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Interactive comment on “Local and regional scale measurements of CH₄, δ¹³CH₄, and C₂H₆ in the Uintah Basin using a mobile stable isotope analyzer” by C. W. Rella et al.

Anonymous Referee #2

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This is an interesting paper that is certainly worth publishing as it describes in detail the development of a new method to measure ethane concentration in the atmosphere, as well as expanding on the measurement of methane from a mobile platform. The abstract and introduction are generally concise and describe the paper well. The flow of the paper is good. The figures are also generally good; I have some minor changes suggested below. Overall, I feel that this paper requires minor revision. Following are a number of queries and suggested changes. 1. In the introduction, no mention is made of what background CH₄ and C₂H₆ levels are in the US. This would be good information, as well as giving the range of values for both as observed in the Uintah.

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For example, background C₂H₆ is ~0.5 ppb. The stated measurement precision of the analyzer is given +/- 20 ppb for C₂H₆: that doesn't sound so good. From figure 16b it appears that C₂H₆ levels around gas wells may be ~1 ppm. I think that some discussion of background vs. observed concentrations of both gasses would be interesting in the Introduction. 2. A lot of effort is put into justifying the need for the "long" tube in the search for the regional signature (as opposed to the "short" tube used for in-field data playback), i.e. that the results capture nearly all of the data seen in the field. While this is interesting and presents an intriguing way to capture data without the need to have an analyzer present, no evidence is shown that supports that the result is "better" than the results for the short tube. There are statements that the advantage is that standards can be used more frequently during the long tube playback, but that that was not done here, etc. It would be interesting to see how the results compare with what was measured in closer to "real time". 3. In figure 12 the $\delta^{13}\text{C}_{\text{CH}_4}$ values for the Uintah gas and oil that are used as "typical" values are based on the centroids/averages of the 28 tube short samples. It seems to me that no matter how well each sample characterizes a single source, the samples are still going to be made up of mixtures of at least "regional air", "gas air" and "oil air". It appears likely that the actual isotope ratio for oil air will be lower than the average, and similarly the gas air will be higher than the average. Similar arguments could be made about the C₁/C₂ ratio. This should be stated. It is interesting to look at the Keeling plot in regards to this, as it appears to be showing this, albeit the statistics on the fit will not be good. You state that there is no fractionation (p. 4882, 1st paragraph), so the difference may be small. 4. I think that the "regional atmospheric signature" section needs to be reorganized. It starts with extensive equation development, breaks suddenly (to me) into a description of data collection (page 4888, line 10) and then breaks back into more equation development and results after that (p. 4889, line 18). This was confusing and should be reorganized. 5. Minor: There are a few places where the authors state things like: "for this experiment, we will assume that the term C'(t) is zero" (p. 4876, line 2). Obviously the assumption is that the contribution from this term is negligible, but this should be stated (and a reason

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given?). 6. Minor: My understanding is that Air Core is a) patented and b) actually a tube closed at one end. Wouldn't it be more correct to describe the two tube-based systems shown here as being "based on Air Core"? 7. Minor: Equation 1 looks like the Allan variance, not the Allan standard deviation. 8. Minor: In page 4872, line 23 mass flow controller should have (MFC) after it to clarify what MFC is in Figure 4. 9. Minor: The equation numbering is wrong in the Supplement. There are two Eq. 2's and 3's. And S Eq. 5 mentioned in the main body does not match the supplement equation 5. Even more minor: the equations in the supplement are numbered with SM, while in the supplement they start with an S. 10. Minor: on page 4872, lines 7 to 8 – it appears that the authors wanted to supply model and vendor information. 11. Minor: p. 4878, line 3 – $\delta^{13}\text{CH}_4$ is not a mole fraction. 12. Minor: p. 4886, line 22, it is stated that $D=0.2\text{ cm}^2/\text{s}$, without attribution. This information is given in the supplement, but not here.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 4859, 2015.

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