Direct Damage, Indirect Costs: The Incidence of Natural Disasters

Amen Jalal* Pol Simpson‡

July 16, 2026

Abstract

Natural disasters affect not only the locations they physically strike, but also surrounding areas through labour and product market linkages. As a result, the economic incidence of disasters differs from their physical footprint. We argue this in the context of Pakistan's 2022 floods, combining an original panel of 5,100 rural households with satellite and administrative damage data, and a re-centered hydrological instrument to separately identify the impacts of direct and indirect flood exposure. On its own, direct flood exposure causes physical damage but otherwise appears to leave households largely unharmed. Accounting for indirect exposure overturns this picture. A one-standard-deviation increase in surrounding flooding raises unemployment by 7 percentage points and local food prices by 9–18%, and lowers real consumption by 16%. All else equal, direct exposure made a household more likely to receive aid, while having flooded neighbours made it less likely. To ask how disaster aid should instead be allocated, we build a spatial general-equilibrium model, and find substantial welfare gains from targeting economic incidence rather than physical damage. Ignoring indirect exposure both understates the cost of large-scale climate shocks and misallocates the assistance that follows.

*London School of Economics (LSE); u.jalal@lse.ac.uk.

†London School of Economics (LSE); p.simpson1@lse.ac.uk.

‡We gratefully acknowledge funding from IGC, CID at Harvard University, CEPR STEG, and STICERD at LSE. We thank Clare Balboni, Oriana Bandiera, Gharad Bryan, Robin Burgess, Maarten De Ridder and Daniel Sturm for invaluable advice and support. For their outstanding research assistance, we thank Zahra Tajammal, Isra Imtiaz and Maheen Memon. For very helpful comments, we also thank seminar participants at the LSE Economics of Environment and Energy (EEE) hour and LSE Environment Camp.

1 Introduction

It is common to think of the costs of a natural disaster as borne by those it directly hits. Most disaster assistance reflects this view, targeting verifiable physical damage to homes and infrastructure. Yet if there is trade, the costs of a natural disaster can be redistributed across space. For example, flooded households close to unflooded areas can buy goods and find work there. This insures the flooded and creates spillovers onto the unflooded.

In this paper we show that a household's economic loss depends not only on whether it was flooded, but on flooding in the surrounding area (which we term indirect exposure). Ignoring indirect exposure leads us to underestimate the impact of floods, overstate the share due to physical damage, and misallocate disaster assistance.

Our context is Pakistan's 2022 floods, which were caused by the heaviest monsoon in four decades and flooded 7% of the country. We collect an original panel of 5,100 rural poor households, first observed in 2017 then resurveyed one and two years after the floods, which we link to satellite flood imagery and administrative damage records. To separately estimate the causal impact of direct exposure (flooding within 1km) and indirect exposure (1-15km), we use a recentered IV design (Borusyak and Hull, 2023, 2025), comparing equally flood-prone households that happened to experience different flood intensities in 2022 because of where the monsoon rains fell. We find that direct exposure causes physical damage, whilst other economic costs, like lost employment and higher prices, are driven by indirect exposure. It follows that directly flooded households who were also surrounded by flooding are worst affected.

To ask how disaster assistance should be allocated in light of these results, we build a quantitative spatial model with trade and commuting. The model allows us to derive the incidence of any hypothetical flood and the optimal policy for a planner whose objective is to compensate loss. We then quantify the welfare gains from optimal targeting relative to targeting on physical exposure alone.

The core empirical challenge in the paper is estimating the causal effect of floods on household outcomes. Comparing households with high and low flood exposure may lead to biased estimates, since

propensity to flood correlates with confounders like agricultural productivity. We address this by exploiting the deviation of a location's realised flooding in 2022 from its expected flooding. We use a hydrological model to simulate expected flooding under many counterfactual rainfall realisations, subtract this from the models prediction for flooding under 2022 rainfall, and use this recentered prediction as an instrument for satellite-detected flooding in 2022. The approach is intuitively similar to controlling for a propensity score, except that we simulate it using our knowledge of how flooding is assigned, rather than estimating it from data. The identifying assumption is that the pattern of 2022 rainfall was as good as random with respect to unobserved drivers of household outcomes, after conditioning on district fixed effects and expected flooding. Separating direct from indirect exposure requires enough spatial variation in flooding to tell them apart, which we show holds in our setting.

With this approach in hand, we proceed to our empirical results. Restricting attention to the impact of direct flood exposure suggest the flood impacts were largely physical, and compensating damage to homes and infrastructure would be an adequate response. We find that direct exposure destroys homes and property, as expected. But on other economic outcomes – employment, food prices, savings, and credit – directly flooded households appear no worse off than the non-flooded, and on some measures they even appear to be better off.

That conclusion changes dramatically once indirect exposure is taken into account. While own flooding has no detectable effect on employment, households surrounded by one standard deviation (SD) more intense flooding become 7 percentage points (pp) more likely to be unemployed. This is driven by a decline in wage employment: the likelihood of being employed for wages falls by 17 pp, while the probability of self-employment rises by 9 pp. We interpret this shift as evidence of labour rationing, whereby some households displaced from wage work are pushed into lower-quality forms of self-employment, consistent with Breza et al. (2021).

One explanation for why directly exposed households are spared this fate lies in commuting patterns. We find that own flooding raises the share of men commuting more than a kilometre for work by 4 pp, as workers in flooded locations seek employment in less flooded markets elsewhere. By contrast, indirect exposure leaves commuting unchanged, since households surrounded by flooding have fewer nearby alternatives.

The same indirect channel prevails in product markets. A one-SD increase in surrounding flooding raises the price paid by households for meat by 9%, dairy by 18% and fruit by 17%. Direct exposure, in comparison, has no effect on prices once surrounding flooding is accounted for.

How do households cope with these losses? Those surrounded by flooding try to smooth the shock through conventional channels, but indirect exposure makes them 10.4 pp less likely to take a new loan, and raises the probability of having tried and failed to obtain one by 4.9 pp, consistent with credit rationing. Instead, they appear to turn to asset sales to make ends meet, though these estimates are imprecise. Direct exposure has no such effect on borrowing or asset sales. Consumption patterns tell a consistent story. Direct exposure alone appears to lower real expenditure by 5.9%, but this vanishes once surrounding flooding is accounted for. A one-SD increase in surrounding flooding is associated with a 16% decline in real expenditure.

Taken together, these results suggest that a flood's physical damage runs through direct exposure, while its economic costs – lost work, higher prices, tighter credit – operate through indirect exposure. A household hit on both margins, flooded and surrounded by flooding, is most affected.

Yet aid flows do not account for this pattern. Using household self-reports, we find that direct exposure raises the probability of receiving any flood assistance by 10.3 pp and food aid by roughly 4 pp. Indirect exposure, by contrast, *reduces* it – a one-SD increase in surrounding flooding lowers the likelihood of receiving food aid by 11.4 pp. This is consistent with either rationing within a local area, or aid providers being unable to reach locations surrounded by flooding.

To interpret these effects and ask how disaster assistance should be allocated across space in response, we build a spatial general-equilibrium model that combines Eaton-Kortum goods trade with commuting. Workers in the model live in one location, commute to a workplace in another, and buy goods produced anywhere. Thus, their welfare depends on their commuting and goods-market access. A flood does two things to the locations it hits: it lowers their productivity and destroys their housing. Because housing is a non-traded endowment, separable in utility, its loss falls entirely on the household whose home is damaged. Lower productivity, by contrast, raises the cost of goods and lowers wages, and in general equilibrium these changes propagate to prices and wages everywhere.

We calibrate the model's parameters from the literature and our own estimates, and simulate flooding in a stylised geography of identical locations. The model generates results consistent with our empirical findings: physical damage is caused entirely by direct exposure while changes in prices, labour-market outcomes, and welfare are driven disproportionately by indirect exposure.

Using the calibrated model, we ask how a fixed aid budget should be allocated across space. We consider a planner whose objective is to minimise the welfare loss remaining after aid, and who can choose a per-worker transfer for each location. We compare the optimal policy to two rules that target physical damage: a flat transfer to every flooded location, and an access-weighted transfer that falls with distance to dry land, reflecting the harder-to-reach pattern we find in the data.

The optimal policy delivers 8.7% more welfare per dollar than damage targeting, and 12.9% more than the access-weighted rule. It can improve on them in two ways: by varying transfers among the flooded, and by extending aid to unflooded locations. It gives most to the locations bearing the largest welfare losses, typically the interior of the flooded zone, far from dry land. So the share of loss compensated rises with the loss itself, where under a flat transfer it falls. The gains come from reallocating among the flooded at low budgets, and from reaching beyond the flood zone only at much larger ones.

How much targeting improves on damage-based aid depends on the disaster, and on three features in particular. The first is economic integration. When transport costs are high, each location is close to autarky, so a flood generates no spillovers and its incidence coincides with physical exposure — targeting on physical damages is then already optimal. Spillovers, and so the gains from targeting, arise only at the intermediate frictions of real economies. The second is the spatial concentration of the flood, which determines how many of the flooded can still reach dry land. In a dispersed flood most can, so the loss spills onto their dry neighbours and optimal aid reaches outside the flood zone. In a concentrated flood the interior cannot, so it bears the loss itself and optimal aid targets those trapped locations. The third is size. As a flood grows, the productivity loss spreads more evenly across flooded and unflooded locations alike, so their losses converge and physical exposure reveals less about who bears the loss. Together, this shows how much can be gained by targeting aid on economic incidence rather than physical exposure, and when those gains are largest.

By quantifying the relative importance of direct and indirect exposure, we contribute to a large literature on the microeconomic impacts of natural disasters, bringing together two strands. The first estimates the impact of direct exposure on household outcomes, using households in physically unaffected locations as controls (Baseler and Hennig, 2023; Deryugina et al., 2018; Groen et al., 2020; Gallagher and Hartley, 2017; Gignoux and Menéndez, 2016; Kocornik-Mina et al., 2020; Boustan et al., 2020; Kirchberger, 2017; Patel, 2024). The definition of the treatment unit and control group determines the mix of direct and indirect impacts these designs recover. We instead include both explicitly in our design. A second strand shows that extreme weather events in one location have spillover effects on locations or firms not directly hit - through supply chain linkages (Barrot and Sauvagnat, 2016; Carvalho et al., 2021; Balboni et al., 2025), multinational production (Boehm et al., 2019; Castro-Vincenzi, 2024), evacuee labour supply (Fifer McIntosh, 2008) and food markets (Burgess and Donaldson, 2010; Allen and Atkin, 2022). We estimate direct and indirect effects jointly, and quantify the overall incidence of a disaster.

We also contribute to a small literature on the design of disaster assistance, by asking whether physical exposure should be a necessary or sufficient requirement for receiving assistance. There is very little normative work in this area - exceptions include Pople et al. (2026) on the optimal timing of disaster assistance, Fu and Gregory (2019) on reconstruction subsidies with rebuilding externalities, Tarquinio (2023) on rules versus discretion, and Gordon et al. (2025), who show that targeting aid on housing damage favours households who were wealthier to begin with, and who may therefore have better access to other means of smoothing.[1] We step back and ask who is affected in the first place, and provide an actionable method for targeting assistance on that basis. The model turns a flood map into a map of welfare incidence, which could be used to trigger payouts to individuals or localities, overcoming the challenges of observing indirect damage. More broadly, our work speaks to how private insurance, public insurance and other social protection can together help people navigate growing climate risks. Identifying who bears the loss lets us assess this system as a whole, and locate the gaps that new climate protection should fill.

[1] A related literature studies the targeting of transfers to the poor (e.g. Alatas et al., 2012, 2016; Hanna and Olken, 2018; Aiken et al., 2022; Banerjee et al., 2024). By contrast, we consider targeting based on loss after a specific event.

The rest of this paper proceeds as follows. Section 2 describes the 2022 floods and our study region. Section 3 introduces the data. Section 4 sets out the research design and Section 4.4 the first stage. Section 5 presents the results. Section 6 develops the spatial model, derives the optimal allocation of aid, and shows when targeting economic incidence improves most on physical exposure. Section 7 concludes.

2 Pakistan's 2022 Floods

Our setting is Pakistan's 2022 floods, which were the worst in the country's 75 year history.

Heavy rain is not unusual in Pakistan, which experiences a monsoon ever summer. However, in 2022 the cumulative July–August rainfall was the greatest it had been in four decades - 114% above the 1981–2021 average, and 27% above the previous wettest year (2010) (Funk et al., 2015). The rain was concentrated in Sindh, the southernmost province and our study region, where it was 267% above the long-run average, and 44% above its own previous record (2003). It is very likely that climate change contributed to this unprecented event. An attribution study by World Weather Attribution estimated that the rainfall was 75% more intense than it would have been without human-induced global warming (World Weather Attribution, 2022).

The rains caused widespread flooding. According to MODIS satellite imagery (Nigro et al., 2014) 7.3% of normally dry land in Pakistan was inundated at some point between July and December 2022. In Sindh the figure is 20.4%, and in the six districts where we work it reached 41%. Because Sindh is low-lying and flat, the water had nowhere to drain, and large areas of the province stayed inundated for months whilst the waters slowly evaporated.

The floods were extreme by global standards, not only Pakistan's. The Global Flood database of Tellman et al. (2021) collates information on the 906 largest floods worldwide since 2000, taking as its starting point the Dartmouth Flood Observatory's log of floods reported in news and government sources. For each flood, it uses satellite imagery to record the share of national land and population that was inundated. Across these floods, a flood at the 90th-percentile of inundated 2.8% of national land and directly affected 1.7% of national population. Pakistan's 2022 floods covered 7.3% of

the country's land and directly affected 14% of its people, placing this event at the 95th and 99th percentiles respectively[2].

This large-scale event is a compelling setting for our research question. In part this is a matter of power since the larger the event, the more likely we are to detect general-equilibrium impacts. But more importantly, enormous floods are worth understanding in their own right, since it is likely that more of the floods we face in coming years will be large ones. Climate change is making weather extremes more frequent, and a single event increasingly covers more of a country at once.[3] We return to the policy implications of growing disaster scale in Section 6.

