

Coral bleaching reduces child growth through nutritional pathways*

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Abstract

Climate change damages ecosystems that people rely on for food and income. Coral reefs are especially vulnerable to bleaching and support fisheries that millions depend on, yet how bleaching affects human nutrition is poorly understood. Using a novel satellite-derived, machine-learning bleaching dataset and long-term sea surface temperature as an instrument, I show that climate-driven coral bleaching in East Africa reduces fish catch and cuts household fish consumption, while children exposed to bleaching in utero or during their first two years are measurably shorter for their age, a marker of early childhood malnutrition. Households, however, do not seem to be able or willing to adapt to these pressures. These results show every link in the chain from a climate-driven ecosystem shock to child health, showing that climate-driven coral bleaching is a considerable threat to the nutrition and long-term wellbeing of coastal communities.

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17 Introduction

18 An important avenue for climate change impacts on human nutrition and health are the
19 irreversible damages climate change already causes to natural systems (IPCC, 2022). Beyond
20 the well-studied mechanism through agriculture (Mendelsohn, 2009), indirect impacts via
21 ecosystems could undermine the global food system. Coral reefs are a prime example to
22 study this relationship as they are among the ecosystems suffering the largest early-onset
23 damages from climate change due to coral bleaching. This phenomenon driven by heat stress
24 can damage or kill corals and subsequently lead to the erosion of entire reefs. Even if the
25 global community manages to limit global warming to the Paris Agreement targets, most
26 coral reefs will likely die as a consequence of this heat stress. Hoegh-Guldberg et al. (2019)
27 estimate that 70 to 90% of warm-water corals might be lost under 1.5°C of warming, with
28 temperatures beyond 2°C driving the loss of 99%. At the same time, coral reefs are essential
29 for fisheries that millions of people depend on (FAO et al., 2023), particularly in developing
30 countries in East Africa and across other tropical regions (Burke et al., 2011).

31 Since fish depend on healthy coral and structurally complex reefs (González-Rivero et
32 al., 2017; Rogers et al., 2018; Graham and Nash, 2012), coral bleaching could significantly
33 reduce fish abundance and thereby catch. This, however, depends on local conditions. Even
34 though some studies, Graham et al. (2007) and Grandcourt and Cesar (2003) for instance,
35 find no decrease in biomass after a bleaching event, Pratchett et al. (2011) document that
36 in 60% of the studied cases in their meta-analysis, coral bleaching reduced the abundance of
37 fish. Even in cases where fish abundance does not decrease, species composition might shift
38 and make fisheries unstable (Robinson et al., 2019). East Africa is particularly exposed to
39 such a risk, due to the important role of artisanal fisheries in the region. In Tanzania, for
40 example, artisanal fisheries relying on coral reefs represent around 70% of fisheries in coastal
41 communities (Mbije and Rinkevich, 2013). Research also indicates the relevance of reefs for
42 the other countries in my study (Schleyer et al., 1999; Samoilys et al., 2017; Jadot et al.,
43 2015).

There is little quantitative literature analyzing coral bleaching impacts on human well-being and nutrition, with existing studies focusing mostly on qualitative approaches or tourism, e.g. Spalding et al. (2017); Speers et al. (2016); Chen et al. (2015). The exception are two studies on large-scale bleaching in Indonesia in 1998 by Chaijaroen (2019, 2021), which find adverse effects on income, labor outcomes, and nutrition but are limited by a small sample size. Also related is the work by Temsah et al. (2018), who focus on the role of marine food sources for child nutrition in Hispaniola and underscore its importance, while not finding an effect of reef degradation.

If bleaching leads to impacts on fisheries, a potential consequence could be protein malnutrition. A good way to detect such malnutrition is by focusing on the growth patterns of children, as children tend to be short for their age, or stunted, if they have access to too little protein during the first two years of their life (Alderman and Headey, 2018). This is especially relevant, as related research, such as McGovern et al. (2017), has shown that stunting can negatively affect outcomes later in life. Analyzing stunting therefore helps to understand whether potential reductions in protein consumption lead to impacts strong enough to affect people in the long term.

Beyond direct impacts on household consumption, coral bleaching may lead to adaptive responses, which determine how coral bleaching ultimately affects populations and who is most vulnerable. Related literature on adaptation to shocks to fishery and agriculture provides some context, Cinner et al. (2009) for instance report that fishers in Kenya are more likely to adapt to decreasing catch rates by shifting away from fishing if they had more wealth or alternative income sources. While there is a rich literature on the adaptation of fishers using case studies or stated-preference surveys (see e.g. Salgueiro-Otero et al., 2022; Schwarz et al., 2011; Cinner et al., 2015; Green et al., 2021; Pike et al., 2022; Freduah et al., 2018; Cinner et al., 2012; Bartelet et al., 2023), there is a need for more research on real-world adaptation (Cinner et al., 2015). This is also relevant since some adaptation strategies, such as dis-saving if one's assets are largely productive, which is often the case in developing

71 countries (Rosenzweig and Wolpin, 1993), can have negative long-term effects on income
72 and lead to poverty traps.

73 Using machine learning, I construct a novel large-scale coral bleaching dataset based
74 on satellite imagery, which addresses the issue of endogenous selection of survey sites by
75 researchers, which can bias results (Methods). This paper is among the first to predict coral
76 bleaching at this spatial and temporal scale based on satellite imagery alone.

77 The paper most related to the prediction approach is the recent work by Gokul et al.
78 (2025), which predicts bleaching for the Red Sea. My approach differs in two respects. First,
79 methodologically: their method fits a least-squares model to bottom reflectance derived from
80 a single satellite image at a time, with four images used for training, whereas I learn bleaching
81 patterns from multi-month sequences of satellite reflectance using a recurrent neural network,
82 allowing the model to draw on temporal information. Second, in scale: my primary test set
83 and external holdout are close to an order of magnitude larger than the 34 site-level sets
84 used to validate Gokul et al. (2025), and are complemented by a independent, out-of-sample
85 bleaching survey from Kenya. Despite this substantially larger and more heterogeneous
86 evaluation setting, my model performs comparably well: on the held-out test set, it reliably
87 distinguishes bleached from non-bleached sites (PR-AUC 0.810, F1 0.731; n=376), with even
88 stronger performance on external reef cells with no overlap with the training data (PR-AUC
89 0.900, F1 0.704; n=58), indicating that the model generalizes to new locations rather than
90 memorizing patterns from its training sites.

91 This combination of scale and generalizability makes the dataset usable for causal in-
92 ference: it provides bleaching measures across space and time that are dense and consistent
93 enough to exploit as a source of identifying variation, rather than being confined to the
94 sparse, unevenly sampled locations covered by field surveys. Relying on this dataset and by
95 exploiting temporal and spatial variation of coral bleaching across East Africa, I show that
96 climate-driven coral bleaching causally reduces fishery outcomes, household consumption of
97 fish and, consequently, hurts child growth. At the same time, households do not seem to

100 adapt, which could hurt them in the long run. These findings rely on an empirical strategy
101 that instruments bleaching with lagged heat stress (Degree Heating Weeks, DHW) to back
102 out the climate-related variation in bleaching, while I also control for a range of weather
103 variables to ensure that effects do not materialize through other channels, such as impacts
104 on agriculture.

103 Results

104 The analysis relies on three different data sets, namely a panel of fishery landings in southern
105 Kenya, survey data on household consumption in neighboring Tanzania, as well as health
106 data on height and weight of children covering both these countries as well as Madagascar
107 and Mozambique. Despite the separation between the sections of my analysis, the countries
108 studied are sufficiently similar in terms of the economic and natural conditions along their
109 coastlines such that the findings of each section are likely to hold across all four countries.
110 For the analysis of child growth, I only consider children within 100 km of the coast; for the
111 household analysis, treated households lie within 100 km of the coast, while the comparison
112 group comprises all other panel households.

113 Coral bleaching reduces fish catch

114 Being affected by coral bleaching in the same month, instrumented with the DHW over the
115 year prior to observation, reduces fish catch per unit effort (fisher $\times$ day) by about 0.41 kg
116 per standard deviation in bleaching intensity (95% CI -0.74 to -0.01) as reported in Figure
117 1. Overall catch per trip is reduced by 0.74 kg (95% CI -1.38 to -0.15). Confidence intervals
118 in the text are based on the wild cluster bootstrap, which accounts for the correlation of
119 trips within the 19 landing sites; with heteroskedasticity-robust standard errors, the effect
120 on catch per unit effort is significant at the 5% level and on catch per trip at the 10% level.

As the average catch per fisher-day is 1.6 kg and per trip 2.5 kg, this effect is considerable and roughly equivalent to 26 and 30% of the mean, respectively.

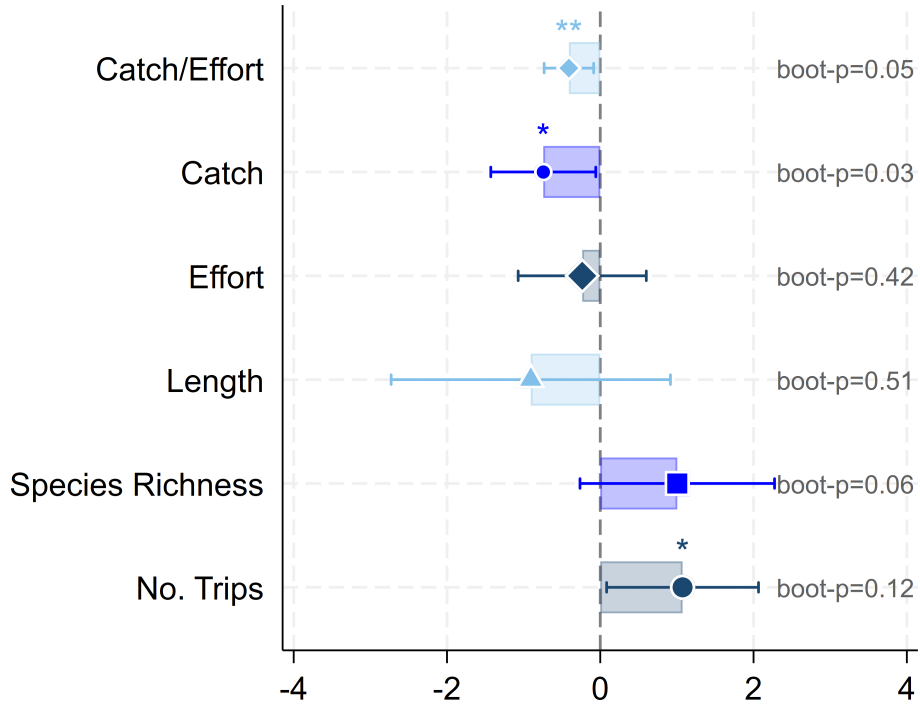
The effort on the intensive margin, meaning fisher-days per trip, is not significantly affected, suggesting that boats do not stay out longer to catch fish. However, when analyzing trips on a site-month level, the number of trips on the extensive margin seems to increase by about one trip per site and month (1.07 trips), even though the effect is not significant when considering bootstrapped standard errors and suffers from a weak first stage. This could imply that because each trip results in lower catch, fishers might be forced to venture out to sea more often. Furthermore, the species richness of the catch tends to increase (1.01 species), which could suggest that fishers have to seek out species they usually would not catch.

These results show that affected fishers do not switch away from fishing but instead seemingly increase effort. This is, however, insufficient to overcome the reduction in catch. The length of fish is not affected in a significant way (Figure 1). If long-term temperature changes were driving the effect, length would likely be affected (van Rijn et al., 2017; Johansen et al., 2024). Therefore, this also suggests that I do not pick up a spurious temperature effect.

Bleaching threshold and mechanism

Focusing on climate-induced bleaching specifically opens the relevant additional question at which temperature stress threshold, measured in Degree Heating Weeks (DHW, Methods), one can observe impacts on bleaching. Results shown in Figure 2a indicate a non-linear temperature–bleaching relationship. The regression relates predicted bleaching (in standard deviations) to the number of months, out of the current and the 12 preceding months, in which DHW fell into each 4-DHW bin, relative to months with DHW below 4. Each additional month with DHW between 4 and under 8 raises predicted bleaching by 0.42 standard deviations (95% CI 0.08 to 0.76). The effect increases to 0.50 for 8–12 DHW (95% CI 0.18 to 0.82) and peaks at 0.72 for 12–16 DHW (95% CI 0.44 to 1.00), before declining

Figure 1: Regressions of Fishery Outcomes on Bleaching



Estimated coefficients from separate instrumental variable regressions of instrumented bleaching on each outcome (N=593 fishing trips at 19 landing sites for catch/effort measures; N=101 site-month observations for number of trips). Bars show 90% confidence intervals based on heteroskedasticity-robust standard errors; all tests are two-sided. Regressions control for wind speed, air temperature, precipitation, sea surface temperature (mean, squared, minimum and maximum), the number of reef pixels within 10 km, and include year, month and site fixed effects. Boot p reports wild cluster bootstrap p-values following Roodman et al. (2019), clustering on site (Rademacher weights, 999 replications, null imposed). Two-sided P values (wild cluster bootstrap; heteroskedasticity-robust): catch per unit effort $P = 0.046$; $P = 0.037$; catch $P = 0.028$; $P = 0.074$; effort $P = 0.418$; $P = 0.643$; length $P = 0.51$; $P = 0.413$; species richness $P = 0.057$; $P = 0.193$; number of trips $P = 0.12$; $P = 0.075$. Full results in Table A.1.

to 0.30 for 16–20 DHW (95% CI 0.06 to 0.55; Table A.2a). This is broadly in line with Humanes et al. (2022) and Razak et al. (2026). Above 20 DHW, additional months have no significant effect (20–24 DHW: -0.12 ; 24 DHW and above: 0.24). This might be due to bleaching not being registered anymore after a reef has been eroded or might be driven by the low number of observations.

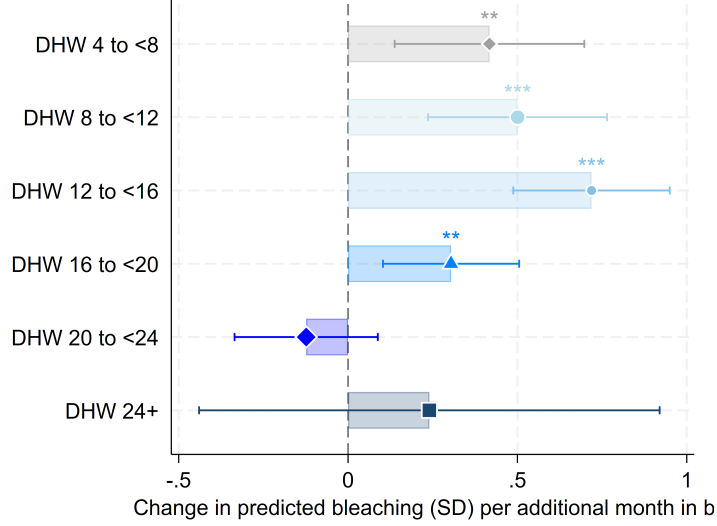
Analyzing the impact of temperature stress on catch directly in areas with no/low and high bleaching allows one to determine the validity of the methodological approach using temperature-related bleaching specifically. Analyses depicted in Figure 2b show that temperature stress does not significantly affect catch in areas with no or low bleaching – the estimator is even positive (5.30) – while there is a strong and significant relationship with catch in areas with high bleaching (current DHW: -2.72 kg per standard deviation of DHW, 95% CI -4.35 to -1.09). This suggests that, controlling for sea surface temperatures and other weather conditions, the relationship between temperature stress and fish catch goes through bleaching, at least in the context studied here, and supports the methodological approach in this paper.

