Mulder, 2024; Kousky, Palim, and Pan, 2020; Sastry, Scharlemann, Sen, and Tenekedjieva, 2025). Related macroeconomic evidence shows that higher insurance coverage is associated with smaller adverse effects of climate-related catastrophes (Giuzio, Kapadia, Kumar, Mazzotta, Parker, Rousová, and Zafeiris, 2025). We contribute to this literature by studying a complementary benchmark: a setting with near-universal and comprehensive coverage, where direct damages are insured but households remain exposed to indirect losses through income and housing wealth.

Our paper also contributes to the broader literature that estimates the impact of temperature fluctuations, extreme weather, and natural disasters on economic growth and other aggregate outcomes. Several papers in this literature estimate mild medium-term effects of increases in the global temperature, as well as minimal or even positive effects on countries at high latitudes such as Norway (see e.g. Burke, Hsiang, and Miguel, 2015; Dell, Jones, and Olken, 2012, 2014; Kahn, Mohaddes, Ng, Pesaran, Raissi, and Yang, 2021; Nath, Ramey, and Klenow, 2024, and the references therein). More recent studies have revised these estimates upward, showing that the impact is generally negative and can be several times larger than previously thought (Bilal and Känzig, 2024; Neal, 2023). Related work studies how extreme weather and climate conditions affect regional activity and inflation (Beirne, Dafermos, Kriwoluzky, Renzhi, Volz, and Wittich, 2024; Costa and Hooley, 2025; Ehlers, Frost, Madeira, and Shim, 2025; Kotz, Kuik, Lis, and Nickel, 2024; Nguyen, Feng, and Garcia-Escribano, 2025; Usman, Fernández, and Parker, 2025). To the extent that natural disasters will become more frequent with higher temperatures, our estimates are consistent with this more adverse interpretation, showing that even with full insurance and at high latitudes, the indirect economic consequences of natural disasters are negative overall.

1.2 The Norwegian insurance scheme

In Norway, any physical asset insured against fire damage (such as real estate and movable property) is also automatically covered for natural damage. Fire insurance is included in standard property insurance, which is held by the vast majority of homeowners.[5] Coverage is also comprehensive: in our sample, deductibles amount to only about 2% of the direct damages due to natural disasters (see Section 3.4). This near-universal coverage stands in stark contrast to many other countries. In the United States, for example, property insurance is generally required only for mortgaged properties, and a considerably smaller share of households hold active policies.

The natural damage insurance scheme is administered by the Norwegian Natural Perils Pool (Norsk Naturskadepool, or NASK), which all companies providing fire insurance are required to join. When a natural disaster occurs, each member company pays compensation to its policyholders and subsequently settles its claims through NASK, in proportion to its market share. Since its introduction in 1980, the program has undergone minimal changes, making the data consistent and comparable over time (Finance Norway, 2024). Natural damage is defined in Section 4 of the Natural Damage Compensation Act as damage directly caused by a natural disaster, such as flood, landslide, storm, storm surge, earthquake or volcanic eruption.

Premiums are uniform across the country, regardless of geographical location or exposure to natural perils risk. The rate is set as a per mille charge on the insured fire value, currently set to 0.08 (in principle updated annually, but around this level throughout our sample period). This uniform pricing reflects the principle of solidarity –a core aspect of the scheme since its inception– which ensures that the risk associated with natural damages is distributed among all residents of Norway. By contrast, in many other countries, natural disaster insurance must be purchased separately and may be unavailable or prohibitively expensive in high-risk areas (see e.g. Sastry et al., 2026; Keys and Mulder, 2024).

Coverage extends not only to households but also to firms insuring property or other objects against fire damage. For companies, insured items such as machines, tank

[5] For example, in 2025 the total number of insurance policies for real estate objects (houses and cabins) reported by Finance Norway was 1,744,450, and the number of properties of the same type reported by Eiendomsverdi AS was 1,749,352, implying that virtually all properties of these kinds are insured. This pattern is also reflected in other types of insurance. For instance, the number of contents insurance policies reported by Finance Norway was 2,574,377, while the total number of property units reported by Eiendomsverdi AS, including apartments in cooperatives and joint ownerships, was 2,600,326. These figures further illustrate that insurance coverage is close to universal among Norwegian households.

farms, or other similar assets are also automatically insured against natural damage if they are insured against fire, subject to some exceptions. Neither households nor firms, however, are insured under this scheme for losses on motor vehicles or boats. In such cases, damages may be covered by regular insurance. If no such coverage exists, households and firms may apply for compensation through the government's natural damage compensation scheme, which covers objects such as agricultural and forestry land, roads, bridges, and concrete quays (Norwegian Agriculture Agency, 2023).

In addition to natural disasters as defined above, weather-related water damages may also occur. These damages typically result from extreme weather events such as heavy rainfall and are most prevalent in urban areas where drainage capacity is limited. Finance Norway includes these damages in its climate reports as part of the broader category of extreme weather events. Unlike natural perils covered by the natural disaster insurance scheme, such weather-related water damages are covered by the water damage insurance component included in standard property insurance policies, for which insurance companies are allowed to vary premiums.

2 Data

We rely on several comprehensive and detailed data sources to analyze the economic impact of natural disasters on Norwegian households. These include insurance payouts from Finance Norway, supplemented with qualitative information gathered from The Norwegian Water Resources and Energy Directorate (NVE), The Norwegian Meteorological Institute (MET), and local newspapers. We complement these data with Norwegian administrative records from Statistics Norway, providing extensive demographic, income, and labor market data at the individual level for every resident of Norway. Finally, electronic transaction data from Nets Branch Norway, the Norwegian retail clearing institution, offer granular insights into household consumption. Combining these datasets enables us to conduct a detailed analysis of household economic outcomes in response to natural disasters.

2.1 Insurance payout data

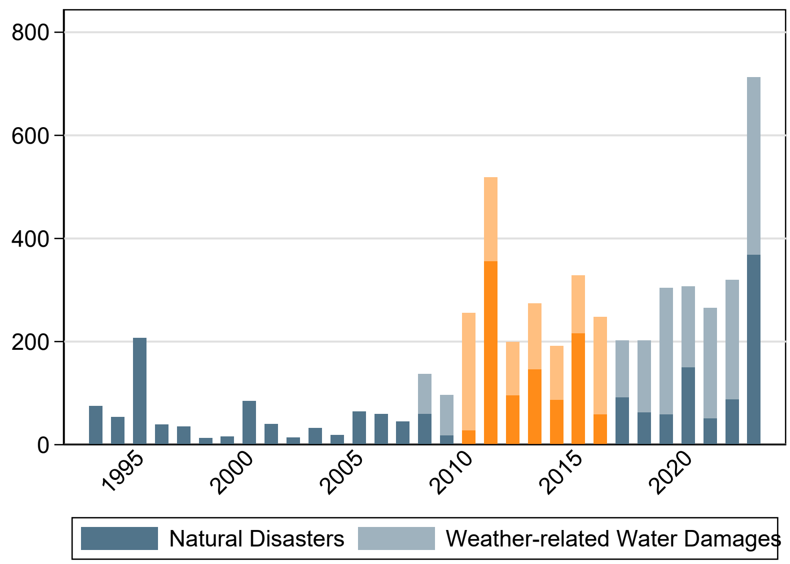
This dataset, provided by Finance Norway (the main industry and employers' association for the financial sector in Norway), contains information on each insurance claim related to natural disasters and extreme weather events in Norway between 1993 and 2023. We use data on insurance payouts due to natural damages from NASK, and insurance payouts due to weather-related water damages from The Water Damage Statistics (VASK). From these datasets we obtain the date and municipality of each claim, along with the total compensation amount paid by the insurance company (including both compensations paid and provisions for reported damages). We also obtain the cause of the incident (storm, storm surge, flood, landslide, heavy rain, or other). Finally, the datasets distinguish between insurance policies covering households and those covering commercial activities. This allows us to determine which sectors of the economy were most affected by each event.

Figure 1 shows total insurance payouts in Norway from 1993 to 2023. The figure shows that payouts have increased over time, with 2011 (with several major floods) and 2023 (with the extreme weather "Hans") standing out as particularly severe years. This trend has also been highlighted by other sources, e.g., Finance Norway (2024).

2.2 Norwegian administrative records

We access detailed information on individuals' wealth, income, and their demographic information from Statistics Norway. The data cover the entire population of Norway aged 16 and over for the period 2005-2018. Demographic information includes the individuals' age, gender, education, place of residence, and family status. Income and wealth data are based on financial reporting from assets and liabilities of each household, as reported to the Norwegian Tax Authority for tax assessments, and thus are highly reliable. Income variables correspond to the cumulative total over a calendar year and comprise several income categories, including labor income, capital income, income from self-employment, pensions, and all government transfers, as well as taxes paid. Wealth variables correspond to the balance sheet positions as of the beginning of each fiscal year, and they are available for several asset classes, including liquid assets

Figure 1: Insurance payouts from natural disasters and extreme weather.



Note: This figure shows the total amount of insurance payouts covering damages due to natural disasters in Norway each year from 1993 to 2023, stated in 2018 USD, millions. Dark shading shows payouts for natural disasters covered under the natural perils insurance scheme, while light shading shows weather-related water damages covered by regular water damage insurance. The orange bars correspond to the years covered by our ultimate sample with household-level outcome variables. Data are aggregated from individual records of insurance claims related to natural damages from The Norwegian Natural Perils Pool (NASK) and weather-related water damages from The Water Damage Statistics (VASK). Data have been provided by Finance Norway. Amounts correspond to the total compensation paid plus provisions for reported damages.

(deposits, cash, listed and non-listed stocks, and mutual funds), debt, and housing wealth. Housing wealth is measured by Statistics Norway based on characteristics of individual units matched to sales in the local area.[6] The main component of liquid assets is bank deposits. Debt primarily includes mortgage debt, but also other debt obligations including car loans, consumer debt, and student loans. We aggregate individual data to the household level using information on the composition of households, also provided by Statistics Norway. For research purposes each individual is anonymized and assigned a unique identification number that allows us to link the data to information on households' consumption, obtained from electronic transaction data, as described below. In our study, all our wealth and income variables in levels are reported in 2018 US dollars

[6]To improve comparability over time and bring tax-reported values closer to market values, we adjust housing wealth using municipality-year adjustment factors based on Fagereng et al. (2020). Section A.1 describes the procedure in further detail.

(USD).

2.3 Electronic transaction data

We collect information on household expenditures from electronic transactions for the years 2006 to 2018 (Ahn et al., 2024). The data are provided by the Norwegian retail clearing institution, Nets Branch Norway, and cover two main payment types. First, we observe debit card payments processed through BankAxept, the domestic Norwegian debit-card system. During our sample period, BankAxept processed almost all debit-card payments made in domestic physical terminals, while debit-card payments abroad, online, or through mobile platforms were typically processed by international card networks. Debit-card payments through BankAxept account for the majority of card payments in Norway during the sample period, and coverage rises to around 80% of total card payment value when we include imputed credit-card payments. Second, we observe bank wire transfers cleared through the Norwegian Interbank Clearing System (NICS), which captures most invoice payments.

The data are aggregated at the person-week-postal-code-category level and classify expenditures into 24 consumption categories. For the purposes of this paper, we aggregate the data to the household-year level, while in Section 4.2 we also use information on the composition of spending. Cash payments are unlikely to be an important omitted component in our setting: Norway is among the least cash-intensive economies, and survey evidence indicates that cash accounted for only a small share of point-of-sale payments by value during our sample period. Consistent with the broad coverage of the data, aggregate growth in our transaction-based expenditure measure closely tracks household consumption growth in the national accounts (Ahn et al., 2024). There is no indication of systematic bias in coverage across geography or household characteristics. Furthermore, since treated households are matched to never-treated controls on pre-disaster consumption, income, wealth, age, education, family structure, home ownership, and municipality size, comparisons are made between households with similar pre-event spending levels and characteristics.

