

The authors have an important dataset that captures changes in emissions sources and meteorological conditions in a location that has seen previous study. The results provide some insight in changes in atmospheric composition and reactivity driven by changes in source and/or meteorological conditions that generally contribute to our understanding of atmospheric chemistry. The methods are appropriate, with some minor comments and suggestions given below. These strengths demonstrate that this manuscript is appropriate for consideration as a measurement report in ACP. Following minor comments and suggestions on the methods are broader scientific comments and concerns that should be address in the revision stage. Some of these comments and concerns will naturally resolve with the change in format for manuscript type, and a tighter focus on the campaign and results.

Minor Comments (with a focus on Methods)

Line 89-Several **hundreds** of compounds have been identified in wildfire smoke (see for example Hatch et al. <https://doi.org/10.5194/acp-15-1865-2015>, Koss et al. <https://doi.org/10.5194/acp-18-3299-2018>, Selimovic et al. <https://doi.org/10.5194/acp-18-2929-2018>, Binte-Shahid et al. <https://doi.org/10.5194/gmd-17-7679-2024>).

Line 94-While it is true that there are significant uncertainties due to the huge variability inherent in fires and emissions, there are a lot of studies that have looked at the influence of wildfire emissions on atmospheric chemistry and reactivity that should be cited here (see for example Gilman et al. [doi:10.5194/acp-15-13915-2015](https://doi.org/10.5194/acp-15-13915-2015), Liu et al. [doi/10.1002/2016JD025040](https://doi.org/10.1002/2016JD025040), Kumar et al. <https://www.nature.com/articles/s41598-017-19139-3>, Permar et al. <https://pubs.rsc.org/en/content/articlehtml/2023/ea/d2ea00063f>).

It might be useful to include a map of the measurement location, particularly showing the influence of biogenic emissions and smoke transport during the study period.

It is recommended to review the first sentences of the paragraph starting on line 99-some of these details would be more appropriate in the methods.

Line 111-why are vehicle emissions expected to persist in the forest?

Line 144-while it makes some sense to use the HRRR forecasts to identify periods with smoke influence (which need to be more clearly defined in the methods-i.e., what periods met the established threshold?), it would strengthen the paper to use measurements to confirm impacts of smoke during these periods (e.g., using PM_{2.5} and/or CO data, or even acetonitrile or other known fire tracers measured during the campaign).

There are a lot of details about operation of the PTRMS that could be moved to the SI (e.g., equations 1-3 and associated text). The authors may want to include additional references

on the use of PTRMS for measurement of monoterpenes and known challenges/interferences if relevant.

It is not clear what value was used for the reaction rate constant of the summed monoterpenes with OH and how that was calculated or what assumptions were made.

Line 430-log saturation “mixing ratio” should be log saturation vapor concentration

Broader Comments on Science and Scope

The mechanisms by which climate change and climate extremes affect biogenic, anthropogenic, and wildfire emissions are very different, and that complexity is not clearly or adequately addressed in the manuscript as currently written. For example, in the abstract lines 17-18 describe the “...response of VOCs to future conditions such as extreme heat and wildfire events...”. It is well documented that biogenic emissions can be temperature dependent, with compound- and species-specific differences. It is also well documented that some anthropogenic emissions can also be temperature dependent, e.g., asphalt emissions, but that strong temperature dependence is typically more indicative of biogenic emissions. In addition, reaction product formation can also have some dependence on temperature (as temperature can influence oxidant levels and reaction rates). This is addressed to some extent in section 3.2, but some refinements are needed. 1-The authors state the at the BVOCs “respond well” to variations in temperature. What does this mean? They behave as expected/consistent with other studies? 2-It appears the isoprene and monoterpenes have the same exponential response to temperature. I do not think that this is completely consistent with other studies (or for example the temperature response as would be predicted in a biogenic emissions model), as isoprene typically shows a much stronger response to temperature. There may be some studies that show very strong dependence of monoterpene emissions in extreme temperatures, but this is not clear as this section is currently written. It is suggested that the temperature sensitivity of isoprene and monoterpenes be discussed separately in this section, including context from references that are specific to isoprene and to monoterpenes. The authors state that the enhanced emissions of monoterpenes “can also be linked to”, suggesting that at least one other mechanistic reason for enhanced emissions has already been discussed, but this is not the case. One suggestion is to more generally summarize (with appropriate citations) the reasons for enhanced BVOC emissions with increased temperatures (and not to get into any specific mechanisms). 3-The discussion of MVK and MACr is confusing as written. The discussion seems to be mixing the effects of temperature on emissions of isoprene (and therefore its oxidation products) with the effects of temperature on lifetime (which could alter the ratio as a function of temperature). These effects need to be more clearly differentiated in this

section and this distinction should be considered throughout the manuscript when discussing temperature effects particularly when the compounds can be oxidation products. 4-Toluene is also emitted from biogenic sources. The discussion around toluene (including attributing everything to interference/fragmentation of monoterpenes) needs to be reconsidered and revised accordingly in this context. 5-“Combustion” is used to describe anthropogenic emissions (as in this section) and wildfire emissions which is confusing and should be revised throughout. It is more typical to refer to wildfire emissions as wildfire emissions, biomass burning emissions, or pyrogenic emissions. It is suggested to choose one of those more commonly used terms and use it throughout (right now there is some use of wildfire and biomass burning but it is inconsistent). Back to the line in the abstract, wildfire emissions don’t “respond” to temperature in the same way that biogenic emissions or anthropogenic emissions do. There are complex relationships between ambient temperature and fuel moisture, and therefore fire ignitions, fire spread, fire severity, fuel consumption, etc. that affect emissions. This complexity is not acknowledged in the manuscript (e.g., by saying that wildfire emissions “respond” to temperature) and the rationale for discussing the wildfire emissions in the context of temperature is not well supported. A related comment, on line 193, it is stated that acetonitrile and catechol don’t follow the trend of temperature (presumably this means they do not increase with temperature like the BVOCs), which is not unexpected based the complexities noted above. However, in line 394 it is concluded that this is in fact because of the “infrequent emissions of BB plumes”. It is not clear what is meant by that.

As noted above, the discussion of wildfire emissions is not sufficiently differentiated from anthropogenic emissions throughout the manuscript. This leads to some confusion about the results and ultimately, the implications of this work. In addition, “monoterpene” is used throughout and described on line 285 as being composed of several organic species. This is not accurate or consistent with existing literature. Monoterpene specifically describes a class of organic compounds with the formula $C_{10}H_{16}$, of which there are several isomers (some of which are listed on line 285). The analytical technique being used can’t differentiate isomers (as noted in the methods), so what is being reported is the sum of monoterpenes (plural) and that needs to be clearer. The monoterpenes in that mixture can have very different reactivities, lifetimes, etc. and the manuscript should be revised with this in mind. For example, what was assumed for the OH reactivity of the monoterpene mixture?

The manuscript has a lot of detail that does not necessarily support or benefit from the measurements and results presented. For example, there is scattered mention of climate, climate forcing, future climate scenarios, and aerosols (including new particle formation). (One of the reviewers notes this of the introduction.) The connections between emissions

composition, chemistry, aerosols, and climate could be made more generally, but the repeated mention does not strengthen the manuscript since the measurements and results are not clearly and sufficiently linked to these complex processes. Refining the focus to the measurements and results, including placing them in the context of similar studies, will help improve this aspect of the manuscript.