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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Numerical simulation of "An American Haboob"" *by* A. Vukovic et al.

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Response to Referee #3

General comments

This manuscript describes high resolution numerical simulation of mobilization of dust due to high velocity surface winds produced by strong convective storm and severe downbursts and comparing results with both remote and in situ observations. Successful numerical modeling small-scale patterns within dust storm is very challenging task. Selected dust model NMME-DREAM (a regional coupled atmospheric-dust model), on high horizontal resolution, is managed to produce an accurate shape, duration and magnitude of the dust storm. Three components of dust model are important: successful simulation of weather event, parameterization of the dust cycle and definition

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Interactive Discussion

Discussion Paper

of the mask of potentially dust productive areas. Since the NMME-DREAM model is already well tested on coarser resolution, over large desert regions, the major focus in this paper is on specification of dust sources through mapping of the areas that are dust productive under favorable weather conditions. According to the obtained modeled dust concentration at surface which is several times less than observed, three major sources of uncertainty are emerges: *dust-related parameterization *better definition of the mask of potentially productive areas *the observations themselves where particles of different origin was included but not simulated in this setup of DREAM. Sensitivity of surface concentration of PM10 (Figure 10) on different masks shows how important dust source mapping is. Scientific value of this paper, which aim is to describe small scale processes within “haboobs”, consists in directing attention toward further investigation not only dust source mapping but also parameterization of dust related processes on high resolution as well as further improvement of observations.

The paper seems to lack citation of newer work from this ïňĄeld.

Authors’ response: The Referee points out most important problems in modeling of this kind. Dust sources mapping is most responsible at this stage of research for improving results in high resolution dust modeling. As commented by the referee, more research is needed in testing/improving other dust related parameterizations in the model when applied to high resolutions. Also, in order to provide proper estimation of the model performance, a longer period of simulation is needed that contains dust and no dust episodes. This is also commented below (response to Ref.#1) and in the paper.

As requested by the Referee additional citations are included in the paper:

Johnson, B.T., Brooks, M.E., Walters, D., Woodward, S., Christopher, S., and Schepanski, K.: Assessment of the Met Office dust forecast model using observations from the GERBILS campaign, Q. J. R. Meteorol. Soc., 137: 1131-1148, doi:10.1002/qj.736, 2011.

Tegen, I., Schepanski, K., and Heinold, B.: Comparing two years of Saharan dust

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source activation obtained by regional modelling and satellite observations, Atmos. Chem. Phys., 13: 2381-2390, doi:10.5194/acp-13-2381-2013, 2013.

Schepanski, K., Flamant, C., Chaboureau, J.-P., Kocha, C., Banks, J., R., Brindley H.E., Lavaysse, C., Marnas, F., Pelon, J. and Tulet P.: Characterization of dust emission from alluvial sources using aircraft observations and high-resolution modeling, J. Geophys. Res., 118(13): 7237–7259, doi: 10.1002/jgrd.50538, 2013.

Response to Referee #1

Referee's comment: This study presents the numerical simulation of a severe dust episode that hit Phoenix on 5 July 2011. The "haboob" was generated from the downdrafts produced by storm activity. Similar episodes are commonly found in various arid areas worldwide. This is the first description and modeling of dust elevation by a gravity current in the US. However the authors need to elaborate more on their results and address a few concerns/comments before the manuscript can be accepted for publication.

Authors' response: The authors thank the referee for very useful comments that will improve the quality and understanding of this paper.

General comments

1) Referee's comment: It is not clear if the scope of this work is to investigate the mechanisms that lead in cold pool formation and dust production over southwest US or if the scope is to evaluate the performance of an operational dust model. In either case this should be clearly stated in the abstract and text.

Authors' response: The scope of the paper is not to investigate the nature of the event, but to verify potential performance of the model for dust transport in severe dust events such is the 5 July 2011 Phoenix dust storm. Knowledge about the mechanisms that lead to this dust storm is collected from the references listed in the paper. Also it was not our purpose to validate quality of the atmospheric model NCEP/NMME, since it

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is a well established model, already tested worldwide and used operationally – until recently also in the US on high resolutions. Our main goal is to estimate dust model performance when used on high resolutions. This is now clarified in the Abstract, adding: "The scope of this paper is validation of the dust model performance, and not use of the model as a tool to investigate mechanisms related to the storm. Results demonstrate the potential technical capacity and availability of relevant data to build an operational system for dust storm forecasting as part of a warning system." We have also added a statement in the first paragraph of Chapter 4.

2) Referee's comment: The analysis of model output reveals several critical sensitivities that are mostly related to storm description and dust mobilization in the model. Additional model runs should be performed in order to investigate possible improvements in model performance from: a) different horizontal and vertical resolutions and b) from different convective parameterizations. Is it possible to obtain similar results with a more standard mesoscale resolution? (e.g. NAM 12 km?)

