Author response to Referee 1's second comment (RC3)

Manuscript: egusphere-2026-3979, "Zooplankton, phytoplankton and pervasive anthropogenic particles along a zonal North Atlantic transect: a multi-vessel citizen-science image survey"

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We thank the referee for this second reading, for the generous assessment of the pace and scope of AC1, AC2 and the addendum, and for continuing to push the manuscript toward something more defensible than the version first submitted. Several of the points below identify real gaps; we accept those in full and describe the changes made. Two points needed correction on our side rather than the manuscript's, and we say so plainly.

On the sand/dust point

The referee is right, and we were wrong to state, as a blanket rule, that "sand is not a candidate class in an open-ocean surface sample." We wrote that line in AC1 while arguing that a North Sea sediment study's 1.4% microplastic-confirmation rate, driven by quartz sand grains, was not informative for our water. That argument about the North Sea study's relevance stands, but the general claim does not, and NASA Earth Observatory's figure of some 800 million tonnes of Saharan dust crossing the tropical Atlantic every year (Hansen, 2020) confirms dust cannot be dismissed a priori.

We can say more than that, though, because we saw it happen. Passing North Africa on the way to Cape Verde, the sails turned dark brown with Saharan dust. We looked for it under the microscope and found the grains hard to pick out: Saharan dust is round and almost completely clear, unlike the dark, colourless particles our pipeline scores as black carbon. We have added this to Sect. 2.7 rather than leave it as a reply-only claim: the manuscript now states that dust was directly observed during the crossing, that its optical properties (round, near-transparent) are unlike the black-carbon class as defined, so we do not expect it to be a significant contributor to the classes reported here, and that its low contrast against the filter background is instead flagged as a possible blind spot of the imaging method for the next campaign's spectroscopic confirmation to test. This is a more precise statement than either the original blanket claim or a generic "cannot be excluded": we have direct field evidence for what the dust looks like and why it should not be confused with the classes we scored, rather than only a reason it might be.

On reframing as a preliminary study / lessons learnt

Accepted, and implemented. The abstract now opens by describing the work as "a preliminary citizen-science survey" rather than asserting the SML framing as established motivation, and the Conclusion adds a paragraph stating plainly that this is offered as "a preliminary, lessons-learnt first crossing rather than a finished protocol," naming the retained-filter gap as the clearest lesson for the next iteration alongside the flowmeter, extended blanks and condition-scoring commitments already in Sect. 2.7.

The title has, in fact, already been changed in this revision for exactly this reason, and the tracked-changes manuscript shows it: "Near-absence of surface zooplankton and pervasive anthropogenic particles along a zonal North Atlantic transect…" is now "Zooplankton, phytoplankton and pervasive anthropogenic particles along a zonal North Atlantic transect: a multi-vessel citizen-science image survey." The reasoning is worth stating plainly. The near-absence result itself is not fragile: 317 stations, each sampling a full 0.5-litre cartridge draw for a combined 158.5 litres of surface water, returned zero confirmed zooplankton, and an independent surface net tow (0.2–0.5 animals per cubic metre, day and night) corroborates a genuinely thin near-surface stock rather than a small-sample artefact: the two methods agree. We had expected, even knowing the water was oligotrophic, to find more sub-300 µm zooplankton than we did; finding none was the observation that motivated the original title. Where this review has changed our thinking is on what that scarcity actually demonstrates. Nobody has measured what surface-layer (0–1 m) zooplankton density should be in this water, and with no pre-existing baseline there is no basis for treating our result as a departure from a known normal, or for a title that carries the inference that something is wrong. The near-absence we measured may simply be the natural state of the equatorial Atlantic away from upwelling. The revised title therefore just names what we measured, without asserting that any of it is anomalous or that one part explains another; the survey now presents itself as setting a first baseline that future campaigns can validate against, and the hedging added in Sect. 5, Sect. 7.1 and the Conclusion (offering the pollution-grazer link as a candidate mechanism, not a demonstrated cause) carries the same discipline through the text. Phytoplankton has also been added to the title in this round. It was present at 87% of stations and is one of the four particle classes this survey actually classified and reported, so leaving it out of the title while naming zooplankton and the anthropogenic particles understated what the crossing measured.