Coral bleaching reduces household fish consumption

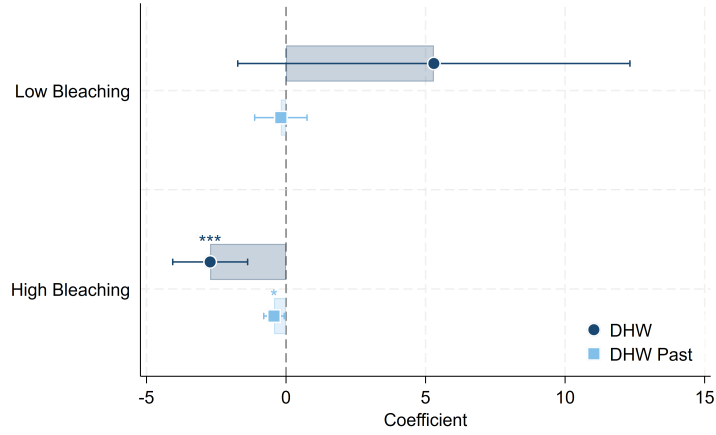
The effect on fishing identified for Kenya is mirrored by an effect on fish consumption by households in Tanzania. Separating bleaching intensity into low and high bleaching at the median, being affected by coral bleaching reduces the amount of fresh fish consumed by 0.24 log points (approximately 21%; 95% CI -0.44 to -0.04) for the high-intensity group and by 0.16 log points (approximately 15%; 95% CI -0.34 to 0.02) for the low-intensity group, compared to non-affected households and the fourth survey round (see Figure 3 a); the former estimate is significant at the 5% level, the latter at the 10% level. Pooling both intensity groups implies a reduction of 0.18 log points (approximately 17%). For reference, households

Figure 2: Regressions on the Relationship of DHW, Bleaching and Catch

(a) Regressions of Bleaching on DHW



(b) Regressions of Catch on DHW by Bleaching



DHW is short for Degree Heating Weeks, a measure of how many weeks temperatures over the past 3 months were above the bleaching threshold, multiplied with the temperature difference (N=593 fishing trips for (a); N=283 for (b) low bleaching and N=308 for (b) high bleaching; two singleton observations are dropped in the low-bleaching sample). In (a), regressors are the number of months (current and 12 lags) with DHW in each bin; coefficients are in standard deviations of predicted bleaching, relative to months with DHW below 4. The instrument explains catch in areas with high bleaching, but not in areas with low bleaching, indicating the IV strategy works as intended; high bleaching is a predicted bleaching intensity above the median. Bars show 90% confidence intervals (two-sided); standard errors clustered on site level (15 sites in the low-bleaching and 17 sites in the high-bleaching sample). All regressions control for wind speed, air temperature, precipitation, sea surface temperature (mean, squared, minimum and maximum) and the number of reef pixels, and include year, month and site fixed effects. Two-sided P values in (a): 4 to <8 DHW $P = 0.019$; 8 to <12 DHW $P = 0.004$; 12 to <16 DHW $P < 0.001$; 16 to <20 DHW $P = 0.017$; 20 to <24 DHW $P = 0.32$; 24 DHW and above $P = 0.55$. In (b), current DHW: high bleaching $P = 0.003$; low bleaching $P = 0.206$. Full results in Table A.2.

171 consume on average 0.77 kg of fresh fish over 7 days (SD 1.34; Table A.6).¹ These results
 172 indicate that bleaching affects household diets and that the effect can be of meaningful
 173 size. Overall consumption of protein (including fish, beef, goat/sheep and chicken) points in
 174 the same direction (see Figure 3 b): it falls by 0.18 log points in the high-intensity group
 175 (approximately 16%; 95% CI -0.42 to 0.06, $P = 0.15$) and by 0.11 log points in the low-
 176 intensity group (approximately 10%; 95% CI -0.33 to 0.11), although these estimates are
 177 not statistically significant. Coastal households thus do not appear to fully replace fish with
 178 other sources of animal protein, but this evidence is less precise. This is in line with the
 179 significant role of fish in the diet of coastal households in Tanzania, as well as throughout
 180 the region. Pre-treatment coefficients are small and insignificant for both outcomes, and the
 181 low- and high-bleaching groups do not differ in the pre-treatment waves (Figure 3; Table
 182 A.4).

183 At the same time, alternative food sources such as staples (rice, maize) or dried fish are
 184 not affected by bleaching (see Figure 3 c and d). This finding is exactly what one would
 185 expect if the empirical strategy worked as intended and suggests that my results are not
 186 driven by broader agricultural effects caused by an insufficient control of weather influences.
 187 The results for staples must, however, be seen cautiously due to potential differences in
 188 pre-bleaching trends: the two treatment groups do not differ before treatment, but the
 189 wave-3 coefficient for low bleaching is positive and marginally significant (0.29 log points,
 190 approximately 34%).

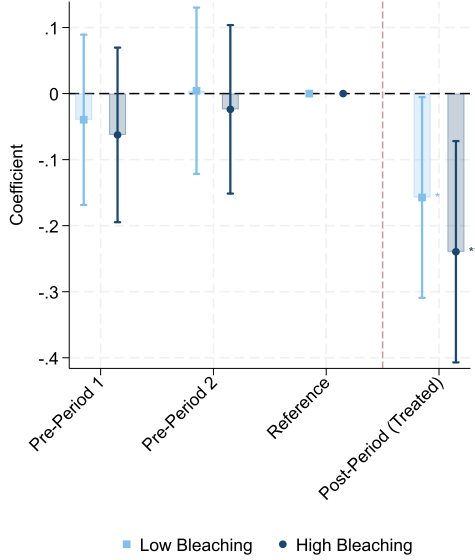
191 **Households do not seem to adapt**

192 As a reaction to the pressure from bleaching, households could either reduce assets or adapt,
 193 for instance by working more outside of the household, which could hurt long-run income as
 194 Fink et al. (2020) show, or by migrating, as e.g. discussed by Ojea et al. (2020). Studying

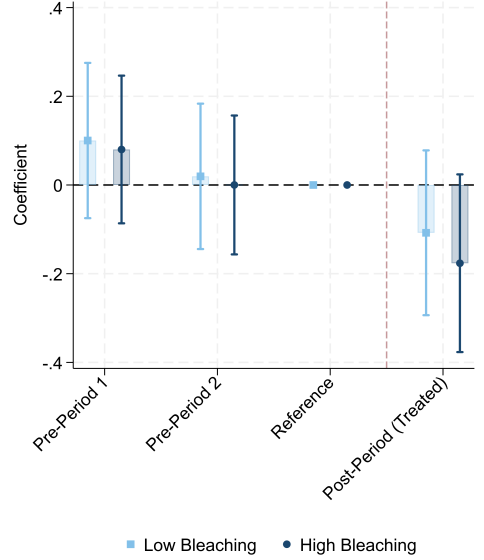
¹Approximate percentage changes are computed as $100 \times (\exp(\hat{\beta}) - 1)$; because outcomes are transformed as $\log(y + 1)$, they are approximate.

Figure 3: Regression Results for Consumption

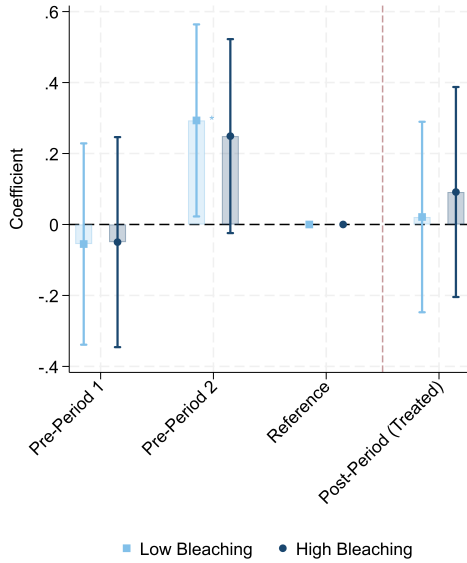
(a) Fresh Fish Consumed (n=1610)



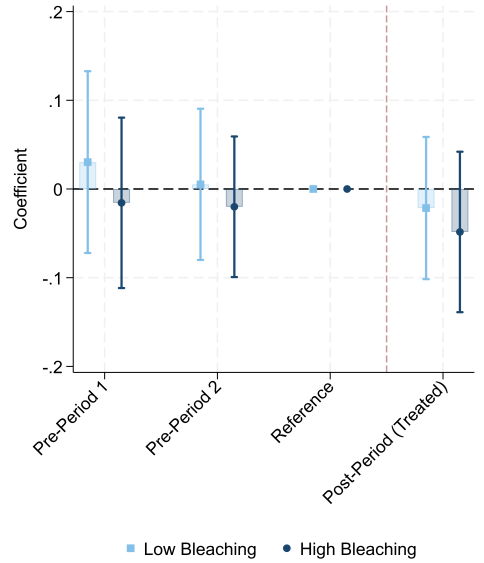
(b) Overall Protein Cons. (n=1610)



(c) Overall Staple Cons. (n=1610)



(d) Dried Fish Consumed (n=1610)



$n = 1,610$ household-wave observations from 446 households in 66 districts (control: 67 households; low bleaching: 187; high bleaching: 192); the same households are observed repeatedly across waves 2–5. Regression coefficients for wave $\times$ instrumented coral bleaching dummies; reference are untreated control units and wave 4. Treatment happens between wave 4 (2014/15) and wave 5 (2019/20 NPS-SDD follow-up; Methods) (red line). Low and High Bleaching are treatment intensity bins split at the median. Outcomes are $\log(y + 1)$. Bars show 90% confidence intervals (two-sided) based on bootstrapped standard errors (1,000 household-level resamples); stars from bootstrap P values. Regressions control for household size and a vector of annual climate controls (see Table A.4), and include survey round and household fixed effects. Two-sided bootstrap P values for wave 5: fresh fish $P = 0.089$ (low) and $P = 0.019$ (high); overall protein $P = 0.34$ (low) and $P = 0.15$ (high); staples and dried fish P between 0.38 and 0.90. Pooling both intensity groups, the fresh fish estimate has a district-clustered $P \approx 0.06$ (Table A.4, column 3). Full results in Table A.4.

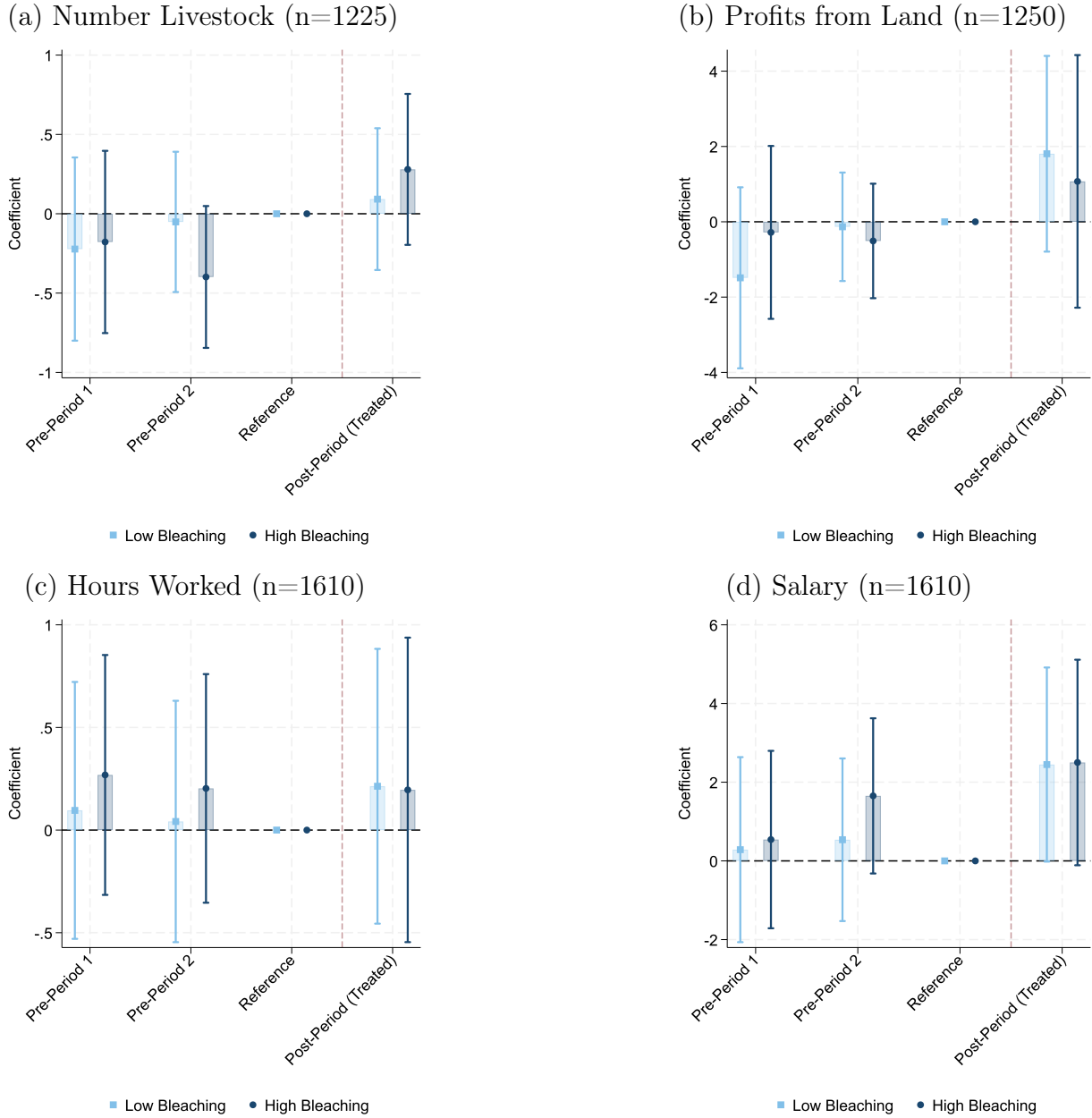
adaptation impacts reveals that households do not adapt to the pressure from coral bleaching. Neither the number of livestock, profits from land, overall working hours or salaries obtained from work outside the house (see Figure 4; no significant pre-treatment differences), nor migration (logit coefficient -0.85 ; with weather controls 1.05) are significantly affected by bleaching (see Table A.12), suggesting that households do not seek or are unable to obtain increased income to buffer the reduction in fish availability or increase in fish price.