3 Research Design

To assess the indirect economic impacts of natural disasters on Norwegian households, we employ a difference-in-differences approach combined with coarsened matching. Because our object of interest is local exposure to a natural disaster, rather than only direct damage to a household's own property, we define treated households as all residents of municipalities experiencing a natural disaster. This definition is deliberate: indirect effects may operate through local labor markets, housing markets, infrastructure, amenities, and local economic activity, and may therefore affect households throughout the exposed area, including those whose own property is not physically damaged. The near-universal insurance coverage of direct damages further allows us to interpret subsequent household responses as reflecting these indirect effects, rather than uncompensated repair costs. We identify affected municipalities using a severity metric for natural damages, described below.

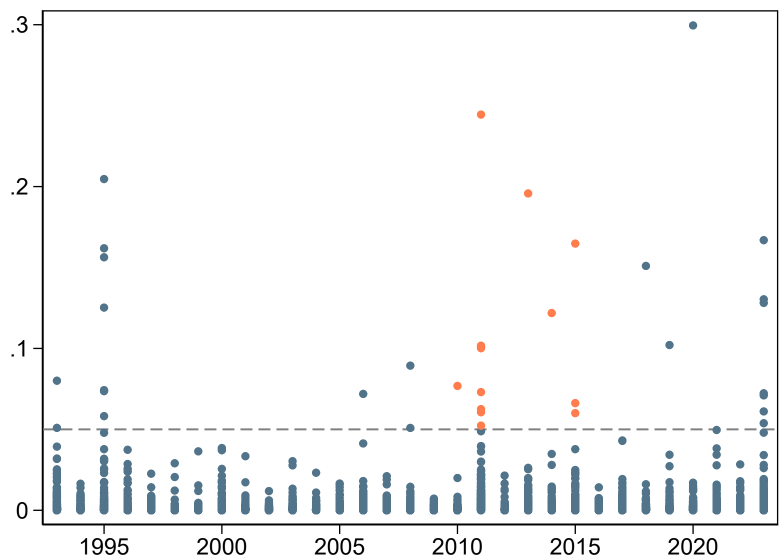
While our event study focuses on the period 2006–2018 due to administrative data availability (as discussed in Section 2.3), we classify all natural disaster events over a 30-year period from 1993–2023 using our proposed severity metric. Extending the classification over a longer horizon allows us to place recent events in historical context and to document how the frequency and intensity of natural disasters have evolved over time.

3.1 Identifying natural disasters

We measure the severity of natural disasters at the municipality-year level as total insurance payouts for natural damages divided by total labor income in the municipality. This ratio captures the size of the event's economic impact relative to the local economy, allowing us to compare events across municipalities of different sizes. The denominator is a broad measure of labor income, including wages, net business income, sickness benefits, and parental benefits. Scaling by labor income also accounts for changes over time in the nominal size of the local economy.

We define a natural disaster as a municipality-year in which total insurance payouts exceed five percent of labor income. As shown in Figure 2, the five percent threshold defines events that are relatively rare. The figure shows the distribution of the insurance to labor income share for all municipalities across the years, with the horizontal dotted line at the five percent threshold. Only 38 municipality-year events, corresponding to 0.34 percent of all observations between 1993 and 2023, exceed this threshold. The number of municipalities affected by natural disasters according to our definition is reduced to 16 in our sample period; these events are represented by the orange dots in the figure.

Figure 2: Insurance payouts in municipality as share of labor income.



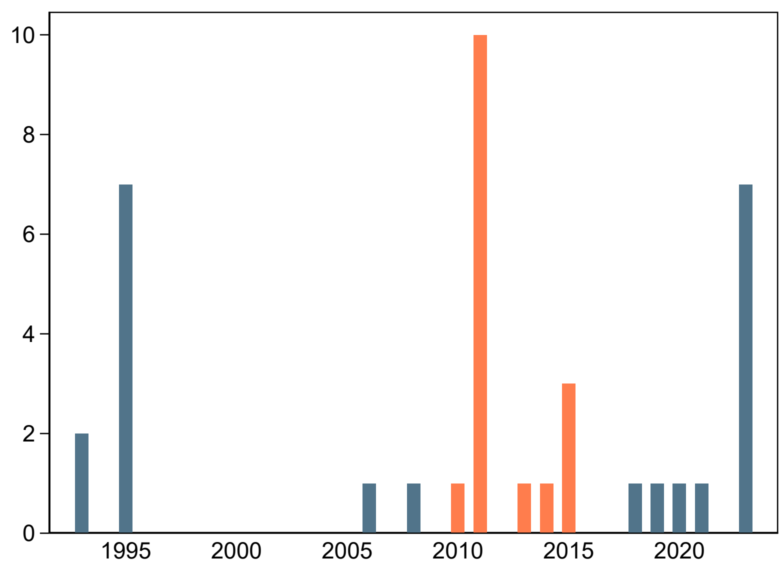
Note: This figure shows a scatterplot of the ratio of total insurance payouts to total labor income in each Norwegian municipality and year, for the period 1993–2023. Each dot in the scatterplot represents an observation for one municipality in a given year. Dots in orange color indicate the natural disaster events that are included in our sample. The horizontal dashed line at the 5% level indicates the threshold above which an event is classified as a natural disaster. The data sources are individual records of insurance claims related to natural damages from The Norwegian Natural Perils Pool (NASK) and from weather-related water damages from The Water Damage Statistics (VASK). Labor income data are provided by Statistics Norway (SSB).

To ensure the reliability of our classification, we manually verify that the events identified by our severity metric are widely recognized as natural disasters. To that end, we rely on qualitative information on natural disasters from The Norwegian Water

Resources and Energy Directorate (NVE), extreme weather warnings from The Norwegian Meteorological Institute (MET) and information from local newspapers. In Section A.2, we show that insurance payouts in an affected municipality spiked around the date identified in these sources as the date of the natural disaster. The additional information also makes it possible to classify the type of natural disaster that is associated with each event.

Table 1 contains a list of all natural disasters identified with our procedure during the period 1993–2023; Figure 3 contains the number of events per year and Figure 4 depicts their geographic location. In the table, the events listed in blue are the ones that make it to our final sample. In the figures, events in orange correspond to those in our sample period.

Figure 3: Number of natural disaster events, 1993–2023.



Note: This figure summarizes the number of natural disasters occurring in each year in Norway during years 1993–2023. Natural disasters are defined as those municipality events where total insurance payouts exceed 5 percent of local labor income. Bars in orange color indicate the natural disaster events that are included in our sample with household-level outcome variables.

Several things are worth noting about the events identified using our methodology. First, as shown in Figure 3, the events are spread out over time; however, they have become somewhat more frequent in recent years. This pattern is in line with the overall trend in insurance payouts over this period shown in Figure 1. Second, natural disasters

Table 1: **Natural disasters classified using our proposed severity metric.**

	Municipality	Year	Date	Payouts to labor income	Verified natural disaster type
1	Gjerdrum	2020	Dec 30th	29.9	Landslide, quick clay slide
2	Holtålen	2011	Aug 16th	24.8	Flood, 200-year flood
3	Stor-Elvdal	1995	June 1st	20.5	Flood, "Vesleofsen"
4	Nord-Fron	2013	May 22nd	20.1	Flood, 200-year flood
5	Halden	2023	April 27th	16.8	Landslide, rockslide
6	Lund	2015	Dec 5th	16.7	Flood, Extreme Weather "Synne"
7	Åsnes	1995	June 2nd	16.2	Flood, "Vesleofsen"
8	Trysil	1995	June 1st	15.6	Flood, "Vesleofsen"
9	Skjåk	2018	Oct 14th	15.2	Flood
10	Sør-Aurdal	2023	Aug 9th	13.8	Extreme Weather "Hans"
11	Nesbyen	2023	Aug 8th	13.2	Extreme Weather "Hans"
12	Sør-Odal	1995	June 4th	12.5	Flood, "Vesleofsen"
13	Aurland	2014	Oct 28th	12.2	Flood, "Oktoberflommen"
14	Værøy	2019	Feb 16th	11.1	Storm
15	Værøy	2011	Nov 26th	10.6	Extreme Weather "Berit"
16	Moskenes	2011	Nov 26th	10.6	Extreme Weather "Berit"
17	Røst	2011	Nov 26th	10.1	Extreme Weather "Berit"
18	Lyngen	2010	Sep 03rd	8.5	Landslide, earth and clay
19	Flakstad	1993	Feb 03rd	8.0	Storm
20	Nord-Fron	2011	June 10th	7.8	Flood
21	Flå	2023	Aug 8th	7.6	Extreme Weather "Hans"
22	Ål	2023	Aug 8th	7.5	Extreme Weather "Hans"
23	Åmot	1995	May 30th	7.4	Flood, "Vesleofsen"
24	Ringebu	1995	June 2nd	7.4	Flood, "Vesleofsen"
25	Høylandet	2006	Feb 1st	7.2	Flood
26	Kvinesdal	2015	Dec 6th	6.8	Extreme Weather "Synne"
27	Nord-Aurdal	2023	Aug 8th	6.6	Extreme Weather "Hans"
28	Ringebu	2011	Jun 11th	6.5	Flood
29	Bjerkreim	2015	Dec 6th	6.4	Extreme Weather "Synne"
30	Vanylven	2011	Dec 25th	6.2	Extreme Weather "Dagmar"
31	Ringebu	2023	Aug 9th	5.9	Extreme Weather "Hans"
32	Øyer	1995	June 2nd	5.8	Flood, "Vesleofsen"
33	Flakstad	2011	Nov 26th	5.45	Extreme Weather "Berit"
34	Tokke	2021	Oct 4th	5.3	Flood
35	Loppa	1993	Feb 1st	5.09	Storm
36	Værøy	2008	Oct 25th	5.09	Extreme Weather "Ulrik"
37	Stryn	2011	Dec 25th	5.08	Extreme Weather "Dagmar"
38	Sel	2011	June 10th	5.07	Flood

