Response to Referee #2 Comments

Overview:

This is a highly technical paper that reports aircraft VOC fluxes measured by a PTR-tof-MS. The paper looks in detail at sources of random and systematic uncertainties in the flux measurements. Due to the small magnitude of the VOC fluxes, the authors found that only DMS (emission) and acetone (deposition) fluxes were unambiguously detected on a consistent basis. Overall, I think this is a useful methods paper. Though it doesn't seem that the flight plans were designed to optimize flux measurements, and the numbers of flux segments were very small, thus it's hard to say how representative the measured fluxes are of air-sea exchange in the North Atlantic.

Thank you for recognizing the uncertainty discussion of the work and for pointing out the limitations in the reported fluxes.

The reviewer is correct that the flight sampling was not designed to specifically optimize flux measurements, but it nevertheless includes numerous MBL legs that are 1) suitable for EC, and 2) of sufficient duration to capture the relevant eddy scales. As a result, NAAMES provides a useful real-world dataset for assessing measurement requirements for airborne EC in the context of air-sea VOC interactions.

We have modified Sect. 5 to make the caveat about limited representativeness more clear.

Thank you also for the thoughtful comments and constructive suggestions that follow. We have included our point-by-point responses below in blue, with the corresponding updates to the manuscript text indicated in green/quoted.

(We have also made some additional minor corrections as reflected in the updated manuscript with tracked revisions, without influence on any discussion points or conclusions.)

Detailed comments:

The introduction had a very comprehensive review of previous techniques.

> Thank you for this comment.

Sensitivity cps/ppb of PTR-tof-MS for main compounds of interest? What's the mass resolution?

> We have updated the manuscript (Sect. 2) to include these and other details on the deployed instrument:
>
> "VOC mole fractions were measured on-board the C-130 by proton-transfer-reaction time-of-flight mass spectrometry (PTR-ToF-MS) during NAAMES-1 (for 4 of the 7 flights), NAAMES-2 (9 of 11 flights), and NAAMES-3 (9 of 12 flights). The employed instrument is an upgraded version of the prototype PTR-ToF-MS 4000 described by Müller et al. (2014) and manufactured by IONICON Analytic GmBH (Innsbruck, Austria). VOCs were sampled from outside the aircraft boundary layer using a winglet and a heated (50°C) ¼" surface-treated stainless-steel line (Sulfinert®, Restek Corporation, Bellefonte, PA, USA). During NAAMES the winglet was positioned in a downward-facing orientation at the fuselage of the C-130. The inlet flow varied with ambient pressure from 15 standard liters per minute ($L \cdot min^{-1}$) at high altitudes to 30 $L \cdot min^{-1}$ at sea level. Reported analytes were calibrated via dynamic dilution of a certified compressed-gas standard (Apel-Riemer Environmental, Inc.), and sensitivities during NAAMES ranged from 320 $cps \cdot ppb^{-1}$ for methanol to 2206 $cps \cdot ppb^{-1}$ for methylethyl ketone. One-second concentration limits of detection were in the low 10s of ppt for most compounds, with an overall range of 9 ppt for toluene to 229 ppt for methanol. Zeroing was performed by passing ambient air through a Pt/Pd catalyst heated to 350°C (Müller et al., 2014). The instrument achieved a mass resolution ($m/\Delta m$, full-width half-maximum) of approximately 4000, with the mass axis calibrated through continuous addition of diiodobenzene (PerMassCal; IONICON Analytik GmbH, Innsbruck, Austria). The

expected e-folding time (after accounting for inlet lag) for this instrument is approximately 0.1 s for most species targeted here but may be longer for sticky compounds (Müller et al., 2016).~~The instrument is described in detail by Müller et al. (2014); t~~The NAAMES measurements were conducted at 5 Hz sampling frequency with an estimated accuracy of 10 % + 5 ppt. **Table 1** lists the compounds examined in this study, which include…"

Line 105. Altitude of low level legs?

The altitudes of low-level legs are summarized in Sect. 4.1 with details listed in Table S1. We have added text at the location mentioned by the reviewer to direct the interested reader to these specifics:

"The C-130 flights included multiple low-level MBL legs (detailed in Sect. 4.1) ~~within the MBL~~, satisfying one of the prerequisites for deriving air-sea fluxes by airborne EC."