Authors' response: The referee points out very important facts, since the performance of the model can be significantly different depending on the resolution and convective parameterizations. The choice of resolutions is based on the resolution of the operational forecast for this region (∼4km), west-central domain, nested in NAM 12km, http://www.emc.ncep.noaa.gov/mmb/namgrids/ More coarse resolution could not resolve dust sources properly, which has been tested in previous projects mentioned in the paper (ENPHASYS and "Airborne Dust Models: A Tool in Environmental Health Tracking,"). Spatial resolution is high, so the use of explicit convection is permitted, as we did in our simulations. We include additional explanations in Chapter 3, along with the model description. Results on 12km resolutions would not be good because the dust sources would not be resolved properly. In this case study, particularly strong convective activity is involved, which requires the non-hydrostatic mode.

3) Referee's comment: Regarding the operational use of the model, the results should be compared to other modeling systems (e.g. WRF-Chem). More similar cases need

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to be simulated and the authors must justify why NMME-DREAM should be the system of choice for US "haboob" forecasts.

Authors' response: We agree with the referee that the model should be verified for other dust and no dust events, so it can be accepted as a relevant, verified tool for dust forecasting. This understanding is now clarified in our paper's conclusion. We do not propose that NMME-DREAM should be the system of choice for dust forecasting. NMME-DREAM is the model that this paper's authors used, and has been used in previous projects, cited in the paper's references, related to dust generation and transport in SW US region. Principal progress reported herein is related to definition of the dust mask; if other non-hydrostatic models that include dust transport use the same or similar approach for creating the masks on high resolution and changeable in time, we believe dust forecast results could be significantly improved in those models. We hope in future there will be opportunities to compare NMME-DREAM with other models in this region, as there have been outside the US organized through the World Meteorological Organization's SDS-WAS project. Indeed, comparison with WRF-Chem particularly could be useful, since the atmospheric part of this model is ARW (Advanced Research WRF), and is operational in NCEP (besides NMM) also for a western-central domain on 4km resolution. Model (and perhaps dust mask) inter-comparisons are critical to develop and improve reliable dust forecast systems. A new statement in the conclusion explains this.

Specific comments

1. Referee's comment: p.3, l.2: "This storm ... was predicted". Is that really so? The authors should clarify if the results demonstrated here come from a forecast or a retrospective study and modify accordingly the relevant parts in the text.

Authors' response: We use "predicted" since the boundary conditions are from the ECMWF forecast. The setup is the same as it would be in operational forecasting, i.e. the regional model would be forced with boundary conditions used from the forecast

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and not from analysis. This is already in the text: “Initial and boundary conditions are downscaled from the ECMWF forecast data”, and we clarified additionally in the text, as requested by the referee.

2. Referee’s comment: p.7, l.6: (and elsewhere) Please keep chronological reference order.

Authors’ response: Done.

3. Referee’s comment: p.9, l.28 – p.10,l2: What caused the formation of a cold pool north of Phoenix? Please explain.

Authors’ response: Cold pool formation is related to the severe thunderstorm outflows that reached the ground and produced severe downburst. This is added in the text.

4. Referee’s comment: p.10, l.24: Local measurements of dust size distributions (as referred at the introduction section) indicate the existence of larger size modes (>10μm in diameter). The authors should justify the selection of the eight size bins in their model for the specific application and if possible perform an additional run extending the size distribution towards larger radii in accordance with the observations.

Authors’ response: The DREAM model reported in this paper is defined as a model with specific 8 bins, and at this stage we did not change the model, we just used it as it is. Since the observations used for verification are already captured with dust model (PM10), this would not change the analysis or the conclusion of this study. For future operational forecasts it will be necessary to include transport of larger particles since they can affect visibility close to the sources significantly. Additional explanation and comment is included in the text, Chapter 3 and Conclusion.

5. Referee’s comment: p.11, l.20-24: Please provide information for the vertical resolution of the model, the size of the domain, the physical parameterizations used (e.g. convective scheme) and the spatial and temporal resolution of boundary conditions.

Authors’ response: Requested information is included in the paper. Convection is

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explicitly resolved as commented above. Information about other parameterizations in the model is included in references related to the model description. Additionally we changed the information about model resolution, since it is 1/40, which means 3.75 at Equator, but somewhat higher resolution on higher altitude. To avoid confusion we put the original resolution in degrees in the text and explained that we will refer to resolution as approximately 4km.

6. Referee's comment: p.11, l.23: Is cold start a realistic approach for the specific area? The authors should justify this choice based on available observations or modify their modeling procedure by adding model spin up time.

Authors' response: The cold start is not a realistic approach for any area. In this case, we can use this approach, because there is no relevant dust transported before the simulation start time. The explanation is added in the text, Chapter 3.

7. Referee's comment: p.12, l.4: Please use reference chronological order.

Authors' response: Done.

8. Referee's comment: p.13, l22: "interpolated" - probably extrapolated into the model resolution.

Authors' response: "Interpolated" is used since it is common term for preparing other datasets form numerical models, like vegetation, soil texture, orography, etc. To avoid misunderstanding, because "extrapolation" can be also be interpreted, for example, as extension of some value, we prefer to use "upscale (area averaged)" in the text, since it is obtained from finer resolution data.