3 Data

To estimate the impact of direct and indirect flood exposure, we focus on a single event and exploit cross-sectional variation in flooding around each household. We measure outcomes from an original, geocoded panel of rural households, administrative records of property damage, and satellite measures of crop cover. We measure flood exposure from satellite imagery. We construct our instruments from a hydrological model. Table 3.1 summarises the sources.

Household survey data

We collect panel data on 5,100 poor, rural households in Sindh, first observed in 2017 and surveyed twice after the floods, in summer 2023 and 2024.

The sample is drawn from a census of all low-income households in six districts of the province (Qambar Shahdadkot, Larkana, Matiari, Tando Muhammad Khan, Tando Allahyar and Sujawal). Whilst restricting attention to the rural poor limits our ability to comment on the experience of wealthier or urban households, its key benefit is that we can compare very similar households that happened to face very different flooding.

[2]It is clear from Patel (2024) that many floods, particularly smaller ones, are omitted from the DFO. This suggests we may be underestimating how extreme Pakistan's 2022 floods were in the global context.

[3]The area a single event covers has widened across hazards, most clearly for heat (Skinner et al., 2025) and drought (Brunner and Chartier-Rescan, 2024), and also for tropical-cyclone rainfall (Qin et al., 2024) and European floods (Kemter et al., 2020). In South Asia, both widespread monsoon rainfall (Roxy et al., 2017) and large-scale heat extremes (Banerjee et al., 2025) have become more common.

Table 3.1: Data sources

Data Type	Source	Years	Resolution
Panel A: Flood Exposure Measurement			
Flood extent (optical)	MODIS MCDWD	2022-23	250m, daily
Flood extent (radar)	Sentinel-1	Aug–Dec 2022	20m, multiple dates
Topography	FABDEM	Static	30m
Panel B: Instrumental Variables			
Rainfall	NASA IMERG	1998-2025	0.1°, daily
Topography	FABDEM	Static	30m
Panel C: Household and Damage Data			
Baseline census	NGO admin records	2017	GPS coordinates
Follow-up surveys	Primary collection	2023-24	GPS coordinates
Village census	Pakistan (Mouza) census	2020	Village level
Property damage	Urban Unit (Sindh govt)	2022-23	Structure level
Building footprints	VIDA (Google/Microsoft)	Static	Building level
Panel D: Agricultural output			
Vegetation greenness (NDVI)	MODIS MOD13	2001–2025	500m, monthly
Cropland mask	ESA WorldCover	2021	10m

The data was collected by a local NGO to determine eligibility for development and poverty alleviation programmes and contains the address of the household, a list of household members (including their age, sex, education and occupation), household asset holdings across 21 asset classes and a poverty score constructed from these asset holdings. In addition to treating this data as a sampling frame, we extract baseline covariates from it, which we use as controls to improve precision and to run balance checks.

We sampled to span the range of flood exposure, choosing survey locations within each district with high and low predicted flooding, and then randomly drawing households from them. We surveyed households in two waves, running July–September of 2023 and 2024. With the assistance of local community members, we located 92% of households in 2023 or 2024; of those, almost all were still living in their 2017 village.[4] Attrition from the panel is low and balanced on flood exposure. In total we have 5,120 households and 33,202 people in 2023, and 4,902 households and 33,645 people in 2024. We conduct our main analysis on the 4,683 households surveyed in both waves.

[4]We had expected substantial permanent migration, but did not find this to be the case in our sample. Anecdotally, households expected reconstruction aid conditional on staying in place, and the worst-hit areas are not normally flood-prone, leaving little reason to relocate to reduce future risk.

The survey is extensive, so we highlight a few key modules here. It starts with a household roster covering demographics, education and labour for every member (including employment status, occupation, location of work, wages, temporary migration for work, and recall of pre-flood employment). A consumption module records total, food and health expenditure, and the quantity and value consumed of meat, dairy, fruit and legumes, allowing us to infer prices paid. An assets module records holdings in the same classes as the 2017 census, alongside housing, flood damage, savings and borrowing. We ask which coping strategies a household used to make ends meet, such as travelling further for work, selling assets at a loss or skipping meals. Finally, we record the receipt of flood relief, by type and provider, and self-reported flood exposure, including its duration, depth, and the household's evacuation decisions.

Administrative damage data

After the floods the Government of Sindh conducted an in-person census of damage to residential property, recording 1.96 million damaged buildings across the province. To convert this into a building damage rate, we aggregate it to a hexagonal grid of radius ~460m (resolution 8 of Uber's H3 system), use the VIDA building-footprint layer to count the buildings in each hexagon, and divide the former by the latter. Because the census covers residential property, we exclude sub-cells whose buildings have an average area per-unit of more than $250m^2$, on the basis that they contain industrial property that the census would never record as damaged. We also cap the resulting damage rate at 1, but this binds for only 1.5% of hexagons. Our results are robust to removing both of these data steps.

Agrilcultural output proxies

We proxy agricultural output using satellite data measuring how green the land is. Specifically, we use the Normalised Difference Vegetation Index (NDVI), a widely used proxy for crop output (e.g. Patel 2024). We take monthly NDVI composites from MODIS at 500m, average them within each hexagon, and keep the hexagons that are predominantly cropland according to the ESA WorldCover land-cover map.

Flood data

We measure flood extent and duration using MODIS, an optical sensor that maps standing water at 250m but cannot see through cloud, and Sentinel-1, a radar sensor that sees through cloud and at night but revisits each location less frequently.[5] We measure depth by combining the Sentinel-1 water maps with the FLEXTH algorithm (Betterle and Salamon, 2024). Intuitively, at the edge of a flood the water surface sits at ground level, so the flood boundary reveals the height of the flood water above sea level, and the depth at any interior point is that water level minus the ground elevation. We describe below how these measures are combined into a single index of flood intensity.

Hydrological model inputs

To construct our instruments we use the LISFLOOD-FP hydrological model (Bates et al., 2010; Neal et al., 2012), which takes daily rainfall and topography (a digital map of ground elevation) as inputs. Our rainfall data is satellite based, from NASA's IMERG product, since Pakistan has few ground weather stations. For topography we use FABDEM, which strips buildings and forest canopy from the Copernicus surface model, leaving the ground surface over which water flows, including drainage channels and irrigation canals. FABDEM does not capture large-scale flood defences, but these are largely absent in our setting.

4 Research Design

Estimating the causal effect of flooding on economic outcomes faces identification challenges that could bias results in either direction. Flatter areas are more flood-prone but also more agriculturally productive – leading us to underestimate flood impacts. Conversely, high flood-risk areas may already be poorer due to past climate shocks or selective out-migration of wealthier households – leading us to overstate flood impacts.

We would avoid this bias if we could compare two locations equally prone to flooding, one of which purely by chance flooded in 2022 whilst the other did not. The insight from Borusyak and Hull (2023) is that we can construct this comparison through simulation, because the selection driving

[5] For an in-depth discussion of the challenges of measuring floods from space, see Patel (2024). We study a recent flood, so we can use the latest satellite products, and one that was large and long-lasting, which limits the scope for missing transient inundation between satellite passes.

flooding has a known source. Flooding depends not just on your own rainfall but on all the rain that falls upstream. So a location's flooding in a given year depends on where it sits in the hydrological network, which does not change, and on how much rain fell in each location that year, which does. Knowing this, we can separate a location's underlying flood-proneness from the part of its flooding driven by the year's particular rainfall.

In practice, we use a hydrological model to simulate how much each location would have flooded under many alternative monsoon rainfall patterns. Averaging over these simulations gives each location's expected flooding, whilst the simulation using 2022 rainfall gives its predicted flooding for that year. We instrument satellite-detected flooding with the 2022 prediction, controlling for expected flooding (which plays the role of a propensity score, computed exactly from the model rather than estimated from data). What remains is variation driven by the idiosyncratic rainfall of 2022.

4.1 Definition of flood exposure

To estimate the impact of direct and indirect flood exposure on household outcomes, we must first define flooding at a location, and then direct and indirect exposure for a household.

To do the former, we divide our study area into small hexagons and use satellite data to measure how badly each hexagon flooded. In particular we construct an index from two satellite data sources (MODIS and Sentinel-1), which offset each other's measurement limitations, and covering three dimensions along which floods differ – extent, duration, and depth. Using each source we compute the mean share of land under water in each hexagon for each month from July to December. From the Sentinel-1 data and the FLEXTH algorithm described above we estimate maximum depth in each cell over the same period. This gives thirteen flood measures per hexagon: six monthly flood-shares from each satellite, and one depth. We combine the thirteen measures into a single index by taking their first principal component.

Since we wish to test whether outcomes depend only on a household's own flooding, or also on flooding in the surrounding area through its effect on local markets, we construct two measures of

exposure. Write $Flood_j$ for the flood index at hexagon j and d_{ij} for the distance from household i to hexagon j, we then define the following:

Direct exposure D_i is the average index over the hexagons within a radius r of where the household lived, each weighted equally:

$$D_i = \frac{1}{n_i} \sum_{j\,:\,d_{ij} \le r} Flood_j, \qquad n_i = \#\{\, j : d_{ij} \le r \,\}. \tag{1}$$

Defining indirect exposure means taking a stance on which other locations' flooding matters for a household living in location i. Trade costs imply that, all else equal, closer locations will matter more than distant ones, so we use a distance-weighted average of flooding in the surrounding area.

Indirect exposure S_i is a weighted average of the index over hexagons beyond that neighbourhood, with weights that decay with distance:

$$S_i = \frac{\sum_{j\,:\,d_{ij} > r} e^{-(d_{ij}-r)/\lambda}\, Flood_j}{\sum_{j\,:\,d_{ij} > r} e^{-(d_{ij}-r)/\lambda}}. \tag{2}$$

The measure has two parameters, the own radius r and the decay scale λ (which we summarise by its half-mass h, the distance within which half the total indirect weight falls).

In our main specification we set the cutoff radius for own exposure at approximately 1.1km. For indirect exposure, we average over locations between 1.1km and 15km from the household. We set the decay parameter at $\lambda \approx 4$ which implies that a hexagon at 4km has half the weight of a hexagon just beyond 1.1km, and that half of the weight of the indirect exposure measure is on hexagons within $\approx$ 7km of the household. Figure 9 in the appendix plots the resulting weights against distance.

These choices reflect a trade-off. Extending the indirect measure further out lowers its correlation with direct exposure, which yields more variation with which to separate their effects. Extending it too far leaves indirect exposure with little variation across households, which costs power. Our results are not sensitive to small changes in any of these parameters. Large changes do move them, but this is to be expected: widening the direct radius to 5km, say, would relabel as direct a subset of

the flooding we currently call indirect, so effects that load on indirect exposure would load on direct instead.

4.2 Recentered IV approach

We would like to estimate the impact of direct and indirect exposure to flooding on household economic outcomes.

$$Y_{i,t} = \beta_D \, D_i(\mathbf{Flood}_{2022}) + \beta_S \, S_i(\mathbf{Flood}_{2022}) + \delta_d + \varepsilon_{i,t}, \tag{3}$$

where $\mathbf{Flood}_{2022}$ is the vector of flooding across all hexagons, $D_i(\cdot)$ and $S_i(\cdot)$ are the direct and indirect exposure defined above, δ_d is a district fixed effect, and $\varepsilon_{i,t}$ collects the unobserved determinants of Y.

The challenge is that flooding is not randomly assigned, so D_i and S_i may be correlated with unobserved determinants of Y. We address this by making the process that determines D_i and S_i explicit, so that we can control for selection.

In stylised form, flooding in each location at time t depends on the rainfall that has fallen at its own location this period, the rainfall that has fallen upstream in previous periods, and the hydrological network F, which determines where water flows and accumulates:

$$\mathbf{Flood}_t = \mathbf{r}_t + F\mathbf{r}_{t-1} + F^2\mathbf{r}_{t-2} + \ldots + \boldsymbol{\nu}_t = \sum_{l=0}^{\infty} F^l \mathbf{r}_{t-l} + \boldsymbol{\nu}_t \tag{4}$$

Here $\mathbf{Flood}_t$ is an $N \times 1$ vector of flooding across locations and $\mathbf{r}_{t-l}$ is the vector of rainfall l periods earlier. F is the $N \times N$ flow matrix: F_{ij} is the share of water that moves from location i to location j each period, determined by topography and by drainage infrastructure such as canals and embankments.

We simulate flooding with LISFLOOD-FP, which produces daily predictions of flood depth and extent over approximately 150 days, yielding over 300 potential predictors of satellite flooding per grid cell. Rather than selecting a single summary measure ad hoc, we use a random forest to find the combination of hydrological outputs that best predicts our satellite-detected flood index – the optimal

instrument in the sense of Chamberlain (1987), following Borusyak and Hull (2025). To avoid the instrument being fitted to idiosyncratic noise in each household's satellite measure (Chernozhukov et al., 2018), we cross-fit, partitioning the grid cells into five spatially contiguous folds and, for each fold, train the random forest on the remaining four before predicting for the held-out fold.

We write the resulting predicted vector of flooding under rainfall $\mathbf{r}$ as $g(f(\mathbf{r}))$. $f(\cdot)$ is the hydrological model, the empirical counterpart of F above, which takes rainfall and topography and gives daily predicted flood depths and extent. The random forest $g(\cdot)$ combines these into the best predictor of our flood index.

Our instruments are the predicted direct and indirect exposure, each recentered by subtracting its value under historical average rainfall $\bar{\mathbf{r}}$[6].

$$\tilde{z}_i^{\text{direct}} = D_i\big(g(f(\mathbf{r}_{2022}))\big) - D_i\big(g(f(\bar{\mathbf{r}}))\big), \tag{5}$$

$$\tilde{z}_i^{\text{indirect}} = S_i\big(g(f(\mathbf{r}_{2022}))\big) - S_i\big(g(f(\bar{\mathbf{r}}))\big). \tag{6}$$

The counterfactuals hold $D_i(\cdot)$, $S_i(\cdot)$, $f(\cdot)$, and $g(\cdot)$ fixed and vary only rainfall $\mathbf{r}$. The subtracted terms, $D_i(g(f(\bar{\mathbf{r}})))$ and $S_i(g(f(\bar{\mathbf{r}})))$, are each household's expected direct and indirect exposure.

