RC2: **'Comment on egusphere-2025-5789', Anonymous Referee #2, 22 Apr 2026**

We thank the reviewer for the careful and constructive evaluation of our manuscript. The comments and suggestions substantially helped us improve the clarity of the presentation, the graphical representation of the Lagrangian analyses, and the discussion of the mechanisms linking Saharan dust deposition to export dynamics in the central–southern CCS. In the revised manuscript, we implemented extensive edits throughout the text, including substantial modifications to the trajectory figures, revision of several discussion sections, implementation of Bonferroni correction in the Pearson correlation analyses, and a more cautious interpretation of dust fertilisation versus mineral-ballasting effects. All comments are addressed below in a point-by-point manner.

This preprint by Guerreiro et al. investigates how seasonal upwelling and Saharan dust deposition jointly control particulate export production in the central–southern Canary Current System. The study integrates Lagrangian backtracking of satellite-derived biogeochemical parameters such as chlorophyll-a, particulate inorganic carbon, and primary production with approximately 13–16 months of sediment trap flux data from two deep-ocean moorings collected between October 2012 and early 2014. These are combined with multivariate statistical analyses, in situ CTD profiles, and extant coccolithophore observations from a 2023 cruise. In their paper, the authors find that there is a physical-atmospheric control on export production. The Saharan dust acts as a mineral ballast, accelerating the sinking of pre-existing biogenic material, with secondary episodic fertilisation responses during dust-associated export pulses.

This paper is generally well written, it is strong because of the different methodological integration, it addresses an important challenge in this region which is the role of Saharan dust and coastal upwelling in driving the export production. The paper clearly resolves the seasonal phases during which deep export is decoupled from surface productivity and links it to lateral and subsurface supply mechanisms.

I have a few comments regarding the writing and the content of the article.

Comments related to the content:

1. My main issue with the paper is that it relies on only a single continuous deployment period of approximately 13–16 months of sediment trap collected over a decade ago (2012–2014), which raises some questions about the representativeness of the findings given the strong interannual variability acknowledged by the authors themselves, particularly at site M1. In Line 597- 601, you note that surface Chl-a was higher in April 2012 and March–April 2014 than during the study period, and attribute this to above-average surface warming during 2013. While you mention the interannual variability, you don't mention or acknowledge the period limitation. I suggest mentioning it explicitly either in the discussion or the conclusions, where the generality of your findings is mostly asserted there.

The authors agree with the reviewer and appreciate this recommendation. We acknowledge that the sediment-trap dataset analysed here is relatively limited in temporal coverage (~12–14 months) and therefore does not fully capture the interannual variability characteristic of the central–southern CCS. Precisely because of this limitation, we made a substantial effort to integrate a diverse suite of independent parameters derived from different observational and analytical approaches, including sediment-trap records, satellite observations, Lagrangian analyses, and in situ hydrographic data, in order to provide the most comprehensive assessment possible of the environmental drivers and export dynamics during the study period.

Furthermore, the apparently strong interannual variability previously suggested for M1 is no longer a prominent feature in the revised manuscript, as the more spatiotemporally resolved perspective provided by the new Hovmöller plots (new Fig. 8; former Fig. 6) does not robustly support the earlier interpretation linking reduced productivity during the deployment period to the above-average warming reported by Guerreiro et al. (2019). We therefore removed this discussion from the revised version and instead placed greater emphasis on the seasonal variability patterns, which represent the temporal scale most robustly addressed by the present sediment-trap time-series dataset.

2. Another issue in the manuscript is that the authors claim that Saharan dust acts primarily through mineral ballasting rather than nutrient fertilisation. While table 1 and Fig 3 are consistent with this interpretation, they don't really show a direct relationship. The multivariate analyses show that dust varies with warm stratified conditions while being negatively correlated with surface productivity indicators, which the authors interpret as evidence against fertilisation. In addition, no direct measurements of nutrient addition, iron bioavailability, or phytoplankton growth responses to dust inputs are available in this dataset, meaning the ballasting versus fertilisation question ultimately remains unresolved, despite the confidence with which the ballasting interpretation is presented in the conclusions. The authors should accurately reflect the indirect and correlative nature of their evidence. The statements presented the mineral ballasting as the demonstrated dominant mechanism; this should be reframed as a working hypothesis consistent with the observed statistical patterns rather than a firmly established conclusion. The authors should also more explicitly acknowledge that dust deposition systematically co-occurs with stratified oligotrophic conditions in this region, making it statistically very difficult to separate a true ballasting signal from the background oceanographic state using correlational methods alone. Maybe the authors can outline what additional direct measurements, such as iron addition experiments, aggregate composition analyses, dust associated nutrient flux measurements, would be required to firmly resolve this question in future work.