Coral bleaching increases child stunting

Results for stunting as a long-term indicator for malnutrition based on data for all four countries in the region (Madagascar, Mozambique, Kenya and Tanzania) show that each one-standard-deviation increase in monthly bleaching exposure during the year before and the two years after birth decreases the log-transformed height-for-age z-score of affected children by 1.8% (coefficient -0.018 , 95% CI -0.029 to -0.008 ; Table A.3, column 2), as reported in Figure 5. Because exposure accumulates over the 36 months, effects add up over time: a one-standard-deviation increase in total exposure within the same DHS cluster corresponds to a decrease of 11%. The first stage is moderately strong, and the effect remains significant under weak-instrument-robust inference (Anderson–Rubin test; Methods). This confirms that coral bleaching does lead to persistent malnutrition symptoms in children exposed to coral bleaching in utero or during their first two years of life and implies that the effects on fish consumption are strong enough to negatively impact long-term outcomes. Controlling for wealth, which is, however, not fully exogenous, lowers this result (-0.013 , 95% CI -0.022 to -0.004), while separate regressions for children in DHS clusters with an average wealth quintile above 2.7 (-0.020 , 95% CI -0.035 to -0.004) or below 2.7 (-0.026 , 95% CI -0.045 to -0.007) yield similar, significant estimates, indicating no clear heterogeneity by wealth.

Results are robust to how incomplete exposure windows are handled. Controlling for the number of observed months in each child’s 36-month window leaves the estimate unchanged (-0.018 , 95% CI -0.028 to -0.008). Restricting the sample to children older than

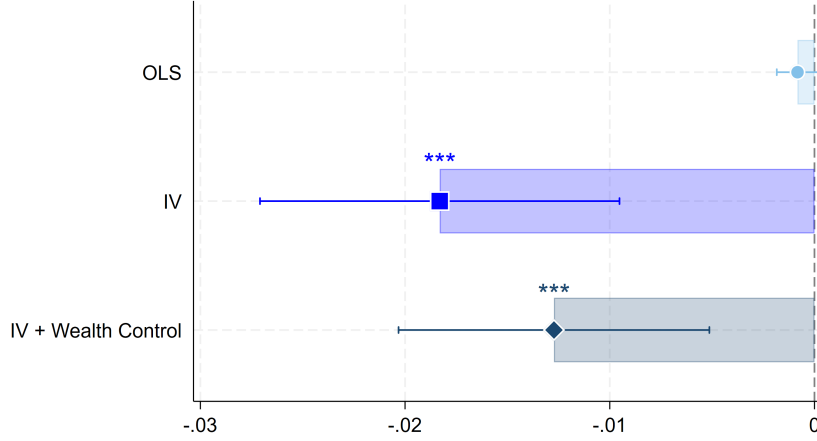
Figure 4: Regression Results for Adaptation



N denotes household-wave observations (households: 443 for (a), 437 for (b), 446 for (c) and (d)). Regression coefficients for wave $\times$ instrumented coral bleaching dummies; reference are untreated control units and wave 4. Treatment happens between wave 4 (2014/15) and wave 5 (the 2019/20 NPS-SDD follow-up of the Extended Panel; see Methods) (red line). Low and High Bleaching are treatment intensity bins split at the median. Outcomes are $\log(y + 1)$. Bars show 90% confidence intervals (two-sided) based on bootstrapped standard errors (1,000 household-level resamples, re-estimating the first stage in each); stars from bootstrap P values. Regressions control for household size and a vector of annual climate controls (see Table A.5), and include survey round and household fixed effects. Two-sided bootstrap P values for wave 5 are between 0.10 and 0.73 for all four outcomes. Full results in Table A.5.

221 24 months at interview, whose entire exposure window precedes measurement, yields a some-
 222 what smaller but still negative and significant estimate (-0.013 , 95% CI -0.021 to -0.004),
 223 and the main estimate lies within its confidence interval (Table A.7).

Figure 5: Regression of Height-for-Age Z-Scores on Bleaching



Estimated coefficients from separate regressions of the log-transformed height-for-age z-score on bleaching exposure ($N = 7,781$ children in 599 DHS clusters; Kenya $N = 1,807$, Madagascar $N = 3,124$, Mozambique $N = 1,651$, Tanzania $N = 1,199$). Bleaching exposure is the sum of standardized monthly predicted bleaching within 10 km over the 12 months before and the 24 months after birth; coefficients are per one-standard-deviation increase in a single month. Bars show 90% confidence intervals (two-sided); standard errors clustered on a 0.01-degree (≈ 1 km) grid of DHS cluster locations (968 clusters). IV regressions instrument bleaching exposure with summed degree heating weeks over the 12 months before and the 24 months after birth (first-stage Kleibergen–Paap $F = 11.3$; 10.9 with wealth control). Regressions control for reef pixels and for 12-month averages of wind speed, precipitation, mean air temperature and sea surface temperature (mean, squared, minimum and maximum) for the year before birth and the first and second years after birth, and include birth-year, birth-month and DHS-cluster fixed effects. The wealth control enters as DHS wealth-quintile dummies. Two-sided P values: IV $P = 0.001$; IV with wealth control $P = 0.006$. Separate IV regressions by cluster wealth (Table A.3, columns 6 and 8; not shown): clusters with an average wealth quintile above 2.7, $n = 4,043$ children in 337 DHS clusters, $P = 0.012$, first-stage $F = 7.9$; below 2.7, $n = 3,738$ children in 262 DHS clusters, $P = 0.009$, first-stage $F = 5.2$. Robustness checks (Table A.7): controlling for the number of observed months, $P < 0.001$, first-stage $F = 12.6$; children older than 24 months, $P = 0.003$, $n = 4,195$, first-stage $F = 8.5$. Full results in Table A.3.

224 Discussion

225 These results show that climate-driven coral bleaching impacts can be traced along all links
 226 of the suggested chain. Coral bleaching reduces fishery yields and fish consumption, and
 227 consequently impacts child growth in a way that suggests protein malnutrition. Despite this,
 228 no adaptation behavior can be observed, potentially due to an inability to adapt to these

climate impacts. The study furthermore shows that an identification of coral bleaching on a large scale based on publicly available satellite data is feasible, which could be used for further work.

The estimates from this study enable an illustrative calculation of the economic magnitude of the effects. While price information for Kenya is not available, one can assume that catch would be similarly affected in Tanzania. In the latest national panel survey wave, the average price of 1kg of seafood there was approximately 2.3 US\$, while Churu (2023) suggests that fishers receive around half when selling the fish. Assuming that the 200,000 full-time small scale fishers in Tanzania (United Republic of Tanzania, 2021) work for 260 days a year, the effect of a reduction of ca. 400 g per fisher per day would result in an overall loss of approximately 24 million US\$ - or 48 million US\$ considering market prices. This equals approximately 0.07% of the Tanzanian GDP (Trading Economics, 2025). Even though this overall effect seems relatively small, my results on consumption indicate that it affects the diets of people in coastal areas.

This study has several limitations. Some estimates may represent a lower bound because bleaching in areas with lower coral cover or at higher depth might not be accurately detected, similar to Xu et al. (2021); Gokul et al. (2025), and because migration cannot be accounted for in the DHS survey data, which may exclude the most severely affected children from the stunting analysis. The satellite record's limited temporal coverage also prevents an assessment of how bleaching impacts might compound over time or interact with a reef's prior bleaching history, even though the fact that effects for consumption only materialize after my predicted bleaching suggests that this does not bias my results. Finally, the three parts of the analysis draw on different data sources and regional contexts, so the results, although likely generalizable given similar underlying ecological and economic conditions, are not directly comparable with one another.

The findings highlight that climate change leads to significant detrimental impacts on fisheries through coral bleaching, thereby endangering food security and health of coastal

households in East Africa and likely throughout tropical regions in the Global South. These findings support calls for more stringent climate policies and must be considered when designing national adaptation plans, calculating overall climate change damages and should be addressed through humanitarian interventions in cases of severe bleaching episodes in the future to avoid malnutrition and related long-term impacts on people affected by coral bleaching.

Online Methods

Coral bleaching data. The underlying bleaching data was created by using a machine learning approach to identify patterns in the brightness of reef areas on satellite imagery, following earlier smaller-scale approaches (Campbell and Trent, 2022; Pink et al., 2022; Konda et al., 2021; Xu et al., 2021). Training data stem from the van Woesik and Kratochwill (2022) database of coral bleaching observations, supplemented with bleaching data from the Great Barrier Reef recorded through aerial surveys. While aerial survey data for 2016 and 2017 are published as part of Hughes et al. (2019), data from subsequent years (2020, 2022, 2024) were sourced from maps published through the Reef Authority Reef Snapshot Reports, its Reef Health Updates website and third-party reports (GBRMPA, 2024, 2025; Skirving, 2020). Using both sources has the advantage of combining wide geographical coverage with the quasi-panel structure of the Australian data. Existing bleaching records are binarized, considering everything above "low bleaching" or 10% bleaching intensity as bleached. This helps in making reports comparable and reduces noise. For training and prediction, a monthly 1 km $\times$ 1 km grid cell panel of brightness values based on satellite images from ESA (2024) is created for all reef/algae pixels following Allen Coral Atlas (2022) contained in the training data and between 27° S to 1° S and 32° E to 55° E. As non-reef sections (rock, rubble, sand) are expected not to change due to a potential bleaching event, they are used to normalize the brightness value of the reef section following a pseudo invariant

features approach similar to Xu et al. (2021). This corrects for changes in overall brightness of the individual pixel. These normalized values are supplemented with additional satellite data from NASA (2025). If data are missing for a single month, the values are imputed linearly, otherwise they are treated as missing. The resulting feature set is used to train a deep recurrent neural network with three stacked Long Short-Term Memory (LSTM) layers at its core (Hochreiter and Schmidhuber, 1997). LSTM models are particularly well suited for time-series predictions, as they consider temporal dependencies in the data (van Houdt et al., 2020). This is especially relevant in detecting coral bleaching, as it is the change in coral brightness over time that is indicative of a coral bleaching event. To capture the relevant temporal dependencies, the model considers 3 leads and 3 lags of each main feature generated from satellite imagery, corresponding to a 7-month window centered on each survey. This approach is comparable to, and in several respects outperforms, similar approaches in the literature, as detailed in Section A.2.4. When tested on held-out reef cells not used at any stage of model development, it achieves an F1 score of 73.1% (PR-AUC 0.810); on an external holdout of previously unused survey sites, F1 reaches 70.4–93.3% depending on region and overlap with training locations. F1, the harmonic mean of precision and recall, indicates that the model balances avoiding false positives (precision) and avoiding false negatives (recall). Further architectural and validation details are provided in the Supplementary Information.

Extended Data Figure A.1 shows the development of mean bleaching for both north-eastern Tanzania and Pemba Island in that region as an example. The results show that there is both small-scale regional and over-time variation in predicted bleaching, in line with research that indicates bleaching exposure can vary between different sites within the same reef system depending on local conditions (Cantin and Lough, 2014; Emslie et al., 2025; Schoepf et al., 2020).

Fishery data (Kenya). For the analysis on catch, the paper uses data from Kenya collected by the Wildlife Conservation Society (WCS), I use data covering 2016-21. This data

was published through Galligan and McClanahan (2024) and was shared by the authors. It covers 1091 trips across various landing sites in southern Kenya. Of these, 593 trips at 19 landing sites enter the catch regressions; trips are dropped if they precede 2016 (485 trips) or miss any relevant variables, e.g. due to a lack of satellite coverage (13 trips).

Household consumption and adaptation data (Tanzania). Consumption and adaptation patterns are studied using the representative National Panel Survey (NPS) in Tanzania (World Bank, 2021; National Bureau of Statistics, Tanzania, 2019), implemented by the National Bureau of Statistics. The original NPS sample of about 3,200 households across Tanzania and Zanzibar was interviewed in 2008/09, 2010/11 and 2012/13. In 2014/15, a nationally representative subsample of these households continued as the “Extended Panel”, while a newly drawn “Refresh Panel” was added. The Extended Panel was re-interviewed in 2019/20 in the NPS with Sex-Disaggregated Data (NPS-SDD) (World Bank, 2021), the Refresh Panel in 2020/21. I use waves 2 (2010/11) to 4 (2014/15) and the 2019/20 NPS-SDD follow-up, referred to as wave 5 throughout, and only rely on households observed in both wave 2 and wave 5. This restricts the sample to Extended Panel households and ensures that the same households are followed over the full period. The resulting sample comprises 446 households in 66 districts (control: 67 households; low bleaching: 187; high bleaching: 192), contributing 1,610 household-wave observations; the same households are measured repeatedly across waves.

Child anthropometric data. Data on anthropometric indicators used to study malnutrition impacts are obtained from the Demographic and Health Surveys (DHS) Program, which has been implementing nationally representative surveys on health across the world for more than 30 years. The DHS surveys are comparable across countries due to the program’s focus on standardized questionnaires (Footman et al., 2015) and are of high quality, as research has confirmed for anthropometric indicators (Harkare et al., 2021). For this study, the latest round of surveys for Kenya (DHS, 2023a), Madagascar (DHS, 2022), Mozambique (DHS,

2024) and Tanzania (DHS, 2023b) are used. The sample comprises 7,781 children in 599 DHS clusters; Kenya $N = 1,807$, Madagascar $N = 3,124$, Mozambique $N = 1,651$, Tanzania $N = 1,199$. Each child is measured once, so the anthropometric data are cross-sectional.

Weather and climate controls. The analysis controls for a range of land and sea weather variables at the treatment unit level. These include mainly sea surface temperature, wind speed, air temperature, and precipitation. The control set is used consistently for all parts of the analysis, with only the consumption analysis using an extended control set (including pressure and evaporation), as indicated in the table. Data for controls and instrument creation are sourced from Muñoz Sabater (2019); Saha et al. (2016) and Casey et al. (2010) using Google Earth Engine. I use the data for controls within a ten kilometer radius around each household, landing site or closest coastal point. To allocate predicted coral bleaching, as well as degree heating weeks and sea surface temperatures, household district, with centroids from Runfola et al. (2020), or cluster locations are mapped onto their closest point on the shore. Bleaching exposure and sea weather, as well as sea surface temperatures, are aggregated within a 10 km radius around this point and allocated to households. For the analysis of fish catch, landing locations are used directly. The 10 km distance was chosen, as artisanal fishers in the region travel mainly short distances of less than 5 km, in some cases up to almost 10 km (Daw et al., 2011).

Empirical strategy. The analysis is implemented using 2SLS instrumental variable regression. This addresses both potential measurement error in the coral bleaching data (Hausman, 2001) and the potential endogeneity of bleaching to economic conditions on land, since bleaching and the vulnerability of coral can also depend on stressors caused by economic activity, such as water pollution from agriculture. As bleaching is mostly caused by long-term exposure to elevated sea surface temperatures (Humanes et al., 2022), I use heat stress in the year prior to each observation as an instrument, specifically Degree Heating Weeks (DHW, see Supplementary Information for details), which are a measure of long-term heat

stress commonly used to determine coral bleaching risk. What is important to consider here is that reefs might recover from bleaching within a few months (Schoepf et al., 2020), which my data does not reflect. This, however, would imply that my estimates are a lower bound, with high-mortality cases likely leading to stronger impacts.