In addition, we add controls chosen by post-double-selection lasso (Belloni et al., 2014) to improve precision, drawn from the predetermined 2017 baseline characteristics. Point estimates are virtually unchanged without the addition of these controls.

4.3 Identifying assumption

Through the lens of the discussion above we can see that a location can be more flood-prone for several reasons: it typically gets more rainfall, it has a lot of upstream cells, or the cells upstream of it typically get a lot of rainfall. Our approach collects all of this into expected exposure, and controls

[6] In the current version we treat the hydrological model as approximately linear, recentering with its prediction under historical average rainfall rather than the average of its predictions across many historical rainfall realisations, which is computationally costly. Tests on a subsample suggest the difference is very small.

for it. Identification therefore comes from how far 2022's upstream rainfall departed from the typical pattern, in either direction.

Our IV is valid under the assumption that, conditional on expected exposure and district fixed effects, rainfall at any location is unrelated to the unobserved determinants of outcomes at the locations it feeds. Because our instrument varies only with rainfall, unobserved determinants of realised flooding – embankment breaches, local drainage failures – do not threaten identification unless they are themselves correlated with the 2022 rainfall deviation. Formally, writing $\text{up}(i)$ for the set of locations whose water reaches i, and collecting district fixed effects and expected exposure in ω,

$$\mathbb{E}\left[\varepsilon_{i,t} \,\middle|\, \{r_{k,2022}\}_{k\in\text{up}(i)},\ \omega\right] = \mathbb{E}[\varepsilon_{i,t} \mid \omega]. \tag{7}$$

Our instrument is a function only of rainfall and terrain. If there are features of the landscape which drive flooding but which our model does not capture, these do not enter our predictions. We think of this primarily as a relevance issue.

Standard errors

We report Conley (1999) standard errors, which allow the error term to be spatially correlated up to a chosen distance cutoff. Intuitively, we want to permit correlation at the scale at which treatment is assigned. Our treatment is generated by a single rainfall shock propagating through the hydrological network, so the relevant scale is the distance over which the instrument itself remains spatially correlated. We read this off the instrument's correlogram, correlating each hexagon's instrument value with those of its neighbours at increasing distances, and set the cutoff at the distance where that correlation falls to zero. The appropriate 'cutoff' will vary across regressions depending on the sample and treatment definition and is reported in all results tables.

4.4 First stage

The first stage regresses each of the satellite-based direct and indirect exposure measures on both recentered predictions and district fixed effects. Figure 1 plots each household's predicted direct

and indirect flood exposure with (right) and without (left) district fixed effects. This is the identifying variation used in our regressions. The correlation between direct and indirect exposure is 0.58 without district fixed effects and 0.63 with them.

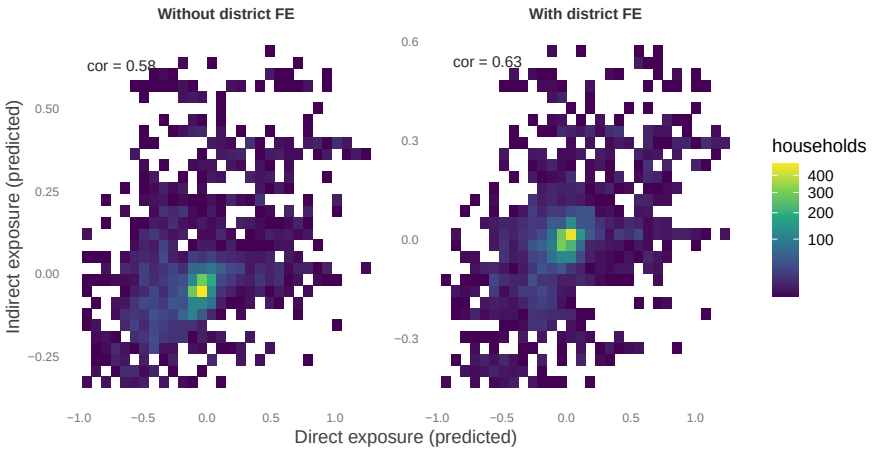


Figure 1: Joint density of predicted direct and indirect exposure

Notes: Bivariate histogram of household-level predicted (instrument) direct and indirect flood exposure, for the balanced panel of 4,683 households. Left: raw. Right: after residualising on district fixed effects.

Table 4.1 shows the first stage. Each instrument predicts its own exposure measure strongly, and the other only weakly, so the two exposures are separately identified.

Table 4.1: First stage

	Direct exposure (satellite)	Indirect exposure (satellite)
Direct exposure (Hydro IV)	1.31***	-0.03**
	(0.21)	(0.01)
Indirect exposure (Hydro IV)	-0.43*	1.30***
	(0.25)	(0.11)
Partial F on direct	37.6	6.5
Partial F on indirect	2.9	150.5
Conley cutoff (km)	14.3	14.3
N	4,683	4,683

Notes: First-stage regressions of each exposure measure (direct, indirect) on both instruments, the hydrological model's predicted flooding for the own and indirect areas. Partial F is the squared t-statistic on the matching instrument. Own-area radius $r \approx 1.1$ km, kernel half-mass $h \approx 7.1$ km. Balanced panel of households; district fixed effects; Conley standard errors. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5 Empirical results

This section establishes three results. Direct, physical exposure to flooding causes physical damage. But other economic losses, such as lost employment, higher prices and lower consumption, are driven by indirect exposure. Aid rises with direct exposure and falls with indirect exposure.

5.1 Direct exposure to flooding causes physical damage

We begin with physical damage. We use the hexagon-level damage rate constructed from the administrative records, an objective measure of physical destruction independent of what households told us. The sample is every hexagon with a non-zero building count in the union councils where we surveyed. Direct exposure to flooding destroys property - a one-standard-deviation increase in flood intensity within 1.1km raises the building-damage rate by 6.3 percentage points (Table 5.1, first row). This compares to a control mean of 39%, calculated as the mean outcome for households

in the bottom 10% of direct exposure. Restricting to hexagons above the median building count, where the damage rate is measured more precisely, raises the own effect to 7.2 percentage points.

Table 5.1: Damage placebo

	Any building damage	Any damage (above-median density)
Direct	6.29**	7.24***
	(2.54)	(2.39)
Direct	6.56**	7.38***
	(2.82)	(2.51)
Indirect	-0.65	-0.34
	(3.43)	(3.03)
Control mean (%)	38.9	36.4
N (hexes)	4,429	2,222
Conley cutoff (naive spec)	12.0	12.0
Conley cutoff (joint spec)	18.0	16.0

Notes. 2SLS of the res-8 hexagon building-damage rate (damaged buildings / VIDA buildings, capped at 1) on flood exposure; building-weighted, district fixed effects, Conley standard errors. Columns: all hexagons; hexagons above the median building count. The Direct coefficient is shown own-only (top) then jointly with Indirect (bottom). Coefficients are in percentage points, per one standard deviation of the respective exposure. Control mean: the lowest decile of predicted own flooding. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

The second panel of Table 5.1 reports the results when we include both direct and indirect exposure in the regression. Including indirect exposure makes very little difference to the coefficient on direct exposure. Moreoever, in both columns the coefficient on indirect exposure is small and far from significant, meaning that floodwaters more than a kilometre away do not damage housing.

The latter result may seem obvious, but that is exactly what makes it a useful test of our design. As raised by Huber (2023), attempts to estimate spillover effects can be biased in the presence of measurement error. If a household's own flooding is mismeasured, the flooding of its neighbours may proxy for the flooding the household actually experienced, biasing the indirect coefficient away from zero even when there are no true spillovers. Physical damage is the one outcome for which we can be confident that we should not find a non-zero effect of indirect exposure, because it is hard to see how water several kilometres away could destroy a building. Finding a precise null therefore

gives us confidence that when we find an impact of indirect exposure on other outcomes, this reflects a genuine spillover effect rather than measurement-error-induced bias.

Another major asset the floods destroyed was the crop that had been planted and not yet harvested. Sindh has two growing seasons, the summer Kharif and the winter Rabi, with peak output in September and February respectively. To look at the impact on peak agricultural output we use a proxy, satellite-measured greenness (NDVI), which captures how much green vegetation covers the land.

Panel A of Table 5.2 regresses NDVI on direct and indirect exposure, using as the sample every cropland hexagon in the union councils where we surveyed, and controlling for the hexagon's 2016–2020 average greenness in that calendar month. Direct exposure sharply reduces greenness in the flooded Kharif, by 0.136 per standard deviation of flood intensity, against a control mean of 0.508 – a fall of 27%. The effect is smaller in the following Rabi, at 0.013 (2%), and gone by the next Kharif. Adding indirect exposure barely moves the direct coefficients, though it makes the effect on Rabi peak output insignificant.

Panels B and C split the sample by the crop grown before the flood. Rice is hit harder but recovers faster, losing 31% of baseline greenness in the flooded Kharif and nothing thereafter. Cotton loses less and stays depressed for longer, at 10% in the flooded Kharif, 8% in the following Rabi and 3% in the next Kharif. Here the indirect effects are sometimes significant, and in different directions. This could reflect changes in prices, or in the availability of inputs.

5.2 Physical exposure does not coincide with economic incidence

We have shown that physical damage to housing and crops was driven largely by direct exposure, and in the case of housing exclusively so. We turn next to other economic outcomes, where the impacts are largely driven by indirect rather than direct exposure.

The floods destroyed one season's crop and waterlogged fields into the next. They may have had persistent impacts on following seasons, through soil quality, damage to agricultural equipment, or farmers' ability to borrow and hire after large losses.A shock of this type would likely depress the

Table 5.2: Impacts on NDVI (agricultural output proxy)

	Sep 2022 (flood)	Feb 2023 (next Rabi)	Sep 2023 (next Kharif)
Panel A. All crops			
Direct (own only)	-0.136***	-0.013**	0.000
	(0.020)	(0.006)	(0.004)
Direct	-0.134***	-0.011	-0.001
	(0.021)	(0.007)	(0.004)
Indirect	-0.004	-0.007	0.003
	(0.012)	(0.013)	(0.010)
Control mean	0.508	0.599	0.646
N	4,075	4,087	4,087
Panel B. Rice			
Direct (own only)	-0.166***	-0.007	-0.007*
	(0.014)	(0.007)	(0.004)
Direct	-0.164***	-0.002	-0.003
	(0.014)	(0.007)	(0.004)
Indirect	-0.005	-0.017*	-0.012*
	(0.006)	(0.010)	(0.007)
Control mean	0.540	0.622	0.681
N	2,705	2,711	2,711
Panel C. Cotton			
Direct (own only)	-0.046***	-0.038***	-0.006
	(0.014)	(0.004)	(0.004)
Direct	-0.044***	-0.041***	-0.013***
	(0.011)	(0.006)	(0.003)
Indirect	-0.004	0.006	0.018***
	(0.008)	(0.009)	(0.001)
Control mean	0.440	0.546	0.499
N	546	546	546

Notes. 2SLS of hexagon-level satellite greenness (MODIS NDVI, level) on flood exposure, for cropland hexagons in the survey union councils. Panel A pools all cropland; Panels B and C restrict to hexagons growing rice and cotton before the flood. Columns are three points in the crop calendar: the flooded Kharif (September 2022), the following Rabi (February 2023) and the next Kharif (September 2023). Each regression controls for the hexagon's 2016–2020 average greenness in that calendar month; the crop panels additionally control for its GAEZ irrigated attainable yield for that crop. District fixed effects, Conley standard errors. The Direct coefficient is shown own-only (top) then jointly with Indirect (bottom), each per one standard deviation of the respective exposure. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

demand for agricultural labour in flooded locations for some time, and, in reducing output, lower the supply of food from these areas.

Under autarky, these effects would be confined to the places which were directly flooded. The fall in agricultural labour demand would lower wages and employment there, whilst the loss of local output would raise local food prices. However, if there is trade, then a productivity or supply shock in one place can be passed through to others, and physical exposure and economic incidence need not coincide.

Labour Markets

We turn first to labour markets. Each outcome is measured at the household level, as an average across its working-age men.[7] Restricting attention to direct exposure alone (first row, Panel A, all households), we find no significant effect of own flooding on the probability that men are in any employment, nor a significant change in the composition of their work. Direct exposure does, however, raise the probability of commuting more than a kilometre to work (i.e. beyond what we treat as the household's own location) by 4 percentage points per standard deviation of flood intensity, against a control mean of 42%.

Taken on their own, these estimates might suggest the floods left the labour market largely unaffected, or that workers were easily able to commute a little further to offset any local reductions in labour demand. However, when we additionally control for indirect exposure, we find a very different picture. Direct exposure still has no significant effect on employment. But a one-standard-deviation increase in flood intensity in the surrounding area reduces the probability of any employment by 7 percentage points, the net result of a 17-point reduction in wage employment – primarily daily labour in this setting – partly offset by a 9-point increase in self-employment.

If your own location floods but the area around you does not, your employment is unaffected. If the area around you floods, it falls, whether or not you were flooded yourself. Households at the centre of large areas of inundation are therefore hit twice: their house is destroyed, and they cannot find work.

[7] We drop women from our sample because the female labour force participation in Pakistan is very low – around 20% (Jalal, 2026; Bandiera et al., 2025).

