Author response to Referee Comment 2

Manuscript: egusphere-2026-3979, "Near-absence of surface zooplankton 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 a careful and clearly expert reading, and for the opening remark that in situ quantitative zooplankton data are needed. We agree with his principle that more data must not come at the cost of data quality, and we accept several of his points without argument.

Two of his six points rest on descriptions of our method that do not match the manuscript, and one is an allegation about our conduct. We answer each point below. But the referee's underlying challenge, that the image classification could not be trusted, deserved more than argument, and we begin with what we did about it.

We re-analysed the entire image archive from scratch. The pipeline used for the first version of this preprint had not been preserved in a runnable form and its output was not linked to the source images, so its numbers could not be reproduced, by the referee or by us. Rather than defend figures we could not reproduce, we rebuilt the analysis as a deterministic, fully documented pipeline: a curation stage that classifies every one of the 1,495 archived frames as a sample micrograph or not (270 were excluded, with a logged reason for each), detection and sizing of every particle above 20 µm in the 1,225 curated micrographs, classification by stated rules, and individual visual inspection of every object of 250 µm and above. The scripts, the per-image output, the curation list and the station-level tables are published as version 2 of the data record (https://doi.org/10.5281/zenodo.21477682), so every number in the revised manuscript can be reproduced end to end by anyone.

The re-analysis reproduced the particle inventory to within about ten percent (34,609 particles against 51,547; the difference is largely the excluded non-sample frames) and confirmed the pervasiveness of black carbon (92% of stations), microplastic (94%) and phytoplankton (87%). It could not substantiate the zooplankton class: none of the 44 reported animal-class objects could be located in the archived images, and every candidate in the animal size range proved on inspection to be a plastic fragment, debris, an aperture artefact or, in frames the curation stage now excludes, printed matter. **The 44 is withdrawn in the revision. The corrected result is an absence: no zooplankton was confirmed in any filter sample, and the surface zooplankton measurement rests entirely on the net tow, where the animals are stained, photographed and individually identifiable.** The referee's scepticism about the image-based zooplankton identification was, in short, correct, and it has made the paper stronger; the central finding it questioned is now supported by better evidence than before.

0. What this survey is, and what it is not

This is a citizen-science project built around what an ocean-going blue-water yacht can do, with the limitations and the advantages that entails. The protocol is constrained by a small crew, underway, without winches, wire, laboratory space or power for depth-resolved work, and we do not pretend otherwise. Against that, cruising yachts cross water that research vessels rarely visit, on tracks and at times that no funded programme covers, and they do so continuously and at negligible marginal cost.

The project was built around a single instrument, the 0.5 litre GOES cartridge, used by every yacht in every ocean from the high latitudes to the equator. That is a deliberate choice and it is the main advantage of the system. When many vessels sample in many places, a fixed volume and a fixed filter make the counts directly comparable between them, which matters more for a distributed fleet than absolute sensitivity at any one station. The cost is that the volume is fixed, and in oligotrophic water it is small. A protocol that changed gear by water type would give better absolute numbers in each and no comparability across the fleet.

The Continuous Plankton Recorder makes the point for us, and we make it without criticism of that survey. The CPR is not an ideal sampler either. It filters through 270 µm silk, behind merchant ships at service speed, at about 10 m depth, so the smaller plankton pass through it, and so does most microplastic and combustion carbon, which it was never designed to retain or to classify. Its value does not rest on being the best available gear. It rests on ninety years of the same instrument deployed the same way, so that change over time can be read without a change of method confounding it. That is the model we have in mind: the same discipline applied globally, and extended to the particles the CPR was not built to see, the soot and the plastic entering the ocean alongside the plankton. A first crossing cannot deliver that. It can establish whether the instrument and the fleet are capable of it.