9. Referee's comment: Please provide also modeling information about the storm development and precipitation.

Authors' response: Discussion about precipitation and model precipitation figure (added to figure 5) are included in the text. The storm development is also found in the text.

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10. Referee's comment: p.14, l.3 : Wind arrows are not legible in figure 2.

Authors' response: Image changed.

11. Referee's comment: p.15, l.19-21: This is a very important and unique finding indicating the complex situation of dust and hydrometeors coexistence. However the quality of the radar images in Figure 3 is not very good - at least in this resolution. A landmark indicating Phoenix could assist the interpretation of the images.

Authors' response: The Phoenix area is mostly in eastern Maricopa County, so we increased the visibility of the counties borders on model images for better orientation; drawing complex Phoenix borders affect the visibility of the results. The landmark for the radar location is added on the images.

12. Referee's comment: p. 16, l.3 : Images in Figure 4 are too small and figure features are not legible.

Authors' response: Image changed.

13. Referee's comment: p. 16, l.15: Figure 5 is also not legible.

Authors' response: Image changed.

14. Referee's comment: "In order to simplify comparison of the model and observed data, we selected the Phoenix/Sky Harbor observation station". The authors should provide model comparison and statistics with all available stations in the area.

Authors' response: Other stations had more missing data during the event. This is why we chose this station. The sentence is changed in the text and additional explanation is included in Chapter 4. Also we added explanation of the purpose of the atmospheric model validation. The referee points out an important fact, that evaluation of model quality has to include statistical scores. In this case we wanted to avoid a statistical approach, since there are not enough measurements available in the area of interest, and the simulation is only for 24 hours. So, we made our discussion more descrip-

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tive, as stated in the text ("Unfortunately, the sum of observational dust evidence made good model verification and validation impossible; model verification became more descriptive than quantitative."). For meteorology verification of the dust front movement, radar images are most important; they help determine front position and follow its path over time. Meteorological observations, surface measurements, are insufficient to provide such information. This is why we focused our discussion on the region inside the radar domain, and did not comment in details about rest of the model domain. Also, a model – observation statistical comparison in this case can lead to a so-called "double penalty problem" (Rossa et al., 2008) and eventually marking model results more blurred, which is common in verification of small scale features, such as precipitation, wind gusts, or dust that is very changeable in space. Figures 5 and Fig. 9 present both model and available station data, together. Statistical verification of the model will be done after performing a longer period of simulation with dust and no dust episodes, which we propose to do in our current work. We added these comments in the text.

Rossa, A., P. Nurmi, and Ebert, E.: Overview of methods for the verification of quantitative precipitation forecasts. In Michaelides, S. (ed), Precipitation: Advances in Measurement, Estimation and Prediction, pp. 417-450, 2008.

15. Referee's comment: p. 17, l.17: To my understanding from Figure 2, the maximum dust concentration is found along 113 W and I would expect to see a plot along this longitude instead of 112.2W.

Authors' response: The dust storm divided into two paths, one heading toward Phoenix, and one going further to the west. The media called it the "Phoenix dust storm" since it is a very populated area, and most interest was about the dust wall that arrived in the urban area. Part of the storm moved westward, across much less populated regions that are dry, bare and more dust productive. Almost all the knowledge and observations about this event are available only for the populated regions, like Phoenix. Unfortunately, there are too few measurements to allow us much comment on the dust front movement in other affected areas, other than what the model tells

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us. A cross-section was selected to go through Phoenix, i.e. to present the dust front arriving in Phoenix, which is our main interest in this paper and an area with available measurements. A sentence that explains this is added in Chapter 4.

16. Referee's comment: p. 17, l.22-24. I was unable to find any "small solenoidal circulations" in Figure 7. Please indicate the position of these features.

Authors' response: We removed "small" from the sentence, since "small" can be differently interpreted (for example, from the synoptic scales it is small, but from local scales it is not that small). Solenoidal circulation is visible behind the front line with its center at about 1km height, which has been added to the sentence.

17. Referee's comment: p.18,l.3-7: Comparison of modeled dust from a selected grid point that is located 0.5 degrees south of the station measurements in Figure 8 is confusing. The authors should provide model-observation dust time series for all measuring locations.

Authors' response: Additional explanation for our choice of this model point has been put in the text, along with more values in the area with measurements. Also, in Fig. 10 model values located in the area with measurements are presented. Here is an example of the "double penalty problem", mentioned above, because concentrations are very changeable on small scales, which is evident from observation values. Hence, for small scale features (local dust storms, convective clouds, etc) it is hard to expect exact overlap of model and observed fields, which is the reason why model values together with observations are presented spatially in Fig. 9.

18. Referee's comment: p.18, l.10: Station values in Figure 9 are illegible.

Authors' response: Image changed.

19. Referee's comment: p.19, l.6-7: "in Fig. 10 shows the model PM10". Please rephrase this sentence (e.g. model PM10 is shown in Fig. 10....)

Authors' response: Done.

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20. Referee's comment: p.19, l.10: Probably you mean weak.