Table 5.3: Household male labour supply (shares of adult men)

	Any employment	Wage employment	Self employment	Commutes >1 km
Panel A. All households ($N = 4,478$)				
Direct	-0.00	-0.02	0.02	0.04**
	(0.02)	(0.03)	(0.02)	(0.02)
Direct	0.02*	0.03*	-0.01	0.05***
	(0.01)	(0.02)	(0.02)	(0.01)
Indirect	-0.07*	-0.17***	0.09***	-0.02
	(0.04)	(0.06)	(0.03)	(0.05)
Control mean	0.81	0.66	0.17	0.42
Panel B. Pre-flood agricultural households ($N = 1,830$)				
Direct	-0.02	-0.06*	0.04**	0.02
	(0.02)	(0.03)	(0.01)	(0.03)
Direct	0.03**	0.04**	-0.00	0.06***
	(0.01)	(0.02)	(0.03)	(0.02)
Indirect	-0.16***	-0.29***	0.12***	-0.11
	(0.05)	(0.06)	(0.04)	(0.08)
Control mean	0.83	0.50	0.34	0.27
Panel C. Pre-flood non-agricultural households ($N = 2,648$)				
Direct	0.01	-0.01	0.01**	0.06***
	(0.01)	(0.01)	(0.01)	(0.02)
Direct	0.01	-0.01	0.01**	0.03*
	(0.01)	(0.01)	(0.01)	(0.02)
Indirect	0.00	-0.00	-0.01	0.11
	(0.03)	(0.03)	(0.01)	(0.07)
Control mean	0.79	0.75	0.04	0.54
Conley cutoff (naive spec)	9.4	9.4	9.4	9.4
Conley cutoff (joint spec)	14.3	14.3	14.3	14.3
SD of Direct	1.00	1.00	1.00	1.00
SD of Indirect	0.72	0.72	0.72	0.72

Notes. 2SLS regressions of the share of adult men in each labour status (distance: mean over all adult men) on flood exposure, controlling for the number of adult men. Three panels: all households; pre-flood all-agricultural households; the remaining households with an adult man. Each panel's N is in its title; the control mean (lowest decile of predicted own flooding) is the last row. Both "Direct" lines and the "Indirect" line add controls chosen by post-double-selection lasso from the predetermined SUCCESS 2017 baseline; district fixed effects and the instruments are always kept. "Direct (own only)": own exposure alone. "Direct"/"Indirect": own and indirect jointly. Conley cutoffs and regressor SDs (shared across panels) are in the footer. Coefficients are per one standard deviation of own flooding at r=1.1 km. Conley standard errors. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

The labour effects we estimate are measured a year after the water receded, and satellite greenness suggests crop cover had largely recovered by then (Table 5.2). What appears not to have recovered is the demand for hired labour. This is consistent with landowners entering the following summer without the working capital to hire, as a result of one lost and one delayed growing season.

These effects show considerable heterogeneity by the type of work a household was doing before the floods. If we split households into those whose working-age men all worked in agriculture before the floods and those working in other sectors, we see that the results are driven almost entirely by the agricultural households (Panel B): for them, the same increment of surrounding flooding lowers any employment by 16 percentage points and wage employment by 29, partly offset by a 12-point rise in self-employment. Among the rest, the effects on employment are essentially zero, their only response being a statistically insignificant 11-point rise in commuting beyond a kilometre (Panel C). This suggests the floods hit agricultural jobs hardest, and that agricultural workers could not readily move into other work. Were they able to, we would expect the reduction in employment to be independent of the type of work men were doing prior to the floods.

Goods Markets

Our household surveys asked about consumption of four food groups over the past week – meat, dairy, fruit and legumes. For each, households reported both the quantity consumed, in kilograms, and its value, in rupees. Dividing value by quantity gives a geocoded consumer price for each good.

Table 5.4 shows that, among households reporting positive consumption of a given food, direct flood exposure has negligible effects on its price – the coefficients are small and insignificant – whereas indirect exposure raises the prices of meat, dairy and fruit. The same increment of surrounding flooding raises meat prices by around Rs 52 per kilogram (about 9% of the control mean), dairy by Rs 38 (18%) and fruit by Rs 34 (17%); the fruit effect is the most precisely estimated, significant at the 1% level, with dairy significant at 5% and meat at 10%, while legumes show no significant response, perhaps due to being more storable and transportable than the other categories.

In robustness checks, these effects strengthen as we place more weight on locations further from the household, suggesting they are driven by market conditions in a broader geographic area – though

our power to detect such regional effects is more limited. This is consistent with lower trade costs, or greater market integration across locations, in goods than in labour.

To foreshadow our model, these results are consistent with a world in which each location produces very similar goods and is small relative to the regional economy. In such a world flooding in the local area alone is not enough to move prices, whereas more regional flooding can.

Table 5.4: Food prices (Rs/kg)

	Meat (Rs/kg)	Dairy (Rs/kg)	Fruit (Rs/kg)	Legumes (Rs/kg)
Direct	6.6	12.6*	13.5*	-4.7
	(9.7)	(7.3)	(7.0)	(3.5)
Direct	-8.5	1.1	3.3	-3.4
	(8.7)	(6.5)	(4.4)	(4.4)
Indirect	52.2*	38.3**	33.5***	-5.2
	(31.4)	(19.4)	(12.5)	(7.7)
Control mean	583.03	211.45	200.44	329.79
N	1,363	3,605	1,182	3,910
Conley cutoff (naive spec)	9.4	9.4	9.4	9.4
Conley cutoff (joint spec)	14.3	14.3	14.3	14.3
SD of Direct	1.00	1.00	1.00	1.00
SD of Indirect	0.72	0.72	0.72	0.72

Notes. 2SLS regressions of household food unit values (Rs/kg, winsorised at p99) on flood exposure, one column per good. Both panels add lasso-selected SUCCESS 2017 baselines; district fixed effects and the instruments are always kept. Top panel: own exposure only. Bottom panel: own and indirect jointly. Each good's *N* is the households buying it. Coefficients are per one standard deviation of own flooding at r=1.1 km. Conley standard errors. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

Consumption and coping

How did households fare given these changes in employment and prices?

Consider direct exposure alone. Total expenditure shows no significant change, but the local price index rises, so real expenditure falls by 5.9% (Table 5.7). On the balance sheet, the directly exposed appear better placed, being around 8,500 rupees less indebted and having 8,900 rupees higher net savings (Table 5.6). Taken at face value, these results suggest a shock that imposed modest consumption losses on the directly exposed while leaving their financial position, if anything, stronger.

Table 5.5: Dissaving: asset sales, savings, and borrowing

	Sold HH assets	Sold productive assets	Spent savings	Took new loans	Tried to borrow, failed
Direct	0.017	-0.002	0.003	-0.035**	-0.003
	(0.013)	(0.019)	(0.021)	(0.017)	(0.025)
Direct	0.003	0.003	0.011	-0.004	-0.018
	(0.010)	(0.020)	(0.024)	(0.016)	(0.019)
Indirect	0.048	-0.014	-0.030	-0.104***	0.049*
	(0.030)	(0.073)	(0.058)	(0.029)	(0.029)
Control mean	0.11	0.24	0.68	0.61	0.47
N	4,683	4,683	4,683	4,683	4,683
Conley cutoff (naive spec)	9.4	9.4	9.4	9.4	9.4
Conley cutoff (joint spec)	14.3	14.3	14.3	14.3	14.3
SD of Direct	1.00	1.00	1.00	1.00	1.00
SD of Indirect	0.72	0.72	0.72	0.72	0.72

Notes. 2SLS regressions of dissaving indicators on flood exposure, all households. Sold productive / HH assets are coping strategies (8) and (10); sold either combines them; spent savings is (3); took new loans is (4); savings or new loans is (3) or (4); tried to borrow and failed is a separate question. Both "Direct" lines and "Indirect" add lasso-selected SUCCESS 2017 baselines; district fixed effects and the instruments are always kept. Top: own exposure only. Bottom: own and indirect jointly. Control mean: the outcome in the lowest decile of predicted own flooding. Coefficients are per one standard deviation of own flooding at r=1.1 km. Conley standard errors. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

Adding indirect exposure changes the picture. Direct exposure no longer moves any balance-sheet outcome, with savings, borrowing and net position all indistinguishable from zero (Table 5.6). Its effect on consumption is likewise weak, with real expenditure falling by 1.2% and no longer significant (Table 5.7).

Table 5.6: Savings, loans, and assets

	Has savings	Has loans	Asset index	Savings (000 Rs)	Loans (000 Rs)	Asset value (000 Rs)	Savings − loans (000 Rs)	Savings + assets − loans (000 Rs)
Direct	0.05	-0.03***	-0.05	0.54	-8.50**	-1.01	8.89**	7.42
	(0.03)	(0.01)	(0.06)	(0.63)	(3.52)	(5.46)	(3.70)	(5.49)
Direct	-0.00	-0.00	-0.01	-0.53	-3.61	3.02	3.21	6.34
	(0.02)	(0.02)	(0.05)	(0.48)	(4.03)	(10.86)	(4.07)	(11.19)
Indirect	0.18	-0.09*	-0.11	3.77*	-15.63*	-15.30	18.42*	1.16
	(0.12)	(0.05)	(0.13)	(2.11)	(9.17)	(28.96)	(10.04)	(36.54)
Control mean	0.20	0.77	0.17	3.65	95.31	124.83	-92.06	34.84
N	4,683	4,683	4,683	4,683	4,683	4,683	4,683	4,683
Conley cutoff (naive spec)	9.4	9.4	9.4	9.4	9.4	9.4	9.4	9.4
Conley cutoff (joint spec)	14.3	14.3	14.3	14.3	14.3	14.3	14.3	14.3
SD of Direct	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
SD of Indirect	0.72	0.72	0.72	0.72	0.72	0.72	0.72	0.72

Notes. 2SLS regressions of household balance-sheet outcomes on flood exposure, all households. Has savings / has loans are indicators and the asset index is the standardised ownership index; savings, loans (new borrowing), the valued movable assets (the ownership-index items priced at 2023 values, land excluded), and the net measures are winsorised levels in thousands of rupees. Both "Direct" lines and "Indirect" add lasso-selected SUCCESS 2017 baselines; district fixed effects and the instruments are always kept. Top: own exposure only. Bottom: own and indirect jointly. Control mean: the outcome in the lowest decile of predicted own flooding. Coefficients are per one standard deviation of own flooding at r=1.1 km. Conley standard errors. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

The balance-sheet response is driven instead by indirect exposure. The indirectly exposed are 10.4 percentage points less likely to take a new loan, against a control mean of 61% (Table 5.5), and borrow around 15,600 rupees less (Table 5.6). This seems to reflect credit constraints rather than a lack of desire to borrow, since indirect exposure also raises the probability of having tried and failed by 4.9 percentage points, against a control mean of 47%.

Unable to borrow, they seem to draw on their assets instead, becoming 4.8 percentage points more likely to sell household durables, around 44% above a baseline of 11%, though this effect is not statistically significant. The directly exposed show no comparable coping. The net savings position of the indirectly exposed rises by about 18,400 rupees, but only because the borrowing they sought was unavailable. Net worth, savings and movable assets net of loans, is unchanged.

Consumption losses are largest for the indirectly exposed. Indirect exposure raises the local price index by 9.7% and lowers real expenditure by 16%, against 1.2% on the direct margin (Table 5.7). Nominal expenditure falls too, by 7.4%, though imprecisely.

Table 5.7: Consumption

	Total exp	Food share of exp	Health share of exp	Price index	Real total exp
Direct	-0.023	0.014***	-0.006	0.038*	-0.059*
	(0.026)	(0.003)	(0.011)	(0.020)	(0.033)
Direct	-0.002	0.013***	-0.019*	0.009	-0.012
	(0.041)	(0.004)	(0.010)	(0.015)	(0.050)
Indirect	-0.074	0.001	0.049	0.097**	-0.159*
	(0.085)	(0.012)	(0.040)	(0.050)	(0.090)
Control mean	9.28	0.21	0.89	-0.40	9.67
N	4,683	4,681	4,681	4,536	4,536
Conley cutoff (naive spec)	9.4	9.4	9.4	9.4	9.4
Conley cutoff (joint spec)	14.3	14.3	14.3	14.3	14.3
SD of Direct	1.00	1.00	1.00	1.00	1.00
SD of Indirect	0.72	0.72	0.72	0.72	0.72

Notes. 2SLS regressions of household consumption on flood exposure, all households. Columns: log total expenditure; the food and health expenditure shares; the food price index; and price-deflated real total expenditure (log total expenditure minus the price index). Both "Direct" lines and "Indirect" add lasso-selected SUCCESS 2017 baselines; district fixed effects and the instruments are always kept. Top: own exposure only. Bottom: own and indirect jointly. Control mean: the outcome in the lowest decile of predicted own flooding. Coefficients are per one standard deviation of own flooding at r=1.1 km. Conley standard errors. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

5.3 Aid tracks physical exposure, not indirect exposure

How did the allocation of aid after this disaster compare to the pattern of economic damage we have documented?

Using our survey data on self-reported receipt of disaster relief in the year after the flood, we find that directly exposed households were 10.3 percentage points more likely to receive some form of formal flood relief, and 4.0 points more likely to receive food aid, against control means of 49 and 23 percent.

Table 5.8: Aid receipt

	Any flood relief	Any food aid
Direct	0.078***	0.006
	(0.024)	(0.020)
Direct	0.103***	0.040*
	(0.033)	(0.022)
Indirect	-0.084	-0.114**
	(0.058)	(0.056)
Control mean	0.49	0.23
N	4,318	4,683
Conley cutoff (naive spec)	9.4	9.4
Conley cutoff (joint spec)	14.3	14.3
SD of Direct	1.00	1.00
SD of Indirect	0.72	0.72

Notes. 2SLS regressions of household aid receipt on flood exposure: indicators for any flood relief and any food aid. Both panels add lasso-selected SUCCESS 2017 baselines; district fixed effects and the instruments are always kept. Top panel: own exposure only. Bottom panel: own and indirect jointly. Control mean: the outcome in the lowest decile of predicted own flooding. Coefficients are per one standard deviation of own flooding at r=1.1 km. Conley standard errors. $^{*}p < .1$, $^{**}p < .05$, $^{***}p < .01$.