Specifically on preserving material for re-analysis: the referee's suggestion is right, and we have added it to Sect. 7 (Recommendations) as a stated protocol lesson, retaining at least a sub-sample of the physical filter material recovered as black carbon and microplastic, so that land-based spectroscopic re-analysis is possible after the fact, which this survey cannot offer because the original filters were not kept. We have also now stated why they were not kept, since the reason matters for how the lesson gets implemented: marine plankton samples degrade very quickly, especially at tropical temperatures, and no preservation step was available on board the volunteer vessels. Retention is therefore not simply a matter of deciding to keep the filters next time. Sect. 7 now states that a practical preservation method, most plausibly drying the filters and sealing them in an airtight container, will itself need to be evaluated before retained-filter analysis can become part of the volunteer protocol.

On the GOES sampler's development potential

We agree, and have added this to Sect. 7: we now state explicitly that further development of the GOES sampler itself, extended to a stratified near-surface series and paired with retained-filter spectroscopy, is plausibly the most useful scientific contribution this programme can make to open-ocean pollution monitoring going forward, alongside the standardised monitoring commitment already there.

While revising this point we also tightened Sect. 2.8's account of the net tow itself. The plankton trawl was towed by the lead author alone, as a one-off confirmation that the imaging protocol was not simply missing animals present in the water, and it is not equipment we expect other citizen-science crews to carry or that we intend as part of the replicable multi-vessel method. We have added a sentence stating this explicitly and cross-referencing Sect. 7, so the manuscript is clear that the GOES cartridge, not the net, is the instrument intended for wider volunteer deployment, which is consistent with, and reinforces, the referee's point above.

On finer vertical resolution as a research need

Also added to Sect. 7: we now state the need for much finer vertical resolution of near-surface zooplankton than existing depth categories allow, ideally a stratified series within the top 1 m, as a research need this survey has identified but not answered, independent of whether it is pursued by further citizen-science deployments or by conventional means. We have strengthened the case for that research need with three further points, added to Sect. 2.3 and Sect. 7. First, the near-surface layer is not inherently uninhabitable: neustonic zooplankton do live in the top metre and in the microlayer itself, and the distinct blue carotenoid pigmentation of pontellid copepods is a recognised adaptation to exactly the ultraviolet exposure and visual predation pressure of that niche (Rahlff et al., 2018), so adapted surface taxa exist elsewhere, which makes the near-absence we recorded more informative than a trivial expectation of emptiness rather than less. Second, very few surveys have sampled the top metre of the ocean anywhere, and we are aware of none dedicated to the equatorial Atlantic specifically; the gap RC1 and RC2 both pushed us to be explicit about is a real one, not a rhetorical concession. Third, we now cite the paper that first characterised the sea-surface microlayer itself as a living, gelatinous biofilm rather than a passive film (Cunliffe and Murrell, 2009), alongside the Cunliffe et al. (2013) synthesis already cited, since it is that biological character of the SML, not just its chemistry, that makes biogenic particles there potentially important for air–sea exchange and aerosol formation, and Sect. 7 now states that this makes surveying the top metre and its atmospheric interface a worthwhile objective in its own right, independent of the zooplankton question.

On Hopkins (1982) and the near-surface comparison

Added to Sect. 2.3. We had missed this reference in the surface-comparison literature search behind AC1 and AC2, and the referee is right that it is the closest geographically relevant precedent available: a 0–15 m near-surface category in the eastern Gulf of Mexico showing marked daytime depletion of many mesozooplankton species relative to deeper strata. We have been careful in how we use it, and the revised text now states the limits on the comparison explicitly. First, its shallowest category still integrates 0–15 m rather than the top 1 m this survey targets. Second, the semi-enclosed Gulf of Mexico is a different hydrographic setting from the open equatorial Atlantic: warmer, strongly influenced by terrestrial runoff and, over much of its shelf, by pronounced eutrophication, which carries further physicochemical consequences. Eutrophication-driven respiration lowers pH and aragonite saturation (Cai et al., 2011), and higher organic loading tends to lower both the redox potential of the water (Breitburg et al., 2018) and the zeta potential of suspended particles (Parrella et al., 2024).

We also checked whether the resulting change in micro-viscosity says anything about zooplankton body size in the Gulf of Mexico relative to this survey, since size structure bears directly on whether Hopkins' community is comparable to the sub-300 µm fraction targeted here. The one study we found that measured viscosity effects on zooplankton swimming speed directly (Larsen et al., 2008) points a specific direction: of their three study animals, the smallest (a rotifer) was the most sensitive to increased viscosity and the largest (a calanoid copepod) the least, which is the opposite of a naive size-drag argument. We have added this to Sect. 2.3: if that pattern holds more generally, the Gulf of Mexico's higher micro-viscosity would disadvantage smaller-bodied zooplankton preferentially, shifting its community toward larger body sizes than the near-surface fraction this survey targets, a further, specific reason the two settings are not a like-for-like match. As with the other physicochemical points above, we are careful to state that none of these parameters was measured in either study and we do not know how, or whether, they bear on zooplankton abundance in practice; we offer the viscosity point as a documented, cited possibility, not a demonstrated mechanism. So we take from Hopkins (1982) only the qualitative pattern of daytime near-surface depletion, not any quantitative expectation for the oligotrophic water sampled here. It is a precedent for the pattern, not a benchmark for the numbers; the quantitative comparison of record remains the true-surface (0–1 m) Tara Pacific datasets.

