
The limits of ecological decodability of a geospatial foundation-model embedding: a registered evaluation of AlphaEarth over shallow Great Barrier Reef habitats

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This is version 2 of this preprint. Version 1 appeared under the title “What can satellite embedding fields tell us about shallow coral-reef habitats? A decodability survey of AlphaEarth over the Great Barrier Reef”; the same study, restructured around its central finding. Supplementary sections §§14–§S20 and supplementary figures S1–S2 carry material condensed from the v1 main text, and a numbering note in the supplement maps v1 section citations onto this version.

The limits of ecological decodability of a geospatial foundation-model embedding: a registered evaluation of AlphaEarth over shallow Great Barrier Reef habitats

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Abstract

Annual satellite embedding fields compress a year of multi-sensor observation into one vector per 10 m pixel, worldwide. We test what ecological information the AlphaEarth embedding holds about shallow (< 10 m) Great Barrier Reef habitats, and where its limits lie. Every candidate output was registered with a pass gate before testing, evaluated under one fixed protocol, and reported pass or fail. Ground truth spans 104,668 photo-quadrat images at one reef over seven years, 67 further reef surveys in two eras, and independent point labels and map products. Benthic state is broadly decodable: eight channels pass at 10–100 m, led by hard coral (held-out p .73 at 10 m, .80 at 50 m); six replicate on never-trained reefs. A persistence control shows this skill mostly reads stable habitat structure: an annually refreshed structure map, not a continuous ecological monitor. Year-over-year change is decodable only in the two thermal-stress event years, only at 20–50 m, and only at the reef with change ground truth (coral-loss AUC .83 in 2024); quiet-year change sits below the noise floor. Post-registration tests against two independent monitoring archives bound deployment: cover levels transfer at a quarter to a third of inter-programme agreement, but decoded trajectories fail with a directional bias (recolonised surfaces read as coral, so gains rank below chance). Ground-truth reliability caps decodability near p .8; naive cross-validation inflates skill by up to +.28. The catalogue and its limits define what these embeddings can and cannot yet contribute to reef monitoring, and how geospatial foundation models should be evaluated against field ecology.

Keywords: satellite embeddings; coral reefs; benthic mapping; Great Barrier Reef; change detection; validation

1. Introduction

Coral reefs are changing faster than monitoring programmes can measure them. Mass thermal-stress events now recur at intervals shorter than reef recovery times (Hughes et al., 2017, 2018): the 2024 event reversed five years of coral-cover gains on the Great Barrier Reef, hitting the southern sector hardest (Ceccarelli et al., 2026) and causing catastrophic

bleaching even inside strictly protected zones near this study's core reef (Byrne et al., 2025). Management depends on knowing where reef condition is changing, but in-water monitoring is sparse: the largest long-term programme on the Great Barrier Reef surveys on the order of a hundred reefs out of roughly three thousand (Sweatman et al., 2011; AIMS, 2025), and most of the world's reefs are never surveyed at all.

Satellite remote sensing fills parts of this gap in two established ways (review: Hedley et al., 2016). Thermal products such as Degree Heating Weeks predict stress exposure, but exposure is not outcome: they indicate where bleaching is likely and cannot report what died (Liu et al., 2014; Skirving et al., 2020). Optical mapping classifies the seafloor, exploiting the separability of coral, algae and sand spectra through shallow water (Hochberg and Atkinson, 2003; Hedley et al., 2018): the Allen Coral Atlas provides global benthic and geomorphic maps (Lyons et al., 2020; Kennedy et al., 2021; Roelfsema et al., 2021a), and Sentinel-2 and Landsat time series have detected individual bleaching events and multidecadal cover trends at well-studied sites (Yamano and Tamura, 2004; Diederiks et al., 2025, at the same core reef used here). But each such time series is a bespoke per-site product. No operational satellite product delivers annual, guild-level, change-capable reef information as a general layer.

Geospatial foundation models offer a different route. Embedding fields, exemplified by AlphaEarth Foundations (Brown et al., 2025), compress a year of multi-sensor satellite observation (optical, radar and thermal inputs) into a fixed-length vector per 10 m pixel, produced globally and released annually. The embedding is not a thematic map: it is a learned summary of the year's sensor record, and whatever information those sensors carry about the shallow seafloor is potentially present in the vector, extractable with a linear read-out (a simple regression) trained against field data. Embedding fields have begun to be exploited on land (Brown et al., 2025; Rahman, 2026); we are aware of no published or preprinted marine-benthos application as of August 2026.

For a general-purpose representation like this, the useful scientific question is not whether it can predict some ecological variable somewhere: with enough candidate outputs and enough sites, something will correlate. The useful question is where decodability ends. This paper therefore measures the limits of ecological decodability of the AlphaEarth embedding over shallow (< 10 m) Great Barrier Reef habitats: what ecological information can be recovered, at what spatial and temporal scales, under what transfer conditions, and where the method fails. Three design principles serve that goal: *registration* (every candidate output was registered with its mechanism, test and pass gate before the confirmatory testing; §2.1 states the exploratory-phase qualification), *one protocol* (every candidate faced the same validation battery, so passes and failures are comparable), and *negatives are results* (failures are reported with the same statistical detail as passes, because the boundary of decodability is the finding).

The resulting map of limits has a two-regime structure, with its evidence organised in explicit tiers (Table 2). Benthic state is broadly decodable, but a persistence control shows that this skill mostly reflects stable habitat structure rather than year-specific condition:

the embedding delivers an annually refreshed map of persistent benthic structure, one of the study's central findings rather than a caveat. Year-over-year change is decodable only in thermal-stress event years, only at 20–50 m aggregation, and, as tested, only at the one reef with multi-year change ground truth. Post-registration tests against two independent monitoring archives bound deployment: cover levels transfer at reduced skill, while decoded trajectories fail with a directional bias. Alongside the reef catalogue, the survey yields methodological results for evaluating any geospatial foundation model against field ecology: quantified cross-validation leakage, annotation-scheme effects larger than the ecology, and a measured decomposition of the skill ceiling. Sections 2–4 present the data and protocol, the catalogue and its limits, and the implications.

2. Materials and methods

2.1 Study design

The study is a decodability survey with three steps applied identically to every candidate output (Fig. 1). Each candidate was *registered* before testing: what the output measures, the mechanism by which an annual embedding could carry it, the test, the pass gate (fixed in advance as cluster-robust $p < .01$, detection ≥ 30 % sensitivity at 10 % false-positive rate, or a cluster-bootstrap CI excluding the null, per output type) and the expected outcome. Every candidate was then *tested* under one fixed validation protocol (§2.6), applied without modification. Third, the full catalogue is *reported*, confirmed to refuted: negatives are published as limits with the same statistical detail as positives. One qualification applies: the register formalises expectations developed in a prior exploratory phase on the same core site, so independent confirmation comes only from the tests on data unavailable to that phase (cross-reef, cross-era, and the independent map-product and point-label comparisons); the full qualification is stated in supplement §S20.

The study domain is shallow (< 10 m depth) coral-reef habitat of the Great Barrier Reef, Australia: 22.7 million candidate 10 m pixels GBR-wide (94.6 % of the mapped GBR10 Coral/Algae class; $\approx 2,270$ km², one benthic class within the $\sim 10,600$ km² of shallow hard substrate reported for the same map product by Roelfsema et al., 2021a). This figure describes potential reach only; all claims are bounded to the ground-truthed reefs. The core training and change-analysis site is Heron Reef (Capricorn–Bunker group, southern GBR), a high-latitude (23.4° S) lagoonal platform reef adjacent to a long-running research station, chosen for photo-record density, not representativeness. Cross-reef validation spans 67 further reef surveys from two habitat-mapping campaigns: 38 genus-resolved survey tables covering 35 named reefs (2017) and 29 reefs (2019), per Table 1. Cross-reef counts in this paper count these survey units, which are also the clustering units for inference (§2.8). The campaigns are a convenience sample, so transfer is measured along one south-to-north, cross-programme axis, not over the reef population at large.

2.2 Satellite embeddings

The predictor throughout is the AlphaEarth Foundations annual embedding field (Brown et al., 2025): a 64-dimensional unit-norm vector per 10 m pixel per calendar year (2017–2025 used here). The model’s inference-time inputs are Sentinel-2 optical, Landsat 8/9 optical and thermal, and Sentinel-1 C-band radar; a wider set of sources enters training as reconstruction targets (Brown et al., 2025, their Table S1). The embedding does not expose which inputs carry which information, so non-benthic input families (sea-surface roughness from radar, temperature from thermal) could in principle contribute to any signal reported here. The Sentinel-2 ablations (§2.6) bound what plain optics deliver and a Sentinel-1-alone baseline bounds the radar family from outside, but no per-sensor ablation of the embedding itself is possible without retraining the upstream model; mechanistic readings in the Discussion are therefore flagged as interpretations.

The layers used are the public Google Earth Engine collection `GOOGLE/SATELLITE_EMBEDDING/V1/ANNUAL`, generated with model version 2.1; the published evaluation describes version 2.0, so all skill measured here is a property of the distributed v2.1 layers. Embeddings were retrieved from the collection’s public cloud-optimised distribution (Source Cooperative) as per-reef windows, dequantised with the provider’s scaling and re-normalised; every exact grid, source URL, retrieval timestamp and hash is in the deposit manifest (§2.10). For state analyses, per-pixel features are the 3×3 neighbourhood mean of the embedding, so the 10 m tier is a 10 m grid with ≈ 30 m effective support at Heron and in the cross-era tests; cross-reef windows are evaluated on raw single-pixel embeddings (full smoothing notes in supplement §S20). A raw-embedding rerun bounds the smoothing choices, moving event-year skill by $\leq .08$ and leaving the 2024 coral-loss axis unchanged.

2.3 Ground truth

Six ground-truth sources play fixed roles (Table 1); acquisition detail, per-image normalisation and label→channel mappings are in supplement §S6 and §S20.

Table 1. Ground-truth sources and their roles.

