Author response to Referee Comment 1

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 are grateful for this review. It is detailed, it is plainly the work of someone who knows this ocean, and it has improved the manuscript already. It has also found an error in our reference list that we accept without reservation, along with several methodological limitations that we accept and will address.

It rests, in part, on an attribution of our zooplankton result to the wrong instrument, and we deal with that first because the referee's central quantitative argument follows from it.

Our approach below is simple. Where the referee is right we say so and state what changes. Where we think he has gone wrong we give the evidence rather than the assertion. Where a question cannot be settled either way, we say that too.

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

Before the specific points, it may help to state the design intent, because several of the referee's criticisms are answered by it and one of them is answered by his own text.

This is a citizen-science project built around what an ocean-going blue-water yacht can actually do. The sampling protocol is constrained by that: a small crew, underway, without winches, wire, laboratory space or power for depth-resolved work. Those constraints are real and we do not pretend otherwise. **This survey does not replicate oceanographic sampling at depth, and we make no such claim. It cannot resolve the deep chlorophyll maximum, it does not retain picoplankton, and it does not substitute for a research cruise.**

What it offers instead is coverage. Cruising yachts cross ocean 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 referee makes this point for us: "Routine CPR sampling is unfortunately not carried out in this ocean area." Nor is the AMT, which runs meridionally along about 20°W; ours is a zonal transect at right angles to it. The water we sampled is not under-described because it is uninteresting. It is under-described because nobody goes there with a ship.

One further point of context bears on the referee's closing remarks about planning. To our knowledge this is the first survey to measure plankton and anthropogenic particles together along a zonal transect of the equatorial Atlantic, from North Africa to the Caribbean, and no prior near-surface survey of that water exists against which a protocol

could have been designed. We did not know in advance what the sampling would find, and a protocol cannot be specified in full for conditions that have never been described. The sampling was also carried out by yacht skippers rather than by trained researchers, people giving their time on passages they were making anyway, and that places a hard ceiling on procedural complexity. A method that a volunteer cannot carry out reliably at sea, in a seaway, at the end of a watch, is not a method available to this project at all. Within those constraints the survey returned 352 stations across a basin, which we offer as a useful first return rather than as a finished protocol. Like any survey conducted for the first time it carries lessons, and the referee has identified several of them. The refinements set out below, namely spectroscopic confirmation, a flowmeter, procedural blanks from every crew and consistent depth terminology, are the second iteration of a method that did not exist before the first.

We would add that the referee's own citations make this case better than we can. Simoniello et al. (2019), which he offers as a model of how citizen science should be done, is in part a study of plankton sampling from cruising yachts, and it observes that their routes "often cover tracts of ocean undersampled by traditional oceanographic cruises", describing the conversion of ordinary yachts into in-situ monitoring platforms at a cost roughly twenty times below that of a research vessel. Hidalgo-Ruz and Thiel (2015), his other citizen-science reference, recommends this design explicitly, proposing projects "with the help of sailing clubs, where long-distance travelers can survey floating marine debris by direct observation at sea, to study the distribution, composition and degradation of marine litter in the open ocean". We take his closing principle seriously. We also note that the two sources he cites for it endorse the approach he questions.

The trade is therefore explicit: depth resolution and analytical control are given up in exchange for spatial and temporal coverage that is otherwise unobtainable. Whether that trade yields anything useful is a fair question. It turns on whether a constrained, well-characterised measurement can be compared like-for-like against other constrained measurements of the same layer, not on whether it matches what a research vessel would have produced. We accept the referee's closing principle without reservation: citizen science in this field has to be planned with rigour, its selectivity stated, and its comparisons made against the right baseline.

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1. The zooplankton densities come from net tows, not from the 0.5 L imaging device

The referee writes that "the limited volume (0.5 litre; i.e. 0.0005 m^3) of GOES device makes it highly unlikely that any single water sample will include zooplankton", and compares it with the Continuous Plankton Recorder's 3.2–3.8 m^3 standard unit (Jonas et al., 2004), concluding that the CPR filters "6,000 to 7,000 times greater volume".