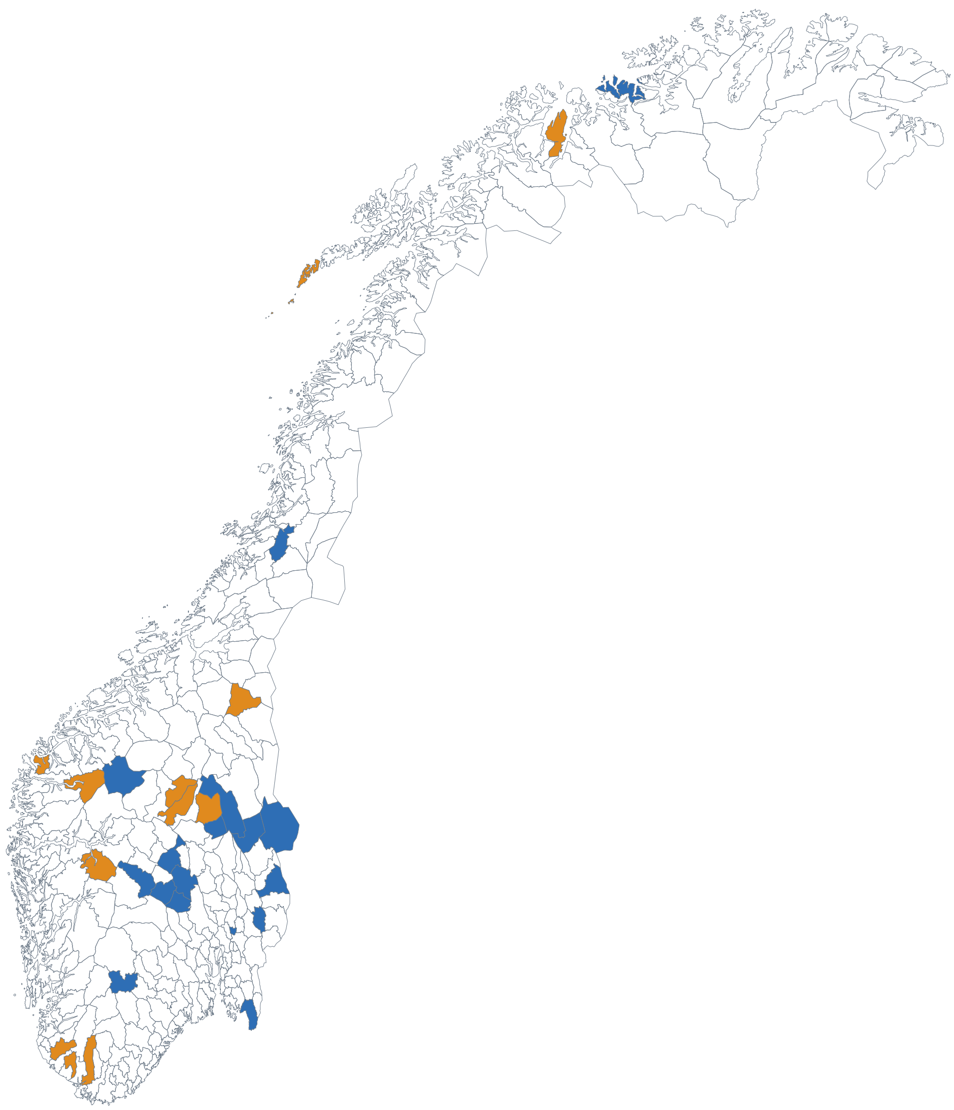
Note: This table contains a list of all natural disasters occurring in Norway during the period 1993–2023. Natural disasters are defined as municipality events where total insurance payouts relative to labor income exceeded 5 percent of local labor income. We restrict to municipalities in which the number of payouts in a given year is at least 15. Natural disasters have been ranked according to the share of payouts to local labor income. Events in blue correspond to the natural disasters within our sample period; the event in Værøy 2011 is within our sample period but treated households in this small municipality did not make it to our ultimate sample with household-level outcome variables after the matching process.

are not geographically clustered. As shown in Figure 4, the events have occurred all over Norway. Third, our classification identifies events of several types, as shown in Table 1. Floods, storms and extreme weather events constitute the majority of recorded disasters, while recent years have seen two large landslides.

Appendix A.3 shows that using alternative measures to identify the severity of the natural disasters (such as the ratio of insurance payouts to total municipality income after tax, or the per-capita insurance payouts in the municipality) leads to a similar ranking of the municipalities most affected by natural disasters.

Summary statistics for affected and non-affected municipalities are reported in Appendix A.4. Affected municipalities are on average smaller and less urban, and they exhibit lower levels of wealth and debt than municipalities that have not experienced a natural disaster. Labor income and consumption, and demographic characteristics such as age and education are broadly similar across the two groups. Insurance payouts are comparable when excluding the disaster years, suggesting that the events identified by our severity metric are large relative to both the size of the local economy and the historical distribution of insurance claims.

Figure 4: Map of natural disasters in Norwegian municipalities, 1993–2023



Note: This map indicates the geographical distribution of natural disasters occurring in Norway between 1993 and 2023. Natural disasters are defined as those municipality-year combinations where insurance payouts exceed local labor income by at least 5%. Municipalities marked in orange are those with events that occurred in our sample years $2010 - 2016$, while municipalities marked in blue are those that occurred outside this time period, but within the years $1993 - 2023$.

3.2 Control group

We employ high-dimensional near-neighbor matching to assign counterfactual controls to each treated household, using a CEM-style approach based on exact matching over coarsened household characteristics (Iacus, King, and Porro, 2012; Fagereng, Onshuus, and Torstensen, 2024). This procedure requires that each household residing in an affected municipality is paired with a group of households that are similar on observable characteristics, but have not resided in a municipality that has experienced a natural disaster throughout the whole observation period. Specifically, we require that the control households have never resided in a municipality where insurance payouts have exceeded two percent of labor income. This set of eligible households is the initial set of "potential controls" (4.5 million households).

Using detailed administrative records, we select a control group from the dataset of potential controls. Given that weather events may be spatially correlated, we employ exact dismatching at the county level to ensure that control households are not indirectly affected by a natural disaster in another municipality that lies in the same county.[7] Dismatching at the county level allows us to account for impacts that may spill over municipal boundaries within the same county.

Our matching procedure requires exact matching as of year-end of the year before the natural disaster for the following discrete variables: home ownership, ownership of risky assets, self-employment status, and an indicator for whether a household has children below the age of 18. We also match exactly on the maximum level of education attained in the household (master's degree or higher, bachelor's degree, upper secondary education, incomplete secondary education).

Additionally, we apply interval matching by selecting control households whose head (eldest member) is within a $\pm$5-year range to the age of the treated household's head. We also match on total consumption, household income after tax, debt level, and liquid assets within a $\pm 20\%$ range. Furthermore, we match on municipality population size within the range $\pm 30\%$ for large municipalities and $\pm 10,000$ inhabitants for small

[7] Norway is geographically divided into 15 counties and 356 municipalities as of January 1, 2020. Municipalities are often responsible for local services and administration, while counties encompass several municipalities and coordinate broader regional policies.

municipalities. This final criterion ensures that treated households and their respective control households reside in municipalities of comparable size.

We match with replacement, meaning that the same household can appear as a control for more than one treated unit, and we allow each treated household to be matched to multiple control households. The number of control households per treated household ranges from 1 to 1,558, with a highly skewed distribution. Notably, 76.6 percent of treated households have fewer than 100 control matches.[8] To ensure comparability across matching strata, we reweight households in the regression so that treated and control units are balanced within each matched stratum.[9]

We impose balancing and restrict our sample to treated and control households that we can observe continuously for at least four years prior to the event and two years after. Additionally, we winsorize consumption, income and wealth at the 1st and 99th percentiles. After matching and cleaning the data we are left with 7,646 unique treated households and 84,645 unique controls.

Table 2 contains summary statistics of the final sample of treated and control households, respectively, as of the start of the year of the natural disaster. The table indicates that the matching process results in a similar distribution between treated and control households. Over 75 percent of households are homeowners, while about one quarter have children. The average household has education below secondary level and has a head with an average age of 60. Additionally, the average household has a disposable income of approximately 42,000 USD, with total consumption around 31,500 USD. Housing wealth is around 125,000 USD, while debt levels average approximately 49,000 USD. Average liquid wealth (deposits and securities) is slightly lower than the amount of debt.

[8]In Appendix Table A6 we show that our results are robust to including a more restricted set of controls, keeping no more than the five closest neighbors according to our matching algorithm.

[9]The CEM weight, denoted $w_{i(m)}$, is the weight assigned to household i within matching group m (Iacus et al., 2012). A treated household i is associated with a single matching group, unique to i, whereas a control household may appear in multiple groups. Each treated household is assigned a weight of 1. Control households within the same bin m receive a distinct weight specific to that bin $w_{i(m)} = \frac{N^T_{i(m)}/N^C_{i(m)}}{N^T/N^C}$. Here $N^T_{i(m)} = 1$ denotes the number of treated households in bin m, $N^C_{i(m)}$ the number of control households in m, while N^C and N^T denote the total number of matched control and treated households across all matching groups.

3.3 Event study

We estimate the dynamic effect of natural disasters using an event-study difference-in-differences design on the matched household sample. Because disasters occur in different calendar years, treatment timing is staggered. However, treated households are matched to never-treated control households, defined as households that are never exposed to a natural disaster during the sample period, and these controls are assigned the event year of the treated households in their matching group. Thus, identification comes from comparisons between treated households and matched never-treated controls assigned the same event year, rather than from comparisons between earlier- and later-treated cohorts. This avoids the contamination of conventional two-way fixed-effects event-study coefficients under heterogeneous dynamic treatment effects emphasized by Sun and Abraham (2021). The resulting coefficients can instead be interpreted as weighted averages of cohort/event-time treatment effects, in the spirit of Callaway and Sant'Anna (2021).

Table 2: Comparison of treated and control households

	Treated (N=7,646)	Control (N=165,281)	Difference	t-statistic	p-value
Homeowner	0.766	0.777	-0.010	-1.845	0.065
Has kids	0.247	0.246	0.001	0.132	0.895
Self employed	0.062	0.057	0.005	1.425	0.154
Maximum HH education	1.747	1.750	-0.002	-0.204	0.839
Age of head of HH	59.921	59.979	-0.058	-0.292	0.770
Income after tax	42,129	41,467	662	3.189	0.001
Total consumption	30,891	30,141	750	2.907	0.004
Housing wealth	126,257	122,620	3,637	2.747	0.006
Debt	49,231	48,563	667	0.821	0.411
Liquid wealth	44,814	43,786	1,028	1.553	0.120
Move to new municipality	0.007	0.006	0.001	0.621	0.534
Move to same municipality	0.017	0.019	-0.002	-1.301	0.193

Note: This table contains the CEM weighted mean values for several variables for the households that were affected by a natural disaster ("Treated") and households that never lived in a municipality hit by a natural disaster ("Control"). Differences in the mean, t-statistics, and p-values are contained in the last three columns. All variables are measured in the year prior to the disaster.

We estimate the regression

$$Y_{i,m,t} = \sum_{\substack{k=-4 \\ k\neq -1}}^{3} \beta_k \mathbf{1}_{t-\tau_m=k} \times T_i + \sum_{k=-4}^{3} \delta_k \mathbf{1}_{t-\tau_m=k} + \eta_m + \epsilon_{i,m,t}. \tag{1}$$

Here, $Y_{i,m,t}$ is the outcome of household i, belonging to matching group m, in calendar year t. The indicator T_i is a time-invariant treated-group indicator equal to one for households living in the municipality affected by the disaster at the time of the event, and zero for matched households in municipalities not affected by any disaster during the sample period. Treatment timing is captured by the event year τ_m, which is defined by the disaster affecting the treated households in matching group m and assigned also to their matched controls. The indicators $\mathbf{1}_{t-\tau_m=k}$ denote event time, so that the interaction $\mathbf{1}_{t-\tau_m=k}T_i$ identifies treated households k years relative to the disaster. The δ_k absorb average differences across event time that are common to treated and control households, while η_m are matching-group fixed effects.

We estimate equation 1 by weighted least squares using the weights described in the previous section; these weights account for the variable number of matched controls per treated household and make the estimates representative of the matched treated population. The coefficients β_k measure differences between treated and matched control households k years relative to the disaster, normalized to zero in the year before the event ($k = -1$). The pre-event coefficients provide evidence on differential trends before the disaster, while the post-event coefficients trace out the dynamic treatment effects.

