We thank the referee for their comprehensive review with highly constructive comments and suggestions, which has helped to improve our paper. Please see our replies below in blue.

Reviewer 1

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.

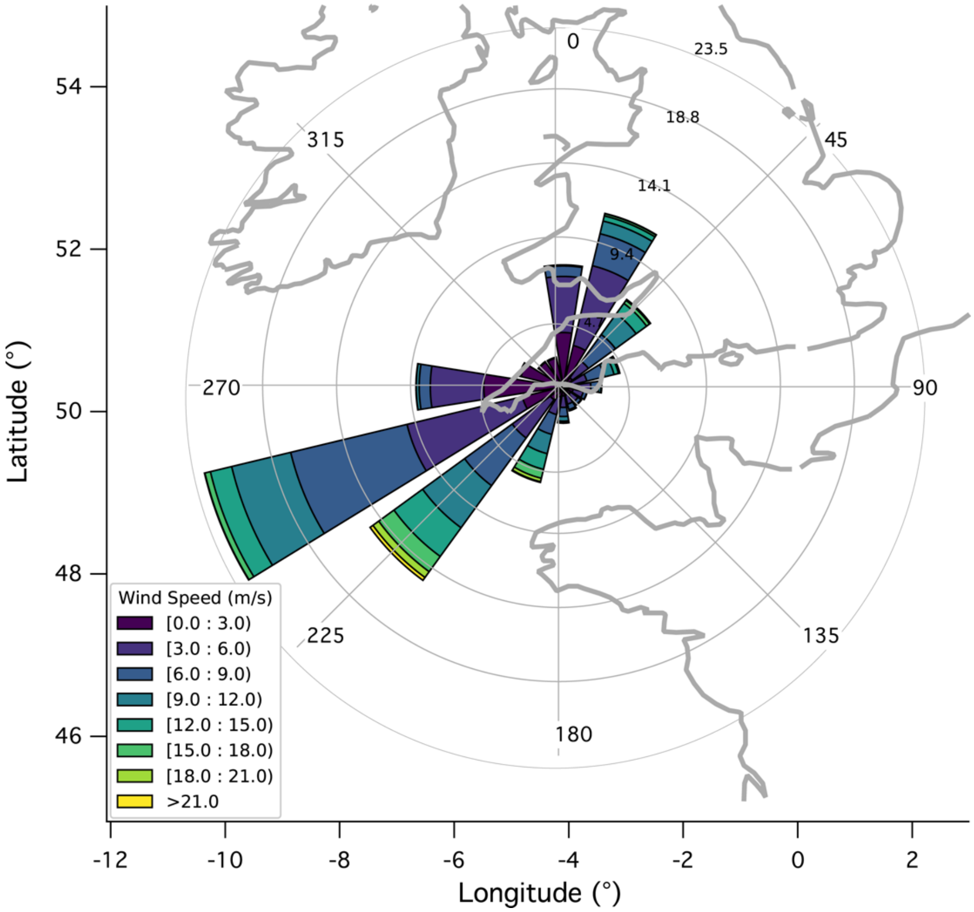
• Forecasting Critique: Link the identified failures in the ECMWF/CAMS modeling chain (steps 2-13 hours) to specific risks in public health advisories and coastal air quality management.

• 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.

We have added the following figure to the Appendix.



Map of the study region, with PPAO located at the center of the wind rose. The wind rose, based on 10 years of observations, shows that the predominant wind direction here is from the southwest (210-260° deg further referred to 'onshore'), followed by northeast (330-60° referred to as 'offshore').

➢ Observational coverage is uneven across variables and needs to be handled more transparently: The manuscript combines datasets with very different temporal coverage: NOx 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.

We have now specified the different respective observational coverages for the different data streams in the captions. For example, the new caption for Figure 6 reads:

'Mean diurnal cycles at PPAO during and outside of sea breeze events for May, June, and July (a) O3 [2014-2024]; (b) NOx [2021-2024]; (c) Ox [2021-2024]; (d) aerosol number concentration [2015-2017]; (e) CH4 [2014-2024]; (f) CO2 [2014-2024]; (g) SO2 [2014-2024]. Mean diurnal cycles during offshore wind flow (hourly wind direction from NE, specifically 330 to 60°) and onshore wind flow (hourly wind direction from SW, specifically 210 to 260°) are also shown.'

➢ 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 30deg. While this provides a structured approach, it lacks a sensitivity analysis. I am concerned that the 30deg 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

We have added the following:
'The identification of sea breeze events is not very sensitive to this standard deviation criterion. Increasing the threshold to 40 deg or reducing it to 20 deg would lead to roughly a 10% reduction/increase in the number of DSB days identified.'

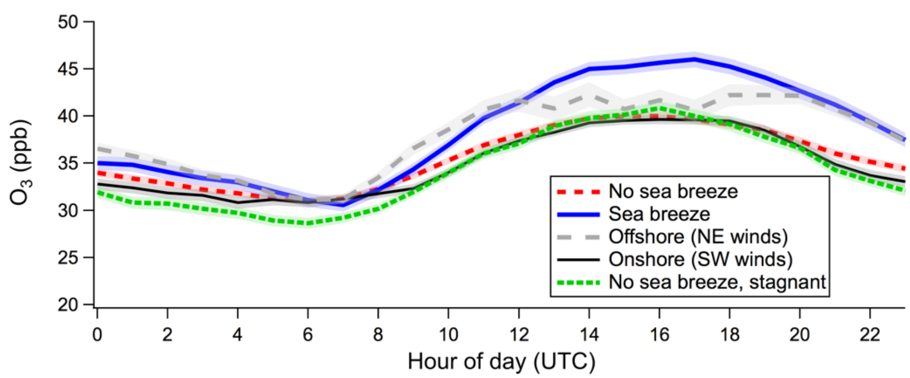
➢ The paper did not separate the DSB effect from the fair-weather and stagnant condition: The authors acknowledge that DSB events tend to occur during fair weather, with higher temperature, pressure, and irradiance and lower wind speed, humidity, and rainfall. They then interpret higher daytime pollutant 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:

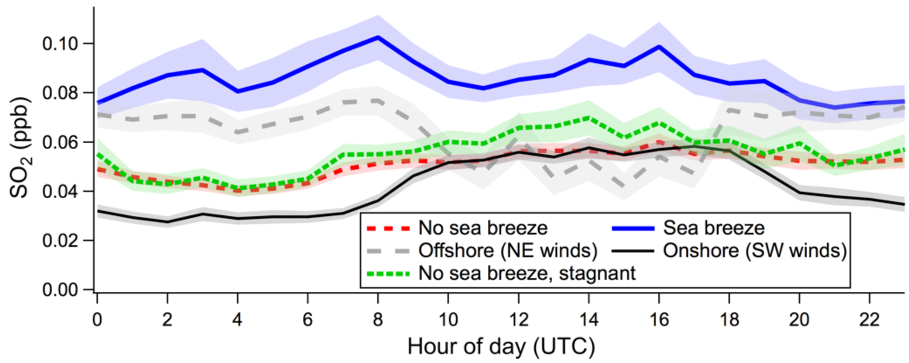
o A matched comparison against non-DSB days with similar temperature, radiation, and wind speed;

o Amultivariate/statisticalframework;

o or at minimum a more cautious phrasing distinguishing "DSB-associated" from "caused by DSB."

Thanks for the good question. Below we've selected non-DSB days between May to July with comparable wind speed and air temperature as DSB days. As shown in the examples below for O3 and SO2, clearly just having fair weather and stagnant conditions do not cause the elevated pollution that is characteristic of DSB days.





Diurnal cycles of O3 and SO2 at PPAO. Shading corresponds to standard errors.

We have added the following to the main text:

“To assess how much of the elevated pollution on DSB days is driven by stagnant conditions and fair weather alone rather than due to DSB circulation, we select non-DSB days between May to July with comparable wind speed and air temperature as DSB days. These stagnant periods with no DSB do not have pollution as elevated as on DSB days.”

➢ 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.

Thanks for the question. While the use of the COARE algorithm that has been tuned to the open ocean for a coastal environment may lead to some bias, any bias will most likely be in the magnitude of the transfer coefficients, but not in the sign of heat flux (or in turn L in the Z/L term). Here we have used the COARE prediction of heat flux and Z/L only in a semi-quantitative way to assess their influence on the occurrence of DSB events. We feel that the COARE model outputs are sufficient for this purpose.

➢ 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?

Figures 1-3 show that the sea-air temperature difference is a much stronger explanatory parameter for DSB (with little lag) than air temperature alone, which leads to the marked seasonality in DSB occurrence.

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?