Authors' response: Yes. Changed.

21. Referee's comment: p.19, l.8-10: Is convection explicitly resolved in the model or parameterized? The authors should investigate if changing the treatment of convection in the model can lead in stronger downdrafts and mobilization of more dust.

Authors' response: Convection is explicitly resolved; this is added in the text and commented above. Stronger vertical movement using this approach can uplift more dust but when the front arrives in the area with somewhat higher altitude south of Phoenix, upward movement is also stronger and can slow down and deform the front. For the atmospheric forecast it should be tested on different mixes of parameterization-explicit convection. This was not the scope of our paper, but it is the quality of dust transport modeling, so we used an explicit approach to avoid extensive discussion about sensitivity on the convective representation in the model. Use of only parameterized convection did not produce strong enough downbursts. This knowledge comes from the NASA project mentioned in the text ("Airborne Dust Models: A Tool in Environmental Health Tracking"). A short comment is now included in the text.

22. Referee's comment: p.19, l25: "Model validation using satellite observations". Comparison with satellite data is a good indication that the model is doing well but the uncertainties from both sides (model and satellite algorithm) do not allow a real quantitative validation.

Authors' response: Comment is added in the text.

23. Referee's comment: p.20-l.18: " Figure 11e shows high agreement with the PM10 model simulation (Fig. 2)." The authors should compare MODIS AOD with modeled AOD instead of PM10.

Authors' response: Figure 11 is divided in figures 11 and 12. Model AOD image is added in figure 12, and the text is changed accordingly.

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24. Referee's comment: p.20-l.28: Images and fonts in Figure 11 are too small to read.

Authors' response: Changed.

Additional note from the Authors:

Changed figures are uploaded, and are of high quality, but longer captions could not be submitted, so we listed the figures' captions here:

Figure 2. NMME-DREAM PM10 surface dust concentration (μg/m3) and wind on 10m height (m/s), on every hour for the period 00 – 08 UTC 6 July 2011.

Figure 3. Radar Zhh at 1:50 (upper left) and 2:46 (upper right) UTC, hv at 1:50 (middle left) and 3:09 (middle right), and velocity at 1:45 (lower left) and 3:05 (lower right) UTC; radar location is marked with "x".

Figure 4. NMME-DREAM wind at 11th model level ($\sim$500m), divergence in 10-4/s (green to purple), magnitude in m/s (blue lines), and direction (arrows) at 02 (upper left), 03 (upper right), and 04 (lower) UTC 6 July 2011; radar location is marked with "x".

Figure 5. NMME-DREAM 2m temperature (blue to red), surface pressure (purple lines) and 10m wind (arrows), and observed values at stations (2m temperature, surface pressure and 10m wind) at at 02 (upper left), 03 (upper right), and 04 (lower left) UTC 6 July 2011; NMME-DREAM 3h accumulated precipitation for the period 01-04 UTC 6 July 2011 (lower right).

Figure 7. NMME-DREAM vertical cross section along 112.2°W, PM10 dust concentration, streamlines (blue), and height of model levels (purple lines) at 02 (upper left), 03 (upper right), 04 (lower left), and 05 (lower right) UTC 6 July 2011.

Figure 9. NMME-DREAM PM10 surface dust concentration and observed values of PM10 (μg/m3) at 03 (upper left), 04 (upper right), and 05 (lower) UTC 6 July 2011.

Figure 11. Satellite Observations of Haboob event July 06 2011 showing total attenu-

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ated backscatter at 532 nm of a dust event as measured by a) the night-time CALIPSO overpass (at 0946 to 0959 UTC) and c) the day-time CALIPSO overpass (at 2053 to 2106 UTC) and CALPSO Aerosol Subtype at b) nighttime and d) daytime. The inset map shows the path of CALIPSO overpass over the globe (black line) and the study region (magenta “night” and green “day” lines).

Figure 12. Aqua MODIS aerosol optical depth (AOD) based on deep blue (DB) algorithm (left) and NMME-DREAM AOD (right) for the nighttime overpass time.