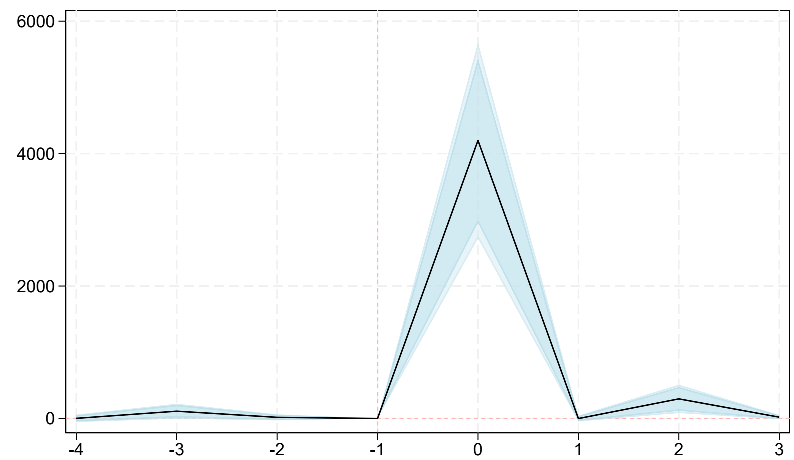
We cluster standard errors at the matching-group level. This corresponds to inference conditional on the realized set of disaster events and the matched comparison structure. Intuitively, clustering on matching groups treats the observed disasters as fixed and measures how estimates would vary under repeated sampling of affected and comparison households within the realized events. As a robustness check, Table A4 in Section A.6 reports standard errors clustered at the municipality level for our main outcome variables. In this case, the relevant thought experiment is one in which a different set of natural disasters could have occurred in different municipalities. Since treatment exposure is measured using municipality-level insurance payouts, this approach treats the municipality-event as the relevant level of treatment assignment and therefore captures uncertainty arising from variation across disaster realizations, rather than only from household-level sampling variation conditional on the realized events. The distinction is closely related to the framework in Abadie, Athey, Imbens, and Wooldridge (2023), which emphasizes that the appropriate level of clustering depends on the underlying sampling and treatment-assignment process, rather than solely on residual correlation within geographic units. Importantly, most of our main results – notably for consumption and wealth – remain statistically significant when standard errors are clustered at the municipality level, although the increase in standard errors is substantial for some outcome variables.

3.4 Direct effect of natural disasters

To benchmark our estimates of the indirect effects of natural disasters, we estimate the average direct damages caused by these events. Figure 5 shows the estimated increase in insurance payouts for natural damages per household in affected municipalities, relative to households in the control group. The black line shows the estimated dynamic treatment effects relative to the year immediately before the disaster. Period 0 on the horizontal axis corresponds to the event year. Insurance payouts increase sharply in the year of the disaster, by about \$4,200 per resident household in affected municipalities relative to non-affected municipalities.

Under the Norwegian natural-perils insurance scheme, covered damages are compensated in full apart from a deductible of about \$1,000 (NOK 8,000), which was fixed throughout our sample period. Because the deductible is small relative to total payouts, observed insurance payments capture most of the direct economic loss. Accounting for the deductible increases the estimated average damage by only about \$100 per household, implying that households paid out of pocket for roughly 2.3 percent of direct damages. Thus, the estimates in Figure 5 can be interpreted as a close approximation to the average direct damage per treated household, while also confirming that households bear only a small share of these direct costs themselves.

Figure 5: Direct damages per household.



Note: This figure shows the estimated coefficients β_k ($k = -4, \ldots, 3$) from Equation 1. The dependent variable is the average insurance payout per household in the municipality. The sample consists of households living in municipalities hit by a natural disaster ("Treated") and a matched sample of households that were never affected by natural disasters ("Control"). The horizontal axis represents the number of years relative to the natural disaster occurring in year 0. Treatment effects are calculated relative to the year immediately before the disaster ($t = -1$), as indicated by the orange vertical line. Amounts are expressed in real 2018 USD. Shaded areas in light blue show 90% and 95% confidence intervals based on standard errors clustered at the municipality level.

4 Results

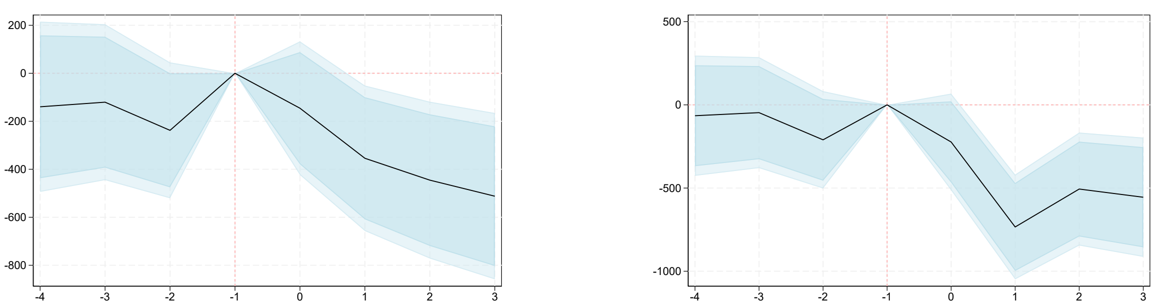
This section establishes the main result of the paper: natural disasters cause a large and persistent decline in household consumption expenditures, even in a setting where direct damages are almost fully insured. We first estimate the response of total consumption and benchmark the magnitude against the direct physical damages measured by insurance payouts. We then exploit the granularity of the transaction data to show how the consumption response differs across consumption categories, namely durables and non-durables as well as essentials and non-essentials.

Like most measures of consumption, our transaction data measure expenditures rather than the flow of consumption services. We therefore use "consumption" as shorthand for household spending, including both durable and non-durable purchases. This distinction is important because durable spending can be postponed without an immediate one-for-one decline in consumption services. The category-level analysis in Section 4.2 addresses this issue by separating durable from non-durable and essential from non-essential expenditures.

4.1 The Indirect Consumption Response

Figure 6, panel (a), shows the dynamic response of household spending to natural disasters. The pre-event coefficients are not significantly different from zero, indicating that treated and matched control households followed similar consumption paths before the disaster. After the event, consumption falls gradually and persistently for households in affected municipalities. Over years 0 to 3, treated households reduce cumulative spending by about $1,457 relative to matched controls. This corresponds to approximately 32% of the average direct damages measured by the cumulative insurance payouts per household in affected areas (see Section 3.4). Thus, even though the direct physical losses are almost fully insured, households reduce spending by an amount that is large relative to the size of the disaster shock.

Figure 6: Effect of natural disasters on consumption.



(a) Total consumption

(b) Total consumption, adjusted

Note: This figure shows the estimated coefficients β_k ($k = -4, \ldots, 3$) from Equation 1. The dependent variable in Panel (a) is unadjusted consumption; in Panel (b) it is consumption net of insurance transfers. In both panels, the sample consists of households living in municipalities hit by a natural disaster ("Treated") and a matched sample of households that were never affected by natural disasters ("Control"). The horizontal axis represents the number of years relative to the natural disaster occurring in year 0. Treatment effects are calculated relative to the year immediately before the disaster ($t = -1$), as indicated by the orange vertical line. Amounts are expressed in real 2018 USD. Shaded areas in light blue show 90% and 95% confidence intervals for the point estimate.

The baseline consumption response may, however, understate the negative indirect effect of the disaster on spending. For most directly damaged properties, insurance companies pay contractors directly and the repair expenditure is not recorded as household consumption. However, a small subset of households instead receive direct transfers from insurance companies and manage the repair or replacement themselves. For these households, measured consumption may mechanically increase because insurance-financed repair spending is included in the transaction data. This is a *direct* effect of the disaster on spending.[10] In Appendix A.7, we argue that the fraction of insurance payments related to natural disasters that go through households is around 10%. To account for this, panel (b) of Figure 6 subtracts the additional insurance transfers received by treated households relative to controls. This adjustment assumes that all excess transfers from insurance companies are spent on repairing or replacing damaged property. It is therefore a conservative adjustment for direct repair-related spending: if some transfers are saved or used for non-repair consumption, the adjustment overstates the direct spending component and makes the estimated indirect decline too large in absolute value. Using this adjusted measure, the cumulative consumption decline after the disaster amounts to 45% of direct damages. We therefore interpret the unadjusted and adjusted estimates as bracketing the indirect consumption response, with point estimates ranging from 32% to 45% of the average direct damages caused by the disaster.

Why does consumption fall so much in a setting where insurance covers almost all direct damages? Standard models of household consumption imply that spending responds to changes in disposable income and household wealth, especially when shocks are persistent or households face borrowing constraints (Friedman, 1957; Carroll, 1997; Kaplan and Violante, 2014). Table 3 reports average effects over the four post-event years for consumption, disposable income, and wealth. The table shows that disposable income falls after a disaster, but by substantially less than consumption. Household wealth also declines sharply. In Section 5, we attempt to interpret the consumption response in light of the changes in income and wealth, and we explain the mechanisms behind these responses.

Table A5 in Section A.6 shows that our main results are robust to alternative defi-

[10] In Figure A3 in Section A.7, we plot estimates of the effect on income and consumption for the subset of households in our sample who receive payments from insurance companies in either year 0 or year 1. While the income response is similar to that in the full sample, the consumption response is substantially smaller in the subsample, in particular in the event year and the following year, suggesting the presence of a positive direct effect.

Table 3: Indirect effects of natural disasters

	Average effect	% Direct damages
Total consumption	-365***	32.3
	(125)	
Adjusted consumption	-505***	44.7
	(131)	
Income after tax	-180**	15.9
	(87)	
Gross wealth	-10730***	950.5
	(1368)	

Note: The first column of this table contains estimates for the average treatment effect on consumption, income and wealth over years 0 to 3, where year 0 is the year of the disaster. The second column expresses this figure as a fraction of the total direct damages of the natural disaster, as calculated in Figure 5. ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$).

nitions of disaster exposure, including alternative severity thresholds and specifications that exclude the 2010 Lyngen landslide.[11]

4.2 Adjustment Across Consumption Categories

The aggregate consumption responses shown in the previous section may mask important differences across types of spending. Before turning to the mechanisms behind the overall decline, we examine how households adjust the composition of their spending after a natural disaster.

Households facing adverse shocks may try to smooth essential and non-durable consumption by reducing more discretionary or postponable expenditures, especially durable goods, for which the timing of purchases is more flexible (Browning and Crossley, 2009). Distinguishing between these margins is therefore useful for interpreting whether the fall in spending mainly reflects postponed purchases or a broader decline in day-to-day consumption. Our transaction-level data allow us to make this distinction directly. We observe spending by detailed consumption categories and estimate responses along two separate dimensions: durable versus non-durable spending, and essential versus non-essential spending.

[11] The latter exercise is useful because Lyngen is the only landslide in the estimation sample; the remaining events are primarily flood-related or officially registered extreme weather events.

Table 4 shows that the spending decline is concentrated in adjustable categories. Durable spending falls by about \$350 per year on average over the four post-event years, implying a cumulative decline equivalent to 31% of direct damages. Non-essential spending falls by a similar amount, about \$330 per year. By contrast, the estimated declines in non-durable and essential spending are smaller and not statistically significant. These patterns suggest that households respond to the disaster primarily by cutting spending categories that are easier to postpone or more discretionary, rather than by reducing essential day-to-day expenditures. This composition of adjustment is consistent with evidence from other shocks. For example, Gareis and Minasian (2025) show that, after monetary policy shocks in the Euro area, durable consumption responds more strongly than non-durable consumption, non-essential spending responds more strongly than essential spending, and durable non-essential items exhibit the largest responses. Similarly, Parker, Souleles, Johnson, and McClelland (2013) find that spending on new vehicles –a category included in durable goods – was an important driver of the consumption response to the 2008 US economic stimulus payments.