The instrument is also not limited to yachts. The same cartridge has been used from piers and from fishing vessels, in rivers, and in rainwater. That range is not incidental to the question. Microplastic and partially combusted carbon reach the ocean from land, by river and through the atmosphere, and they cannot be understood from open-ocean samples alone. A single sampler usable at every point along that path, from rainfall to river to shelf to gyre, yields one internally comparable record of the whole route rather than four records that cannot be set against each other.

There is also a practical reason why this particular layer is under-sampled, and it is not neglect. Ship time is the scarcest resource in oceanography, and monitoring a global surface boundary layer from research vessels is not affordable at the coverage the question needs. It is also not the natural platform. A large hull, its bow wave and its wake disturb the top metre, which is the layer the sample is meant to represent, and microlayer work is for that reason commonly done from small craft launched clear of the ship rather than from the ship itself. A small sailing vessel drawing a single fixed volume is a more practical platform for that layer than a research ship, although, as we accept at point 4, it raises contamination questions of its own. Neither the economics nor the hydrodynamics favours doing this

work from a ship. Nor does the geography. A research vessel cannot be where these samples were taken. The Atlantic Meridional Transect runs a single meridional line once or twice a year, the CPR follows commercial routes, and neither crosses the trade-wind belt zonally. Cruising yachts cross it in numbers, every season, on passages they are making anyway.

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1. Sampling volume

The referee is right that 0.5 litre cannot quantify mesozooplankton in oligotrophic surface water. We agree, and the manuscript agrees. Two things follow that we should have made clearer.

The zooplankton result does not come from the 0.5 litre cartridge. The manuscript states this at line 178: "the zooplankton result comes instead from the surface net tow (Sect. 2.8, Table 2)." Section 2.8 describes a 120 µm mesh net of 0.30 m mouth diameter, towed at the surface at 1 to 2 knots for 10 minutes per haul, straining approximately **43,600 litres, or 43.6 m^3, per haul**, in paired day and night sets. That is within the "tens of cubic metres" the referee gives as the requirement for mesozooplankton, and it is the reason the net tow was run at all.

The first referee made the same attribution. When two expert readers reach the same wrong conclusion, the fault lies with our presentation and not with their attention. **In revision the net-tow method, its mesh and its filtered volume will appear in the abstract and at the first mention of any zooplankton density**, and the cartridge will be described only as what it is, a particle sampler.

The cartridge targets a size fraction the referee's argument does not address. His scaling rule, that volume must rise as concentration falls, is correct, and applied to mesozooplankton it gives the tens of cubic metres he states. The cartridge does not sample mesozooplankton. It resolves everything above **20 µm**, and the fraction that matters here is the one between 20 and 200 µm: copepod nauplii, early copepodites and other small metazoan stages. Essentially every previous measurement of these waters used a net of 200 to 330 µm, which passes that entire fraction. The surface layer of the tropical Atlantic has scarcely been examined down to 20 µm at all.

We expected low counts. We did not expect none. Mesozooplankton are scarce in oligotrophic surface water and we knew that before sailing. What we expected, sampling down to 20 µm, was that the smaller fraction would appear well above what we in fact recorded. Instead the deposited re-analysis confirms **no zooplankton in any curated sample**: 122.5 litres examined at 20 µm resolution across 317 station-level sampling events, and not one animal. This is not a single small sample but the sum of 1,225 of them. **In revision that expectation is stated in the introduction, with the size ranges named**, so that the reader knows which fraction the claim concerns.

The same sample volume detected everything else. This is the point we would ask the referee to weigh, because it is an internal control rather than a comparison with someone

else's ocean. The identical 0.5 litre cartridge, the identical filter and the identical 20 µm size floor classified **34,609 particles across the curated archive**. Phytoplankton were present at **87%** of stations, black carbon at **92%**, microplastic particles at **94%**, with the busiest stations reaching several hundred per litre. All of these are at or near the numbers we expected. Zooplankton were present at **no station at all**.