The interannual variability in DSB occurrence is very intriguing indeed, and is likely due to a combination of different factors, including the sea-air temperature difference and prevailing winds during the spring/summer months. In the new Figure 1, we have added the relative wind speed and nighttime wind direction (as a proxy for the prevailing wind direction in the absence of DSB). The quite negative sea-air temperature difference and prevailing weak winds from the northeast (and correspondingly low relative humidity) in spring/summer 2018 was conducive for DSB formation. 2021 and 2022 also had periods of weak winds from the northeast. In contrast, even though the sea-air temperature difference was very negative in 2023, the prevailing winds were mostly from the southwest in the spring/summer period, and thus did not result in DSB per our operational definition.

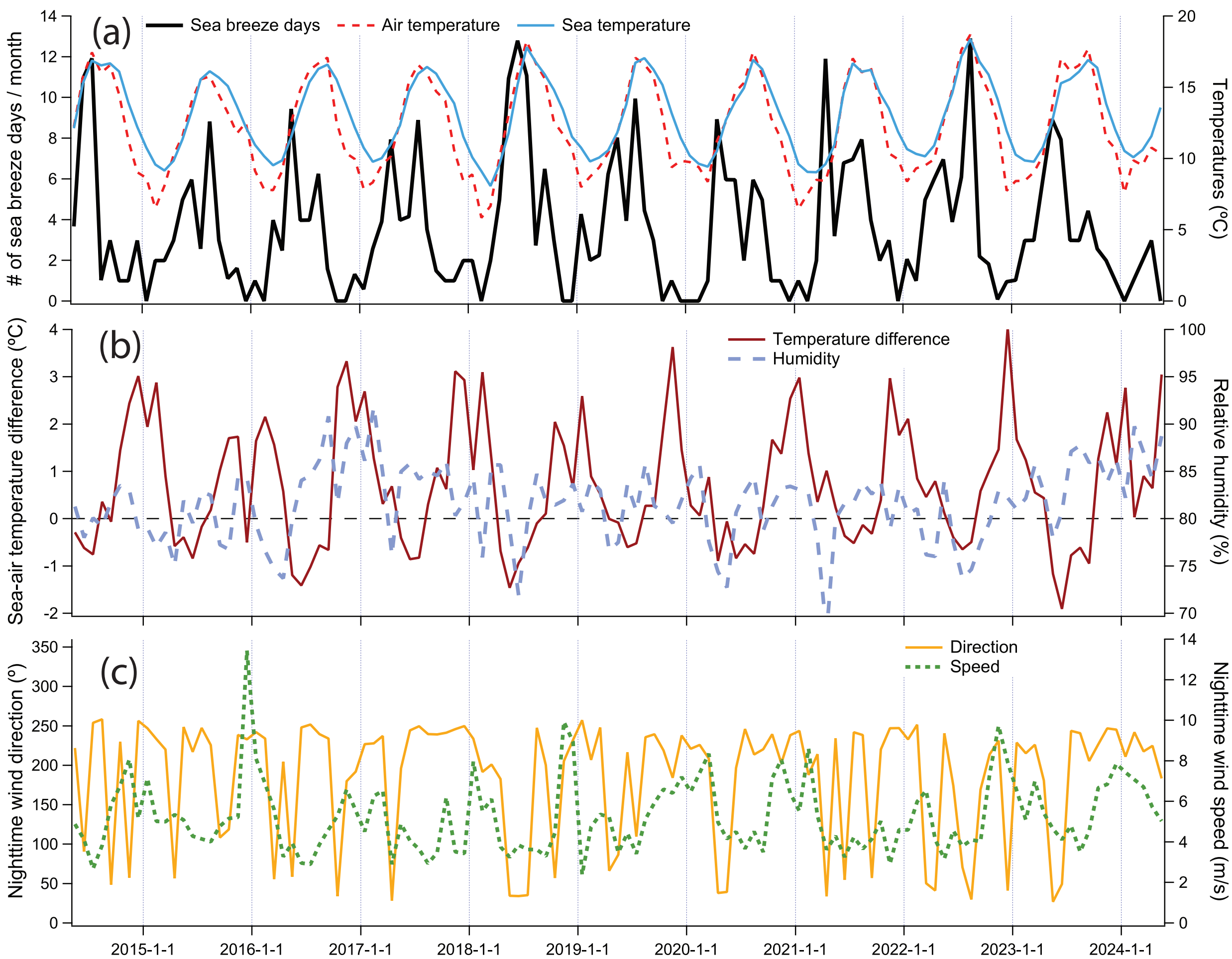


Figure 1. Monthly time series of (a) Sea breeze days identified using PPAO data; (b) sea-air temperature difference and relative humidity; (c) median nighttime wind direction and wind

speed. Air temperature, humidity, and wind measurements were from PPAO, while near surface sea temperature was from L4.

➢ 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.

Here we made the implicit assumption that the solar irradiance data from the two years of measurements are reasonably representative of the mean seasonal cycle (Figure 2) and diurnal cycle (Figure 4) of light for the entire 10 years. We feel that this is reasonable since over seasonal and diurnal timescales, the cycles in solar irradiance should be very consistent.

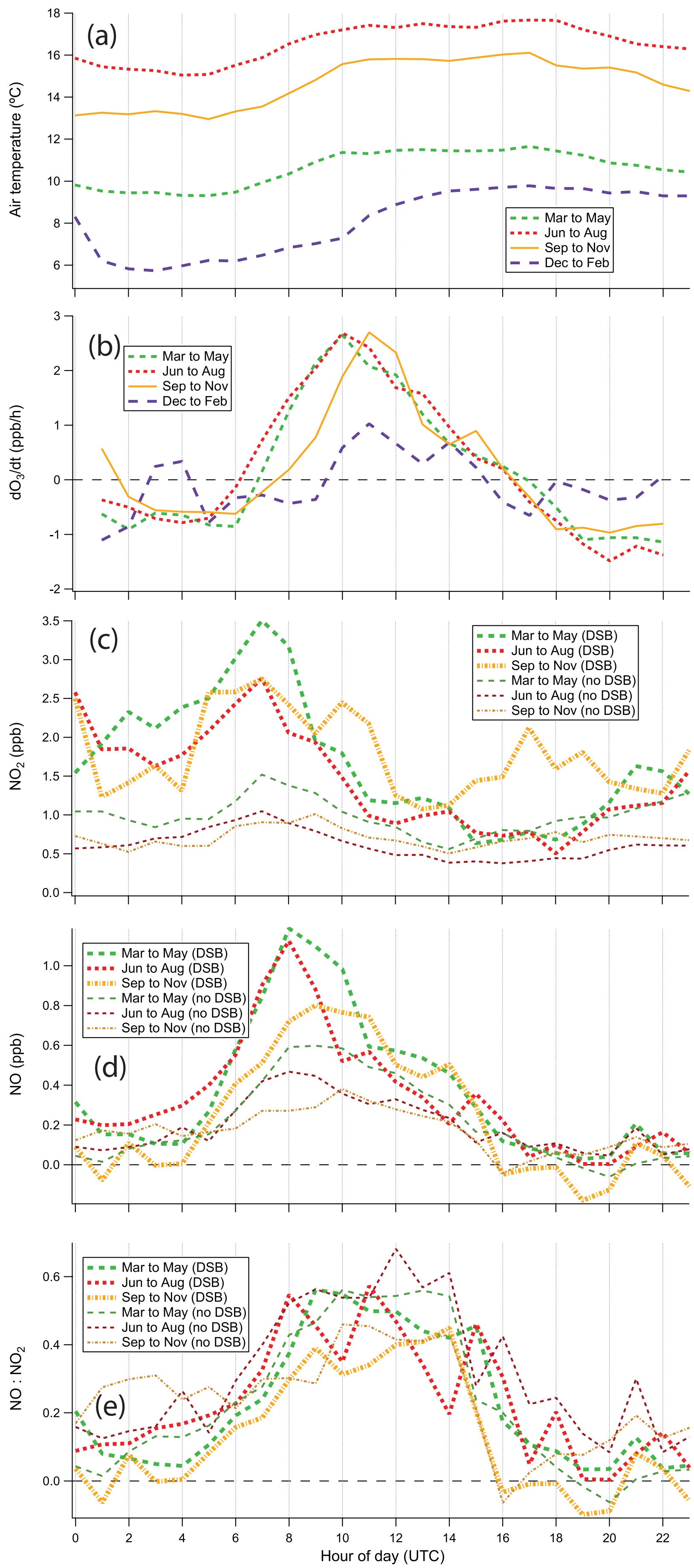
➢ Analysis of Discrepancies in Model Forecasting: Section 4.1 reveals a significant failure of the ECMWF short-range forecast (specifically, steps 2 to 13 hours) to capture the abruptness of DSB transitions in August 2022. This systematic error has profound implications for the reliability of CAMS air 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.

That operational numerical weather prediction models struggle to fully capture DSB events have been reported elsewhere (e.g. the very recent work of Brown et al. 2026). We agree with the reviewer that it is important to comprehensively assess the importance of this potential bias in the CAMS air quality forecast, but feel that it is beyond the scope of this paper.