Indirect exposure pushes in the opposite direction. More indirectly exposed households are 11.4 points less likely to receive food aid ($p < 0.05$) and 8.4 points less likely to receive any flood relief ($p \approx 0.15$). This is consistent with rationing within a region, where holding your own flooding fixed, more flooding around you means less assistance, or with aid struggling to reach locations surrounded by water (reflecting anecdotal evidence that aid flows to 'easier to reach' locations). Either way, a household that was flooded and surrounded by flooding received less than one that was flooded and surrounded by dry land.

6 A model of disaster incidence

The empirical results show that the incidence of a flood departs from its physical footprint, but aid did not follow incidence. How, then, should aid be allocated across space? To answer this we develop a spatial general-equilibrium model, and use it to evaluate counterfactual aid policies.

Setup. We model a closed economy with N locations, inhabited by workers, landowners and firms. Each location i has a fixed residential population of L_i workers, who supply labour in the location of their choice. Workers derive utility from consuming goods and housing services, the latter from a non-tradeable endowment of housing. Each location also has a single landowner who owns the land, and consumes goods bought with land rents.

Goods market. The goods market builds on Eaton-Kortum, with a few departures. There is a continuum of goods, which can be produced anywhere. Each good is produced from labour and land using Cobb-Douglas technology with labour share α. Regions differ in their average productivity and their idiosyncratic productivity for each good. Productivity for good u in location j is $z_j(u)$ – drawn from a Fréchet distribution with scale parameter T_j and shape parameter θ_T. T_j captures the absolute advantage of a location, whilst θ_T governs dispersion across goods. The unit cost of producing good u in location j is:

$$\frac{c_j}{z_j(u)} \quad \text{with} \quad c_j \propto w_j^{\alpha} r_j^{1-\alpha} \tag{8}$$

where w_j is the wage rate and r_j is the land rental rate. Firms operate in perfect competition, so this will also be the mill price of good u in location j.

Workers and landowners have identical CES preferences over the continuum of goods, with elasticity of substitution σ. They can buy goods from anywhere, but shipping is subject to iceberg trade costs $\kappa_{ij}^G = \exp(\tau^G d_{ij})$, so that κ_{ij}^G units must be shipped from j for one to arrive in i. They source each good from the least-cost location, and the resulting share of spending by i on goods from j is:

$$\pi_{ij}^G = \frac{T_j \left(c_j \kappa_{ij}^G\right)^{-\theta_T}}{\sum_k T_k \left(c_k \kappa_{ik}^G\right)^{-\theta_T}}, \tag{9}$$

All else equal, i will buy more from j if it has higher productivity, lower wages and land rents, or is closer. The price index, or cost of living, in i falls with its goods-market access: $P_i \propto \text{GMA}_i^{-1}$, where $\text{GMA}_i \equiv \left[\sum_k T_k (c_k \kappa_{ik}^G)^{-\theta_T}\right]^{1/\theta_T}$.

Labour market. Workers reside in i but choose where to work. All else equal, workers prefer locations that pay higher wages w_j and have lower commuting costs $\tau_{ij} = \exp(\tau^L d_{ij})$. They also

draw an idiosyncratic taste for each workplace ζ_{ij} from a Fréchet distribution with shape parameter θ_L. Putting this together, the share of i's residents who commute to j is

$$\pi_{ij} = \frac{(w_j/\tau_{ij})^{\theta_L}}{\sum_k (w_k/\tau_{ik})^{\theta_L}}, \tag{10}$$

rising in the wage on offer and falling in distance, with θ_L governing how much workers sort on the financial benefits of jobs rather than idiosyncratic preferences. The expected wage is $\bar{w}_i = \sum_j \pi_{ij} w_j$. Their expected utility $\bar{v}_i$ is higher, reflecting the value of choosing their preferred job. Analogous to the price index, it rises with commuting market access $\text{CMA}_i \equiv \left[\sum_j (w_j/\tau_{ij})^{\theta_L}\right]^{1/\theta_L}$, so that $\bar{v}_i \propto \text{CMA}_i$.

Factor markets and incomes. Firm revenue in location j is total spending on its goods, $X_j = \sum_i \pi^G_{ij} E_i$, where E_i is income in i and π^G_{ij} its expenditure share on j. We assume factor markets are perfectly competitive, so revenue is split in fixed Cobb–Douglas shares: workers earn $w_j L^w_j = \alpha X_j$, where $L^w_j = \sum_i L_i \pi_{ij}$ is the workforce commuting in, and the landowner earns $r_j H_j = (1-\alpha) X_j$.

Welfare. A worker spends labour income on goods and enjoys their housing endowment. Their utility is Cobb–Douglas in consumption and housing services (since housing is a non-traded endowment, all of the worker's income is spent on consumption goods). The expected utility of a worker resident in i (averaging over their idiosyncratic workplace preference draws) is therefore

$$V^w_i = \left(\frac{\bar{v}_i}{P_i}\right)^{1-\beta} B_i^{\beta} \;\propto\; \left(\text{CMA}_i \cdot \text{GMA}_i\right)^{1-\beta} B_i^{\beta}, \tag{11}$$

where $\bar{v}_i$ is the option value of the jobs they can reach, P_i their cost of living, B_i their housing services and β the weight of housing in utility. Worker welfare rises with market access: to well-paying jobs (CMA_i) and to cheap goods (GMA_i). The landowner has no housing and consumes only goods, so their expected utility is $V^r_i = (1-\alpha) X_i / P_i$.

Competitive equilibrium. Given location fundamentals $\{T_i, H_i, B_i, L_i\}_{i=1}^N$, parameters $\{\alpha, \beta, \theta_T, \theta_L, \sigma\}$, and bilateral trade and commuting costs $\{\kappa^G_{ij}, \tau_{ij}\}$, a competitive equilibrium is a vector of wages $\{w_j\}_{j=1}^N$ at which agents solve the problems above – pinning down c_j, π^G_{ij}, P_i, π_{ij}, $\bar{w}_i$ and $\bar{v}_i$ – and all markets clear:

(i) Labour: The wage bill equals labour's share of revenue, $w_j L_j^w = \alpha X_j$, where $L_j^w = \sum_i L_i \pi_{ij}$ is the workforce commuting in.

(ii) Land: Rents are pinned by land's share of revenue, $r_j = (1-\alpha) X_j / H_j$.

(iii) Goods: Revenue equals spending on each location's output, $X_j = \sum_i \pi_{ij}^G E_i$, where income is $E_i = L_i \bar{w}_i + (1-\alpha) X_i$.

We take the geometric mean of wages as the numeraire, normalising it to one, $\prod_j w_j^{1/N} = 1$.

Flooding in the model. We define a flood as a simultaneous negative shock to productivity and housing. Locations which are flooded experience a reduction in T_j, lowering average productivity, and B_j, as houses are destroyed.

In the model, destruction to housing has no general equilibrium consequences. Housing is non-traded across households or locations – so there is no way that households can act to change their access to housing services in response to damage – and it is separable in utility, so it does not affect their labour supply or consumption decisions. In practice, housing damage may have wider consequences, through reconstruction demand and building-supply prices, but we omit these from the model for now, since the large reconstruction housing programme was only just beginning in 2024, the last time we surveyed our households.

Changes in productivity, on the other hand, can have impacts that ripple out across the entire economy. To see this, recall that the price in a location is pinned down by unit cost. Let p_j be the mill price of an average good produced in j. Firms are perfectly competitive, so they price at marginal cost – the input bundle c_j per unit of output:

$$p_j \propto \frac{c_j}{z_j}, \qquad c_j = w_j^{\alpha} r_j^{1-\alpha}, \tag{12}$$

A flood lowers T_j, hence z_j: it takes more land and labour to make a unit of output, so unit cost rises. Differentiating, and splitting the bundle cost into its two factors,

$$\underbrace{-a_j}_{\text{unit cost up}} = \underbrace{d\ln p_j}_{\text{buyers pay more}} - \underbrace{\alpha\, d\ln w_j}_{\text{workers earn less}} - \underbrace{(1-\alpha)\, d\ln r_j}_{\text{landowners earn less}}, \tag{13}$$

where a_j is the proportional change in average productivity, negative under the flood. Zero profits imply that a fall in productivity must be absorbed by some mix of a higher price paid by buyers, lower wages, and lower land rents.

Intuitively, the trade and commuting elasticities govern how the loss is divided between prices, wages, and rents. If the trade elasticity is low, the flooded location passes its lower productivity into a higher price, leaving wages and rents largely unchanged. If it is high, the price cannot move, and the split between wages and rents depends on the commuting elasticity. If labour is immobile, the two fall together. If labour is mobile, the wage is held up by workers leaving to find work elsewhere, and the residual loads onto land rent.

These changes occur at every production location at once. To turn them into the welfare of the people who *live* in a given location i, recall that residents shop and work in places other than where they live. As a result, a resident of i is exposed to prices and wages wherever they buy goods and supply labour. Linearising around the pre-flood equilibrium we can collect these exposures into a single expression.

$$d\ln V_i^w = \underbrace{\beta\, d\ln B_i}_{\text{housing}} + (1-\beta)\Big[\underbrace{\textstyle\sum_j \pi_{ij}\, d\ln w_j}_{\text{wage}} - \underbrace{\textstyle\sum_j \pi_{ij}^G\, d\ln p_j}_{\text{cost of living}}\Big], \tag{14}$$

The change in average welfare for a worker living at i depends on the change in their housing endowment $\ln B_i$, the change in wages in all locations $d\ln w_j$ weighted by the probability that they commuted to each location prior to the flood $\pi_i j$, and the change in prices in all locations $d\ln p_j$ weighted by the share of their expenditure that was spent on goods from j prior to the flood. The wage and price changes are themselves general-equilibrium responses to the flood.

The landowner holds no housing and lives off rent, so two channels collapse to one:

$$d \ln V_i^r = \underbrace{d \ln r_i}_{\text{rent}} - \underbrace{\textstyle\sum_j \pi_{ij}^G \, d \ln p_j}_{\text{cost of living}}, \qquad d \ln r_i = d \ln X_i, \tag{15}$$

Together these equations map any flood to its welfare cost at every location. We now calibrate the model to quantify them.

6.1 Calibration

The numerical results that follow come from a simulation of a stylised economy (we calibrate to Pakistan in ongoing work). The economy consists of $N = 400$ locations arranged on a 20×20 grid. Locations are equally spaced on a torus (a donut shape), so the grid has no edges and every location faces the same distribution of distances to all other locations. We also impose that all locations are identical in endowments ($L_i = H_i = B_i = 1$) and baseline productivity ($T_j = T$). Together these imply that each location has identical market access in the baseline.

To parameterise the model we estimate some parameters from our data, take conventional values from the literature where available, and otherwise choose illustrative values for the simulation. The labour share in production is $\alpha = 0.55$, which we take from Sotelo (2020). Preferences are CES across goods with elasticity of substitution $\sigma = 1.1$, which we estimate from the HIES, and Cobb–Douglas between goods and housing with a housing share $\beta = 0.09$, which we estimate as the rural-Sindh rent share in the 2018–19 HIES. The two Fréchet shape parameters are the trade elasticity $\theta_T = 4$, which we take from Simonovska and Waugh (2014), and the commuting elasticity which we set to $\theta_L = 1.5$. Trade and commuting costs are iceberg and exponential in distance, with a common gradient which we set to $\tau^G = \tau^L = 0.30$. A flooded cell loses a fraction $a = 0.50$ of its productivity and $b = 0.50$ of its housing, which are illustrative but will later be estimated from housing-damage and agricultural-output data.

6.2 Disaster aid in the model

Our objective is to compare real-world examples of disaster aid policy to the model-implied 'optimal' allocation. To do this we need two things. First, a model of who is harmed by the disaster, which we have outlined above. Second, we need to write down what we think the planner is trying to achieve. We can compare any allocation of aid to the distribution of welfare impacts from the model, but to quantify how much worse one allocation is than another we need a way of aggregating individual outcomes into social welfare.

We assume the planner chooses a per-worker transfer y_i in each location to minimise the welfare loss that remains once aid is paid, subject to a fixed budget B:

$$\min_{\{y_i\}} \sum_i \omega_i L_i \left[u\left(V_i^{w,\text{pre-flood}}\right) - u\left(V_i^{w,\text{post-flood}} + y_i\right) \right] \quad \text{s.t.} \quad \sum_i L_i \, y_i = B, \qquad u(V) = \frac{V^{1-\varepsilon}}{1-\varepsilon} \tag{16}$$

where ω_i is the welfare weight on location i. Individual utility is linear in income, so the household's welfare V_i^w enters linearly. The planner, however, applies a concave transformation $u(\cdot)$, so that social welfare places more weight on households left worse off. Without this curvature the planner would be indifferent across allocations. Intuitively, the planner values each worker's flood loss in utility terms and allocates aid to minimise what remains of it. Since pre-flood welfare is sunk, this is equivalent to maximising post-aid social welfare.

This simple problem embeds several substantive assumptions.

First, the objective is to compensate households for what they lost, not to alleviate poverty. In principle, we allow pre-flood standing to enter the welfare weight ω_i, so the planner may value a given loss more when it falls on a poorer household. However, as a simplification we set $\omega_i = 1$, since our population of interest is rural and poor.

Second, the planner solves a static problem, allocating a fixed budget once the flood has happened. We ignore the dynamic impacts of assistance, which would require the planner to weigh the benefits of aid today against the cost of crowding out future private risk reduction (Baily, 1978; Chetty, 2006). Moral hazard would change the allocation derived here only if it were heterogeneous and

positively correlated with damage, so that the worst-hit also changed their adaptation behaviour most in response to aid. Otherwise it changes only the total budget. In our context aid may even raise private risk reduction, both through income effects and by relaxing the credit constraints that stop poor households financing it.

Third, we ignore how the funds are raised. This is reasonable in our context, where most disaster assistance is externally financed by international donors and the affected population pays little tax.