A sampled volume too small to find zooplankton should also have been too small to find phytoplankton, and it found them at nearly nine tenths of all stations. Whatever explains the absence of animals has to explain why the same water, drawn through the same filter and imaged to the same threshold, yielded abundant counts of three particle classes and none at all of the fourth. Sampled volume alone does not do that.

The plankton trawl was run because of that shortfall, not to confirm it. The obvious explanation for detecting almost nothing was that the cartridge was the wrong instrument, which is precisely the referee's argument. So we put a 120 µm net of 0.30 m mouth diameter in the water and strained 43.6 m^3 per haul, by day and by night. The net returned 0.2 to 0.5 animals per m^3. That is the test his objection calls for, and it was run for his reason rather than against it.

A note on the figures used as our benchmark. His volume requirements, and the expected range he invokes at point 6, are given without a reference, a location, a sampling depth or a mesh size. Those four attributes settle the number completely. Mesh alone spans three orders of magnitude in the same water, from tens of thousands per cubic metre at 20 µm to a few per cubic metre at 330 µm. Depth matters as much, for the reasons set out at point 6 below. Without those four attributes an expected range cannot be evaluated, and we would genuinely welcome the source: if it is a surface measurement at a comparable mesh in comparable water, it is the comparison the literature otherwise lacks, and we would adopt it.

We would add that his stated requirement of several cubic metres for the 20 to 200 µm fraction does not match routine practice, which quantifies that fraction from discrete water samples of one to a few litres rather than from cubic metres of filtered water. If we have misunderstood the basis of that figure we would be glad to be corrected.

2. Missing and inaccessible method detail

Accepted in full, and the inaccessible link is the worst of it. The referee reports an HTTP 403 on the field guide that carries the build details, which means he could not check the instrument at the centre of the paper. That is our fault and it is being fixed. We note that the first referee did reach the same page, since he cites the deployment video hosted on it, so the failure appears to be intermittent rather than a dead address. That is not a mitigation. A specification a referee cannot open when he needs it is a specification we have not published.

In revision the full cartridge specification will appear in the main text rather than in an appendix: dimensions, filter plate geometry, aperture count and diameter, filter medium and pore size, fill mechanism, and the deployment procedure step by step. The

field guide link will be repaired and, because a live URL is not a durable citation, the guide will also be deposited with a DOI so that it cannot break again.

3. Intake geometry and avoidance

There is a geometry error here, and it is ours rather than his.

The intake is 50 mm in diameter, not 5 mm. The 5 mm holes the referee refers to are in the two acrylic plates at the base of the tube, downstream of collection, where the filter papers sit. They are the deposition and viewing apertures, not the orifice through which water is drawn. The 0.5 litre enters through the 50 mm mouth of the tube, and everything it carries is deposited onto the five 5 mm filter sections, which are then examined under the microscope. Those holes size-select nothing. They are where the sample ends up, not where it enters.

The first referee read the same drawing the same way, describing a 50 mm aperture but treating the filtration geometry as the limiting one. Two expert readers reaching the same conclusion about the same figure means Appendix A is not clear enough, and that is ours to fix. **In revision the water path will be described step by step, and Figure A1 will label the intake and the filtration plates separately so that the two cannot be confused.**

The referee's underlying point survives in part, and we accept it in part. It also depends on how the cartridge fills, which the manuscript states only in part. The methods do record that the tube is flushed three times with ambient water before the sample is taken. What they do not say is that the tube is then dropped into the water, so that the sample is scooped by immersion rather than drawn by suction. No sustained intake flow exists for an animal to detect, and none for it to swim clear of before the tube is full.

A larger, strong-swimming animal may still have scope to escape a scoop, and for mesozooplankton we make no claim that the cartridge is quantitative. That is precisely why the zooplankton number comes from a net of 0.30 m mouth diameter, and why the net was put in the water at all. But avoidance requires an organism to sense the disturbance and to out-swim it, and that capacity falls away steeply with body size. It is not a plausible explanation for the 20 to 300 µm fraction, which is the fraction the near-absence claim actually concerns. **In revision the filling mechanism will be described explicitly alongside the flushing procedure, since an intake that draws and one that scoops raise different questions.**

4. Contamination from the vessel

The referee's specific mechanism does not apply, but his underlying concern is sound and we accept part of it.