Separating bleaching from direct temperature effects on fish. A crucial aspect of the identification strategy is how direct temperature effects on fish can be separated from bleaching effects. This is relevant, as the literature shows that high temperatures can lead to distributional shifts of fish species and to changes in catch composition (Espasandín et al., 2025; Lam et al., 2016). These effects, however, materialize gradually and over the long run. Ranges for example shift with a speed of upward of 200km per decade in some cases (Pinsky et al., 2021), which should matter less for year to year changes, in particular because my sites are located well within the tropics. While there can also be medium-run effects of heat on fish abundance (Chaikin et al., 2026), such effects have not been documented for reef fish to the best of my knowledge.

For the case of coral reef fish, research has shown that fish can temporarily relocate to deeper waters to avoid heat (Nay et al., 2015). This leads to a decrease in observed biomass, and likely also catchability, during a heat wave, while biomass quickly recovers afterwards (Magel et al., 2020). This might be one of the reasons why some reef fishes have been shown to be adaptable to heat, even though their performance might be affected (Van Wert et al., 2024). Another relevant short-term aspect is that fishers fish less during warm temperatures (Fiorella et al., 2021).

Corals specifically suffer from an uninterrupted buildup of heat stress over the medium run as measured by DHW, while temperature variability shields them from bleaching (Safaie et al., 2018). This is important because bleaching leads to a separate effect on fish: Stuart-Smith et al. (2018) show for the case of the Great Barrier Reef that reefs suffering from substantial coral-cover loss see a decrease in coral-eating fishes (corallivores) as well as fish

species richness, while such effects do not materialize on reefs exposed to similar temperatures without coral loss - even though they do observe ecological changes due to temperature on the whole sample.

This means that for a direct temperature effect on reef fish, long-term changes and short-term fluctuations matter most, while the effect via bleaching materializes in the medium run. I address this econometrically by using annual fixed effects and controlling for current mean, mean squared, minimum and maximum sea surface temperatures in the month of each observation to account for the direct temperature effect on fish, while I instrument bleaching with degree heating weeks. My instrument therefore captures the difference between the medium-run, bleaching-specific heat stress and both the long-term changes and short-term fluctuations that matter for fish, allowing me to isolate the exogenous, climate-driven part of bleaching. That this approach works is suggested by my results in Figure 2, which shows that DHW only significantly affects catch in regions with above median bleaching, subject to my controls.

Estimation by outcome. The analysis is split in three parts, which differ in the data used and therefore rely on slightly different identification strategies.

For the analysis on catch, I regress fishery outcomes in levels on instrumented bleaching, as I am interested in the absolute impact on catch and consider bleaching on the intensive margin, meaning the share of reef pixels in a 10km radius around each landing site that is bleached. Conventional standard errors are heteroskedasticity-robust. Because trips from the same landing site are likely correlated and the number of sites is small (19), conventional cluster-robust standard errors might over-reject, which is why I also report wild cluster bootstrap p -values (Cameron et al., 2008; MacKinnon and Webb, 2018; Roodman et al., 2019). The wild cluster bootstrap p values (Roodman et al., 2019) are clustered by landing site, with the null imposed and symmetric two-sided P values, using Rademacher weights and 999 replications. Conventional t statistics use the small-sample correction with $N - K$

degrees of freedom. Because the main first stage uses 13 excluded instruments (current DHW and 12 lags) with only 19 clusters, results with fewer instruments are reported in Table A.11; all point estimates remain negative.

To study the impact of bleaching on household consumption, I assign data to households based on interview month and exploit within-household variation by using an instrumented treatment intensity difference-in-differences approach. The measure of bleaching is the aggregated number of reef-months with predicted bleaching within a 10 km radius over the year before the implementation of the survey, as I expect that bleaching events in the preceding season are the relevant shock. Correspondingly, all controls are considered for the two years before the implementation of the survey, and current as well as two years of lagged degree heating weeks are used as instruments. The latter are aggregated into a current and past DHW to avoid risks of overfitting and instrument proliferation. As treatment intensity is only available for wave 5, the first-stage regression is estimated separately, limited to wave 5, with intensity for all non-bleached cases set to 0. To account for the generated regressor, the first stage, the predicted intensity and its median split are re-estimated in each of 1,000 bootstrap samples drawn with replacement at the household level; standard errors are the standard deviation of the bootstrap estimates, and P values use the normal approximation. In a second step, bleaching intensity between waves 4 and 5 is split into two treatment intensity bins at the median, which are assigned across all waves for each household. Because households in the same district and interview month receive identical first-stage predictions, 36 households have a predicted intensity exactly equal to the median; they are assigned to the low-intensity group. This approach follows Callaway et al. (2024), facilitates interpretation and enables a comparison of pre-treatment outcomes between the different groups. I additionally control for the number of household members as well as household and wave fixed effects. Outcomes are log transformed as $\log(y + 1)$, since many households report zero consumption, as I assume that bleaching has a relative impact on consumption and adaptation behavior.

In the last part of the analysis, potential impacts on stunting are studied using a pseudo panel of anthropometric indicators based on children’s birth dates. This allows a reconstruction of early childhood nutrition by measuring a child’s height years later. I estimate the regressions using both an ordinary least squares (OLS) as well as an instrumental variable approach instrumenting coral bleaching with degree heating weeks. First and second stage are estimated jointly by two-stage least squares (`ivreghdfe`), so the reported standard errors account for the estimated first stage. Coral bleaching exposure covers the time between 12 months before and up to 24 months after birth. The instruments are the summed DHW over the 12 months before and the 24 months after birth (two excluded instruments, one endogenous regressor). Regressions include birth-year, birth-month and DHS-cluster fixed effects, so identification comes from differences in exposure between birth cohorts within the same DHS cluster. Controls are aggregated into 12-month averages covering the same period (reef pixels; mean 10 km wind speed, precipitation and mean 2 m air temperature; mean, maximum, minimum and squared mean SST), entered separately for the year before birth and the first and second years after birth. Specifications adding household wealth are sensitivity checks only, as wealth may itself respond to bleaching. The wealth control enters as DHS wealth-quintile dummies.

Even though the employed empirical strategy differs between the three sections of the analysis, the underlying logic remains the same and enables the isolation of climate-related bleaching as well as a correction for potential measurement error. Further details on data assignment and the empirical strategy can be found in the Supplementary Information.

Statistics. All hypothesis tests are two-sided. In the text, point estimates are reported with 95% confidence intervals; exact P values, test statistics and sample sizes are given in the figure legends, tables and Supplementary Information. Figures show 90% confidence intervals.

Units and repeated measurements. The fishery analysis uses repeated trip-level observations from the same 19 landing sites over 2016–2021 ($N = 593$ trips; $N = 101$ site-months for the number of trips). The consumption and adaptation analysis uses repeated measurements of the same households across survey waves 2–5 (2010/11 to the 2019/20 NPS-SDD follow-up; $N = 1,610$ household-wave observations from 446 households). The anthropometric analysis uses a single cross-sectional measurement per child ($N = 7,781$ children in 599 DHS clusters). The ML evaluation uses scored 7-month windows; overlapping windows from the same reef cell are not independent, which is why cell counts are reported alongside window counts.

Inference. Fishery regressions use heteroskedasticity-robust standard errors with a small-sample correction. Because trips are clustered in 19 landing sites, the main inference is the wild restricted efficient cluster bootstrap by site (Roodman et al., 2019), with Rademacher weights, 999 replications and the null imposed. Consumption and adaptation regressions use a pairs bootstrap with 1,000 replications that resamples households and re-estimates the first stage and the treatment-intensity split in each replication (Cameron et al., 2008), which accounts for the generated regressor; conventional district-clustered standard errors are reported alongside. Anthropometric regressions use standard errors clustered on a 0.01-degree (≈ 1 km) grid of DHS cluster coordinates (968 clusters); results are robust to clustering by DHS cluster (599 clusters; $P = 0.004$) and on a 0.1-degree (≈ 10 km) grid ($P = 0.004$), and weaken on a 1-degree (≈ 100 km) grid (113 clusters; $P = 0.085$), which also serves as a check for spatial correlation.

Instrument diagnostics. Instrument strength is assessed with the Kleibergen–Paap rk Wald F statistic or, for the separately estimated consumption first stage, the conventional first-stage F statistic. Overidentifying restrictions are tested with Hansen’s J test ($df = 12$ for fisheries, $df = 1$ for anthropometrics); no overidentification test is reported for the consumption and adaptation analysis, where the first stage is estimated separately and the treatment enters as binned, generated regressors. Because first stages in the anthropometric

analysis are moderate (Kleibergen–Paap $F \approx 11$), I additionally report weak-instrument-robust Anderson–Rubin tests (Anderson and Rubin, 1949), which reject the null of no effect for the main specification ($P < 0.001$) and with wealth controls ($P = 0.004$).

Pre-trends. For the difference-in-differences analysis, pre-treatment coefficients are tested individually using the bootstrap, and jointly for equality across the two treatment-intensity groups with a Wald test on the district-clustered variance matrix (Tables A.4 and A.5).

Effect sizes. In the fishery analysis, bleaching is standardized, so coefficients are expressed in kg per standard deviation of bleaching and additionally as a percentage of the outcome mean. In the anthropometric analysis, exposure is the sum of 36 standardized monthly bleaching measures, so coefficients are per one-standard-deviation increase in bleaching in a single month; cumulative effects are reported per within-cluster standard deviation of total exposure (6.56). Consumption and adaptation effects are reported in log points, with approximate percentage changes computed as $100 \times (\exp(\hat{\beta}) - 1)$; because outcomes are transformed as $\log(y + 1)$, these percentages are approximate.

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A Appendix

A.1 Extended Data - Tables

Table A.1: Regressions of Fishery Outcomes on Bleaching

	(1) Catch/Effort	(2) Catch	(3) Effort	(4) Length	(5) Species Richness	(6) No. Trips
Bleaching	-0.409** (0.20)	-0.744* (0.42)	-0.235 (0.51)	-0.907 (1.11)	1.005 (0.77)	1.074* (0.59)
N	593	593	593	593	593	101
R^2	0.073	0.079	0.050	0.053	0.117	0.803
Adj. R^2	0.003	0.009	-0.022	-0.019	0.050	0.674
First-Stage F	197.33	197.33	197.33	197.33	197.33	5.88
Hansen J p	0.13	0.10	0.36	0.02	<0.01	0.54
Boot p	0.05	0.03	0.42	0.51	0.06	0.12

Heteroskedasticity-robust standard errors (small-sample corrected) in parentheses. P-values are calculated based on two-tailed tests for all tables: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Bleaching is standardized; coefficients are per standard deviation. Columns (1)–(5) are estimated at the trip level, column (6) at the site-month level. Instruments are current DHW and its 12 monthly lags. All regressions control for the number of reef pixels within 10 km, mean wind speed, mean 2 m air temperature, precipitation, and mean, squared mean, minimum and maximum sea surface temperature, and include site fixed effects and year and month dummies. First-Stage F reports robust Kleibergen-Paap rk Wald F-Statistics for the first stage. Hansen J p reports the p -value of Hansen’s J test of overidentifying restrictions (12 degrees of freedom); with 13 instruments and 19 landing sites, the test is of limited reliability. Boot p reports wild cluster bootstrap p -values following Roodman et al. (2019), clustering on site (19 clusters; Rademacher weights, 999 replications, null imposed).

Table A.2: Regressions on Bleaching Threshold and Mechanism

(a) Regression of Bleaching on Long-Term Heat Stress (Degree Heating Weeks, DHW) Bins			(b) Regression of Catch on DHW in Areas with Low and High Bleaching		
	(1) Predicted Bleaching			(1) Low Bleaching	(2) High Bleaching
Months with DHW 4 to <8	0.418**	(0.16)			
Months with DHW 8 to <12	0.501***	(0.15)	DHW	5.297	-2.719***
Months with DHW 12 to <16	0.719***	(0.13)		(3.99)	(0.77)
Months with DHW 16 to <20	0.304**	(0.12)	DHW Past	-0.187	-0.432*
Months with DHW 20 to <24	-0.124	(0.12)		(0.53)	(0.21)
Months with DHW ≥ 24	0.240	(0.39)			
N	593		N	283	308
Sites (clusters)	19		Sites (clusters)	15	17
R^2	0.945		R^2	0.254	0.181
Adj. R^2	0.940		Adj. R^2	0.141	0.076

DHW is short for Degree Heating Weeks, a measure of accumulated heat stress over the past 12 weeks above the bleaching threshold (Equation A.1). Regressors in Table A.2a are the number of months (current and 12 lags) with DHW in each 4-DHW bin and with DHW of 24 or more; the reference is months with DHW below 4. The results in Table A.2b indicate that the instrument explains catch in areas with high bleaching, but does not in areas with low bleaching, suggesting the IV strategy works as intended. High bleaching implies a predicted bleaching intensity above the median, low bleaching an intensity below the median; two singleton observations are dropped in the low-bleaching sample. In Table A.2b, DHW and DHW Past (the sum of the 12 preceding months) are standardized. Standard errors clustered on site in parentheses (19 sites in Table A.2a; 15 sites for low and 17 sites for high bleaching in Table A.2b). P-values are calculated based on two-tailed tests for all tables: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions control for the number of reef pixels within 10 km, mean wind speed, temperature, precipitation, as well as mean, min, max and squared sea surface temperature. Regressions also include year, month and site fixed effects.

Table A.3: Regression of Height-for-Age Z-Scores on Bleaching

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	Wealth	Wealth IV	Poor	Poor IV	Rich	Rich IV
Bleaching	-0.000827 (0.00062)	-0.0183*** (0.0053)	-0.000477 (0.00059)	-0.0127*** (0.0046)	-0.00207** (0.00099)	-0.0258*** (0.0098)	-0.000704 (0.00077)	-0.0198** (0.0078)
Wealth Control			✓	✓				
N	7,781	7,781	7,781	7,781	3,738	3,738	4,043	4,043
R^2	0.018	-0.154	0.046	-0.038	0.039	-0.189	0.016	-0.225
First-Stage F		11.28		10.92		5.22		7.91
Hansen J p -value		0.10		0.31		0.01		0.42

Standard errors clustered on a 0.01-degree (≈ 1 km) grid of DHS cluster coordinates in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (two-sided). The outcome is the log-transformed height-for-age z-score, rescaled by subtracting its sample minimum and adding one. Columns (3) and (4) add DHS wealth-quintile dummies as controls. Columns (5)–(8) split the sample by whether the average wealth quintile of the DHS cluster is below (Poor) or above (Rich) the sample mean of 2.7. Bleaching exposure is the sum of standardized monthly predicted bleaching within 10 km over the 12 months before and the 24 months after birth. IV regressions instrument bleaching exposure with summed degree heating weeks over the 12 months before and the 24 months after birth (two excluded instruments). All regressions control for the number of reef pixels and for 12-month averages of wind speed, precipitation, mean air temperature and sea surface temperature (mean, squared, minimum and maximum) for the year before birth and the first and second years after birth, and include birth-year, birth-month and DHS-cluster fixed effects. First-Stage F reports robust Kleibergen-Paap rk Wald F-Statistics for the first stage. Hansen J p -value reports the p -value of the Hansen J test for overidentification of all instruments (1 degree of freedom).

