Compounded Climate Shocks and Food Security: Evidence from Back-to-Back Cyclones in Madagascar*

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Abstract

Extreme weather events increasingly arrive in clusters, and losses from back-to-back shocks may compound rather than simply add up. We study two intense tropical cyclones, *Batsirai* and *Emnati*, that struck Madagascar less than three weeks apart in February 2022. Exploiting the plausibly exogenous paths of the two storms across 112 districts, we estimate difference-in-differences specifications that compare households in districts struck never, once, or twice, using four rounds of household survey data (2020–2023). A single strike reduces the Food Consumption Score (FCS) by 12 percent of the unaffected-district mean and raises the Food Insecurity Experience Scale (FIES) by 9 percent; a second strike more than doubles the FCS loss to 26 percent and lowers a composite index of the two measures by 0.68 standard deviations. Households struck twice lose staple harvest value and cut food spending by a third, while their non-food spending, led by housing repairs, rises. Our findings suggest that sequential shocks compound food insecurity both by destroying agricultural production and by crowding food out of household budgets.

JEL Classifications: O12, O13, Q18, Q54, Q56

Keywords: climate shocks, cyclones, compounded shocks, food security, agricultural production, Madagascar

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1 Introduction

Climate change is altering not only the frequency of extreme weather events but also how they combine: hazards increasingly interact and compound, and storms, floods, and droughts can strike the same places in close succession (Zscheischler et al. 2018; Zscheischler et al. 2020). Clustering matters because recovery takes time. After one shock, households rebuild gradually—drawing down savings, selling assets, borrowing, or sending members away to work (Deaton 1991; Gröger and Zylberberg 2016)—and replanting a flooded field or repairing a damaged home takes a season or more. A second shock that lands inside this window meets households whose buffers are already spent, so its damage need not simply add to the first's: losses can compound, with the marginal shock hurting more than the initial one. The concern is greatest in poor rural economies, where formal insurance is scarce (Cole et al. 2013), social protection is thin (Beegle et al. 2018), and disaster response is slow (Clarke and Dercon 2016). There, clustered shocks threaten not only the chronically poor but also the vulnerable non-poor—households above but near the poverty line, for whom one bad season is survivable but a sequence of them can trigger the asset sales and disinvestment that convert a transitory loss into persistent poverty (Carter et al. 2007; Balboni et al. 2022).

Most existing evidence on weather extremes, however, comes from isolated events. A large literature shows that individual shocks depress income, consumption, and food security in low-income settings (Dercon 2004; Jayachandran 2006; Anttila-Hughes and S. Hsiang 2013), and a related literature traces macroeconomic losses to tropical cyclones specifically (Strobl 2012; S. M. Hsiang 2010), though for large disasters more broadly the growth evidence is contested (Cavallo et al. 2013). How losses accumulate when the same communities are struck repeatedly within a single season remains largely undocumented, in part because closely spaced severe events that also generate clean spatial variation in exposure are rare.

Madagascar in early 2022 offers precisely such variation, and we use it as a test case for the welfare effects of back-to-back weather shocks. Cyclone *Batsirai* made landfall on the country's southeastern coast on 5 February 2022 with mean winds near 165 km/h and gusts above 235 km/h; Cyclone *Emnati* followed on 23 February, tracking across a largely overlapping corridor (FAO 2022). Between them, the two storms killed 136 people, displaced more than 130,000, and destroyed close to 30,000 hectares of rice fields (Table A1).

The storms' paths generated sharp spatial variation in exposure: of the 112 districts in the southern half of Madagascar covered by our data, 47 were never struck, 28 were struck by Batsirai only, and 37 were struck by both cyclones; every district in Emnati's path had already been hit by Batsirai. We link this exposure classification to four rounds of repeated cross-sectional household surveys (2020–2023; 49,695 households) collected by the Ministry of Agriculture and Livestock, and estimate difference-in-differences (DiD) specifications with district and year fixed effects. We measure food security with the Food Consumption Score (FCS), which captures dietary quantity and diversity over a seven-day recall; the Food Insecurity Experience Scale (FIES), which captures experienced food insecurity over a twelve-month recall; and a composite Food Security Index that aggregates the two (Anderson 2008).

Because exposure is determined by where the storms happened to track rather than by the characteristics of the districts in their path, these comparisons support a causal interpretation. Cyclone corridors in Madagascar shift from event to event—Cyclones *Gafilo* (2004) and *Enawo* (2017), the two most intense storms of the two decades before 2022, traversed largely different districts (Figure A1; Figure A2)—so exposure is not a fixed trait of certain areas. Twice-struck districts are worse off in levels even before 2022, but district fixed effects absorb level gaps, and pre-cyclone trajectories are parallel: event-study leads are null for once-struck districts and small relative to the treatment effects for twice-struck ones (Table A11). And if the cyclones affected nearby comparison districts through regional markets or aid diversion, such spillovers would attenuate our estimates, making the effects we report, if anything, conservative.

Three findings emerge. First, even a single cyclone imposes substantial welfare costs: compared with unaffected districts, households struck by Batsirai alone see their FCS fall by 4.69 points—12 percent of the 2021 unaffected-district mean—and their FIES rise by 0.55 points (9 percent). Second, the second strike compounds the damage: in twice-struck districts the FCS falls by 9.96 points (26 percent), over twice the single-strike loss, and the composite Food Security Index drops by 0.68 standard deviations versus 0.45; equality of the single- and double-strike coefficients is rejected at the 1 percent level for both. The FIES effect is also larger (0.64 points), though not statistically distinguishable from the single-strike effect. Third, the mechanisms line up with a simple account of how the compounding operates. Twice-struck districts lose about 3 percent of staple harvest value, with rice hit hardest, and cut food expenditures by 34 percent, while their non-food expenditures rise by a fifth—driven primarily by housing repairs, which increase by nearly half relative to the control mean—leaving total outlays statistically unchanged on average. Once-struck districts, by contrast, show no detectable losses in production or food spending: they finance repairs by modestly raising total spending, keeping their food budgets intact. The pattern is consistent with households absorbing one storm but not two.

Our study contributes, first, to the literature on the welfare costs of weather shocks in developing countries. Individual events reduce income and consumption (Dercon 2004; Jayachandran 2006; Khandker 2007), trigger coping through disinvestment, migration, and external flows (Anttila-Hughes and S. Hsiang 2013; Gröger and Zylberberg 2016; Yang 2008), and can leave scars in health and human capital that persist across years and even generations (Maccini and Yang 2009; Hoddinott and Kinsey 2001; Caruso 2017)—although recovery is possible where post-disaster aid rebuilds assets (Gignoux and Menéndez 2016); for tropical cyclones, the closest evidence comes from single storms or from pooled seasonal exposure, including in Madagascar itself (Keller and Mulangu 2025; Strobl 2012).[1] We add the margin this literature rarely observes: holding the setting, data, and design fixed, we compare households struck once with households struck twice within three weeks, and show that the FCS loss from two strikes is more than double that from one—so single-event estimates understate the welfare cost of a season in which storms arrive back to back.

Second, we speak to the literature on risk and insurance in poor economies. Informal risk sharing and self-insurance are incomplete (Townsend 1994; Udry 1994; Deaton 1991), consumption risk deters profitable investment in ways that can perpetuate poverty (Dercon and Christiaensen 2011), shocks near asset thresholds can have persistent consequences (Carter et al. 2007; Balboni et al. 2022), and safety nets can strengthen resilience

[1] Keller and Mulangu (2025) evaluate the same 2022 cyclone season in Madagascar using a different data source: the national EPM household survey (INSTAT), whose 2020 round serves as their pre-period and whose 2022 round, fielded from late 2021, as their post-period. Their headline specification pools exposure into a single affected–unaffected contrast, while extensions estimate storm-specific effects, windspeed-intensity tiers, and pairwise storm interactions, finding limited evidence of compounding across storms. Our design instead separates once- from twice-struck districts as the primary exposure margin and focuses on food security outcomes; on that margin, we find strong compounding.

to climatic shocks (Macours et al. 2022). Our contribution is to document what the failure of these buffers looks like when shocks arrive faster than buffers can be rebuilt: a forced reallocation of household budgets in which repairs crowd out food while total spending does not rise. The contrast with once-struck households, whose repairs are absorbed without cutting food, points to depleted coping capacity as a central source of the compounding, though our discrete exposure measures cannot fully separate the sequence of strikes from their cumulative physical intensity.

Third, we connect economics to the emerging work on compound climate events. Climate science warns that compound and consecutive extremes will become more frequent (Zscheischler et al. 2018; Zscheischler et al. 2020) and has begun to quantify the food-security toll of weather extremes (Reed et al. 2022), but the social-science evidence on overlapping crises remains largely descriptive or associational (Randell et al. 2021; Edwards et al. 2021; Rakoto Harison et al. 2024). To our knowledge, ours are among the first causal household-level estimates in a low-income country to separate single from repeated within-season storm exposure by design, using mutually exclusive exposure groups and direct equality tests.

The remainder of the paper proceeds as follows. Section 2 describes the setting and the 2022 cyclones. Section 3 presents the data, outcome measures, and empirical strategy. Section 4 reports the main results and examines mechanisms. Section 5 concludes.

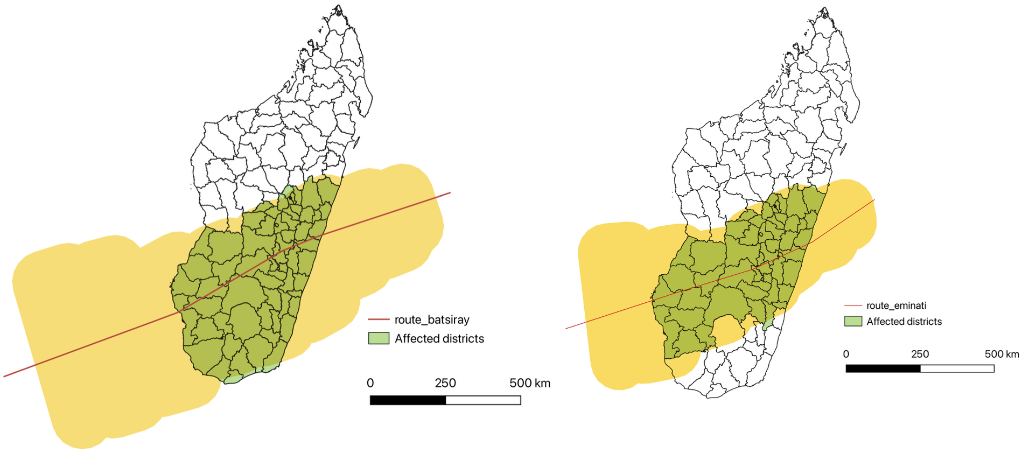
2 Context and Setting

Madagascar's east coast sits squarely in the southwest Indian Ocean cyclone corridor, and severe storms strike the island almost every year. In cumulative terms, tropical cyclones are the country's most damaging natural hazard: they have affected more people than any other disaster type, over six million according to EM-DAT records (Table A2). Beyond their immediate toll, cyclones repeatedly destroy bridges, roads, schools, and hospitals, isolating communities, disrupting supply chains, and slowing recovery in a country with limited fiscal space for reconstruction.

