We thank the referee for their constructive review, which has helped to improve our paper. Please see our replies below in blue.

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).

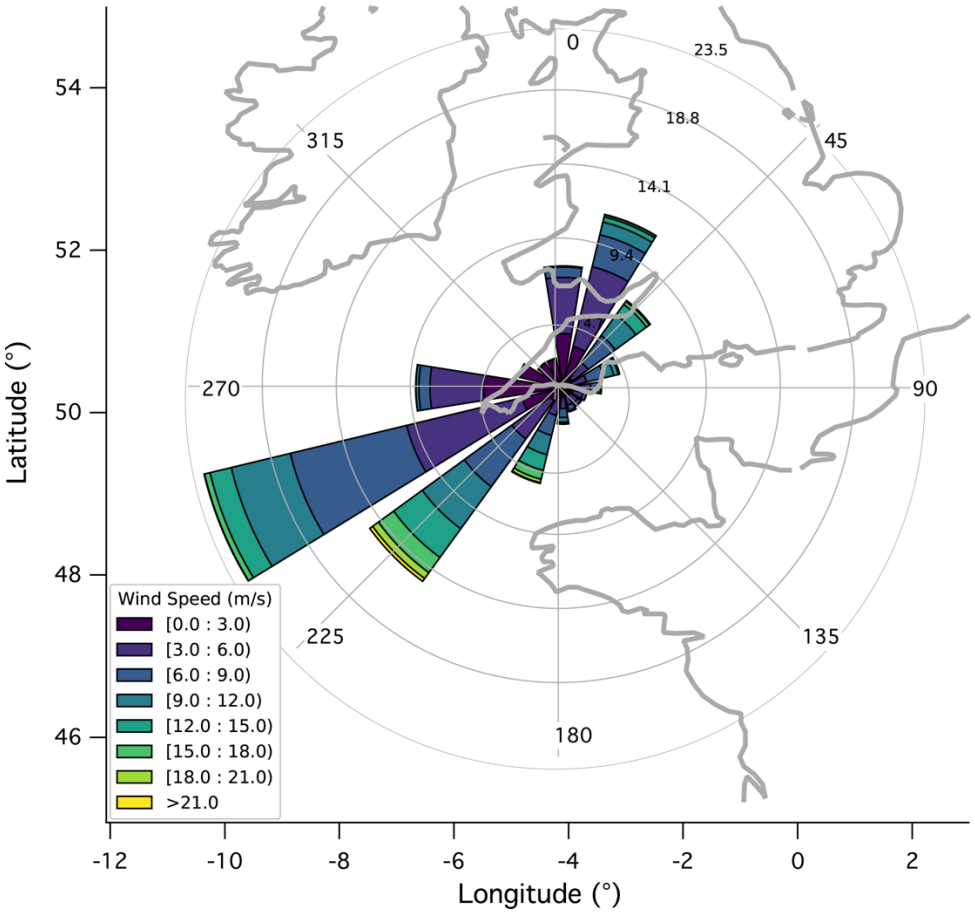
Line 17. The main result is "elevated concentrations"? There must be a quantitative result.

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.'