Table A.4: Regression of Consumption Outcomes on Bleaching

	Fresh Fish Consumed			Overall Protein Consumption			Overall Staple Consumption			Dried Fish Consumed		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	First	Wave	DiD	First	Wave	DiD	First	Wave	DiD	First	Wave	DiD
DHW Past	0.244*** (0.028)			0.244*** (0.028)			0.244*** (0.028)			0.244*** (0.028)		
DHW Current	0.928*** (0.21)			0.928*** (0.21)			0.928*** (0.21)			0.928*** (0.21)		
Wave 2 × Low Bleaching		-0.0396 (0.078)			0.100 (0.11)			-0.0552 (0.17)			0.0303 (0.062)	
Wave 2 × High Bleaching		-0.0626 (0.080)			0.0801 (0.10)			-0.0498 (0.18)			-0.0156 (0.058)	
Wave 3 × Low Bleaching		0.00429 (0.077)			0.0195 (0.10)			0.293* (0.16)			0.00524 (0.052)	
Wave 3 × High Bleaching		-0.0239 (0.078)			0.0001 (0.095)			0.249 (0.17)			-0.0200 (0.048)	
Wave 5 × Low Bleaching		-0.157* (0.092)			-0.108 (0.11)			0.0210 (0.16)			-0.0215 (0.049)	
Wave 5 × High Bleaching		-0.239** (0.10)			-0.176 (0.12)			0.0915 (0.18)			-0.0484 (0.055)	
Wave 2 × Treatment			-0.0492 (0.066)			0.0916 (0.081)			-0.0542 (0.14)			0.00397 (0.057)
Wave 3 × Treatment			-0.00913 (0.054)			0.0106 (0.075)			0.267 (0.16)			-0.00773 (0.047)
Wave 5 × Treatment			-0.184* (0.095)			-0.129 (0.11)			0.0351 (0.16)			-0.0335 (0.048)
N	511	1,610	1,610	511	1,610	1,610	511	1,610	1,610	511	1,610	1,610
R ²	0.934	0.709	0.708	0.934	0.642	0.641	0.934	0.554	0.553	0.934	0.388	0.388
First-stage F		43.87	43.87		43.87	43.87		43.87	43.87		43.87	43.87
Equal pre-trends <i>p</i> (wave 2/3/joint)		0.78/0.56/0.84			0.82/0.73/0.93			0.97/0.69/0.90			0.19/0.42/0.41	

Column (1) reports the first stage, estimated on wave-5 households, using current DHW and the sum of past DHW as instruments (identical across outcomes). Column (2) reports coefficients for wave × instrumented bleaching-intensity dummies (low vs. high bleaching, split at the median of predicted intensity); standard errors are household-level bootstrap standard errors (1,000 replications, re-estimating the first stage and the median split in each), and stars are based on bootstrap *P* values (normal approximation). Column (3) reports a pooled difference-in-differences specification with district-clustered standard errors. The reference category is untreated control units and wave 4. Outcomes are $\log(y + 1)$. All regressions control for the number of household members and a vector of annual climate controls (u and v components of mean wind speed; minimum and maximum air temperature; pressure; evaporation; precipitation; mean, mean squared, minimum and maximum sea surface temperature), and include survey round and household fixed effects. First-stage F is the conventional Wald F statistic for the two excluded instruments. Equal pre-trends *p* reports Wald tests (district-clustered) of equal low- and high-bleaching coefficients in wave 2, in wave 3, and jointly. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (two-sided).

Table A.5: Regression of Adaptation Outcomes on Bleaching

	Number of Livestock			Plot Profits			Hours Worked (7 Days)			Salary		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
	First	Wave	DiD	First	Wave	DiD	First	Wave	DiD	First	Wave	DiD
DHW Past	0.244*** (0.028)			0.244*** (0.028)			0.244*** (0.028)			0.244*** (0.028)		
DHW Current	0.928*** (0.21)			0.928*** (0.21)			0.928*** (0.21)			0.928*** (0.21)		
Wave 2 $\times$ Low Bleaching		-0.222 (0.35)			-1.488 (1.46)			0.0959 (0.38)			0.286 (1.43)	
Wave 2 $\times$ High Bleaching		-0.178 (0.35)			-0.282 (1.40)			0.269 (0.36)			0.542 (1.37)	
Wave 3 $\times$ Low Bleaching		-0.0513 (0.27)			-0.132 (0.88)			0.0419 (0.36)			0.537 (1.26)	
Wave 3 $\times$ High Bleaching		-0.398 (0.27)			-0.509 (0.92)			0.203 (0.34)			1.652 (1.20)	
Wave 5 $\times$ Low Bleaching		0.0923 (0.27)			1.806 (1.58)			0.214 (0.41)			2.449 (1.50)	
Wave 5 $\times$ High Bleaching		0.280 (0.29)			1.073 (2.04)			0.196 (0.45)			2.501 (1.59)	
Wave 2 $\times$ Treatment			-0.195 (0.25)			-0.719 (0.94)			0.199 (0.28)			0.414 (1.11)
Wave 3 $\times$ Treatment			-0.216 (0.31)			-0.300 (0.62)			0.130 (0.31)			1.153 (1.23)
Wave 5 $\times$ Treatment			0.105 (0.26)			1.745 (1.57)			0.234 (0.43)			2.603 (1.64)
N	511	1,225	1,225	511	1,250	1,250	511	1,610	1,610	511	1,610	1,610
R^2	0.934	0.716	0.714	0.934	0.643	0.643	0.934	0.589	0.588	0.934	0.539	0.538
First-stage F		43.87	43.87		43.87	43.87		43.87	43.87		43.87	43.87
Equal pre-trends p (wave 2/3/joint)		0.88/0.16/0.29			0.48/0.48/0.63			0.49/0.38/0.62			0.78/0.12/0.29	

Column (1) reports the first stage, estimated on wave-5 households, using current DHW and the sum of past DHW as instruments (identical across outcomes). Column (2) reports coefficients for wave $\times$ instrumented bleaching-intensity dummies (low vs. high bleaching, split at the median of predicted intensity); standard errors are household-level bootstrap standard errors (1,000 replications, re-estimating the first stage and the median split in each), and stars are based on bootstrap P values (normal approximation). Column (3) reports a pooled difference-in-differences specification with district-clustered standard errors. The reference category is untreated control units and wave 4. Outcomes are $\log(y+1)$. All regressions control for the number of household members and a vector of annual climate controls (u and v components of mean wind speed; minimum and maximum air temperature; pressure; evaporation; precipitation; mean, mean squared, minimum and maximum sea surface temperature), and include survey round and household fixed effects. First-stage F is the conventional Wald F statistic for the two excluded instruments. Equal pre-trends p reports Wald tests (district-clustered) of equal low- and high-bleaching coefficients in wave 2, in wave 3, and jointly. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (two-sided).

Table A.6: Summary Statistics

Variable	Obs	Mean	SD	Min	Max	Unit
<i>Panel A: Fish Catch (Kenya)</i>						
Catch per Unit Effort	593	1.59	1.651	0.021	20.263	Kg/fisher-day
Total Landings	593	2.49	2.884	0.028	27.300	Kg
Effort	593	2.26	3.678	1	25	Fisher-days
Average Fish Length	593	24.23	10.122	8.346	121	cm
Species Richness	593	3.04	3.130	0.049	35.186	Count
Fishing Trips	593	8.37	5.281	1	27	Count
Predicted Bleaching	593	0.56	0.216	0.097	1	Share
Reef Pixels	593	194.24	115.613	8	507	Count
DHW	593	9.45	5.486	1.128	21.310	°C-weeks
Mean Past DHW	593	10.58	2.672	6.592	15.770	°C-weeks
<i>Panel B: Household Consumption and Adaptation (Tanzania)</i>						
Fresh Fish Consumed	2,529	0.77	1.337	0	7	Kg
Protein Consumption	2,529	1.64	1.785	0	11.3	Kg
Staple Consumption	2,529	9.10	7.321	0	56	Kg
Number of Livestock	1,577	15.08	88.609	0	2,500	Count
Hours Worked (7 Days)	2,529	26.47	42.858	0	228	Hours
Predicted Bleaching	2,529	37.36	81.374	0	241	Sum
Reef Pixels	2,529	7.44	16.044	0	43	Count
DHW	1,827	21.82	13.082	0	58.23	°C-weeks
<i>Panel C: Child Malnutrition (Kenya, Madagascar, Mozambique, Tanzania)</i>						
Child Height-for-Age	30,401	-1.16	1.335	-5.97	5.85	z-score
Child Weight-for-Age	30,528	-0.85	1.146	-5.92	4.95	z-score
Child Weight-for-Height	30,589	-0.27	1.163	-5.00	4.99	z-score
Predicted Bleaching	31,452	2.97	6.532	0.00	29.24	Sum
Reef Pixels	6,200	324.05	521.086	1.00	2,108.00	Count
DHW	31,452	14.70	9.721	0.00	66.72	°C-weeks

Panel A is based on data from Galligan and McClanahan (2024). *Panel B* is based on data from National Bureau of Statistics, Tanzania (2019); World Bank (2021); consumption and working hours refer to the past 7 days. *Panel C* is based on data from DHS (2023a, 2022, 2024, 2023b). Panels B and C cover all household-wave observations and all children in the surveys, respectively. DHW refers to Degree Heating Weeks, the sum of daily sea surface temperature values above the bleaching threshold of 1°C above the long-term maximum monthly mean, divided by 7, over the preceding 12 weeks. Reef pixels is the number of 1 km × 1 km grid cells within a 10 km radius that contain a reef according to Allen Coral Atlas (2022). Predicted bleaching in Panel A is the share of bleached reef pixels among all reef pixels within a 10 km radius; in Panel B, the sum of bleaching dummies over all pixels within 10 km across all months in the year preceding the survey; in Panel C, the sum of monthly predicted bleaching within 10 km from 12 months before to 24 months after birth.

A.2 Supplementary Information

A.2.1 Details on the Empirical Approach

Tropical corals are animals that form the basis of the coral reef ecosystem and are essential for its structure (Magel et al., 2019). If sea temperatures exceed the corals' temperature tolerance range (Speers et al., 2016) for longer periods of time, the animals suffer from a phenomenon called coral bleaching (Brown, 1997). Central to this phenomenon is that corals live in symbiosis with algae that provide nutrients necessary for their survival. I refer the reader to Helgoe et al. (2024) for details on the underlying mechanism. Under thermal stress, corals expel these symbiotic partners, which turns the coral translucent and exposes its skeleton - the coral bleaches. While corals can recover, severe cases can be fatal and erode the structure of the reef (Magel et al., 2019), affecting fish (Rogers et al., 2018).

Accumulated heat stress, the main trigger of coral bleaching, is usually determined based on Degree Heating Weeks (DHW). As shown in Equation A.1, they are computed as the sum of daily occurrences of sea surface temperatures above the bleaching threshold divided by 7 over a period of 12 weeks (84 days). The bleaching threshold, also called Hot Spot HS , is reached at temperatures of 1°C above the maximum monthly climatological baseline and expressed in $^\circ\text{C}$ -weeks. Mortality increases substantially at 6-10 DHW (Humanes et al., 2022). The calculation is implemented at the day d level, but for the analysis I use the maximum value for each month.

$$DHW_{i,d} = \sum_{k=0}^{83} \frac{HS_{i,d-k}}{7}, \quad HS_{i,d-k} \geq 1, \quad (\text{A.1})$$

In addition to temperature, other stressors such as light, cold, acidification, hyposalinity, or infection can also contribute to coral bleaching and increase the vulnerability of coral. Most important in the context of this work are high levels of dissolved nitrogen and hypoxia, low oxygen events that can be triggered by the enrichment of organic matter and nutrients (Helgoe et al., 2024). Research has shown that nitrogen pollution in particular

can make corals more susceptible to bleaching (Donovan et al., 2020). This likely explains why some studies have found considerable differences between temperature-based metrics and in-situ observations (DeCarlo and Harrison, 2019; Khalil et al., 2023). Nitrogen often stems from anthropogenic sources, such as fertilizers and agricultural runoff (Jani and Toor, 2018), as well as sewage (Pan et al., 2021). This, in turn, implies that, apart from anthropogenic climate change, localized anthropogenic stressors related to agricultural production and urbanization are also relevant drivers of bleaching.

This means that bleaching is not fully exogenous, necessitating a so called instrumental variable approach to avoid biased results, which also addresses potential measurement error. In simple terms, this approach relies on a part of the variation in bleaching which can be explained by an exogenous instrument, here DHW. Crucial for the validity of the instrumental variable approach are both instrument relevance, meaning that lagged degree heating weeks do indeed explain variation in bleaching, and that it is only related to the outcome of interest through coral bleaching. Based on first stage F-Statistics reported in each of the regression tables, this instrument is relevant: Kleibergen–Paap rk Wald $F = 197.3$ for the main catch specification (13 excluded instruments), $F = 11.3$ for height-for-age (2 excluded instruments; 10.9 with wealth controls), and a conventional first-stage $F = 43.9$ for the consumption analysis (2 excluded instruments; 511 wave-5 households). However, the first stage for the number of trips per site and month, which is computed on an aggregate level, is weak ($F = 5.9$). For height-for-age, weak-instrument-robust Anderson–Rubin tests (Anderson and Rubin, 1949) reject the null of no effect ($P < 0.001$; with wealth controls $P = 0.004$). Depending on the model, I either report the robust Kleibergen–Paap rk Wald F Statistic or, since the treatment intensity approach necessitates a separate estimation of the first stage for the consumption/adaptation regressions, regular F-Statistics. My instrument is arguably exogenous, there is no other plausible way in which accumulated long-term heat stress, conditional on current weather conditions and sea surface temperatures, can affect fish, people, or households. Furthermore, Hansen’s J test of overidentifying restrictions does

not reject for the main regressions (catch per unit effort: $P = 0.13$, $df = 12$; height-for-age: $P = 0.10$, $df = 1$; with wealth controls $P = 0.31$), although it rejects in the poorer-cluster height-for-age subsample ($P = 0.015$) and for two secondary fishery outcomes, fish length ($P = 0.02$) and species richness ($P < 0.001$), for which the test is of limited reliability given 13 instruments and 19 landing sites.

Beyond exogeneity, a crucial assumption for a causal interpretation of my estimates is the Stable Unit Treatment Value Assumption (SUTVA). The validity of inference does not require homoskedasticity or the absence of serial correlation, as fishery regressions use heteroskedasticity-robust standard errors together with wild cluster bootstrap P values clustered by landing site, anthropometric regressions use standard errors clustered on a ≈ 1 km grid, and the consumption and adaptation regressions use a bootstrap. For the analysis of household consumption and adaptation, regular clustered standard errors are invalid due to the manual two-stage approach. Therefore, a household-level cluster bootstrap (Cameron et al., 2008) with 1,000 iterations, re-estimating the first stage in each, is used as the main specification and reported alongside the results with regular clustered standard errors on district-level. All controls and explanatory variables are standardized. SUTVA could be violated due to spillovers if bleaching in one area means that fish move to other areas. I try to alleviate this concern by considering bleaching within a 10 km radius. This should be sufficient, as most coral reef fishes move less than 10 km (Green et al., 2015). The only exceptions are individual species of predators, such as groupers or sharks.