The welfare consequences of this hazard exposure reflect low adaptive capacity as much as the storms themselves. Livelihoods in rural Madagascar depend heavily on rain-fed cultivation of rice, cassava, and other staples; many households rely on market purchases to eat during the December–March lean season; physical isolation makes markets hard to reach; and savings, credit, and formal safety nets are scarce (Harvey et al. 2014; Herrera et al. 2021). A cyclone can therefore reduce food security through several channels at once: destruction of standing crops and productive assets, disruption of local markets, and losses of income from reduced agricultural labor demand. These channels motivate our focus on agricultural production and household expenditures alongside food security outcomes.

In early 2022, two powerful systems crossed the island within eighteen days. Batsirai came ashore on 5 February 2022 with mean winds near 165 km/h; Emnati followed on 23 February with mean winds of 120 km/h, on a corridor that overlapped substantially with Batsirai's (FAO 2022). The two storms rank among the most destructive cyclones in Madagascar's recent history, and their close spacing left virtually no time for recovery between strikes. Table A1 summarizes official damage statistics from the national disaster management agency (BNGRC): together the storms killed 136 people, displaced more than 130,000, flooded, damaged, or destroyed close to 50,000 homes, and wiped out nearly 30,000 hectares of rice fields. In the post-cyclone assessment of the Food and Agriculture Organization (FAO), 61 percent of the cultivated food-crop area in the regions along the storms' path was affected; food crop losses, mostly rice and cassava, were estimated at roughly US$61 million, with a further US$78 million lost in cash crops (FAO 2022).

Figure 1: Cyclone Routes



(a) Cyclone Batsirai

(b) Cyclone Emnati

Note: The figure displays the trajectories of Cyclones Batsirai (left) and Emnati (right) across Madagascar and the districts classified as affected. Red lines indicate cyclone routes; shaded districts represent affected areas.

The two storms' paths generated the variation we exploit. As Figure 1 shows, Batsirai crossed a broad band of southern Madagascar, while Emnati's narrower path fell entirely within the area already struck by Batsirai. We classify districts into three mutually exclusive exposure categories based on the observed tracks: *Unaffected*, *First cyclone only*, and *Both cyclones*. This categorical classification, formalized in Section 3.4, is the backbone of our difference-in-differences design.

3 Data and Empirical Strategy

3.1 Data

Our data come from annual household surveys conducted by the Ministry of Agriculture and Livestock of Madagascar. The surveys collect detailed information on household demographics, agricultural production, expenditures, and food security, and were fielded in four rounds covering 2020 through 2023. Each round is labeled by its survey program year; fieldwork took place at the close of the preceding calendar year—in November 2019, November 2020, and November 2021 for the 2020, 2021, and 2022 rounds, and in November–December 2022 for the 2023 round—according to the Ministry's fieldwork records. This timing has two implications for the design. First, the 2022 round was completed more than two months before the first cyclone's landfall in February 2022 and is therefore pre-treatment. Second, the 2023 round—our post-period—was completed roughly nine months after the storms and before Cyclone Freddy struck the same stretch of coast in February 2023, so the estimates are not contaminated by the following season's storms. Retrospective modules reference the calendar year in which fieldwork occurs: harvest values reported in round t cover the agricultural seasons of calendar year $t-1$. The surveys are repeated cross-sections rather than a panel: each round draws a fresh sample of households. Our analysis covers the 112 districts of the southern half of the country, the area spanned by the two cyclones' paths and their plausible comparison districts. Pooling the four rounds, the estimation sample comprises 49,695 household observations; the 2021 round alone includes 12,242 households, of which 5,632 reside in unaffected districts, 3,305 in districts struck by the first cyclone only, and—coincidentally an identical count—3,305 in districts struck by both.[2]

We supplement the surveys with district-level precipitation data from CHIRPS, the Climate Hazards group Infrared Precipitation with Stations archive (Funk et al. 2015). For each district and survey year we compute total annual precipitation and standardize it into a z-score using the pooled mean and standard deviation across the estimation sample. These data enter the robustness analysis in Section 4.3, which asks whether the estimated cyclone effects merely reflect rainfall variation.

[2]Sample sizes vary marginally across outcomes owing to item nonresponse (e.g., 49,679 observations for food expenditures).

Table 1: Summary Statistics by Cyclone Exposure (2021)

	(1) Unaffected	(2) First Cyclone only	(3) Both Cyclones
Panel A. Household head characteristics			
Age	38.16	40.89	39.84
	[14.89]	[15.01]	[14.98]
=1 if married	0.66	0.68	0.67
	[0.47]	[0.47]	[0.47]
=1 if completed primary education	0.31	0.29	0.26
	[0.46]	[0.45]	[0.44]
=1 if completed secondary education	0.16	0.15	0.15
	[0.37]	[0.36]	[0.35]
=1 if rural	0.48	0.56	0.55
	[0.50]	[0.50]	[0.50]
Panel B. Economic welfare			
Value of staple crop harvest ('000 MGA)	1791.72	1917.74	1835.57
	[577.48]	[476.64]	[543.38]
Food expenditures ('000 MGA)	41.03	42.99	34.02
	[49.74]	[54.47]	[49.06]
Non-food expenditures ('000 MGA)	108.47	90.07	72.25
	[216.57]	[211.81]	[185.08]
Panel C. Food security			
Food Consumption Score	38.08	37.05	34.26
	[14.68]	[16.25]	[14.49]
Food Insecurity Experience Scale	5.85	6.38	6.80
	[1.54]	[1.53]	[1.27]
Food Security Index	0.00	-0.31	-0.63
	[1.00]	[1.09]	[0.93]
Number of observations	5,632	3,305	3,305

Note: Descriptive statistics for the pre-treatment year 2021. Means are reported with standard deviations in brackets. Column 1: unaffected districts; Column 2: districts struck by the first cyclone only; Column 3: districts struck by both cyclones. The recall period for food and non-food expenditures is the past 7 days. Continuous monetary variables are winsorized at the 1st and 99th percentiles. The value of the staple crop harvest aggregates rice, maize, and cassava—the outcome examined in Table 3—and covers all seasons in the previous calendar year (typically two harvests). Corresponding statistics for 2020, 2022, and 2023 are reported in Table A3, Table A4, and Table A5.

Table 1 reports summary statistics for the 2021 round by exposure group. Household head characteristics—age, marital status, and education—are broadly similar across the three groups, with somewhat higher rural shares in exposed districts. Economic and food security outcomes, by contrast, differ in levels: households in districts that would later be struck by both cyclones enter the period with lower expenditures and markedly worse food security than those in unaffected districts—a lower Food Consumption Score (34.26 versus 38.08) and a higher Food Insecurity Experience Scale (6.80 versus 5.85)—while staple harvest values are comparable across the three groups. These level differences reflect the persistent disadvantages of the southeastern cyclone corridor. Our identification strategy does not require levels to be equal across groups; it requires parallel trends, which we assess in Section 3.6.

3.2 Measuring Food Security

We measure household food security with two complementary indicators and a composite index.

The first indicator is the Food Consumption Score (FCS), developed by the World Food Programme (World Food Programme 2008). The survey asks how many of the past seven days the household consumed each of eight food groups: main staples, pulses, vegetables, fruit, meat and fish, milk, sugar, and oil. The FCS is computed by multiplying each group's consumption frequency by its standard weight (2, 3, 1, 1, 4, 4, 0.5, and 0.5, respectively) and summing, yielding a score from 0 to 112. Higher values indicate greater dietary quantity and diversity over the short run.

The second indicator is the Food Insecurity Experience Scale (FIES), developed by the FAO (Cafiero et al. 2018). The FIES consists of eight yes/no questions on experiences of food insecurity over the past twelve months due to lack of money or other resources: worrying about food, being unable to eat healthy and nutritious food, eating only a few kinds of food, skipping meals, eating less than one should, running out of food, being hungry but not eating, and going a whole day without eating. We use the raw score, ranging from 0 to 8, with higher values indicating more severe food insecurity.[3]

The two indicators are complementary in horizon and content: the FCS captures dietary adequacy over the preceding week, while the FIES captures experienced hardship over the preceding year.[4] We therefore interpret FCS effects as short-horizon dietary adjustments and FIES effects as medium-horizon experiential shortfalls. To summarize overall food security, we also construct a composite Food Security Index that aggregates

[3]Given the fielding dates reported in Section 3.1, the twelve-month recall window of the post-period round spans roughly December 2021 through December 2022, and thus includes about two months that predate the February 2022 landfalls. To the extent that responses partly reflect those pre-storm months, the estimated FIES effects are attenuated toward zero and are, if anything, conservative.

[4]In prior work, high-frequency measures of food security and expenditures have been combined with event-study and DiD designs to separate seasonal movements from shock responses (Aggarwal et al. 2022); the recall structure here serves a similar purpose at an annual frequency.

the two indicators using inverse-covariance weighting in the spirit of Anderson (2008).[5]

3.3 Measuring Agricultural Output and Staple Prices

The surveys record crop output in heterogeneous units—kilograms, tins, medium bags, and large bags, among others—each potentially combined with several commodity forms (cob, grain, shelled, unshelled, whole). Standard FAO technical factors for converting quantities into kilograms cannot be applied to all unit–form combinations and do not distinguish among commodity forms, so we convert output into monetary values instead. For each crop, the questionnaire records the total quantity produced, the quantities consumed, given away, and sold, and the total revenue from sales. We compute the implicit unit price of each commodity as sales revenue divided by the quantity sold, and multiply this price by the total quantity produced to obtain the value of each household's harvest in the national currency, the Malagasy ariary (MGA). Our harvest outcome aggregates the resulting values for the three staple crops—rice, maize, and cassava—over all seasons in the previous calendar year.

The staple prices used in the balance checks of Section 3.6 apply the same unit-value approach to the unit–form combination most commonly reported by farmers for each crop: large bag/grain for rice, large bag/whole for cassava, and kilogram/grain for maize. The resulting prices are constructed at the district level and are constant across survey rounds; they therefore serve to check balance across exposure groups rather than as time-varying outcomes.

3.4 Cyclone Exposure Classification

We assign districts to exposure categories using the storms' observed tracks and the official classification of affected districts. Of the 112 districts in the study area, Batsirai struck 65; Emnati subsequently struck 37, all of which had already been struck by Batsirai. This nesting yields three mutually exclusive groups: 47 *Unaffected* districts, 28 *First cyclone only* districts, and 37 *Both cyclones* districts. The categorical definition avoids double counting households in the overlap and gives the two treatment coefficients a transparent interpretation: the effect of one strike, and the effect of two strikes in close succession.

[5] Components are signed and standardized so that higher values of the index indicate greater food security.