We have also made some minor corrections to the content in Sect. 4.1 to match the details in Table S1:

"**Table S1** lists the 34 flux legs from 11 NAAMES-2 and NAAMES-3 flights identified in this way, which average 9~~8~~ min (~70~~64~~ km) in duration and range from 3-25 min (~20~~2~~-210~~194~~ km) at mean sampling altitudes of 126-219 m. Mean airspeeds range from 111~~02~~-157 $m \cdot s^{-1}$ across the flux legs, corresponding to approximately one 5 Hz data point per 22~~0~~-31 m."

Line 110. This might have been described briefly, but is worth repeating.

Was the PTR calibrated against standard gases?

VOCs are calibrated using a multiple component calibration gas standard (Apel-Riemer Environmental, Inc.) that was dynamically diluted. We have now added this information to the instrument description (see our reply to this reviewer's prior question about instrument specifications).

What was used as the background or blank measurement?

Zeroing was performed by passing ambient air through a heated Pt/Pd catalyst held at 350°C. We have now added this information to the instrument description (see our reply to this reviewer's prior question about instrument specifications).

Type/length of inlet?

VOCs were sampled with a winglet and a heated surface-treated stainless steel line. Inlet details have now been added to the instrument description (see our reply to this reviewer's prior question about instrument specifications).

Flow rate?

The inlet flows varied with ambient pressure 15 to 30 $L \cdot min^{-1}$. We have now added flow information to the instrument description (see our reply to this reviewer's prior question about instrument specifications).

What's the expected e-folding time (response time) of the system?

The e-folding response time of the complete inlet/PTR-ToF-MS system was not specifically measured for this study. It was reported previously by Müller et al. (2016) to be approximately 0.1 s for most species targeted here but longer for sticky compounds. The Müller et al. (2016) value of 0.1 s is in-line with our expectations for NAAMES based on the volume exchange time of the inlet and PTR-ToF-MS (both ~0.1 s). We have added this additional context to the instrument description in Sect. 2:

"The expected e-folding time (after accounting for inlet lag) for this instrument is approximately 0.1 s for most species targeted here but may be longer for sticky compounds (Müller et al., 2016)."

And to the discussion of results for DMSO and C9-aromatics, which are the analyzed species most likely to be impacted by such effects:

"• DMSO/C9 aromatics. To our knowledge there have not been any reported air-sea exchange rates for these species. Their fluxes were undetectable during NAAMES, and the uncertainty range for the campaign-mean results suggests that any air-sea exchange that did occur had a magnitude less than approximately 0.1 $\mu mol \cdot m^{-2} \cdot d^{-1}$ (Table 1). However, it is possible that wall interactions may have inhibited flux detectability for these lower-volatility species."

Line 135. Was a correction for the aircraft motion on the measured relative winds necessary for these flights?

All corrections from calibration maneuvers that are described in detail in Thornhill et al. (2003) are applied to the full Lenschow (1986) equations and those values are based on statistics collected from multiple calibration maneuvers that were performed throughout the multi-year NAAMES EVS campaign. No additional corrections were required for the aircraft wind to force any segments of the data to be ideally 0.0 $m \cdot s^{-1}$. We have updated the text to provide more detail on this point:

"Three-dimensional winds are computed from the full air motion equations (Lenschow, 1986) and corrected for aircraft motion as described by Thornhill et al. (2003) based on statistics derived from calibration maneuvers performed throughout the multi-year NAAMES EVS campaign. No additional corrections were required, and the uncertainty in the resulting wind fields is estimated at 10 %~~with uncertainty estimated at 10%~~."

Figure 2. please use different marker shapes for people who might struggle with the colors.

Thank you for the suggestion. We have updated Fig. 2 as requested:

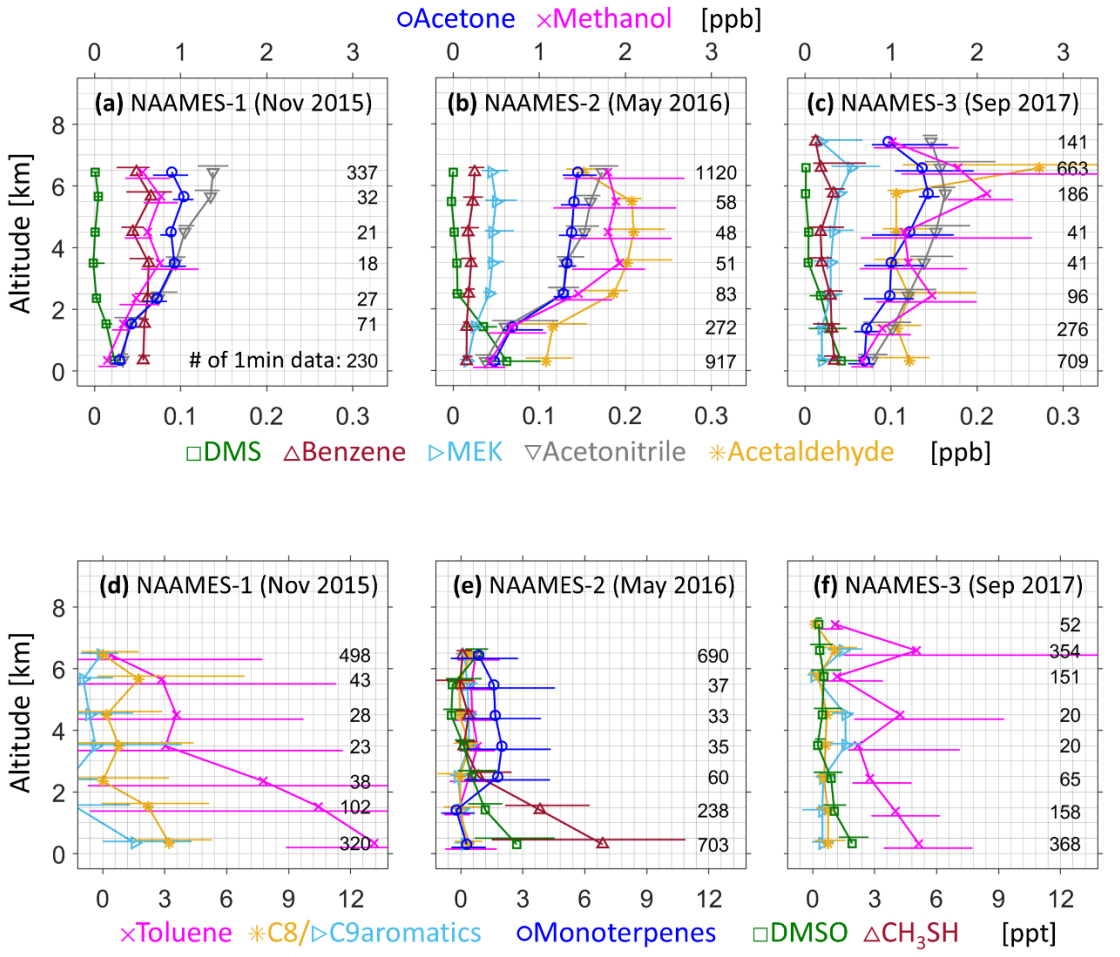

Figure 2. (updated)

Line 176. Random uncertainty in flux can usually be reduced by more averaging (scales with 1/sqrt(N)). Here a rather large range in averaging time (and so space) was chosen: 3-25 km, which by itself preassembly leads to quite different noise levels. Why not just using 3 km averaging period through out? And if needed, further average the 3-km fluxes afterwards?

Sampling lengths need to be long enough to capture all relevant eddy scales. The 95 % eddy scales summarized in Table S1 range from 1-30 km, and show that the leg lengths as used here meet this requirement but a uniform 3 km length would not.

Line 186. How different are flux results using this approach vs a simple linear detrend?

Although the two detrending approaches result in highly consistent flux estimates (Fig. R2-1 panels a, b), the 100 s moving average approach is better at filtering out extremely large scale motions (panel d) than is linear detrending (panel c). But in any event, the overall flux agreement indicates that these large scale motions at the low-frequency tails are not contributing appreciably to the derived values.

We have clarified this in the manuscript:
"The resulting data subsets undergo mean-removal and detrending to isolate the scalar concentration fluctuations that are driven by turbulence. We employ 100 s (11-16 km) moving-average mean removal; as shown in the **Supplementary Spectra** this acts as a high-pass filter to remove low-frequency, non-turbulent components (e.g., caused by air

mass variability) that might not be excluded with a simple linear detrend (Novak et al., 2021; Moncrieff et al., 2005)."

