

Interactive comment on “The summertime Boreal forest field measurement intensive (HUMPPA-COPEC-2010): an overview of meteorological and chemical influences” by J. Williams et al.

A.R. MacKenzie (Referee)

r.mackenzie@lancaster.ac.uk

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This paper describes rationale, experiment design and execution and regional-scale influences on the HUMPPA-COPEC field intensive in July/August 2010: this is an “overview” or introductory special-issue paper. Such papers are useful to provide a one-stop-shop for contextual information when many subsequent papers will draw on measurements made during the field intensive, and prevents repetition of such material in the subsequent papers.

C6250

The experimental methodology set out in the paper is very high calibre. I expect that the campaign will yield many interesting results. I recommend this paper for publication subject to the following points being addressed.

Section 2 reads well and gives a clear account of the science targets of HUMPPA-COPEC. However, I think the text takes the wrong perspective in several places, and sounds more like a proposal than a paper written after the event: P15929, lines 13-15, for example. The sentence on lines 9-11 of the following page is also written from the wrong perspective: the authors should be able to say by now whether or not the HUMPPA-COPEC measurements can be compared to theoretical nucleation rates. See also p15932, line 2.

Given the arguments of section 2.8 and the potential importance of segregation of bVOC-laden air and OH-laden air (Pugh et al., Atmos. Chem. Phys., 11, 4121-4134, 2011, and references therein), there should be a more precise discussion of inlet positions in section 3.

The relevance of the high temperatures during the campaign for understanding forest biogeochemistry under climate change is over-stated, I think. Living systems respond differently to short-term and long-term stresses. The passages in the abstract, section 4.1 and Conclusions should explain the extent to which a short high temperature event is like climate change, or these sentences should be dropped.

I would like the Conclusions to mirror the substantive results from the foregoing discussion: that three distinct chemistry-transport regimes were identified (see my comments on Figure 3a and associated text, below); that the boundary layer was surprisingly low (see my comments regarding p15937, below); and that the sawmill increased the extent of “VOC:NO_x space” encountered, hence providing a more rigorous test of our understanding of photochemistry (see my comments on p15940, below).

P15924, line 4: I think “comprises” does not take “of”; the confusion may be with “is comprised of”

C6251

P15926, line 22: Are PTR-MS and GC-PID really instruments to measure “reactivity”?

P15930, line 18: might “biochemically distinct” capture the significance here and help distinguish the responses in pollinators etc from the atmospheric chemistry? Could the sentence beginning on line 25 be re-written to be a little more explanatory? The abstract of the Stokes et al paper contains much clearer descriptions of the experimental results and their implications.

P15931, line 9: it is of course NO_x and VOCs that are necessary for ozone production. This sentence is a bit too vague. The sentence beginning on line 14 has been broken in half.

P15931/15932: the study of Pugh et al. (Atmos. Chem. Phys., 10, 279–298, 2010, www.atmos-chem-phys.net/10/279/2010/) provides further discussion of processes that are important when trying to reconcile measured and modelled NO, biogenic VOCs, OH, etc.

P15932, line 12: “origin in the vertical”? Could this be re-phrased for clarity? What constitutes a “failed event” is not made clear at line 16 – connect to p15941, line 24. I expected to see a reference to a special-issue paper on line 24 or 25.

P15934, line 8: rather than “direct flux measurements” could the flux method be given ((disjunct) (virtual) eddy covariance, presumably).

Figure 1c is a bit too small for my eyes, even when I’m reading the ACPD pdf full-screen. I think you could increase the size of Fig1(c) relative to 1(a) and 1(b) without harming the overall aesthetics.

Figure 3a. I get the general drift of this graphic, but wonder if it might have more value if it was clear how to interpret the arrows. Are we to assume that air parcel trajectories stayed inside these arrows for their 3-day duration or is there another criterion for their classification? Is the blunt end of the arrow the average distance travelled in 3 days? Where is the other 15.3% of air-parcel trajectories? I think I would pre-

C6252

fer a graphic that didn’t raise so many questions. Please also include in the figure or main text some information on the vertical transport by air parcels. Are these trajectories isentropic, isobaric, or kinematic (model vertical velocity)? I expect that many trajectories started at 25m agl crash into the ground before 3 days, how has this been handled in your statistics? It is particularly important to be precise about the trajectory analysis because it forms the basis of an extended discussion over pages 19535 and 19536. Note that a citation is expected for use of hysplit trajectory data in papers: see http://ready.arl.noaa.gov/HYSPLIT_traj.php.

P15935, line 19: should say “wind directions measured at xx m at the site”, surely. Could Figure 3b have the average histogram for this period in previous years so that the anomalous nature of 2010 could be seen easily?

P15935, line 20: as teachers we generally discourage use of “stable” in this context in meteorology – “steady” or “persistent” is less ambiguous.

P15936, lines 4-5. This sentence is not very clear. It is not the year that has a southerly component, but trajectories or winds in that year. Does 74.4% mean this fraction more than the average of other years or this fraction of all winds in 2010? You do not need to be equivocal about whether the southerly winds bring warm air, you can test this statistically with your data and report how much warmer the air was when coming from the south.

P15936, line 28: please name the characteristic turbulent timescale used and/or provide the equation.

P15937, line 3: Is the maximum potential temperature gradient really the only criterion used? At 10am in Figure 4 it looks as if the theta gradient at the top of the domain is at least as big as that at 1000 m.

P15937, line 6: is the boundary layer growth rate or maximum depth of boundary layer being referred to here? I would like to see a slightly more precise discussion

C6253

here, at least showing briefly what boundary layer would be expected to evolve given the surface sensible heat flux. This need not cut across the special issue paper on boundary layer dynamics.

P15938, line 18: It seems to me that it the land cover of urban area should be weighted by the relative difference in emission intensities between forest and city if we are to think about the contribution of urban emissions to photochemistry in air from the southwest. You cannot make the statement on line 26 without a calculation of this kind. Please connect this discussion to that given in section 5.4 so that the reader is aware that these issues are to be covered in more depth subsequently.

P15939, line 24: spelling of field site name.

P15939, line 25: I would prefer a statement here that explains what aspects of these measurements will help us understand which aspects of Arctic haze.

P15940, line 15: rather than the bland statement “may hold clues”, could this section say more precisely by how much the dynamic range of monoterpene concentrations was expanded by the sawmill-impacted episodes, and what other chemical issues might arise (e.g. changes to VOC speciation and associated NO_x emissions)?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 15921, 2011.