Brown, A., Vincent, C., and Short, E.: Identifying sea breezes from atmospheric model output (sea_breeze v1.1), Geosci. Model Dev., 19, 933–953, https://doi.org/10.5194/gmd-19-933-2026, 2026.

➢ O_3 Dynamics and Background Air: The manuscript reports a morning O_3 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.

We have now added the net rate of change in O_3 (dO_3/dt) and the $NO:NO_2$ ratio to the appendix and the following to the text.



(a)
Air temperature (ºC)
Mar to May
Jun to Aug
Sep to Nov
Dec to Feb
(b)
dO3/dt (ppb/h)
(c)
NO2 (ppb)
Mar to May (DSB)
Jun to Aug (DSB)
Sep to Nov (DSB)
Mar to May (no DSB)
Jun to Aug (no DSB)
Sep to Nov (no DSB)
(d)
NO (ppb)
(e)
NO : NO2
Hour of day (UTC)

Figure A3. Diurnal cycles of (a) air temperature during DSB days divided by different seasons; (b) net rate of change in O3 during DSB and non-DSB days; (c) NO2 during DSB and non-DSB days; (d) NO during DSB and non-DSB days; (e) NO:NO2 ratio during DSB and non-DSB days. Note that the O3 data spans the entire 10 years, while the NO2 and NO data are from 2021 to 2024.

"During the warmer months, the net rate of change in O3 is positive throughout the day, and the peak in dO3/dt in the morning coincides with when the winds change from offshore to onshore during DSB events. The peak net rate of change in O3 during DSB days is about twice as high as during non-DSB days for a given season, largely due to the higher NOx concentrations accumulated during DSB events."

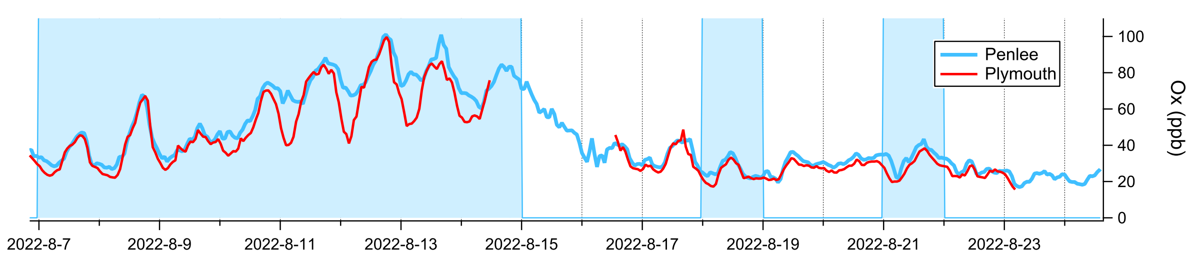
We agree that the phrase 'much too rapid' is too strong and have now changed it to 'more rapid than on non-DSB days'.

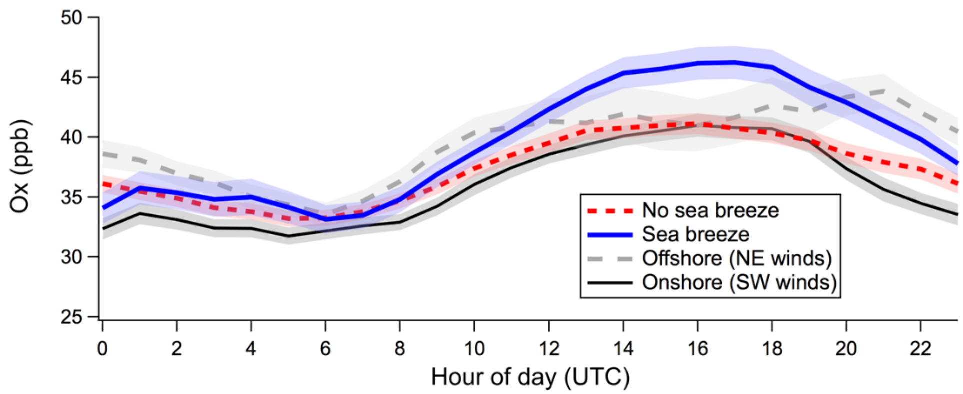
Furthermore, the claim that PPAO observations must exclude DSB events to represent "background" North Atlantic air is problematic. Given that DSB events occur on up to 40% of days during peak seasons, excluding them may create an idealized rather than representative background. The authors should offer a more nuanced justification for why a seasonal, persistent circulation pattern should be stripped from the regional baseline.

We see the confusion and have changed the wording from 'background' North Atlantic air to 'remote' North Atlantic air. In other words, how representative are observations from PPAO of the open ocean North Atlantic, far away from the coast.

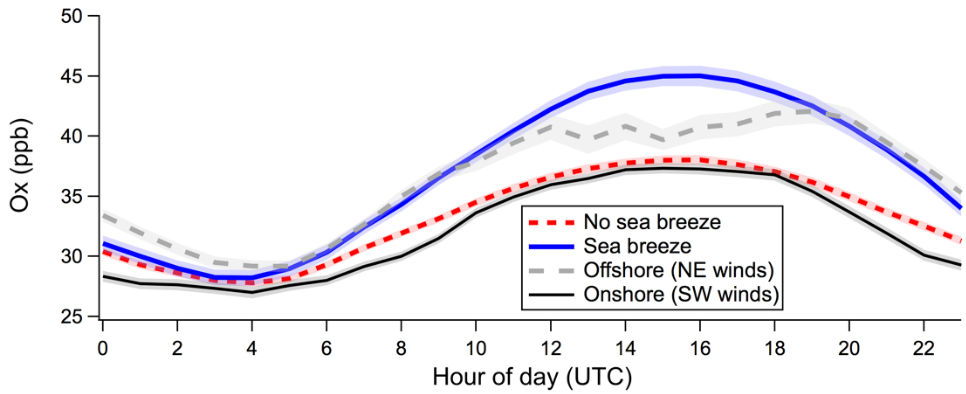
➢ The definition of Ox appears chemically problematic: A major concern is the manuscript's use of Ox = O3 + NOx in the abstract and main text. If the goal is to reduce the influence of NO titration, the conventional metric is usually O3 + NO2, not O3 + NOx. Using O3 + NOx does not properly remove the effect of rapid O3-NO-NO2 partitioning.

We apologize for the error and have changed all incidences of Ox = O3 + NOx to Ox = O3 + NO2, which doesn't modify our main findings. See below.





Diurnal cycles at PPAO.



Diurnal cycles at city centre.

➢ Clarification of Anthropogenic vs. Natural Signals: The manuscript notes that SO2 concentrations during DSB events were more than twice as high as during periods of onshore flow. While the authors suggest slow SO2 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.

Thanks for the question. We have looked at our data in more detail and added the following.

'Halogen emissions from the coast contribute to this new particle formation, as PPAO data show enhanced aerosol number concentration when the tidal height is less than ~2 m. However, removing these low tide data (20% of the time) lowers the mean aerosol number concentration at around midday by only ca. 200 cm^{-3}, and does not eliminate bulk of the diurnal cycle. Thus the large diurnal cycles in aerosol number concentration may be mostly driven by ship-emitted SO_2, which is roughly twice as high on DSB days than non-DSB days. We note that the aerosol number concentration data here were collected prior to the 2020 International Maritime Organisation regulation on ship sulfur emission reduction. Further aerosol number concentration observations post regulation as well as composition measurements of nucleation mode aerosols would shed light on the sources of new particles at PPAO.'

➢ 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 rationale for looking at the offshore/onshore composites in addition to DSB/non-DSB events is to some extent separate the impact of larger scale transport from the smaller scale circulation from DSB. For example, the fact that during the daytime the pollutants are generally higher during DSB events than during offshore or onshore flow demonstrate the unique effect of DSB themselves. We have added the following to the main text:

"To separate the impact of larger scale transport from the smaller scale circulation from DSB, we select for periods with either offshore or onshore flow and compare their means to DSB/non-DSB events."

➢ The air-quality exceedance analysis needs clearer definitions: The manuscript states that O3 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. PM2.5 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".

Thanks for the comment. We have added the following table, as requested. We have further removed the exceedance statistics for PM2.5, as the DSB effect on them seems fairly weak.

Table 1. O_3 exceedance at PPAO over EU 8-h mean (60 ppb)

	Day and night, all seasons	Daytime only, all seasons	Daytime only, April to June
DSB days	2.7%	2.4%	7.7%
Non-DSB days	0.5%	0.3%	0.8
All observations	0.8%	0.6%	4.4%

"We can further evaluate the impact of DSB in terms of air quality regulation exceedance. At PPAO, out of the measured pollutants O_3 is the parameter that exceeds regulation the most frequently. The European Union (EU, https://environment.ec.europa.eu/topics/air/air-quality/eu-air-quality-standards_en) limit for O_3 (eight hour mean) is 60 ppb. Considering all observations, the PPAO measurements exceed that limit 0.8% of the time. When limiting the observations to daytime only during DSB events, the rates of exceedance across all seasons increases to 2.4% due to episodes of very high O_3. The highest exceedance was found in the daytime between April and June during DSB events (7.7%), when the background O_3 concentration is already elevated in this region and the DSB events occurring most frequently."