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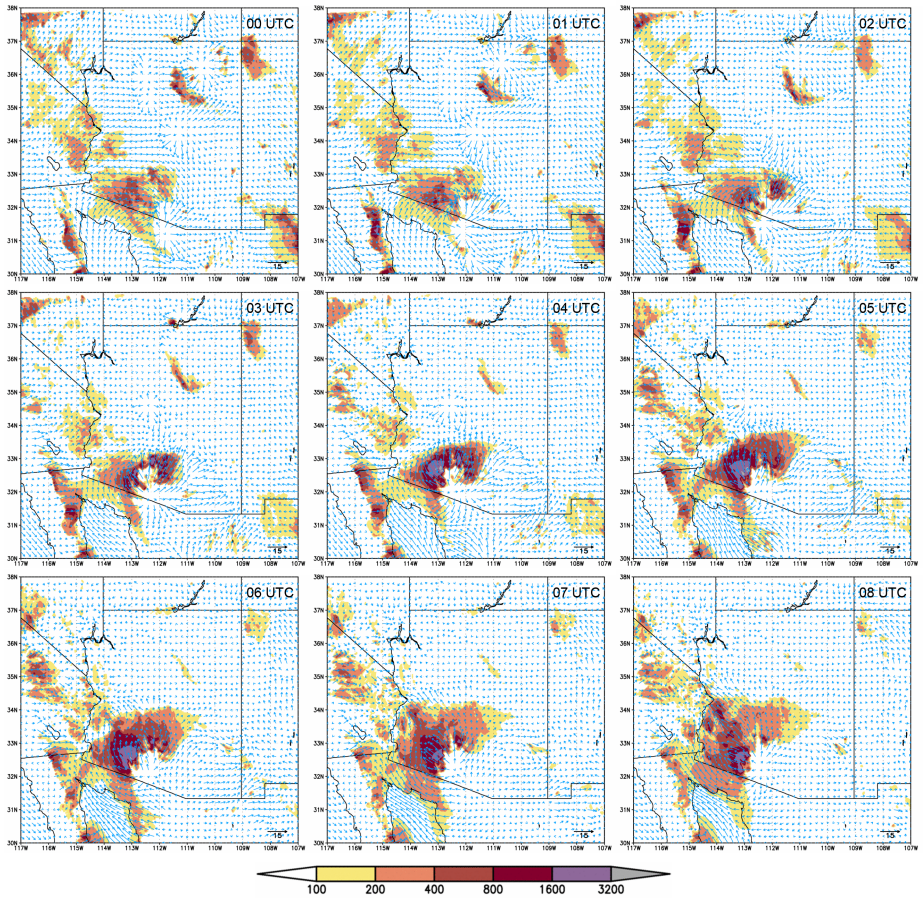

Fig. 1. Figure 2.

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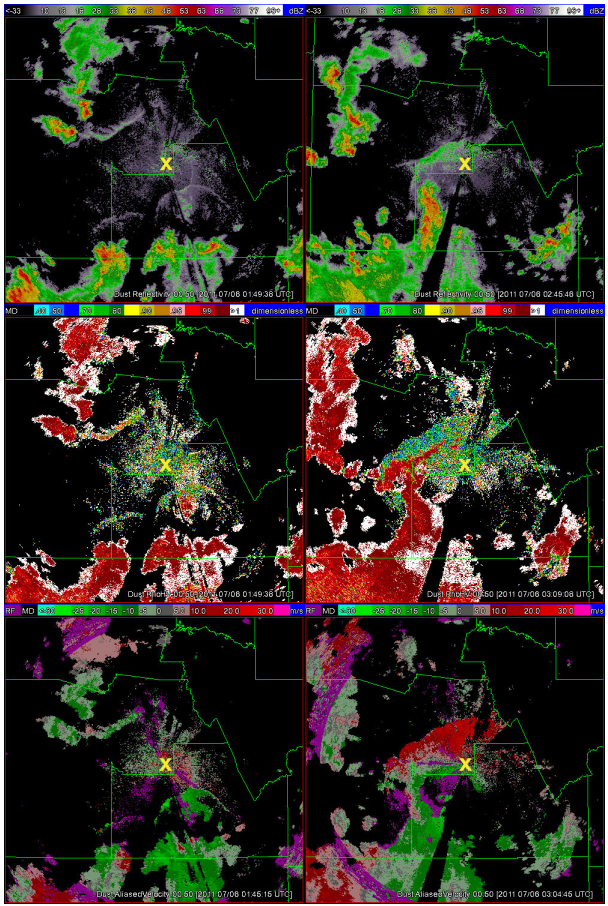

Fig. 2. Figure 3.

