Review of ACP-2016-866

This paper is the overview paper for a special issue on the SAFIRED experiment, and is intended to introduce the project, provide details on the instrumentation, meteorology, and give a brief synopsis of the results that will be discussed in the individual papers. This paper does a reasonable job of this, although with a number of places that require some clarification (see below). One major drawback with this paper is that it is too long and spends too much time and detail on the results. Presumably, those results are covered in detail in the individual papers. Another problem is that the text in almost all the figures is too small to read, and the time needs to be defined, is it local time?

Abstract; This section is too long and needs to be tightened up considerably. There are also the following problems:
Line 45. How does one measure the "mercury cycle"? It is possible to measure the chemical species that make up the mercury cycle.
Line 47. The word "emitted' is redundant.
Lines 52 and 53. What distinguishes 'intense' and 'close' smoke plumes?
Lines 59 and 60. These few sentences are examples of extraneous material not appropriate to an abstract.

Introduction:
Lines 80 and 81. Savannah and grassland fires are not the largest source of carbon to the atmosphere, as is clear when comparing the numbers from the quoted references with the global anthropogenic source of CO_2 for example. Do the authors mean the largest source of black carbon?
Line 109. NOx is not an incomplete combustion product, in fact NO is most definitely a flaming stage compound. The authors would know this if they referred to the numerous references that have come after Crutzen and Andreae [1990], Akagi et al [2011] (referenced later on)s is a nice recent review of BB emissions.

Description of experiment;
Line 194. If this is meant to be only place the details of PTRMS calibration is discussed, then we need a reference or further explanation for calculating approximate response factors.
Line 241. The term PBM need to be defined here.
Lines 256-258. Doesn't the CO_2 from fires mess with the 'transfer velocity' measurements?
Lines 265-267. The half-life of Radon is much longer than that of NOx (which is about 3-4 hours) about the same as SO_2, and shorter than aerosols (which is about 2 weeks), not sure how to think about water vapor.
Line 307. What is a 'Total Suspended Particulate style inlet'? I've never heard of this, so it needs to be explained further or a reference given that explains it.
Line 335. Are you saying that the extracts have not been analyzed yet?

Overview of the Campaign;
Lines 446-450. Isn't it both boundary layer mixing and time spent over land that determine Rn concentrations? This section needs a better explanation of how these two effects were differentiated.

Line 457 and Table 1. The term “background concentration” is difficult to define and is not consistently applied in this paper. Those medians should not be considered “background” values. Background to me means the value that would be observed in the absence of a continental source (urban, fire etc.). This is particularly true for CO, 130ppbv is much higher than background, which is probably around 90. I point out that later in the paper, (line 554) much lower numbers were quoted for O3 and CO, 10 ppbv and 66ppbv, which are obviously too low.
Table 1. The measurements do not justify the number of significant figures reported for most of these quantities.
Figure 5 Caption. Plot (d) is mistakenly attributed to ‘nitrogen dioxide’ and should be nitrous oxide.
Figure 6. The legends and scale insets are too small and, in one case, not next to the panel to which they refer.
Lines 496-498. It is well known in the community that the AMS technique does not work for much of the chloride that one finds in the atmosphere, particularly near coasts, because it is in the form of refractory salts. What is worse is that because of background subtraction issues, other chloride is actually under-measured. Fires likely emit chloride as ammonium chloride, which is volatile and will be measured by AMS. These are probably the main reasons for your observations.
Lines 504-506. Another cause of this effect is the above limitation of the AMS instrument.
Lines553-558. The ratio $\Delta O_3/\Delta CO$ is not a good indicator of photochemical age or processing. One only has to look at the high O_3 plume during BBP4 (6/26 as far as I can tell), which reaches 100ppbv. This obviously had substantial photochemical processing, how else does that much O_3 get made? Yet the ratio is low, probably because the CO had not been mixed out as much as in the other plumes. You need to find some other indicator.
Line 623. The authors seem to be ignoring the high O_3 plume during BBP4 which indicates faster O_3 production in this plume. This would seem to be one of the more interesting observations of this study.