3.5 Estimation

We exploit the spatial variation in exposure generated by the storms' tracks and estimate the following difference-in-differences specification:

$$\begin{aligned} Y_{i(d)t} = {} & \beta_1\, FirstCyclone_d \cdot Post_t + \beta_2\, BothCyclones_d \cdot Post_t \\ & + \delta_d + \gamma_t + \theta' X_i + \varepsilon_{i(d)t}, \end{aligned} \tag{1}$$

where $Y_{i(d)t}$ is the outcome of household i in district d in year t; $FirstCyclone_d$ and $BothCyclones_d$ are indicators for districts struck once and twice, respectively; $Post_t$ equals one in 2023 and zero in 2020–2022;[6] X_i is a vector of household-head controls (age, marital status, completion of primary education, completion of secondary education, and rural residence, each paired with an indicator for missing values); and δ_d and γ_t are district and year fixed effects. The fixed effects absorb time-invariant differences across districts and shocks common to all districts in a given year. We cluster standard errors at the district level (112 clusters), the level of treatment assignment.

The coefficients of interest, β_1 and β_2, measure the average post-cyclone change in outcomes in once- and twice-struck districts relative to the change in unaffected districts. For each outcome we also report the p-value from a test of $\beta_1 = \beta_2$, which asks whether the second strike added to the damage of the first.

3.6 Identification

The validity of the design rests on parallel trends: absent the cyclones, outcomes in exposed and unexposed districts would have evolved similarly. Three pieces of evidence support this assumption.

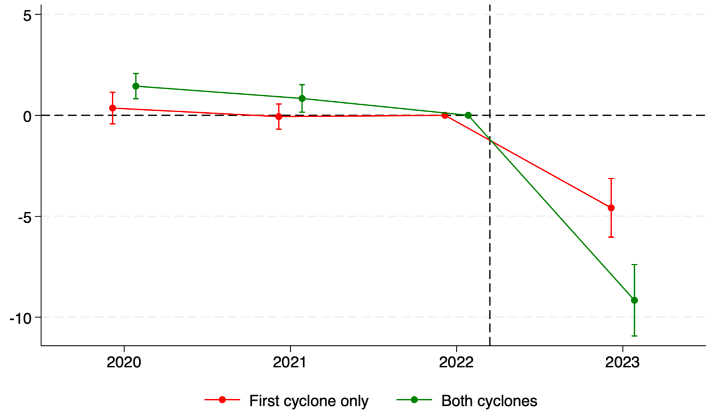
First, pre-cyclone trajectories are parallel. Figure A3 plots annual means of the FCS

[6] The 2022 survey round was fielded in November 2021, more than two months before the cyclones made landfall (Section 3.1); we accordingly treat it as pre-treatment. Three features of the data corroborate this classification. Harvest outcomes in the 2022 round reference the 2021 calendar year by construction, and the FIES's twelve-month recall window in that round closes in November 2021, entirely before the storms. The event-study leads in Table A11 show no trace of the post-cyclone collapse in the 2022 round: had interviews in struck districts followed the landfalls, the 2020–2021 leads measured against the 2022 base would approach the treatment effect itself rather than the small values we estimate. And dropping the 2022 round entirely leaves the estimates essentially unchanged (FCS effects of −4.72 and −10.27 for once- and twice-struck districts, against −4.69 and −9.96 in the baseline). Because the survey data are annual, we cannot exploit higher-frequency variation around the landfall dates.

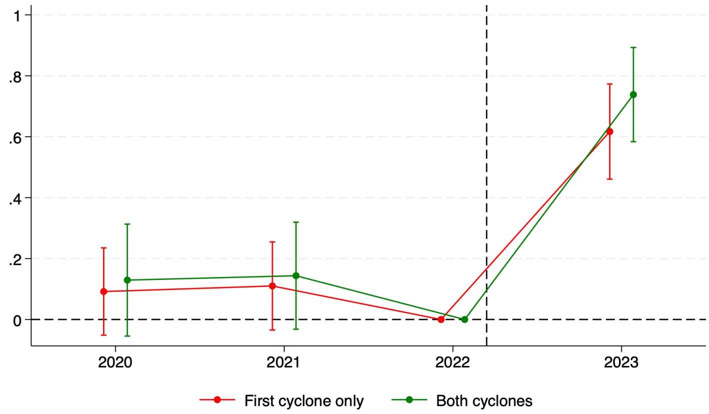
and FIES by exposure group over 2020–2023. Levels differ across groups throughout the pre-period, consistent with the summary statistics, but the three groups move together until 2022 and diverge sharply only in 2023, after the cyclones. Pre-trends for the composite index show the same pattern; trajectories for staple harvest value and food expenditures are more variable across rounds, an issue we examine formally in Section 4.3 (Figure A4). The level gaps themselves are absorbed by district fixed effects; what matters for identification is the common pre-period slope. Event-study estimates that interact exposure with survey years (base year 2022, the last pre-cyclone round) formalize this: for once-struck districts, all leads are individually insignificant and jointly indistinguishable from zero for every food security outcome (joint p-values of 0.42, 0.30, and 0.23 for the FCS, FIES, and index); for twice-struck districts, leads are jointly insignificant for the FIES ($p = 0.23$) and the index ($p = 0.19$), while the FCS shows small positive leads (1.45 and 0.84 points in 2020 and 2021), indicating a mild downward relative drift into 2022 (Table A11; Figure 2). That drift is an order of magnitude smaller than the 2023 treatment effect of -9.16 points, and extrapolating it linearly through 2023 would account for less than a tenth of the estimated impact.

Figure 2: Event-Study Estimates: Food Security Outcomes

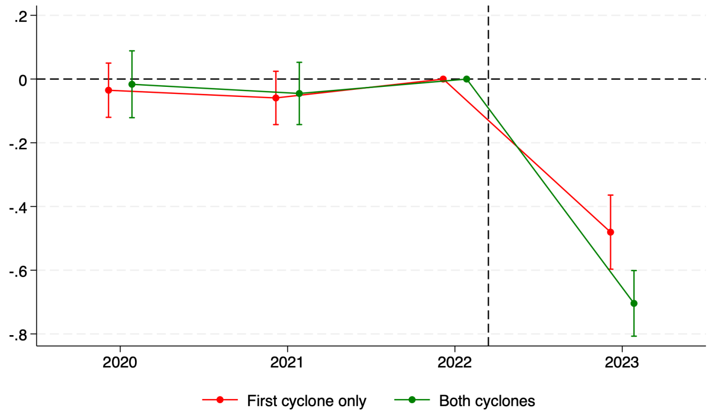
(a) Food Consumption Score (FCS)



(b) Food Insecurity Experience Scale (FIES)



(c) Food Security Index



Note: Event-study coefficients and 95 percent confidence intervals from the specification in Table A11: outcomes are regressed on interactions of exposure group with survey-year indicators, omitting 2022 (the last pre-cyclone round) as the base period, with district and year fixed effects and household-head controls. The 2022 base period is shown at zero without a confidence interval; the vertical dashed line marks the February 2022 cyclones. Red: first-cyclone-only districts; green: both-cyclones districts. Standard errors clustered at the district level.

Second, cyclone exposure in Madagascar is event-specific rather than a fixed characteristic of certain districts. Figure A1 and Figure A2 map the tracks of Cyclone Gafilo (2004) and Cyclone Enawo (2017), the two most intense cyclones to strike the country in the two decades before 2022. Their paths—Gafilo entering from the northeast and looping across the southwest, Enawo crossing the interior from north to south—overlap only partially with the 2022 corridor: many districts struck in 2022 were untouched by either storm, and vice versa. If the same districts were struck by every major storm, our design would risk conflating the 2022 shocks with chronic structural vulnerability; the shifting footprint of successive cyclones limits this concern.

Third, exposure is uncorrelated with pre-existing local price environments. Staple food prices in our data are district-level unit values that do not vary over time, so they cannot serve as outcomes; instead, we use them as a balance check. Cross-sectional regressions of log staple prices on exposure indicators with region fixed effects yield small and statistically insignificant coefficients for rice, maize, and cassava alike (Table A6; levels in Table A7), indicating that the storm paths did not systematically select districts with different price environments.

Finally, a violation of the stable unit treatment value assumption (SUTVA) would work against us. The cyclones may have affected unstruck districts through regional commodity markets, displaced populations, or diverted aid; related evidence shows that the local price effects of weather shocks depend on market integration, consistent with connected markets diffusing shocks across space (Hill and Fuje 2020). To the extent such spillovers worsened outcomes in control districts, they compress the treatment–control contrast, and our estimates are likely conservative.

4 Results

4.1 Effects on Food Security

Table 2: Cyclone Impacts on Food Security

	(1) FCS	(2) FIES	(3) Food Security Index
First Cyclone Only × Post	-4.69***	0.55***	-0.45***
	(0.82)	(0.07)	(0.06)
Both Cyclones × Post	-9.96***	0.64***	-0.68***
	(0.93)	(0.07)	(0.06)
p-value: equality	0.000	0.175	0.001
Control mean	38.08	5.85	0.00
Control SD	14.68	1.54	1.00
R-squared	0.237	0.220	0.279
Observations	49,695	49,695	49,695

Note: Regression estimates of equation (1). Outcomes are the Food Consumption Score (FCS, scale 0–112), the Food Insecurity Experience Scale (FIES, scale 0–8), and the composite Food Security Index (standardized; higher values indicate greater food security). All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The p-value tests equality of the two interaction coefficients. Control mean and SD are for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2 reports the estimated impacts of cyclone exposure on household food security. A single strike is costly: relative to unaffected districts, households in first-cyclone-only districts experience a decline in the FCS of 4.69 points—12 percent of the pre-cyclone (2021) control mean—and an increase in the FIES of 0.55 points (9 percent). The composite Food Security Index falls by 0.45 standard deviations. Given the recall windows of the two indicators, these estimates imply worse dietary adequacy roughly nine months after the storms and more frequent experiences of food insecurity over the intervening months.

The second strike substantially deepens the losses in dietary adequacy. In districts struck by both cyclones, the FCS falls by 9.96 points—26 percent of the control mean and more than double the single-strike effect—and the composite index falls by 0.68

standard deviations; for both outcomes, we reject equality of the single- and double-strike coefficients at the 1 percent level. The FIES effect is larger under double exposure as well (0.64 points, or 11 percent), but we cannot reject equality with the single-strike coefficient ($p = 0.17$). One reading of this contrast is that the FIES, already high at baseline (5.85 out of a maximum of 8 in unaffected districts), has limited room to register further deterioration, while the FCS continues to capture cuts in the quantity and diversity of what households actually eat. The second strike thus deepens losses on every measure, and the form of the compounding differs across them: for the FCS, the incremental damage from the second cyclone (-5.27 points) exceeds the entire effect of the first (-4.69), whereas for the FIES and the index the increments are smaller than the initial effects. By the measure that captures what households actually eat, the marginal storm hurt more than the first one.

4.2 Mechanisms

To understand how the compounded shock translates into food insecurity, we examine the proximate channels our data can speak to: what households produce, what they spend, and the local prices they face.