The cartridge is not a towed net. Appendix A describes a self-contained tube which, because it hangs from its line, filters vertically, and which takes a fixed 0.5 litre on each fill, independent of the vessel's speed through the water. It fills once, it is recovered, and the filter stays capped from collection until analysis. It does not strain water continuously behind the hull in the way the referee's description implies.

A correction to Appendix A. The text describes the cartridge as suspended on a Kevlar cord. During this survey it was suspended on stainless steel wire. Kevlar is the intended replacement, chosen for durability, and the appendix was written against the current design rather than the one that took these samples. That is our error and **the appendix will be corrected to state stainless steel wire for the reported survey, with the change to Kevlar noted for future campaigns.** It happens to bear on the referee's point, since steel sheds no fibres, but we would be disclosing it either way.

Deployment. The sample is taken off the stern or the leeward side of the vessel. The manuscript does not say so, and it should. **In revision the deployment will be specified exactly: the position relative to the hull, whether the vessel is making way or stopped, and the depth of immersion.** We note plainly that stern water has passed the hull, so as it stands this is not a geometry that disposes of the referee's concern. **We will therefore specify the leeward side only for future sampling, so that the question does not arise.**

We would also put the scale of the expected effect on the record. These are small sailing yachts working under sail, with no engine running while the sample is taken. The wetted area is a small fraction of a research vessel's, there is no cooling-water discharge, no exhaust plume and no propeller wash, and the volume of water disturbed is correspondingly small. On that reasoning we expect any vessel-derived contribution to be very small. It is reasoning and not measurement, and we offer it as such, since we have not put a number on it.

Handling. The risk we identified as the largest was the operator rather than the vessel: synthetic clothing worn by whoever takes the sample. The protocol addresses it directly, and both controls are already in the methods. The tube is flushed three times with ambient water before the sample is taken, which displaces anything carried from the deck or retained from the previous station, and it is capped as soon as it leaves the water and uncapped only immediately before examination, so the sample spends no time open on deck. Since the referee has read the paper closely and still reached the contamination question, these controls are evidently not prominent enough where they sit. **In revision they will be gathered into a single contamination-control paragraph rather than distributed through the method description.**

What the fibre sizes say. Cordage is the near-field source the referee would expect to dominate, and a fibre shed from a rope in use is of the order of millimetres. The fibres we recovered are far smaller than that, with plastic fibres forming a distinct mode at a median length of 204 μm, which is the wrong size distribution for rope shedding. Ropes that are not under load or running are in any case not shedding. None of this is proof, since a fibre can fragment, but the sizes run against the mechanism rather than for it.

Antifouling. Cruising yachts increasingly carry hard antifouling coatings rather than ablative ones, and a hard coating is not designed to slough. We do not treat hull coating as the leading contamination risk for this reason. That is as far as we can take it, since we did not test the coatings.

Blanks. The blanks were run by the lead author rather than by the crews, and they found no contamination of the samples. They tested for airborne contamination, which is all we claim for them.

What we accept without reservation:

- The blanks addressed airborne contamination, not waterborne contamination from the vessel itself.
- Antifouling paint, deck runoff, degrading cordage and sailcloth, and exhaust deposition are all plausible near-field sources, and we did not test for them.
- We cannot presently quantify what fraction of the particle load is vessel-derived.

We would add one observation about the principle rather than the case. The referee writes that every vessel is a significant point source of local contamination, and we agree. That applies with more force to a research ship than to a sailing yacht, since a large vessel carries more machinery, more coatings, more paint and more waste streams, and it disturbs a far larger volume of the layer being sampled. This is not an argument that our samples are clean. It is an argument that the problem he identifies is general to shipborne sampling of the surface layer, and it is not solved by using a bigger vessel.