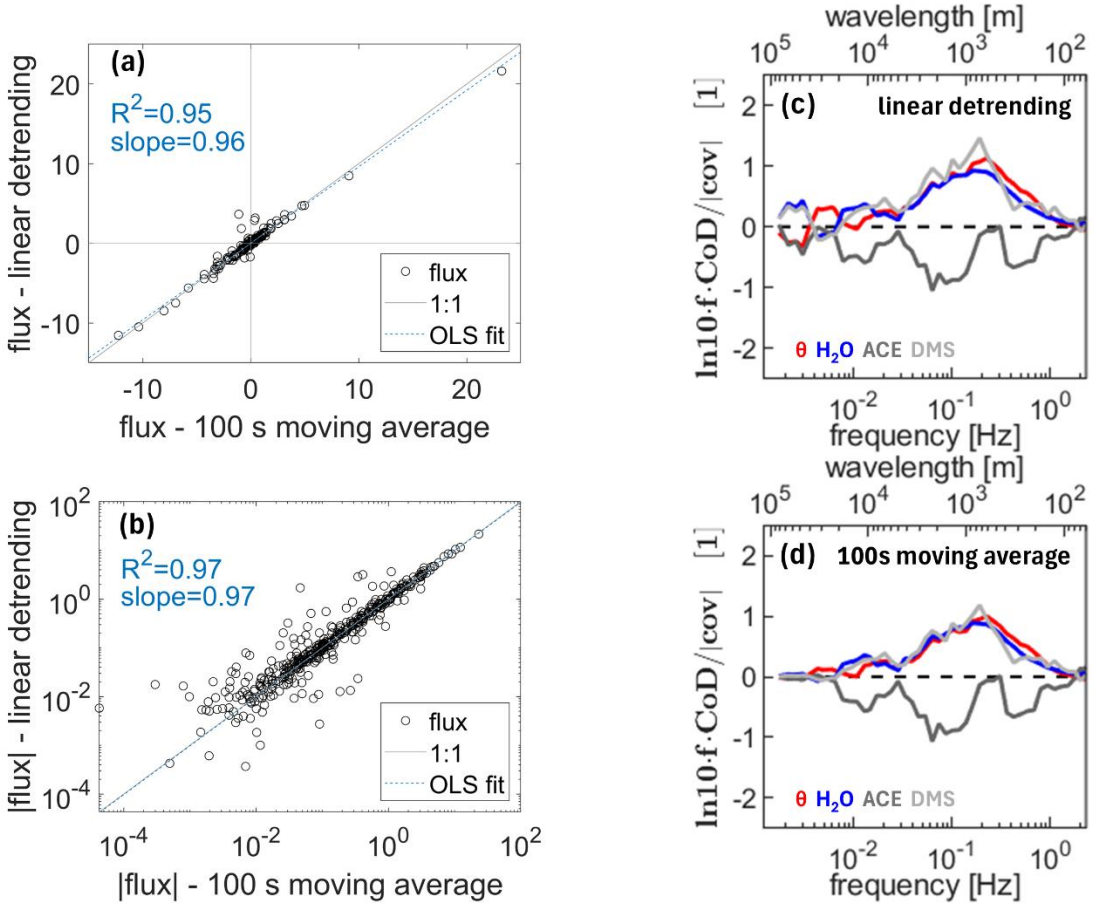

Figure R2-1. Comparison of VOC fluxes derived using 100 s moving average detrending vs. linear detrending on linear (a) and log (b) scales. Example covariance-normalized and frequency-weighted cospectra from flight 20160528 L2 with linear detrending (c) and 100 s moving average detrending (d).

Line 190. What is roughly the expected lag time based on the inlet dimensions and flow rates?

The total inlet length was approximately 3.3 m, corresponding to an internal volume of 26 cm^3. With flow rates of 15-30 $L \cdot min^{-1}$ this corresponds to an expected physical lag time of 0.1 s or less. The lag times derived via cross-covariance analysis are frequently greater than this (up to 11.8 s, depending on the flight) and that is because this derivation is also accounting for clock offsets between the PTR-ToF-MS and wind measurement systems. There was not a common time server used on these flights and therefore there were minor differences between the instrument time stamps that varied by flight. We have clarified this point in the revised manuscript:

"In multiple cases the cross-covariance for an individual flux leg lacks a sufficiently clear peak for time-lag diagnosis (**Fig. 3**), and we therefore employ two alternative treatments as follows. (1) Flight-specific: here the time lag is defined based on the strongest cross-covariance peak for any species across the entire flight. The 11 lags calculated in this manner range from 0.4-10.8 s. (2) Species- and leg- specific: here the time lag is allowed to be species- and flight-leg specific. The latter treatment yields an ensemble of time lags that

range from 0-2.8 s or from 6.2-11.8 s, depending on the flight; for cases without a clear and physically reasonable peak the lag is set to the mean of the within-flight ensemble. Derived lag times of up to 11.8 s primarily reflect clock offsets between the PTR-ToF-MS and wind measurement systems, which varied by flight; the physical lag time expected for a 3.3 m (0.3175 cm ID) inlet with flows exceeding 15 L·min^{-1} is < 0.1 s.

Treatment 1 above assumes that the targeted VOCs interact similarly with the sampling inlet and instrument, and that inter-species lag differences mainly reflect statistical artifacts. Fluxes derived in this way are conservative relative to those obtained with treatment 2, which (in addition to clock synchronization) allows for species-specific sampling effects and tends to derive the largest possible flux magnitude. In the following analysis we use treatment 1 (a single flight-specific lag applied across species) as default ~~unless otherwise noted~~."

Line 190-continued. Have authors tested with injections of VOC standards to see whether different VOC show comparable or different responses within their inlet?

Please see our reply to the reviewer's prior question about instrument e-folding times.

Figure 3. on my screen there looks to be breaks in the cospectral density plots?

Thank you for capturing this. This is now clarified in the caption:

"**Figure 3.** Example *s*-*w* cross-covariances (panels a, f) and wavelet-based spectral analyses (b-e, g-j). The top row shows an example flux leg in which clear cross-covariance peaks and well-behaved cospectra/ogives are obtained for water vapor (H_2O) and potential temperature (θ) but not for VOCs. The bottom row shows an example leg with clear cross-covariance peaks and reasonably well behaved cospectra/ogives for all plotted scalars. The impact of white noise on the VOC measurements is evident in the high-frequency range of the power spectra in panels (b) and (g). The wavelength scale is computed as the aircraft ground speed divided by frequency, without any height scaling. Line breaks in panel (c) occur where the flux power is in the opposite direction to that of the overall (net) flux value."

Line 254. Again, what's the response time of the PTR/inlet system?

This has been added. Please see prior responses.

Line 254-continued. It might be that the cospectra are too noisy to say with confidence that high frequency attenuation is negligible.

We agree and have now clarified this point:

"*B. High-frequency attenuation*. Loss of high-frequency flux contributions can arise from an insufficient sampling frequency, inadequate instrument time response, or inlet-line damping. If uncorrected, the result is a systematic flux underestimate. The NAAMES VOC-*w* cospectra~~, while noisy,~~ do not show clear evidence of such an effect when compared to those for the non-VOC scalars (**Fig. 3**, **Supplementary Spectra**). It is possible that the instrument noise is obscuring some attenuation, but since there is no direct evidence of this w~~W~~e ~~therefore do not~~ perform ~~any~~ no high-frequency correction ~~for NAAMES, with the expectation~~and expect that other error sources are more important."

Line 254-continued. Were there conditions of stable atmosphere, where attenuation might be more pronounced?

Thank you for the suggestion. As examined in Fig. R2-2, for NAAMES airborne fluxes analyzed in this work, we do not see clear evidence of the level of attenuation as a function of atmospheric stability.