4.2.1 Agricultural Production

Table 3: Cyclone Impacts on Agricultural Harvest ('000 MGA)

	(1) Staple crops (total)	(2) Rice	(3) Maize	(4) Cassava
First Cyclone Only × Post	-0.11	-0.92	1.06	-0.24
	(9.22)	(8.57)	(0.73)	(1.01)
Both Cyclones × Post	-60.15***	-52.19***	-1.69**	-6.27***
	(12.28)	(11.97)	(0.71)	(1.34)
p-value: equality	0.000	0.000	0.004	0.000
Control mean	1,791.72	1,153.22	341.75	296.75
Control SD	577.48	579.39	30.18	29.99
R-squared	0.188	0.182	0.036	0.049
Observations	49,695	49,695	49,695	49,695

Note: Regression estimates of equation (1). Outcomes are the value of the agricultural harvest in the previous calendar year, in total and by staple crop, expressed in thousands of Malagasy Ariary ('000 MGA); Section 3.3 details the valuation method. The total aggregates the three staple crops (rice, maize, and cassava). All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The p-value tests equality of the two interaction coefficients. Control mean and SD are for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3 shows that measurable production losses are confined to districts struck twice. In first-cyclone-only districts, the estimated change in staple harvest value is small and statistically indistinguishable from zero for every crop. In twice-struck districts, by contrast, total staple harvest value falls by 59.0 thousand MGA per household, about 3 percent of the control mean, and the equality of the two coefficients is rejected at the 1 percent level. The losses are concentrated in rice, the dominant staple, whose harvest value falls by 51.2 thousand MGA (4 percent of its control mean); cassava and maize decline by 6.1 and 1.7 thousand MGA, respectively. Poisson estimates, which retain zeros and measure proportional effects, show the same pattern, with significant declines only in twice-struck districts (Table A18). Physical output quantities by crop and unit (Table A8–Table A10), which are observed only for the subset of households reporting production in the relevant unit, are noisier: rice shows declines concentrated in twice-struck districts, most clearly for grain in large bags, while the far smaller cassava and maize samples yield imprecise estimates.

Two features of these results matter for interpretation. First, the production channel is real but quantitatively modest: a 3 percent decline in staple harvest value seems unlikely, on its own, to generate a 26 percent decline in dietary adequacy. Second, the fact that a single strike leaves no detectable production losses while two strikes do suggests a nonlinearity consistent with the agronomy of the setting: fields flooded once can often be replanted or partially salvaged within the season, while a second inundation weeks later destroys the replanting itself. The next subsection shows that household budgets carry much of the remaining explanation.

4.2.2 Household Expenditures

Table 4: Cyclone Impacts on Household Expenditures ('000 MGA)

	(1) Food	(2) Non-food	(3) Total
First Cyclone Only × Post	-0.52	11.58*	10.81*
	(0.89)	(6.09)	(6.50)
Both Cyclones × Post	-13.82***	22.62***	8.03
	(1.10)	(4.63)	(5.32)
p-value: equality	0.000	0.065	0.670
Control mean	41.03	108.47	150.48
Control SD	49.74	216.57	242.96
R-squared	0.218	0.159	0.196
Observations	49,679	49,695	49,679

Note: Regression estimates of equation (1). Outcomes are weekly household food, non-food, and total expenditures in '000 MGA, winsorized at the 1st and 99th percentiles. All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The p-value tests equality of the two interaction coefficients. Control mean and SD are for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4 examines household spending, and the contrast between one and two strikes is stark. Households struck once do not cut food spending: the coefficient is a precisely estimated zero (-0.52 thousand MGA against a control mean of 41.0). Instead, their non-food spending rises by 11.6 thousand MGA (11 percent) and their total outlays increase by a similar amount (marginally significant), indicating that the repair bill after a single storm was financed by expanding the budget—through means our data do not directly observe, such as savings, transfers, or borrowing—rather than by eating less.

In twice-struck districts, that strategy was no longer available. Total expenditures are statistically unchanged on average (8.0 thousand MGA, $p > 0.1$)—though, as we show below, this average conceals sharply divergent responses across households—and the composition shifts markedly: food spending falls by 13.8 thousand MGA per week—34 percent of the control mean—while non-food spending rises by 22.6 thousand MGA (21 percent). Table 5 shows the food cuts are broad-based, reaching every category we observe: staples (−3.3 thousand MGA, 21 percent of the category mean), meat (−3.6; 32 percent), fruits and vegetables (−3.3; 52 percent), and smaller but uniformly negative and significant changes in milk, oil, snacks, and spice and sugar. Cuts of this breadth—spanning both calorie-dense staples and nutrient-dense animal products and vegetables—are mirrored in the 26 percent decline in the FCS.

Table 5: Cyclone Impacts on Food Expenditures by Category ('000 MGA)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Household expenditures (past week):							
	Staples	Fruits & vegetables	Meat	Milk	Oil	Snack	Spice & sugar	Total
First Cyclone Only × Post	-0.31	-0.27*	0.37	-0.02	-0.03	-0.08	0.01	-0.52
	(0.45)	(0.15)	(0.23)	(0.03)	(0.05)	(0.09)	(0.03)	(0.89)
Both Cyclones × Post	-3.31***	-3.28***	-3.55***	-0.33***	-1.01***	-1.56***	-0.75***	-13.82***
	(0.46)	(0.23)	(0.34)	(0.04)	(0.07)	(0.10)	(0.05)	(1.10)
p-value: equality	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Control mean	15.65	6.32	11.10	0.72	2.03	2.78	1.47	41.03
Control SD	26.77	7.58	15.39	2.05	2.55	4.88	1.95	49.74
R-squared	0.127	0.237	0.191	0.097	0.196	0.136	0.156	0.218
Observations	49,679	49,679	49,679	49,679	49,679	49,679	49,679	49,679

Note: Regression estimates of equation (1). Outcomes are weekly household expenditures on each food category in '000 MGA, winsorized at the 1st and 99th percentiles. Columns (2) and (7) report fruits and vegetables, and spice and sugar, as combined categories. All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The p-value tests equality of the two interaction coefficients. Control mean and SD are for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Cyclone Impacts on Non-Food Expenditures by Category ('000 MGA)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Household expenditures (past week):						
	Health	Transport, Energy & Communication	Clothing & Footwear	Education & Leisure	Hygiene & Personal Care	Housing repairs	Total
First Cyclone Only × Post	0.33	-0.59	3.30***	0.06	1.05***	5.33***	11.58*
	(0.22)	(3.12)	(0.78)	(0.10)	(0.32)	(1.51)	(6.09)
Both Cyclones × Post	-0.03	5.81**	0.42	0.03	1.19***	11.35***	22.62***
	(0.18)	(2.26)	(0.85)	(0.09)	(0.31)	(1.60)	(4.63)
p-value: equality	0.104	0.026	0.002	0.746	0.664	0.000	0.065
Control mean	3.48	26.96	28.84	1.85	8.17	22.80	108.47
Control SD	15.91	76.38	45.70	5.19	16.72	69.94	216.57
R-squared	0.034	0.127	0.162	0.083	0.148	0.085	0.159
Observations	49,695	49,695	49,695	49,695	49,695	49,695	49,695

Note: Regression estimates of equation (1). Outcomes are weekly household expenditures on each non-food category in '000 MGA, winsorized at the 1st and 99th percentiles. Column (2) combines transport, energy and utilities, and communication; column (4) combines education and training with leisure, social, and religious expenses. All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The p-value tests equality of the two interaction coefficients. Control mean and SD are for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6 shows where the reallocated spending went. Housing repairs dominate: repair spending rises by 5.3 thousand MGA per week (23 percent of the category mean) after one strike and by 11.3 thousand MGA (50 percent) after two—the largest increase of any category and, on its own, half of the total non-food increase in twice-struck districts. Combined spending on transport, energy, and communication rises as well (5.8 thousand MGA), alongside a smaller increase in hygiene and personal care—a pattern consistent with rebuilding costs and disrupted local services rather than discretionary consumption. Notably, the annualized food-expenditure cut in twice-struck districts is roughly an order of magnitude larger than the estimated loss in staple harvest value, suggesting that the compression of food budgets, rather than lost own-production alone, is the dominant proximate driver of the deterioration in food security.[7]

For proportional effects, we report Poisson (PPML) estimates rather than log specifications, because post-cyclone spending has substantial mass at zero: in twice-struck districts, the probability of any food purchase during the recall week falls by 51 percent-

[7] A reduction of 13.8 thousand MGA per week corresponds to roughly 719 thousand MGA over a year, compared with an estimated staple harvest value loss of 60 thousand MGA per year (Table 3). The comparison is indicative: harvest value covers only the three staple crops and is measured with imputed unit values (Section 3.3).

age points from a pre-period base of essentially one (Table A19), so log transformations would conflate intensive and extensive margins. The Poisson results sharpen the picture: proportionally, food spending falls steeply and total spending declines for the typical household (-0.95 and -0.25 log points), while the non-food increase loses significance—the OLS means for non-food and total spending are held up by continued outlays among purchasing households, concentrated in repairs, that offset the collapse at the extensive margin (Table A17); staple harvest losses mirror those in Table 3 (Table A18). The zero-purchase households do not appear to be substituting toward other food sources: food received as gifts or assistance is flat across exposure groups and years, and households reporting no purchases record lower Food Consumption Scores than purchasing households in the same districts.

4.2.3 Food Prices

A natural remaining question is whether local food price increases contributed to the decline in food security. As noted in Section 3.3, our data do not permit a direct test: staple prices are time-invariant district unit values, so price dynamics around the cyclones are unobservable to us, and the balance checks in Table A6 and Table A7 establish only that exposure is uncorrelated with pre-existing price levels. To the extent the cyclones did raise prices regionally—in struck and neighboring control districts alike—our difference-in-differences estimates would understate the total welfare cost.

4.3 Robustness

Four additional exercises probe the stability of these results. First, the pre-period overlaps a severe drought in Madagascar's Grand Sud, which culminated in a widely reported food crisis in 2021–2022 and affected several districts in our comparison pool. Dropping the three Grand Sud regions (Androy, Anosy, and Atsimo-Andrefana) leaves the conclusions intact—FCS effects of -3.55 and -8.98 for once- and twice-struck districts, respectively, against -4.69 and -9.96 in the full sample—and sharpens the single-versus-double contrast: excluding these regions, we reject equality of the two exposure coefficients for all three food security outcomes, including the FIES ($p = 0.029$; Table A14).

Second, the estimates are not an artifact of rainfall variation. Cyclones are extreme rainfall events as well as wind events, and harvests and food security in southern Madagascar track precipitation closely, so equation (1) could in principle attribute to the storms what is really weather. Controlling for the standardized district-level annual precipitation from CHIRPS leaves the estimates essentially unchanged: the FCS effects move from -4.69 and -9.96 in the baseline to -4.22 and -9.15, staple harvest losses remain confined to twice-struck districts (-58.9 thousand MGA against -60.1 in the baseline), and the equality tests reach the same verdicts as in the baseline (Table A15; Table A16). A fully interacted specification tells the same story: the exposure effects evaluated at average rainfall are nearly identical to those with the rainfall control alone, and the interactions of the exposure effects with rainfall are jointly insignificant for every outcome (joint p-values of 0.11 or higher), with a single individually marginal term—the once-struck FCS interaction ($p = 0.046$)—suggesting, if anything, that effects attenuate in wetter years.