The 0.5 L device is not the source of the zooplankton densities. 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 that tow: a 120 µm mesh net of **0.30 m mouth diameter**, towed

at the surface at 1–2 knots for 10 minutes per haul, straining **approximately 43,600 litres, or 43.6 m^3, per haul** (mouth area 0.071 m^2 × ~617 m towed), run in paired day and night sets.

Three consequences follow:

- The filtered volume behind the reported densities is **roughly 11 to 14 times larger** than the CPR standard unit, not thousands of times smaller. The comparison in the review is inverted.
- The mouth diameter is 300 mm, not the 50 mm the referee attributes to it, which changes the evasion argument materially.
- Diel vertical migration, raised by the referee as though unaddressed, is the explicit reason the tows were run by day and by night, and both sets are reported in Table 2.

On the aperture criterion specifically, we would ask the referee to apply it consistently. The Continuous Plankton Recorder he proposes as the comparison standard samples through a **1.25 × 1.25 cm aperture** in its nose cone, on **270 µm** silk, at about **10 m** depth. Our tow net has a 300 mm mouth on 120 µm mesh at the surface; even the imaging device he criticises has a 50 mm aperture, four times the width and some sixteen times the area of the CPR's. If a small aperture disqualified a sampler from quantifying larger, actively swimming zooplankton, it would disqualify the longest plankton time series in existence. It does not. The reason it does not is that a known and stated selectivity can still support quantitative comparison, provided like is compared with like, and that is the same reason we report our mesh, mouth diameter and filtered volume explicitly. We accept the selectivity; we do not accept that it is disqualifying here and not there.

The same principle applies to optical measurement. The Phytoplankton Colour Index, one of the CPR survey's principal outputs, is a visual assessment of the silk's greenness against a standard colour chart, described by the survey itself as a semi-quantitative estimate of biomass, and it exists precisely because the chloroplasts of broken cells and the smaller phytoplankton cannot be counted under the microscope. An optical, semi-quantitative index is therefore not disqualifying in itself, since one such index underpins the longest plankton record in existence. Our own counts are of a different kind, and the distinction is worth drawing. The imaging pipeline enumerates individual particles above a stated size floor within a known volume, which yields a concentration rather than an index. What is provisional in our case is the assignment of those counts to material classes, not the counting itself. We are careful about what that does and does not license. It does not excuse our own particle classification from spectroscopic confirmation, which we accept in full at point 4 below, because identifying a material is a different claim from estimating a biomass. It does mean that an objection to optical methods as such cannot be sustained.

There is a mechanical reason for the difference in what the two methods can see. The CPR is towed behind merchant vessels on their normal sailings, and Jonas et al. (2004), cited by the referee, establishes that the volume filtered falls as ship speed rises. Plankton meeting the silk at those speeds are pressed between two layers of mesh, which is the reason a colour index is needed for the fraction that survives as pigment rather than as countable cells. Our imaging step applies no filtration, no concentration and no tow of any kind. The

0.5 litre sample is a direct fill of the sample tube, and the whole of it is then examined across five apertures, so the organisms are never drawn across a mesh, never pressed between layers of silk and never subjected to the mechanical stress of a high-speed tow. They are imaged in the condition in which they were collected. The separate confirmation net, which is a different instrument, is towed at 1 to 2 knots rather than at service speed. We have not quantified specimen condition and we claim no figure for it. We will add an image-based condition score to the next campaign so that this comparison can rest on evidence rather than on inference.

The mesh comparison also bears on the particle result, and here it is decisive. The CPR silk is 270 μm. Our imaging pipeline sized and classified 51,547 particles larger than 20 μm across 352 stations, and abundance in that record is concentrated at the smallest resolvable size, with plastic fibres forming a distinct mode at a median length of 204 μm. The great majority of the particles reported in this manuscript are therefore smaller than the aperture of a CPR silk and would pass through it. We make no criticism of the CPR in saying so, since it was built for a different purpose and long predates the question. The point is only that the instrument the referee proposes as the standard could not have produced this result, which is part of why the result did not exist before now.