Source	Extent	Years	Annotation	Role
Heron Reef photo-quadrats (Golding et al., 2026; via ReefCloud, AIMS, 2024)	104,668 images, 37 sites, one reef	2019–2025, annual Oct–Nov	point count	training; all change analysis
PANGAEA photo-transects 2017 (Roelfsema et al., 2018a,b)	35,720 images; 38 survey tables (35 named reefs); 8,965 pixels	2017	CoralNet, genus-resolved	cross-reef transfer; reef-identity floors
PANGAEA photo-transects 2019 (Roelfsema et al., 2022a,b)	37,592 images; 29 reefs; 7,237 pixels	2019	CoralNet, different scheme	pooled transfer (source-demeaned only); macroalgae condition
Heron photo-transects (Roelfsema et al., 2018c, 2021b)	~1,600 pixels per year	2017–2018	point count	cross-era test (never-trained years and method)
Reef Life Survey point labels via Squidle+ (RLS and IMOS, 2026; Edgar and Stuart-Smith, 2014; Edgar et al., 2020)	3,447 points; 101 pixels	2018–2019	third, separately operated programme	independent spot-check (confirmatory for sand)
Allen Coral Atlas benthic + geomorphic maps (Allen Coral Atlas, 2022; Lyons et al., 2020; Kennedy et al., 2021)	~322,000 Heron window pixels	2020 epoch	independent map product	map agreement (\$2.9)

Twelve registered state channels were computed per image: hard-coral cover, *Acropora*-complex fraction, branching-guild fraction, sand, turf/epilithic algal matrix (EAM), standing-dead coral, rubble, crustose coralline algae, macroalgae, soft coral, a reef condition index (live coral / (coral + dead + rubble + macroalgae), computed where the denominator $\geq 10\%$), and morph-bin entropy (Shannon entropy over coral morpho-taxonomic bins, not species diversity). Three definitional dependencies are stated up front: at Heron, turf/EAM is the exact sum of standing-dead and rubble; the *Acropora* complex is largely a subset of the branching guild; and the condition index contains hard coral in both numerator and denominator. The channels are correlated by construction (pixel-year Spearman: coral-branching .95, turf-standing-dead .97; full matrix in supplement §S3), so channel counts in this paper are not counts of independent ecological signals. The *Acropora* complex is a morpho-taxonomic construct, not the genus (genus-labelled morphs contribute 32 % of channel cover, and the cross-reef target is a related but not identical morph set, so its transfer results are transfer of the complex; §S4).

Images were georeferenced onto the embedding grid; a *pixel-year* is the mean of all images falling in one 10 m cell in one survey year, retained when supported by ≥ 2 images (13,351 pixel-years; the min-1-image variant, 16,119, is the n-sensitivity check). The full path from photographs and satellite years to the two read-outs is sketched in Fig. 2. Consecutive-year pixel pairs (3,056 transitions across six year-pairs) form the change ground truth. Surveys are austral-spring (October–November) snapshots while embeddings integrate calendar years, so an event-year composite averages pre-event, event and post-event months: this attenuates the measured change signal and is the stated power limit of the bleached-cover control (§3.3). Four off-epoch campaigns are excluded by the October–November epoch rule and named for auditability: the March–May 2024 peak-bleaching surveys (10,970 images; the designated peak-season-control data, specified before that control was run), August 2024, March 2025 and June 2025.

Auxiliary data. GBR30 bathymetry (Beaman, 2017) defines the < 10 m domain (datum and resolution notes in supplement §S9); depth strata in the confound controls use the survey programme’s recorded site depths (reef flat < 3.5 m). NOAA Coral Reef Watch Degree Heating Weeks (NOAA Coral Reef Watch, 2018; Liu et al., 2014; Skirving et al., 2020) identify event years, auditable at the site: maximum annual DHW at Heron is 7.6 in 2020 and 11.4 in 2024 against ≤ 0.8 in every other study year, including 0.5 in 2022, when the GBR-wide event spared this sector (table S12). The split follows the conventional DHW ≥ 4 threshold with no discretionary cases. Annual Sentinel-2 L2A composites provide the optical baselines; cyclone exposure and crown-of-thorns pressure (§3.4) use the Bureau of Meteorology tropical-cyclone database (Bureau of Meteorology, n.d.; Courtney et al., 2021) and AIMS manta-tow data (AIMS, 2017). Every dataset, role and access route is in the data-availability statement; per-run dataset consumption is recorded in the released ledger (§2.10).

2.4 Decoding models

Simple decoders were used throughout. Continuous channels use ridge regression on the standardised 64-dimensional features ($\alpha = 10$; a four-order-of-magnitude α sweep moves held-out skill by $\leq .011$). Class outputs use one-vs-rest ridge scoring evaluated by AUC. No deep models, feature engineering or hyperparameter search: the survey measures what the embedding carries, not what an optimised estimator can extract. Skill is Spearman rank correlation on held-out data (ρ); detection results use a *detection score*, the sensitivity at a fixed 10 % false-positive rate for the ecologically relevant quartile: a discrimination summary, not an operational detection rate (a training-fixed-threshold variant was measured for both regimes: state thresholds transfer, change thresholds do not; §3.3, §S17).

2.5 Spatial scales

Every test was run at six scales: 10, 20, 30, 50, 70 and 100 m. Blocks of $k \times k$ pixels were formed on the embedding grid with fixed tiling; block ground truth is the mean of member pixel-years and the block feature the mean embedding, requiring ≥ 2 ground-truthed pixels per block (support sensitivity in §3.2). Each channel is reported at every scale rather than at its best scale.

2.6 Fixed validation protocol

Every candidate passed the same battery. *Spatial holdout*: plain leave-one-site-out cross-validation (LOSO; 37 named, spatially separated sites) is the headline holdout: every headline skill comes from a model that never saw the test site. LOSO controls site identity, not spatial proximity, so spatial isolation is measured by a 600 m buffered LOSO variant reported alongside the headline values (§3.2); channels whose apparent skill is spatial leakage fail under it. *Spatio-temporal holdout*: site + year double holdout (train on other sites in other years only).

Confound controls: depth-stratified re-tests; a site-identity floor (a predictor given only each site's mean, isolating within-site signal from between-site ranking); label permutation; and a Sentinel-2 ablation against three baselines (plain median composite, multi-temporal percentile stack, and a red-edge/SWIR-widened variant; constructions in §S16).

Robustness: regularisation sweep, minimum-image sensitivity, site jackknife, ± 1 -pixel georeferencing jitter ($\leq .007$), a nonlinear-decoder check (gradient boosting vs ridge under identical buffered folds), a buffer ladder (600–1800 m), and the persistence control (previous-year, next-year and time-averaged embeddings).

Generalisation: leave-one-reef-out replication across the 38 and 67 reef-survey units (§2.1); a reef-identity floor; prevalence screening; cross-era transfer (2019–25-trained decoders on 2017/18 ground truth). Change-specific attacks are in §2.7.

A channel is reported at a tier only if it passes the gates under the full battery; conditional channels are labelled with their condition, and a condition derived post hoc from the same

data is either demoted to the supplement (rubble prevalence, §S11) or labelled post hoc with confirmation pre-registered (macroalgae abundance, §3.4).

Several elements were added during revision as falsification-side attacks: deliberate attempts to break the registered claims (the buffer ladder, persistence control, threshold-transfer tests, maximum-power quiet attack, bleached-cover controls and the extra baselines among them). Attacks that produced new descriptive numbers are reported outside the confirmatory family with their own confidence intervals, flagged as post hoc wherever they appear; none raises any registered claim above the level its registered test earned. The complete list is released with the supplement (§S12) so the two analysis generations are separable.

2.7 Change analysis

Change decoding operates on the embedding difference vector $\Delta e = e(\text{year } b) - e(\text{year } a)$ for consecutive-year pixel pairs, demeaned within each transition (removing the year-pair mean shift, which carries tide, water and sensor conditions). The demeaning is transductive and its support is the ground-truthed pixel set, so the decoder estimates change relative to the average shift across the surveyed pixels; deployed change maps should be read as contrasts within the mapped extent.

Signed per-guild change decoders were fit by ridge on Δe and evaluated by leave-one-transition-out cross-validation. The record contains exactly two event transitions, so every event-transition number is a one-event-to-one-event transfer: conservative rather than leaky, and generalisation from a single training event. Two summary axes were fit the same way: a *coral-loss axis* (trained on event-transition coral loss; functionally a substrate-conversion detector, since the target is live-cover loss) evaluated as a loss detector, and a *recovery axis* (trained on one rebound transition, tested on the other) evaluated as a gain detector.

Event gating was tested, not assumed: every change output was evaluated separately on the two thermal-stress event transitions (2019→20, 2023→24, identified by DHW) and the four quiet transitions (specificity nulls), with site-demeaned re-tests and a maximum-power quiet attack bounding any residual quiet-year signal from above (§3.4). Because the ground-truth scheme labels bleached colonies separately, a bleached-cover channel was differenced alongside coral as a control on the coral-loss axis (cover loss or whitening; §3.3). Both gate-opening events are thermal; behaviour under other acute disturbances is untested, and all change products are scoped to thermal-stress event years.

2.8 Statistical inference

No naive p-values are reported anywhere. All inference is by cluster bootstrap: $B = 2000$ resamples over the clustering unit that carries the dependence (37 sites at Heron; 38 or 67 reef-survey units in cross-reef tests, §2.1; spatial blocks where no site structure exists), with 95 % percentile confidence intervals and a one-sided cluster p of $(k+1)/(B'+1)$, where B' is the number of valid resamples. The smallest reportable p is therefore $\geq .0005$. Sub-

analyses on fewer than ~15 clusters are flagged as approximate. AUCs use the same procedure around a Mann–Whitney estimate. Ground-truth reliability was measured directly by split-half correlation with Spearman–Brown correction, and headline skills are additionally reported disattenuated for it (construction in supplement §S19). Holm correction was applied post hoc within each channel × test family across the scale ladder and is reported strictly as a family-size disclosure (the bootstrap p-floor times family size caps adjusted values, so claims at the floor cannot fail by construction; table in the supplement, §S2).

