

Diurnal sea breeze worsens coastal air quality and complicates monitoring of background North Atlantic air

Overall comment

This manuscript presents a valuable long-term observational analysis of diurnal sea-breeze (DSB) effects at Penlee Point, using 10 years of coastal measurements and identifying 428 DSB days. The paper connects mesoscale coastal meteorology with both air-quality exceedance and the representativeness of a background marine monitoring site. The central result is that DSB days are associated with higher daytime concentrations of multiple pollutants and altered interpretation of southwest-sector “background” air.

The **primary strengths of the paper** include:

- The manuscript uses an unusually long observational record for this topic and addresses both air quality and background-atmosphere representativeness in one study.
- The seasonal analysis of DSB occurrence and timing is useful, and the figures clearly show that DSB occurrence is concentrated in spring–summer and linked to cooler sea than land, low wind speed, and strong irradiance.
- The comparison between PPAO and Plymouth City Centre is a strong aspect of the paper.

However, the manuscript currently **exhibits weaknesses** in its **methodological justifications, specifically regarding air-sea flux algorithms, and the interpretation of model-data discrepancies**. My overall recommendation would be **Major Revision**. The next version of the manuscript must address these three critical points:

- **Methodological Validation:** Justify the use of the **COARE3.5 bulk flux algorithm** in a coastal setting and provide a sensitivity analysis for the DSB identification thresholds.
- **Forecasting Critique:** Link the identified failures in the **ECMWF/CAMS** modeling chain (steps 2-13 hours) to specific risks in public health advisories and coastal air quality management.
- **Chemical Attribution:** Use the specific rates of O₃ increase (**2-3 ppb hr⁻¹**) and the **two-fold increase in SO₂** to more clearly distinguish between transport, shipping emissions, and natural cycles.

Major comment

- **Supporting identify the site's location might be support for the paper description:** The authors need to add the map to indication the different characteristic between the sites. This can support to answer the question that how these sites are necessary for the DLB observation.
- **Observational coverage is uneven across variables and needs to be handled more transparently:** The manuscript combines datasets with very different temporal coverage: NO_x is available only from 2021-2024, aerosol number from 2015-2017 with gaps, and solar irradiance only from 2014-2015. However, some figures and statements are phrased broadly as though all variables span the full decade, for example, “over the entire measurement period” in **Figure 6**.
- **The paper needs to clarify the DSB Identification Criteria and Sensitivity:** The authors employ a three-point operational classification system based on meridional velocity shifts and a wind direction standard deviation exceeding 30°. While this provides a structured approach, it lacks a sensitivity analysis. I am concerned that the 30° thresholds may exclude "near-miss" events, days on which circulation shifts occur but are slightly less pronounced, that still significantly alter the chemical environment. The authors should clarify if they tested alternative thresholds and discuss the potential for underestimating the aggregate impact of sea-breeze-like circulations on local air quality
- **The paper did not separate the DSB effect from the fair-weather and stagnant condition:** The authors acknowledge that DSB events tend to occur during fair weather, with higher temperature, pressure, and irradiance and lower wind speed, humidity, and rainfall. They then interpret higher daytime pollutant levels during DSB as resulting from insufficient nocturnal dispersion plus photochemistry and recirculation. This is plausible, but the analysis as written does not convincingly isolate DSB from coincident meteorological conditions that would also favor pollution buildup even without a true sea-breeze circulation. I would encourage the authors to add one of the following:
 - A matched comparison against non-DSB days with similar temperature, radiation, and wind speed;
 - A multivariate/statistical framework;
 - or at minimum a more cautious phrasing distinguishing “DSB-associated” from “caused by DSB.”

- **Evaluation of Boundary Layer Methodology (COARE3.5):** In **Section 3.2**, the authors utilize the COARE3.5 bulk flux algorithm to calculate sensible heat flux and the Obukhov length (Z/L) stability parameter. This algorithm designed for the open ocean (marine environment), while PPAO is a complex coastal environment. PPAO is characterized by land-sea transitions and potential fetch-limited conditions, so is this method suitable for PPAO? The authors need to justify the validity of this open-ocean model for their site or discuss how coastal influences might bias the calculated stability parameters and heat fluxes.
- **Clarify the explanation in Figure 1:** According to the author mentioned, the DSB usually associated with the high temperature, so why in the **Figure 1**, there are some years, DSB peak occurs earlier than the high temperature from 1 to 2 months. Is there any reason for these time lag? Additionally, why 2018, 2021, and 2022 the number of the DSB cases were observed to be much higher than other years? Is there any mechanism that control this pattern?
- **Solar Irradiance and Climatological Gaps:** Solar irradiance data were only available for the 2014 - 2015 period. In **Figure 2**, the authors correlate DSB frequency with solar cycles across the full decade. It is unclear how they bridged this data gap. The authors must explicitly state whether they used a two-year climatological mean or another proxy for the 2016 - 2024 period in their correlations.
- **Analysis of Discrepancies in Model Forecasting:** **Section 4.1** reveals a significant failure of the ECMWF short-range forecast (specifically, steps 2 to 13 hours) to capture the abruptness of DSB transitions in August 2022. This systematic error has profound implications for the reliability of CAMS air quality forecasts mentioned in the text. The authors must expand their discussion on the risks this poses to public health management. If operational models consistently miss these transitions, the resulting pollution forecasts for coastal populations are fundamentally compromised.
- **O₃ Dynamics and Background Air:** The manuscript reports a morning O₃ increase of 2 to 3 ppb hr⁻¹, noting it is too rapid for local photochemistry alone. The authors need to provide a clearer partitioning between advection and local chemistry to explain this rate. Furthermore, the claim that PPAO observations must exclude DSB events to represent "background" North Atlantic air is problematic. Given that DSB events occur on up to 40%