In revision this will be stated as a limitation in its own right rather than folded into the general caveats, and the particle concentrations will be described as an upper bound on the environmental load. The next campaign will carry a vessel-source test: paired samples taken upwind and upstream of the vessel while hove to and while under way, with paint, cordage and sailcloth reference spectra held for comparison against the confirmed particle spectra. If a vessel signature exists, that design will find it.

5. The hand-count comparison

This point began as an allegation about our conduct, and the honest answer to it turned out to be larger than the sentence the referee quoted. In re-analysing the archive we examined the crew hand-logs in full, and they disagree with the image analysis in both directions: two crews logged totals of 973 and 1,148 “zooplankton” that the archived micrographs do not support at any size, while other crews logged zero. Visual identification of small, degraded particles by non-specialists is not reliable, and the first version of the manuscript should not have implied that a single hand-counted leg constituted validation. The revision reports the hand-logs for what they are, qualitative context retained in the deposited workbook, and rests the zooplankton measurement on the net tow, where identification is not in doubt. We did not bury inconvenient validation; but we did lean on a validation that could not bear weight, and the referee was right to press on it.

6. What the net tow shows

This is the substantive scientific disagreement and we set out the evidence.

Before doing so we correct something in our own earlier reply. In responding to the first referee we stated that no surface-only mesozooplankton measurement exists for this water. Further searching since has located one, the Tara Pacific surface datasets described

below, and we correct that statement here rather than leave it standing. It was our slip and we would rather flag it ourselves than have someone else find it.

The referee states that our net-tow concentrations "fall within the expected range for the studied region" and that "the net tows detected normal zooplankton levels". No source is given for that expectation, and we do not think the literature supports it.

It is worth noting what is not in dispute. The referee accepts the net-tow measurement itself; his argument is about what 0.2 to 0.5 animals per m^3 means, not about whether we measured it. That narrows the disagreement to a single question, which is what counts as normal at the surface.

The comparison has to be surface against surface. In stratified oligotrophic water the animals are not in the top metre. Comparing our surface tow against a depth-integrated value is the error the first referee identified in our own manuscript, and we have accepted it there. Taking published surface data against a published 0 to 200 m integration for the same trophic regime, the top decimetre to metre of the daytime oligotrophic ocean holds of order **1 to 6%** of the mean 0 to 200 m concentration. A 0 to 200 m value of about 200 ind m^{-3} (Fernández de Puelles et al., 2019, 300 µm, oligotrophic tropical and subtropical ocean) therefore scales to a daytime surface expectation of roughly **2 to 12 ind m^{-3}**, not to 200.

Against true surface data, our values are low and not ordinary. The Tara Pacific expedition published quantitative datasets of the top 0 to 1 m sampled in daylight across the Pacific and North Atlantic (Mériguet et al., 2025). The values below are computed by us from the archived data rather than quoted from the paper, which prints no summary statistics; the datasets are open at SEANOE (https://doi.org/10.17882/102336 and https://doi.org/10.17882/102537) and the computation is reproducible.

With the 330 µm high-speed net, the median total metazoan mesozooplankton concentration in the North Atlantic subtropical gyre is **2.20 ind m^{-3}** and the lowest station is 0.76; across 130 tropical and subtropical surface stations the median is **3.82 ind m^{-3}**. The authors describe that net as semi-quantitative and report it undersampling by roughly a factor of four relative to a conventional neuston net, which is why we also give the second gear: the 333 µm Manta neuston net on the same expedition returns a tropical median of **11.4 ind m^{-3}** with a minimum of 2.23. Correcting the high-speed values for the stated bias moves them towards the Manta figures rather than towards ours.

Our value is **0.2 to 0.5 ind m^{-3}**, and it was obtained with a **120 µm** mesh. Those Tara nets are 330 and 333 µm. A finer mesh retains more, not less, so a like-for-like comparison would raise our number relative to theirs rather than lower it. On the only published true-surface dataset for tropical and subtropical water, our concentrations sit at or below the bottom of the range, measured with gear that should have read higher.