2.9 External map agreement

Allen Coral Atlas benthic and geomorphic polygons were rasterised onto the embedding grid. Agreement was tested decoder-side (do this study’s decoders separate Atlas classes?) and embedding-side (are Atlas classes directly decodable under 200 m spatial-block cross-validation?), both scored by cluster-robust AUC. One lineage fact is disclosed: the embedding model’s training-site sampling drew on Atlas-derived reef extent (Brown et al., 2025); the Atlas informed *where* the model sampled, not what it was trained to predict, so Atlas agreement is between independently produced products with shared sampling ancestry, not fully independent validation.

2.10 Software and reproducibility

All analyses run from one shared Python library, so every run shares identical joins and preprocessing; the released reproduction pack includes a self-contained companion library implementing the same conventions. Every analysis script carries its registration in its header (released verbatim as REGISTRATION-HEADERS.md in the reproduction pack), writes a per-run log, and appends structured result rows to a machine-readable ledger from which the results tables are assembled mechanically.

The reproduction scripts are released at <https://doi.org/10.5281/zenodo.22065301>, and the full ledger is deposited with the supplement (<https://doi.org/10.5281/zenodo.22064963>). The pack recomputes the cross-reef transfer, reef-identity floor, cross-era and threshold-deployment legs end-to-end, and executes the public-test-data results from released decoder weights. The remaining public-data analyses are traceable row-by-row in the ledger; the Heron-restricted tables additionally require the custodian data.

Because the Earth Engine collection can be revised by the provider (the v2.0→v2.1 change has already occurred), the exact embedding windows used are archived at <https://doi.org/10.5281/zenodo.22065348> (CC-BY 4.0) with a manifest of per-window source URLs, retrieval timestamps, georeferencing and SHA-256 hashes. Every result therefore remains reproducible against its exact inputs. Trained decoder coefficients and calibrated thresholds are released as derived parameters, valid for the archived v2.1 windows they were trained on. Analysis code was implemented and run with the assistance of AI coding tools (Cursor, with the Fable 5 and Grok 4.6 models); all analyses

were designed, registered, reviewed and interpreted by the author, who takes full responsibility for them.

2.11 Post-registration validation campaign

After the registered analysis and its revision rounds were closed, a further campaign of 36 runs tested the catalogue’s external transfer and temporal limits against datasets the registered study never touched. These were: the AIMS Long-Term Monitoring Program manta-tow series (528 reef-years at 184 reefs, 2017–2023, plus the AIMS dashboard modelled series through 2025) and fixed-site photo-transect archive (306 sites at 102 reefs, 2017–2023), Allen Coral Atlas class packs at seven never-trained reefs, NOAA DHW rasters, an eReefs water-quality climatology, and the GBR10/GBR30 domain rasters. Each run pre-stated its design and pass gates in its script header before results existed; inference follows §2.8 with reefs (or sites) as clusters; external site read-outs average decoded cover over square patches (50–510 m) centred on the published site coordinates.

Campaign results carry their own confidence intervals, are never promoted into the registered confirmatory family, and are Holm-corrected within each round: every p-carrying campaign number quoted survives the correction, failing rows are labelled leads, and the full sweep plus a map from each quoted number to its certified row are released with the supplement (tableS16_holm_campaign.csv, tableS17_quoted_campaign_map.csv). Summary in §3.5; run-by-run detail in §S13.

3. Results

Conventions: correlations are held-out Spearman ρ ; p-values are cluster-bootstrap (§2.8; the floor $p = .0005$ means no resample crossed the null); quantities bounded by one (ρ , AUC, p) are written without a leading zero, other quantities keep it; confidence intervals from the registered and robustness tiers are shown in parentheses, post-registration campaign intervals in square brackets (§3.5); LOSO = leave-one-site-out, DH = site + year double holdout; detection = sensitivity at a fixed 10 % false-positive rate (§2.4).

3.1 Overview and evidence structure

The findings rest on four tiers of evidence, kept separate throughout: registered confirmatory tests (gates fixed before testing, §2.1); robustness and falsification attacks added at revision (descriptive, never promoted, §2.6); post-registration external validation against archives the study never touched (§2.11); and exploratory leads, withheld under the study’s post-hoc rule with confirmatory tests pre-registered. Table 2 maps the headline findings onto these tiers.

Table 2. Headline findings by evidence tier.

Finding	Tier	Verdict	Where
State catalogue: 8 of 12 registered channels	Confirmatory	Pass; six transfer to never-trained reefs and eras (two more channels pass at the training reef only and are not catalogued; §S1)	§3.2
Depth and geomorphic-zone layers	Confirmatory	Pass, near ceiling	§S14
Event-year change (coral-loss axis, 20–50 m)	Confirmatory	Pass, both thermal events, calibration reef	§3.3
Quiet-year change	Confirmatory	Fail at every scale and lag (bounded negative)	§3.4
Recovery (coral-gain) detection	Confirmatory	Fail; externally invalidated, directional bias	§3.4–3.5
Leakage, buffering, persistence, strata, ablations, thresholds	Robustness / falsification	State skill survives; macroalgae and soft coral fail (leakage); state skill is mostly stable structure	§3.2
Transfer and trajectories vs two AIMS archives (36 runs)	Post-registration external	Levels transfer at reduced skill; trajectories fail	§3.5
Nonlinear quiet residual; rubble prevalence; loss-side composite	Exploratory leads	Withheld; confirmation pre-registered	§3.4, suppl.

One reading rule applies to every count in this paper: the “eight validated channels” are catalogue entries, not eight independent ecological signals. Several channels are definitionally linked (§2.3), and the held-out *predictions* of the coral-guild block correlate at .97–.99 (supplement §S3), so the catalogue spans fewer effective axes than channels; a partialling analysis shows they are nonetheless not a single latent axis (§3.5).

3.2 Benthic state channels

Of twelve registered channels, eight are catalogued at one or more scales under the full protocol, and six of these validated beyond the training reef. Two further channels (crustose coralline algae and morph-bin entropy) pass their gates at the training reef only, with the weakest effect sizes; they do not clear the battery's generalisation legs, are not catalogue entries, and are reported in the supplement (§S1). Skill at the core site (13,351 pixel-years, 37 site clusters, 7 years) rises steadily with block size for every channel (Fig. 3; Table 3). Every cell shown has a cluster CI excluding zero (cluster $p \leq .002$, most at the .0005 floor; Holm disclosure per §2.8).

Table 3. State-channel skill (held-out Spearman ρ).

Channel	10 m (LOSO/DH)	50 m	100 m	Cross-reef, 38 survey tables (10→100 m)	Cross-era 2017 (10/50 m)
Hard coral	.73/.57	.80/.62	.84/.64	.55→.60	.76/.84
<i>Acropora</i> complex	.74/.66	.81/.71	.84/.74	.45→.46	.69/.78
Branching guild	.70/.58	.78/.63	.82/.66	.51→.52	.68/.77
Sand	.69/.66	.73/.70	.74/.71	.61→.69	.71/.75
Turf/EAM	.63/.49	.72/.56	.75/.58	(no comparable class)	—
Standing- dead coral	.61/.47	.69/.55	.73/.57	.33→.41	.53/.64
Condition index (live- coral ratio; §2.3)	.63/.43	.70/.46	.73/.47	.51→.54	.68/.79
Rubble	.48/.42	.54/.48	.57/.51	no unconditiona l transfer (lead: §S11)	.23/.30

Robustness. Under the 600 m buffered LOSO variant the core channels retain their skill (coral .66/.72, *Acropora* .69/.74, sand .68/.72, turf .57/.65 at 10 m / 50 m), while macroalgae (.01–.02) and soft coral collapse and are treated as leakage-confirmed at the training reef (macroalgae re-enters conditionally; §3.4). Further attacks are in supplement §S17: the buffer ladder (600–1800 m; Fig. 4b), a habitat-composition control (coral holds at .71/.82 restricted to the Atlas Coral/Algae class vs .73/.80 in the full domain: within-

habitat gradation, not habitat-class separation), depth-strata re-tests (no catalogue channel is reducible to a depth map), and detection-score and threshold-transfer detail. A genus-strict *Acropora* target decodes at .56/.71 (10/50 m) versus .74/.81 for the complex, confirming the channel is a morpho-guild complex, not genus *Acropora* (§2.3). Standing-dead coral and rubble are both EAM-covered surfaces, yet only standing-dead transfers cross-reef unconditionally; neither is a live-coral inverse (pixel-year correlations .27, -.28, -.31; candidate carrier in §4.1).

Generalisation. The channels were stress-tested along four axes. (i) *Space*: leave-one-reef-out replication across the 67 reef surveys (source-demeaned pooled p .34–.54 for the core channels). Pooled numbers include between-reef ranking. The pixel-level quantity to quote is the *reef-identity floor* (Fig. 4d): the median within-reef p on never-trained reefs, which runs from .16 (*Acropora* complex) to .47 (sand), with hard coral at .30. That is roughly half the within-programme skill for the strong channels, and it is the number any no-local-ground-truth deployment must be read against (§4.2). Sign tests and reef-resampled intervals harden the floors (hard coral positive in 35 of the 38 survey tables, $p = 3 \times 10^{-8}$; the *Acropora* floor is weakest, 26/38, $p = .017$, below the study's $p < .01$ convention, and is read accordingly; full set §S12, table S2).

(ii) *Time and programme*: decoders trained on 2019–2025 photo-quadrats predict 2017/2018 photo-transects (never-trained years, a different survey method, the earliest embedding years) at p .53–.84 for coral guilds, condition, standing-dead and sand. (iii) *Independent annotation programme*: a third-programme point-label spot-check, run entirely on public PANGAEA-trained decoders, confirms sand (+.48, CI +.05 to +.70; 13 clusters, approximate); coral is directionally right but under-supported ($n = 101$ pixels).