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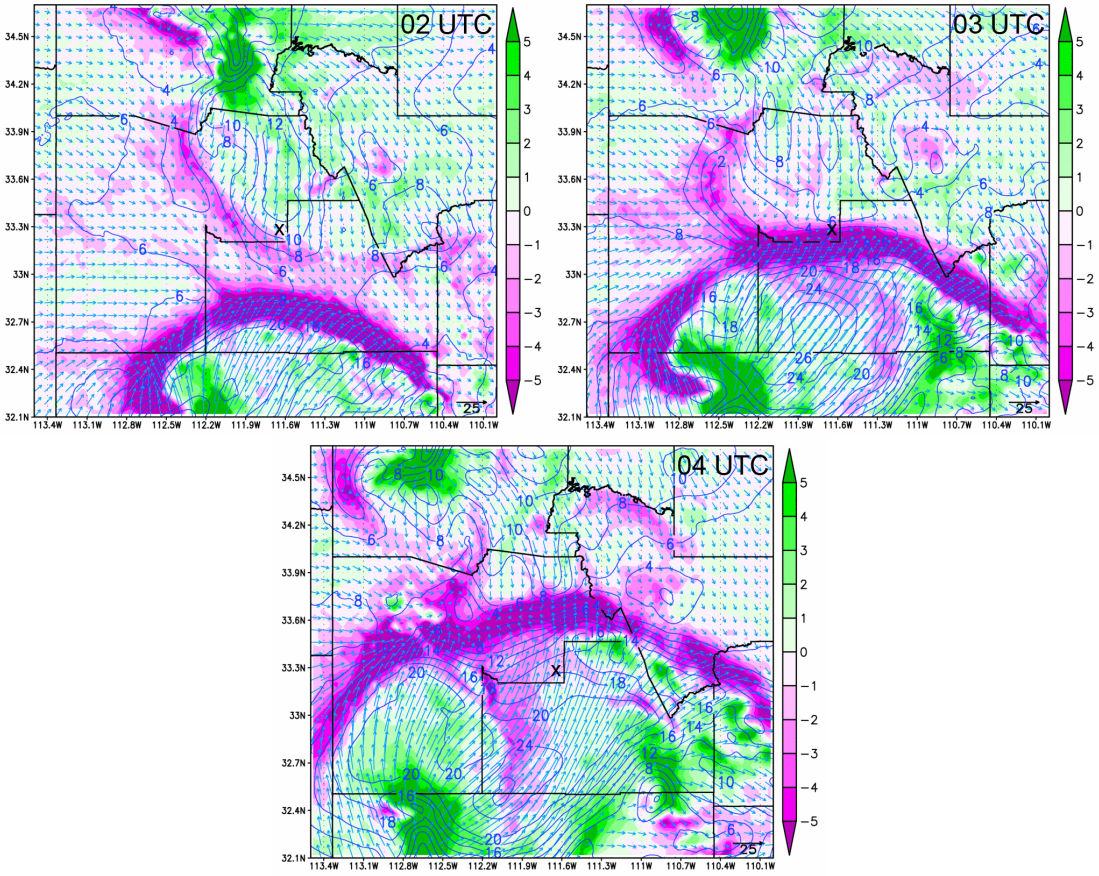

Fig. 3. Figure 4.

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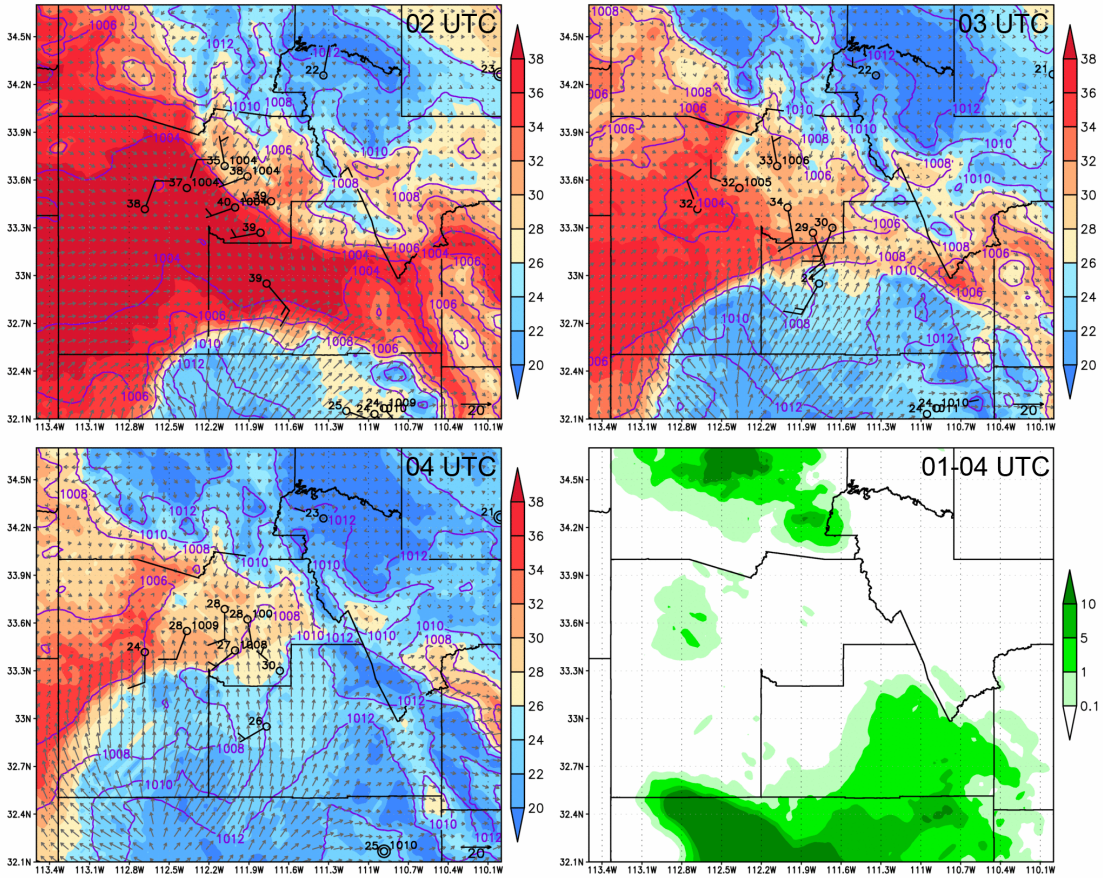

Fig. 4. Figure 5.