Table 4: Consumption categories

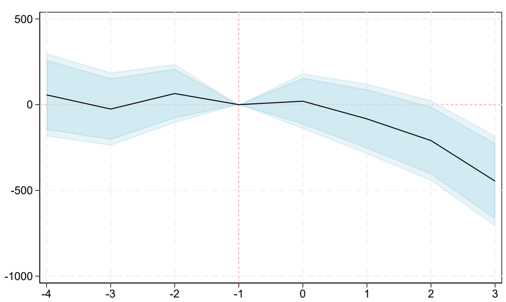
	Average effect	% Direct damages
Durable goods	-353***	31.3
	(117)	
Non-durable goods	-71	6.3
	(104)	
Essential spending	-98	8.7
	(67)	
Non-essential spending	-326***	28.9
	(123)	

Note: This table contains estimates for the average treatment effect on consumption categories over years 0 to 3, where year 0 is the year of the disaster. Durable goods consist of the following COICOP (Classification of Individual Consumption According to Purpose) categories: housing, furnishings and household equipment, vehicles, audio equipment, photographic equipment, IT equipment, and durable outdoor equipment; non-durable goods are all remaining consumption categories net of transfers to banks, insurance companies and public institutions and financial services. Essential spending consists of food (excluding alcoholic beverages), health, communications, and personal care; non-essential spending consists of all remaining categories net of transfers to banks, insurance companies and public institutions and financial services. ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$).

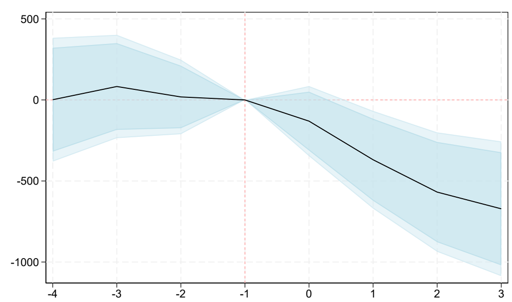
5 Transmission Channels

The results above show that natural disasters lead to a large and persistent decline in household consumption spending, concentrated in durables and non-essentials, even though direct physical damages are almost fully insured. To understand this response, we first examine how disasters affect the two main household balance-sheet variables that can transmit shocks to consumption: income and wealth.

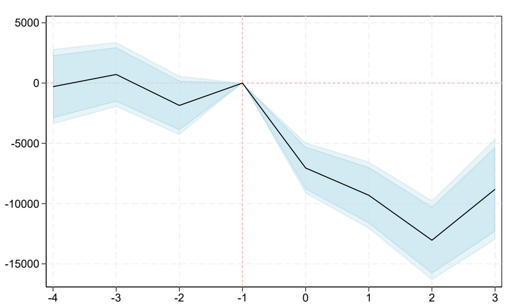
Figure 7: Effect of natural disasters on income and wealth.



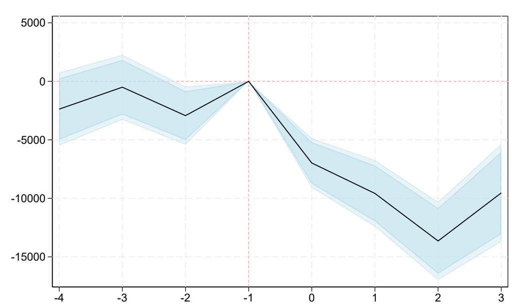
(a) Income After Tax



(b) Labor Income



(c) Net Wealth



(d) Housing Wealth

Note: This figure shows the estimated coefficients β_k ($k = -4, \ldots, 3$) from Equation 1. The dependent variable in Panel (a) is income after tax; labor income in Panel (b); net wealth in Panel (c), and housing wealth in Panel (d). In all panels, the sample consists of households living in municipalities hit by a natural disaster ("Treated") and a matched sample of households that were never affected by natural disasters ("Control"). The horizontal axis represents the number of years relative to the natural disaster occurring in year 0. Treatment effects are calculated relative to the year immediately before the disaster ($t = -1$), as indicated by the orange vertical line. Amounts are expressed in real 2018 USD. Shaded areas in light blue show 90% and 95% confidence intervals for the point estimate.

Figure 7 shows the dynamic effects on disposable income, labor income, net wealth, and housing wealth; the first column of Table A3 reports average treatment effects of all outcome variables over the post-disaster period. The figure shows that disposable income falls after a natural disaster, and this decline is driven by lower labor income. However, due to the role of the tax system in cushioning any fall in labor income into disposable income, the former drops by more than the latter. The other major component of income, capital income, shows a small and statistically insignificant response, as shown in Table A3. Household wealth also falls sharply and persistently. The decline is fully explained by falling housing wealth, while financial wealth does not respond. Thus, the aggregate responses point to two specific margins through which natural disasters may depress consumption: the labor market and the housing market. We study these margins in turn. Section 5.1 examines whether the fall in labor income reflects disruptions to local firms and labor demand by investigating the difference between natural disasters that mainly affect firms and those that mainly affect households directly. Section 5.2 studies whether the decline in housing wealth contributes to the consumption response by examining the differential responses between homeowners and renters as well as responses in debt, housing transactions, and mobility.

5.1 Labor-Market Disruptions and Income Losses

The decline in disposable income documented above is driven primarily by lower labor income. This points to the labor market as one channel through which natural disasters affect household consumption. A natural disaster may reduce local labor demand if it damages firms' productive capacity or disrupts infrastructure in a way that reduces local economic activity. In that case, the income response should be stronger when direct damages are concentrated among firms rather than households. To examine this channel, we split the disaster events according to whether direct damages are primarily covered by business or household insurance policies. We classify an event as a firm event if more than half of total insured damages are covered by business insurance, and as a household event otherwise.[12] This split is useful because the two types of events

[12] Appendix A.8 contains a description of the disasters classified as firm events and discusses how these differ from the rest.

differ in who is directly hit by the physical damages, while both occur in the same institutional setting with near-complete insurance coverage. Table 5 and columns 1–3 of Table A3 report average post-event effects for the full sample and separately for firm and household events.

Table 5: Natural disasters and the labor market

	(1) Baseline	(2) Firm event	(3) HH event
Panel A: Income components			
Income after tax	-180**	-361**	-101
	(87)	(158)	(104)
Labor income	-435***	-695***	-323*
	(139)	(254)	(166)
Capital income	-160*	-244	-124
	(88)	(165)	(104)
Panel B: Labor market status			
Unemployment	-0.001	0.009*	-0.006
	(0.003)	(0.005)	(0.004)
Self-employment	0.002	0.005	0.001
	(0.002)	(0.003)	(0.002)
Panel C: Insurance payouts and consumption			
Payouts to households	4463***	3585***	4844***
	(23)	(36)	(28)
Total consumption	-365***	-711***	-215
	(125)	(236)	(148)

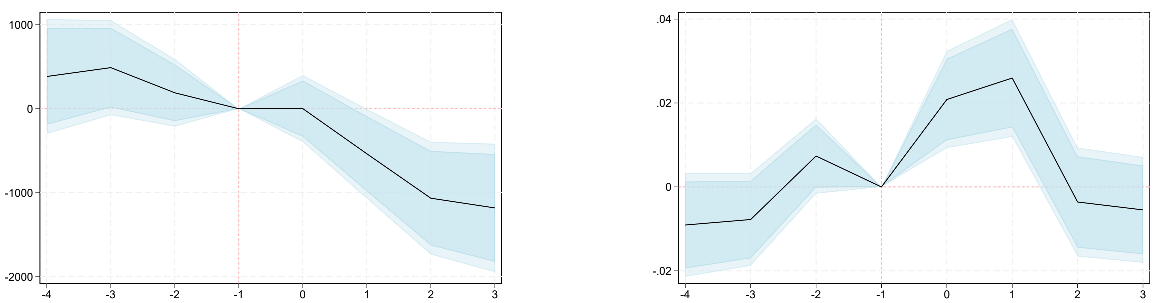
Note: This table contains estimates for the average treatment effects over years 0 to 3, where year 0 is the year of the disaster. Each row corresponds to a different outcome variable, as indicated in the leftmost column. Estimates correspond to the average treatment effect for the full sample of treated and control households in column 1. In columns 2 and 3, the average treatment effect is estimated separately for natural disasters that have a large effect on local firms or on households, respectively. Firm events correspond to those where more than half of total insured damages are covered by business insurance. ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$).

The results show that the indirect effects are concentrated in firm events. Following firm events, disposable income falls by about \$361 on average over the four post-event years, while consumption falls by about \$711. By contrast, household events generate smaller and statistically insignificant income responses. The decline in disposable income is driven by lower labor income; capital income does not respond meaningfully.

This pattern is consistent with a labor-demand channel, where natural disasters affect households indirectly by disrupting firms and local production rather than through uncompensated damages to household property.

Figure 8 provides additional evidence on this mechanism by showing the dynamic response of labor income and unemployment after firm events. Labor income declines gradually after the disaster, while unemployment increases sharply in the year following the event. The increase in unemployment is about two percentage points one to two years after the disaster, a large effect relative to the average unemployment rate in Norway. Together, the decline in labor income and the rise in unemployment indicate that firm-related natural disasters reduce household resources partly through weaker local labor demand.

Figure 8: Effect of natural disasters on income and unemployment in firm events.



(a) Labor Income **(b)** Unemployment

Note: This figure shows the estimated coefficients β_k ($k = -4, \ldots, 3$) from Equation 1. The dependent variable in Panel (a) is labor income; in Panel (b) it is an indicator taking the value one if the household is unemployed. In both panels, the sample consists of households living in municipalities hit by a natural disaster ("Treated") and a matched sample of households that were never affected by natural disasters ("Control"). Moreover, the sample is restricted to natural disasters where damages to firms are large. The horizontal axis represents the number of years relative to the natural disaster occurring in year 0. Treatment effects are calculated relative to the year immediately before the disaster ($t = -1$), as indicated by the orange vertical line. Amounts are expressed in real 2018 USD. Shaded areas in light blue show 90% and 95% confidence intervals for the point estimate.

How much of the average consumption decline can be explained by the fall in income? As a back-of-the-envelope exercise, we ask whether the consumption response is consistent with the response of disposable income for a reasonable marginal propensity to consume. Empirical estimates of the MPC out of unexpected, temporary income shocks are typically in the range of 30–50%.[13] Using variation from unemployment spells among Norwegian workers, Fagereng, Onshuus, and Torstensen (2024) estimate an MPC of 40%, while Bilbiie, Galaasen, Gürkaynak, Mæhlum, and Molnar (2025) find an average MPC of 38% using the same consumption data as in this paper.[14] Applying an MPC of 40% to the estimated decline in disposable income implies a consumption response of about \$72 ($= \$180 \times 0.4$) per year. This accounts for only around 20% of the baseline consumption decline (= 72/365). Hence, it suggests that while labor-market disruptions may be an important part of the transmission mechanism, they are too small to account for the full consumption response.

5.2 The Housing Market

The fact that income losses explain only a minority of the consumption decline points to household wealth as an additional transmission channel. The aggregate wealth response documented above is concentrated almost entirely in housing wealth. This should not be interpreted as a mechanical effect of damage to the physical structure of individual homes. As explained in appendix A.1, our housing-wealth measure reflects third-party estimated market values based on hedonic regressions and machine learning that take into account local sales and characteristics such as age, size and location that are not directly affected by the disaster or by subsequent repairs. Furthermore, damaged homes constitute only a subset of the housing stock in affected municipalities. The estimated response therefore primarily captures a broader repricing of housing in the affected area. To the extent that insurance-financed rebuilding improves the quality of some

[13] See, for example, Andersen et al. (2023); Patterson (2023), who estimate MPCs out of unemployment shocks. Parker et al. (2013); Jappelli and Pistaferri (2014); Parker (2017); Aguiar et al. (2025); Fagereng et al. (2021); Gelman (2022); Boehm et al. (2025); Hamilton et al. (2023); Borusyak et al. (2024); Orchard et al. (2025) find similar magnitudes for MPCs out of windfall income gains.

[14] Strictly speaking, the relevant object is a marginal propensity to spend (MPX) rather than a marginal propensity to consume in the flow-utility sense (Maxted et al., 2025). This distinction does not affect the calculation in this paragraph, since the empirical MPC estimates we refer to are also based on spending measures that include both durable and non-durable expenditures.

damaged properties and this is reflected in assessed values, it would attenuate rather than explain the decline in housing wealth.