Third, inference does not hinge on large-sample approximations. Re-randomizing the exposure map across the 112 districts 1,000 times—preserving the group sizes and the nesting of Emnati's corridor within Batsirai's—never once generates placebo coefficients as large in absolute value as the observed ones, for any of the three food security outcomes or either exposure margin (permutation $p < 0.001$; Table A13).

Finally, we examine event-study dynamics for the mechanism outcomes (Table A12), and here the leads are less clean than for food security: pre-period interactions are erratic for non-food spending in both exposure groups, and twice-struck districts show a sizable harvest deviation in the 2020 round. Two considerations temper this concern. Harvest values refer to the previous calendar year and expenditure recalls are short, so wave-to-wave comparability is inherently more fragile for these outcomes than for the food security measures, and the 2020 deviation plausibly reflects earlier agricultural disruptions rather than a trend. More importantly, the conclusions do not depend on the choice of base year: measured against 2021 rather than 2022, the implied 2023 effects are nearly identical for food expenditures (-13.7 versus -13.8 thousand MGA), similar for non-food expenditures ($+19.1$ versus $+22.6$), and larger for staple harvest value (-81.2 versus -60.1). We accordingly read the mechanism magnitudes with somewhat more caution than the food security estimates, while noting that the qualitative pattern—production losses and budget reallocation concentrated in twice-struck districts—is robust to the base year.

4.4 Interpretation

Taken together, the results form a consistent pattern. A first cyclone damaged homes but left production capacity and food budgets largely intact: households financed repairs by expanding total spending, and food security fell, though by far less than after the second strike. The second cyclone, arriving eighteen days later, destroyed whatever had been salvaged or replanted—staple harvest value fell only when districts were struck twice—and confronted households with a second repair bill they could no longer absorb. With total outlays no higher than before, spending was reallocated toward housing repairs and utilities, food expenditures fell by a third, with cuts in every category we observe, and both dietary adequacy and experienced food insecurity deteriorated sharply. The amplification thus operated through the joint exhaustion of agricultural and financial buffers rather than through any single catastrophic channel. In the language of Sen (1981), the second cyclone undermined food entitlements—what households could grow and what they could afford to buy—rather than aggregate food availability alone.

5 Conclusion

This paper quantifies the welfare costs of sequential climate shocks by studying two cyclones that hit Madagascar eighteen days apart in early 2022. Using four rounds of household survey data and a difference-in-differences design across 112 districts, we find that a single cyclone reduces the Food Consumption Score by 12 percent and raises the Food Insecurity Experience Scale by 9 percent, while back-to-back exposure more than doubles the FCS loss to 26 percent and lowers the composite Food Security Index by 0.68 standard deviations. Only twice-struck districts show losses in staple agricultural production, and only they cut food spending—by roughly a third—while reallocating their budgets toward housing repairs.

The mechanism evidence points to buffer depletion as the source of the amplification. This interpretation connects our findings to the broader literature on incomplete insurance and asset-based poverty dynamics: when a second shock arrives before buffers are restored, temporary weather events translate into the kind of consumption collapses that this literature associates with persistent poverty (Carter et al. 2007; Balboni et al. 2022).

Several limitations qualify these conclusions. Our data are annual repeated cross-sections, so we observe neither within-household adjustment paths nor high-frequency dynamics around the storms, and only one post-cyclone survey round is available, leaving longer-run recovery or persistence beyond the scope of this paper. Repeated cross-sections also cannot track displaced households: if families uprooted by the storms are missing from the post-cyclone sampling frame, our estimates omit their losses, although group-level sample sizes are essentially unchanged between the 2022 and 2023 rounds, which argues against a collapse of the frame in struck districts. Exposure is measured at the district level as a discrete classification rather than a continuous intensity gradient, so we cannot separate the effect of being struck twice from that of greater cumulative storm intensity. Pre-period dynamics are also less stable for the recall-based production and spending measures than for the food security outcomes (Section 4.3), so the mechanism magnitudes carry wider uncertainty than the food security estimates. Food security outcomes are recall-based, and staple prices are observable only as time-invariant district unit values, so the price channel cannot be tested directly. Finally, spillovers onto comparison districts, if present, imply our estimates understate the true costs.

The findings carry two concrete policy implications. First, the speed of post-disaster support matters as much as its size: the pattern documented here is consistent with a second shock arriving before households had rebuilt their buffers, and ex-post aid flows typically replace only part of the losses and arrive with lags (Yang 2008). Evidence from other settings shows that rapid, rules-based disaster financing can sustain local economic activity in the window between shock and recovery (del Valle et al. 2020), and that productive safety nets can keep consumption from collapsing when shocks strike (Macours et al. 2022). The content of that support matters as well: post-disaster agricultural input programs do not automatically rebuild production capacity—design and delivery determine whether they help or backfire (Gignoux et al. 2023). Second, the dominant role of housing repairs in crowding out food spending suggests that investments in cyclone-resistant housing and reconstruction support are, in practice, also food security policy: reducing the repair bill after the next storm would directly relax the constraint that forced households to eat less. As compound and temporally clustered extremes become a growing concern (Zscheischler et al. 2018; Zscheischler et al. 2020), designing protection systems around the possibility of consecutive strikes—rather than the average single event—will become increasingly important.

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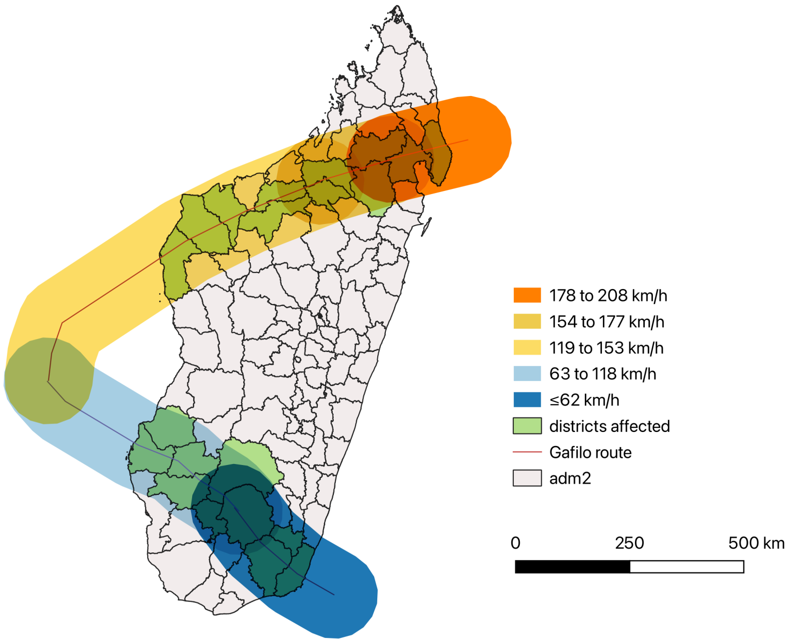
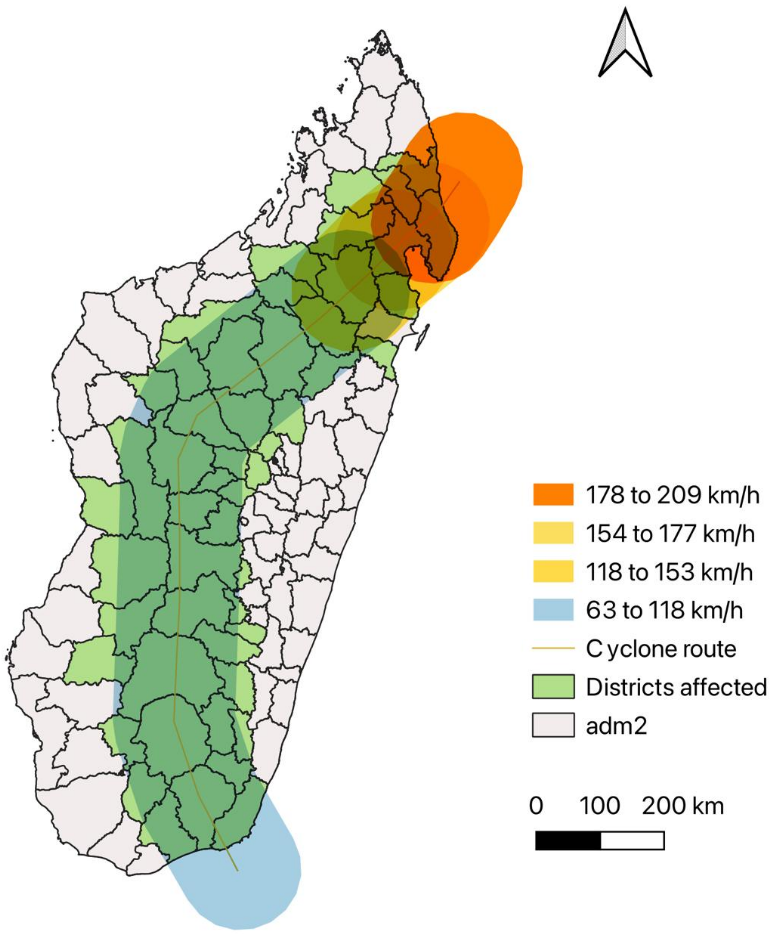
Appendix. Additional Figures and Tables

Figure A1: Cyclone Track: Gafilo (2004)



Note: Districts affected by Cyclone Gafilo (2004) are shown in green. High- and low-intensity wind zones are shaded in dark and light red, respectively. Gafilo entered from the northeast and, after looping through the Mozambique Channel, made a second landfall in the southwest. The partial overlap and partial non-overlap with the 2022 cyclone tracks (Figure 1) indicate that cyclone exposure in Madagascar shifts across districts from event to event, supporting the validity of the DiD design by mitigating the concern that exposure reflects chronic, structurally determined vulnerability.