(iii) *Independent map product*: the sand decoder agrees with the Allen Coral Atlas at AUC .962 over 322,000 pixels, and the embedding reproduces every mappable Atlas class at AUC .87–.97 (200 m spatial-block cross-validation; shared-ancestry disclosure, §2.9). An ablation control bounds what this certifies: plain Sentinel-2 nearly ties on sand (.958) and beats the embedding on the optically derived Coral/Algae class (.94 vs .66), so Atlas agreement is a consistency check for optically recoverable channels, not embedding validation (§S16).

The skill ceiling. Split-half reliability of the photo-quadrat ground truth is .78–.93 for most channels; correcting for it moves hard coral from .73 to .78 and *Acropora* from .74 to .80, so even against noise-free ground truth decodability would top out near $p \approx .8$ at 10 m (approximate; construction and figure in §S19). The remaining gap is signal limitation, not sampling noise. A gradient-boosted decoder under identical buffered folds gains only +.04–.05 p on the coral guilds and $\leq .015$ elsewhere: a real but small nonlinear component, so simple decoders are not the binding constraint.

Ablation. Replacing the embedding with Sentinel-2 baselines under identical folds costs skill on every channel except sand. Against a plain four-band annual median composite the embedding adds +.14 to +.27 p ; against the strengthened multi-temporal baseline it

still adds +.07 to +.16; and against the red-edge/SWIR-widened variant the increment holds at +.08 to +.11 (paired cluster CIs excluding zero) on every catalogue channel except rubble (+.04, spans zero) and sand. Sand runs the other way (embedding .69 vs S2 .73–.77): a brightness signal readable without the embedding, retained as a product channel but not evidence for the embedding method. A Sentinel-1-alone baseline decodes the catalogue channels only weakly (p .08–.26), and adding radar to the strong optical baseline moves every catalogue channel by under +.03 p , so the increment over optics is not radar-carried (constructions, per-channel increments and figure: §S16).

Persistence structure. A feature-swap control bounds what the cross-sectional skill means (Fig. 4c): fed the previous year's, the next year's, or the 2019–2025 mean embedding instead of the matching year's, the coral decoder still reaches p .74–.75 against .76 same-year on the common subset, and sand is indistinguishable across all four. Cross-sectional state skill is therefore dominated by persistent between-pixel habitat structure rather than year-specific state. This is a central finding of the survey: the state product is an annually refreshed habitat-structure map, and the year-specific information of the annual layer is concentrated in the event-gated change regime (§3.3, §3.4).

Scales. Skill increases monotonically with block size, so the product architecture is a design choice rather than an optimum: a 10 m drill-down tier ($\approx$ 30 m effective support), a 50 m primary tier (LOSO .54–.81) and 100 m roll-ups. The site-identity floor quantifies the meso-scale caveat: for the coral guilds nearly all pixel-level skill is between-site ranking plus year tracking (floor increment +.01–.02 p at 10 m), whereas turf and standing-dead carry genuine within-site skill (+.17–.21). Coarse-tier support is sparse (blocks need only $\geq$ 2 ground-truthed pixels; full-support restriction costs .04–.07 p), so coarse-tier skills predict sparse-pixel aggregates, not exhaustive block means.

3.3 Change channels

Year-over-year change decoding operates under a strict regime split. In the two documented bleaching-event transitions (2019→20, 2023→24), the signed change in hard coral, branching guild and condition index is recoverable from the embedding difference vector at p .36–.64 depending on scale and event (leave-one-transition-out; both events cluster $p < .01$ at multiple scales). Turf and standing-dead change join at 20–30 m only (full per-channel, per-scale grid with marginal cells daggered: supplement table S3). With two events in the record, each event number is a one-event-to-one-event transfer (§2.7). Because of the definitional links of §2.3, the five passing channels reduce to two effectively independent findings, a coral-loss axis and a substrate-conversion axis (§4.1).

In the four non-event transitions the same decoders flag nothing at or below the false-positive budget, but the specificity structure is informative (Fig. 5a). The two rebound transitions (2020→21, 2024→25) score *below* chance on the loss task (AUC .29–.35; the axis reads first-year regrowth as anti-loss; §3.4), while the two true-quiet transitions sit at or just below chance (AUC .38–.49; detection 3–13 % against the 10 % budget, so below-chance pooled AUCs reflect the rebound component, not false alarms). Two scale bounds

define the operating band: at 10 m, site-demeaning collapses signed skill to .04–.09, so the 10 m signed map ranks sites, not pixels (genuine within-site signal appears only at the block tier, demeaned .13–.25 at 50 m); at 100 m the event gate itself leaks. Change products are therefore valid at 20–50 m only, in thermal-stress event years.

Within that band, the coral-loss axis discriminates ≥ 10 -point loss blocks in the 2024 event at 20 m at AUC .83 (cluster CI .69–.92); its detection sensitivity at the 10 % false-positive budget is estimable only to a wide site-clustered interval (13–85 %), against 3–13 % in non-event transitions. The two events are not symmetric: 2019→20 passes the registered event gate at 10 m and 50 m (AUC .65/.73, cluster $p < .01$) and is under-powered at 20 m (AUC .66, $p = .07$). Alarm thresholds do not transfer between events: a threshold fixed on one event either collapses sensitivity (16 % on 2024) or exceeds the false-positive budget (39 % realised FPR on 2020) on the other (Fig. 5b). The change product is therefore a *ranked triage map* (top-quantile flagging within the new layer; Fig. 6), not a fixed-threshold alarm. State thresholds do not share this problem: a hard-coral alarm threshold transfers cleanly within the training programme; beyond it the false-positive rate drifts, and a label-free per-reef recalibration restores the budget but not detection (§S17).

Two bleached-cover controls show the axis reads realised cover loss rather than whitening. Conditioning on surveyed bleached fraction leaves the loss association unchanged at survey time (2023→24 partial $p = -.60$ vs $-.60$ raw). The same holds in the designated peak-season control on the March–May 2024 surveys, where whitening is present (20 m partial $-.36$, cluster CI $-.58$ to $-.13$; reverse partial spans zero; full cells and power limits, table S14). The task definition is not load-bearing: sweeping the loss threshold and false-positive budget degrades the 2024 result gracefully (AUC .80–.85), with no knife-edge at the registered choices.

Change in sand is undetectable everywhere (event CIs rule out $p > .24$ in 2019→20 and $> .13$ in 2023→24 at 10 m), and the unsigned change screener generalises to only one of the two event types; both are negative results with measured upper limits. Within event transitions, combining the coral-loss axis with a rebound-trained recovery axis separates coral-gaining from coral-losing pixels at AUC .672/.729 (both cluster $p \leq .006$ at 10 m; Fig. 5c): a bounded demonstration that the difference vector carries directional information beyond change magnitude (detail in supplement §S15).

3.4 Limits and conditional channels

One channel carries an explicit validity condition, and one transfer lead is held in the supplement. Macroalgae is eliminated by a 600 m spatial buffer at the training reef, where it is rare, but transfers on the 29 reefs where it is abundant (leave-one-reef-out $+ .41$, p at the bootstrap floor; median within-reef $p = +.31$ with 28 of 29 reefs positive, sign-test $p = 6 \times 10^{-8}$). It is an abundance-conditional channel, not a full pass: the training-reef buffer failure stands. The abundance rule was derived from these data and is labelled post hoc, with confirmation pre-registered; all 29 reefs come from a single southern-GBR campaign (one region, year and annotation programme), so generalisation awaits a second campaign.

Rubble fails naive cross-reef transfer (+.05); a prevalence-conditioned re-test recovers a signal but rests on 9 reef clusters, below the study's own cluster-reliability line, so it is a supplement-only lead (§S11).

Four negative results bound the instrument.

- (i) *Quiet-year change is undetectable* at every scale, in all four non-event transitions, and at every quiet temporal separation in the record (lags of one to three years), with trend estimators faring no better (§3.5). This is an embedding-side limit, not a ground-truth power limit: quiet-year ground-truth deltas are themselves measurable (split-half reliability .35–.55), yet decoders trained on them find nothing. Any undetected quiet signal is small: pooled across the four quiet transitions, the upper 95 % confidence limit falls below $p = .16$ at 10 m for every guild (most below .09) and below .26 at 20–50 m (estimation bounds, not a formal equivalence test).

A maximum-power attack (decoders trained only within quiet transitions) still finds nothing under the registered linear protocol (pooled p .01–.02, every CI spanning zero): the quiet null is a property of the registered linear read-out. A nonlinear variant under the same folds recovers at most a small residual inside those bounds; it emerged from a revision-round search and is withheld under the post-hoc rule, with its confirmatory test pre-registered and the full search grid published (supplement §S5).

- (ii) *Recovery (coral-gain) detection did not validate*: continuous rate prediction fails (p .05–.16, n.s.), rebound-era detector transfer is unreliable, and the post-registration campaign closed the direction externally, with true gains ranking below chance (§3.5).
- (iii) *Genus-level channels could not be evaluated on the 2019 export*. The *Acropora* complex collapses to +.01 within that source while transferring at +.19 from the genus-resolved 2017 tables, and injection experiments show random label noise cannot produce this failure, so the annotation errors of that export are systematic. The finding is scoped to this export and construct: both archives are CoralNet-annotated (§2.3), so the failure attaches to this export's label scheme and training, not to machine annotation as such, and total-cover and substrate channels are robust to the same labels.
- (iv) *Reef-scale attribution from window-mean change is not supported at the grain tested*. Across 7,611 window-transition pairs (~960 2.56 km reef windows, 2017–2025), thermal-stress dose, crown-of-thorns pressure and cyclone exposure show no within-site association with window-mean embedding change that is robust to the choice of demeaning. The crown-of-thorns join is far smaller, so that null is weaker evidence. The null is at whole-window aggregation, far coarser than the 10–50 m tiers where the decoded signals live; a per-pixel loss-axis re-test partially revises the thermal side (§3.5).

Two methodological results carry general relevance. A naive random-pixel train/test split inflates apparent skill by $+0.10$ – 0.28 ρ and up to doubles detection rates relative to spatially separated cross-validation (Fig. 4a), replicated beyond the training reef (pooled ρ inflated by $+0.07$ to $+0.23$ on the cross-reef set, every channel). And pooling ground-truth sources with different annotation schemes inflates substrate-channel skill to 0.75 – 0.82 through pure source contrast; all pooled results here use source-demeaned inference, and per-source decoders outperform pooled training wherever the ground truth is clean.