The negative effect on housing wealth is consistent with existing evidence that natural disasters can be capitalized into local property values. Using Norwegian insurance-claims data, Kivedal (2023) finds that natural disasters reduce regional house prices. Such effects may arise even when insurance covers the rebuilding costs of damaged homes, because disasters can reduce local housing demand by lowering amenities, disrupting infrastructure, or increasing perceived future disaster risk and thereby reducing the attractiveness of the local area. Gallagher (2014) shows that flood insurance take-up rises sharply after a flood (before gradually decaying), while Iversen and Aanesen (2026) find that directly affected Norwegian homeowners report higher demand for climate-related insurance, driven primarily by heightened climate-related fear. Even more directly, Gibson and Mullins (2020) find that flood-risk signals reduce property values by changing beliefs about future risk. Lower housing values can in turn affect consumption even when households do not sell their homes: for homeowners, a decline in housing wealth reduces net worth and collateral, may tighten borrowing constraints, and can induce higher saving or debt repayment in order to repair household balance sheets.

To assess whether this channel is important, we split treated households by their pre-disaster homeownership status. This split is informative because homeowners and renters are exposed to many of the same local shocks, including disruptions to employment opportunities, infrastructure, and local amenities, but they differ sharply in their exposure to local house-price declines. Homeowners own the asset whose value is affected by the disaster, while renters do not. If the housing-market channel contributes to the consumption response, we should therefore expect consumption to fall more for homeowners than for renters, even if their disposable income responds similarly. Table 6 and Table A3 (columns 1 and 4–5) report average post-event effects for the full sample and separately for homeowners and renters.

The results are consistent with an important housing-market channel. While the point estimates for disposable income are similar across the two groups, the consumption

Table 6: Natural disasters and the housing market

Outcome	(1) Baseline	(2) Homeowner	(3) Renter
Panel A: Wealth components			
Total gross wealth	-10730***	-12329***	-4137*
	(1368)	(1607)	(2420)
Housing wealth	-9934***	-11651***	-2906
	(1315)	(1551)	(2267)
Financial wealth	-31	-186	575
	(268)	(293)	(649)
Debt	-1181***	-1258***	-863
	(381)	(441)	(741)
Net wealth	-9550***	-11047***	-3379
	(1304)	(1544)	(2171)
Panel B: Housing transactions			
House purchase	-0.007***	-0.009***	-0.003
	(0.002)	(0.003)	(0.003)
House sale	-0.001	-0.002	0.001
	(0.002)	(0.003)	(0.005)
Panel C: Relocations			
Move within municipality	0.003	0.001	0.011*
	(0.002)	(0.002)	(0.006)
Move out of municipality	-0.001	-0.002	0.006**
	(0.001)	(0.002)	(0.003)
Panel D: Income and consumption			
Income after tax	-180**	-169*	-230
	(87)	(100)	(169)
Total consumption	-365***	-401***	-232
	(125)	(145)	(250)

Note: This table contains estimates for the average treatment effects over years 0 to 3, where year 0 is the year of the disaster. Each row corresponds to a different outcome variable, as indicated in the leftmost column. Estimates correspond to the average treatment effect for the full sample of treated and control households in column 1. In columns 2 and 3, the average treatment effect is estimated separately for homeowners and renters, respectively. ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$).

response is about twice as large for homeowners, who are directly exposed to declines in local property values. Among homeowners, housing wealth falls sharply after a disaster, while financial wealth shows little response. Homeowners also reduce debt and become less likely to purchase new housing. These patterns are consistent with lower housing demand and balance-sheet adjustment: households may take on less new mortgage debt because they postpone or reduce housing purchases, and may also increase saving to pay down existing debt.

Additional evidence comes from mobility responses. Renters become more likely to move after a disaster, while homeowners do not. This pattern is consistent with a lock-in effect among homeowners: when local house prices fall, selling the home may require realizing a capital loss, and lower collateral values may make it harder to finance a new purchase. This interpretation is related to evidence from other negative regional shocks in Norway: Bojeryd (2025) finds substantial differences in migration responses between renters, low-housing-wealth homeowners, and higher-housing-wealth homeowners (see also Fonseca and Liu, 2024). Homeowners may therefore remain in the affected municipality while adjusting on other margins, including consumption and debt.

We use a simple accounting exercise to assess whether the housing-wealth response is large enough to explain the remaining fall in consumption, having previously shown that lower income can only account for about 20% of the fall in consumption. After accounting for the income channel, the residual consumption decline is of a magnitude that lines up with standard estimates of MPCs out of housing wealth. To explain this residual through housing wealth, our estimates imply an MPC out of housing wealth of around 3%.[15] This magnitude is in line with the literature on consumption responses to exogenous movements in house prices, which typically finds MPCs between 3% and 5%. For instance, Guren et al. (2021) estimate a benchmark MPC of 3.3%, while Aastveit et al. (2025) estimate an MPC of 3.6% in Norway using the same consumption data as in this paper. The homeowner–renter split further supports this interpretation: while the point estimate of the income response is similar for renters and homeowners, the

[15]To obtain this number, we assume that 40% of the average yearly decline in income after tax, about \$72, is transmitted to consumption. The remaining consumption response then equals approximately 2.9% of the average decline in housing wealth: $0.029 \times 9{,}934$. For the adjusted consumption measure, the equivalent number will be slightly higher, but still within the range of MPC estimates reported in the literature.

consumption response is much smaller for renters, who are not directly exposed to the fall in local housing wealth. Taken together, the estimates are consistent with housing wealth playing a central role in transmitting natural disaster shocks to household spending.

The labor-market and housing-market channels should not be interpreted as independent mechanisms. Disruptions to firms and local production may reduce labor demand directly, but they may also make the affected area less attractive to live in, thereby weakening local housing demand and amplifying the decline in property values. Consistent with this interpretation, the fall in housing wealth is substantially larger following firm events than following events in which direct damages are concentrated among households (see Table A3). The consumption response may therefore reflect a mutually reinforcing process: weaker local labor markets reduce household income, while the associated deterioration in local economic prospects is capitalized into house prices and further depresses spending through household balance sheets. Our evidence does not separately identify each link in this process, but it shows that the indirect effects of natural disasters propagate through interconnected local labor and housing markets.

6 Conclusion

This paper finds that natural disasters impose substantial costs on households in affected areas even when direct physical damages are almost fully insured. Using Norwegian insurance claims, administrative records, and transaction-level spending data, we show that disasters lead to a persistent decline in household consumption that is large relative to the direct damages caused by the events. This implies that the household costs of natural disasters extend well beyond the insured value of damaged property. Two mechanisms can explain the fall in spending. First, disasters reduce labor income, especially when direct damages are concentrated among firms, consistent with disruptions to local labor demand. This income channel is economically meaningful, but standard marginal propensities to consume imply that it explains only a minority of the spending decline. Second, disasters reduce housing wealth. Homeowners cut consumption substantially more than renters despite similar income losses, reduce debt, and become less likely to purchase new housing, while renters are more mobile. These patterns suggest that local housing markets and household balance sheets play a central role in transmitting disaster shocks to consumption.

These findings contribute to debates on the sustainability and design of disaster insurance as natural disasters become more frequent and severe. Internationally, rising insurance costs and reduced coverage in high-risk areas highlight the challenges of maintaining broad protection against climate-related disasters. Norway's insurance scheme substantially mitigates immediate, direct losses, yet significant indirect effects – via depressed local housing markets and reduced labor earnings – remain. The dominant role of housing wealth suggests that natural disasters can matter for financial stability even when direct damages are insured. If disasters lead to a repricing of housing in affected areas, they reduce the value of real-estate collateral and may weaken household balance sheets. This is particularly relevant in insurance systems where premiums are universal or weakly related to local risk: high property values in exposed areas may look sustainable ex ante, but adjust downward once disasters reveal or update beliefs about local risk. The effects are also distributional. Homeowners bear the housing-wealth losses and may become locked in to affected areas, while renters are less exposed to capital losses and appear more mobile. Taken together, the evidence points to an important category of uninsurable risks: even comprehensive property insurance and a generous welfare system such as Norway's cannot fully protect households against disruptions to local housing markets, labor markets, and economic activity.

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A Appendix

A.1 Adjustment of housing wealth with municipal weights

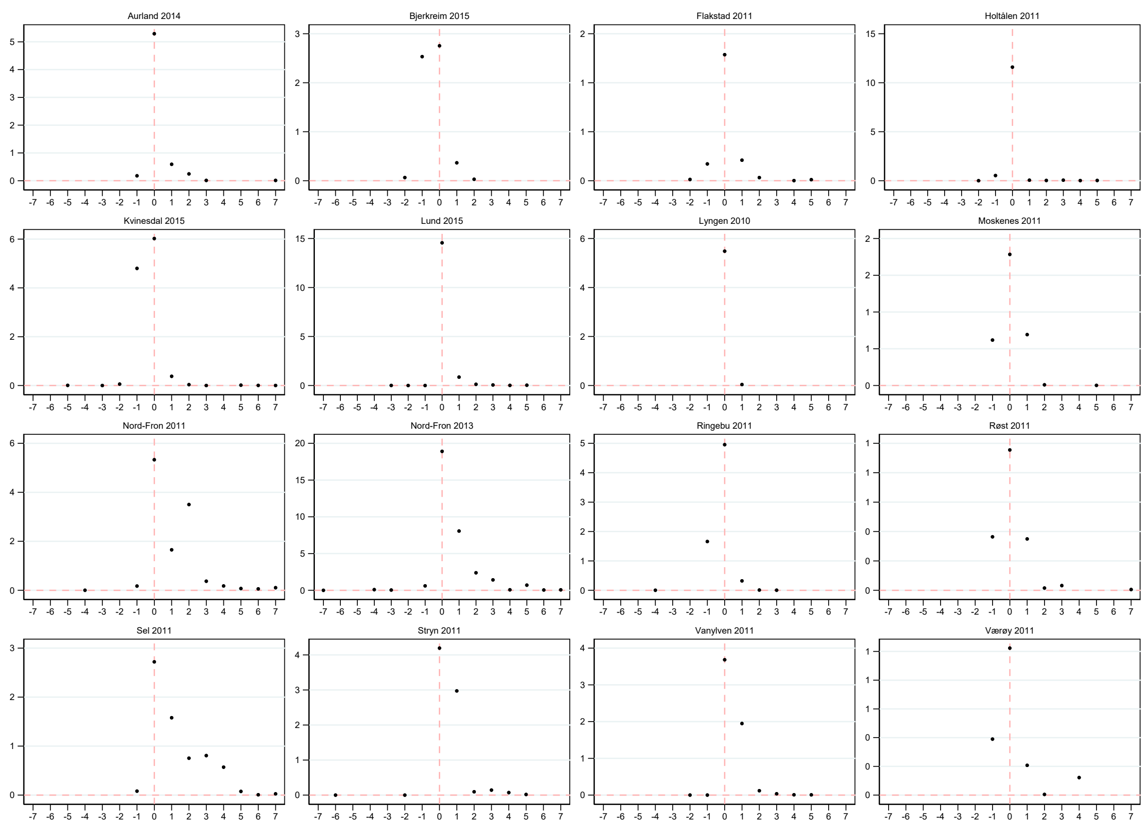
Starting with the 2010 tax year, Statistics Norway implemented a new method for calculating the value of housing for tax purposes. Before 2010, the tax value of a house was based on the price of the house when first constructed, updated annually using a common adjustment factor for all residential properties in Norway. As of 2010, each residential property is assigned its own market value every year, based on predicted values from hedonic regressions that include characteristics such as age, size, location and type of house.