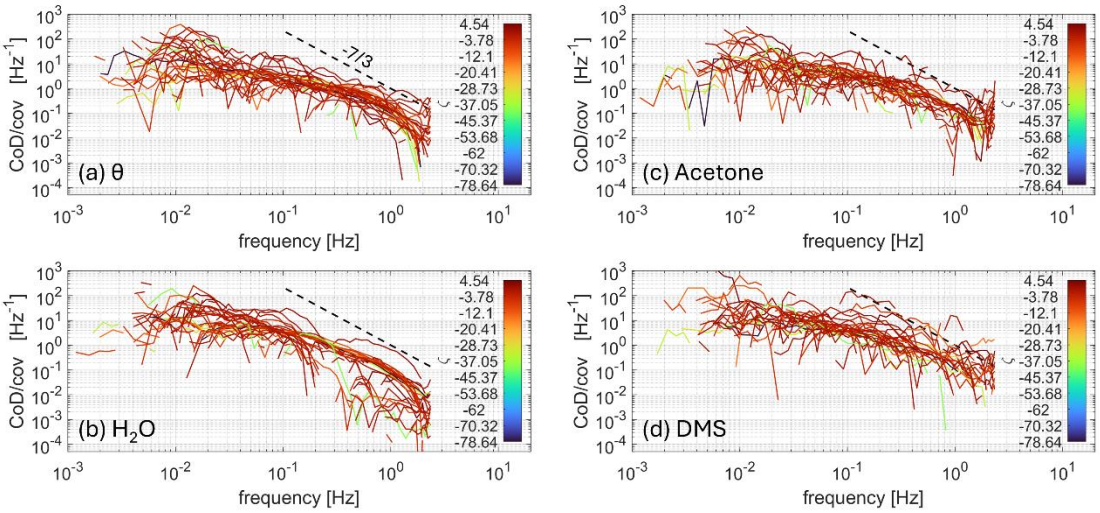

Figure R2-2. NAAMES cospectra (covariance-normalized) colored by flight altitude atmospheric stability. The stability indicator is calculated as $\zeta = \frac{z}{-(\overline{w'u'}^2+\overline{w'v'}^2)^{0.75}}$, where $\kappa = 0.41$, $g = 9.81\ m \cdot s^{-2}$, and θ, $\overline{w'u'}$, $\overline{w'v'}$,$\overline{w'\theta'}$ are obtained at flux leg altitude z.

Line 260. The detrend method used implicitly assumes that there are no low frequency eddies that contribute to flux, right?

To the contrary, we use this detrending method specifically to remove low frequency, non-turbulent components. Please see our response to Line 186 for details.

Line 260-continued. Again, a simple comparison with this approach vs linear detrend would be useful.

Done. See response to Line 186.

Line 314. The use of 'VOCs' here is ambiguous. Surely the random error contribution to well resolved fluxes such as DMS and maybe acetone is smaller than for VOCs such as benzene and monoterpenes?

The main purpose of this passage is to compare behavior between VOCs and θ / H_2O. It is correct that VOCs with better-resolved fluxes have a smaller error contribution from USN.

For example, we calculate mean ± SD of $\frac{\sigma_{f,USN}^2}{\sigma_{f,RE}^2}$ values as follows:

Acetone: 77% ± 23% (total number of fluxes: 34)
DMS: 83% ± 22% (34)
Benzene: 98% ± 11% (28)
Monoterpenes: 104% ± 16% (14)

However, the overall point still stands: the VOC flux errors are in general USN-dominated whereas flux errors for θ and H_2O are in general turbulence-dominated. We have updated the text to convey this point:

"The empirical approach outlined above combines ideas from Lenschow et al. (2000), Langford et al. (2015), and Wolfe et al. (2015), and is referred to as LLW hereafter. When we compare the resulting $\sigma_{f,USN}$ estimates to the $\sigma_{f,RE}$ values obtained earlier we find that the total flux random error is dominated by the USN contribution for VOCs (93~~89~~ % of $\frac{\sigma_{f,USN}^2}{\sigma_{f,RE}^2}$ ratios exceed 50 %, with smaller ratios for species with better-resolved fluxes) but by turbulent stochasticity for θ and H_2O. This flux-based finding aligns with the concentration-

based autocovariance results above and shows that the VOC flux measurements are occurring in a noise-limited regime. The situation therefore differs from that encountered for air-sea flux measurements of CO_2, where sampling uncertainties are often the principal random error source (Dong et al., 2021)."

Figure 5. This is a neat way to illustrate the importance of getting the lag time correctly. But I don't get why such a large lag wind window (+/- 50s) is needed for the lag correlation calculation. Was the pump flow so variable (e.g. with altitude)?

In fact a search window of 0 ± 10 s is applied in the non-prescribed runs, and this is now clarified in the Fig. 5 caption.

"**Figure 5**. Flux histograms for the noise perturbation ensemble. H_2O fluxes from NAAMES flight 20160601 L1 are subjected to five different noise levels ($\sigma^2_{imposed}$) according to $log_{10}\left(\frac{\overline{[H_2O]}^2}{\sigma^2_{imposed}}\right) = 4.5, 3.5, 2.5, 1.5, 0.5$ ~~as described in text~~. Results are shown for scenarios in which the scalar-wind time lag is known (-2.2 s) and prescribed (a-e) and in which the time lag is determined independently (0 ± 10 s search window) for each flux derivation (f-j)."