➢ Some interpretive claims are stronger than the evidence currently shown:
o 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.

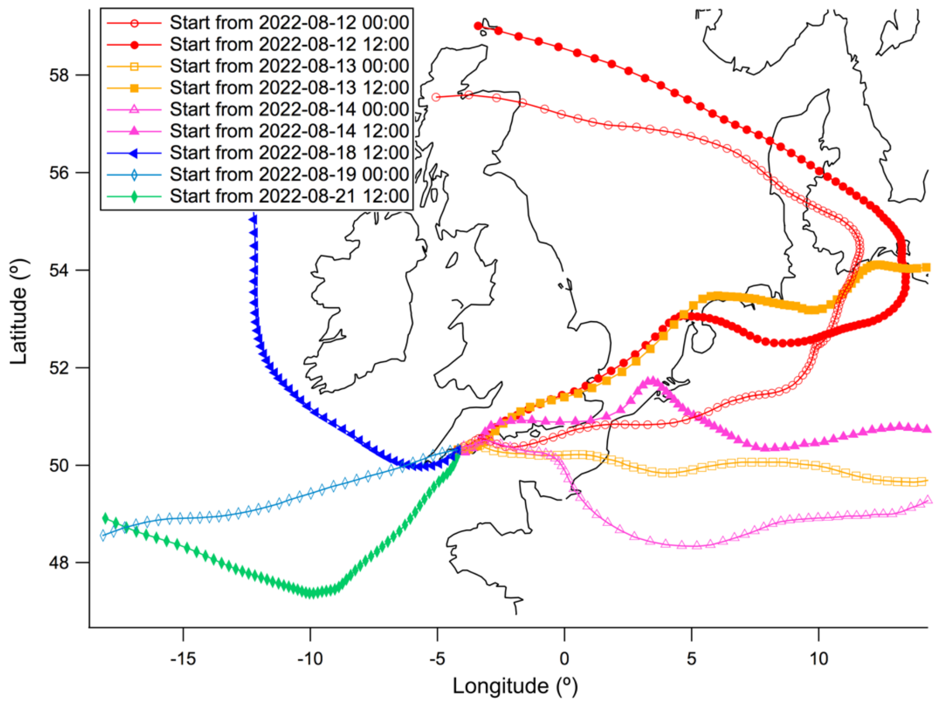
We have now removed this phrase. Please see above for separation of the effects of advection vs. chemistry.

o 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.

Please see above where this been addressed.

o 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.
Thanks for the suggestions. We ran some HYSPLIT back trajectories and will add the following figure to the appendix. Even using the high resolution GFS meteorological data (0.25 deg resolution), the trajectories do not well capture the sea breeze events. Trajectories started between 12 and 14 August 2022 only showed the prevailing (nighttime) northeasterly winds, and do not show the daytime switch to onshore flow. Trajectories started between 18 and 19 August 2022 showed winds mostly from the North Atlantic, and did not capture the briefly periods when the winds switched to offshore at night/predawn.

Nearby boundary layer information would've been useful but were not available during our study period. Finally, in our opinion the methane mixing ratio acts like an independence tracer for terrestrial influence. We are not sure what an independent marine tracer might look like. DMS might be useful but was not measured long term at PPAO.



Major comment

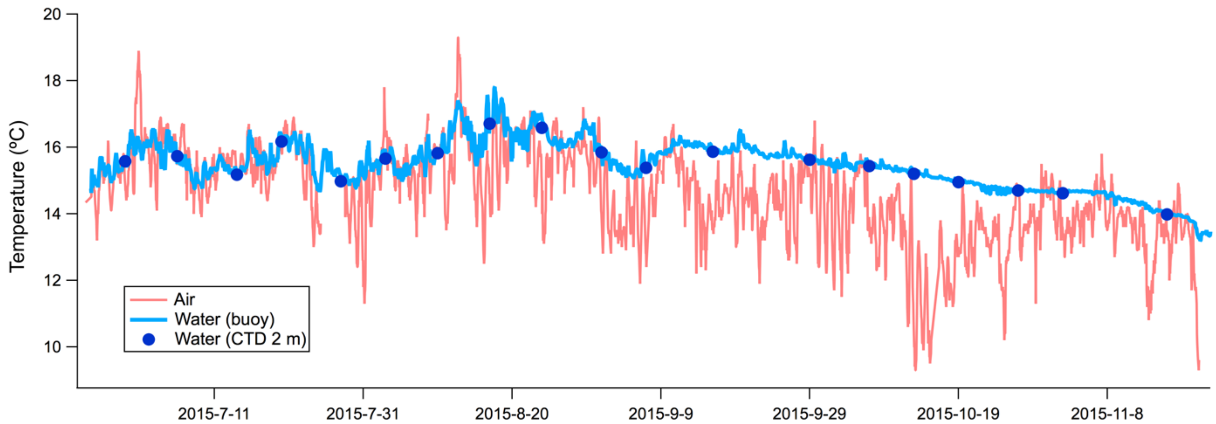
➢ The manuscript should clarify the sign convention for meridional wind more explicitly when defining positive and negative flow in the DSB criteria.

These have been defined already in the selection of DSB events:

1. Minimum meridional wind velocity is negative (from south to north), occurring during the daytime between 10:00 and 18:00 UTC, and
2. Maximum meridional wind velocity is positive (from north to south), occurring at night between 00:00 and 08:00 UTC

➢ 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.

Thanks for the comment. We agree that higher resolution SST data would've been preferable, but they were not consistently available over the 10 year period. We have looked at a period of several months in 2015 when high resolution SST data from the L4 buoy were also available, which is shown below. While there are variations in SST on short timescales, especially in the summer months, most of the variability in the sea-air temperature difference (a key drive for DSB formation) is due to variability in air temperature, which was always measured at a high frequency.



➢ 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.

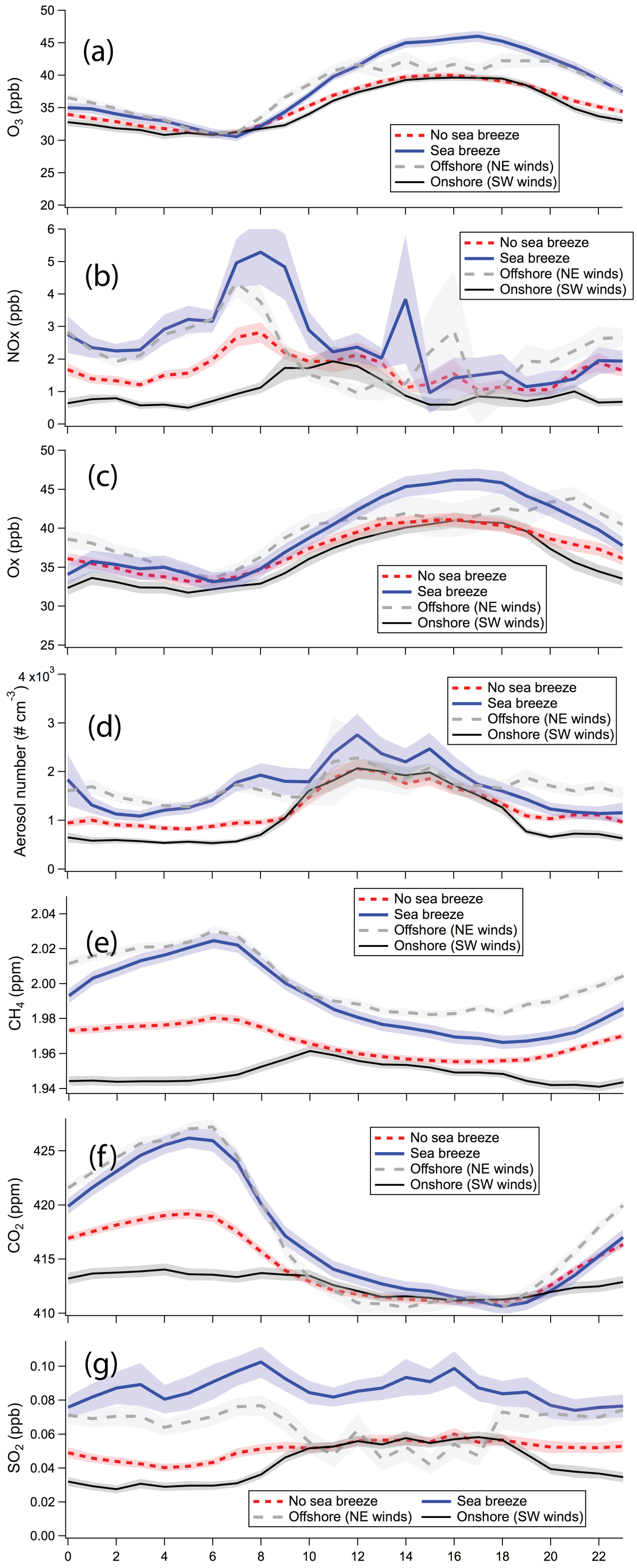
We refer readers to the legend in Figure 4, which show the number of days for different seasons.