Bleaching is only observed from 2016 onward. This covers the important global bleaching wave until 2017, when especially East Africa suffered from very high sea surface temperatures (Eakin et al., 2018), as well as part of the more recent marine heat wave affecting the region in 2020. For the Tanzanian household panel, the post-treatment round (the 2019/20 NPS-SDD follow-up) was fielded between late January 2019 and mid-January 2020, with additional tracking interviews in March and April 2020 (World Bank, 2021). Since bleaching exposure is measured over the 12 months before each household's interview, the household

analysis mainly captures bleaching in 2018 and 2019. Nevertheless, these reefs were likely also affected by bleaching before 2016. If that bleaching was correlated with bleaching during my observation period, it would mean that the results could represent lower bound estimates due to the fact that additional bleaching in an already degraded area is likely to have a lower marginal effect. However, the analysis focused on household behavior in Tanzania does not indicate that households more affected by bleaching during my analysis period were different from less affected households in pre-treatment periods, which suggests that this is unlikely to meaningfully affect the results.

For the first part of the analysis regarding fish catch, the outcomes of interest are catch, and, to facilitate a better understanding of the process, catch per unit effort (fisher×day), average fish length, and species richness. Furthermore, I back out a measure of effort (fisher×day) from the information on catch and catch per unit effort. Besides this intensive margin measure of fishing effort, I also consider the number of trips per month and site in a separate regression on site level as a measure of extensive margin effort.

Taking the outcome of interest per fishing trip i , monthly time t , and site s , instrumenting bleaching in the month of observation with contemporaneous degree heating weeks and its individual lags up to lag 12 (DHW), and controlling for a vector of climate and reef-exposure variables $X_{s,t}$ as well as year y , month m , and site s fixed effects, the first and second stages of the regressions are as follows,

$$\text{Bleaching}_{s,t} = \pi_0 \text{DHW}_{s,t} + \sum_{l=1}^{12} \pi_l \text{DHW}_{s,t-l} + X'_{s,t} \pi + \alpha_y + \alpha_m + \alpha_s + u_{s,t}, \quad (\text{A.2})$$

$$Y_{i,s,t} = \beta_1 \widehat{\text{Bleaching}}_{s,t} + X'_{s,t} \beta + \alpha_y + \alpha_m + \alpha_s + \varepsilon_{i,t,s}. \quad (\text{A.3})$$

The vector $X_{s,t}$ includes sea-surface temperature (mean, minimum, maximum, and mean squared), mean wind speed, 2m air temperature, and total precipitation, together with a control for the number of 10 km reef pixels surrounding the site.

Regarding the analysis of bleaching impacts on household consumption and adaptation, the treatment group comprises all survey households within 100 km of the shore with ob-

served coral bleaching within 10 km of their closest coastal point. Control households are all other households. Outcomes of interest are the amount of fresh fish consumed, the overall consumption of animal protein in the form of fresh and dried fish, beef, poultry, as well as goat and sheep meat². For adaptation, the outcomes of interest are the number of livestock, profits from land, hours worked, salary received, as well as the likelihood of migration. To analyze the latter, treatment status in wave 5 (the 2019/20 follow-up) is assigned based on household location in wave 4 (2014/15). I then regress a dummy indicating whether a household moved between 2014/15 and 2019/20 on treatment status in a logit regression. Migration is defined as living in a different district at the follow-up than in 2014/15, so moves within a district are not captured. The results are shown in Table A.12 and do not indicate a significant relationship between migration and treatment status (coefficient -0.85 , $P = 0.11$; with weather controls 1.05 , $P = 0.25$; $n = 1,511$ households, $1,480$ with weather controls).

The first stage is estimated on wave-5 household observations i , using bleaching, DHW and controls measured at the level of the household's district h and interview month t . The second stage is then implemented at a household i , district h , and monthly time t level, while the main variation I study is between survey waves w . The regression equations are specified as follows, with controls for the number of household members, a vector of climate controls $X_{i,t,h}$ as well as household i and wave w fixed effects. Treatment intensity is split into two bins at the median (low and high bleaching).

$$\text{Bleaching}_{h,t} = \pi_1 \text{DHW}_{h,t} + \pi_2 \text{DHW_past}_{h,m} + \pi X'_{h,t} + u_{h,t}, \quad (\text{A.4})$$

$$\log(Y_{i,h,t} + 1) = \underbrace{\sum_{q=1}^Q \sum_{w \neq w_4} \beta_{q,w} D_{i,q} D_w}_{\text{Treat. Int. Bin} \times \text{Wave}} + \theta_1 \text{HHmembers}_{i,h,t} + \theta X'_{i,h,t} + \alpha_i + \alpha_w + \varepsilon_{i,h,t} \quad (\text{A.5})$$

²The analysis focuses on different meats and fish as important protein sources. Even though a vegetarian diet could also deliver sufficient protein, animal-source foods, particularly fish, play a critical role in supplementing diets with high-quality protein (Obiero et al., 2019).

Results using a standard difference-in-differences approach without treatment intensity are also reported. Due to the manual two-stage approach, regular clustered standard errors are invalid. Therefore, the household-level cluster bootstrap (Cameron et al., 2008) with 1,000 iterations is used for the main event-study specification and reported alongside the results with regular clustered standard errors on district-level; the pooled difference-in-differences and the interaction specifications report district-clustered standard errors only.

For the analysis of stunting and wasting, outcomes of interest are height-for-age, as well as weight-for-age and weight-for-height z-scores. To reduce potential biases due to migration, the data is limited to children born after the first of January 2015; all children in the final sample were born in 2016 or later. This assumes that the likelihood of young children having migrated is low, even though I do not have information in this regard, which is a limitation of the analysis. However, if bleaching would be correlated with households moving, this would likely mean that I fail to identify children hit hardest by the shock as treated, which would bias the results towards zero. Furthermore, as shown above, research using household panel data for Tanzania shows that coral bleaching did not affect migration, even though this result does not necessarily hold across all countries. Since household wealth is likely the most relevant factor in determining the relationship between bleaching and consumption as well as adaptation, I add household wealth (DHS wealth-quintile dummies) as a control and implement separate specifications for DHS clusters whose average wealth quintile is above and below the sample mean of 2.7 ($n = 4,043$ and $n = 3,738$ children, respectively). Wealth is measured in the form of an index compiled from information on assets and living conditions. This index is computed according to the guidelines suggested by the DHS³ and is based on measurable indicators of wealth, such as ownership of a motorcycle or TV, which I expect to be relatively stable over time and not directly affected by a potential price change or change in availability of fish, even though endogeneity cannot be fully ruled out.

³For more information, see dhsprogram.com/topics/wealth-index/Wealth-Index-Construction.cfm, last visited 24-10-2025.

The main explanatory variable of interest is bleaching per household-cluster h and monthly time t . The regression controls for sea and land weather variables aggregated into 12-month averages for the year before birth and the first and second years after birth, as well as birth-year (y), month (m), and cluster (h) fixed effects. Since I expect relative changes, the outcome of interest in logs is regressed on coral bleaching exposure, defined as the sum of standardized monthly predicted bleaching within 10 km over the 12 months before and the 24 months after birth. The latter is instrumented with degree heating weeks (DHW), aggregated into two event-time windows relative to the child’s month of birth: the summed DHW over the 12 months prior to birth and the summed DHW over the 24 months following birth. Both stages are estimated jointly at the child level by two-stage least squares, with exposure and instruments assigned to each child i based on their cluster h and birth month t . Standard errors are clustered on a 0.01 degree grid level (approximately 1 km; 968 clusters), defined by rounding household coordinates. Results are robust to clustering by DHS cluster (599 clusters; $P = 0.004$) and on a 0.1-degree (≈ 10 km) grid (667 clusters; $P = 0.004$), which approximates the level at which exposure is assigned; on a 1-degree (≈ 100 km) grid the estimate is marginally significant (113 clusters; $P = 0.085$; Table A.7). Since height-for-age, weight-for-age, and weight-for-height z-scores can take negative values, each outcome is rescaled prior to logging by subtracting its sample minimum and adding one before estimation.

Results are also robust to how incomplete exposure windows are handled. For some children, the 36-month window extends beyond the period covered by the bleaching or temperature data; missing months enter the exposure sum as zero, i.e. at the sample mean of the standardized monthly measure. Controlling for the number of observed months leaves the estimate unchanged (-0.018 , 95% CI -0.028 to -0.008 ; $P < 0.001$; first-stage $F = 12.6$). Because the 24-month post-birth window extends beyond the measurement date for younger children, I also restrict the sample to children older than 24 months at interview, whose entire exposure window precedes measurement. The estimate is somewhat smaller but remains

negative and significant (-0.013 , 95% CI -0.021 to -0.004 ; $P = 0.003$; $n = 4,195$; first-stage $F = 8.5$), and the main estimate lies within its confidence interval (Table A.7).

$$\text{Bleaching}_{h,t} = \pi_0 + \pi_1 \text{DHW_past}_{h,t} + \pi_2 \text{DHW_future}_{h,t} + \pi X'_{h,t} + \alpha_y + \alpha_m + \alpha_h + u_{h,t}, \quad (\text{A.6})$$

$$\log(Y_{i,h,t}) = \beta_0 + \beta_1 \widehat{\text{Bleaching}}_{h,t} + \beta X'_{h,t} + \alpha_y + \alpha_m + \alpha_h + \varepsilon_{i,h,t}. \quad (\text{A.7})$$

Table A.7: Robustness of the Height-for-Age Estimate

	(1) Main	(2) DHS cluster	(3) 10 km grid	(4) 100 km grid	(5) Months observed	(6) Age >24 months
Bleaching	-0.0183*** (0.0053)	-0.0183*** (0.0063)	-0.0183*** (0.0064)	-0.0183* (0.0106)	-0.0183*** (0.0052)	-0.0125*** (0.0041)
N	7,781	7,781	7,781	7,781	7,781	4,195
Clusters	968	599	667	113	968	909
First-Stage F	11.28	8.04	9.68	4.42	12.63	8.48
Hansen J p	0.10	0.13	0.12	0.20	0.13	0.67

IV regressions of the log-transformed height-for-age z-score on bleaching exposure, following the main specification (Table A.3, column 2). Columns differ only in the level of clustering or the sample: (1) 0.01-degree (≈ 1 km) grid (main); (2) DHS cluster; (3) 0.1-degree (≈ 10 km) grid; (4) 1-degree (≈ 100 km) grid; (5) adds dummies for the number of observed months in the 36-month exposure window; (6) restricts the sample to children older than 24 months at interview. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (two-sided). Instruments are summed DHW over the 12 months before and the 24 months after birth. All regressions control for reef pixels and 12-month averages of wind speed, precipitation, mean air temperature and sea surface temperature (mean, squared, minimum and maximum) for the year before birth and the first and second years after birth, and include birth-year, birth-month and DHS-cluster fixed effects. First-Stage F reports the Kleibergen-Paap rk Wald F statistic; Hansen J p has 1 degree of freedom.

A.2.2 Construction and Processing of Satellite Imagery Data

The main data source for satellite data are high-resolution (10m-60m, depending on the band) images from Sentinel-2 (ESA, 2024), which are merged with the location of reefs as well as their benthic zonation, meaning reef (algae/coral), sand, rock, or rubble, respectively, from the Allen Coral Atlas (2022). While Level-1C, meaning uncorrected top-of-atmosphere data, is used due to its longer temporal coverage, a mask with a cutoff of 0.6 for Google's cloud

score is applied to remove clouded areas from the images.⁴ I argue that further atmospheric correction is not needed due to my normalization approach which compares pixels that are affected by the same water and atmospheric conditions. To the main data from Sentinel-2, I add atmospherically corrected red and near-infrared surface reflectance from MODIS-Aqua (Vermote and Wolfe, 2021), which provides these two bands at its native 250m resolution. The resulting data is projected on a consistent 5m resolution and then masked according to the different benthic zones. The brightness values from each Sentinel-2 reflectance band up to B8 (782.5nm (S2A) / 779.7nm (S2B)) are averaged by month and benthic class and the above mentioned variables from MODIS are added. The resulting data is exported at a 1000m resolution.

Each of the seven main brightness bands from Sentinel-2 in zones designated to be coral by the Allen Coral Atlas (2022) is normalized by the corresponding brightness value for sand, rock, and rubble, respectively, in the same pixel. Even though the benthic zonation by Allen Coral Atlas (2022) might not be perfectly accurate and might change over time, this approach is unlikely to lead to false positives. Imperfect assignment would likely only weaken a potential brightness change signal. These normalized brightness values are supplemented with their non-normalized versions, seasonally adjusted variants of both, and the MODIS reflectance variables described above, and used as inputs for the neural network. In a comparison of three feature sets on the validation data, namely raw bands only, raw bands with benthic-normalized ratios, and the full set including seasonally adjusted variants, the full feature set including seasonally adjusted variants achieved the highest validation PR-AUC and was retained for the final model. This indicates that deseasoning meaningfully improves performance once the full feature set is used.

⁴For details, refer to <https://medium.com/google-earth/all-clear-with-cloud-score-bd6ee2e2235e>, last visited 20-09-2025.

A.2.3 Details on the ML Approach

I employ a deep recurrent neural network with stacked Long Short-Term Memory (LSTM) layers at its core (Hochreiter and Schmidhuber, 1997). LSTM models are particularly well suited for time-series predictions, as they consider temporal dependencies in the data (van Houdt et al., 2020). This is especially relevant in detecting coral bleaching, as it is the change in coral brightness over time that is indicative of a coral bleaching event. A gated recurrent unit (GRU) variant was also evaluated, but underperformed compared to the LSTM on the validation set, therefore only the LSTM variant was retained.

The ML approach is trained and evaluated over the described global and GBR-focused datasets. For this, I match satellite observations in a radius of 879 meters (the maximum possible distance of the centroid to the outer corners of a grid cell) to the coral bleaching observations. This results in an overall set of 1167 training, 390 validation and 390 testing observations. After removing cases without sufficient satellite coverage, that results in 1,128, 390 (226 reefs), and 376 (216 reefs) observations, respectively.

To determine how much temporal context the model should use, I jointly search over the window length (months of satellite data considered before and after each survey) and the maximum length of a data gap that may be linearly interpolated, evaluating both on the validation set. Gaps longer than the chosen maximum were left missing rather than interpolated, and interpolation never extrapolated beyond the first or last available observation for a given reef cell. The chosen configuration considers 3 leads and 3 lags of each feature (a 7-month window), with gaps of up to 1 month interpolated.