Figure 7. Again, I think it'd be useful to specify which VOC here, as the S/N must be very different for say DMS vs benzene.

There is insufficient space in Fig. 7 to label all 13 VOCs. However, to provide the requested context on this point we calculate examples of mean (± SD) SNR_f: 3.56±1.72 (acetone), 2.39±2.22 (DMS), 1.57±1.30 (benzene), 0.56±0.54 (monoterpenes), and have added two of these examples to the accompanying text:

"The results in **Fig. 7** provide a framework that can be used to assess whether an instrument's noise performance is sufficient for a given flux application and desired SNR_f. That is, for an expected flux magnitude, the plotted curves can be used to identify the maximum $\sigma_{s,USN}$ that is likely to yield an appropriate SNR_f under the NAAMES sampling conditions. For the ten actual flux legs examined in **Fig. 7**, 1~~8~~5 % of the VOC fluxes have $SNR_f > 3$; for example, the mean (± SD) SNR_f value for acetone is 3.56±1.72 but just 0.56±0.54 for monoterpenes. W~~w~~e find that if USN was lowered by a factor of 18, 75 % of these VOC fluxes would exceed SNR_f=3. A 10-20× advance in instrumental noise performance compared to the PTR-ToF-MS deployed during NAAMES would therefore provide significant benefits in our ability to quantify ocean-air VOC fluxes."

Table 1. where it says 'uncertainty of the mean flux', this seems to be 3 stdev. Is this the flux noise or the flux detection limit? It seems like it might be the latter to me.

The "Uncertainty of the mean flux (3σ)" included in Table 1 is propagated from the individual flux random errors (3σ), through $\sigma_{mean} = (1/N)\sqrt{\sigma^2_{f,RE,1} + \sigma^2_{f,RE,2} + \cdots}$, to give an estimate of the uncertainty in the campaign-mean flux. The flux limit-of-detection is discussed elsewhere in the paper in the context of individual (rather than averaged) flux measurements and so we do not use that term here.

Table 1-continued. How are error-weighted mean flux and its uncertainty computed?

These are computed following methodologies outlined in Taylor (1997). We have updated these citations to refer to the specific chapter in each case:

"**Table 1.** Summary of airborne VOC flux measurements during NAAMES.

VOC	Ion formula	*m/z*	Number of flux legs	Flux range	Campaign mean flux[a]	Uncertainty of the mean flux[b]	Error-weighted campaign mean flux[c]	Uncertainty of the weighted mean flux[bd]	Above-LOD flux observations[dc]

				(μmol·m^{-2}·d^{-1})					upward(↑) / downward(↓)
Acetone	$C_3H_6O{\cdot}H^+$	59.049	*31*	[−25.5, +3.7]	−4.7	*1.3*	-2.5	*0.55*	0↑ / 16↓
Dimethyl sulfide (DMS)	$C_2H_6S{\cdot}H^+$	63.026	*29*	[−1.3, +5.0]	+1.4	*0.65*	+0.52	*0.19*	7↑ / 0↓
Methanol	$CH_4O{\cdot}H^+$	33.033	*29*	[−38.1, +85.4]	+0.10	*4.3*	-2.6	*1.9*	1↑ / 2↓
Methylethyl ketone (MEK)	$C_4H_8O{\cdot}H^+$	73.065	*30*	[−2.1, +11.4]	+0.56	*0.38*	+0.11	*0.18*	5↑ / 1↓
Acetaldehyde	$C_2H_4O{\cdot}H^+$	45.033	*28*	[−5.8, +33.4]	+2.9	*1.5*	+0.51	*0.66*	4↑ / 2↓
Acetonitrile	$C_2H_3N{\cdot}H^+$	42.034	*28*	[−2.9, +5.1]	+0.41	*0.61*	+0.080	*0.21*	2↑ / 1↓
Benzene	$C_6H_6{\cdot}H^+$	79.054	*23*	[−0.7, +2.0]	+0.31	*0.36*	+0.19	*0.15*	2↑ / 0↓
Methanethiol	$CH_4S{\cdot}H^+$	49.011	*11*	[−0.3, +1.4]	+0.26	*0.40*	+0.13	*0.12*	0↑ / 0↓
Dimethyl sulfoxide (DMSO)	$C_2H_6SO{\cdot}H^+$	79.021	*28*	[−0.8, +0.6]	+0.0038	*0.11*	+0.00013	*0.034*	0↑ / 0↓
Toluene	$C_7H_8{\cdot}H^+$	93.070	*28*	[−0.9, +0.6]	+0.0066	*0.16*	+0.071	*0.078*	0↑ / 0↓
Monoterpenes	$C_{10}H_{16}{\cdot}H^+$	137.132	*12*	[−0.4, +0.5]	−0.028	*0.36*	+0.028	*0.16*	0↑ / 0↓
C8 aromatics	$C_8H_{10}{\cdot}H^+$	107.086	*13*	[−0.5, +0.4]	+0.0087	*0.20*	+0.028	*0.076*	0↑ / 0↓
C9 aromatics	$C_9H_{12}{\cdot}H^+$	121.101	*11*	[−0.1, +0.4]	+0.079	*0.12*	+0.056	*0.078*	0↑ / 0↓