This same reasoning points the other way for the water in this survey. The equatorial Atlantic is open, is cooler, and has lower micro-viscosity than the Gulf of Mexico. If the pattern in Larsen et al. (2008) holds more generally, that lower viscosity should favour small-bodied zooplankton over large ones, the opposite of what we describe for the Gulf of Mexico above. We therefore expected to find more small-bodied zooplankton in this water, not fewer. Finding almost none was not the result we expected, and that surprising result is a genuine concern, not a neutral observation. However, we cannot say it points to one specific cause. We also cannot say it marks a change from an earlier, normal state, because no earlier baseline exists for surface zooplankton density in the equatorial Atlantic. This survey is the first attempt anyone has made to measure that density. We have added this same point to Sect. 2.3 of the manuscript, directly after the viscosity discussion above, so the reasoning is stated in both places.

On predation risk and UV avoidance as candidate mechanisms

Added to Sect. 2.3, immediately alongside the Hopkins citation. We now name both mechanisms explicitly: visual predation risk in the high-clarity water of this transect, and avoidance of damagingly high ultraviolet exposure, which penetrates unusually deep in the clear water of the tropical Atlantic, with the three citations the referee supplied (Johnsen and Widder, 2001; Tedetti and Sempéré, 2006; Al-Aidaroos et al., 2015). Consistent with the co-occurrence-not-causation discipline we have applied throughout this revision, we present both as candidate explanations for future testing rather than as demonstrated causes of the scarcity we observed; this survey was not designed to distinguish between them, or from a genuine absence, or from residual sampling limitations.

On the night/day tow difference

Accepted: Sect. 2.8 previously attributed the roughly twofold higher night-time net-tow density to diel vertical migration without considering the alternative the referee raises. We now state that we cannot rule out passive night-time mixing of animals up from the deep chlorophyll maximum as an alternative to active swimming, since the light gradients relevant to both visual predation and UV avoidance are much reduced after dark and we see no obvious biological reason for purposeful upward migration under those conditions.

On the surface-microlayer and climate material

We accept the criticism and have made the cut. In responding to the first review we stated that the wider implications material had been added because we were asked to broaden the paper beyond a narrow survey report, without being able to document that claim at the time, and it was fair for the referee to press us on it. We can now substantiate it properly. Before this manuscript was opened for public review, the handling editor's own screening decision required exactly that broadening, stating plainly that we needed to spell out what is learnt from the data set and what it implies for our understanding of the ocean, and that a paper confined to reporting the survey itself would be better suited to a data journal than to Ocean Science. That instruction is why the manuscript discusses pollution sources, ecosystem consequences and recommendations alongside the survey itself, rather than stopping at the method and its raw results. It does not, on its own, excuse the specific length or certainty of the surface-microlayer and climate material in Section 4, and we still accept that this particular section overreached on both counts. The broader scope of the paper, however, was not an unexplained addition on our part. Without meeting that condition, the paper would not have been sent out for review.

Ocean Science's own published aims and scope make the same point in stronger terms than the screening decision did. The journal states that it "publishes studies with important implications for our understanding of the state and behaviour of the ocean, its impacts on and responses to climate, including coupling at the ocean–atmosphere, ocean–sea ice–ice sheet and ocean–coast interfaces" (Ocean Science, 2026). This transect sits inside that remit in a way it did not when the manuscript was a narrower survey report. The equatorial Atlantic, and its top metre specifically, has rarely been measured at all, and we are aware of no comparable near-surface survey of this water. What the crossing adds beyond the zooplankton result is a picture of plastic and partially combusted carbon co-occurring in the same near-surface water, at the same stations, in a combination we do not believe has been reported this way before. We then connect that picture to the sea-surface microlayer, whose biological character is already established in the literature (Cunliffe and Murrell, 2009; Cunliffe et al., 2013), and to its documented role in ocean–atmosphere coupling: air–sea gas exchange, aerosol formation and evaporation control. That role sits close to the centre of the journal's own stated scope, yet it remains a surprisingly under-studied part of ocean science given how directly it bears on climate regulation. Our contribution is not a new mechanism; the mechanism is already understood and cited. It is the first evidence that the pollution and the microlayer occupy the same place, in this specific and consequential water. We think this observation opens a line of enquiry that ocean science has rarely pursued.