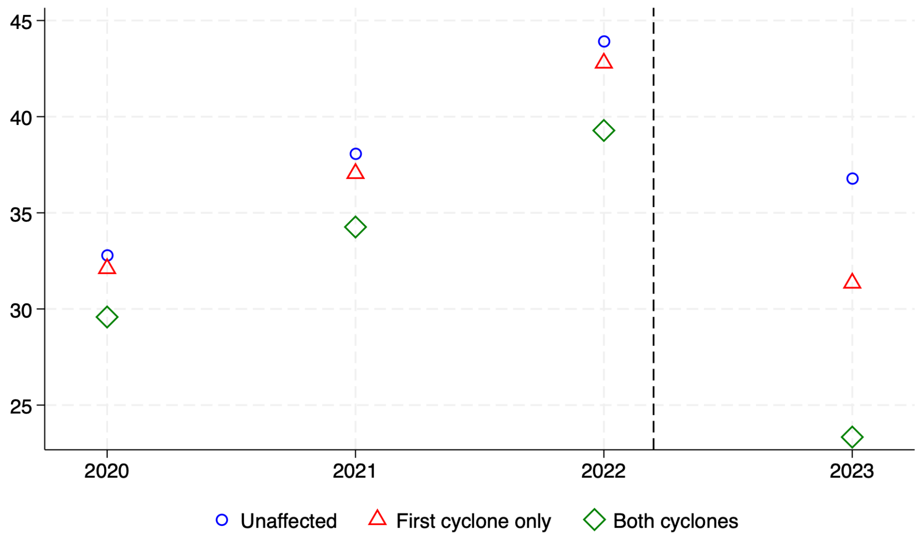
Figure A2: Cyclone Track: Enawo (2017)



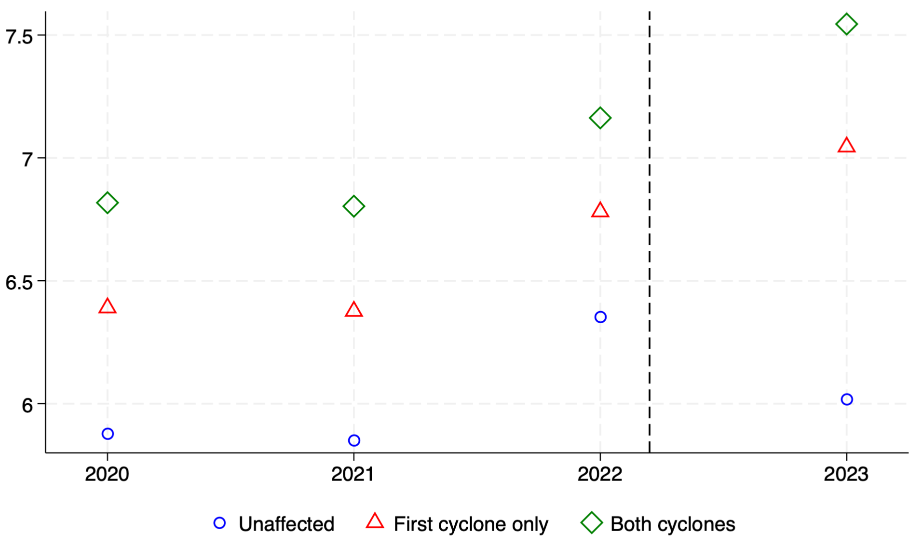
Note: Districts affected by Cyclone Enawo (2017) are shown in green; shaded bands indicate maximum wind speeds along the track, from 63–118 km/h (blue) to 178–209 km/h (orange). Enawo, the strongest cyclone to strike Madagascar since Gafilo (2004), made landfall in the northeast near Antalaha in March 2017 and crossed the island from north to south. Neither storm's footprint coincides with the 2022 corridor mapped in Figure 1.

Figure A3: Pre- and Post-Cyclone Trends in Household Food Security by Cyclone Exposure

(a) Food Consumption Score (FCS)



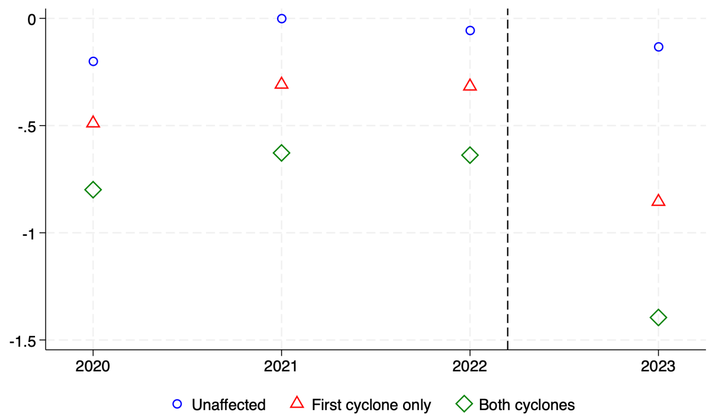
(b) Food Insecurity Experience Scale (FIES)



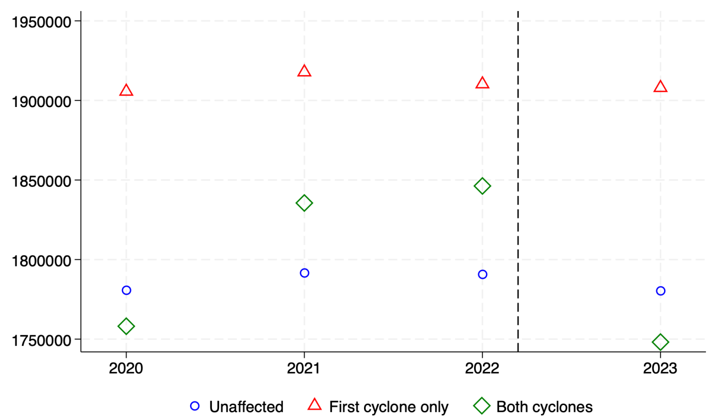
Note: Each panel plots annual group means for unaffected districts (circles, blue), first-cyclone-only districts (triangles, red), and both-cyclones districts (diamonds, green). The vertical line marks the February 2022 cyclones. Pre-2022 trends are broadly parallel across the three groups, supporting the parallel-trends assumption underlying the DiD design.

Figure A4: Pre- and Post-Cyclone Trends in Additional Outcomes by Cyclone Exposure

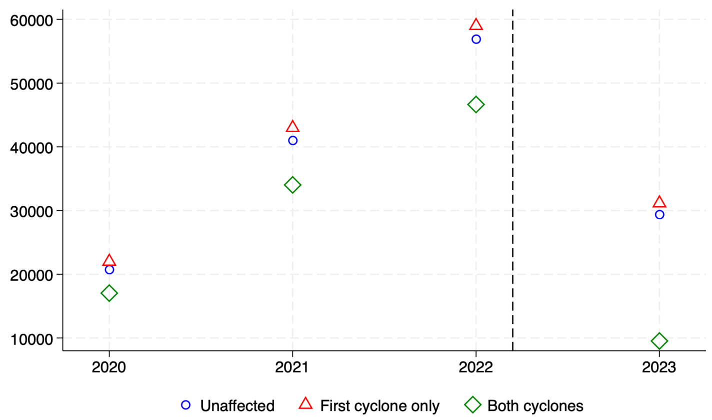
(a) Food Security Index



(b) Staple Harvest Value



(c) Food Expenditures



Note: Each panel plots annual group means for unaffected districts (circles, blue), first-cyclone-only districts (triangles, red), and both-cyclones districts (diamonds, green). The vertical line marks the February 2022 cyclones.

Table A1: Damages from the 2022 Cyclone Events

	1st Cyclone (*Batsirai*)	2nd Cyclone (*Emnati*)
Deaths	121	15
Injured	21	12
People affected	146,671	169,583
Displaced	56,949	74,994
Homes flooded	8,661	8,177
Houses destroyed	9,105	6,241
Houses damaged	5,000	12,057
Schools destroyed	2,562	11
Schools damaged	1,986	3
Rice fields destroyed (ha)	22,424	6,751

Note: Official damage counts by cyclone, as reported by the Bureau National de Gestion des Risques et des Catastrophes (BNGRC), Madagascar. "People affected" corresponds to the BNGRC category *sinistrés*.

Table A2: Total Number of People Affected by Natural Disasters in Madagascar

Natural disaster type	Total affected
Tropical cyclone	6,092,639
Drought	5,665,290
Locust infestation	2,300,000
Flood (general)	170,360
Viral disease	124,656
Riverine flood	82,987
Coastal flood	33,000
Flash flood	24,000

Source: EM-DAT, The International Disaster Database (2023). Technological (non-natural) disaster categories are omitted.

Table A3: Summary Statistics by Cyclone Exposure (2020)

	(1) Unaffected	(2) First Cyclone only	(3) Both Cyclones
Panel A. Household head characteristics			
Age	37.38	39.69	38.52
	[14.82]	[14.77]	[15.01]
=1 if married	0.66	0.69	0.67
	[0.47]	[0.46]	[0.47]
=1 if completed primary education	0.31	0.28	0.25
	[0.46]	[0.45]	[0.43]
=1 if completed secondary education	0.16	0.15	0.16
	[0.37]	[0.36]	[0.36]
=1 if rural	0.48	0.56	0.55
	[0.50]	[0.50]	[0.50]
Panel B. Economic welfare			
Value of staple crop harvest ('000 MGA)	1780.86	1905.61	1758.11
	[586.69]	[488.60]	[601.79]
Food expenditures ('000 MGA)	20.76	21.99	17.04
	[29.31]	[31.03]	[26.17]
Non-food expenditures ('000 MGA)	22.13	16.86	15.64
	[66.65]	[55.03]	[74.08]
Panel C. Food security			
Food Consumption Score	32.80	32.10	29.58
	[13.69]	[15.02]	[13.36]
Food Insecurity Experience Scale	5.88	6.39	6.82
	[1.53]	[1.49]	[1.27]
Food Security Index	-0.20	-0.49	-0.80
	[0.98]	[1.05]	[0.89]
Number of observations	6,404	3,750	3,716

Note: Descriptive statistics for the pre-treatment year 2020. Means are reported with standard deviations in brackets. Column 1: unaffected districts; Column 2: districts struck by the first cyclone only; Column 3: districts struck by both cyclones. The recall period for food and non-food expenditures is the past 7 days. Continuous monetary variables are winsorized at the 1st and 99th percentiles. Agricultural harvest value covers all seasons in the previous calendar year (typically two harvests).

Table A4: Summary Statistics by Cyclone Exposure (2022)

	(1) Unaffected	(2) First Cyclone only	(3) Both Cyclones
Panel A. Household head characteristics			
Age	38.16	40.81	39.60
	[14.77]	[14.77]	[14.90]
=1 if married	0.67	0.69	0.68
	[0.47]	[0.46]	[0.47]
=1 if completed primary education	0.30	0.28	0.27
	[0.46]	[0.45]	[0.44]
=1 if completed secondary education	0.16	0.15	0.15
	[0.37]	[0.36]	[0.36]
=1 if rural	0.48	0.58	0.55
	[0.50]	[0.49]	[0.50]
Panel B. Economic welfare			
Value of staple crop harvest ('000 MGA)	1790.84	1910.24	1846.25
	[574.67]	[479.73]	[535.07]
Food expenditures ('000 MGA)	56.93	59.00	46.65
	[63.69]	[68.95]	[60.06]
Non-food expenditures ('000 MGA)	220.55	171.15	138.23
	[312.13]	[297.06]	[252.31]
Panel C. Food security			
Food Consumption Score	43.93	42.79	39.28
	[15.35]	[17.32]	[15.74]
Food Insecurity Experience Scale	6.35	6.78	7.16
	[1.16]	[1.14]	[1.09]
Food Security Index	-0.06	-0.32	-0.64
	[0.82]	[0.90]	[0.85]
Number of observations	5,468	3,195	3,164

Note: Descriptive statistics for the pre-treatment year 2022 (fielded in November 2021, before the February 2022 cyclones). Means are reported with standard deviations in brackets. Column 1: unaffected districts; Column 2: districts struck by the first cyclone only; Column 3: districts struck by both cyclones. The recall period for food and non-food expenditures is the past 7 days. Continuous monetary variables are winsorized at the 1st and 99th percentiles. Agricultural harvest value covers all seasons in the previous calendar year (typically two harvests).