➢ Figure 4: The "abrupt" versus "gradual" velocity transitions described in the text should be more clearly marked or highlighted in the visual representation.

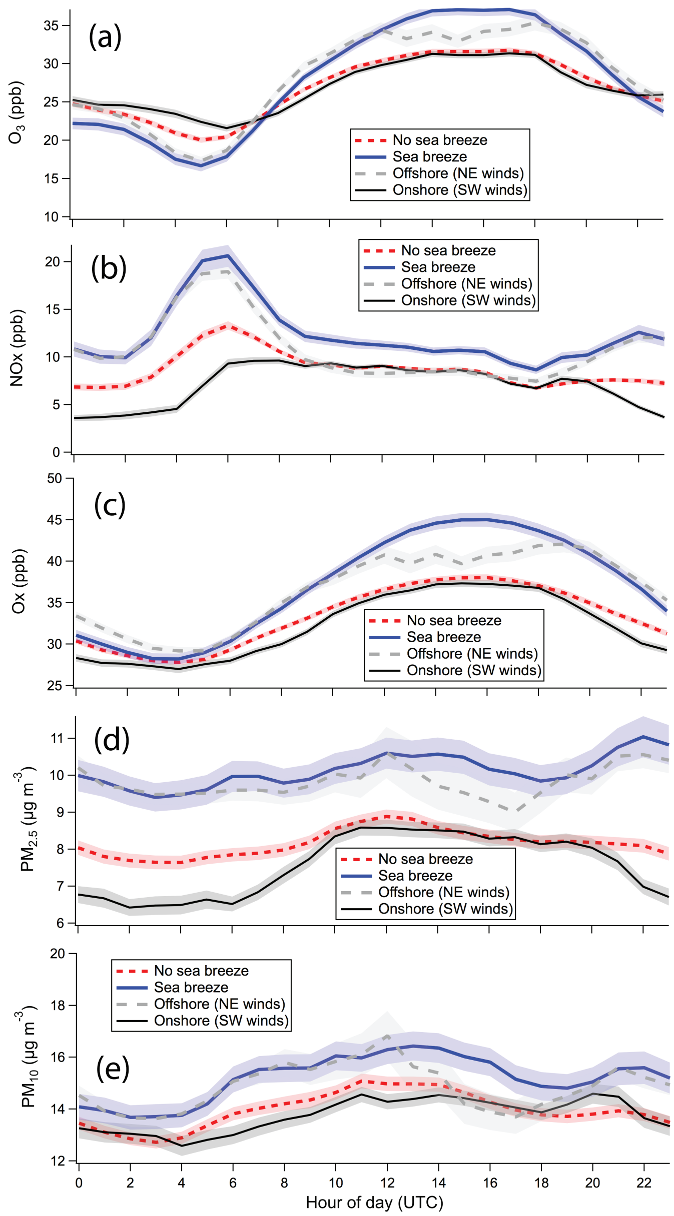
To avoid adding too much clutter to the plots, we now additionally specify the time periods when these transitions occur.

➢ Figures 6-8 would benefit from uncertainty shading, confidence intervals, or at least sample counts.

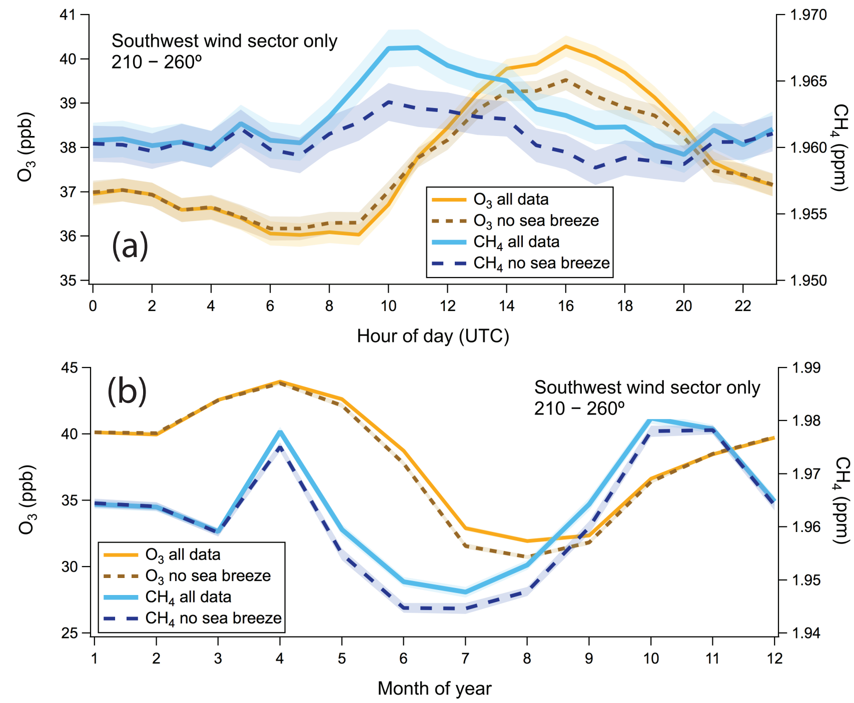
We have added uncertainty shading, where shading corresponds to standard error. Please see below.



(a)
(b)
(c)
(d)
(e)
(f)
(g)
O3 (ppb)
NOx (ppb)
Ox (ppb)
Aerosol number (# cm-3)
4 x10^3
CH4 (ppm)
CO2 (ppm)
SO2 (ppb)
No sea breeze
Sea breeze
Offshore (NE winds)
Onshore (SW winds)
Hour of day (UTC)



(a)
O3 (ppb)
No sea breeze
Sea breeze
Offshore (NE winds)
Onshore (SW winds)
(b)
NOx (ppb)
(c)
Ox (ppb)
(d)
PM2.5 (µg m-3)
(e)
PM10 (µg m-3)
Hour of day (UTC)



➢ There are several language edits needed:

o PM2.5 and PM10 should be PM2.5 and PM10

o "more than twice as higher" should be "more than twice as high." o "We can to an extent account..." should be rephrased.

o Figure 7 caption: "diurnals cyles" should be "diurnal cycles."

o "PPAO are archived" should be "PPAO data are archived." o Figure A3: "effecting most pronounced" appears to be a typo.

Thanks. All changed.

Reviewer 2

The manuscript by Yang et al. reports a comprehensive long-term study of sea breeze effect on urban air quality. The study is fairly well developed, but I am not convinced that it deserves publication in ACP due to the lack of fundamental advances. Sea breeze is a too well-established phenomenon while its impact on air quality is mostly qualitatively established without numeric indicators or predictive value. I would suggest that the paper is published in Atmospheric Pollution Research or similar journals unless it is significantly more developed and advanced.

We think the key value in our study is the long-term nature with concurrent observations of meteorology and sea surface temperature, which enabled us to assess the seasonal influences of diurnal sea breeze (DSB) on atmospheric composition and air quality. Studies of such breadth are rare as far as we know. We have now added uncertainty estimates where needed, made a new table showing the air quality exceedance, and provided additional observations that help to tease out the processes and controls in DSB events (please see replies to reviewer 1 for some of these new materials).