What we cannot claim. No published study reports total mesozooplankton in the top few metres with a 100 to 200 µm mesh in the oligotrophic tropical North Atlantic. The exact measurement that would settle this does not exist, which is why the survey was

undertaken. We therefore say that our values are low against the closest available surface data, and we do not say that a comparison of record has been made.

In revision the comparison will be rebuilt on surface data only, with Tara Pacific as the principal reference, the mesh difference stated in the same sentence as the ratio, and the depth-integrated values used only after the surface fraction has been applied. The wording "far below densities typical of productive open ocean" will go, because productive open ocean is the wrong baseline and we should not have used it.

On the conclusion

The referee's closing charge is that the data do not support a near-absence of surface zooplankton or a causal link with pollution. On the second we agree, and we will say so more clearly: **the manuscript reports a co-occurrence and does not establish causation, and any wording implying otherwise will be removed.**

On the first, the re-analysis has made the evidence simpler than it was. What the revision claims is what the data carry: a reproducible surface particle inventory across a basin that has not been surveyed this way before; no zooplankton confirmed in any of 1,225 curated filter samples; and a net-tow surface mesozooplankton concentration at or below the lowest values in the published surface record, measured with a finer mesh than those records used.

That concession is about what this survey can prove, and not about whether the question is a reasonable one to ask. The mechanism is not ours and it is not speculative. Zooplankton ingest microplastic, which was shown directly by Cole et al. (2013) across thirteen taxa. Plastic leachate suppresses growth and oxygen production in *Prochlorococcus*, the most abundant photosynthetic organism in the ocean (Tetu et al., 2019). Plastic surfaces adsorb lipophilic organic contaminants and carry them, and the affinity and the leachate toxicity both rise as the particle weathers (Liu et al., 2020; Ferrari et al., 2024). Every step of that chain is in the literature. What is missing is a measurement of exposure in the surface layer where the particles and the animals actually meet, and that gap is the one this survey was built to close. A monitoring survey establishes the exposure. It does not establish the effect, and we will not write as though it does.

A word on why the survey was designed this way, since it bears on the referee's view of what the data are worth. Plastic particle analysis was the primary purpose of the crossing, and the buoyant fraction can only be measured at the surface. Density decides this, and it does not follow the everyday division between soft and hard plastics. Polyethylene at roughly 0.91 to 0.97 g cm^{-3} and polypropylene at roughly 0.90 to 0.92 float in seawater of about 1.025, and those two are the largest-volume polymers ever produced (Geyer et al., 2017). Expanded polystyrene floats. Polyester and PET at about 1.38, PVC from 1.16 upwards, acrylic at about 1.18 and ABS at about 1.05 all sink, so they leave the surface layer and are lost to a surface survey from the start. Biofouling eventually takes down much of what floats as well, which makes the surface a residence time rather than a permanent home for the buoyant fraction. Whatever is going to be seen there has to be caught there.

The vertical structure of that fraction is measured, not assumed. Buoyant plastic concentrates in the top few centimetres and falls away with depth as the wind mixes it down, so a sample taken below the immediate surface, or taken in a blow, returns a lower number than the water actually holds (Kukulka et al., 2012; Reisser et al., 2015). Add the mesh. The conventional instrument is a manta trawl of about 333 μm towed at the surface or a net worked deeper, and everything smaller than the mesh passes through it. Our filters resolve to 20 μm. Between the depth and the mesh, the great majority of the particles reported here would not appear in a conventional survey at all, which is the reason the survey was run rather than a defence of it after the fact.