We would also be wary of treating any single filtered-volume figure as a fixed benchmark. Jonas et al. (2004), the source the referee cites, found that the volume filtered by a CPR falls as ship speed rises. Takahashi et al. (2006), in the same journal, found the correlation positive, attributed it to the flow meter itself, and reported reductions of up to 60% from clogging and up to 78% from clogging and ship speed combined. The CPR's own sampled volume therefore carries a wide uncertainty, which is a further reason to compare methods by their stated selectivity rather than by a single number.

We should say plainly where this leaves the comparison, because we do not want the preceding paragraphs read as a criticism of the CPR. The two systems carry different limitations and different strengths, and we do not think either should be treated as the standard by which the other is judged. A CPR resolves taxonomy along routes that have been occupied for ninety years, which nothing else in oceanography can offer. A direct-fill imaging sample resolves the small particle fraction in water that no research vessel visits, which the CPR was never designed to do. Neither is a substitute for the other, and a direct comparison of the two obscures what each contributes. The more useful position is that different instruments extend the field of measurement in different directions, and that the marine ecosystem is described better by both together than by either alone. We would welcome the difference being taken that way, and we offer this reply in that spirit.

We do not offer any of this as a complaint. If a careful referee attributed the result to the wrong instrument, the manuscript did not separate the two methods clearly enough, and that is our responsibility to fix. **In revision the net-tow method, its mesh and its filtered volume per haul will appear in the abstract and at the first mention of any zooplankton density**, so that no reader can associate those numbers with the imaging device.

Two elements of the referee's concern survive this correction and we accept them. A 120 µm mesh retains larger taxa while passing the smallest copepods and nauplii, and our filtered volume is computed from tow geometry rather than from a flowmeter. Both are already stated in the manuscript (lines 197–198); both will be given more prominence, and a flowmeter will be fitted for the next campaign.

2. The citation error is real, and we accept it

The referee states that Woodd-Walker et al. (2000), as we cite it, does not exist. He is correct, and we apologise for it. We note without complaint that the same appraisal appears in the referee's own text as Loder and Gerdis (2013) and in his reference list as Löder and Gerdts (2015). Citations are treacherous things, and we are in no position to be smug about it.

The source we intended is **Woodd-Walker, R. S., Ward, P., and Clarke, A. (2002), "Large-scale patterns in diversity and community structure of surface water copepods from the Atlantic Ocean", *Marine Ecology Progress Series* 236, 189–203, doi:10.3354/meps236189**, which draws on 259 samples from Atlantic Meridional Transect cruises between 60°N and 63°S and therefore does span the tropical Atlantic, rather than the 35 to 60°N band of Gallienne et al. The entry will be corrected, and every remaining reference in the manuscript has been re-checked against Crossref.

Correcting it, however, repairs only the geography, and we would rather set out the consequence than leave the referee to find it. Despite its title, that study sampled with a WP2 net through the top 200 m, so the objection the referee raises against Fernández de Puelles et al. applies to it equally. Taken together with his other two points, the position is this: **none of the three sources we cited is a like-for-like surface measurement.** Two integrate the epipelagic and one is a deep-sea synthesis.

On the two related points, we agree. Vereshchaka et al. (2017) is a deep-sea synthesis and is not an appropriate comparison for a surface stock. Fernández de Puelles et al. (2019) integrate to 200 m; we described this as the "epipelagic layer" (line 96), which is accurate but does not make the comparison like-for-like, because that integration includes the deep chlorophyll maximum. **In revision the comparison will be restricted to surface-sampled datasets wherever such data exist, and where an integrated value is used it will be stated as an upper bound on the surface concentration rather than as a comparable measurement.** We note in passing that the difficulty of finding a genuine surface comparison for this water is itself the coverage gap described in Section 0. The surface mesozooplankton stock of the tropical North Atlantic has scarcely been measured as a surface stock, which is why the comparison is hard to make and why we think it is worth making.

