

Interactive comment on “Instrumentation and Measurement Strategy for the NOAA SENEX Aircraft Campaign as Part of the Southeast Atmosphere Study 2013” by C. Warneke et al.

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We thank both reviewers for their insightful comments, which have helped to improve the organization of the paper and added clarification in various places. The main change to the manuscript is moving the individual instrument descriptions from the SI to the main text in an Appendix as suggested by reviewer 2. In the following we respond to each comment individually and describe the changes to the manuscript.

Reviewer 1:

The first sentence of the abstract should be a statement that can be quantified. Has it or hasn't warmed more than the rest of the US?

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There is still substantial discussion in the community about the Southeastern US “warming hole”. Several papers have demonstrated the existence of the warming hole and what is being discussed are the underlying causes and whether or not it is going away. Nevertheless, we have decided to remove this statement from the abstract rather than trying to quantify it.

Line 55: The authors are teasing the reader “...were almost all within the stated uncertainties.” The authors should spill the beans. Which ones weren’t within the stated uncertainties?

We have added that two of the three NO₂ measurements comparisons were outside the stated uncertainties.

Minor comment: the authors used many sections at the start of the paper. Sections 2-4 ought to be lumped together.

We have combined sections 2-4 into one as suggested: 2. Aircraft measurement description.

In section 5, where the authors start to discuss individual compounds, this reader thought it might be useful to explain their relevance to the SENEX science objectives. Just a few sentences to set the scene.

We mention in the text in Section 2 that the WP-3D payload was specifically designed to answer all the SENEX science questions, but we cannot list all the science questions that will be addressed by each measurement, because a combination of most of the instruments is needed to answer the various science questions.

Line 217: Several VOCs? Could the authors be less vague? A minor edit would help the reader navigate the paper much better.

We have added the list of VOCs to the text as suggested.

Line 265: Tell the reader how the power plants plumes were removed.

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We were looking at the location of the power plants, the wind direction and the large NO_x enhancements in those plumes and removed those data for this plot. During flight planning we have targeted all these power plant and the plumes are very obvious in the data and easily removed. We have added this to the text.

Line 270. Last statement about missing fraction “may be” comprised largely. . . . supposition? The authors need to provide a little more argument.

Lee et al., 2014 have detected many isoprene and monoterpene nitrates, but the sum of organic nitrates has not been quantified during SENEX. This means that the difference between the sum of the individual NO_y compounds and the total NO_y measurements contains isoprene and monoterpene nitrates, but we cannot quantify how large they are. We have reformulated this sentence accordingly.

In places, the text was a bit wordy. For example, Line 288...We then plotted it.... punchier text would be “Figure 8 shows”. . . . A related comment was that description of the data being plotted should be reserved for the figure caption leaving the main text for interpretation of the data.

We have reworded this sentence as suggested by the reviewer.

Line 299: Again the authors are teasing the reader. Space is limited but a short summary of the findings would be appreciated.

The list of instruments that can be compared between the two platforms is extensive and a short summary would not do this platform inter-comparison justice, in fact the inter-comparison is so extensive that it could almost warrant a separate paper. The data from both platforms are publicly available, so if readers were interested in a specific measurement comparison, it would be easy to look at this flight. So we want to let the reader of this manuscript know that there was an inter-comparison effort between platforms, but a description is beyond the scope of this paper. We understand the reviewers concern, but nevertheless have decided to leave the mention of the platform

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inter-comparison unchanged.

Line 392: The modelling section is poor. The authors have done a good job at describing the experiment, instrument payload etc but they have provided almost no information about the models being used to interpret the data. Surely, they are using more than FLEXPART? Even if we assume the authors are just relying FLEXPART, more information should be provided on the calculations being shown, e.g. spatial and temporal resolution of meteorological data being used, etc.

We have added more details to the modeling section as suggested by the reviewer. Several papers have been published since the submission of this paper and we have added all the new references to the AM3 model, MCM model and FLEXPART-WRF. We also moved the list of available models to the begin of this paragraph and explain that the modeling results shown in Figure 13 is only an example of all the available modeling data. We have also added the details of the FLEXPART model calculations that were used for the model output shown in Figure 13.

Figure 5: The color coding is a good idea but the palette used is so coarse that you could assume from the Figure that SENEX included only 3-4 flights. Maybe include a color scale? Why was the yellow flight different?

We have changed the color coding to show better that this Figure includes all the flights. As stated in the text, we don't know why some flights were different than others.

Figure 9: The lower panel is very busy. A suggestion would be to plot the isoprene emissions in grayscale and overplot the data in color. Otherwise it is difficult to interpret.

We have changed the isoprene emissions color scale as suggested.

Figure 12: The color scale for CO₂ has tick mark labels that overlap. You can guess the numbers but using a small font is advisable.

We have fixed the CO₂ color scale label.

Reviewer 2:

Major comments: This is maybe one for the editor to decide but I was surprised to find all the detail on the instruments to be in the supplementary information. I realise moving it to the main text would make the paper considerably longer, however I found it quite difficult to follow, especially the section on instrument comparisons, without having the instrumental descriptions directly to hand in the main text.