There is a second point in the same area which we think matters more, and it has had almost no attention. Our filters recovered partially combusted carbon particles alongside the plastic, and the carbon, not the plastic, dominated the load. That is the one classification the hand count and the automated pipeline agreed on without qualification. Almost no marine plastics survey counts these particles, and most sampling designs would not retain them in the first place. Yet black carbon is among the strongest sorbents of hydrophobic organic compounds known in the environment, binding them one to two orders of magnitude more strongly than amorphous organic matter (Cornelissen et al., 2005). The comparison is worth setting beside Koelmans et al. (2016), who concluded from modelling that the plastic vector is modest relative to other exposure pathways. We cite that paper knowing it cuts against the strongest version of the plastic argument, because it also points somewhere uncomfortable: if plastic is a modest sorbent and is nevertheless the particle the community counts, then the stronger sorbent in the same water is going almost entirely unmeasured.

We are not claiming to have demonstrated that soot is toxic to marine life, and we have run no experiment that could. We are saying that a particle class with a documented affinity for exactly the compounds that concern us is present in the surface ocean in quantity, that hardly anyone measures it, and that the question of what it does there is open and answerable. **In revision we will state that as a finding of the survey in its own right, since a gap of that size in marine monitoring is a result and not an aside.**

A last word on where we think this leads, because it bears on how the paper should be weighed. The value of the approach is not this one crossing. It is the access an extended network could give the wider community: a large and continuously growing body of water and marine science data, from places and times that funded programmes do not reach, available to any academic who wants to use it. That only works on two conditions, and both referees have effectively named them. The filter has to stay simple and cheap enough that volunteers can use it everywhere, from a rain gauge to a mid-ocean passage. And the sampling procedure has to be repeatable to a standard that lets one crew's numbers be set against another's, and this year's against next year's. Read that way, the criticisms in this review are a specification for the second version of the protocol, and we are grateful for them.

Summary of changes

1. The entire image analysis re-run as a deterministic, deposited pipeline (Zenodo v2 of 10.5281/zenodo.21477682): frame curation with a logged reason for every excluded file, detection, sizing, classification, and individual inspection of every object ≥250 µm. The 44 animal-class objects of the first version are withdrawn; the corrected result is no zooplankton confirmed in any filter sample. All particle totals, Table 1, Table 3 and Figures 2 to 4 regenerated from the curated archive.
2. Net-tow method, mesh and filtered volume stated in the abstract and at first mention of any density; the cartridge described only as a particle sampler, with an explicit statement that no zooplankton abundance is derived from it.
3. Full cartridge specification moved into the main text, with the water path described step by step and Figure A1 labelling the 50 mm intake and the 5 mm filtration plates separately; the field guide link repaired and the guide deposited with a DOI.
4. Deployment geometry specified exactly, with leeward-side sampling specified for future campaigns, and the contamination controls already in the methods, the three ambient flushes and the capping of the tube on leaving the water, gathered into a single paragraph where a reader will find them. Appendix A corrected to state stainless steel wire for the reported survey, with the change to Kevlar noted for future campaigns. Vessel-derived contamination stated as a limitation in its own right; particle concentrations described as an upper bound on the environmental load; a vessel-source test written into the next campaign.
5. Section 2.7 rewritten to disclose the re-analysis in full and to report the crew hand-logs as qualitative context that disagrees with the imagery in both directions; the zooplankton measurement assigned to the net tow alone.
6. The zooplankton comparison rebuilt on published surface data, with the mesh difference stated alongside every ratio and depth-integrated values used only after the surface fraction is applied.
7. “Far below densities typical of productive open ocean” removed.
8. All wording implying a causal link between the particle load and the zooplankton concentration removed.
9. The near-absence of combustion carbon from marine particle monitoring stated as a finding of the survey in its own right, with the sorption literature that makes it consequential.
10. The rationale for a true-surface, 20 µm sampling design stated explicitly in the introduction: polymer density, the measured vertical distribution of the buoyant fraction, and the mesh of the conventional instrument.

We are grateful for the review. The challenge to the image classification led directly to the deposited re-analysis, which is the single largest improvement this paper has received, and the broken link should never have reached a referee.

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