

Airborne pollen, wildfires, and Saharan dust control atmospheric carbohydrates and shape their biogeochemical impacts in northern Mediterranean

General comments

This manuscript presents a comprehensive investigation of the occurrence, sources, seasonal variability, and deposition pathways (dry, wet, and total) of atmospheric carbohydrates in the northern Mediterranean coastal environment. The authors further evaluate the influence of three major regional atmospheric processes—high airborne pollen episodes, Saharan dust intrusions, and biomass burning—on carbohydrate concentrations and discuss their potential implications for coastal marine biogeochemistry.

The study clearly falls within the scope of ACP. It combines atmospheric aerosol chemistry, source apportionment, atmospheric deposition, and atmosphere–ocean interactions, while employing a comprehensive analytical approach based on PM₁₀ sampling, wet and total deposition collectors, carbohydrate biomarker analysis by HPAEC-PAD, dissolved carbohydrate determination, WSOC measurements, and meteorological and trajectory analyses. The experimental methodology is sound and the manuscript is generally well written and logically organized. The work also represents one of the few studies simultaneously addressing all deposition pathways while comparing three contrasting natural source types.

Overall, I consider the manuscript scientifically valuable and potentially suitable for publication in ACP. Nevertheless, several aspects require revision before publication. My main concerns relate to the interpretation of the results rather than to the quality of the measurements themselves. In several places, the discussion extends beyond what is directly supported by the data, particularly regarding the inferred biogeochemical impacts on coastal waters. The manuscript would also benefit from a more balanced discussion of methodological uncertainties, especially those associated with dry deposition estimates and the limited wet deposition dataset. Finally, several discussion sections could be condensed to avoid repetition and improve readability.

For these reasons, I recommend **Major Revision**.

Major comments

1. The title and several conclusions overstate the demonstrated biogeochemical impacts.

The manuscript repeatedly states that pollen, Saharan dust and biomass burning "shape biogeochemical impacts" or "deliver important pulses of bioavailable carbon". However, the study quantifies atmospheric deposition and carbohydrate composition; it does not directly measure microbial activity, carbon uptake, respiration, bacterial production, or any biological response in seawater. The presented data certainly demonstrate a potential delivery of labile carbon to coastal waters, but they do not demonstrate the magnitude of the resulting biogeochemical response. I therefore recommend moderating the wording throughout the manuscript. Expressions such as

- potential impacts on marine biogeochemistry,
- potential supply of bioavailable carbon,

- potential stimulation of microbial activity

would better reflect the actual evidence presented. This point is particularly relevant for both the title and the Abstract.

2. The limitations associated with the sampling period should be discussed more explicitly.

The field campaign spans only March to September 2024, intentionally covering the seasons of highest pollen activity, Saharan dust intrusions and wildfire occurrence. This is entirely reasonable for the objectives of the study. However, throughout the discussion the authors occasionally generalize their conclusions to the northern Mediterranean as a whole. Since autumn and winter were not sampled, annual carbohydrate budgets and seasonal source contributions cannot be fully assessed. I suggest adding a short paragraph discussing how the restricted sampling period may influence the interpretation of seasonal variability and annual deposition estimates.

3. Dry deposition estimates carry considerable uncertainty.

Dry deposition fluxes are calculated using fixed deposition velocities derived from literature values (0.1 and 2 cm s⁻¹). Although these values are widely used and properly referenced, deposition velocities remain one of the largest sources of uncertainty in dry deposition calculations. The manuscript briefly mentions uncertainties approaching 100%, but the implications of this uncertainty are not discussed further. The authors should comment on how sensitive the calculated deposition fluxes may be to the selected deposition velocities and whether this uncertainty could influence the comparison between dry, wet and total deposition.

4. Statistical robustness of wet deposition results.

Only eight wet deposition samples were collected during the entire campaign, and several event categories contain only two or three observations. Consequently, several comparisons rely on very limited sample sizes. Although the authors acknowledge this limitation in some places, the discussion occasionally presents these differences as robust findings. I recommend consistently emphasizing the exploratory nature of these comparisons and avoiding strong conclusions where statistical power is clearly limited.

5. Source apportionment discussion is often repetitive.

The discussion repeatedly explains the terrestrial origin of glucose, the association of glucose with pollen, and the distinction between marine and terrestrial carbohydrate sources. While scientifically correct, much of this information is repeated across several paragraphs (especially Sections 3.2.1 and subsequent source-specific discussions). The manuscript would benefit from condensing these sections and avoiding reiteration of arguments already established earlier.

6. Significance versus environmental relevance.

Several percentage increases reported throughout the manuscript appear impressive (for example increases exceeding 200% during pollen episodes). However, some of these increases originate from relatively low background concentrations. The discussion should distinguish more clearly between statistical significance and ecological significance.

Minor comments

Abstract

The abstract is well structured but should avoid wording implying demonstrated ecosystem responses.

Introduction

The introduction is comprehensive and well referenced. However, it could conclude with one or two explicit hypotheses rather than only listing the study objectives.

Results

Tables are generally clear.

Nevertheless, the manuscript sometimes reports percentage increases without simultaneously indicating the corresponding absolute concentrations.

Including both values would improve interpretation.

Discussion

The discussion is scientifically rich but could be shortened by approximately 15–20%. Several paragraphs summarize literature that has already been introduced in previous sections.

Conclusions

The conclusions generally summarize the results well. However, I recommend distinguishing more clearly between observations directly supported by measurements and broader ecological implications.