The cut itself: the abstract's opening paragraph, previously some eight sentences motivating the survey through the microlayer's climate role, is now one sentence stating the theme without asserting the argument. Section 4, previously roughly a thousand words with its own figure (ERA5 water-vapour and cloud-amplitude reanalysis, redox/zeta-potential speculation, the CLAW hypothesis, and named 2022 storm attribution studies), is now two sentences: that the microlayer's suppression of air-sea CO_2 exchange and its contribution to cloud condensation nuclei are measured facts, that its effect on evaporation is an open, testable hypothesis this survey cannot answer, and that we intend to pursue that quantification separately, with dedicated eddy-covariance flux measurements, in a paper of its own. The removed material, namely the ERA5 reanalysis, the figure, and the fuller discussion, is marked as a tracked deletion in the revision rather than silently discarded, so the full prior text remains visible in the redline for the record. This also resolves a loose end from AC1: we had earlier promised that the fuller quantitative treatment would be “a separate paper, which we intend to pursue,” but never actually stated that commitment in the manuscript text itself. It now sits in the two sentences that replace Section 4.

We want to be precise about what we are and are not conceding, because the cut was about proportion, not about whether the underlying mechanism is real. The relationship linking phytoplankton productivity, the zooplankton that graze it and the resulting stability of the surface microlayer is established in the literature, not speculative; Cunliffe et al. (2013), already cited in the manuscript, is a synthesis of exactly that biological control. What is genuinely novel, and to that extent still speculative, is not the mechanism but its application here: nobody appears to have taken these measurements, in this combination, in this water, before. That absence is itself worth naming, because the water in question is not an arbitrary stretch of ocean. This transect crosses the tropical Atlantic’s Main Development Region, the corridor of African easterly waves from which most Atlantic hurricanes affecting the Caribbean and the US Gulf and southeast coasts draw their initial organisation (Goldenberg and Shapiro, 1996). An established mechanism and a specific, consequential body of water that nobody has measured together are, on their own, a good reason to look, even before either party has proof. We have added one paragraph to the Conclusion making exactly this case in the same hedged register as the rest of the revision (established mechanism, unmeasured application, first baseline rather than demonstrated result), without reopening the evaporation, cloud or CLAW material the referee asked us to cut. We think this is a fair middle ground: we accept that Section 4 overreached in length and certainty, and we have cut it accordingly, but we do not think the underlying reason this water is worth studying should be cut along with it.

On Gerdis/Gerdts

No correction needed on our side: the manuscript’s reference list already reads “Gerdts, G.” (2015), which is correct, and the referee’s own typo in the review does not appear anywhere in our text. We are grateful for the flag regardless.

Summary of changes

1. Sect. 2.7: windborne Saharan dust (Hansen, 2020; NASA Earth Observatory) added, correcting the overstated claim in AC1 that sand cannot be a candidate in an open-ocean sample. Direct field observation (sails visibly darkened with dust off North Africa) and microscopy (round, near-transparent grains, unlike the dark, colourless black-carbon class) are both now stated; we do not expect dust to be a significant contributor to the classes reported here, but flag its low optical contrast as a possible detection blind spot.
2. Abstract and Conclusion: explicit preliminary/lessons-learnt framing added; the Conclusion now states this is a first crossing rather than a finished protocol, naming the retained-filter gap as the clearest lesson for the next iteration. Title changed in this revision (tracked): “Near-absence of surface zooplankton…” → “Zooplankton, phytoplankton and pervasive anthropogenic particles…”, on the reasoning given above.

The near-absence result is robust (158.5 L across 317 stations, corroborated by the independent net tow), but with no pre-existing baseline for this water we no longer present it as evidence that something is wrong, only as a first baseline measurement, and the title no longer carries that inference.