3.5 External validation and temporal limits (post-registration campaign)

A campaign of 36 runs after the registered analysis closed (§2.11; run-by-run detail in supplement §S13) tested the catalogue against external monitoring archives it had never touched. Campaign numbers carry their own confidence intervals, sit outside the confirmatory family, and survive the campaign's Holm sweep unless quoted as leads or characterisations.

State levels transfer at reduced skill; calibration does not. At 289 never-trained LTMP fixed photo-transect sites (1,047 site-years, 102 reef clusters; coordinate screen in §S13), the Heron-trained coral decoder reads site-level cover at $\rho +0.25$ – 0.26 across 50–510 m patches, and at $+0.376$ [$+0.205$, $+0.513$] within mapped reef-slope geomorphology. Manta-tow reef windows carry $+0.17$ – 0.20 . The attenuation is genuine transfer loss: the same protocol at Heron scores $+0.869$, and the two AIMS survey instruments agree with each other at only $+0.712$ on levels, so external skill is roughly a third of that inter-programme agreement at site grain and a quarter at window grain.

An elimination battery removes coordinate jitter, patch size, water depth (skill holds to ~ 9 m), survey timing, water clarity and training breadth as explanations. Leave-region-out passes in both directions, and skill persists with every training reef at least 200 km away ($+0.373$ [$+0.235$, $+0.485$]; §S13). Absolute units do not travel: internal maps are nearly unbiased (slope 0.94), but externally the slope falls to 0.29 with a $+14.5$ -point over-prediction, so off the training programme the product ranks sites rather than measuring them. The ranking still adds information: at 930 site-years with a mapped GBR10 class, the annual index predicts measured cover at $+0.320$ [$+0.185$, $+0.433$] where the static habitat-class fraction carries none (-0.020), though the two products differ in kind.

Trajectories fail externally, with a measured directional bias. Transition-demeaned deltas and multi-year trends track the surveys at neither external grain (manta deltas -0.10 , CI below zero; per-reef trends -0.43 [-0.59 , -0.23]; fixed-site deltas spanning zero), and reefs or sites that truly gained ≥ 10 points of coral rank *below* chance (gain AUC 0.42 – 0.44). The failure belongs to the read-out, not the reference: the two survey instruments agree with each other on the same deltas ($+0.538$) and trends ($+0.657$). A pre-stated test at Heron identified the mechanism: given true cover, the coral decoder over-predicts where turf-covered standing-dead skeleton is present (partial $\rho +0.398$ [$+0.172$, $+0.575$]). The companion pre-stated change-course gate failed, so the mechanism is measured in the post-event cross-section at the training reef and inferred elsewhere (§S13). No combination of

catalogue channels rescues the gain direction; one succession-corrected composite (coral – turf) makes external site-level loss detection significant (AUC .635 [.549, .710]), a bounded lead. Decoded trajectories are directionally biased rather than merely noisy: apparent gains should be distrusted wherever local change ground truth is absent.

Reef-scale change detection is bounded, at the calibration reef only. At Heron, site-mean indices track the two event transitions pooled at $p + .868 [+ .76, + .92]$. The implied minimum detectable change is about 14 true points of coral cover at site scale and about 7 at whole-reef scale (derivation in §S13), at any speed the record can test: the quiet floor is flat across measured lags, so a slow decline surfaces once it accrues past the floor. The scope is strict: run without ground truth at 508 external reef windows, the same estimator shows quiet-year reef-mean swings near 15 points, consistent by elimination with a per-year whole-window offset, and no 2023→24 event contrast (§S13). The reef-scale bound does not generalise without local calibration.

Event-regime scope. The coral-loss axis outperforms plain state differencing at the severe event (paired AUC difference $+ .068 [+ .022, + .132]$), and temporal generalisation is clean: decoders trained only on past years read a never-trained year at undiminished skill, including across the unseen 2024 event, with macroalgae the named exception. The axis footprint tracks the spatial thermal-stress gradient in moderate DHW years ($p + .19-.28$ across ~500 reefs; year-by-year cells and one disclosed low-variance anomaly in §S13) but saturates in the severe 2023→24 event (median flagged fraction .87, nothing left to rank). Windows more than about a year past an event decay toward baseline: detection has a roughly one-year shelf life. Cyclone-driven structural loss remains untested.

Two further results bear on the catalogue. Decoded sand agreement with the Allen Coral Atlas extends to all seven never-trained test reefs across ~10° of latitude (AUC .64–.85, every CI > .5; one window re-cut, disclosed in §S13). And a partialling analysis shows the channels are not one latent axis: every non-derived channel keeps skill after the coral prediction is removed (partials $+ .19$ to $+ .80$), with branching the exception, re-scoped as a coral-nested correlate.

4. Discussion

4.1 The limits of decodability: a two-regime instrument

The central result is a regime structure, not a single skill number. In the *state regime*, a general-purpose annual embedding carries enough information about the shallow benthos to support guild-level cover mapping: linear read-outs recover hard-coral cover, guild fractions, substrate classes and a condition index on held-out sites, years and reefs, surviving depth stratification, spatial buffering, and transfer across programmes and eras. But the persistence control (§3.2) shows what that skill is made of: mostly stable between-pixel habitat structure, not year-specific condition. The state product is best described as

an annually refreshed map of persistent benthic structure, with some additional information about large event-related changes, rather than a continuous ecological monitor.

In the *change regime*, the same embedding supports year-over-year inference only when an acute event intervenes, only at 20–50 m aggregation, and, as evidenced, only for the two thermal-stress events at the one reef with multi-year change ground truth; quiet-year change sits below the noise floor of the registered linear protocol at every scale and lag, a limit of the read-out rather than of the ground truth (§3.4).

The regime boundary has a physical reading, offered as the most parsimonious interpretation (the embedding also ingests non-optical sensors and per-sensor attribution is beyond a decodability survey, §2.2). An annual embedding averages a year of observation, so it measures the persistent state of a pixel well and its within-year trajectory poorly: slow compositional drift is smaller than the year-to-year noise of the composite, while an event-scale substrate conversion (live coral to standing dead structure to turf) changes the persistent character of the pixel and is readable.

Consistent with this, the signed change decoders behave as substrate-conversion detectors: they track realised cover loss rather than whitening, read rebound-year regrowth with inverted sign, and find nothing where no conversion occurred (§3.3). This is also the two-axis reduction promised in §3.3: the correlated coral-guild channels (hard coral, branching guild, condition index) read the coral-loss side of the conversion, while the definitionally linked turf and standing-dead channels read the same conversion from the substrate side, so the five passing change channels carry two effectively independent findings, not five. The same logic suggests why standing-dead coral transfers across reefs while rubble does not (standing structure persists across a compositing year; rubble is mobile): supported by an optical probe but confounded with wave exposure, so it remains an interpretation (supplement §S18).

The external campaign adds the third behaviour: a *succession artifact*. Given true cover, the coral decoder over-predicts where turf-covered standing skeleton is present, so decoded trajectories run optimistic after loss and rank true gains below chance at never-trained reefs (§3.5); algal turf recolonises standing structure within a compositing year, restoring a coral-like optical character long before the ecology recovers. Together, the persistence control, the event gate and the succession artifact define the instrument's temporal limits: annual structure, event response, and nothing trustworthy in between.

4.2 Implications for reef monitoring

The catalogue translates into a bounded set of map products: 10 m drill-down, 50 m primary and 100 m roll-up tiers for state, and a 20–50 m event-response tier for change. The state tiers should be read as maps of largely static habitat structure, annually re-observed, whose value is coverage and consistency rather than year-to-year sensitivity.

Extension beyond surveyed reefs is measured, and each number must be quoted as what its test measured. Cross-reef transfer is mostly reef ranking: within a never-trained reef, a decoder trained elsewhere ranks pixels at roughly half the within-programme skill for the strong channels (the reef-identity floor, §3.2), and off the training programme the product ranks sites rather than measuring them (absolute calibration does not transfer, §3.5). The measured envelope covers the kinds of reef the transfer tests sampled: to ~9 m depth, across regional holdouts, with all training reefs 200 km away. The ranking still adds information over a static habitat map (§3.5), and a label-free per-reef recalibration restores a deployed alarm's false-positive budget, though not its detection (supplement §S17). None of this supports mapping without ground truth: the conditional channels need exactly the local field data that skipping ground truth would save.

The event-response tier is a ranked triage map from the coral-loss axis: once the annual layer covering a thermal-stress year is released, the axis ranks ≥ 10 -point loss blocks at 20 m (AUC .83 in 2024; the 2020 event passes its gate at 10 and 50 m, and its 20 m estimate of .66 is under-powered, §3.3). Alarm thresholds do not transfer between events, so the deliverable is a ranking within the new layer, and rebound-year layers must not be run through the loss axis at all (§3.3).

The campaign sharpens the tier: the axis outperforms plain differencing, tracks the thermal-stress gradient in moderate years but saturates in a severe one, and decays within about a year of the event, so the niche is planning the following field season, not rapid response (annual layers publish months after the composite year closes). Both validated events are thermal; cyclone- and predation-driven loss are untested. At calibrated reefs, reef-mean aggregation adds one bounded capability: resolving about 7 true points of change at whole-reef scale at any speed; at never-trained windows the per-year offset raises that floor roughly five-fold (§3.5).

The conditionality results are as operationally important as the skills: rubble maps do not transfer beyond the training reef; macroalgae maps are valid only where macroalgae is abundant, on the evidence of one campaign; and no quiet-year change product should be issued at any scale. A programme adopting embedding-based mapping needs the catalogue's conditions, not just its headline correlations.