The overall architecture was tuned using Optuna (Akiba et al., 2019), searching over the number and size of recurrent layers, dropout and recurrent dropout rates, learning rate, optimizer, batch size, activation function, and use of a bidirectional wrapper, scored by average precision on the validation set. The final architecture consists of three bidirectional LSTM layers with 32, 8, and 16 units, respectively, a dropout layer with rate 0.53 (recurrent dropout 0.09), and two dense hidden layers with 64 and 64 units, respectively, and tanh

1020 activation, mapping to a single output that gives a bleaching probability between 0 and 1.
1021 The model uses binary cross-entropy loss, the RMSprop optimizer with a learning rate of
1022 0.00134, mini-batches of size 128, and up to 100 epochs with early stopping (patience 10)
1023 monitored on the validation loss, achieving a best validation PR-AUC of 0.866. To reduce
1024 the influence of any single random initialization, the final configuration was fit 10 times with
1025 different random seeds; validation PR-AUC had a standard deviation of 0.016 across these
1026 runs, ranging from 0.811 to 0.866.

1027 **Data splitting.** Because consecutive monthly satellite observations from the same reef cell
1028 produce highly correlated, overlapping training sequences, splitting individual observations
1029 at random would allow similar sequences from the same location to appear in both the
1030 training and evaluation sets. Reef cells were therefore split by location: 60% for training,
1031 20% for validation, and 20% for testing, with no cell appearing in more than one split.
1032 All architecture, window-length, feature-set, and threshold decisions described above were
1033 made using the training and validation sets only; the test set was not scored until every
1034 modeling choice had been fixed. Within the training set, class imbalance between bleached
1035 and non-bleached observations was addressed via class weighting; the validation and test
1036 sets retained their natural class balance, since the classification threshold (below) is chosen
1037 to reflect real-world prevalence rather than an artificially balanced one.

1038 **Classification threshold.** Rather than fixing the classification threshold at $p = 0.5$, I se-
1039 lected it on the validation set by maximizing balanced accuracy. An F1-maximizing threshold
1040 was also computed (0.35) but not used, since the validation set’s positive rate did not match
1041 that of the held-out test and external data, and selecting on F1 in this setting would favor
1042 thresholds that over-predicted the majority class; the chosen threshold of 0.65 performed
1043 more consistently across held-out sets.

A.2.4 Model Performance

Table A.8 reports performance separately for the validation set, which was used to select the parameters above, and therefore expected to be optimistic, and the held-out test set, comprising reef cells never used in any tuning step. Validation PR-AUC reached 0.866, compared to test PR-AUC of 0.810 (Test accuracy 0.723, F1 0.731, 376 test windows) – a gap consistent with the sample sizes available for model selection, and smaller than corresponding gaps observed in configurations with lower validation PR-AUC.

I additionally evaluate the model on two external holdout sets not used at any stage of model development: (i) bleaching surveys from the same underlying data sources set aside prior to model construction, reported both including and excluding the reef cells that coincide spatially with training locations, and (ii) an independently published 2024 bleaching survey from the Kenyan coast (Mayfield, 2025), entirely unconnected to any data used in model development. Restricted to reef cells with no spatial overlap with training, the first external set achieved PR-AUC 0.900 (accuracy 0.638, F1 0.704; $n = 58$ windows, 50 cells), consistent with the primary test-set result. My approach could further match 11 out of the 20 surveyed sites in the Mayfield dataset to satellite imagery, of which 8 (one shared with the training locations and excluded here) had sufficient surrounding satellite coverage to construct a complete evaluation window. 7 of these 8 sites were correctly classified as bleached (recall 0.875; precision 1.000, as no site was incorrectly flagged as bleached). Because every matched site in this dataset recorded bleaching, this comparison contains no true-negative cases; ROC-AUC and PR-AUC are consequently undefined and omitted, though precision, recall, F1, and accuracy remain computable and are reported. It demonstrates the model’s sensitivity to a real, severe, independently documented bleaching event, but cannot speak to specificity, which is instead assessed on the test and primary external holdout sets above.

Regional performance. One risk is that model performance varies by region due to uneven geographic coverage of the training data. Table A.9 reports performance by region

for both the test and external sets. Regions well represented in training show moderate and consistent prediction quality across all held-out sets. Regions with limited training coverage, notably the Northern Hemisphere, perform close to chance, which indicates that there is a slight domain shift issue present. Due to low coverage, these results are, however, likely unreliable and the separate test on the Mayfield data indicates that the model performs sufficiently well for East Africa.

Comparison to the literature To the best of my knowledge, no prior published work directly applies a recurrent neural network directly to satellite reflectance to determine site-level coral bleaching, as opposed to derived thermal and ocean-color indices.

However, the paper by Xu et al. (2021) is most closely comparable to this work. They used Sentinel-2 brightness changes to detect coral bleaching on a single reef (Lizard Island) and report an overall classification accuracy of 88.9%. This is not directly comparable to my results due to the imbalanced validation sample in their case, on which a majority class classifier would already achieve a high accuracy. Restricting the comparison to the bleached class, their precision is 71.4% and outperformed by the precision of my approach, which ranges between 77 and 89% across the different test sets.

A more recent Red Sea study, Gokul et al. (2025), uses Sentinel-2-derived bottom reflectance to predict bleaching in one of four categories using a least-squares classifier. Based on 34 observations, they reach a high level of precision, especially considering that they predict four classes, with an overall accuracy of 82% and, calculated based on their reported table, an F1 score of 82%. These results are broadly similar to my performance metrics, outperforming my model on accuracy and F1 for the primary test set, though comparable to my external holdout figures. However, the comparison should be read cautiously, as their evaluation set is an order of magnitude smaller than my primary test set, contains only one non-bleached observation, and is not accompanied by separate precision, recall, or PR-AUC

figures that would allow a like-for-like assessment. Furthermore, their approach focuses on one small region, which likely shows less variability than my global training set.

Outside the Red Sea, Chintya et al. (2026) propose an approach based on both ocean color and heat stress, which my model deliberately excludes. The relevant comparison, their validation based on in-situ observations, reports a ROC-AUC of 0.855, slightly above my results. However, ROC-AUC alone understates the picture: my PR-AUC values (0.781–0.900 across evaluation sets) exceed their reported average precision of 0.498 based on field only validation, which is relevant considering the class imbalance present in several of their subsets. Generally, their work confirms that models drawing on a longer satellite-derived environmental history outperform thermal-stress-only baselines.

Another similar work is the Allen Coral Atlas bleaching monitoring tool. The tool itself is currently only available in a beta version, cannot be obtained for research, and there are no published performance numbers for their bleaching detection approach. They do, however, point towards a related paper relying on a slightly different methodology, namely Xu et al. (2020), which uses a single reflectance band and is based on z-score brightness deviations from a historical baseline after comprehensive data cleaning. Their best performing approach, validated against 18 field-confirmed bleaching locations in the Hawaiian Islands, reports a detectability of 66.1%. This figure is a detection rate on a sample with only bleached observations, and the authors state explicitly that assessing false positive and false negative rates was left to future work. This means that this result is not directly comparable, but similar to the validation against the all-positive Mayfield (2025) dataset, where my approach correctly scored 7/8 bleached observations at the chosen threshold. On the overall evaluation, this is most similar to recall, which I calculate with the positive class as the reference. There, my approach reaches 66.8% on the primary test set and 58.1% on the new sites from the external holdout set – the latter below the Xu et al. (2020) benchmark, reflecting the higher classification threshold (0.65) selected here to balance precision and recall rather than to maximize sensitivity alone. While Xu et al. (2020) additionally report several machine-

learning approaches underperformed that statistical base line, the results presented here show that this limitation is not general to machine-learning approaches in this context.

Overall, this comparison indicates that my results are both different from other approaches in the literature in terms of methodology and/or scale, but obtain results that are comparable or better than similar prediction exercises.

Table A.8: Model Performance Across Evaluation Sets

	Accuracy	Bal. Acc.	Precision	Recall	F1	ROC-AUC	PR-AUC	N
Validation ^a	0.713	0.723	0.834	0.687	0.753	0.783	0.866	390
Test ^b	0.723	0.731	0.806	0.668	0.731	0.770	0.810	376
External holdout (all sites) ^c	0.714	0.717	0.771	0.644	0.702	0.786	0.781	569
External holdout (new sites only) ^d	0.638	0.691	0.893	0.581	0.704	0.781	0.900	58
East Africa, independent ^e	0.875	0.875	1.000	0.875	0.933	–	–	8

Notes: All metrics use the classification threshold of 0.65, selected on the validation set by maximizing balanced accuracy. *N* denotes scored windows (7-month survey-anchored sequences), not distinct reef cells; see Table A.9 for cell- and survey-level counts by region.

^aUsed for model selection; not an estimate of out-of-sample performance.

^bReef cells held out from all stages of model development (216 cells).

^cIncludes reef cells that coincide spatially with training locations (465 cells).

^dSame source as (c), restricted to reef cells with no spatial overlap with training (50 cells).

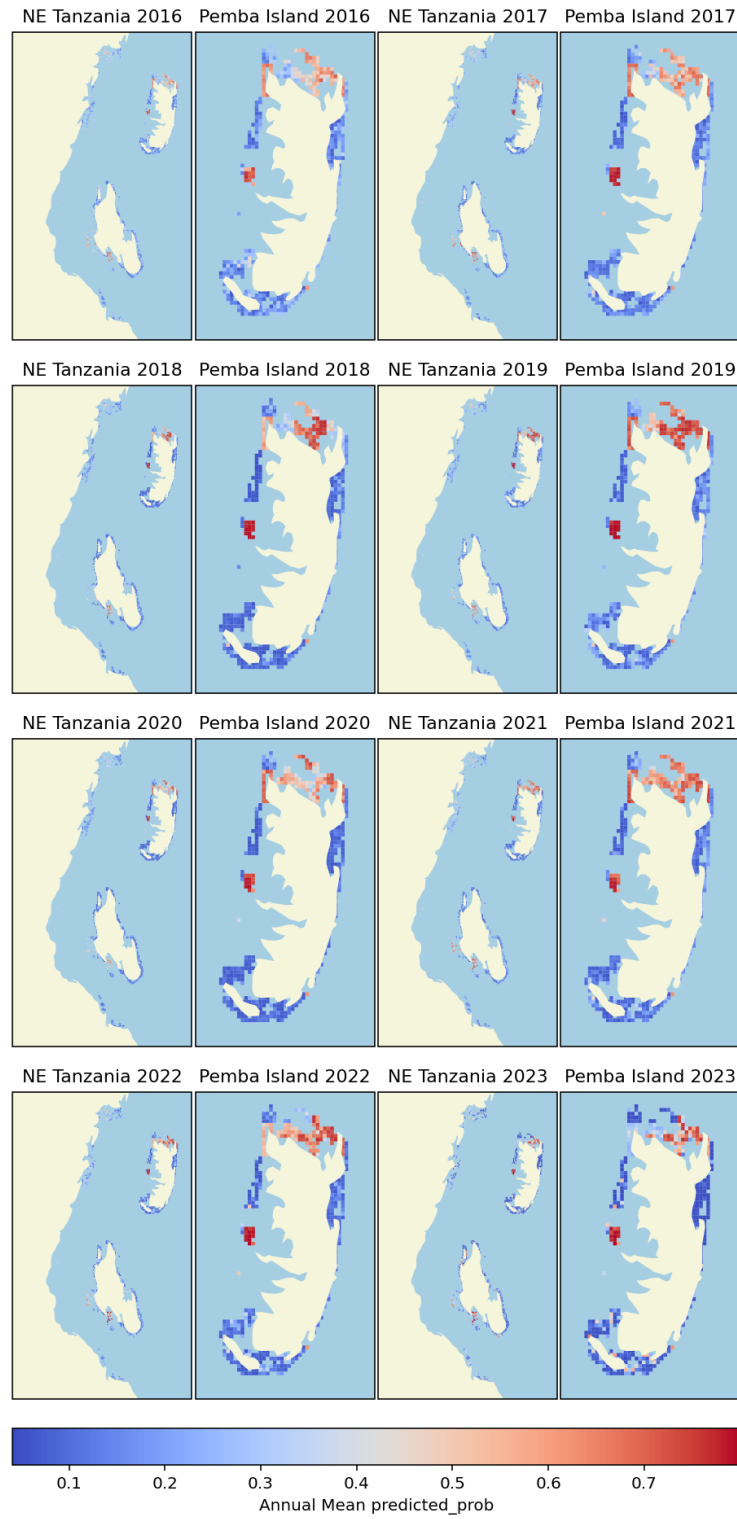
^eMayfield (2025); 8 of 20 surveyed sites yielded a complete evaluation window. As no true negatives exist in this sample, ROC-AUC and PR-AUC are undefined and omitted; precision, recall, F1, and accuracy remain computable and are reported (see Section A.2.4).

Table A.9: Model Performance Across Regions (Test Set)

Region	Cells	Surveys	Accuracy	Bal. Acc.	Precision	Recall	F1	ROC-AUC	PR-AUC
Australia	157	283	0.721	0.733	0.854	0.694	0.766	0.767	0.860
Vert. Trans. 1*	16	37	0.838	0.738	0.429	0.600	0.500	0.619	0.507
Vert. Trans. 2*	5	5	0.400	0.250	0.667	0.500	0.571	0.500	0.887
Vert. Trans. 3	195	334	0.716	0.727	0.824	0.673	0.741	0.765	0.827
S Hemisphere	176	323	0.728	0.735	0.827	0.702	0.760	0.771	0.837
N Hemisphere	40	53	0.698	0.514	0.286	0.154	0.200	0.515	0.271

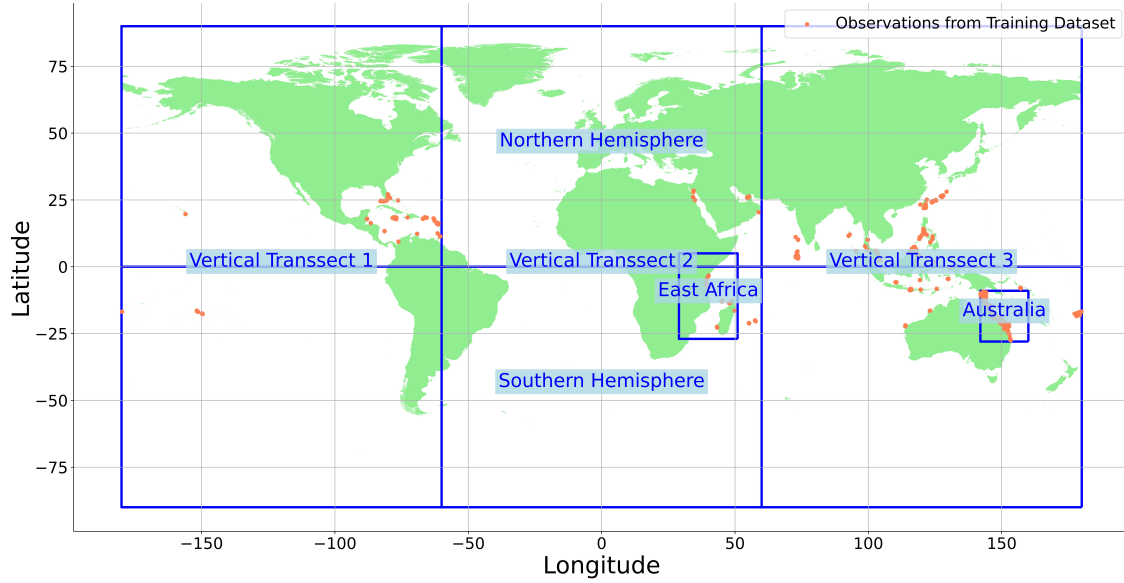
The table summarizes model performance by region on the held-out test set (reef cells not used in any stage of model development; threshold 0.65, see Table A.8). *Cells* is the number of distinct reef locations and *Surveys* the number of matched bleaching observations underlying each region's metrics; both are reported because overlapping monthly satellite sequences from the same reef are not independent observations. Regions marked * have fewer than 20 held-out reef cells and their metrics should not be interpreted as reliable standalone estimates of regional performance; East Africa and Vertical Transect 2 share an identical confusion matrix at this sample size.