3. Oligotrophy, the deep chlorophyll maximum, and what we do and do not claim

We accept the substance of the referee's framing point. The North Atlantic subtropical gyre is strongly stratified and nutrient-depleted; pico- and nanophytoplankton dominate carbon

fixation there, at 84% of primary production in the western tropical Atlantic on the referee's own source (Tilstone et al., 2017); the chlorophyll maximum lies well below our sampling depth (Rees et al., 2015); and our sampling reaches roughly the top 1 m. We did not sample the DCM.

One detail of that framing needs correcting, and we raise it only because the depth bears on the comparison. The referee places the deep chlorophyll maximum at 50 to 200 m and states that productivity there increases by one to two orders of magnitude, citing Rees et al. (2015). That paper is a seven-page retrospective of the AMT programme. It reports no primary production data and contains no such statement. Its Figure 2 places the tropical Atlantic chlorophyll maximum at roughly 45 to 100 m, at values a few times the surface concentration rather than ten to a hundred times it. The maximum is real, it lies below our sampling, and we accept the point. Its depth and its magnitude are both smaller than the review states.

On phytoplankton the position needs stating precisely. The imaging device does count a phytoplankton class. What is reported in the manuscript is this: pigmented aggregates present at 81.8% of stations, median 10 per litre (Sect. 2.11). Every count in this survey is subject to a stated size floor of 20 μm (line 236). Pico- and nanophytoplankton fall below that floor, so the referee is right that they are neither retained nor identified by our method.

We are not certain, though, what the criticism amounts to, and we would welcome the referee's clarification. If the point is that we drew conclusions about productivity from a size-limited surface count, we accept it without reservation, and the rewrite described below removes them. If the point is that a 20 μm floor is itself a defect, we do not follow the argument. Every plankton method carries a size floor, the CPR's is 270 μm, and ours is declared in the manuscript rather than uncovered in review. A stated selectivity is a parameter of a method and not a fault in it, which is the same principle we applied to the CPR at point 1 above.

The consequence is a limit on interpretation rather than an absence of data, and we will make it explicit. **Our phytoplankton class is an optical count of pigmented particles of 20 μm and larger. It is not a measure of phytoplankton biomass, of chlorophyll, or of primary production, and no inference about productivity in this region should be drawn from it.** What it can support is a comparison of the ≥20 μm particle field between stations along the transect, and that is the use we make of it.

We would note that we are aware of the region's oligotrophy: lines 94–96 frame the comparison explicitly against oligotrophic subtropical gyres, name the Malaspina circumnavigation as the comparison, and record that its 300 μm net undercounts the smallest copepods. The claim under test is not "productivity here is low", which is not in dispute and would not be worth reporting. It is that **the surface mesozooplankton stock is far below what the surface literature reports for comparably unproductive water**. We can settle this now, and the answer is that there is no surface data. The thousandfold figure comes from setting our surface value of 0.2 to 0.5 animals per m^3 against epipelagic values of upward of 200 per m^3. That sets a surface stock against a 200 m integration, so it

is not like-for-like, and we cannot make it like-for-like because a surface-only mesozooplankton measurement for this water does not exist. None of the three sources we cited provides one, and we have not found one elsewhere. **In revision we will say precisely that, give the ratio against integrated values with the integration named in the same sentence, and withdraw the thousandfold figure as a statement of surface deficit.** The absence of a surface baseline is not a weakness in the argument. It is the gap the survey was designed to fill.

