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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Environmental controls on pyrocumulus and pyrocumulonimbus initiation and development" *by* N. P. Lareau and C. B. Clements

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Lareau and Clements (hereafter LC) present two case studies wherein pyroconvection is measured at close proximity (within ∼10 km) by Doppler lidar, and characterized further by photography, temperature/humidity/wind sounding, automated surface-weather station, operational weather radar, and satellite-based fire hot spots. By this measurement strategy LC were able to record with great fidelity and temporal resolution the growth of the pyrocloud and nearby environmental information including column thermodynamics. These were two independent wildfire cases in northern California in 2014 and 2015. LC's main objective is to characterize the pyrocloud's base and height

evolution, and associate these data with meteorological metrics that are popularly used to assess fire danger and behavior. From these two cases the authors argue that the pyroconvective cloud base is sufficiently higher than would be expected from standard parcel theory to conclude that combustion-generated moisture is an inconequential ingredient to the updraft. These observations and interpretation add valuable content to an ongoing debate about the role of in situ vs. ambient moisture in fire plume dymanics. Hence this paper is a good fit for ACP.

This work has several strengths. The data set is unique and the up-close, active measurement approach for wildland fire plumes is promising, indeed exciting. The writing is clear and straightforward. However, there is a substantial list of concerns that must be addressed before it can be accepted for publication. These will be broken out below, but in a nutshell, I find the paper is in need of 1. a thorough explanation of their data processing methods, 2. clarity of the lidar-data visualization, and 3. fuller use of strategic ancillary data such as radar and surface weather data. In the paper's present state I find LC's findings intriguing but unconvincing. However, the concerns I raise and the suggestions offered do not amount to a rejection of fundamentals, but rather a call for a fuller accounting of the building blocks of their analysis and fuller use of all the data at their disposal.

Below I record my substantial concerns, followed by minor concerns. Technical items are documented as comment bubbles in the PDF of LC's manuscript, uploaded with this report.

Substantial Concerns/Questions:

Data and Methods: The two case studies are of course different, but they are approached with a similar measurement strategy and instrument suite. To the extent of the similarity, I think a separate section entiteld "Data and Method" is called for. In this section LC can describe the Doppler lidar characteristics, derived products,ancillary data characteristics, the measurement strategy and chronology. E.g. when was measurement started w.r.t. pyroCu onset, how frequent were the scans? Were there any interruptions during the measurement interval?

Regarding all the lidar range-height type backscatter plots, the reader needs some basic information and a fuller description of the way the data are plotted:

* There is a smooth gray background inside the measurement arc. But that shading appears to change with distance from the lidar. What is the reason for that?

* Do the circles faithfully characterize the lidar spatial resolution?

* There seems to be overlap of the circles in some locations. Does this represent multiple/overlapped samples of a given volume?

* What range of azimuth is scanned?

* How many scans comprise these views?

* How much time is taken to fill the scans as shown?

* The circles appear to be open; i.e. only the perimeter of the circle is shaded according to backscatter. Is that correct?

* Some areas of these plots have a diffuse appearance (like the smoke above the lidar in the second case). Is this just a dense bunch of circles with near-homogeneous backscatter? Or is something else being plotted?

Suggestion: Because of the lidar attenuation issue and because LC do employ NEXRAD data to some extent (echo top analysis), could the radar reflectivity data be gainfully employed along with the lidar data to characterize the pyrocloud in the lidar attenuation shadow? Would this add important information that strengthens the lidar analysis?

P29051, L19. Smoke-particle size distributions usually show a mode between 100-200 nm. This does not seem to fall into the category of "micron-sized aerosol." I suggest a

fuller characterization of the lidar's wavelength and the range of particle sizes to which the lidar is sensitive.

In my viewing of the lidar range-height type plots, the distance from front-edge backscatter to total attenuation is as important to understand as the backscatter itself. I.e. the "shadow" behind the cloud is as indicative as the backscatter along the edge. This shadow is seen above/behind the cloud and at altitudes below the cloud as well. Hence there is some ambiguity as to what particles are causing the strong backscatter and shadowing in the different parts of the scanning column. This should be addressed. The Conclusions section mentions debris in the plume. I suspect that this might be a large factor in the strong attenuation below the presumed cloud base. I suggest that the authors discuss in detail the factors causing attenuation variations within and outside the cloud.

P29057 (Section 3.1): Winds displayed in Figure 8 are characterized "ambient" and reported in terms of compass direction and speed. Presumably these data are derived from the lidar. How are wind direction and speed calculated from the lidar data? The Doppler lidar is known to get radial wind information, but it's not clear how 360-degree wind information is retrievable. Also, no information was given as to where the wind information comes from within the scanning volume of the lidar. Please elucidate.