We adjust the housing values from tax returns to better account for the market value of housing wealth in all years. We do this by applying adjustment factors that link the tax-assessed values to estimated market values from the machine learning model in Fagereng et al. (2020). These adjustment factors were constructed from their underlying data and shared with us by the authors. Specifically, for each year and municipality, the ratio of the estimated market value to the reported tax value is computed at the household level, and the median of these ratios within a municipality is used as the adjustment factor. We use these factors to scale reported housing wealth in the tax data to approximate market values.

A.2 Identification of natural disaster event dates

Table 1 in Section 3.1 represents a list of natural disasters classified using our proposed severity metric. To determine the exact date of each natural disaster, we identify the day on which the municipality experienced the highest insurance payouts. Figure A1 shows that insurance payouts are sharply concentrated around the identified event date.

Figure A1: Insurance Payouts Around Event Dates (±7 Days).



Note: This figure shows daily insurance payouts around the event date for each natural disaster in the estimation sample. Each panel corresponds to one municipality-year event. The event date, indicated by day 0 on the horizontal axis, is defined as the day on which the municipality experienced the highest insurance payouts during the event year. The horizontal axis shows days relative to the event date, from seven days before to seven days after the event. Insurance payouts are expressed in millions of 2018 USD.

A.3 Robustness of the severity metric

Table A1 compares the ranking of the largest municipality–year natural disaster events under our baseline severity measure (insurance payouts as a share of labor income) with two alternative normalizations: payouts as a share of income after tax and payouts per capita. The table shows that the ranking is highly stable across measures. The top five events are identical and appear in the same order under all three definitions, and for the remaining events the rank changes are modest. Moreover, the same set of municipality–year events remains among the most severe across all metrics. We restrict attention to events with at least 15 payouts, ensuring that the rankings are not driven by a few isolated claims.

Table A1: Ranking of Natural Events by Different Metrics.

Municipality	**Year**	**Payouts as share of labor income**	**Payouts as share of income after tax**	**Payouts per capita**
Holtålen	2011	1	1	1
Nord-Fron	2013	2	2	2
Lund	2015	3	3	3
Aurland	2014	4	4	4
Værøy	2011	5	7	7
Moskenes	2011	6	5	6
Røst	2011	7	6	5
Lyngen	2010	8	8	11
Nord-Fron	2011	9	9	10
Kvinesdal	2015	10	11	9
Ringebu	2011	11	13	13
Bjerkreim	2015	12	10	8
Vanylven	2011	13	12	12
Flakstad	2011	14	15	17
Stryn	2011	15	14	15
Sel	2011	16	16	19

The number of payouts in a municipality in a given year is at least 15.

A.4 Summary statistics for municipalities

Table A2: Summary Statistics for Affected and Not Affected Municipalities

Affected Municipalities					
	Mean	Median	St.dev.	Min	Max
Population	2501.59	2043.00	1635.65	473.54	5519.46
Age	49.79	52.00	16.09	26.08	78.92
Higher Education	0.17	0.17	0.03	0.11	0.24
Labor Income	29224.23	29072.22	2973.83	25315.19	36857.61
Total Gross Wealth	133857.33	136041.99	17514.90	98957.33	168319.02
Debt	46447.17	44386.16	8744.45	36839.93	70591.05
Total Consumption	22467.27	22033.36	3521.06	18827.93	34385.33
Total Payouts incl. Events	347.36	288.60	147.59	160.30	654.60
Total Payouts excl. Events	38.02	35.40	26.41	10.68	124.79
Not Affected Municipalities					
	Mean	Median	St.dev.	Min	Max
Population	11649.67	4125.08	32806.79	167.62	499905.15
Age	49.82	49.54	15.87	19.69	91.00
Higher Education	0.21	0.20	0.06	0.12	0.48
Labor Income	30579.08	29952.37	4443.74	22443.32	48349.65
Total Gross Wealth	150083.36	141284.36	43603.51	86504.60	386650.83
Debt	56592.05	54631.96	15480.58	24306.11	102876.72
Total Consumption	21720.30	21595.17	2316.55	15848.26	30853.15
Total Payouts	68.10	60.05	39.62	14.67	254.42

Notes: Total number of observations is 356 (municipal level).
An affected municipality is a municipality that has experienced a natural disaster in one of the years in the sample period.
All monetary values are in real USD (2018 prices), per capita.
Higher education indicates the share of individuals in a municipality who have attained either a lower or higher university degree.
Based on data for the population of Norway aged 16 and over.

Table A2 presents summary statistics for affected and not affected municipalities, respectively. Affected municipalities are on average smaller in terms of population, consisting of about 2,500 residents (median ≈ 2,040), whereas not affected municipalities average around 11,600 residents (median ≈ 4,125). These differences are primarily due to the inclusion of larger cities in the latter group – the vast majority of Norwegian municipalities are small. The ten most populous municipalities account for 36 percent of the total population in Norway.

The average age is quite similar between the two groups, and education levels are also quite comparable: affected municipalities report that about 17 percent of residents have higher education, compared to roughly 21 percent in not affected municipalities. When it comes to financial indicators, while labor income and consumption are quite comparable, affected municipalities report notably lower total gross wealth, and debt compared to their unaffected counterparts.

When it comes to insurance payouts, we see from Table A2 that the two groups are comparable when excluding the years in which the natural disaster occurred (see Total Payouts including and excluding the events), and there is no indication that affected municipalities generally receive higher insurance payouts than non-affected ones. This highlights that the events we are analyzing are large not only in relation to the local economy but also when compared to historical data, and it supports the use of these events as natural experiments.

A.5 All baseline results

Table A3: Average effects of natural disasters

	Baseline	Event Type		Household Type	
Outcome	(1)	Firm event (2)	HH event (3)	Homeowner (4)	Renter (5)
Income after tax	-180**	-361**	-101	-169*	-230
	(87)	(158)	(104)	(100)	(169)
Net wealth	-9550***	-19675***	-5156***	-11047***	-3379
	(1304)	(2397)	(1550)	(1544)	(2171)
Consumption	-365***	-711***	-215	-401***	-232
	(125)	(236)	(148)	(145)	(250)
Labor income	-435***	-695***	-323*	-469***	-313
	(139)	(254)	(166)	(162)	(254)
Capital income	-160*	-244	-124	-199*	-13
	(88)	(165)	(104)	(108)	(100)
Self-employed	0.002	0.005	0.001	0.002	0.002
	(0.002)	(0.003)	(0.002)	(0.002)	(0.005)
Unemployed	-0.001	0.009*	-0.006	-0.002	0.002
	(0.003)	(0.005)	(0.004)	(0.004)	(0.005)
Housing wealth	-9934***	-19348***	-5848***	-11651***	-2906
	(1315)	(2373)	(1578)	(1551)	(2267)
Financial wealth	-31	-228	54	-186	575
	(268)	(505)	(317)	(293)	(649)
Gross wealth	-10730***	-20398***	-6534***	-12329***	-4137*
	(1368)	(2487)	(1635)	(1607)	(2420)
Debt	-1181***	-1134*	-1201***	-1258***	-863
	(381)	(676)	(462)	(441)	(741)
House buy	-0.007***	-0.002	-0.010***	-0.009***	-0.003
	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)
House sell	-0.001	0.000	-0.002	-0.002	0.001
	(0.002)	(0.004)	(0.003)	(0.003)	(0.005)
Move within	0.003	0.006	0.003	0.001	0.011*
	(0.002)	(0.004)	(0.003)	(0.002)	(0.006)
Move out	-0.001	-0.002	0.000	-0.002	0.006**
	(0.001)	(0.003)	(0.002)	(0.002)	(0.003)

Note: This table contains estimates for the average treatment effect over years 0 to 3, where year 0 is the year of the disaster. Each row corresponds to a different outcome variable, as indicated in the leftmost column. Estimates in column (1) correspond to the average treatment effect for the full sample of treated and control households. In columns 2 and 3, the average treatment effect is calculated separately for natural disasters that have a large effect on local firms or on households, respectively. In columns 4 and 5, the average treatment effect is estimated separately for homeowners and for renters, respectively. All amounts correspond to real USD in 2018 prices. Standard errors are in parentheses ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$).

A.6 Robustness analysis

Table A4: Baseline results, different clustering levels

Clustering	Total cons.	Income after tax	Net wealth
Municipality	-365*	-180	-9550**
	(217)	(459)	(3948)
Municipality and matching group	-365*	-180	-9550**
	(210)	(457)	(3942)

Note: This table contains estimates for the average treatment effect over years 0 to 3, where year 0 is the year of the disaster. Each row corresponds to a different level of clustering of standard errors, as indicated in the leftmost column. ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$).

Table A5: Baseline results, for different thresholds and excl. landslide event

	(1) Severity > 6%	(2) Severity < 10%	(3) Without "Lyngen"
Total consumption	-315**	-467***	-441***
	(156)	(139)	(131)
Income after tax	-52	-265***	-220**
	(106)	(97)	(89)
Labor income	-368**	-469***	-528***
	(171)	(155)	(143)
Wealth	-10402***	-11502***	-9847***
	(1771)	(1520)	(1408)
Housing wealth	-9664***	-10517***	-8945***
	(1720)	(1466)	(1346)

Note: This table contains estimates for the average treatment effects over years 0 to 3, where year 0 is the year of the disaster. Each row corresponds to a different outcome variable, as indicated in the leftmost column. Column (1) restricts the estimation sample to events with severity above 6 percent. Column (2) excludes the largest events by restricting the sample to events with severity below 10 percent. Column (3) excludes the 2010 Lyngen landslide from the baseline event sample. Standard errors in parentheses ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$).

Table A6: Average effects of natural disasters, restricted control group

	Baseline	Event Type		Household Type	
Outcome	(1)	Firm event (2)	HH event (3)	Homeowner (4)	Renter (5)
Income after tax	-95	-341*	12	-60	-231
	(99)	(179)	(119)	(115)	(185)
Net wealth	-10434***	-21298***	-5720***	-12138***	-3340
	(1504)	(2825)	(1770)	(1785)	(2422)
Consumption	-321**	-671**	-169	-344**	-235
	(149)	(283)	(175)	(171)	(307)
Labor income	-351**	-737***	-184	-359**	-316
	(151)	(274)	(182)	(178)	(267)
Capital income	-167*	-221	-144	-202*	-32
	(92)	(170)	(110)	(112)	(121)
Self-employed	0.003	0.005	0.002	0.003	0.002
	(0.002)	(0.003)	(0.002)	(0.002)	(0.005)
Unemployed	-0.002	0.007	-0.006	-0.003	0.003
	(0.003)	(0.006)	(0.004)	(0.004)	(0.005)
Housing wealth	-10879***	-21090***	-6447***	-12919***	-2450
	(1526)	(2801)	(1816)	(1803)	(2544)
Financial wealth	-152	-187	-137	-278	343
	(314)	(596)	(368)	(341)	(773)
Gross wealth	-11800***	-22729***	-7057***	-13618***	-4238
	(1612)	(3001)	(1907)	(1902)	(2735)
House buy	-0.007***	-0.002	-0.009***	-0.008***	-0.002
	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)
House sell	-0.002	-0.002	-0.002	-0.003	0.001
	(0.002)	(0.004)	(0.003)	(0.003)	(0.005)
Move within	0.002	0.007	0.001	0.001	0.009
	(0.002)	(0.004)	(0.003)	(0.002)	(0.007)
Move out	-0.000	-0.003	0.001	-0.002	0.006**
	(0.001)	(0.003)	(0.002)	(0.002)	(0.003)

Note: This table contains estimates for the average treatment effect over years 0 to 3, where year 0 is the year of the disaster, for the sample with not more than five households in the control group of each treated household. These are the households in the original set of controls that are closest to the treated households along the consumption and income after tax dimensions, using the L^2 norm. Each row corresponds to a different outcome variable, as indicated in the leftmost column. Estimates in column (1) correspond to the average treatment effect for the full sample of treated and control households. In columns 2 and 3, the average treatment effect is calculated separately for natural disasters that have a large effect on local firms or on households, respectively. In columns 4 and 5, the average treatment effect is estimated separately for households owning a house and for renters, respectively. All amounts correspond to real USD in 2018 prices. Standard errors are in parentheses ($^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$).