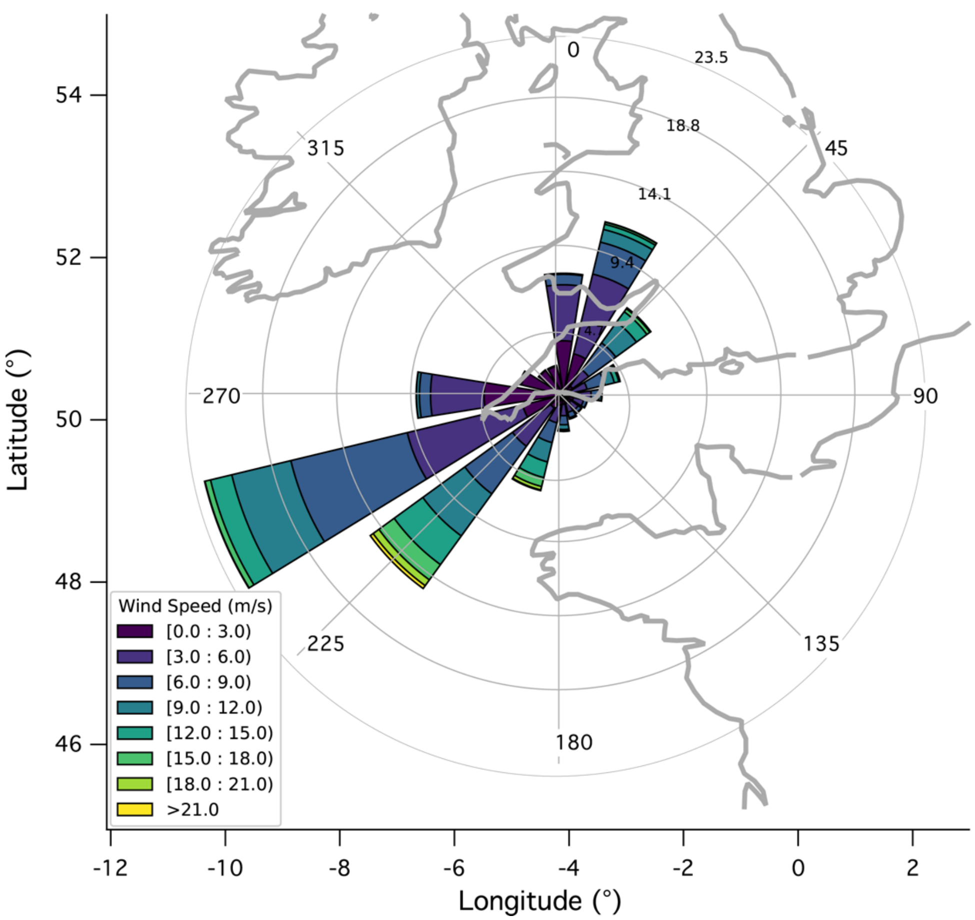
Line 17. The main result is "elevated concentrations"? There must be a quantitative result.

The main results are summarized in section 4. Because we are looking at so many different pollutants, it's cumbersome to mention the influences of DSB on all of them in the abstract.

Line 61. There should be a more transparent account of data overlap, e.g. Table or better time line (Graph).

To provide further details, we have now changed 'year' to 'month year'.

The geographical setting of the site together with the sea breeze directions would add contextual clarity.
We have added the following figure to the Appendix.



Map of the study region, with PPAO located at the center of the wind rose. The wind rose, based on 10 years of observations, shows that the predominant wind direction here is from the

Southwest (210-260° deg further referred to ‘onshore’), followed by Northeast (330-60° referred to as ‘offshore’).

Line 100. Was it challenging when tried various tests or just by the visuals?

Regressions of monthly data, yearly averaged data, and seasonally detrended monthly data lead to inconsistent results, suggesting that even 10 years of data is not long enough to tease out all the causes for interannual variability.

We have modified this paragraph and added a new panel to the figure:

‘In total, out of 10 years of data, 428 days (~12%) were classified as DSB events. Figure 1a shows the number of sea breeze days per month over the 10-year time series, along with air and sea temperatures, while Figure 1b shows the sea-air temperature difference and wind speed. Sea breeze events tend to peak in the early summer and nearly vanish in the winter, with substantial year to year variability. This interannual variability is likely due to a combination of factors, including the sea-air temperature difference and prevailing winds during the spring/summer months. For some spring/summer months, up to 40% of the days were classified as sea breeze events. Interestingly, spring/summer 2023 was well known for an exceptionally strong marine heat wave in the Northeast Atlantic (e.g. Berthou et al. 2024). Our observations show that the air temperature was much warmer than the sea during that period, but the occurrence of DSB was not exceptional, probably because the prevailing winds were mostly from the southwest in the spring/summer period. In contrast, many DSB events were observed in spring/summer 2021 and 2022, which coincided with prevailing weak winds from the northeast (and correspondingly low relative humidity). Given this interannual variability, even the 10 years of observations seem too short to ascertain whether there is a significant trend in the frequency of DSB events.’

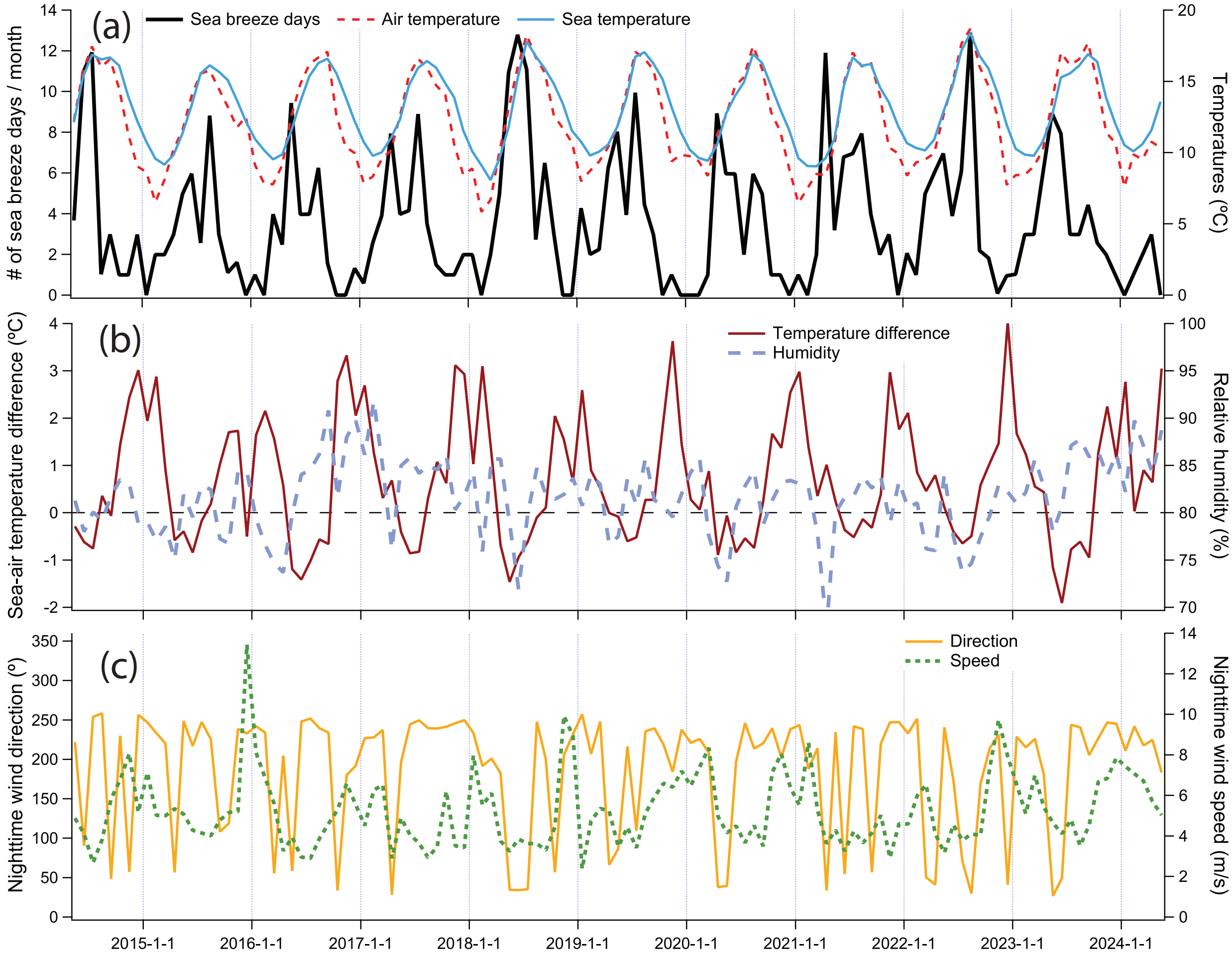
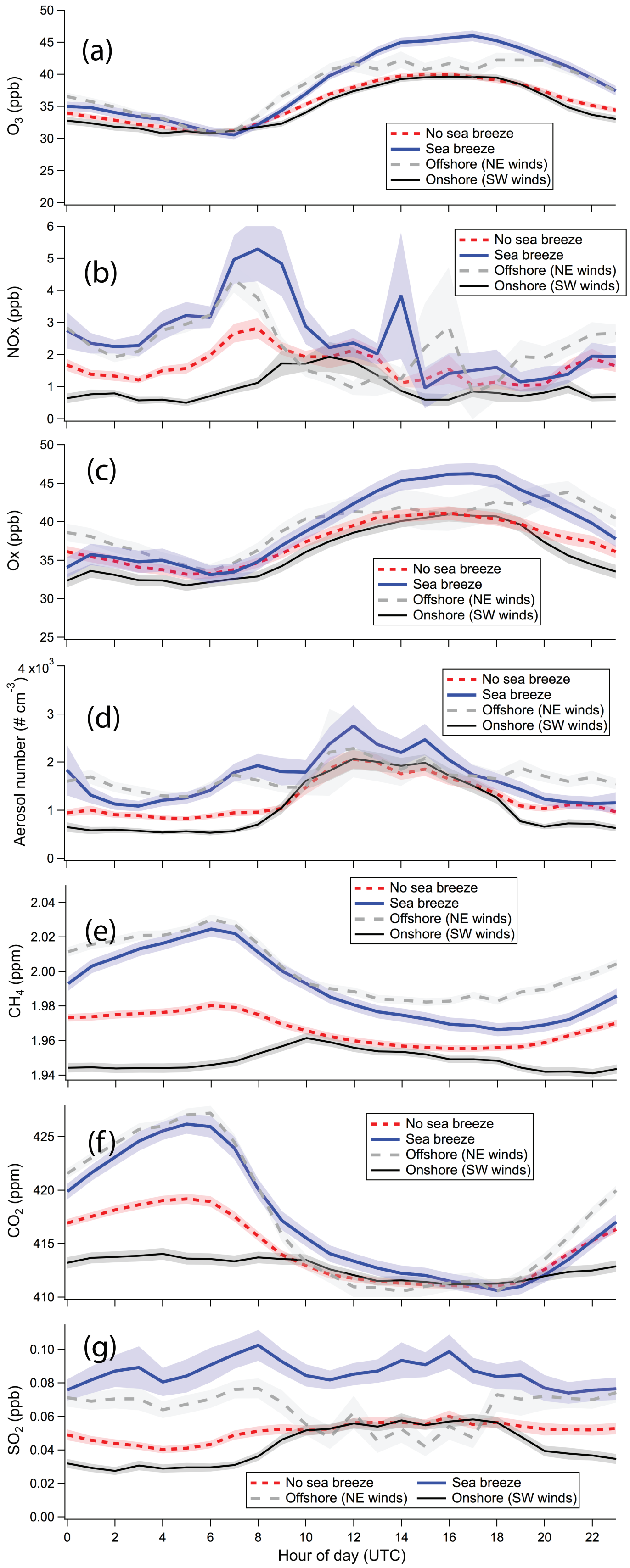