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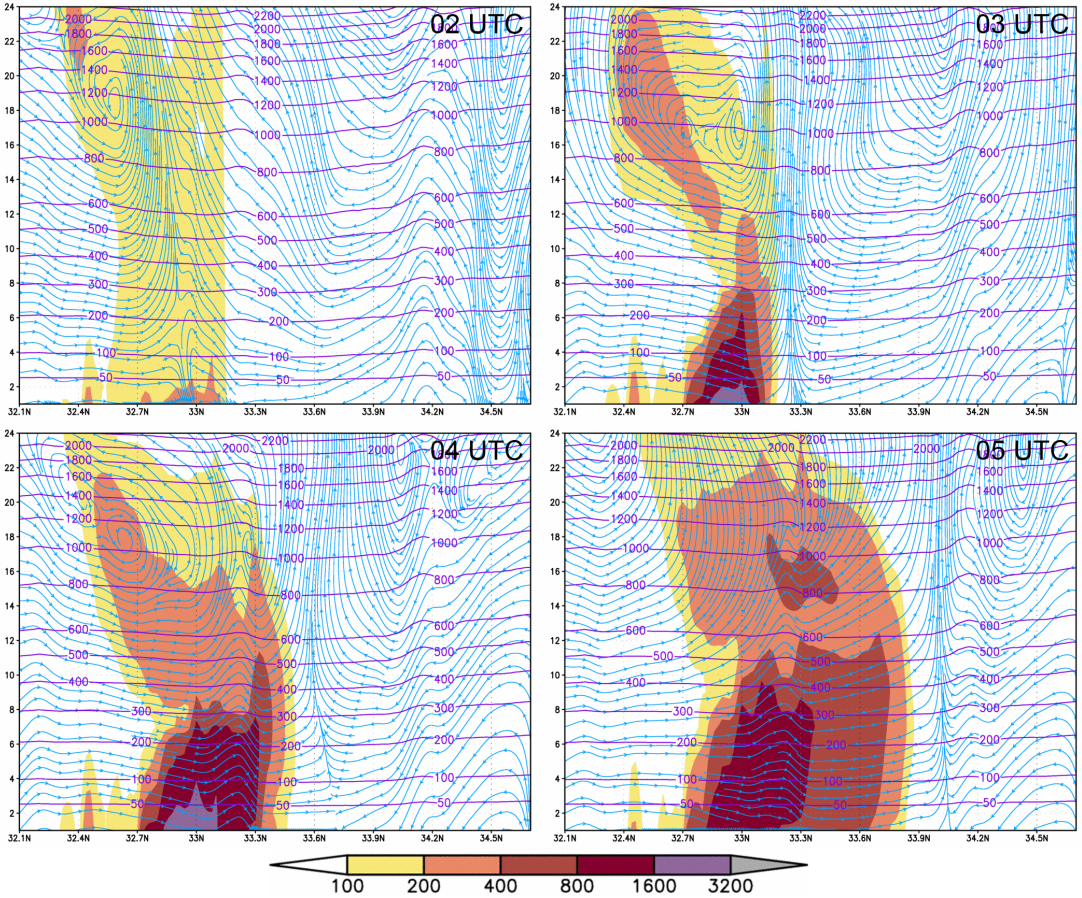

Fig. 5. Figure 7.

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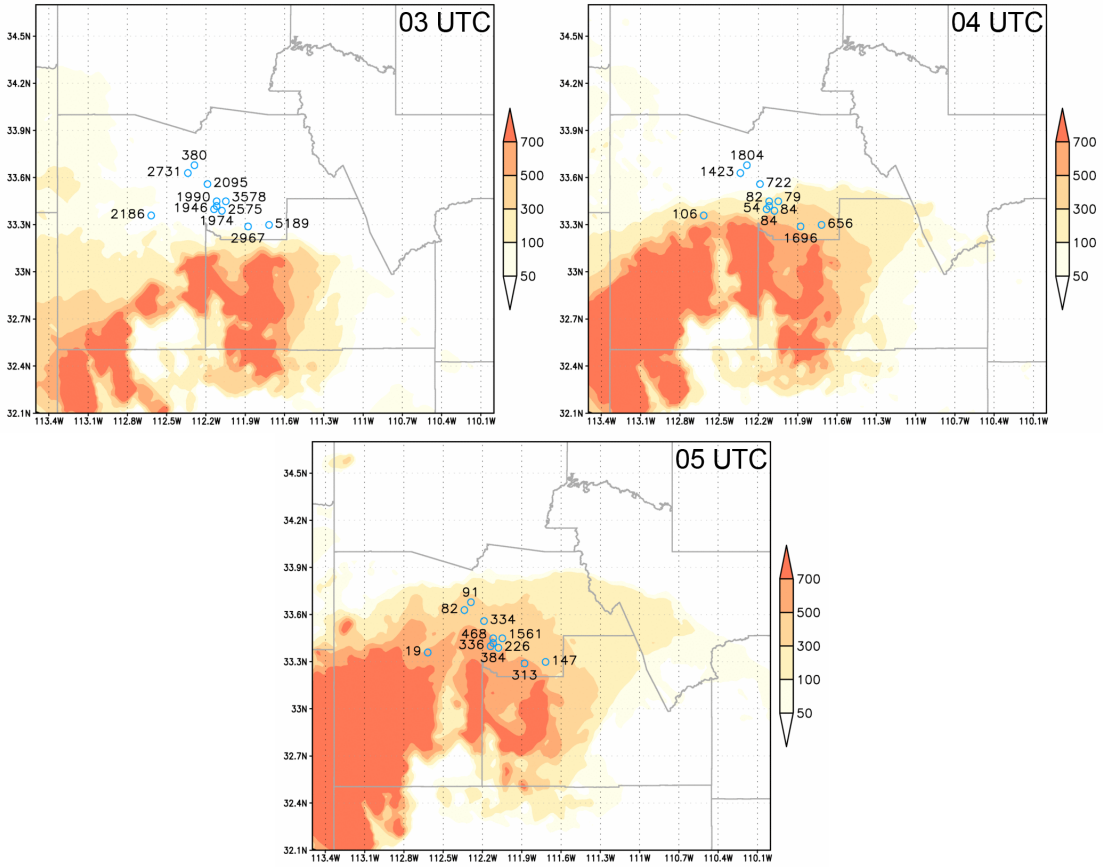