Last, we assume aid takes the form of a lump-sum cash transfer with no general-equilibrium effects, so each household's welfare depends only on its own transfer, $V_i^{w,\text{post-aid}} \equiv V_i^{w,\text{post-flood}} + y_i$. The advantage is simplicity. With the loss distribution fixed by the flood, we can ask the core question directly – how well different targeting rules approximate the true distribution of losses – without the losses themselves shifting as we change the allocation. Were aid to have general-equilibrium effects, a transfer to one location would move the prices and wages faced by others, so every household's welfare would depend on the entire vector of transfers, $V^{w,\text{post-aid}} = V_i^{w,\text{post-flood}}(\mathbf{y})$. We leave the general-equilibrium effects of aid to future work.

6.3 Solving for Optimal Aid Policy

Given the planner's problem above, we solve for the optimal allocation. We consider two variants – one where every worker who receives aid gets the same size transfer $\bar{y}$ (the discrete optimal), and another where the transfer is allowed to vary across locations y_i (the continuous optimal). In both cases the transfer is the same for all workers who live in a given location.

The discrete case corresponds to policies such as the BISP cash transfer after the 2022 floods, a fixed per-household payment for which the government chose which districts were eligible. Here, the planner is allowed to choose the level of the transfer per worker $\bar{y}$ and which locations receive it. Intuitively, we can think about the planner deriving the optimal policy by solving this problem in two steps. For each possible transfer size, the planner ranks locations by their marginal welfare gain, $u(V_i^{w,\text{post-flood}} + \bar{y}) - u(V_i^{w,\text{post-flood}})$, and, starting from those with the largest marginal gain, adds them to the list of eligible locations until the budget is exhausted. The optimal transfer size

is then chosen by comparing the social welfare achieved by the optimal allocation at each transfer level. With common pre-flood welfare, as in our simulation, ranking by the marginal welfare gain is equivalent to ranking by the size of the loss; in general it is not, since the planner targets the worst-off rather than those who fell furthest.

In the continuous case the planner can give any non-negative amount to each location. The optimal policy will take the form of choosing a welfare cutoff $\bar{V}$, giving nothing to locations already at or above it, and topping the rest up to $\bar{V}$, with the cutoff set to exhaust the budget. Unlike the discrete case, this requires knowing not just who is worst off but by how much.

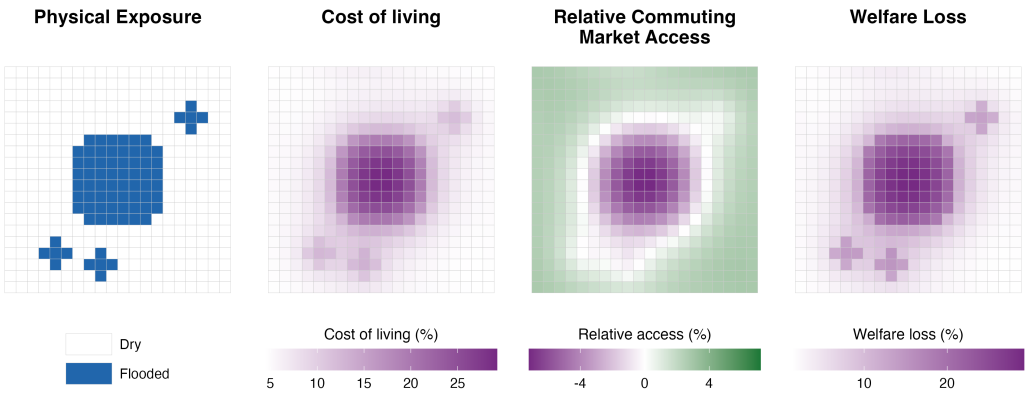
We contrast these two optimal policies against two stylised versions of the rules used in practice, both of which target physical damage rather than welfare loss. The first, damage targeting, pays the same amount to every worker in a flooded location. The second, access-weighted damage targeting, also pays only flooded locations but scales the amount down with distance to the nearest dry land, reflecting that remote locations are harder to reach.

6.4 Welfare gains from optimal policy

We begin with a single flood, modelled as a discrete event – a location is either flooded or not. In this flood, 19% of locations (75 out of 400) were flooded. In each flooded location 50% of the housing endowment was destroyed and productivity fell by 50%. Since locations are symmetric in this economy, this implies that 9.5% of the whole housing stock was destroyed, and, in partial equilibrium, output in the economy would fall by 9.5%.

Figure 2 shows the flood (far left), the resulting welfare loss (far right), and the two general-equilibrium channels through which the flood reaches welfare: the cost of living residents face and their commuting market access. On average, across the whole economy, worker welfare falls by 7.8% as a result of this flood. However, across locations there is a wide range in the welfare impact of this event - ranging from 29.2% for the most affected location to 1.4% in the least affected.

Figure 2: Physical Exposure, Price Changes and Welfare Loss



Notes: From left: flooded locations (blue); the change in the cost of living residents of i face, $\Delta \ln P_i$ (the price index over the goods they buy); the change in their commuting market access, $\Delta \ln \text{CMA}_i$ (the option value of the workplaces they can reach); and worker welfare loss, $(V_i^{w,\text{pre-flood}} - V_i^{w,\text{post-flood}})/V_i^{w,\text{pre-flood}} \times 100$. Purple marks the adverse direction throughout. Average wage changes are not identified (since they are the numeraire) so panel 3 shows relative impacts on commuting access.

How does the welfare loss relate to physical exposure? For the average physically exposed worker this figure is 21.9% and for the average worker who was not flooded this figure is 4.5%. However, physical exposure is not a sufficient statistic for welfare loss - amongst the flooded the loss varies from 11.8% to 29.2%, whilst amongst the unflooded it varies from 1.4% to 12.9%, so that the worst-hit unflooded locations lose more than the least-hit flooded ones.

We showed above that in the model flood losses depend not only on flooding in your own location, but on flooding everywhere, weighted by each location's importance as a trading or employment destination. We make this concrete by running the reduced form of Section 4.2 on the model's own output, regressing each location's outcomes on its direct and indirect exposure, built with the same kernel as in the data (Table 6.1). Own flooding predicts housing damage perfectly and indirect exposure adds nothing. For welfare loss the ranking reverses. Indirect exposure alone explains 94% of the variation across locations, against 80% for direct exposure, and with both included a standard deviation of indirect exposure raises the loss by 5.2 percentage points (ppts) against 2.9ppts for direct. Indirect exposure also raises the cost of living by 4.7ppts and lowers relative commuting market access by 2.4ppts, in both cases by more than direct exposure does.

Table 6.1: Direct and indirect exposure regression using model data

	Housing damage (%)	Cost of living (%)	Relative commuting access (%)	Welfare loss (%)
Panel A: direct exposure only				
Direct exposure	19.54	5.37	-2.07	6.81
R^2	1.00	0.74	0.64	0.80
Panel B: indirect exposure only				
Indirect exposure	14.81	6.08	-2.58	7.37
R^2	0.57	0.95	1.00	0.94
Panel C: both				
Direct exposure	19.54	1.80	-0.25	2.87
Indirect exposure	0.00	4.72	-2.39	5.19
R^2	1.00	0.99	1.00	1.00
SD (direct exposure)	0.39	0.39	0.39	0.39
SD (indirect exposure)	0.16	0.16	0.16	0.16
Control mean (least-exposed)	0.00	4.48	2.88	1.40
Locations	400	400	400	400

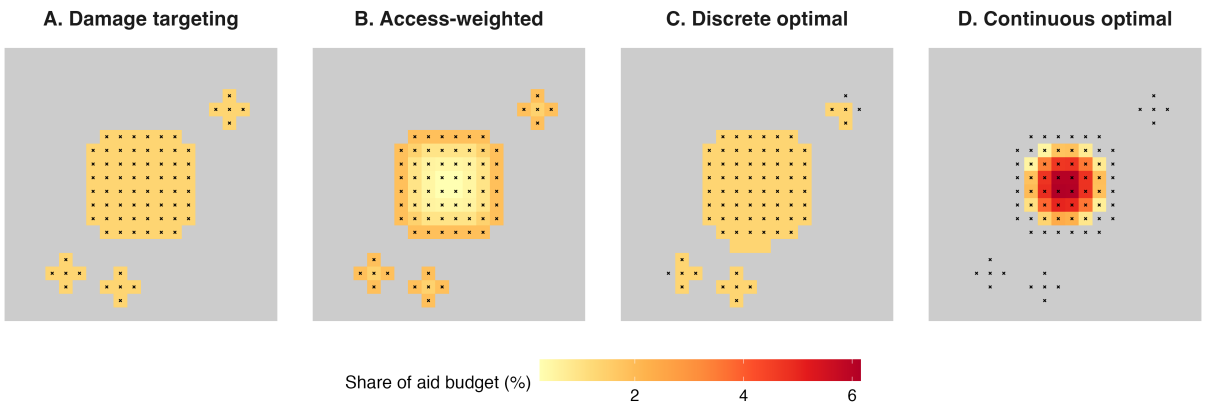
Notes: One observation per grid location. Each column regresses one outcome on standardised direct and indirect exposure, using the empirical kernel of Section 4.2; each coefficient is the percentage-point change in the outcome per one standard deviation of exposure. Panels A and B include each exposure alone; Panel C includes both. Housing damage is the share of the local housing stock destroyed; cost of living is $\Delta \ln P_i$; relative commuting access is $\Delta \ln \mathrm{CMA}_i$, measured against the economy-wide average (the numeraire fixes the average wage); welfare loss is in % of baseline. SD rows give each exposure's spread; the control mean is each outcome at the least-exposed location.

We now ask how a policymaker would allocate aid after this flood. We take the two optimal policies derived above, discrete and continuous, and compare them to the two status-quo policies, damage targeting and access-weighted damage targeting. We set the aid budget at 0.2% of pre-flood aggregate worker welfare, $B = 0.002 \cdot \sum_i V_i^w$, which equals 2% of pre-flood output, and the welfare curvature to $\varepsilon = 1.5$.

Figure 3 shows the resulting allocation under each policy. Damage targeting (Panel A) mirrors the flood map exactly, giving every flooded worker the same transfer of 1.07% of baseline income. The access-weighted policy (Panel B) assigns transfers from 0.20% to 1.50%, so that conditional on being flooded, a standard deviation more indirect exposure lowers the transfer by 0.37 percentage points of income. The discrete optimal (Panel C) is nearly identical to damage targeting, covering 75 locations at the same fixed transfer and swapping only the three least-affected flooded locations for the three worst-hit unflooded ones. The continuous optimal (Panel D) is almost the mirror image of

the access-weighted policy, with transfers ranging from 0.42% to 4.93%, the largest going to locations at the centre of the main flooded zone and nothing at all to 43 of the 75 flooded locations closest to dry land.

Figure 3: Aid allocation under each policy

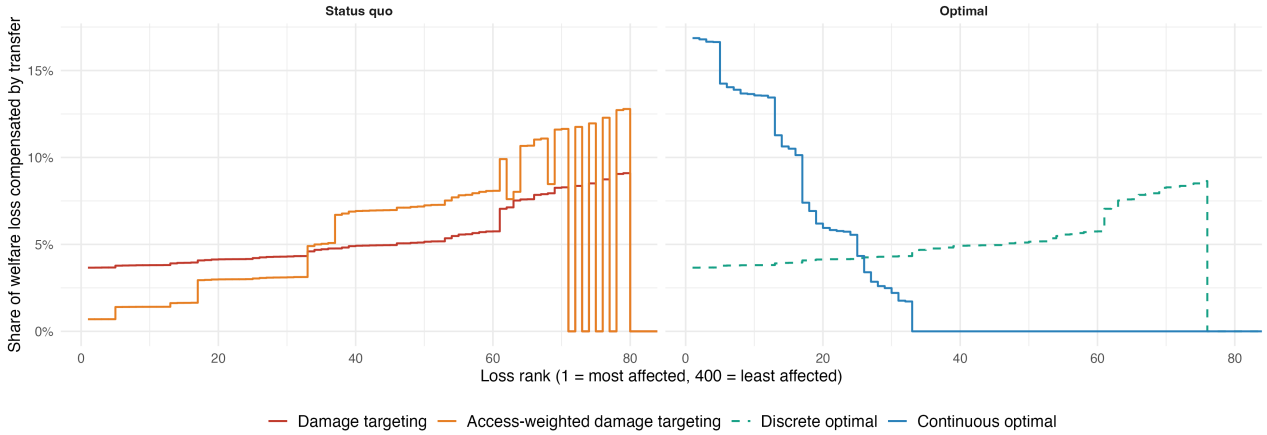


Notes: Each location's transfer as a share of the total aid budget, across the 400 grid locations; crosses mark flooded locations, grey cells receive nothing. A gives every flooded location the same transfer; B scales it down with distance to dry land; C is the discrete optimum; D is the continuous optimum.

These policies imply large cross-location variation in the share of loss that is compensated (Figure 4). A flat transfer, like the discrete optimal or damage targeting, gives the same amount to all, so it compensates a smaller share of loss for those hit wors (the worst-affected recover $\sim$ 4% of their loss against $\sim$ 9% for the least-hit flooded locations). Access-weighted damage targeting is worse still, since the worst-hit locations are the hardest to reach and so receive the least, recovering only $\sim$ 1%. The continuous optimal does the opposite, directing the largest compensation rate to the worst-affected, who recover $\sim$ 17%.

We also compare the cost-effectiveness of assistance across these policies, in terms of welfare gained per dollar spent. The continuous optimal delivers 8.7% more welfare per dollar than damage targeting, and 12.9% more than access-weighted damage targeting. The discrete optimal essentially ties damage targeting (since it only differs by 3 marginal locations). The continuous rule's advantage comes from reweighting among flooded locations rather than from reaching unflooded ones, directing more to the very middle of the flood and less to the periphery. Access-weighted damage targeting does the reverse, favouring accessible locations near dry land, which is why it does worst.

