

I appreciate the author's response to my review. I suggest publication of the paper after consideration of some minor points below.

- 1) I am still concerned that the measurements were made with an inlet heated to 200 C. The authors state that this did not impact lab calibrations. I would not expect them too. I would be concerned that heating the inlet leads to secondary chemistry that impacts their pyruvic signal under ambient conditions in the boreal forest. I agree this will never be sorted out since they didn't test this in the field but I think this needs to be mentioned as a potential issue.
- 2) I understand much better how the background was determined. However, I don't agree with ignoring the results from the scrubber. In fact, I would expect the instrument background of a condensable substance to behave this way. So I think you should subtract the measured background. I think the zero air background is largely meaningless. The authors do put a large error bar on the measurement to account for the background. However, I think they need to take out the statement that they it is inappropriate to use the scrubbed background.
- 3) I don't understand why you can't report a typical sensitivity. I understand you are normalizing your signal (even though you may not have measured all your effective reagent ions) to the I- signal. So I still think it is worth reporting your sensitivity at your average level of reagent ion.
- 4)
The use of the GC vs. PTR-MS data for isoprene and monoterpenes still needs to be clarified as they look pretty different. This is an important point as I think the GC data should be more immune to interferences than the PTR data. So why leave this off Figure 2? Why not show both? Which dataset is more applicable? There needs to be some discussion of this issue.
- 5) I am still bothered that the pyruvic never goes to zero and is often maximum at night. I understand the monoterpenes can do the same at night as well. This is partly due the sink not being as large as night as well as the collapse of the boundary layer. However, the monoterpenes are not soluble. So I think the striking difference between the Matilla et al work and the boreal results in this paper at least needs to be acknowledged and discussed to some extent. The Farmer group data was a very nice examination of both the altitude and diurnal profile of pyruvic. Their results are inconsistent with the results in this work. Perhaps it is due to the differences in emission rates due to vegetation etc. (plausible) or very different boundary layer dynamics (hard to believe). So not discussing the comparison of the measurements is a detriment to this paper.