Table A5: Summary Statistics by Cyclone Exposure (2023)

	(1) Unaffected	(2) First Cyclone only	(3) Both Cyclones
Panel A. Household head characteristics			
Age	38.42	41.08	40.04
	[14.85]	[14.86]	[15.12]
=1 if married	0.66	0.70	0.66
	[0.47]	[0.46]	[0.47]
=1 if completed primary education	.	.	.
	[.]	[.]	[.]
=1 if completed secondary education	.	.	.
	[.]	[.]	[.]
=1 if rural	0.47	0.56	0.55
	[0.50]	[0.50]	[0.50]
Panel B. Economic welfare			
Value of staple crop harvest ('000 MGA)	1780.48	1907.95	1748.15
	[583.83]	[486.68]	[605.60]
Food expenditures ('000 MGA)	29.40	31.15	9.53
	[39.09]	[42.24]	[25.89]
Non-food expenditures ('000 MGA)	47.81	37.70	33.07
	[102.36]	[94.34]	[107.59]
Panel C. Food security			
Food Consumption Score	36.80	31.35	23.34
	[14.63]	[15.81]	[12.55]
Food Insecurity Experience Scale	6.02	7.04	7.54
	[1.32]	[1.09]	[0.87]
Food Security Index	-0.13	-0.85	-1.40
	[0.89]	[0.90]	[0.74]
Number of observations	5,417	3,180	3,159

Note: Descriptive statistics for the post-treatment year 2023 (fielded in November–December 2022, after the February 2022 cyclones and before Cyclone Freddy's February 2023 landfall). Means are reported with standard deviations in brackets. Educational attainment is not reliably recorded in the 2023 round and is treated as missing; education enters the regressions for this round only through the missing-value indicators. Column 1: unaffected districts; Column 2: districts struck by the first cyclone only; Column 3: districts struck by both cyclones. The recall period for food and non-food expenditures is the past 7 days. Continuous monetary variables are winsorized at the 1st and 99th percentiles. Agricultural harvest value covers all seasons in the previous calendar year (typically two harvests).

Table A6: Cyclone Exposure and Pre-Existing Staple Food Prices (Log)

	(1)	(2)	(3)
	Log price of the following:		
	Rice	Maize	Cassava
First Cyclone Only	-0.04	0.16	-0.05
	(0.24)	(0.20)	(0.33)
Both Cyclones	-0.03	-0.04	-0.27
	(0.25)	(0.22)	(0.30)
p-value: equality	0.943	0.572	0.263
Control mean	6.66	6.50	5.50
Control SD	0.47	0.47	1.04
R-squared	0.316	0.772	0.659
Observations	108	80	86
Region FE	Yes	Yes	Yes
Unit of obs.	District (cross-section)		

Note: Balance check. Staple food prices are district-level unit values constructed from household crop sales (Section 3.3) and do not vary over time; each regression is a cross-sectional OLS at the district level of the log price on exposure indicators with region fixed effects. Null coefficients indicate that cyclone exposure is uncorrelated with pre-existing local price environments. Price data are available for 108 (rice), 80 (maize), and 86 (cassava) of the 112 districts. The p-value tests equality of the two exposure coefficients. Control mean and SD are for unaffected districts. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A7: Cyclone Exposure and Pre-Existing Staple Food Prices (Levels, '000 MGA)

	(1)	(2)	(3)
	Price of the following ('000 MGA):		
	Rice	Maize	Cassava
First Cyclone Only	-0.06	0.33	0.03
	(0.27)	(0.34)	(0.06)
Both Cyclones	-0.04	-0.29	-0.09
	(0.27)	(0.34)	(0.08)
p-value: equality	0.929	0.328	0.050
Control mean	0.88	0.73	0.38
Control SD	0.49	0.33	0.32
R-squared	0.396	0.829	0.612
Observations	108	80	86
Region FE	Yes	Yes	Yes
Unit of obs.	District (cross-section)		

Note: Balance check. Staple food prices are district-level unit values constructed from household crop sales (Section 3.3) and do not vary over time; each regression is a cross-sectional OLS at the district level of the price in '000 MGA on exposure indicators with region fixed effects. Price data are available for 108 (rice), 80 (maize), and 86 (cassava) of the 112 districts. The p-value tests equality of the two exposure coefficients. Control mean and SD are for unaffected districts. Robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A8: Rice Production Outcomes

	(1) Cob	(2) Grain	(3) Shelled	(4) Unshelled
Panel A: Unit medium bag				
First Cyclone Only × Post	-0.06	-0.01	0.03	0.31
	(0.06)	(0.13)	(0.04)	(0.34)
Both Cyclones × Post	-0.21**	-0.12	-0.19	0.09
	(0.08)	(0.20)	(0.16)	(0.32)
p-value: equality	0.066	0.552	0.199	0.603
Control mean	0.42	0.68	0.06	1.59
Control SD	3.64	4.41	1.15	7.53
R-squared	0.150	0.035	0.088	0.114
Observations	8,267	8,267	8,267	8,267
Panel B: Unit large bag				
First Cyclone Only × Post	-0.07	-0.13	-0.05**	-0.29
	(0.06)	(0.22)	(0.03)	(0.23)
Both Cyclones × Post	-0.28*	-0.77***	-0.06	-0.45*
	(0.14)	(0.26)	(0.05)	(0.24)
p-value: equality	0.143	0.017	0.907	0.555
Control mean	0.23	3.37	0.20	2.11
Control SD	2.23	11.36	2.17	8.05
R-squared	0.067	0.071	0.033	0.104
Observations	8,267	8,267	8,267	8,267
District Fixed Effect	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Regression estimates of equation (1). Outcomes are quantities of rice produced in the previous agricultural season, disaggregated by commodity form (cob, grain, shelled, unshelled) and reported for two unit sizes: medium bag (Panel A) and large bag (Panel B). All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The p-value tests equality of the two interaction coefficients. Control mean and SD are for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A9: Cassava Production Outcomes

	(1) Cob	(2) Grain	(3) Shelled	(4) Unshelled
Panel A: Unit small bag				
First Cyclone Only × Post	-0.08	-0.24	0.01	0.74
	(0.05)	(0.22)	(0.03)	(0.57)
Both Cyclones × Post	-0.29	-0.20	-0.02	0.61
	(0.25)	(0.20)	(0.03)	(0.54)
p-value: equality	0.432	0.281	0.355	0.212
Control mean	0.00	0.33	0.00	0.67
Control SD	0.00	1.98	0.00	4.42
R-squared	0.304	0.217	0.088	0.389
Observations	852	852	852	852
Panel B: Unit large bag				
First Cyclone Only × Post	-0.26	0.48	-0.13	-0.04
	(0.27)	(0.36)	(0.12)	(0.05)
Both Cyclones × Post	-0.38	0.12	-0.01	-0.06
	(0.28)	(0.51)	(0.04)	(0.05)
p-value: equality	0.230	0.350	0.283	0.814
Control mean	0.30	0.21	0.00	0.05
Control SD	1.98	0.89	0.00	0.30
R-squared	0.216	0.231	0.115	0.369
Observations	852	852	852	852
District Fixed Effect	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Regression estimates of equation (1). Outcomes are quantities of cassava produced in the previous agricultural season, disaggregated by commodity form (cob, grain, shelled, unshelled) and reported for two unit sizes: small bag (Panel A) and large bag (Panel B). All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The p-value tests equality of the two interaction coefficients. Control mean and SD are for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A10: Maize Production Outcomes

	(1) Cob	(2) Grain	(3) Shelled	(4) Unshelled
Panel A: Unit small bag				
First Cyclone Only × Post	-0.10**	-0.21	0.10	0.02
	(0.04)	(0.26)	(0.13)	(0.04)
Both Cyclones × Post	-0.10	-0.29	0.33	-0.33*
	(0.08)	(0.28)	(0.24)	(0.17)
p-value: equality	0.997	0.829	0.270	0.035
Control mean	0.03	1.41	0.14	0.00
Control SD	0.19	7.06	0.74	0.00
R-squared	0.497	0.332	0.187	0.317
Observations	412	412	412	412
Panel B: Unit large bag				
First Cyclone Only × Post	0.15	3.31	0.20	-0.65**
	(0.28)	(2.86)	(0.25)	(0.32)
Both Cyclones × Post	-0.22	2.62	0.24	-0.66**
	(0.20)	(2.28)	(0.25)	(0.32)
p-value: equality	0.247	0.343	0.794	0.951
Control mean	0.00	3.62	0.90	2.10
Control SD	0.00	15.97	4.83	6.41
R-squared	0.594	0.296	0.429	0.773
Observations	412	412	412	412
District Fixed Effect	Yes	Yes	Yes	Yes
Year Fixed Effect	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Note: Regression estimates of equation (1). Outcomes are quantities of maize produced in the previous agricultural season, disaggregated by commodity form (cob, grain, shelled, unshelled) and reported for two unit sizes: small bag (Panel A) and large bag (Panel B). All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The p-value tests equality of the two interaction coefficients. Control mean and SD are for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A11: Event-Study Estimates: Food Security Outcomes

	(1) FCS	(2) FIES	(3) Food Security Index
First Cyclone Only × 2020	0.36	0.09	-0.04
	(0.40)	(0.07)	(0.04)
First Cyclone Only × 2021	-0.06	0.11	-0.06
	(0.31)	(0.07)	(0.04)
First Cyclone Only × 2023	-4.58***	0.62***	-0.48***
	(0.73)	(0.08)	(0.06)
Both Cyclones × 2020	1.45***	0.13	-0.02
	(0.32)	(0.09)	(0.05)
Both Cyclones × 2021	0.84**	0.14	-0.05
	(0.34)	(0.09)	(0.05)
Both Cyclones × 2023	-9.16***	0.74***	-0.70***
	(0.89)	(0.08)	(0.05)
p: leads = 0 (First only)	0.417	0.303	0.227
p: leads = 0 (Both)	0.000	0.229	0.190
p: all leads = 0	0.000	0.429	0.307
Control mean (2021)	38.08	5.85	0.00
R-squared	0.237	0.220	0.279
Observations	49,695	49,695	49,695

Note: Event-study estimates. Outcomes are regressed on interactions of exposure group with survey-year indicators, omitting 2022 (the last pre-cyclone round) as the base period. All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The joint p-values test the joint significance of the 2020 and 2021 lead interactions. Control mean is for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A12: Event-Study Estimates: Mechanism Outcomes