Figure 4: Share of welfare loss compensated by transfer, by loss rank



Notes: For each affected location ($\ell_i = V_i^{w,\text{pre-flood}} - V_i^{w,\text{post-flood}} > 0.01$), the share of loss covered by the transfer, y_i/ℓ_i, against the rank of ℓ_i from largest (rank 1) to smallest. Left: the two status-quo rules. Right: the two model-based rules.

At a budget of 0.2% of pre-flood worker welfare, equivalently 2% of GDP and 2.6% of total welfare losses, the entire gain over the flat transfer comes from reallocating among the flooded rather than from reaching outside the zone (Figure 5). As the budget rises, the worst losses for flooded households are covered and the largest remaining losses fall outside the flooded area. Sending resources outside the flood zone begins to contribute once the budget covers about a third of total losses, and accounts for half the gain once it covers roughly half.

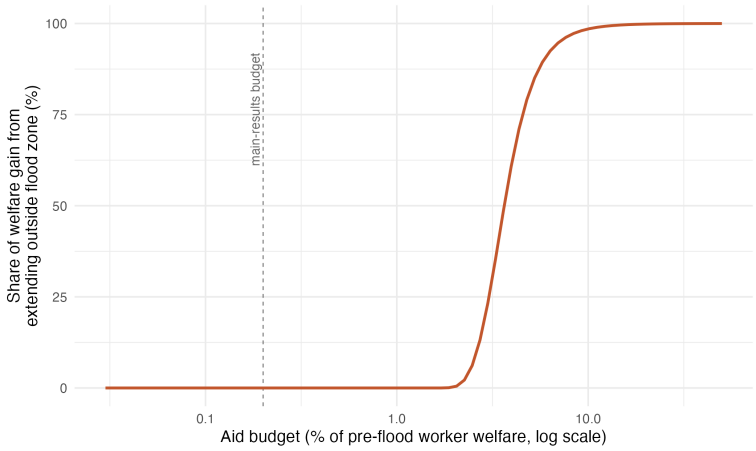
6.5 Determinants of the gains from targeting

This illustrative example shows there is room to improve on status-quo aid policy when the incidence of the flood differs from physical exposure. The gains from targeting depend on the gap between the two, which is shaped by three factors – trade costs, the spatial dispersion of the flood, and its size.

Transport costs

We vary τ^G and τ^L from near-frictionless to near-autarky, and re-solve the optimal rule at each value. When transport costs are very high, each location operates in autarky. There are no spillovers to unflooded locations and every flooded location loses the same, so the optimal policy is a flat transfer to the physically exposed. When transport costs are very low, the productivity loss spreads

Figure 5: Where the gains from optimal aid come from, by budget size



Notes: Share of the continuous-optimal policy's welfare gain over the flat status-quo transfer that comes from extending transfers outside the flood zone, rather than reallocating among the flooded, for the main-results flood, as the aid budget varies. The budget is on the horizontal axis as a percentage of pre-flood worker welfare (log scale); the dashed line marks the main-results budget. The remainder of the gain comes from reallocating among the flooded.

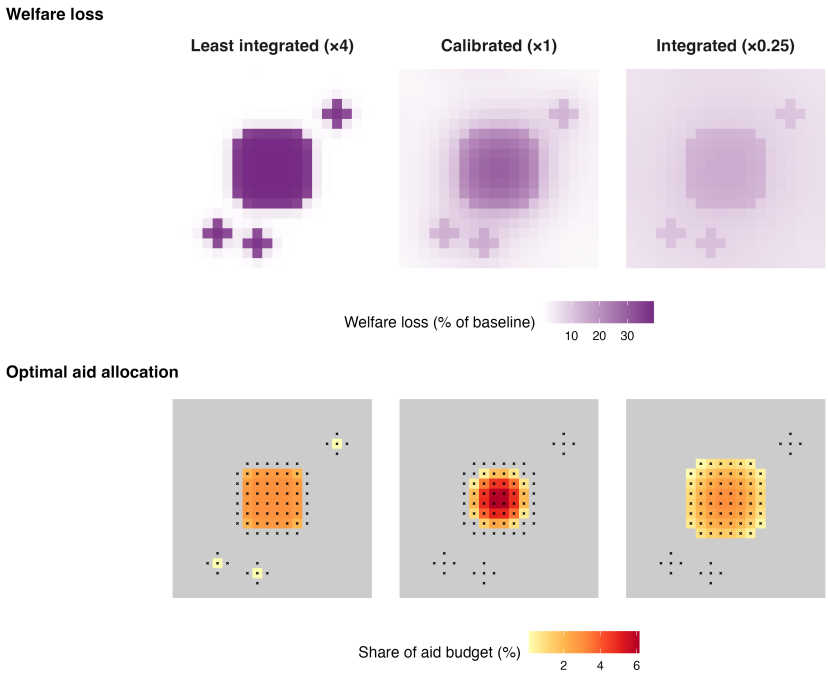
evenly across the economy. The optimal policy still varies transfers across locations, but with little difference in loss between them there is little to gain over a flat transfer. The gains from optimal targeting are largest in between, where losses neither stay local nor spread out, peaking at around 12% more welfare per dollar than a flat transfer.

Spatial concentration

Holding fixed the number of locations which are affected by a flood, the spatial dispersion of a flood (how spatially correlated it is) changes its incidence. When a flood is very dispersed, most flooded locations have unflooded neighbours to commute to and trade with, so more of the loss is shared with neighbours. (Figure 7) gives an example - 65% of the costs are borne outside of physical flood zone when the flooding is very dispersed (bottom row), compared to 40% when it is very concentrated (top row).

Under a concentrated flood the loss is also more unequally distributed amongst the flooded. The centre of a concentrated flood is far from dry land, while its edge can still reach the unflooded locations nearby, so the worst-hit location loses 31% of baseline welfare against 19% in the dispersed flood.

Figure 6: Welfare loss and the optimal aid allocation under alternative trade costs



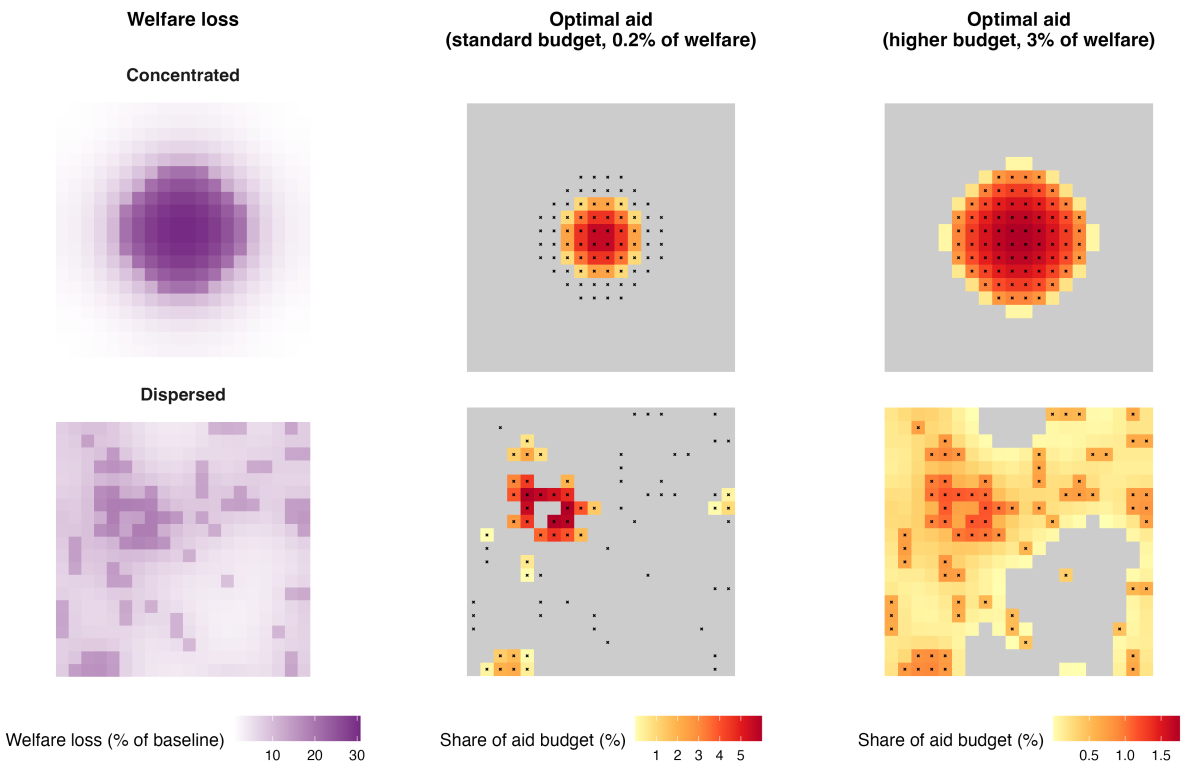
Notes: Columns are three levels of economic integration, from least integrated (left) to most integrated (right). Top row: worker welfare loss (% of baseline). Bottom row: the continuous-optimal aid allocation (each location's share of the aid budget, on a common scale across panels; the budget is the same in every world); crosses mark flooded locations.

Accordingly, the optimal policy gives most resources to the centre and little to locations at the edge (Figure 7, centre column). The welfare gain over a flat transfer is roughly 2.4 times larger than for a dispersed flood of the same size. A dispersed flood spreads the loss more evenly, leaving less for targeting to do within the zone. At a larger budget the optimal policy begins to reach beyond the flood zone, and does so far more under the dispersed flood (Figure 7, right column).

Size of the flood

The directly flooded almost always lose more than the unflooded, for two reasons. They suffer housing damage, and flooding is spatially correlated, so a flooded location is more likely to have flooded neighbours and to lose more market access. At a flood covering 19% of locations, a flooded location loses around 25% of baseline welfare, 4.5 points of it due to housing damage and the rest due to productivity losses, against 4% for an unflooded location, all of it productivity (Figure 8, left).

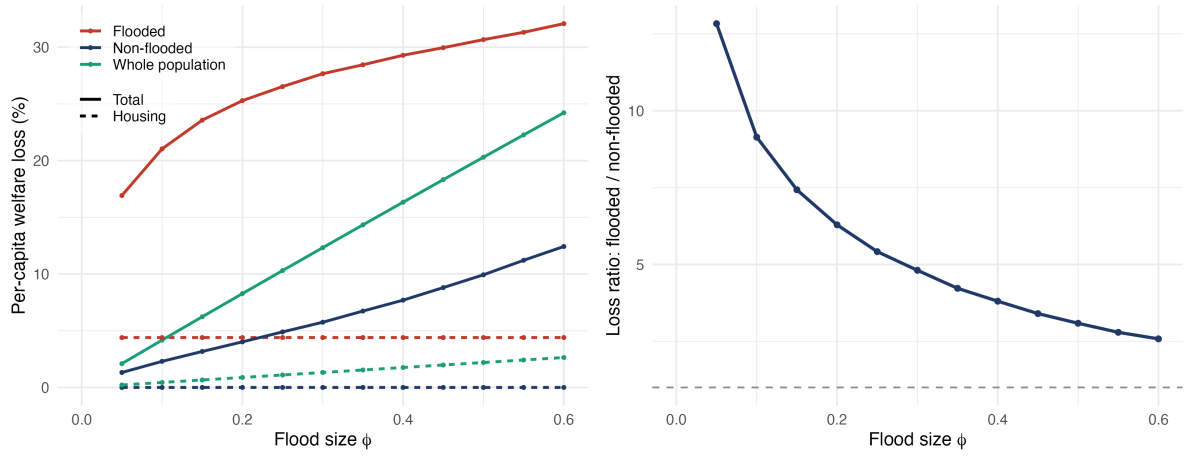
Figure 7: Flood incidence and optimal aid, by flood dispersion



Notes: Two floods of the same size ($\phi = 0.19$): concentrated (top row) and dispersed (bottom row). Left column: worker welfare loss (% of baseline). Centre and right columns: the continuous-optimal aid allocation (each location's share of the aid budget), at the standard budget (0.2% of pre-flood worker welfare) and at a higher budget (3%); crosses mark flooded locations.

The gap between them shrinks in relative terms as the flood grows. Consider what happens when one more location floods. Its housing loss is the same as every other flooded location's, so the housing loss per flooded household does not change. It joins at the edge, so it has more dry neighbours than the locations it joins, and the flooded already inside lose only one more dry neighbour from a position where they had few. The unflooded, meanwhile, gain a flooded neighbour. The ratio of flooded to unflooded loss therefore falls from 13 at $\phi = 0.05$ to 2 at $\phi = 0.6$ (Figure 8, right), and physical exposure reveals less about who bears the loss as the flood grows.

Figure 8: Flood size and the distribution of losses between flooded and unflooded households



Notes: Both panels sweep over flood size ϕ (concentrated discs, calibrated frictions). Left: per-capita worker welfare loss for directly flooded, unflooded, and whole-population locations, with the housing own-cell component (dashed) separated from the total (solid). Right: the ratio of average loss, flooded to unflooded, as ϕ rises.

7 Conclusion

This paper asks how the economic costs of a flood depend not only on whether a household is itself inundated, but on how intensely the area around it flooded. We combine a panel of rural households in Sindh with a recentered instrumental-variables design that separately identifies the impact of direct and indirect flood exposure. Direct exposure causes physical damage, but on its own it leaves employment and prices largely unmoved. Indirect exposure lowers employment, raises food prices, and reduces real expenditure. Physical exposure is therefore a poor guide to economic incidence.

Households hit on both margins, flooded and surrounded by flooding, bore the largest costs and received less aid than those with dry neighbours to fall back on. In our model, optimal disaster assistance directs most resources to those households, who in practice received the least.

The broader lesson from our results is that informal coping, including trade with unflooded areas, works best against localised shocks, and breaks down when the shock is common to the markets and neighbours a household would otherwise turn to. As climate change makes large floods more frequent, and a single event covers more of a country at once, the indirect costs of widespread flooding are likely to become increasingly central to disaster policy.