We appreciate this feedback from the reviewer and fully agree that our dataset presents several limitations in desintangling the effects of dust fertilization vs. ballasting. Nevertheless, we would like to stress the fact that, in a previous study by our team – also based on sediment-trap data collected at the same water depth and during the same period as trap M1, but in the even more strongly stratified western tropical North Atlantic – our multiparametric flux dataset provided clearer evidence for Saharan dust fertilisation within the UPZ under both windy winter–spring conditions and ITCZ-influenced boreal autumn conditions:

Guerreiro, C., V., Baumann, K.-H., Brummer, G.-J. A., Fischer, G., Korte, L. F., Merkel, U., Sá, C., de Stigter, H., and Stuut, J.-B. W., 2017. Coccolithophore fluxes in the open tropical North Atlantic: influence of thermocline depth, Amazon water, and Saharan dust, Biogeosciences14, 4577-4599, https://doi.org/10.5194/bg-14-4577-2017].

It was largely based on this previous knowledge that we have drawn several of the interpretations presented in Section 5.3. We would like to highlight that our Discussion already argues that dust fertilisation should not be discarded as a potential driver of productivity during the stratified season in the here-studied eastern tropical North Atlantic. In any case, as recommended by the reviewer, we have further reinforced this idea in the new version of the ms., as indicated below (in bold):

"**While our multivariate statistical analyses argue against dust being a dominant fertilising driver at seasonal scales, the absence of direct measurements of nutrient addition, iron bioavailability, or phytoplankton physiological responses means that the relative importance of fertilisation versus ballasting can only be partially resolved with the present dataset. Furthermore, individual events of enhanced UPZ/LPZ ratios during dust-associated export pulses may show transient fertilisation superimposed on ballasting during both upwelling-driven blooms as well as during the more stratified phase.** This was the case in February–March at both traps, in late July and early September at M1, and in May and July–August at CB (Fig. 10), suggesting species-specific responses to atmospheric inputs under both dry and wet depositional regimes. While episodic dust intrusions may have enhanced export at both sites primarily through ballasting, a fertilisation contribution cannot be fully ruled out, particularly during summer–autumn when UPZ/LPZ ratios increased sharply at both trap locations despite weakened or suppressed upwelling. This **interpretation** is consistent with recent evidence of dust-stimulated **fast-blooming species *E. huxleyi* and *Gephyrocapsa oceanica* together with N_2-fixing diazotrophic *Trichodesmium* sp. in the tropical NE Atlantic** (AMT28; Brotas et al., 2023; Guerreiro et al., 2023), with earlier sediment-trap records documenting sharp increases in POC, placolith coccoliths, UPZ/LPZ ratios**, and coccolith Sr/Ca ratios** during dust events in stratified tropical waters (Guerreiro et al., 2017, 2024), and with long-term satellite-derived co-occurring AOD and Chl-a maxima downwind of NW African dust sources (Guerreiro et al., 2023). **Additional direct observations combining higher-resolution sediment-trap records with time-series measurements from dust collectors at the surface and seawater biogeochemical properties across**

the photic zone, including e.g. particle aggregate composition analyses, and measurements of dust-associated nutrient and trace-metal fluxes, would however be required to more conclusively disentangle fertilization from mineral-ballasting effects."

Comments related to the writing: Some of the phrases are overcomplex or very long.

1. Abstract: Here we combine Lagrangian backtracking of satellite-derived chlorophyll-a (Chl-a), particulate inorganic carbon (PIC), and primary production (PP) with one year of sediment trap fluxes from Cape Blanc (CB) and M1, integrating lithogenic, organic, and coccolith species export with satellite-based upwelling, sea surface height (SSH), aerosol optical depth (AOD), and in situ water-column observations. What is M1 here? It is not defined in your abstract. this sentence is very long, I suggest splitting it into two sentences, one describing the lagrangian approach and one the datasets integrated.

This has been edited to:

"Here we combine Lagrangian backtracking of satellite-derived chlorophyll-*a* (Chl-*a*), particulate inorganic carbon (PIC), and primary production (PP) with one year of sediment trap fluxes of coccolith species, biogenic particles and lithogenic material (proxy for aeolian dust fluxes) from moorings CB (21ºN, 20ºW) and M1 (12ºN, 23ºW), representing distinct open-ocean settings offshore of NW Africa. These fluxes are further integrated with data from satellite-derived upwelling indices, sea surface height (SSH), aerosol optical depth (AOD), and in situ water-column observations collected at the trap locations."

2. some repetition, check lines 382, 391, and 591: notable notable differences, of of Chl-a, thus thus restricting

Done

3. What are the fast-blooming placolith-bearing coccolithophores? it is used everywhere but it would be useful to explain what it is before introducing it.

We thank the reviewer for this helpful comment. We agree that the term "fast-blooming placolith-bearing coccolithophores" may not be immediately clear for non-experts upon first appearance in the Abstract. However, in the context of marine phytoplankton ecology and ocean-colour studies, this terminology is commonly used to refer to opportunistic, rapidly responding coccolithophore taxa associated with productive conditions and relatively high growth rates. Given the space limitations and synthetic nature of the Abstract, we preferred to avoid introducing detailed taxonomic explanations at that stage. Instead, we slightly revised the wording to "fast-blooming (r-selected) placolith-forming coccolithophores", which better conveys the intended ecological meaning while maintaining conciseness. Representative taxa associated with this ecological group (e.g., *Emiliania huxleyi* and *Calcidiscus leptoporus*) are subsequently introduced and discussed later in the Abstract and throughout the manuscript.