	(1) Staple harvest ('000 MGA)	(2) Food exp. ('000 MGA)	(3) Non-food exp. ('000 MGA)
First Cyclone Only × 2020	6.92	-1.09	43.26**
	(10.02)	(2.86)	(16.80)
First Cyclone Only × 2021	-0.11	-0.78	29.29***
	(8.20)	(1.38)	(9.65)
First Cyclone Only × 2023	2.39	-1.17	36.84**
	(13.26)	(2.08)	(14.39)
Both Cyclones × 2020	-70.42***	6.85***	76.29***
	(18.67)	(2.22)	(12.24)
Both Cyclones × 2021	-6.92	3.49***	46.40***
	(8.01)	(1.27)	(7.23)
Both Cyclones × 2023	-88.10***	-10.19***	65.48***
	(18.80)	(2.10)	(11.00)
p: leads = 0 (First only)	0.760	0.844	0.012
p: leads = 0 (Both)	0.001	0.009	0.000
p: all leads = 0	0.004	0.006	0.000
Control mean (2021)	1,791.72	41.03	108.47
R-squared	0.189	0.219	0.163
Observations	49,695	49,679	49,695

Note: Event-study estimates as in Table A11. Outcomes are the value of the staple crop harvest and weekly food and non-food expenditures, in '000 MGA. Harvest values refer to the previous calendar year, so wave-to-wave comparability is more fragile for these outcomes than for the food security measures; see Section 4.3 for discussion, including the stability of the 2023 estimates to using 2021 as the base period. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A13: Randomization Inference: Permutation p-Values

	(1)	(2)	(3)	(4)
	First Cyclone Only × Post		Both Cyclones × Post	
	Estimate	Permutation p	Estimate	Permutation p
FCS	-4.69	0.000	-9.96	0.000
FIES	0.55	0.000	0.64	0.000
Food Security Index	-0.45	0.000	-0.68	0.000
Permutation draws		1000		

Note: Randomization inference for the estimates in Table 2. The exposure map is re-randomized across the 112 districts 1,000 times, preserving the observed group sizes (47 unaffected, 28 first cyclone only, 37 both cyclones) and the nesting of the twice-struck corridor within the once-struck corridor, and equation (1) is re-estimated on each draw. The permutation p-value is the share of placebo draws whose coefficient is at least as large in absolute value as the observed estimate; a value of 0.000 indicates that no placebo draw exceeded the observed estimate ($p < 1/1{,}000$).

Table A14: Cyclone Impacts on Food Security: Excluding the Grand Sud

	(1)	(2)	(3)
	FCS	FIES	Food Security Index
First Cyclone Only × Post	-3.55***	0.50***	-0.38***
	(0.76)	(0.07)	(0.06)
Both Cyclones × Post	-8.98***	0.68***	-0.67***
	(1.05)	(0.08)	(0.07)
p-value: equality	0.000	0.029	0.000
Control mean	38.08	5.85	0.00
Control SD	14.68	1.54	1.00
R-squared	0.201	0.160	0.202
Observations	43,706	43,706	43,706

Note: Regression estimates of equation (1) excluding the three Grand Sud regions (Androy, Anosy, and Atsimo-Andrefana), which experienced severe drought conditions during the pre-treatment period. All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The p-value tests equality of the two interaction coefficients. Control mean and SD are for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A15: Cyclone Impacts on Food Security: Controlling for Precipitation

	(1) FCS	(2) FIES	(3) Food Security Index
Panel A. Precipitation as a control			
First Cyclone Only × Post	-4.22***	0.52***	-0.42***
	(0.83)	(0.07)	(0.06)
Both Cyclones × Post	-9.15***	0.60***	-0.63***
	(0.95)	(0.08)	(0.06)
Precipitation (z-score)	-1.66***	0.09*	-0.10***
	(0.35)	(0.05)	(0.03)
p-value: equality	0.000	0.283	0.002
R-squared	0.237	0.220	0.280
Panel B. Fully interacted			
First Cyclone Only × Post	-4.35***	0.53***	-0.43***
	(0.75)	(0.07)	(0.06)
Both Cyclones × Post	-9.10***	0.61***	-0.64***
	(0.96)	(0.08)	(0.06)
Precipitation (z-score)	-1.47***	0.07	-0.09**
	(0.47)	(0.05)	(0.04)
First Cyclone Only × Post × Precip.	2.21**	-0.04	0.10
	(1.10)	(0.09)	(0.08)
Both Cyclones × Post × Precip.	0.73	0.07	-0.01
	(1.09)	(0.08)	(0.07)
p-value: equality	0.000	0.248	0.001
p-value: joint interactions	0.110	0.203	0.151
R-squared	0.238	0.221	0.280
Control mean	38.08	5.85	0.00
Control SD	14.68	1.54	1.00
Observations	49,695	49,695	49,695

Note: Regression estimates of equation (1) augmented with district-level annual precipitation from CHIRPS (Funk et al. 2015). The precipitation z-score standardizes each district's annual total using the pooled mean and standard deviation across the estimation sample (1,421 mm and 617 mm). Panel A adds the z-score as a control. Panel B interacts it fully with the exposure and Post indicators, reporting the exposure effects evaluated at average rainfall and the triple interactions; lower-order exposure × precipitation and Post × precipitation terms are included but not reported, and the joint p-value tests the two triple-interaction coefficients. All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The equality p-value tests equality of the two exposure interaction coefficients. Control mean and SD are for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A16: Cyclone Impacts on Agricultural Harvest: Controlling for Precipitation

	(1) Staple crops (total)	(2) Rice	(3) Maize	(4) Cassava
Panel A. Precipitation as a control				
First Cyclone Only × Post	0.60	0.52	0.97	-0.89
	(9.59)	(8.85)	(0.77)	(1.11)
Both Cyclones × Post	-58.93***	-49.71***	-1.84**	-7.39***
	(13.16)	(12.59)	(0.75)	(1.54)
Precipitation (z-score)	-2.47	-5.06	0.30	2.28***
	(6.93)	(6.73)	(0.38)	(0.68)
p-value: equality	0.000	0.000	0.003	0.000
R-squared	0.188	0.182	0.036	0.049
Panel B. Fully interacted				
First Cyclone Only × Post	-0.74	-1.45	0.86	-0.15
	(10.19)	(9.49)	(0.88)	(1.08)
Both Cyclones × Post	-53.61***	-43.19***	-2.01**	-8.41***
	(14.41)	(13.75)	(0.82)	(1.75)
Precipitation (z-score)	0.39	-1.51	0.27	1.64***
	(6.98)	(6.82)	(0.36)	(0.51)
First Cyclone Only × Post × Precip.	0.60	-0.35	-0.43	1.38
	(12.10)	(11.42)	(1.32)	(1.05)
Both Cyclones × Post × Precip.	-12.73	-15.07	1.07*	1.27
	(15.18)	(14.66)	(0.63)	(1.08)
p-value: equality	0.001	0.006	0.006	0.000
p-value: joint interactions	0.553	0.445	0.196	0.297
R-squared	0.188	0.182	0.036	0.049
Control mean	1,791.72	1,153.22	341.75	296.75
Control SD	577.48	579.39	30.18	29.99
Observations	49,695	49,695	49,695	49,695

Note: Regression estimates of equation (1) augmented with district-level annual precipitation from CHIRPS (Funk et al. 2015); outcomes are the value of the staple crop harvest in the previous calendar year ('000 MGA), in total and by crop. The precipitation z-score standardizes each district's annual total using the pooled mean and standard deviation across the estimation sample (1,421 mm and 617 mm). Panel A adds the z-score as a control. Panel B interacts it fully with the exposure and Post indicators, reporting the exposure effects evaluated at average rainfall and the triple interactions; lower-order exposure × precipitation and Post × precipitation terms are included but not reported, and the joint p-value tests the two triple-interaction coefficients. All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The equality p-value tests equality of the two exposure interaction coefficients. Control mean and SD are for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A17: Cyclone Impacts on Household Expenditures: Poisson Estimates

	(1) Food	(2) Non-food	(3) Total
First Cyclone Only × Post	0.006	-0.008	0.030
	(0.026)	(0.053)	(0.038)
Both Cyclones × Post	-0.948***	0.048	-0.253***
	(0.048)	(0.059)	(0.050)
p-value: equality	0.000	0.394	0.000
Control mean ('000 MGA)	41.03	108.47	150.48
Pseudo R-squared	0.306	0.360	0.381
Observations	49,679	49,695	49,679

Note: Poisson pseudo-maximum-likelihood (PPML) estimates of equation (1), which retain zero outcomes and measure proportional effects: a coefficient b implies a proportional change of $\exp(b) - 1$. Outcomes are weekly household food, non-food, and total expenditures. All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The p-value tests equality of the two interaction coefficients. Control means (levels, '000 MGA) are for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A18: Cyclone Impacts on Agricultural Harvest: Poisson Estimates

	(1) Staple crops (total)	(2) Rice	(3) Maize	(4) Cassava
First Cyclone Only × Post	-0.000	-0.001	0.003	-0.001
	(0.005)	(0.007)	(0.002)	(0.003)
Both Cyclones × Post	-0.034***	-0.045***	-0.005**	-0.022***
	(0.007)	(0.011)	(0.002)	(0.005)
p-value: equality	0.000	0.000	0.003	0.000
Control mean ('000 MGA)	1,791.72	1,153.22	341.75	296.75
Pseudo R-squared	0.149	0.114	0.011	0.018
Observations	49,695	49,695	49,695	49,695

Note: Poisson pseudo-maximum-likelihood (PPML) estimates of equation (1), which retain zero outcomes and measure proportional effects: a coefficient b implies a proportional change of $\exp(b) - 1$. Outcomes are the value of the staple crop harvest in the previous calendar year, in total and by crop. All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The p-value tests equality of the two interaction coefficients. Control means (levels, '000 MGA) are for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A19: Extensive Margin: Any Spending or Harvest

	(1) Any food spending	(2) Any non-food spending	(3) Any staple harvest
First Cyclone Only × Post	0.00	0.00	
	(0.01)	(0.00)	
Both Cyclones × Post	-0.51***	-0.01**	
	(0.02)	(0.01)	
p-value: equality	0.000	0.001	.
Control mean	1.00	0.99	1.00
Control SD	0.04	0.09	0.00
R-squared	0.467	0.106	.
Observations	49,679	49,695	49,695

Note: Linear probability estimates of equation (1). Outcomes are indicators for any positive weekly food spending, any positive weekly non-food spending, and any positive staple harvest value. Column 3 is blank because essentially all sample households report a positive staple harvest value, leaving no variation in the indicator. All regressions include district and year fixed effects and household-head controls (age, marital status, completed primary education, completed secondary education, and rural residence, each with an indicator for missing values). The p-value tests equality of the two interaction coefficients. Control mean and SD are for unaffected districts in 2021. Standard errors clustered at the district level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.