3. Sect. 7: filter/sample preservation for land-based spectroscopic re-analysis added as a stated protocol lesson, with the reason the filters were not kept (rapid degradation of marine plankton samples, especially at tropical temperatures, with no preservation step available on board) and the consequent need to evaluate a practical preservation method (e.g. drying and sealing in an airtight container) before retention can join the volunteer protocol.
4. Sect. 7: further development of the GOES sampler itself named as plausibly the programme's most useful scientific contribution to open-ocean pollution monitoring.
5. Sect. 7: finer vertical resolution of near-surface zooplankton (ideally a stratified series within the top 1 m) stated as an identified but unanswered research need. Sect. 2.3 and Sect. 7 further strengthened with: neustonic zooplankton (blue-pigmented pontellid copepods) as evidence the near-surface layer is habitable and adapted to, not inherently empty (Rahlff et al., 2018); the observation that almost no surveys have sampled the top metre anywhere, and none we are aware of in the equatorial Atlantic specifically; and the sea-surface microlayer's characterisation as a living, gelatinous biofilm (Cunliffe and Murrell, 2009), stated as the reason biogenic particles at the air–sea interface are a worthwhile survey target independent of the zooplankton question.
6. Sect. 2.3: Hopkins (1982) Gulf of Mexico near-surface (0–15 m) depth-distribution data added as the closest available geographically relevant precedent, with the limits of the comparison stated explicitly. These include the 0–15 m depth integration versus this survey's top-1-m target, and the Gulf's different hydrographic setting (semi-enclosed, warmer, terrestrially influenced and in large part eutrophic) versus the open oligotrophic equatorial Atlantic, including the eutrophication's physicochemical consequences (lower pH and aragonite saturation, Cai et al., 2011; lower redox potential, Breitburg et al., 2018; lower particle zeta potential, Parrella et al., 2024) and, specifically, higher micro-viscosity's likely size-selective effect toward larger-bodied zooplankton in that setting (Larsen et al., 2008). Used as a qualitative pattern precedent only, not a quantitative benchmark. A further sentence is now added to Sect. 2.3 stating the reverse expectation for our own water: lower viscosity here should favour small-bodied zooplankton, so we expected more of them, not fewer. Finding almost none was a genuine surprise, but it cannot be linked to a specific cause because no earlier baseline exists for surface zooplankton density in the equatorial Atlantic.
7. Sect. 2.3: predation risk and UV avoidance added as candidate explanations for the observed near-surface scarcity (Johnsen and Widder, 2001; Tedetti and Sempéré, 2006; Al-Aidaroos et al., 2015), framed as hypotheses rather than demonstrated causes. (In-text citation year for Tedetti and Sempéré corrected from 2007 to 2006 to match the reference-list entry and the publisher's own record, a transcription slip on our part, caught while auditing the reference list for this response.)
8. Sect. 2.8: passive night-time mixing from the deep chlorophyll maximum added as an alternative to active diel vertical migration for the observed day/night net-tow difference.

9. Abstract paragraph 1 cut from eight sentences to one; Section 4 cut from roughly a thousand words and one figure to two sentences with two references, with the removed material retained as a tracked deletion; the AC1 commitment to a separate, dedicated quantitative treatment is now stated in the manuscript text itself rather than only in correspondence. One new paragraph added to the Conclusion stating that the phytoplankton-zooplankton-microlayer mechanism is established, not speculative (Cunliffe et al., 2013), and that this transect's location in the Atlantic hurricane Main Development Region (Goldenberg and Shapiro, 1996) is part of why the water is worth studying, framed as motivation for a first baseline rather than a demonstrated result. The provenance of the paper's broader scope, previously withdrawn for lack of documentation, is now substantiated: the handling editor's pre-review screening decision required the paper to discuss the data's wider implications rather than stand as a narrow survey report, on pain of being redirected to a data journal instead, which explains the manuscript's coverage of pollution sources, ecosystem consequences and recommendations beyond the citizen-science method itself, though it does not excuse Section 4's specific length or certainty. We have also set that broader scope against the journal's own published aims and scope, which states that Ocean Science publishes studies with important implications for the ocean's behaviour and its coupling with climate; the transect's near-surface measurements of plastic and partially combusted carbon, connected to the already-established biology of the sea-surface microlayer, sit inside that remit and, we argue, open a line of enquiry the field has not much pursued.
10. Sand/dust claim in AC1 corrected as above; no manuscript change required for the Gerdis/Gerdts point, since the reference list already reads Gerdts (2015) correctly.
11. Sect. 2.8: the net-tow's status clarified. It was towed by the lead author alone as a one-off confirmation of the imaging protocol, not equipment intended for the replicable multi-vessel method, and the GOES cartridge is stated as the instrument intended for wider volunteer deployment (cross-referenced to Sect. 7).

We are grateful for this second, equally careful reading. Between RC1, RC2 and this second comment, the manuscript has been substantially strengthened; we think it is now a materially more honest account of what a first citizen-science crossing of this kind can and cannot show.

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