The Berg et al. paper is a really nice summary of a lot of work. It describes new algorithms incorporated into WRF-Chem that will be used by the community to study aerosol-cloud interactions and their role in climate. Although the new algorithms are not complete, it is important to document the work that has already been completed. The authors have responded to the reviewers' comments well, although the point about using the average properties for cloud chemistry and transport may warrant more discussion.

At this point, I think the authors need to spend some time making it easy for a colleague to read the paper. Most of this work should be focused on the figures. Specific comments on how to make these improvements are below.

Another major comment is that it seems that the only discussions of previous work are to papers from their own group or institution. The article would receive more appreciation and recognition by the community if other papers are cited. For example, the statement, "the treatment of cloud-aerosol interactions has largely been limited to grid-resolved clouds (Chapman et al., 2009)" could also cite Saide et al. ACP (2012), Zhang et al. ACP (2013), Mashayekhi and Sloan ACP (2014) and/or Eidhammer et al. JGR (2014). In addition, the findings in the results/discussion sections are very much the same as findings from the 1980s when studies were focused on acid rain.

Comment to the responses

Reviewers 1 and 2. Response to Sect. 2.2.1 on choosing the average vertical velocity. Not all processes are represented best by the average, and average vertical velocity for representing average cloud drop number concentration (CDNC) and aqueous chemistry and transport may be one of them. The authors say that aqueous chemistry and transport are less sensitive to cumulus updrafts than CDNC, but did they test this? How big are the differences if maximum w (wmax) or 75% of wmax is used compared to average w? What I would really like to see implemented in the manuscript is a quantitative estimate of how different the results could be from the assumption put forth.

Comments on the manuscript

First, I think the figures could be redrawn to be much easier to read.

Figure 1. It would be great if the font size could be increased. I would also suggest using black font everywhere and decreasing the background color to a light red, light blue, light green, light orange, and light purple.

Figure 2. I suggest making the 3-panel figure one column. It would be nice to have the background color for the WRF simulations be gray. The font size on the color bars and x and y axes need to be larger.

Figure 3. The font size on the x and y axes need to be larger.

Figure 4. The hatching impairs the reader's ability to read the color fill regions. Why not just plot the contour lines (with thick lines) on the color fill? In the figure

caption, make use of the "a), b), c), d)" panel labels to more clearly state what is drawn.

Figure 5. It needs to say "Same as Figure 4, …"

Figure 6. This figure is really hard to read. Font sizes need to be larger. Panels need to be larger.

Figure 7. This is minor, but "Contours in (a) mark contours" is redundant.

Figure 9. Consider making this a column of panels so that the panels can be bigger. The yellow boxes are near impossible to see.

Figure 11. In the figure caption, indicate which simulation is shown (ShallowOnly). It may be useful to add a "total" aerosol mass loading to make it easy for the reader to compare observations and simulation. The "a) 25th June" and "b) 25th June" are not useful. I'd suggest changing them to "a) Mass concentration" and "b) Volume fraction".

Specific Comments on the Manuscript

1. Page 6, lines 26-28. I don't understand how the sub-grid convection can be the same proportion of the grid cell for simulations run at 100 km, 36 km, and 12 km, all of which require a cumulus parameterization. I would appreciate it if the authors could address these intermediate scales that still require a cumulus parameterization and not just jump to the < 5 km grid spacing.
2. Page 11. Lines 17-25. Is there a reason that impaction scavenging, which can occur inside the cloud and below the cloud, is not included? There are many schemes available, even with large simplifications (Dana and Hales, 1976).
3. Page 12, Line 16. Please quantify "this simplification has little impact". Is it < 10% impact when tested at dx = 10 km?
4. Page 13. Lines 8-9 on direct uptake of gases by rain. While I agree it is a small effect, the Seinfeld and Pandis textbook addresses this process. It is not just a mass diffusion process to consider, but also an impaction of gases by rain.
5. Page 15, line 9. "The net changes to the aerosol would be the same in either case". How do the authors know this? Could the differences be quantified?
6. Page 20, lines 18-20. We have known this information on aqueous-phase sulfate production since the acid rain studies in the 1980s. I suggest citing these earlier studies, e.g. Taylor, JAS, 1989; Tremblay and Leighton, JCAM, 1986; Hegg et al., JGR, 1986; Chaumerliac et al., JGR, 1987; Easter and Hobbs, JAS, 1974; Wang and Chang, JGR, 1993.
7. Page 22, lines 1-2. There have been LES studies looking at tracers, simple chemistry and shallow cumulus (Vila et al., ACP, 2005), and gas chemistry and shallow cumulus (Kim et al., JGR, 2012) that discuss the transport of boundary layer species to the cloud layer. It may be good to include their work in support of your findings.
8. Page 23-24. Comparison to B200 measurements. Do you get the same results for ShallowOnly minus Control? I find there is a lot of comparison of the

DeepShallow simulation, but it is not obvious how much is from a good simulation of the shallow cumulus or from the deep convection or both. My impression is that the shallow cumulus parameterization does a good job. I don't know if the deep convection parameterization is good. Figure 8 shows that deep convection was not well simulated.

9. Page 26, lines 25-29. How does the ShallowOnly simulation compare (similar or different) for figure 10? Perhaps a second line could be added to Fig. 10b.
10. Page 27, lines 17-19. We have known about the importance of nitric acid uptake and dissociation since the acid rain studies. The authors could cite these references (see comment 6; Hegg et al. 1986 could be useful here for nitric acid). Also Chang et al., JGR, 1986 (RADM model) may be appropriate to cite.
11. Page 27, lines 20-30. Say which simulation is discussed and why you chose to discuss those results.
12. Page 28, lines 30-31. Again, it is known that nitric acid is dissolved into drops and becomes part of the aerosol.

<u>Clarifications in the manuscript</u>

1. Abstract. Line 13. "WRF-Chem" should be spelled out and V3.2.1 should be added. (and V3.2.1 should be removed from line 2 of page 2).
2. Page 1, line 23 does not need "as well as a high-resolution simulation that does not include parameterized convection"
3. Page 5, line 14. It is awkward to say "less than 2 to 4". Why not just say 2 to 4 km (or 1 to 4)?
4. Page 11. Line 9. What is the value of the time sub-step? Please include.
5. Page 16. Line 23. Please add "subgrid" to say "impact of subgrid shallow clouds".
6. Page 16, last paragraph. Please clarify that grid-scale aerosol-cloud activation, cloud chemistry, and effect on autoconversion is employed in these simulations. Is it possible to do these simulations with grid-scale aerosol-cloud interactions turned off but subgrid-scale interactions turned on?
7. Page 18, line 21. What is the "fine spatial resolution" (dx is what value)?
8. Page 19, lines 9-10. Please include something like, "and the KF-CuP simulation compared better with observations then the KF only simulation"
9. Page 21, line 7. Suggest writing as, "fire located in central New Mexico"
10. Page 23, lines 12-13. "due to issues with the simulated mass loading" is rather vague. I encourage the authors to be a little more specific.
11. Page 26. Line 4. I think it should be "cloud updraft velocity". And "CO" should be defined.
12. Page 28, line 10. May be better to say "… less than was observed"
13. Page 29, line 14. I suggest not using "version" as it can be confused with official versions of WRF-Chem.
14. Page 29, line 22. "latter" should be "last"

15. Table 1. Could the aqueous chemistry be included with a reference?
16. Table 2. Consider having a 3 column table making it more of a matrix.