The embedding neither replaces optical mapping nor in-water monitoring; it sits between them. It reproduces the Allen Coral Atlas classes (AUC .87–.97) and adds annual revisit, continuous cover channels and event response; it adds measured skill over plain, multi-temporal and red-edge/SWIR-widened Sentinel-2 baselines on every catalogue channel except sand, with the increment not radar-carried (§3.2), so the supported claim is the measured increment, not “only embeddings can do this”. And the dependence on field programmes runs the other way: every decoded channel exists only because photo-quadrat ground truth trained it. Embedding-based mapping is a multiplier on field programmes, not a substitute, and archived photo-quadrat data gains new value as training data.

4.3 Evaluating foundation-model embeddings against field ecology

This study is, in the end, an evaluation template for geospatial foundation models in a real environmental setting, and several of its findings apply to any pairing of learned satellite representations with field data. First, the binding constraints are physics and ground truth, not models: ridge regressions on 64 features were sufficient throughout (learning curves plateau by a quarter of the training data; the nonlinear gain is real but small, §3.2), and measured ground-truth reliability places the internal ceiling near $p .8$. The external analogue is sharper: two independent survey programmes agree with each other at only $p .71$ on cover levels, and external decoding reaches roughly a third of that agreement, while the trajectory failures have no such excuse (the programmes agree on change where the index reads nothing, §3.5).

Second, four methodological hazards were quantified, each large enough to change conclusions (§3.4–3.5). Spatial leakage: a naive random-pixel split inflated skill by up to $+.28 p$ on identical data, replicated externally; studies reporting embedding decodability under random splits are reporting leakage. Annotation schemes: pooling ground truth across programmes inflated substrate skill through pure source contrast, and one export’s systematic labels destroyed a genus channel while leaving totals intact; treat ground-truth source like site, as a clustering unit to demean and hold out. Clustered inference: several channels significant under naive p -values fail under cluster bootstrap. And trajectory screening: decoded state indices were directionally biased on post-disturbance reefs at every external grain tested, consistent with succession restoring a surface’s optical character before its ecology; any satellite index of ecosystem state should be screened for this artifact class before its trends are interpreted.

What remains open is sharply defined and data-bounded: a second change-calibrated reef (the reef-scale detection bound is calibration-reef-scoped, and the per-year whole-window offset blocking its generalisation is measured but unexplained); a validated non-thermal event; the single-campaign macroalgae conditional and the rubble prevalence lead; and the succession-corrected loss composite awaiting registered confirmation (§3.5). The survey design (registered predictions, one protocol, reported negatives) is intended to be rerun as that ground truth and each annual embedding layer arrives.

4.4 Conclusions

A global, general-purpose annual satellite embedding, read with linear decoders against photo-quadrat ground truth, supports a validated catalogue of shallow-reef information: eight benthic state channels across 10–100 m scales (fewer effective axes, §3.1), event-gated loss detection at 20–50 m validated at one reef, and near-ceiling auxiliary layers. The equally important product is the map of limits, several of which are the study’s own controls: state skill is dominated by persistent habitat structure; cross-reef pixel skill is roughly half the within-programme number; quiet-year change is undetectable by the registered linear read-out; external cover levels transfer at a quarter to a third of the inter-

programme agreement while decoded trajectories fail with a measured directional bias; and reef-mean change resolution holds only at locally calibrated reefs.

For reef monitoring the practical meaning is bounded: within surveyed reefs, decoding runs near the ground-truth ceiling; on never-trained reefs it is a ranking, not a calibrated measure; in event years a change ranking exists at one validated reef; and every tier remains anchored to the field programmes whose archives train it. For Earth observation, the meaning is that embedding fields carry usable marine information they were not built to expose, and that finding the boundary of that information (what can be recovered, at what scales, under what transfer conditions, and where it fails) requires the effort to go into validation rather than model complexity.

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As this was a desk-based study of archived data held by custodial monitoring programmes, with no new data collection, no Indigenous consultation was sought.

Prescribed data-custodian texts: The AlphaEarth Foundations Satellite Embedding dataset is produced by Google and Google DeepMind. Contains modified Copernicus Sentinel data [2017–2025]. Degree Heating Week data were provided by NOAA Coral Reef Watch. Allen Coral Atlas maps, bathymetry and map statistics are © 2018–2023 Allen Coral Atlas Partnership and Arizona State University and licensed CC BY 4.0. The eReefs datasets (model simulations, satellite or in-situ observations; Baird et al., 2020) and software were produced as part of the eReefs project (ereefs.org.au), which is a collaboration between Australia’s national science agency CSIRO, the Australian Institute of Marine Science (AIMS) and the Queensland Government, with observations obtained through the Integrated Marine Observing System (IMOS) and the Great Barrier Reef Marine Park Authority (GBRMPA). eReefs is funded by the Australian Government’s Reef Trust.

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had no role in study design; collection, analysis and interpretation of data; writing of the report; or the decision to submit the article for publication.

Data availability

All inputs are public except the Heron Island ReefCloud photo-quadrat summaries (available on request from the custodian). The released pack recomputes the cross-reef, cross-era, identity-floor and threshold-deployment results from public inputs; the remaining analyses are traceable in the released run ledger, and the Heron tables additionally require the custodian data. The AlphaEarth embedding windows used are archived at <https://doi.org/10.5281/zenodo.22065348> under the dataset's CC-BY 4.0 licence, and trained decoder coefficients are released with the pack (§2.10). Full source and licence tables are provided in DATA-AVAILABILITY.md, released with the reproduction pack (<https://doi.org/10.5281/zenodo.22065301>).

Code availability

The public reproduction pack (companion analysis library, per-run scripts, and the registration headers) is released at <https://doi.org/10.5281/zenodo.22065301>; the scripts acquire the public datasets directly from their sources. The supplementary material, including the full run ledger, is deposited at <https://doi.org/10.5281/zenodo.22064963>.

Author contributions (CRediT)

A.C.: conceptualisation, data curation, formal analysis, investigation, methodology, software, validation, visualisation, writing — original draft, writing — review and editing.

Declaration of competing interest

The author is founder and director of OSIBOT PTY LTD, a marine robotics and survey company that hosts the author's PhD@Work candidature and in which intellectual property arising from the candidature vests. OSIBOT has no commercial product based on the methods evaluated here. The author declares no other competing interests.

Ethics and permits

Field data were collected by the custodial programmes under their own permits and approvals; this study analyses archived data held by custodial monitoring programmes (publicly available or available on request) and involved no new field collection. No human-participant or animal research was undertaken and no ethics approval was required.

Declaration of AI and AI-assisted technologies in the writing process

During the preparation of this work the author used Cursor, with the Fable 5 and Grok 4.6 AI models, in order to assist in drafting the manuscript. After using these tools, the author

reviewed and edited the content as needed and takes full responsibility for the content of the publication.

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Figure captions

Figures 1–2 are schematics drawn from the published design and headline values (no independent analysis); Figures 3–6 are generated from the confirmatory run ledger or rebuilt with the published pipeline, one script per figure. Supplementary figures S1 (ground-truth reliability and the decoding ceiling) and S2 (optical-baseline ablation) are in the supplement with their captions (§S19, §S16).

A registered decodability survey

every candidate output: registered before testing → tested under one fixed protocol → reported, pass or fail

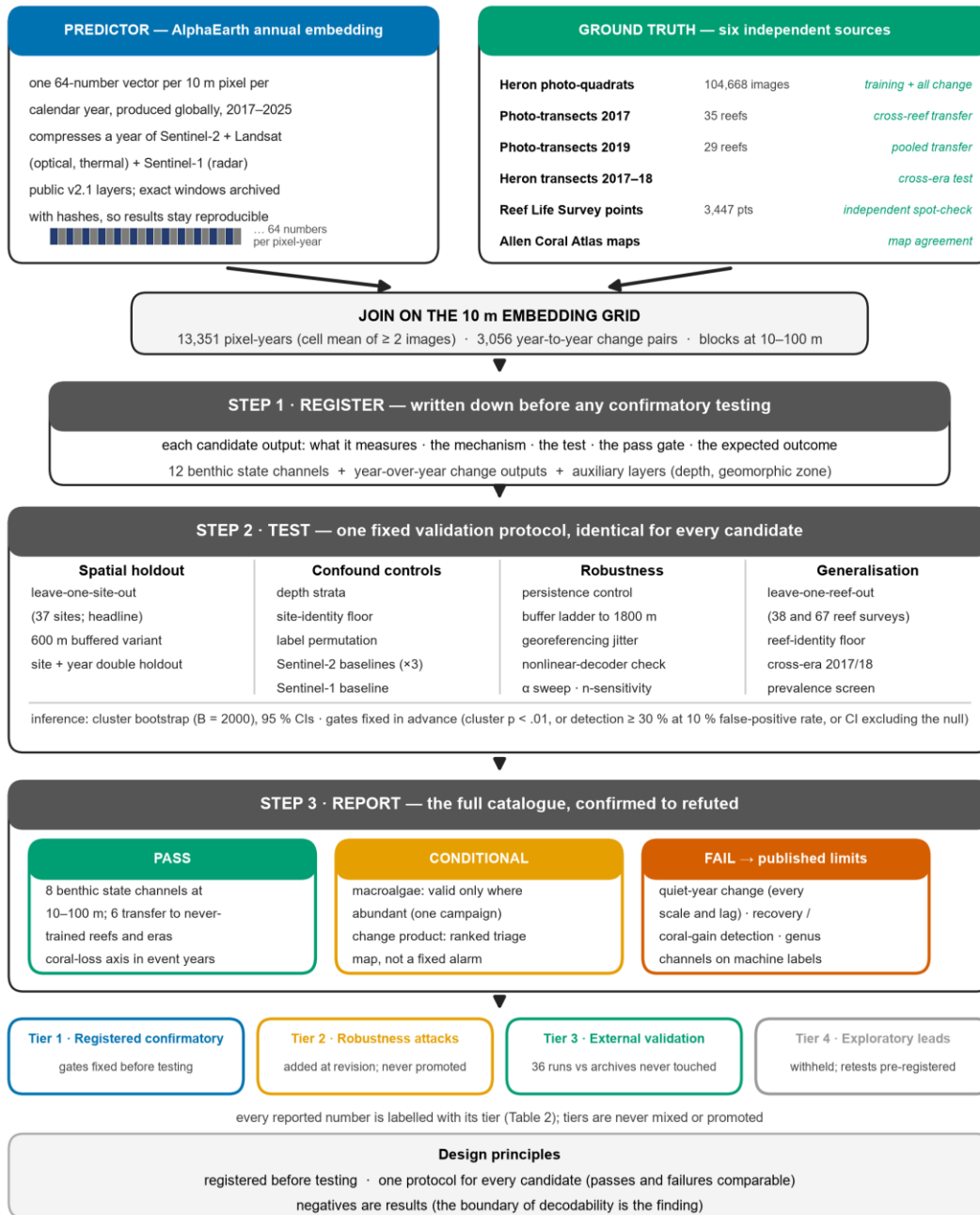


Figure 1. Study design: the registered decodability survey (fig01_study_design)