Fig. 6. Figure 9.

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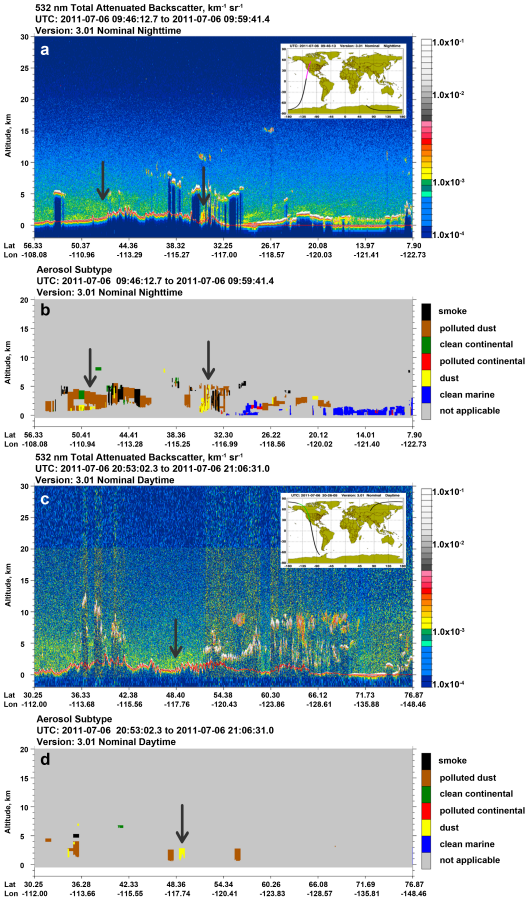

Fig. 7. Figure 11.

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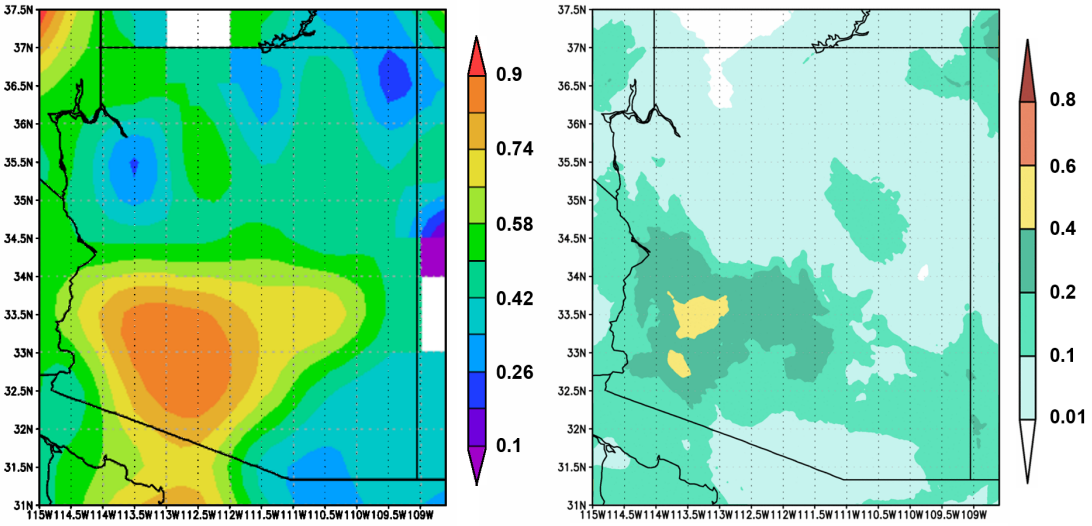

Fig. 8. Figure 12.

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