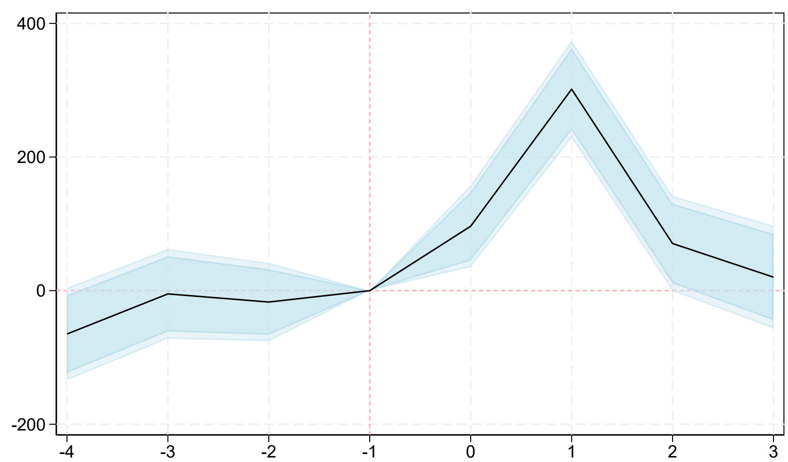
A.7 Adjusting Consumption for Direct Effect

This appendix provides additional evidence on the role of insurance transfers paid directly to households. The purpose is twofold. First, we quantify how much of the direct damage measured in the municipality-level insurance data appears as transfers to households. Second, we examine whether households receiving such transfers display a less negative consumption response, consistent with insurance-financed repair and replacement spending raising measured consumption.

Figure A2 shows the effect of natural disasters on transfers from insurance companies to households. Unlike the municipality-level natural-damage payouts used to define the events, these transfers are observed at the household level, but they include all insurance payments received by the household. The figure shows no differential pre-trends before the disaster. After the event, transfers increase sharply for treated households relative to matched controls, confirming that the municipality-level disaster events are associated with increased insurance payments at the household level. The size of these transfers is small relative to total direct damages, however. Summing the estimated excess transfers over the post-event years and comparing them to the direct damages measured by municipality-level natural-damage payouts, we find that direct transfers to households account for roughly 10% of total insured damages. This is consistent with the institutional setup: in most cases, the insurance company manages the repair process and pays contractors directly. While households may opt to receive a direct payout instead, this option is typically chosen only for small damages, such as losses to movable property.

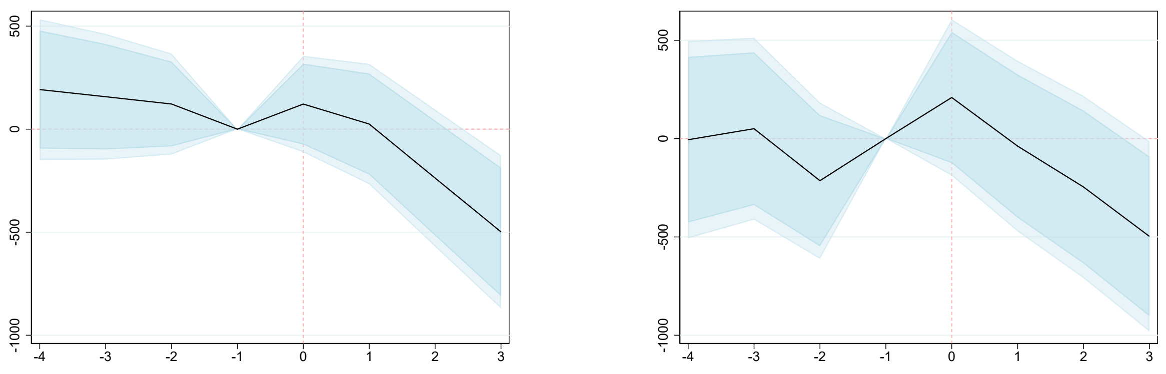
We next examine the consumption response among households that receive direct insurance transfers. This exercise is diagnostic rather than a separate causal estimate for an ex ante subgroup, since receiving an insurance transfer is itself an outcome of the disaster. Nevertheless, it is informative about the sign of the direct spending component. If households use direct insurance transfers to repair or replace damaged property, measured consumption should fall less for transfer recipients than for the full treated sample, especially in the event year and the following year. Figure A3 shows the estimated response of disposable income and consumption for treated households that receive positive transfers from insurance companies in year 0 or year 1, together with their matched controls. The income response is similar to the response in the full sample. By contrast, the consumption response is substantially less negative, particularly in the event year and the following year. This pattern is consistent with a positive direct effect on measured consumption from insurance-financed repair and replacement spending. It motivates the adjusted consumption measure used in the main text, where we subtract excess insurance transfers received by treated households relative to controls.

Figure A2: Insurance transfers to households



Note: The black line shows the estimated coefficients β_k ($k = -4, \ldots, 3$) from Equation 1. The dependent variable is annual transfers from insurance companies to each household. The sample consists of households living in municipalities hit by a natural disaster and matched never-treated control households. The horizontal axis denotes years relative to the disaster year. Coefficients are normalized relative to the year before the disaster ($k = -1$). Amounts are expressed in real 2018 USD. Shaded areas show 90% and 95% confidence intervals.

Figure A3: Consumption and income response for households receiving insurance transfers



(a) Income after tax

(b) Consumption

Note: This figure shows the estimated coefficients β_k ($k = -4, \ldots, 3$) from Equation 1. The dependent variable is income after tax in Panel (a) and total consumption in Panel (b). The sample consists of treated households that receive positive transfers from insurance companies in year 0 or year 1, together with their matched control households. The horizontal axis denotes years relative to the disaster year. Coefficients are normalized relative to the year before the disaster ($k = -1$). Amounts are expressed in real 2018 USD. Shaded areas show 90% and 95% confidence intervals.

A.8 Categorizing natural disasters by damage type: Firm- vs. household-related events

Using the insurance payout data from Finance Norway (see Section 2.1), we can differentiate between damages covered by business insurance and those covered by household insurance. Specifically, we calculate the fraction of damage covered by business insurance relative to the total damage. If this ratio exceeds 50 percent, the event is classified as predominantly affecting firms, and the indicator variable "Firm damage" is set to 1; otherwise, it is set to 0. Using this method, we identify 6 events as firm-related, with the remaining 10 events classified as household-related, see Table A7.

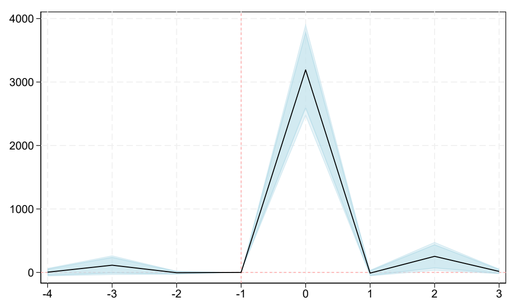
Table A7: Natural disasters by largest impact: Firm vs. household events.

Municipality	**Year**	**Payouts as share of labor income**	**Fraction of firm-related Damage**	**Firm event**
Holtålen	2011	24.8	0.69	1
Nord-Fron	2013	20.1	0.035	0
Lund	2015	16.7	0.87	1
Aurland	2014	12.2	0.20	0
Værøy	2011	10.6	0.63	1
Moskenes	2011	10.6	0.43	0
Røst	2011	10.1	0.53	1
Lyngen	2010	8.5	0.021	0
Nord-Fron	2011	7.8	0.17	0
Kvinesdal	2015	6.8	0.30	0
Ringebu	2011	6.5	0.81	1
Bjerkreim	2015	6.4	0.65	1
Vanylven	2011	6.2	0.44	0
Flakstad	2011	5.45	0.35	0
Stryn	2011	5.08	0.44	0
Sel	2011	5.07	0.30	0

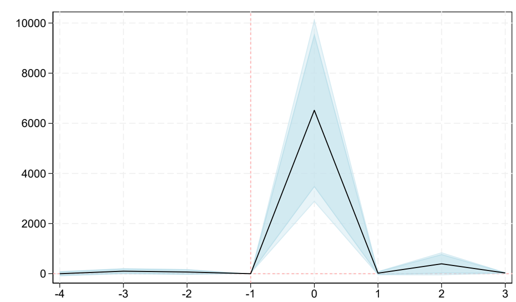
Table A7 shows that the natural events that predominantly affect firms are of similar magnitude as those affecting primarily households: The ratio of payouts to local labor income is 9 percent for household damages and 11.8 percent for firm damages. An alternative classification which considers events where the ratio of business payouts to local labor income exceeds 5 percent leads to the same classification for 6 of these firm events.

We further examine insurance payouts paid under household policies to assess whether the larger effects observed after firm-related events could be explained by greater direct exposure of households. This is an important distinction because total payouts are, on average, larger in firm-related events, see panels (a) and (b) in Figure A4. If households in these municipalities also received larger household-policy payouts, the estimated effects could partly reflect more severe direct damages to households rather than spillovers from firms. Figure A4, panels (c) and (d) show that this is not the case: household-policy payouts per household are not larger following firm-related events. In the absence of spillovers from firms to households, one would therefore expect firm-related events to generate weaker household responses than events in which damages are concentrated under household policies. The fact that the opposite pattern emerges supports the interpretation that the larger effects of firm-related events reflect indirect transmission through local firms and labor markets, rather than mechanically larger direct damages to households.

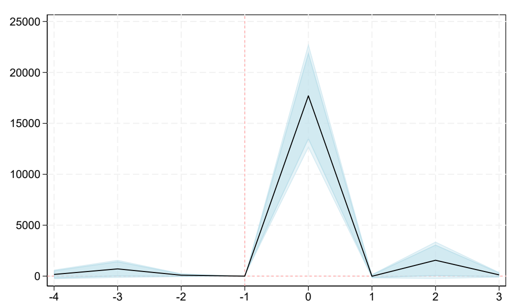
Figure A4: Direct damages per household by event and policy type.



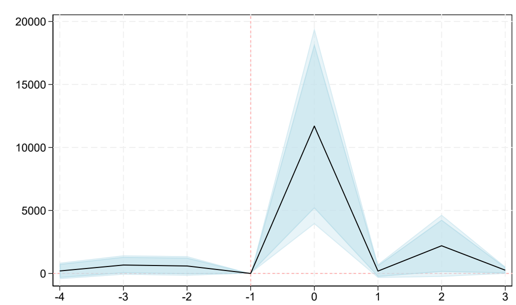
(a) All policies: Household events



(b) All policies: Firm events



(c) Household policies: Household events



(d) Household policies: Firm events

Note: The black line shows the estimated coefficients β_k ($k = -4, \ldots, 3$) from Equation 1. The dependent variable in panels (a) and (b) is total insurance payouts across all policy types per household in the municipality. The dependent variable in panels (c) and (d) is insurance payouts paid under household policies per household in the municipality. Panels (a) and (c) show household-related events, while panels (b) and (d) show firm-related events. The sample consists of households living in municipalities hit by a natural disaster and matched never-treated control households. The horizontal axis denotes years relative to the disaster year. Coefficients are normalized relative to the year before the disaster ($k = -1$). Amounts are expressed in real 2018 USD. Shaded areas show 90% and 95% confidence intervals.