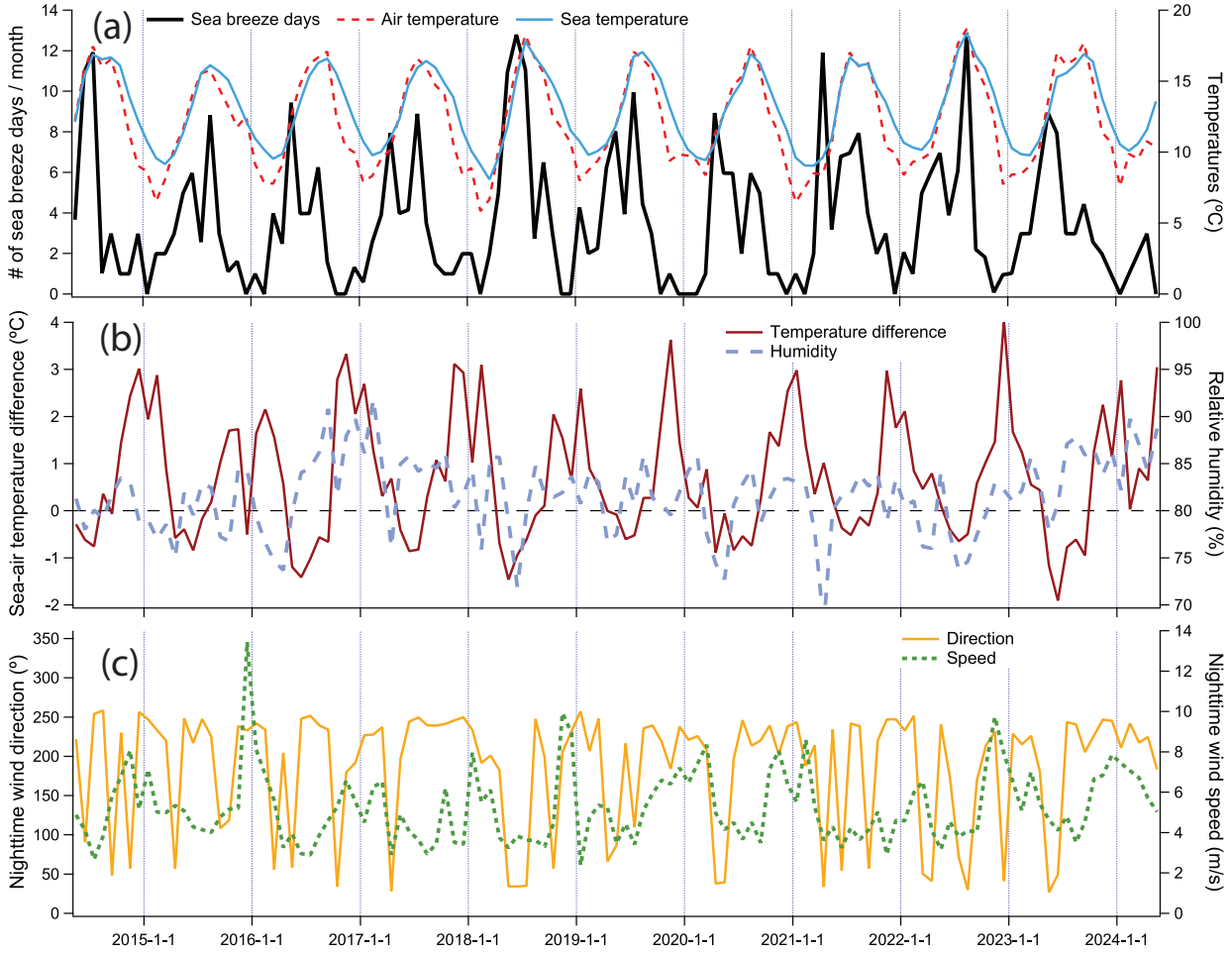
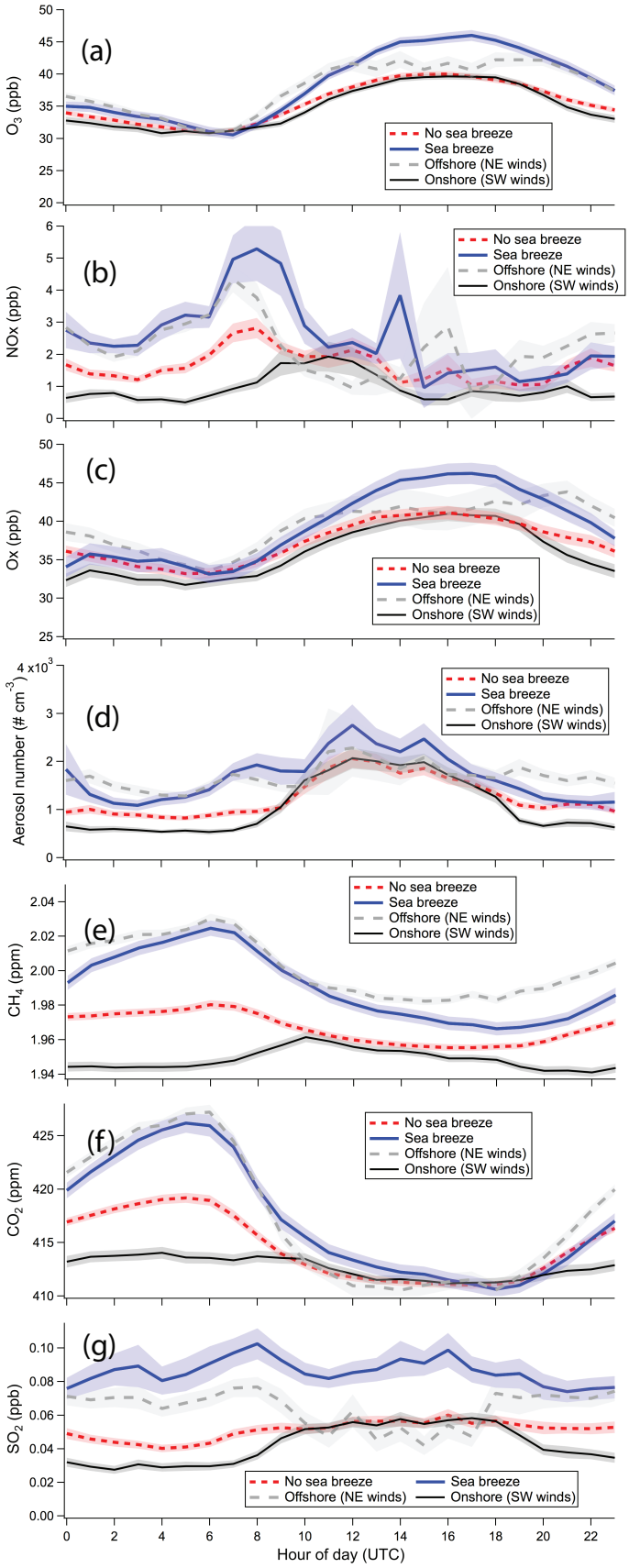