of days during peak seasons, excluding them may create an idealized rather than representative background. The authors should offer a more nuanced justification for why a seasonal, persistent circulation pattern should be stripped from the regional baseline.

- **The definition of Ox appears chemically problematic:** A major concern is the manuscript's use of $Ox = O_3 + NOx$ in the abstract and main text. If the goal is to reduce the influence of NO titration, the conventional metric is usually $O_3 + NO_2$, not $O_3 + NOx$. Using $O_3 + NOx$ does not properly remove the effect of rapid O_3 -NO- NO_2 partitioning.
- **Clarification of Anthropogenic vs. Natural Signals:** The manuscript notes that SO_2 concentrations during DSB events were more than twice as high as during periods of onshore flow. While the authors suggest slow SO_2 destruction in dry conditions and recirculation as causes, they should more definitively address the role of shipping emissions. Given this substantial multiplier, the authors should discuss whether the diurnal cycle in aerosol numbers during southwest winds (**Section 4.1**) is more likely driven by shipping emissions rather than natural coastal halogens.
- **Clarify the purpose of the LSB classification:** Please clarify the scientific rationale for separating the analysis into offshore flow, onshore flow, and with/without sea-breeze cases. Since offshore/onshore wind direction is already part of the DSB circulation, it is not fully clear what additional physical insight is gained by treating these categories separately. Are the offshore/onshore composites intended as meteorological controls, or as proxies for land-influenced versus background marine air?
- **The air-quality exceedance analysis needs clearer definitions:** The manuscript states that O_3 exceeds the WHO 8-h mean guideline 78% of the time at PPAO, while the EU limit is exceeded 0.8% of the time overall; then “daytime only during DSB events over the entire calendar year” increases the EU exceedance to 2.4%, with 8% in April-June. $PM_{2.5}$ daily exceedance in Plymouth rises from 3.1% overall to 4.6% for DSB events. This limit exceedance's part is difficult to digest. I request that the authors include a structured Markdown table in their revision to compare exceedance rates at PPAO and Plymouth City Centre during DSB versus Non-DSB days. This would significantly improve the readability of the public health impact section. Additionally, the section mixes “**all time**,” “**daytime only**,” “**calendar year**,” and “**DSB events only**,” which makes the comparison hard to interpret rigorously. The authors also need to answer that “**whether the**

comparisons are statistically significant given the unequal sample sizes”.

- **Some interpretive claims are stronger than the evidence currently shown:**
 - The statement that the morning O₃ increase of 2 - 3 ppb h⁻¹ is “much too rapid” for photochemical production alone is plausible, but no quantitative benchmark or budget analysis is shown.
 - The claim that because ECMWF forecast winds miss the DSB shift, CAMS pollutant forecasts “will also be inaccurate” is reasonable but not demonstrated in this paper.
 - The conclusion that removing DSB events makes the record more representative of background North Atlantic air is likely correct, but that interpretation would be much stronger with trajectory analysis, boundary-layer information, or an independent marine tracer.

Major comment

- The manuscript should clarify the sign convention for meridional wind more explicitly when defining positive and negative flow in the DSB criteria.
- The use of approximately weekly SST measurements from station L4, 6 km south of PPAO, may be too coarse for discussing event-scale sea-air contrast and heat fluxes. The authors should justify this more carefully, especially where mechanism is discussed.
- Please report the number of DSB cases per season in **Section 3.3**, since the text notes that winter results are less certain because few events occur.
- **Figure 4:** The “abrupt” versus “gradual” velocity transitions described in the text should be more clearly marked or highlighted in the visual representation.
- **Figures 6-8** would benefit from uncertainty shading, confidence intervals, or at least sample counts.
- There are several language edits needed:
 - **PM2.5 and PM10** should be PM_{2.5} and PM₁₀
 - “**more than twice as higher**” should be “more than twice as high.”
 - “**We can to an extent account...**” should be rephrased.
 - **Figure 7** caption: “diurnals cyles” should be “diurnal cycles.”

- **“PPAO are archived”** should be **“PPAO data are archived.”**
- **Figure A3:** **“effecting most pronounced”** appears to be a typo.