References

Aiken, Emily, Suzanne Bellue, Dean Karlan, Christopher Udry, and Joshua E. Blumenstock, "Machine Learning and Phone Data Can Improve Targeting of Humanitarian Aid," *Nature*, 2022, *603* (7903), 864–870. [Return to page 6]

Alatas, Vivi, Abhijit Banerjee, Rema Hanna, Benjamin A. Olken, and Julia Tobias, "Targeting the Poor: Evidence from a Field Experiment in Indonesia," *American Economic Review*, 2012, *102* (4), 1206–1240. [Return to page 6]

_ , Ririn Purnamasari, Matthew Wai-Poi, Abhijit Banerjee, Benjamin A. Olken, and Rema Hanna, "Self-Targeting: Evidence from a Field Experiment in Indonesia," *Journal of Political Economy*, 2016, *124* (2), 371–427. [Return to page 6]

Allen, Treb and David Atkin, "Volatility and the Gains from Trade," *Econometrica*, 2022, *90* (5), 2053–2092. [Return to page 6]

Baily, Martin Neil, "Some Aspects of Optimal Unemployment Insurance," *Journal of Public Economics*, 1978, *10* (3), 379–402. [Return to page 35]

Balboni, Clare, Johannes Boehm, and Mazhar Waseem, "Firm Adaptation in Production Networks: Evidence from Extreme Weather Events in Pakistan," *American Economic Review*, 2025. Forthcoming. [Return to page 6]

Bandiera, Oriana, Amen Jalal, and Nina Roussille, "The Illusion of Time: Gender Gaps in Job Search and Employment," NBER Working Paper 34051, National Bureau of Economic Research 2025. [Return to page 22]

Banerjee, Abhijit, Rema Hanna, Benjamin A. Olken, and Diana Sverdlin Lisker, "Social Protection in the Developing World," *Journal of Economic Literature*, 2024, *62* (4), 1349–1421. [Return to page 6]

Banerjee, Abhirup, Shraddha Gupta, Pranava Priyanshu, Ankan Kar, Ruby Saha, Tanujit Chakraborty, Dibakar Ghosh, Jürgen Kurths, and Chittaranjan Hens, "Recent changes in

spatiotemporal patterns of heat extremes in South Asia," *npj Climate and Atmospheric Science*, 2025, *8* (293). [Return to page **8**]

Barrot, Jean-Noël and Julien Sauvagnat, "Input Specificity and the Propagation of Idiosyncratic Shocks in Production Networks," *The Quarterly Journal of Economics*, 2016, *131* (3), 1543–1592. [Return to page **6**]

Baseler, Travis and Jakob Hennig, "Disastrous Displacement: The Long-Run Impacts of Landslides," *American Economic Journal: Applied Economics*, 2023. Forthcoming; World Bank Policy Research Working Paper 10535. [Return to page **6**]

Bates, Paul D., Matthew S. Horritt, and Timothy J. Fewtrell, "A simple inertial formulation of the shallow water equations for efficient two-dimensional flood inundation modelling," *Journal of Hydrology*, 2010, *387* (1–2), 33–45. [Return to page **11**]

Belloni, Alexandre, Victor Chernozhukov, and Christian Hansen, "Inference on Treatment Effects after Selection among High-Dimensional Controls," *The Review of Economic Studies*, 2014, *81* (2), 608–650. [Return to page **15**]

Betterle, Andrea and Peter Salamon, "Water depth estimate and flood extent enhancement for satellite-based inundation maps," *Natural Hazards and Earth System Sciences*, 2024, *24* (8), 2817–2836. [Return to page **11**]

Boehm, Christoph E., Aaron Flaaen, and Nitya Pandalai-Nayar, "Input Linkages and the Transmission of Shocks: Firm-Level Evidence from the 2011 Tōhoku Earthquake," *The Review of Economics and Statistics*, 2019, *101* (1), 60–75. [Return to page **6**]

Borusyak, Kirill and Peter Hull, "Nonrandom Exposure to Exogenous Shocks," *Econometrica*, 2023, *91* (6), 2155–2185. [Referenced on pages **2** **and** **11**]

_ **and** _ , "Optimal Formula Instruments," Working Paper 33594, National Bureau of Economic Research 2025. [Referenced on pages **2** **and** **15**]

Boustan, Leah Platt, Matthew E. Kahn, Paul W. Rhode, and Maria Lucia Yanguas, "The Effect of Natural Disasters on Economic Activity in US Counties: A Century of Data," *Journal of Urban Economics*, 2020, *118*, 103257. [Return to page **6**]

Breza, Emily, Supreet Kaur, and Yogita Shamdasani, "Labor Rationing," *American Economic Review*, 2021, *111* (10), 3184–3224. [Return to page 3]

Brunner, Manuela I. and Corentin Chartier-Rescan, "Drought Spatial Extent and Dependence Increase During Drought Propagation From the Atmosphere to the Hydrosphere," *Geophysical Research Letters*, 2024, *51* (e2023GL107918). [Return to page 8]

Burgess, Robin and Dave Donaldson, "Can Openness Mitigate the Effects of Weather Shocks? Evidence from India's Famine Era," *American Economic Review*, 2010, *100* (2), 449–453. [Return to page 6]

Carvalho, Vasco M., Makoto Nirei, Yukiko U. Saito, and Alireza Tahbaz-Salehi, "Supply Chain Disruptions: Evidence from the Great East Japan Earthquake," *The Quarterly Journal of Economics*, 2021, *136* (2), 1255–1321. [Return to page 6]

Castro-Vincenzi, Juan Manuel, "Climate Hazards and Resilience in the Global Car Industry," Working Paper, Princeton University 2024. [Return to page 6]

Chamberlain, Gary, "Asymptotic efficiency in estimation with conditional moment restrictions," *Journal of Econometrics*, 1987, *34* (3), 305–334. [Return to page 15]

Chernozhukov, Victor, Denis Chetverikov, Mert Demirer, Esther Duflo, Christian Hansen, Whitney Newey, and James Robins, "Double/debiased machine learning for treatment and structural parameters," *The Econometrics Journal*, 2018, *21* (1), C1–C68. [Return to page 15]

Chetty, Raj, "A General Formula for the Optimal Level of Social Insurance," *Journal of Public Economics*, 2006, *90* (10–11), 1879–1901. [Return to page 35]

Deryugina, Tatyana, Laura Kawano, and Steven Levitt, "The Economic Impact of Hurricane Katrina on Its Victims: Evidence from Individual Tax Returns," *American Economic Journal: Applied Economics*, 2018, *10* (2), 202–233. [Return to page 6]

Fu, Chao and Jesse Gregory, "Estimation of an Equilibrium Model with Externalities: Post-Disaster Neighborhood Rebuilding," *Econometrica*, 2019, *87* (2), 387–421. [Return to page 6]

Funk, Chris, Pete Peterson, Martin Landsfeld, Diego Pedreros, James Verdin, Shraddhanand Shukla, Gregory Husak, James Rowland, Laura Harrison, Andrew Hoell, and Joel Michaelsen, "The climate hazards infrared precipitation with stations—a new environmental record for monitoring extremes," *Scientific Data*, 2015, *2* (1), 150066. [Return to page 7]

Gallagher, Justin and Daniel Hartley, "Household Finance after a Natural Disaster: The Case of Hurricane Katrina," *American Economic Journal: Economic Policy*, 2017, *9* (3), 199–228. [Return to page 6]

Gignoux, Jérémie and Marta Menéndez, "Benefit in the Wake of Disaster: Long-Run Effects of Earthquakes on Welfare in Rural Indonesia," *Journal of Development Economics*, 2016, *118*, 26–44. [Return to page 6]

Gordon, Matthew, Yukiko Hashida, and Eli P. Fenichel, "Targeting Disaster Aid: A Structural Evaluation of a Large Earthquake Reconstruction Program," Technical Report, SSRN Working Paper 4862074 2025. [Return to page 6]

Groen, Jeffrey A., Mark J. Kutzbach, and Anne E. Polivka, "Storms and Jobs: The Effect of Hurricanes on Individuals' Employment and Earnings over the Long Term," *Journal of Labor Economics*, 2020, *38* (3), 653–685. [Return to page 6]

Hanna, Rema and Benjamin A. Olken, "Universal Basic Incomes versus Targeted Transfers: Anti-Poverty Programs in Developing Countries," *Journal of Economic Perspectives*, 2018, *32* (4), 201–226. [Return to page 6]

Huber, Kilian, "Estimating General Equilibrium Spillovers of Large-Scale Shocks," *The Review of Financial Studies*, 2023, *36* (4), 1548–1584. [Return to page 19]

Jalal, Amen, "Screening Women Out? Pay Transparency in Job Postings," Job Market Paper 2026. [Return to page 22]

Kemter, Matthias, Bruno Merz, Norbert Marwan, Sergiy Vorogushyn, and Günter Blöschl, "Joint Trends in Flood Magnitudes and Spatial Extents Across Europe," *Geophysical Research Letters*, 2020, *47* (e2020GL087464). [Return to page 8]

Kirchberger, Martina, "Natural Disasters and Labor Markets," *Journal of Development Economics*, 2017, *125*, 40–58. [Return to page 6]

Kocornik-Mina, Adriana, Thomas K. J. McDermott, Guy Michaels, and Ferdinand Rauch, "Flooded Cities," *American Economic Journal: Applied Economics*, 2020, *12* (2), 35–66. [Return to page 6]

McIntosh, Molly Fifer, "Measuring the Labor Market Impacts of Hurricane Katrina Migration: Evidence from Houston, Texas," *American Economic Review*, 2008, *98* (2), 54–57. [Return to page 6]

Neal, Jeffrey, Guy Schumann, and Paul Bates, "A subgrid channel model for simulating river hydraulics and floodplain inundation over large and data sparse areas," *Water Resources Research*, 2012, *48* (11), W11506. [Return to page 11]

Nigro, Joseph, Daniel Slayback, Frederick Policelli, and G. Robert Brakenridge, "NASA/DFO MODIS Near Real-Time (NRT) Global Flood Mapping Product Evaluation of Flood and Permanent Water Detection," Technical Report, NASA Goddard Space Flight Center, Greenbelt, MD 2014. [Return to page 7]

Patel, Dev, "Floods," 2024. Working paper. [Referenced on pages 6, 8, 10, and 11]

Pople, Ashley, Ruth Hill, Stefan Dercon, and Ben Brunckhorst, "Importance of Being Early? Anticipatory Cash Transfers for Flood-Affected Households," *The World Bank Economic Review*, 2026. Advance access. [Return to page 6]

Qin, Lianjie, Laiyin Zhu, Baoyin Liu, Zixuan Li, Yugang Tian, Gordon Mitchell, Shifei Shen, Wei Xu, and Jianguo Chen, "Global expansion of tropical cyclone precipitation footprint," *Nature Communications*, 2024, *15* (4824). [Return to page 8]

Roxy, M. K., Subimal Ghosh, Amey Pathak, R. Athulya, Milind Mujumdar, Raghu Murtugudde, Pascal Terray, and M. Rajeevan, "A threefold rise in widespread extreme rain events over central India," *Nature Communications*, 2017, *8* (708). [Return to page 8]

Simonovska, Ina and Michael E. Waugh, "The Elasticity of Trade: Estimates and Evidence," *Journal of International Economics*, 2014, *92* (1), 34–50. [Return to page 34]

Skinner, Christopher B., Danielle Touma, Mathew Barlow, Deepti Singh, and Troy King, "The spatial extent of heat waves has changed over the past four decades," *Communications Earth & Environment*, 2025, *6* (662). [Return to page **8**]

Sotelo, Sebastian, "Domestic Trade Frictions and Agriculture," *Journal of Political Economy*, 2020, *128* (7), 2690–2738. [Return to page **34**]

Tarquinio, Lisa, "The Politics of Drought Relief: Evidence from Southern India," Technical Report, SSRN Working Paper 4163045 2023. [Return to page **6**]

Tellman, B., J. A. Sullivan, C. Kuhn, A. J. Kettner, C. S. Doyle, G. R. Brakenridge, T. A. Erickson, and D. A. Slayback, "Satellite imaging reveals increased proportion of population exposed to floods," *Nature*, 2021, *596* (7870), 80–86. [Return to page **7**]

World Weather Attribution, "Climate change likely increased extreme monsoon rainfall, flooding highly vulnerable communities in Pakistan," https://www.worldweatherattribution.org/climate-change-likely-increased-extreme-monsoon-rainfall-flooding-highly-vulnerable-communities-in-pakist September 2022. Scientific attribution statement released 14 September 2022. Attributes a 75% increase in 60-day Sindh/Balochistan rainfall intensity to anthropogenic warming. [Return to page **7**]

A Appendix figures

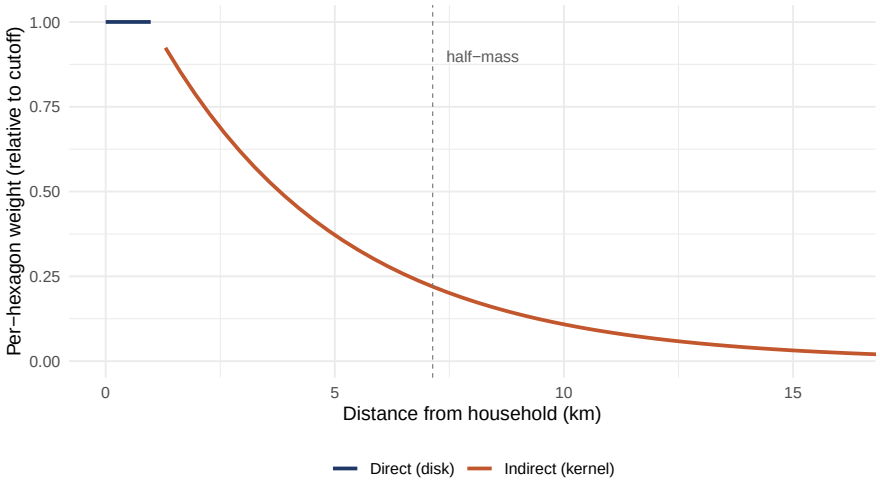


Figure 9: Exposure weights by distance