4. line 655: can you modify surprisingly into unexpectedly?

Done.

5. The paper alternates between "lithogenic flux", "dust flux", "mineral dust", and "aeolian dust" when referring to essentially the same proxy measurement, if I am not mistaken. These terms are not really identical in the strict meaning, and the inconsistencies are a bit confusing. Can you please define which term is used as the dust proxy?

We appreciate this remark from the reviewer, and we are pleased to provide further context regarding our use of the terms "lithogenic flux" and "dust flux". Strictly speaking, the non-biogenic material collected by sediment traps is broadly referred to as "lithogenic", regardless of its origin. However, when traps are deployed in open-ocean settings, away from the influence of significant river discharge — as is the case for both CB and, even more clearly, M1 — the lithogenic particles captured are assumed to originate predominantly from atmospheric inputs, i.e. aeolian dust deposition. Given the location of the two traps, moored offshore of major desert dust source regions, this assumption is particularly well supported, as demonstrated by multiple studies conducted in the area over the past decades.

In line with this rationale, the Methods section of the revised manuscript explicitly states that the lithogenic flux fraction is used as a proxy for Saharan dust deposition at the M1 and CB trap sites (Section 3.1, line 242 of the revised manuscript). We have also included a more detailed explanation in the caption of Figure 2: "(…) and (e) lithogenic flux (orange bars), rain ratio ($CaCO_3$/POC, blue line), and the molar ratio between fluxes of biogenic silica and $CaCO_3$ ($bSiO_2/CaCO_3$, green line) used as a proxy for the ratio of silicifying to calcifying plankton (see Cermeño et al. 2008). The lithogenic flux (i.e., residual fraction) results from subtracting the weights of biogenic constituents (i.e., $CaCO_3$, organic matter and $bSiO_2$) from the total mass flux, and has been used as a proxy for dust deposition in the Atlantic Ocean (Jickells et al., 1998), including our study area (e.g., Fischer et al. 2016; Guerreiro et al. 2017, 2021; Korte et al. 2017). Data sources: Korte et al. (2017), Guerreiro et al. (2019, 2021)."

In the manuscript, our approach was to preferentially use the more generic term "lithogenic flux" when describing the results, and the term "dust flux" when discussing the data in the context of environmental drivers.

In any case, to improve the clarity of the manuscript, we have implemented the following edits throughout the text (highlighted in bold):

Abstract: "Here we combine Lagrangian backtracking of satellite-derived chlorophyll-a (Chl-*a*), particulate inorganic carbon (PIC), and primary production (PP) with one year of sediment trap fluxes of coccolith species, and biogenic **and lithogenic particles (proxy for aeolian dust)** from moorings CB (21ºN, 20ºW) and M1 (12ºN, 23ºW), representing distinct open-ocean settings offshore NW Africa. These fluxes are further integrated with data from satellite-derived upwelling, indices sea surface height (SSH), and aerosol optical depth (AOD), and in situ water-column observations collected at the trap locations."

Line 128-130: "Sediment traps remain the dominant method of directly quantifying temporal variations in bulk composition of vertical particle fluxes, including not only organic matter but also $CaCO_3$, biogenic silica, and lithogenic material, providing essential ground-truthing data for assessing biogeochemical responses to climate forcing **and to aeolian dust deposition in the ocean**."

Line 300: "(…) and the percentages of $CaCO_3$, $bSiO_2$, organic matter, and **dust** ~~(lithogenic particles)~~ together with satellite-derived parameters (…)"

6. Line 893: "contribute to refine" can be replaced by "improve our mechanistic understanding…."

Done

7. page 26 and 27: why are windy, dry months and warmer months in bold?

This was a mistake, and it has been corrected.

8. Figure 4 and 5 with the lagrangian maps, with 36 individual map panels per figure, these figures are extremely difficult to read. The individual panels are very small, making it nearly impossible to discern spatial detail in the trajectory patterns. The red dots indicating trap locations are very small and hard to locate consistently across panels. Maybe it would be best to move some of these to the supplementary.

We appreciate the feedback of the reviewer and we agree that the submitted figures could be improved. In this new version, we have reduced figures 4 and former Figures 5a-c from including 36 panels of two-week duration to only 20 panels of 1 month duration, slighlty extending the monitored period until May 2014 (thus improving our discussion on seasonal/interannual variability), while also reducing their eastward extend. We have also included former Figures 5a-c to Figures 5, 6 and 7. We believe the results are now efficiently shown in our paper.

9. Fig 6b,c, the x-axis labels are very small and densely packed.

The authors appreciate the reviewer's feedback and agree that the former Figs. 6b,c were not fully effective at representing the complex and seasonally variable water-mass connectivity between the coast and the sediment-trap locations. To address this issue, we replaced the former trajectory histogram plots with Hovmöller diagrams (new Fig. 8), which we believe provide a clearer and more spatially continuous representation of the spatiotemporal variability discussed throughout the manuscript.