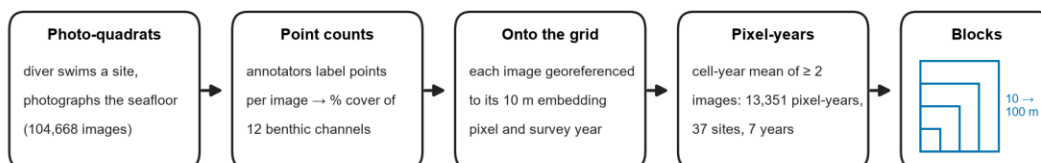
The survey pipeline read top to bottom. Two input streams — the AlphaEarth annual embedding (one 64-number vector per 10 m pixel per year; §2.2) and six independently collected ground-truth sources (Table 1) — are joined on the 10 m embedding grid into

pixel-years and year-to-year change pairs (§2.3). Every candidate output was then (1) registered before testing, with its mechanism, test and pass gate written down in advance; (2) tested under one fixed validation protocol, identical for every candidate (spatial holdout, confound controls, robustness attacks, generalisation tests; §2.5–§2.6); and (3) reported whatever the verdict — pass, conditional pass, or fail (§3.2–§3.4). The bottom strip shows the four evidence tiers used throughout the paper (Table 2); tiers are never mixed or promoted. Counts and gates as published in §2 and §3.

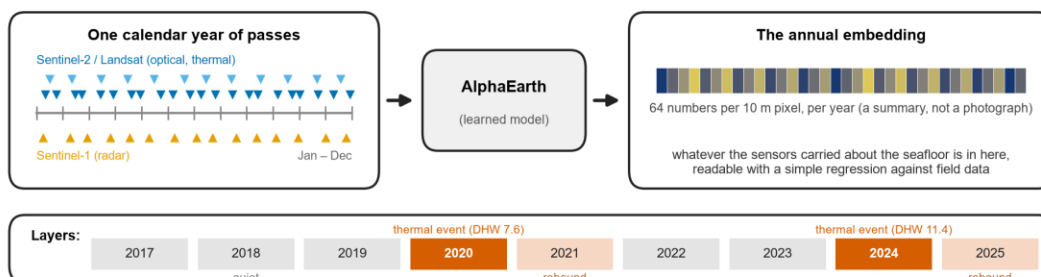
What happens to the data

from reef photographs and satellite years to maps, and where each product stops being trustworthy

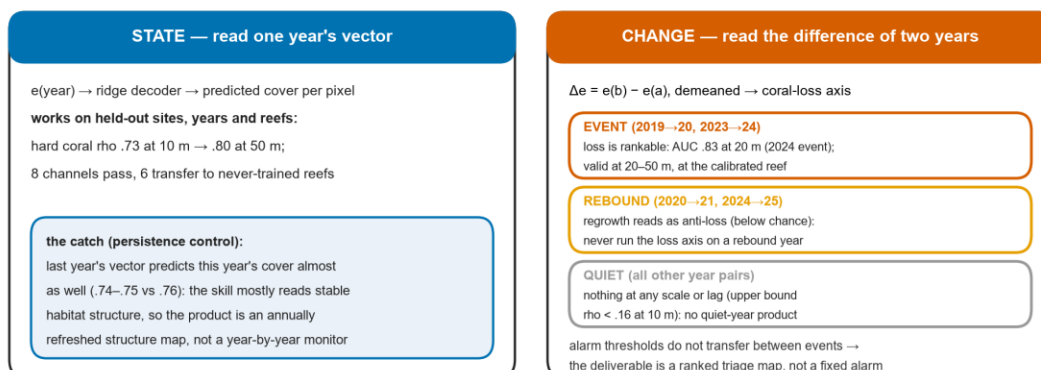
a Ground truth: from photographs to pixel-years



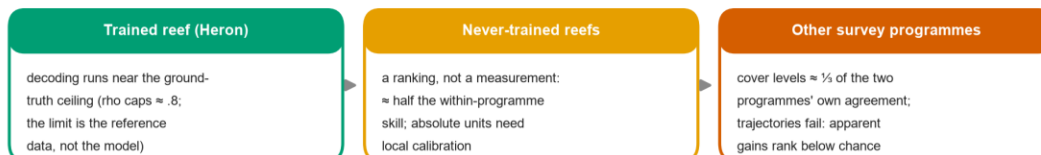
b Satellite: a year of observation becomes one vector per pixel



c Two read-outs, two regimes



d Where the numbers can be trusted



every product tier stays anchored to the field programmes whose archives train it: embedding maps multiply field data, they do not replace it

Figure 2. Data flow: what happens to the data, and where each product stops being trustworthy (fig02_data_flow)

(a) Ground truth: photo-quadrat images are point-count annotated to percent cover, georeferenced to the 10 m embedding grid, and averaged into pixel-years (cell-year mean of ≥ 2 images), then aggregated to blocks of 10–100 m (\$2.3). (b) Satellite: a full calendar

year of Sentinel-2, Landsat and Sentinel-1 observation is compressed by the AlphaEarth model into one 64-number vector per pixel; one layer per year, 2017–2025, spanning two thermal-stress events (2020, 2024), two rebound transitions and quiet years (§2.2, §2.7). **(c)** The two read-outs. *State*: a single year’s vector is decoded by ridge regression into predicted cover; skill is high on held-out sites, years and reefs, but the persistence control shows most of it reads stable habitat structure (§3.2). *Change*: the difference of two years’ vectors feeds a coral-loss axis that ranks event-year loss but reads rebound regrowth as anti-loss and detects nothing in quiet years; thresholds do not transfer between events, so the deliverable is a ranked triage map (§3.3). **(d)** Trust gradient from the calibration reef through never-trained reefs to external survey programmes (§3.2, §3.5). All values are published headlines quoted from §3.

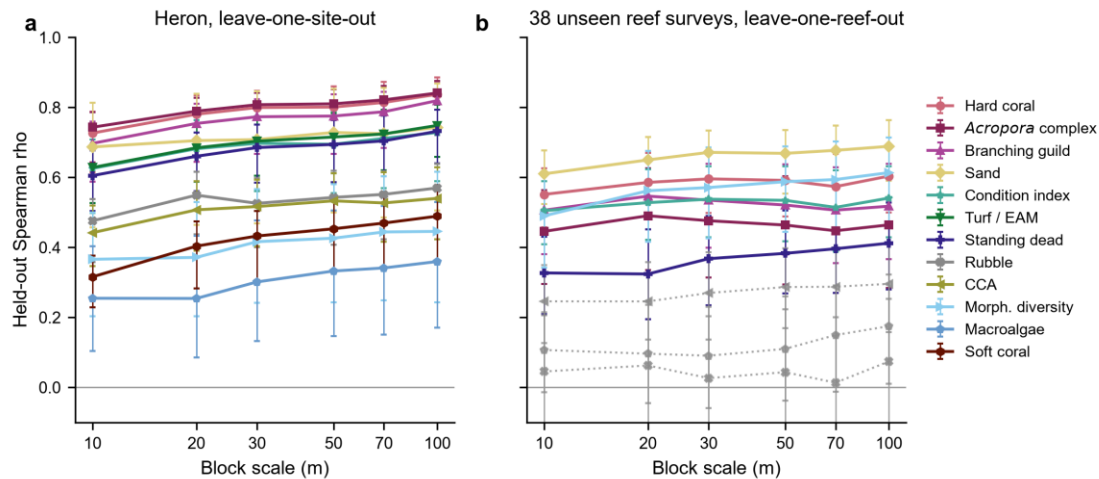


Figure 3. State-channel decodability by spatial scale (fig03_skill_by_scale)

Held-out Spearman ρ for every evaluated state channel at 10–100 m block scales. **(a)** Heron Reef, leave-one-site-out (min. 2 images per pixel-year); whiskers are site-cluster bootstrap 95 % CIs. **(b)** The 38 genus-resolved 2017 survey tables (35 named reefs), leave-one-reef-out; whiskers are reef-cluster CIs. Dotted grey marks the two register channels not carried in the catalogue (crustose coralline algae, morph-bin entropy; §3.2). Pooled cross-reef ρ includes a between-reef ranking component, decomposed in Fig. 4d. Sand is optically decodable and not primary evidence for the embedding method (supplement §S16, Fig. S2).