Figure A.1: Predicted Bleaching for Northeastern Tanzania and Pemba Island



The figures depict mean annual predicted bleaching probability for each pixel.

Figure A.2: Geographic Regions for Testing and Distribution of Observations



The map shows the geographic extent of the different regional sub-samples the model performance was also evaluated on, alongside the distribution of observations used for training.

A.2.5 Additional Results for Fish Catch

In this section, I briefly review how the results on catch change based on different specification choices. The first of these choices is the set of fixed effects, with Table A.10 showing the main estimates for both OLS and IV specifications across different fixed effect specifications. The first notable result is that the usage of the IV matters, as OLS results are smaller for the main regression, although they remain in the same order of magnitude. This is in line with the potential reverse causality problem that motivated my use of instrumental variables. Both OLS results as well as the IV results change with the set of fixed effects, which indicates that the choice of the most comprehensive set of fixed effects (site fixed effects plus year and month dummies) is warranted in my case.

Another relevant robustness check is to consider different instrument specifications, as demonstrated in Table A.11. The specifications differ in how they consider the main instrument, degree heating weeks (DHW). Besides my main approach using current DHW as well as individual lags of DHWs up to the 12th lag (column 4), reported specifications consider current DHW only (column 1), current DHW and the sum of DHW over the 12 preced-

Table A.10: Regressions of Fishery Outcomes on Bleaching (Alternative Fixed Effects Specifications)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	IV	OLS	IV	OLS	IV
Predicted Class	-0.343* (0.19)	-0.409** (0.20)	-0.0129 (0.086)	0.126 (0.13)	-0.236* (0.13)	-0.229 (0.15)
Fixed Effects	Site, Year, Month		Site		Site, Month	
N	593	593	593	593	593	593
R^2	0.150	0.073	0.124	0.042	0.139	0.062
Adj. R^2	0.087	0.003	0.082	-0.006	0.082	-0.002
First-Stage F		197.33		38.10		88.63
Hansen J p		0.13		0.43		0.50
Boot p		0.03		0.46		0.16

Standard errors in parentheses: clustered on site in column (1), heteroskedasticity-robust in columns (2)–(6). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions control for the number of reef pixels within 10 km, mean wind speed, mean 2 m air temperature, precipitation, and mean, squared mean, minimum and maximum sea surface temperature, and include the indicated fixed effects (site fixed effects; year and month dummies). IV columns instrument bleaching with current DHW and its 12 monthly lags. First-Stage F reports robust Kleibergen-Paap rk Wald F-Statistics for the first stage. Hansen J p-value reports the p-value of the Hansen J test for overidentification of all instruments (12 degrees of freedom). Boot p reports wild cluster bootstrap p -values, clustering on site (Rademacher weights, 999 replications, null imposed).

ing months (column 2) as well as DHW lags 1–12 only (column 3). My chosen approach has by far the highest F-statistic, which indicates that incorporating the detailed DHW lag structure matters for the model. All estimators are negative, while only the last two are significant when considering bootstrapped standard errors with site-clusters. Because wild bootstrap P values are simulated (999 replications), they vary slightly across tables for the same specification (0.03–0.05 for the main catch per unit effort estimate).

Table A.11: Regressions of Fishery Outcomes on Bleaching (Alternative Instruments)

	(1)	(2)	(3)	(4)
	Instrumental Variable Regression (IV)			
Bleaching	-2.741*	-2.590**	-0.465**	-0.409**
	(1.52)	(1.10)	(0.20)	(0.20)
N	593	593	593	593
R^2	-0.169	-0.139	0.073	0.073
Adj. R^2	-0.258	-0.226	0.002	0.003
First-Stage F	8.53	7.76	158.02	197.33
Hansen J p	–	0.88	0.10	0.13
Boot p	0.27	0.23	0.02	0.04

The regressions employ the following variables as instruments: (1) current degree heating weeks (DHW) only; (2) current DHW and the sum of DHW over the 12 preceding months; (3) DHW lags 1–12 only; (4) current DHW and DHW lags 1–12 (main specification). Heteroskedasticity-robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions control for the number of reef pixels within 10 km, sea surface temperature (mean, maximum, minimum and mean squared), mean wind speed, mean 2 m air temperature and precipitation, and include site fixed effects and year and month dummies. First-Stage F reports robust Kleibergen-Paap rk Wald F-Statistics for the first stage. Hansen J p reports the p -value of the Hansen J test for overidentification of all instruments (not applicable for the exactly identified specification in column 1). Boot p reports wild cluster bootstrap p -values (Roodman et al., 2019), clustering on site (Rademacher weights, 999 replications, null imposed).

A.2.6 Additional Results Household Consumption and Adaptation

A potential threat to the validity of the results on household consumption is attrition. If treatment would be related to the likelihood of a household being in the sample in wave 5 (the 2019/20 NPS-SDD follow-up), this could lead to biases. Overall attrition of the Extended Panel between 2014/15 and 2019/20 was low (4.9%; World Bank, 2021). To test for this issue, wave 5 bleaching status is allocated to all coastal households interviewed in

2014/15 based on their wave 4 location, and logit regressions of a dummy for being in the sample in wave 5, and of a dummy for having moved between the survey waves, on treatment status are implemented. For households not observed in wave 5, the interview date is set to July 2020. Results are shown in Table A.12 and indicate that treatment status does neither significantly predict presence of households in wave 5 nor whether they have moved between the waves (attrition: coefficient -0.24 , $P = 0.53$, $n = 1,695$ households; moving: -0.85 , $P = 0.11$, $n = 1,511$). The test covers all coastal households interviewed in 2014/15, including Refresh Panel households that are not part of the main sample; among these, 184 of 1,695 households (10.9%) are not observed in wave 5.

Table A.12: Regression of Migration and Survey Presence (Attrition) on Treatment Status

	(1) Present in 2019/20 follow-up	(2) Moved between 2014/15 and 2019/20	(3) Moved between 2014/15 and 2019/20 (Weather Controls)
Treated based on 2014/15 location	-0.237 (0.37)	-0.854 (0.53)	1.048 (0.90)
N	1,695	1,511	1,480
Pseudo R ²	0.001	0.019	0.254

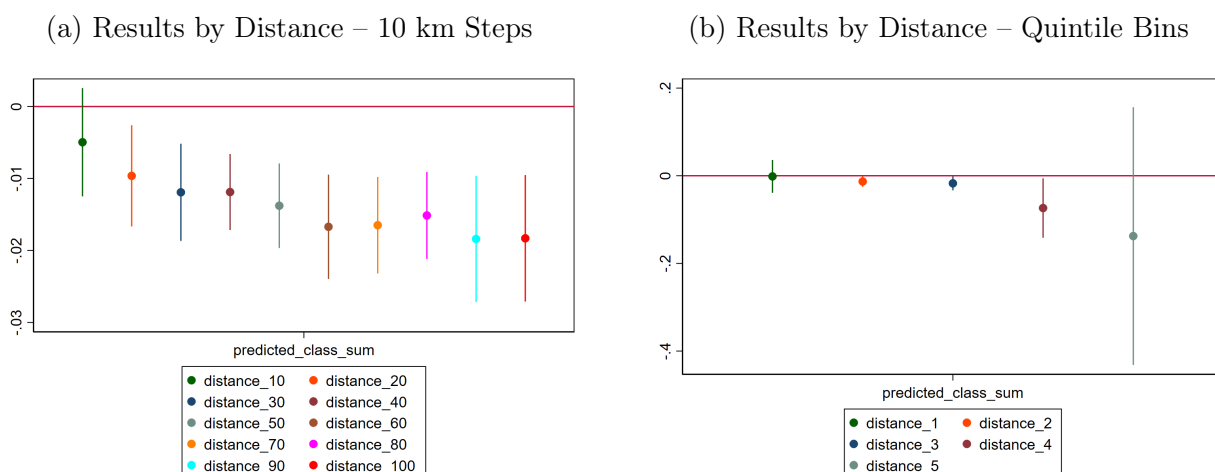
This table reports results from logit regressions on households interviewed in 2014/15 within 100 km of the shore. The outcome in column (1) is a dummy for whether a household is present at the 2019/20 follow-up; in columns (2) and (3), for whether a household lives in a different district at the follow-up than in 2014/15, conditional on being present. Column (3) repeats the specification in column (2) adding weather controls. Treated equals one if any bleaching was predicted within 10 km of the closest shore point of the household's 2014/15 district in the 12 months before the follow-up interview (July 2020 for households not re-interviewed). Column (3) controls for averages of sea surface temperature (mean, maximum, minimum and squared), precipitation, zonal and meridional wind speed, evaporation, surface pressure, and minimum and maximum air temperature over the 12 months and the 13 to 24 months before the follow-up interview. Standard errors clustered on district level in parentheses (28 districts in columns (1) and (2); 27 in column (3)). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (two-sided). Two-sided P values for the treatment coefficient: $P = 0.53$ in column (1), $P = 0.11$ in column (2) and $P = 0.25$ in column (3).

A.2.7 Additional Results Stunting and Wasting

As exposure is assigned based on the closest point at the shore, not the individual household cluster location directly, it is sensible to check how the effect develops with distance to the shore. Due to an unevenness in the number of observations over distance, which would lead

to some distance bins with very few observations when using fixed cutoffs, regressions are implemented using a) fixed 10 km-step distance-to-shore cutoffs including all observations up to the respective distance and b) distance-to-shore quintile bins. Results for both approaches are shown in Figures A.3a and A.3b. They indicate that people living very close to the shore are not affected, while the effect materializes starting from a few kilometers from the shore; estimates for the two outermost quintiles are imprecise. Even though these results have to be interpreted with caution, as households could differ in other dimensions beside distance, this could indicate that people directly at the shore retain access to seafood, while more regional markets are suffering.

Figure A.3: Regression Results by Distance to Shore



The figures plot estimated regression coefficients of separate regressions, the approach follows the main IV result, column (2) in Table A.3. In panel (a) each regression considers all observations in the sample $\leq$ the respective distance cutoff, in panel (b) observations per distance-to-shore quintile. Bands show 90% confidence intervals (two-sided). Quintile cutoffs in panel (b) are 2km; 7.9km; 22.7km; and 56.7km. In panel (a), the number of children increases from 3,432 within 10 km of the shore to 4,467, 5,152, 5,651, 6,030, 6,344, 6,753, 7,058 and 7,389 for the 20–90 km cutoffs, and 7,781 (the full sample) at 100 km; in panel (b), quintiles contain 1,550, 1,556, 1,544, 1,563 and 1,553 children.

Wasting, meaning that one is too light for one's height, can indicate short-term malnutrition. Exposure to coral bleaching during early childhood should not have an impact on current malnutrition if the effects do not persist due to a lack of recovery of the reef or adaptation. To test this possibility, regressions of weight-for-height, a measure for wasting, as

1180 well as weight-for-age, which combines stunting and wasting and is a more general measure
1181 for being underweight, are shown in Table A.13.

1182 Regarding weight-for-height z-scores, which are an indicator for contemporaneous malnu-
1183 trition, bleaching exposure also reduces the log-transformed weight-for-height z-score (-0.008 ,
1184 95% CI -0.015 to -0.002 ; $P \approx 0.01$; $n = 7,894$; first-stage $F = 11.5$). The estimate is sim-
1185 ilar when controlling for wealth (-0.007 , 95% CI -0.013 to -0.001 ; $P = 0.02$; $F = 11.2$).
1186 It is larger and significant in poorer clusters (-0.018 , 95% CI -0.032 to -0.004 ; $P = 0.01$;
1187 $n = 3,806$; $F = 5.3$) but not significant in richer clusters (-0.006 , $P = 0.16$; $n = 4,088$; $F = 8.0$).
1188 This indicates that bleaching exposure in early childhood is associated with persistent short-
1189 term malnutrition at the time of the survey, particularly among poorer households, although
1190 the weak first stages in the wealth subsamples call for caution. Weight-for-age, which is
1191 estimated in z-score units, decreases significantly (-0.078 standard deviations per unit of
1192 exposure, 95% CI -0.123 to -0.033 ; $P < 0.001$; $n = 7,818$; first-stage $F = 11.3$), also when
1193 controlling for wealth (-0.057 ; $P < 0.01$) and in both poorer (-0.137 ; $P < 0.01$; $F = 5.2$) and
1194 richer clusters (-0.070 ; $P < 0.05$; $F = 7.9$). As weight-for-age is a composite measure that
1195 also covers the shorter height of affected children, this result is likely due to both the former
1196 results on stunting as well as the results on weight-for-height.

Table A.13: Regression of Weight-for-Height and Weight-for-Age Z-Scores on Bleaching

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Weight-for-Height				Weight-for-Age		
	IV	Wealth IV	Poor IV	Rich IV	IV	Wealth IV	Poor IV	Rich IV
Bleaching	-0.00818*** (0.0032)	-0.00726** (0.0031)	-0.0178** (0.0071)	-0.00573 (0.0041)	-0.0777*** (0.023)	-0.0565*** (0.020)	-0.137*** (0.048)	-0.0701** (0.031)
Wealth Control		✓				✓		
N	7,894	7,894	3,806	4,088	7,818	7,818	3,751	4,067
R^2	-0.024	-0.012	-0.159	0.006	-0.162	-0.059	-0.413	-0.131
First-Stage F	11.52	11.18	5.25	7.97	11.32	10.98	5.21	7.92
Hansen J p	0.62	0.48	0.49	0.73	0.21	0.53	0.24	0.24

Standard errors clustered on a 0.01-degree (≈ 1 km) grid of DHS cluster coordinates in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (two-sided). Weight-for-height enters as a log-transformed z-score (rescaled by subtracting its sample minimum and adding one); weight-for-age enters in z-score units. All regressions follow the main IV specification for height-for-age (Table A.3, column 2): bleaching exposure is instrumented with summed degree heating weeks over the 12 months before and the 24 months after birth; controls are the number of reef pixels and 12-month averages of wind speed, precipitation, mean air temperature and sea surface temperature (mean, squared, minimum and maximum) for the year before birth and the first and second years after birth; and all regressions include birth-year, birth-month and DHS-cluster fixed effects. Wealth IV columns add DHS wealth-quintile dummies; Poor IV and Rich IV columns split the sample by whether the average wealth quintile of the DHS cluster is below or above the sample mean of 2.7. First-Stage F reports robust Kleibergen–Paap rk Wald F-statistics. Hansen J p-value reports the p-value of the Hansen J test for overidentification of all instruments (1 degree of freedom).

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