Section 3.1 Plume geometry being introduced here (P29057, L23-26), so some definition of "plume center" is called for. And the reader needs to know what the "objective" and "semi" parts are of the "semi-objective" delineation of plume "center." I bring this up because of the attenuation factor...there's presumably a lot of plume down-range of the wall of strong backscatter seen on the plots. That wall might rightly be viewed as the upwind edge of the updraft column, with an absence of knowledge of the true plume horizontal thickness and vertical shape. This part of the paper is intended to infer updraft velocity, which would be imporant to know. But to make this convincing, much more detail and clarity are needed.

P29058, L4-8. Discussion of a smoke feature between the lidar and the pyrocloud: The sign-change boundary is inconsistent with the backscatter feature that extends from overhead to the pyroCu column. The zero line is ~2300 m; winds below are away from the lidar yet the backscattering feature that is over the lidar bottoms out a few hundred meters lower. Moreover, this feature is over the lidar from the earliest time shown, which means if it came from the Rocky fire it was injected well before that time, and there is no wind data prior to that figure panel to determine where that might have come from. If that overhead smoke layer is to be discussed, I think another explanation, or a more rigorous defense of the present interpretation, is needed.

Section 3.4, discussion of the two-day disparity in pyroconvection occurrence: This argument is interesting but may not tell the whole story. Does the MesoWest data include temperature and windspeed? Might there be a day-day difference in the windspeed near the fire that could explain the different fire behavior? By arguing that increased low-level moisture is partly responsible for the pyroCu, you're seemingly proposing a departure of the moisture component in the "hot, dry, windy" prescription for intense fire behavior. So I think it is important to look at wind, T, and RH in addition to absolute moisture.

Figure 4 and 10: Please describe the fire-perimeter data. Is it from GOES? Fig. 1 has fire-perimeters but these are from MODIS, which are not at 00 UTC (local evening). So it seems these are from another source. The dotted and solid boundaries are not easy to discern due to overlap or proximity. I don't think there is discussion of the fire movement in the text, so do you need to make this distinction? Are the methods consistent between Fig. 4 and 10?

Figure 12: Is it possible that the time here is UTC, not PDT? According to the figure the FRP peaks at 11pm-12 am local time on these two days. This is inconsistent with my expectations; which would have fire intensity peaking closer to late afternoon.

Minor Concerns:

P29051, discussion of Fig. 1: Is it worth it to give the Terra FRP too, in order to partially characterize the fire intensification from morning to afternoon?

P29051, L19: Inform the reader of the lidar's wavelength.

P29051, L21-23: Can you give a number for the smoke SNR? Isn't this issue part and parcel of the above question about the lidar wavelength and particle sensitivity?

P29052, L11-15, discussion of plume geometry and vortices: Isn't the feature being described here just a signature of the cellular nature of cumulus, i.e. the cauliflower texture? Can one infer vortical circulations from these cloud-edge structures? It wasn't convincing to me, but perhaps this just needs to be explained in greater detail or supported with citations of previous such findings.

Fire-generated debris is montioned but only in the concluding section. If this is part of the signature seen in the lidar (and I believe it is), this should be discussed in the body of the paper too.

P29056. Section 3. Please inform the reader of the truck location and distance from the fire.

P29056, L13-16: Were any measurements taken before the cloud formed? If so, what do they look like?

P29057, L3-5, discussion of second flare-up of Rocky fire: Was the lidar moved between these events? Fig 6 and 7 show that the fire plume is 1500-2000 m farther downrange during the 1800 event. If the lidar was not moved, does this signify that much of an advance of the fire front?

P29057, L18-20: If the lidar wind info is coming predominantly from the upwind edge of the plume, the lowest returns are ~600 m above ground level. Hence I would contend that "near the surface" does not apply as well as, perhaps, "lowest-retrieved wind (roughly ??? m AGL)."

P29057, L27: It's not self evident how this number (18m/s) is arrived at. Please elaborate.

P29060, L19 (and generally throughout the paper): Either settle on a single time convention or give PDT as well as UTC here.

Figures 2, 6, 7: Suggestion: If all the photos were taken at the same location, the distance to the fire is constant among photos. Hence if they were all cropped at the same bottom geolocation, the analyst/reader could better compare the column vertical development.

Figure 6, 7: It would be preferable for the RHI plots to have the same aspect ratio. They both go to 8000 m in the vertical but Fig 6 panels are squashed relative to Fig. 7.

Figure 7: What are the whitish blobs apparently lower than the cloud base in the 3 rightmost photos? Is this whiter smoke, or is it cloud? If it is an optical illusion of cloud, some explanation of what the eye beholds is called for.

Figure 10: It is very difficult to make out the fire perimeters on the two days. Please make these more distinct.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C9361/2015/acpd-15-C9361-2015-supplement.pdf

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