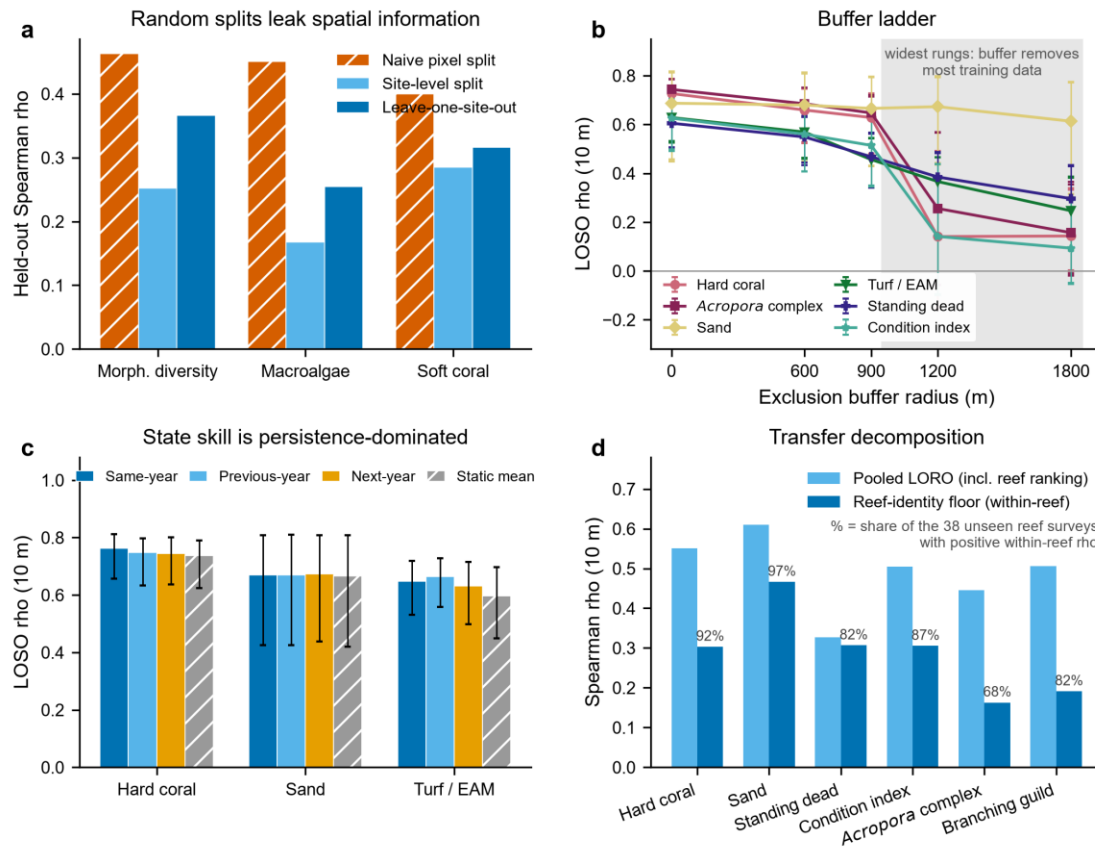


Figure 4. The validation battery (fig04_validation_battery)

(a) Spatial-leakage quantification: a naive random-pixel 75/25 split inflates apparent skill relative to a site-level split and leave-one-site-out on identical data and models (three channels; r_{10}). **(b)** Buffer ladder: 10 m LOSO ρ with exclusion buffers of 0–1800 m; skill is stable to 900 m; the shaded region marks the widest rungs, where the drop reflects domain truncation rather than leakage (r_{30}). **(c)** Persistence control: replacing same-year embeddings with previous-year, next-year or static-mean embeddings barely degrades state skill (r_{34}). **(d)** Cross-reef transfer decomposition: pooled leave-one-reef-out ρ versus the reef-identity floor (median within-reef ρ on never-trained reefs); percentages give the fraction of reefs with positive within-reef ρ (r_{05}).

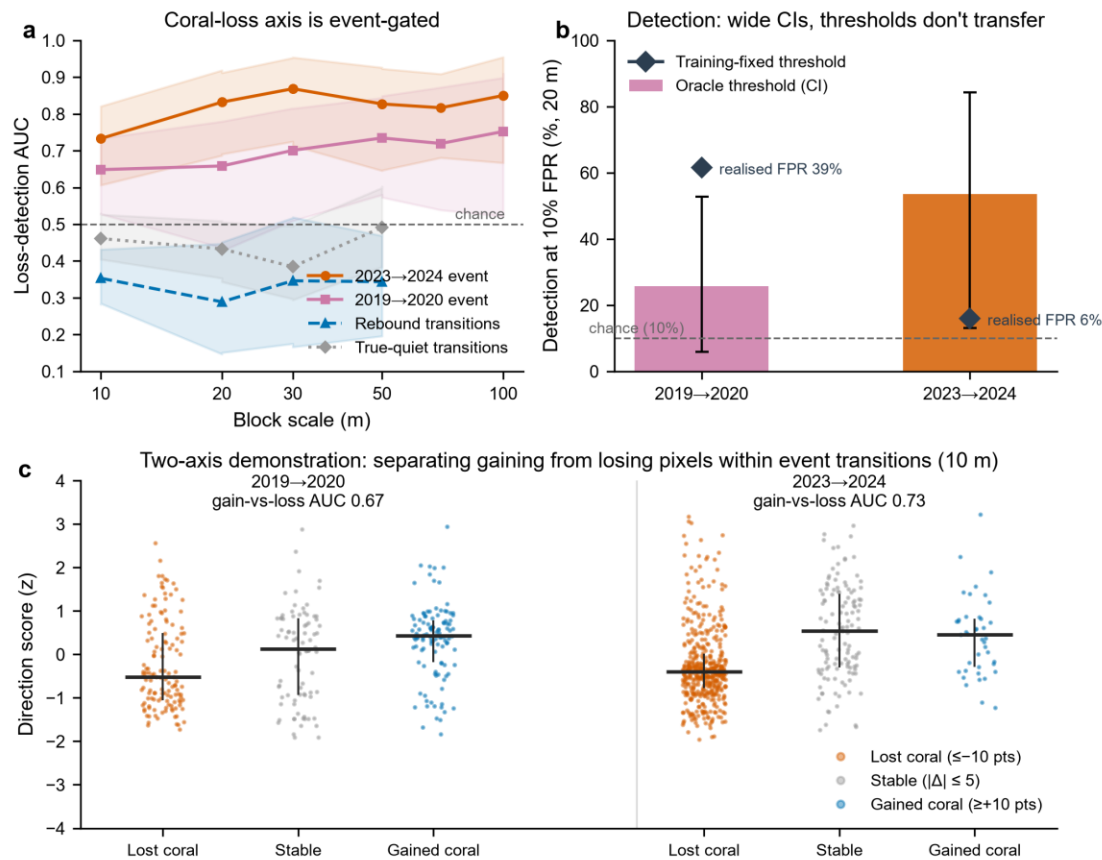


Figure 5. The change regime (fig05_change_regime)

(a) Coral-loss axis AUC (≥ 10 -pt loss vs stable, leave-one-transition-out) by scale, split by transition class: two thermal-stress events, two rebound transitions, two true-quiet transitions; shading = cluster 95 % CIs. Rebound transitions sit below chance: the axis reads direction, not disturbance per se (r03, r27). **(b)** Detection at a 10 % false-positive budget, 20 m: oracle-threshold detection with cluster CIs, and the operating points realised when the threshold is fixed on the other event; thresholds do not transfer (r26). **(c)** Two-axis demonstration at 10 m: combined recovery+loss direction score for pixels that lost (≤ -10 pts), stayed stable ($|\Delta| \leq 5$) or gained ($\geq +10$ pts) hard coral within each event transition (axis training excludes the evaluated pair; ticks group medians, lines interquartile ranges; detail in supplement §S15; r09).

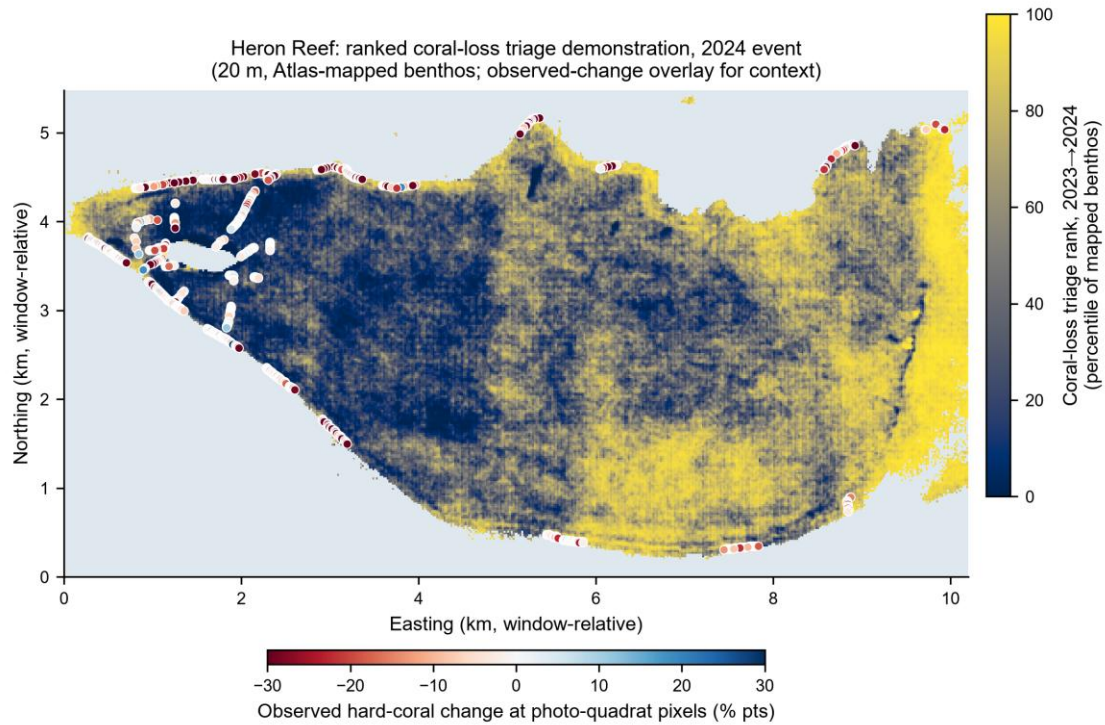


Figure 6. Ranked coral-loss triage demonstration, Heron 2024
(fig06_loss_triage_map)

Coral-loss axis applied to the full Heron Δ -embedding field for 2023→2024, trained only on the other transitions, aggregated to 20 m and expressed as percentile ranks over Atlas-mapped benthos (unmapped/deep water in grey-blue). Overlaid circles: ground-truth pixels coloured by observed hard-coral change (the estimands differ; context, not a validation scatter). Demonstration only: the field is demeaned over the displayed domain, whereas confirmatory metrics demean over ground-truth support (§2.7).