On line 419–420 the surrounding paragraph concerns *Prochlorococcus*, numerically the most abundant photosynthetic organism in the ocean and the organism whose sensitivity to plastic leachates (Tetu et al., 2019) motivates the passage, rather than the chlorophyll maximum. But "where the ocean's most abundant photosynthetic organisms live" reads as a claim about where production occurs, and in this region that is subsurface. **The sentence will be rewritten to say more clearly what it means: that the near-surface layer we sampled carries a high particle load and is inhabited by picocyanobacteria, without any implication that it is the region's productivity maximum.**

4. Particle identification

Procedural blanks were run, by the lead author, and they showed no obvious contamination (lines 226 to 229). What we did not do was ask each volunteer crew to run blanks of their own, and that was a deliberate design decision rather than an oversight. Every additional procedure asked of a skipper underway is a procedure that will be performed inconsistently or skipped altogether, so we minimised what the crews were asked to do and relied instead on blanks run under controlled conditions together with a single common microscope design across all vessels. There is also a practical difficulty with a blank at sea that is worth naming, because it bears on how useful per-crew blanks would have been. A procedural blank is only as good as the water used to make it, and a cruising yacht carries no certified particle-free supply. The water available is tank water that has passed through the vessel's own plumbing and filters, and it may carry fibres and particles of its own, so a blank prepared from it risks adding error rather than measuring it. Any remedy also has to fit the vessel, and stowage on a cruising yacht is scarce, so carrying prepared blank water for every station is not practical. Two measures that do fit are written into the next campaign. The first is a small number of sealed blank-water samples per voyage rather than per station. The second is an exposure blank taken at the point of analysis, a prepared slide left open beside the working sample, which requires no water at all, occupies no space, and tests the airborne fibre pathway that is the main contamination risk we identified in the manuscript.

The manuscript already flags the limitation (lines 226–231): the optical "black carbon" class awaits spectroscopic confirmation and the plastic-versus-phytoplankton partition is provisional. The difficulty of identifying microplastics reliably by eye is well documented (Löder and Gerdts, 2015), and his point stands. We would note, for the record rather than in objection, that the 1.4% confirmation rate he quotes from that chapter comes from North Sea sediment in which 96% of the particles examined were quartz sand grains left behind by an incomplete density separation, an outcome the authors themselves describe as surprising and use to argue for routine FTIR. Sand is not a candidate class in an open-ocean

surface sample. We expect our own confirmed fraction to be higher, and we will report it whatever it proves to be.

We should be straight about what can and cannot be done now. **Spectroscopic confirmation cannot be completed for this manuscript.** It needs material collected under a protocol designed for it, which means the next sampling campaign rather than a reanalysis of this one. We would rather say that than promise a measurement we cannot deliver in revision.

What we will do in revision is withdraw the inference that confirmation would have supported. **The optical classes will be labelled as optical classes throughout, with no claim about material composition, in the abstract and in every table and figure caption as well as in the text.** *Sargassum* fragments will be named as a candidate contributor rather than assumed absent, which is the referee's suggestion and a good one. FTIR or Raman confirmation is written into the next campaign, and the confirmed fraction will be reported whatever it turns out to be.

5. The meaning of "surface"

Accepted. The manuscript does use "surface" for the microlayer, for the top metre and, in the comparison literature, for the upper 200 m, and the referee is right that steep physical, chemical and biological gradients sit between those layers in stratified tropical water. **In revision we will define three terms at first use, the microlayer (<1 mm), the near-surface layer (0 to 1 m, which is the layer we sampled), and the epipelagic (0 to 200 m), and we will use them consistently, including in the figure captions and table headings.**

6. The microlayer, evaporation and climate

For context, the manuscript as first drafted was confined to the sampling; the microlayer and evaporation material was added when an earlier external referee asked us to widen the scope beyond the survey itself. We note that only to explain how it came to be here.

On the substance, we ask the referee to distinguish two claims, because we think the review conflates them, and we accept part of what he says.