The reviewer has a good point about the instrument descriptions in the supplement and not in the main text. Before submission, we had this discussion among the co-authors with varying opinions. As a compromise in this revised version, we have moved the instrument descriptions as an Appendix into the main text. In this way, the instrument descriptions (and the inlet information as discussed in the following comment) are easily available and part of the manuscript, but do not increase the length of the main text.

Section 2: One thing I found obviously missing from any description is details of the inlets. The authors should explain how the air is sampled into the aircraft cabin and how does this differ from instrument to instrument? Also, has any characterisation work been carried out on the inlet systems, even if it is cited work from other sources? This is particularly important for the aerosol instrumentation and instruments measuring more reactive or ‘sticky’ compounds (e.g. reactive nitrogen compounds) and would no doubt be of interest to readers of the manuscript. Also for the gas phase measurements, the section on instrument comparisons would be strengthened by knowing that instruments had the same (or at least similar) inlets and losses were minimized. I realise some of this information is in the SI included in the information on individual instruments, however a summary of the inlets and maybe including the type of inlet in the instrument table would be beneficial.

We agree that the information about the inlets is very important, but was somewhat hidden in the supplementary information in the original manuscript. As described above,

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we have now moved the instrument descriptions with the inlet information to the Appendix to have it easier accessible. In addition, we have added a short paragraph at the end of the gas phase instrumentation section summarizing the various inlets for the gas phase instrumentation and also stated clearly that all the aerosol instruments sampled from the low turbulence inlet (LTI). Only the NMASS measuring ultrafine particles had a dedicated inlet.

Section 5, line 217: The authors talk about comparison between the iWAS/GCMS and PTR-MS VOC measurements and state that the PTR-MS data are averaged over an interval 10s before to 10s after the WAS sampling period. I do not really understand why they have done this. Surely averaging over the full WAS period would make more sense. In the PTR-MS instrumental description it is stated that it has a time resolution of 1s so I would have thought using, for example, the middle 5 s of the Was period would have been better. Could the authors further explain their reasoning here?

The canister fill time is 3-10s and the PTR-MS measured every compound for 1s every 17s, which means that the PTR-MS data are sparse. To ensure that all the canisters are used in the inter-comparison we average the PTR-MS over this longer time period. This increases the scatter for highly variable compounds such as isoprene, but improves the statistic because of the larger number of data points. This is now explained in the main text as well.

Section 5, line 270: Why are the organic nitrates derived from isoprene or monoterpene oxidation not measured by the total NO_y instrument? I would have thought that these compounds would also degrade to NO on the heated gold catalyst.

The organic nitrates are indeed measured by the NO_y instrument, but they were not added to the sum of individually measured NO_y components. We have clarified this in the text. Please also see similar comment from Reviewer 1.

Section 6.3: why have the authors chose NO_y as a compound for the example comparison with the FLEXPART model. They state that the FLEXPART model does not

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contain any chemistry so surely a more specific and inert species such as CO or CO₂ would provide a better initial comparison, as appose to something as complex as total reactive nitrogen.

NO_y makes for a very easy to explain example of how FLEXPART works, NO_y has a large enhancements from local sources on top of a relatively low atmospheric background. NO_y is much better conserved than NO and NO₂ and can be seen as an inert tracer on the timescales shown in Figure 13. NO_y is therefore an appropriate species to be compared with FLEXPART. CO on the other hand is comprised of North American emissions, biomass burning and an Asian emission transport contribution. The comparison of CO measurements and FLEXPART model will be the topic of a paper that is in preparation right now. This paper will also include a section about secondary CO (and organic aerosol) likely formed from biogenic isoprene emissions. CO₂ was not modeled with FLEXPART. We have therefore decided not to change to CO in this manuscript.

Minor comments: Section 2: The authors should also mention the speed that the aircraft travels. This is an important factor, especially when considering the type of data the project is interested in (emissions using mass balance and eddy covariance calculations).

In the boundary layer the aircraft travels at about 115 m/s, which means that for most instruments measuring at 1Hz one data point is an average of 115m. This has been added to the text as suggested.

Figure 8: change the colour scale of density

We have changed the color scale as suggested.

Figure 9: Unit of isoprene emissions?

We have added the units of the isoprene emissions.

Figure 11: Could a different (may logarithmic) colour scale be used for NO_y to show

the features that are no doubt in the data flying over the city?

We want to compare the 1999 data in Figure 11 to the 2013 data in Figure 9 and have chosen the color scale to match in both Figures, so that the higher mixing ratios are clearly visible between the two Figures. We think it is more important to show this difference than the features of flying over the city in 1999 and have therefore left the color scale unchanged, but pointed out the color scale comparison in the text.

Figure 12: Make the CO₂ colour scale font smaller so the individual numbers can be seen.

We have fixed the CO₂ color scale label.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2015-388, 2016.

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