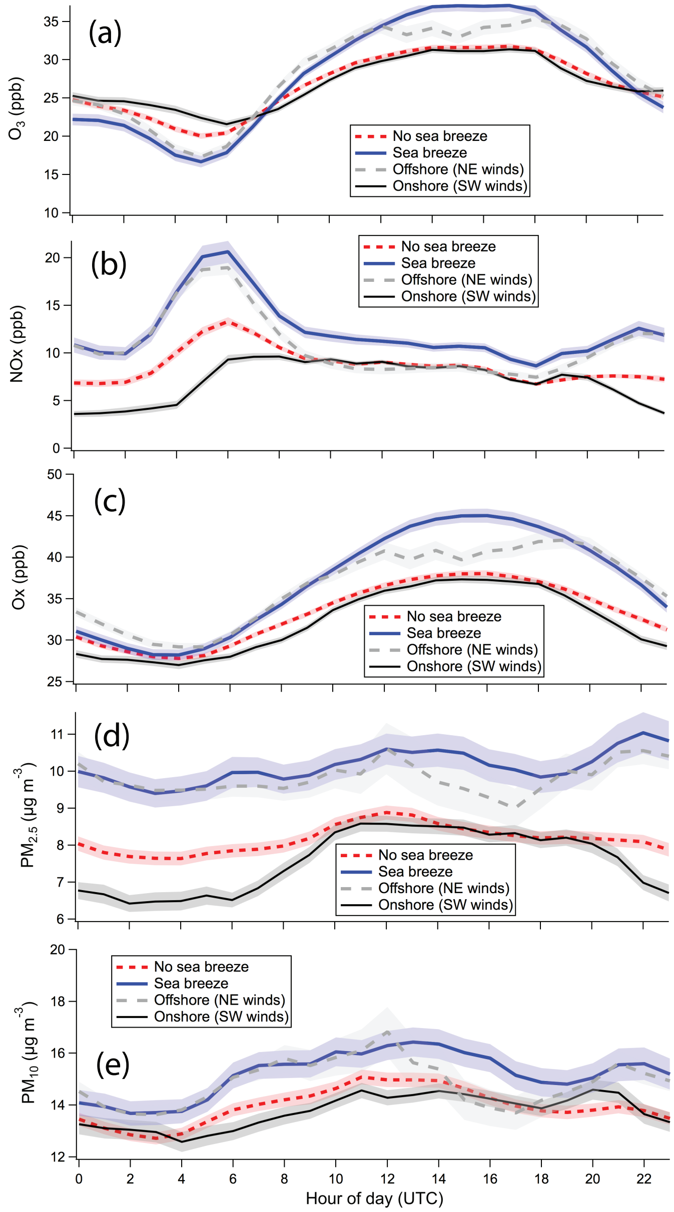
Figure 1. Monthly time series of (a) Sea breeze days identified using PPAO data; (b) sea-air temperature difference and relative humidity; (c) median nighttime wind direction and wind speed. Air temperature, humidity, and wind measurements were from PPAO, while near surface sea temperature was from L4.

Figure 2 and all. All Figures lack uncertainties to judge statistical significance which is also never quoted.

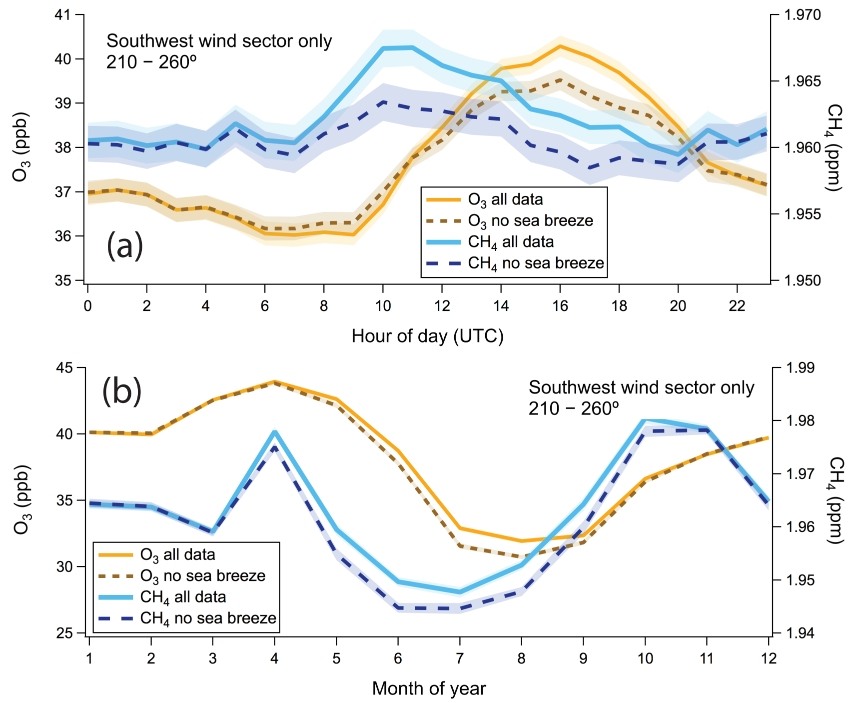
We have added uncertainty shading to figures with averaged atmospheric composition, where shading corresponds to standard error. Please see below.



(a)
O_3 (ppb)
No sea breeze
Sea breeze
Offshore (NE winds)
Onshore (SW winds)
(b)
NOx (ppb)
(c)
Ox (ppb)
(d)
Aerosol number (# cm^{-3})
4 x10^3
(e)
CH_4 (ppm)
(f)
CO_2 (ppm)
(g)
SO_2 (ppb)
Hour of day (UTC)



(a)
O_3 (ppb)
No sea breeze
Sea breeze
Offshore (NE winds)
Onshore (SW winds)
(b)
NOx (ppb)
(c)
Ox (ppb)
(d)
$PM_{2.5}$ (µg m^{-3})
(e)
PM_{10} (µg m^{-3})
Hour of day (UTC)