What we do not claim. We do not have a magnitude for the effect of microlayer condition on ocean evaporation, and we are not aware of a published quantification of it. To the extent that our framing implies a quantified climate effect, that is an overstatement and we will correct it. What we would maintain is narrower: a surfactant-rich microlayer is expected to alter the rate of evaporation, and with it the humidity supplied to the atmosphere above, because that is what organic films do at an air-water interface. The major syntheses of microlayer science quantify its suppression of gas exchange in detail and offer no comparable treatment of the water flux (Engel et al., 2017; Wurl et al., 2017), so whether the effect at sea is large enough to matter for climate remains unestablished, in our work and in the wider literature.

What the gap itself tells us. We would go further and offer that absence as a finding rather than as an embarrassment. Every water molecule that evaporates from the 71% of the planet's surface covered by ocean passes through the microlayer, and that same layer is known to suppress the transfer of gas across it by a large fraction. That a layer of such position, and of such demonstrated influence on one exchange, has attracted no quantitative study of its influence on the other is a gap worth naming rather than passing over in silence. We would rather the manuscript be read as flagging that neglect than as claiming to have measured its consequences.

What we would resist. The referee describes the effect as "trivial, with temperature, wind-speed and ocean area being much more important by several orders of magnitude", while also stating that "detailed evaluation of this aspect of the MS is beyond the expertise of this reviewer". We accept the second statement in good faith and note that it applies to the first: the several-orders-of-magnitude figure is an expectation, not a measurement, and the same objection we accept against our framing applies to it.

Why we regard the question as legitimate rather than idle. The retardation of evaporation by insoluble organic monolayers is established physics, studied since Langmuir and Schaefer and applied, with well-documented practical difficulty, to water storages; the documented weakness of those films is precisely that wind and wave action disrupt them, which is the referee's point and a fair one. Separately, and more directly relevant, natural surfactants in the sea-surface microlayer are now known to suppress the in-situ CO_2 transfer velocity by a large fraction across the Atlantic (Pereira et al., 2018). Gas transfer and evaporation are not the same resistance and we do not conflate them. But a demonstration that natural marine films exert a large control on one exchange across the air-sea interface is the reason we consider the question about the other exchange worth asking, rather than answered in advance. There is a further reason, and it bears directly on the referee's point about wind. He treats wind and wave action as the thing that destroys an organic film, and for a monolayer on a reservoir that is correct. At sea it is also the mechanism by which the microlayer enters the atmosphere. Bubbles bursting through the microlayer inject its organic material into the marine boundary layer as sea spray aerosol, and that aerosol carries a biogenic organic fraction which contributes to the cloud condensation nuclei over the remote ocean (O'Dowd et al., 2004). Whether this amounts to a regulating feedback on cloud albedo is disputed (Quinn and Bates, 2011), and we rest nothing on the stronger version of that claim. What it does establish is that the condition of the microlayer already has a documented pathway to cloud formation, so a question about its influence on the water flux is not an idle one.

What we propose. The retardation of evaporation by organic films at an air-water interface is established physics. That the microlayer suppresses gas transfer at sea by a large fraction is measured (Pereira et al., 2018). That microlayer organics reach the atmosphere as aerosol and contribute to marine cloud condensation nuclei is measured (O'Dowd et al., 2004). None of that is hypothesis. What is hypothesis, and the only part of it that is, is the magnitude: how much the condition of the microlayer alters the water flux in the open ocean, and whether that is large enough to matter for climate. That single question will be labelled as open, with the observation that would settle it, namely paired eddy-covariance latent heat flux measurements inside and outside natural slicks set against

measured surfactant coverage. The manuscript should not be read as claiming that this survey measured a climate effect, because it did not. What it sampled was particles and plankton. We would not accept, however, that the microlayer is climatically inert, and the manuscript says why. That layer already exerts two documented influences on climate. It suppresses the transfer of carbon dioxide across the ocean surface by a large fraction (Pereira et al., 2018), and it supplies the organic material that helps seed cloud over the remote ocean (O'Dowd et al., 2004). A layer through which every molecule of ocean evaporation must pass, which already throttles one gas flux and already feeds cloud formation, is not plausibly without effect on the water flux as well. What is genuinely unknown is the extent of that effect, and the extent is what the manuscript will flag.