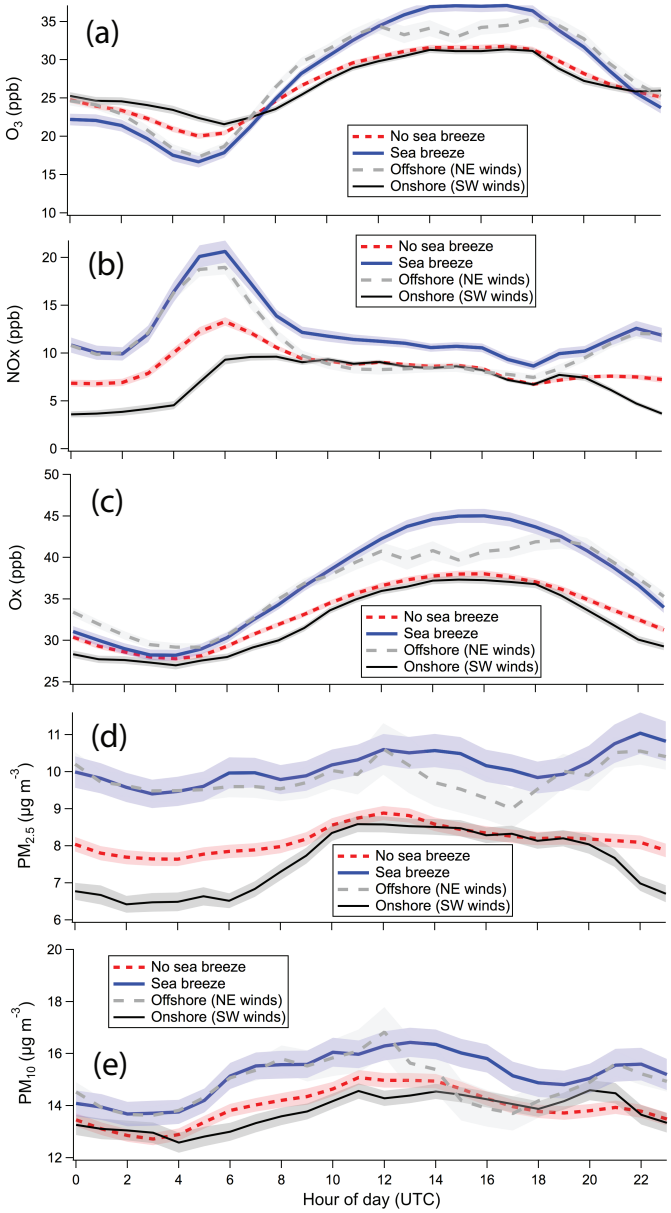