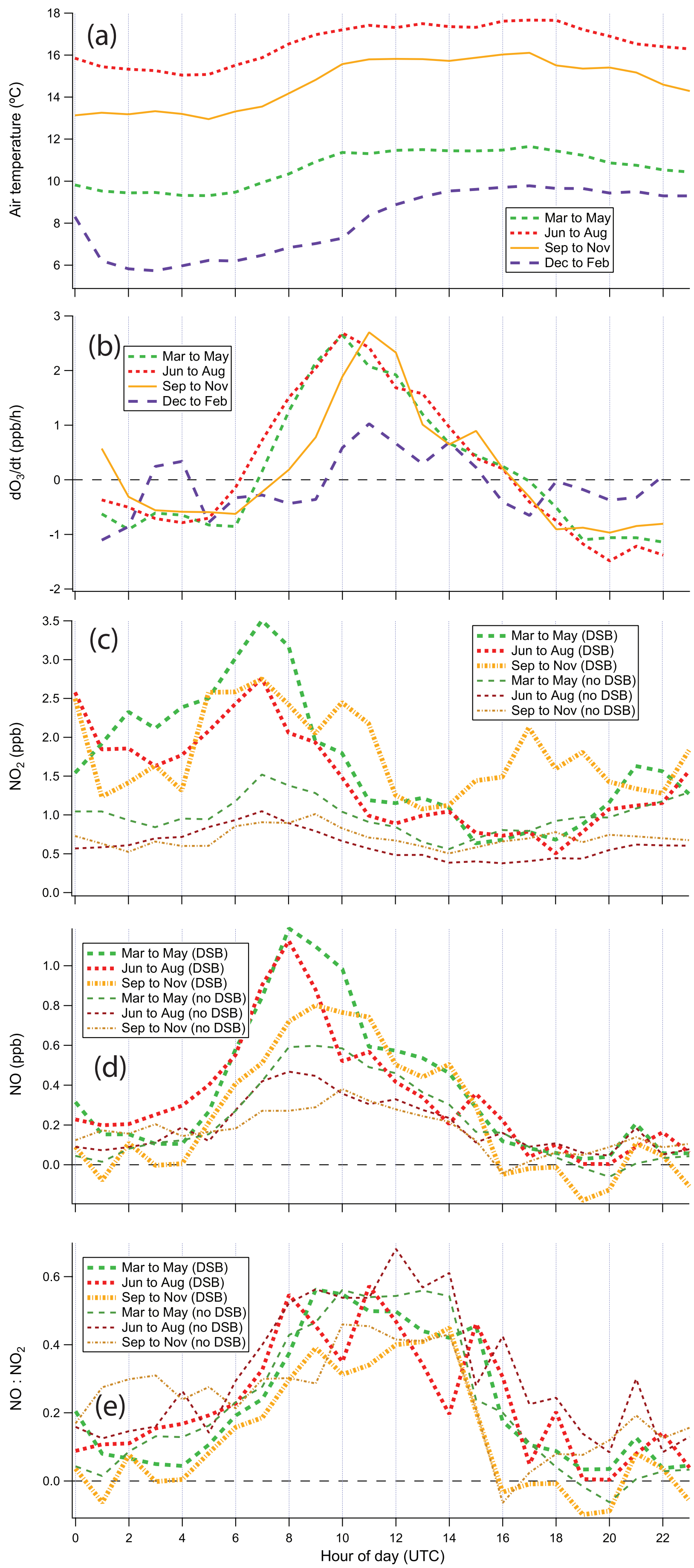


Line 185. To better account of total oxidants is to add normalized O3 and NOx instead of absolute, because O3 is typically higher than NOx almost all the time. Also, NOx is a sum of NO (rapidly reacting with O3) and NO2 (reaction product), so short-lived NO is better excluded. Instead, correlation of O3 vs NO would also be informative.

Thanks for you comments. Referee 1 had a similar comment, and we have now recomputed Ox as O3 + NO2, excluding NO.

Figure 6. Same normalization problem as there is no difference in a and b due to NOx being 10% of O3.

It's not clear to us how the reviewer would like us to normalize the O3 and NOx data, as O3:NOx ratio? While it's true that O3+NO2 at PPAO is dominated by the O3 signal, this is not the case in the Plymouth City Centre, where NO2 contributes substantially to O3+NO2 (Figure 7). We feel that it is useful to keep to the current absolute format. To provide additional information, we have further added the following to the Appendix:



(a)
Air temperature (ºC)
Mar to May
Jun to Aug
Sep to Nov
Dec to Feb
(b)
dO3/dt (ppb/h)
(c)
NO2 (ppb)
Mar to May (DSB)
Jun to Aug (DSB)
Sep to Nov (DSB)
Mar to May (no DSB)
Jun to Aug (no DSB)
Sep to Nov (no DSB)
(d)
NO (ppb)
(e)
NO : NO2
Hour of day (UTC)

Figure A3. Diurnal cycles of (a) air temperature during DSB days divided by different seasons; (b) net rate of change in O3 during DSB and non-DSB days; (c) NO2 during DSB and non-DSB days; (d) NO during DSB and non-DSB days; (e) NO:NO2 ratio during DSB and non-DSB days. Note that the O3 data spans the entire 10 years, while the NO2 and NO data are from 2021 to 2024.

Line 214. Oxidized, not destroyed.

Suggestion accepted.

Line 224. No need for mentioning "unknown" when suggesting fairly established reason of new particle formation from mostly iodine along the entire SW coast and around Plymouth or indeed shipping emissions if the pattern supported by ship frequency.

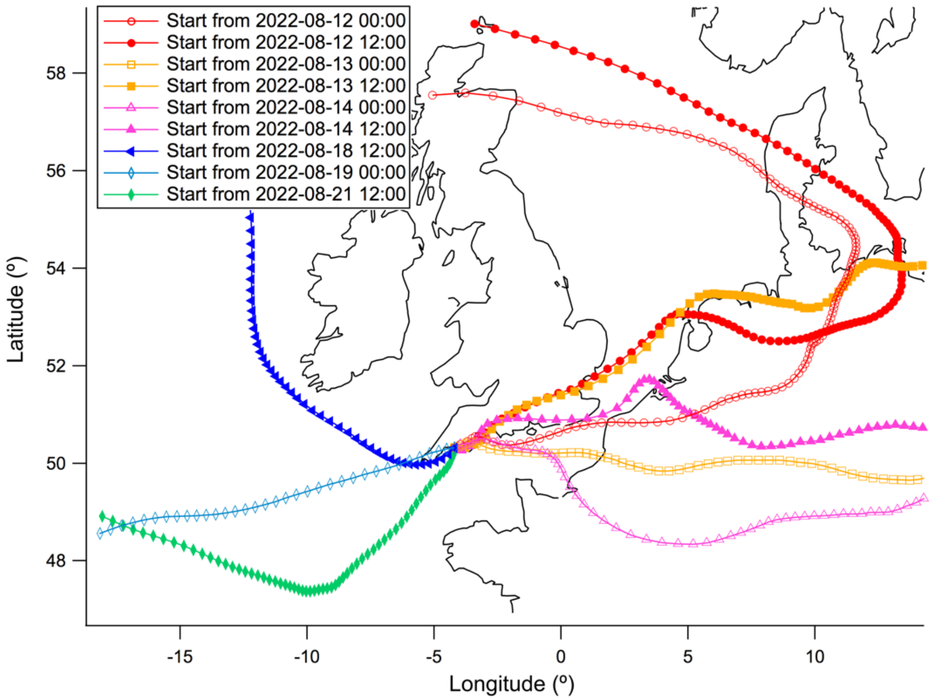
We have replaced ‘unknown’ with ‘uncertain.’ We have further added the following:

‘Halogen emissions from the coast contribute to this new particle formation, as PPAO data show enhanced aerosol number concentration when the tidal height is less than ~2 m. However, removing these low tide data (20% of the time) lowers the mean aerosol number concentration at around midday by only ca. 200 cm^{-3}, and does not eliminate bulk of the diurnal cycle. Thus the large diurnal cycles in aerosol number concentration may be mostly driven by ship-emitted SO_2, which is roughly twice as high on DSB days than non-DSB days. We note that the aerosol number concentration data here were collected prior to the 2020 International Maritime Organisation regulation on ship sulfur emission reduction. Further aerosol number concentration observations post regulation as well as composition measurements of nucleation mode aerosols would shed light on the sources of new particles at PPAO.’

Figure 7. Ox pattern becomes more complex when O3 concentration becomes similar to NOx. Hence, normalization is preferred.

See comment above.

Line 255. Intuition must not inform definition of a background site, research must. It should be rephrased. It would very short-sighted if someone made the decision on the representativeness based on wind direction alone. Air mass trajectory analysis or anthropogenic indicators like black carbon should accompany definition of a background site.
We have run some HYSPLIT back trajectories and will add the following figure to the appendix. Even using the high resolution GFS meteorological data (0.25 deg resolution), the trajectories do not well capture the sea breeze events shown in the example (Figure 5 in the paper). Trajectories started between 12 and 14 August 2022 only showed the prevailing (nighttime) northeasterly winds, and do not show the daytime switch to onshore flow. Trajectories started between 18 and 19 August 2022 showed winds mostly from the North Atlantic, and did not capture the briefly periods when the winds switched to offshore at night/predawn.



A separate indicator like black carbon would've been useful indeed as a tracer. Unfortunately it was not measured.

Line 291. Can ~20% more pollution be called severe? Conclusions do not have a single number qualifier.

We have changed it to 'greater level of pollution'. We have not used a single number qualifier because the extent that DSB increases the pollution level depends on the pollutant of interest.