Review of Lee et al., ACPD https://doi.org/10.5194/acp-2022-600

The clear explanation of the set up of the measurements at the 3 stations, scale propagation and uncertainty would be valuable to others developing and refining greenhouse gas measurement systems. The publication of the high quality data from the stations, and discussion of the regional context is good. I think further work should be done on the source identification (section 3.5). This may change the overall conclusion of the work – i.e. whether the changes in the region are a relative increase in biogenic sources or not.

Before publication some edits are required.

Isotopic signatures of source regions

The section on the isotopic signatures for identifying the predominant sources is interesting but needs some more work:

The Keeling plot technique ($\delta^{13}C$ against $1/CH_4$) is only appropriate for a constant background. Some detail is required about how these Keeling plots were constructed. Is a constant background appropriate or are you plotting data over a period when there will be seasonal variability or interannual differences in the background? If you are plotting data over several years and the global background methane mole fraction and $\delta^{13}C$ are changing then Keeling plots should not be used. If the constant background assumption cannot be made then Miller-Tans plots could be used instead to identify the source isotopic composition, e.g. Miller and Tans, 2003 https://doi.org/10.1034/j.1600-0889.2003.00020.x; Al-Shalan et al., 2022 https://doi.org/10.1016/j.atmosenv.2021.118763; Varga et al., 2021.

Figure 9

In (c) and (d) we see that source signatures for CS and KL increased in 2016-2020 compared with 2006-2010. This seems to contrast with line 13 on page 21 which talks of a decreasing trend in $\delta^{13}C$

What are the uncertainties in the trends in (e) and (f). Can you really say there is a trend?

References

The reference lists needs editing. Some of the references were missing from the reference list:

Watanabe et al., 2000; Remann et al., 2004; Remann et al., 2008; Li et al., 2017; Turnbull et al., 2015.

The reference Shuang-Xi Fang et al. (2013) should be deleted as this is already listed as Fang et al., 2013.

Kim et al., 2014 on page 2 should be Kim et al., 2015 to match the reference list.

Other questions:

As AMD is affected by local sources it would be helpful to use an inventory to suggest quantitatively what the anthropogenic emissions sources are (e.g. EDGAR, or UNFCCC) in the introduction.

Figure 1 – add a scale bar to this map.

What was the reason for drying the air rather than using the water correction built into Picarro software (see Rella et al., 2013)?

Page 2, line 31. A large ratio, CH_4/C_2H_6 – explain what that means. Is high methane but low ethane indicative of a biogenic source?

Page 3 – lines 29/30. Were there 2 garbage incinerators or one? This part needs clarifying.

Page 7 – it's not clear how the filtering was applied to the data using HS, CD and MS. Are data outside of 1 s.d. of the mean filtered out?

Page 8 – line 15 – typo in HYSPLIT

Figure 7 – The growth rates for AMY, JGS and ULD aren't shown for 2018. I think this is because they are negative (-1 ppb) but this is still a result so they should be shown.

Why are some of the numbers in Table 4 written in bold?

Figure 8 – 2009 was unusual because there was no seasonal cycle – can you comment on why this was?

I didn't fully understand the PSS analysis – I think that needs some more detail.