[a,c] Includes below-LOD fluxes.

[b,d] 3σ uncertainty.

[b] Calculated via error propagation (Taylor, 1997; see Ch. 3).

[c,d] ~~Includes below-LOD fluxes;~~ Computed following Taylor (1997; see Ch.7).

[d,e] Number of flux measurements that exceed the corresponding observation-specific flux LOD (3σ)."

Section 5.1 This section is interpreted in the context of the expected air-sea fluxes. However the authors never mentioned the height of their 'low level legs'.

In fact this information is provided in Sect. 4.1 and in Table S1. See our response to Line 105.

Section 5.1-continued. EC fluxes are generally thought to be representative of surface fluxes if they're measured within the surface layer of the atmosphere, which is roughly lowest 10% of the marine boundary layer. Thus if the boundary layer is 500 m, the aircraft would need to fly below 50 m for the flues to be representative of surface exchange. Above the surface layer there's expected to be a vertical gradient in flux (driven by the strength of surface flux and entrainment flux at top of the marine boundary layer).

We have now added a paragraph discussing the potential role of vertical flux divergence:

"5.3 Potential impact of flux divergence
Prior studies have examined the role of vertical flux divergence for VOCs in the MBL, focusing on DMS (e.g., Bandy et al., 2002; Stevens et al., 2003; Faloona et al., 2005; Conley et al., 2009) and its oxidation products (Novak et al., 2021). Processes that can lead to a change in EC-measured fluxes with height include: i) photochemical oxidation or production of a given VOC; ii) entrainment of VOC-depleted or VOC-enriched air from aloft; iii) horizontal advection / changing source footprint with height; or iv) cloud processing. The sign of the expected effect varies depending on the dominant process and on the source-sink profile for a given VOC. Flux legs analyzed here range in elevation from 126-219 m, and based on the gradients reported in the above-cited studies we expect air-sea exchange to be the predominant influence within that range. However, NAAMES did not include sufficient multi-level sampling for us to quantify the impact of vertical flux divergence across the study, and we cannot rule out its role entirely."

Section 5.2 Eddy covariance fluxes, as the authors have shown, have relatively large random uncertainties that require sufficient averaging to draw robust conclusions. I'm not saying that the conclusions in this section is wrong, but given the very low number of above LOD flux segments for these VOCs, it's very difficult to say how representative the aircraft fluxes are. I'd be tempted to minimize/remove this section. But if the authors want to keep it in, put a disclaimer at the beginning of the section about how limited the data coverage is.

Thank you for the suggestion. We have added a disclaimer about representativeness to the opening paragraphs of Sect. 5.

"Across VOCs, acetone and DMS have the greatest number of flux measurements with magnitudes exceeding the corresponding observation-specific LOD (**Table 1**). The fraction of above-detection fluxes for other species ranges from 0-21 %. However, analyses above showed that the NAAMES VOC flux errors and LODs are dominated by random USN effects, and it follows that these uncertainties are reduced through averaging. Accordingly, we see in **Table 1** that in some cases the magnitude of the campaign-mean flux (and/or the error-weighted mean flux) exceeds its uncertainty level—even with just a few above-detection flux measurements. These compound-specific results are discussed further in the next section. In all cases the limited number of flux observations and the regional sampling during NAAMES mean that results need to be interpreted with caution in terms of their broader spatial/temporal representativeness."

Note: I co-reviewed this paper with my PhD student, Irene Monreal-Campos

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