We do not propose to remove this material, and we would ask the referee to consider why. It is the reason the survey was designed to sample the near-surface layer rather than any other. The pathway from the microlayer to cloud formation is documented rather than speculative. And the absence of any quantification of the water flux, in a literature that has quantified the gas flux carefully, is a finding this manuscript is well placed to record. A full quantitative treatment will need dedicated flux measurements and will be a separate paper, which we intend to pursue. Stating the question, and stating that nobody has yet answered it, does not require that paper to exist first.

7. A single survey cannot establish a trend

We did not claim that this survey represents a season, and we did not claim a measured trend. Where the manuscript sets our counts against older surveys it says the contrast "may reflect a genuine change in the equatorial Atlantic over the intervening two decades rather than sampling differences alone", which is a hedge and not an assertion.

The limitation behind the referee's point is nonetheless real and we accept it. One occupation cannot separate seasonal variability, episodic enrichment from dust deposition or storms, and long-term change, and nothing in the manuscript should be read as separating them. **In revision the results will be presented as a baseline against which repeat occupations can be compared, and every comparison with a historical survey will be labelled as such rather than as a measured change.** The long-term programmes the referee names, the AMT (Rees et al., 2015) and BATS (Madin et al., 2001), are the context against which a repeat occupation should eventually be judged. Neither samples this zonal transect, which is the gap the survey was designed to address and the reason a repeat occupation is planned.

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Summary of changes

1. Net-tow method, mesh and filtered volume per haul stated in the abstract and at first mention of any density; imaging device and net tow separated unambiguously throughout.
2. Woodd-Walker et al. reference corrected to Woodd-Walker, Ward and Clarke (2002), MEPS 236, 189–203, with a note that this repairs the geography only, since that study also integrates the top 200 m; all references re-verified against Crossref.
3. The comparison restated as a ratio against depth-integrated values, with the integration named in the same sentence; the thousandfold figure withdrawn as a statement of surface deficit; the absence of any surface-only mesozooplankton measurement for this water stated explicitly, since that absence is itself the coverage gap; Vereshchaka et al. (2017) removed from the comparison.
4. Line 419–420 rewritten; no implication that the sampled layer is the region's productivity maximum. The 20 µm size floor restated wherever the phytoplankton class appears, with an explicit statement that the class is an optical count and not a measure of biomass or production.
5. Spectroscopic confirmation deferred to the next campaign, since it cannot be completed for this manuscript; optical classes labelled as optical throughout, with no claim about material composition, in the abstract and in every table and figure caption; *Sargassum* fragments named as a candidate contributor rather than assumed absent.
6. Microlayer, near-surface and epipelagic defined at first use and applied consistently.
7. Evaporation and climate material restated with the status of each part made explicit, the established mechanism separated from the open question of magnitude, that

question labelled as open and given its test, and the quantification pursued in a separate paper.

8. Every comparison with a historical survey labelled as a comparison rather than as a measured change; results presented as a baseline against which repeat occupations can be judged.
9. Flowmeter to be fitted for the next campaign; blanks extended to every crew by means that fit a small vessel, namely a few sealed blank-water samples per voyage and an exposure blank at the point of analysis; an image-based specimen condition score added, so that sample condition is measured rather than inferred.
10. A short methods comparison added, setting the tow net's mouth, mesh, depth and filtered volume against those of the CPR, so that the selectivity of each is explicit.
11. The design rationale and its constraints stated explicitly in the introduction: what a yacht-based protocol can and cannot do, and the coverage it buys in return.

We thank the referee for the time this review evidently took. The citation error in particular should not have survived our own checking, and we would rather it was caught here than after publication. We thank too the skippers and crews who collected these samples on their own passages, and we hope this exchange makes the next campaign a better one than the first.

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