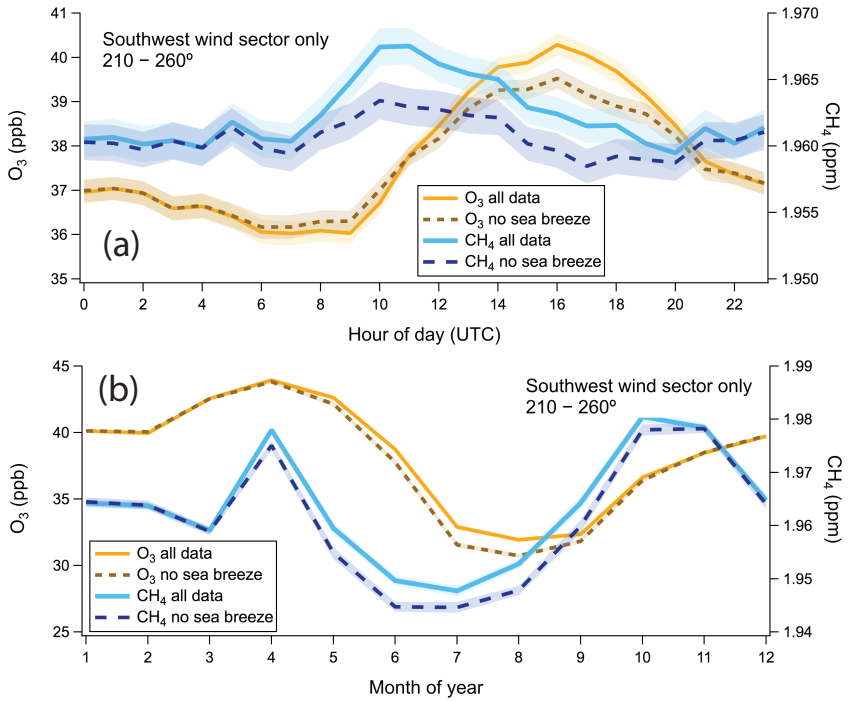

Figure 1. Monthly time series of (a) Sea breeze days identified using PPAO data; (b) sea-air temperature difference and relative humidity; (c) median nighttime wind direction and wind speed. Air temperature, humidity, and wind measurements were from PPAO, while near surface sea temperature was from L4.

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)
O3 (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)
CH4 (ppm)
(f)
CO2 (ppm)
(g)
SO2 (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)
$PM_{2.5}$ ($\mu g\ m^{-3}$)
(e)
$PM_{10}$ ($\mu g\ m^{-3}$)
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:

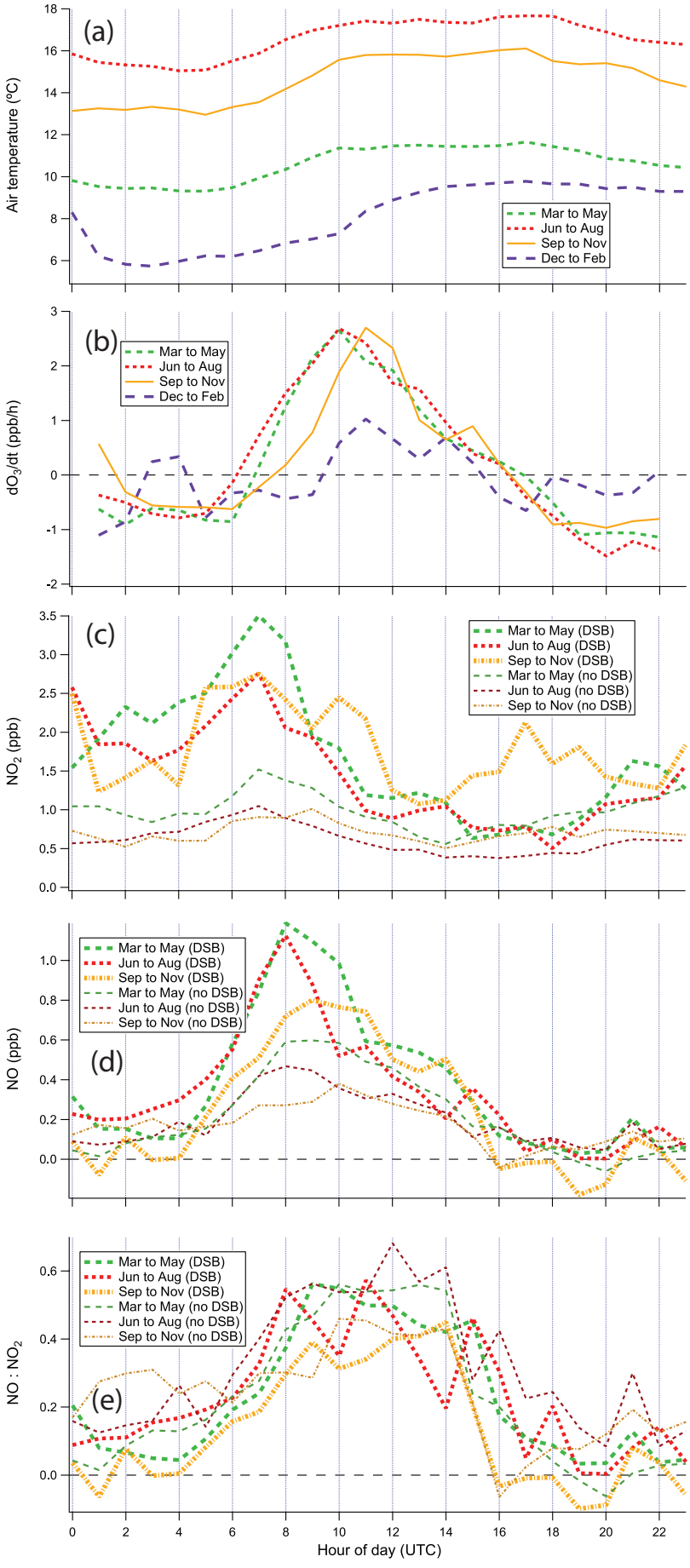

(a)
Air temperature (°C)
Mar to May
Jun to Aug
Sep to Nov
Dec to Feb
(b)
dO3/dt (ppb/h)
(c)
NO2 (ppb)
Mar to May (DSB)
Jun to Aug (DSB)
Sep to Nov (DSB)
Mar to May (no DSB)
Jun to Aug (no DSB)
Sep to Nov (no DSB)
(d)
NO (ppb)
(e)
NO : NO2
Hour of day (UTC)

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.

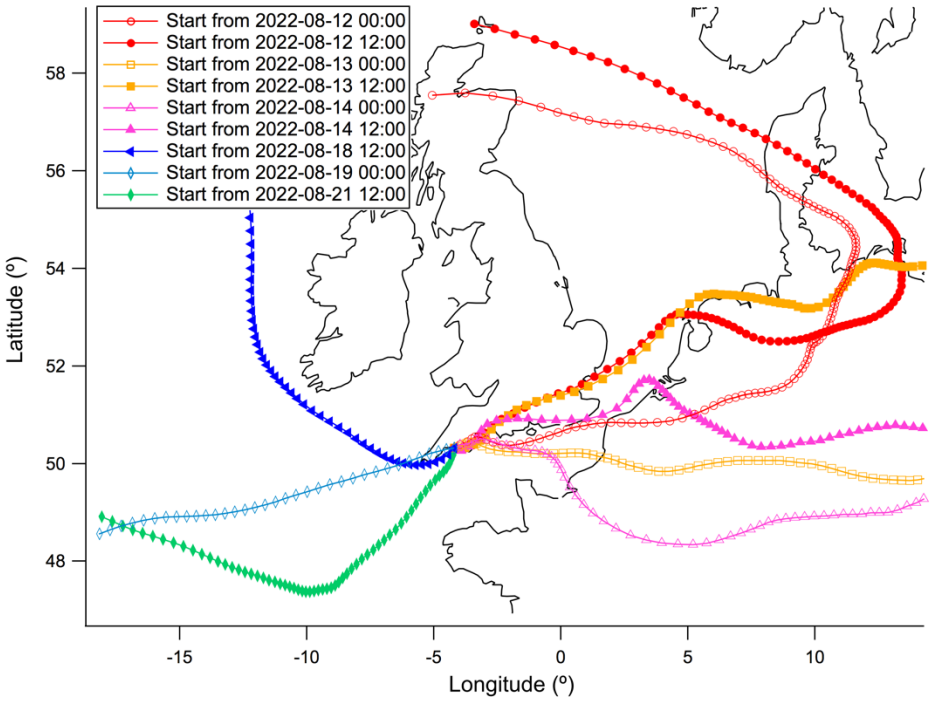
We have replaced ‘unknown’ with ‘uncertain.’ We have further added the following:

‘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.