Review in connection with the manuscript

Influence of high-resolution surface database on the modeling of local atmospheric circulation systems

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General considerations

In this paper the authors report from a study, in which the surface databases in the ARPS meso-scale atmospheric model are updated by using high-resolution topography (3s-SRTM), high-resolution land use information (10s-ESA) in connection with the 30s-ESA LAI and FAPAR databases. While the title of the paper claims to investigate the influence of this newly introduced high-resolution surface information, relatively little is provided in this respect in the paper itself (see major comment 1). Rather, there is a lengthy discussion on the performance of the modeling system at individual sites, which in fact seems to reveal (but see major comment 2) that the introduction of the high-resolution surface database has relatively minor impact on the overall quality of the simulation. Since this is a model development journal, it is the present reviewer's opinion that the weights must be reversed and the impact of the new databases must clearly be demonstrated before the paper can be recommended for publication.

Major comments

1 The new databases are compared to the old ones in Figs. 2 to 7, what possibly is a little overdone (basically one sees on all figures that there is a higher resolution, and this is quite obvious). According to the title, the paper should focus on the differences between CTL (low resolution) and HR (high resolution) surface information. The authors provide 7 figures (Figs. 8 through 14) with quite identical information (at the seven sites with measurements) and Tab 5 with the statistics concerning the comparison between CTL and HR. All the figures seem to reveal that essentially potential temperature and specific humidity are 'equal' and quite far from the observations, while for wind characteristics (especially speed) the HR indeed is somewhat better from HR run (e.g., Fig. 8). Table 5 summarizes this by revealing that the HR statistics are worse for potential temperature in 6 of the 14 statistics, in 8 out of 14 statistics for mixing ratio, 5/14 for wind direction and 2/14 for wind speed. Similar results emerge from the vertical profiles (Figs. 15 and 16). An immediate conclusion would therefore probably have to be that the HR surface information is not the primary reason for the

discrepancies between ‘model’ and ‘observation’. It is suggested to i) reduce the number of figures (only show an exemplary comparison plus the table) and associated discussion and ii) try to make a case for what the authors think is the ‘better’/’more realistic’ performance due to the HR information (one difference that seems to be influential is the north-west region of G5, p.23, l. 23, where temperatures are much lower over the water body in the CTL run). Finally, when comparing to observations the authors should diagnose the model variables at the same heights where the observations were made so that we do not always have different levels (e.g., p. 22, l. 6ff).

2 As far as the case studied (September 6/7 2007 in the MARJ region) is concerned, again it is believed that the presented material is not very convincing. First of all, the central figure (Fig. 17) has a very bad quality (see detailed comments). The same is true for Figs. 18 and 19. It is quite hard to follow the authors’ argumentation (Section 4.3) simply because the TKE shading basically reveals an ‘on-off’ characteristic (some ‘grey’ areas where apparently TKE is ‘more than zero’ and the rest in white (less than 0.05m2s-2). Also, a substantial part of the discussion focuses on the penetration of the sea breeze front, so that some graphical support concerning ‘where in the cross-section do we actually have land, where sea’ would be helpful. Most important, however, is the question what we see: is it resolved-scale TKE or sub-grid scale parameterized TKE or the sum of the two? How is this distinction (if at all) influenced by the resolution of the surface information? It is suggested to focus the discussion on the characteristics that actually determine the development of ‘the case’ (which is the development of the sea breeze front, as I understand) and only show the CTL vs HR when it is crucial (i.e., when the author can show, that some of a ‘more realistic’ performance is due to the HR surface information).

Detailed comments

P2, l. 16 the exact depths of the soil layers is probably too detailed information for the abstract.

P3, l. 23 …ARPS allows significant….

P4, l. 15 ….may not add…: isn’t it clear that the higher resolution is only advantageous (useful) if we also have correspondingly high surface information?

P4 l. 16 …in his simulations: if the authors acknowledge Fotini (Tina) Chow it is probably appropriate to say here: ‘her simulations’.

P5, l. 6 what was the resolution in this study?

P5, l. 9. …although sensitive to the soil temperature

P6, l. 8 …occur on the subgrid scales

P7, l. 8 …to compute LES: please re-formulate

P7, l. 17 …to assimilate: this is NOT what we usually understand under data assimilation.

P8, l. 23 …stable conditions, such that….

P9, l. 4 observational data…: from which height? WMO standard? In any case, the model output should be extrapolated to those heights (see major comment 1).

P9, l. 6 …as seen in Fig. 1

P9, l. 8 METAR should probably be explained (or at least it should be mentioned what information the authors extracted from the METAR).

P9, l. 18 what do the authors mean with 'high-order numerical method of ARPS'?

P9, l. 20 …the more sophisticated choice….: do the authors mean 'choice of more sophisticated schemes'? In any case: on what was this choice based (and which schemes were selected)?

P10, l. 2 We set ARPS up to…

P10, l. 11 ...is set up…

P10, l. 25 where are air basins I, II and III?

P12, l. 11 n_z should be defined

P12, l. 26 …degraded the representation…: based on what was this judged?

P12, l. 28 coarser not coaser

P13, l. 11 …processed by the numerical grid: it is certainly not the grid which processes NDVI and LAI, so what do the authors mean?

P14, l. 17 …in our runs

P15, l. 9 …seems better: how is this judged?

P16, l. 10 …there is no significant discrepancy…..: see major comment 1. This basically summarizes the 'impact' of the HR surface data sets.

P16, l. 18 …which is normally at 2m agl: see above, should be made clear that model and obs refer to the same heights.

P17, l. 22 ...we believe….: based on what?

P17, l. 28 ….differences a found when…

P120, l. 10 …collected at the Galeao airport: where is this airport? I don't think it has been introduced earlier.

P20, l. 22 …the CTL run performs better….

P20, l. 26 ...the ARPS results reproduce correctly: this is of course a matter of taste. Still, I see both models to have a steadily decreasing mixing ratio, while in the observations there is a clear Mixed Layer topped by a quite sharp decrease around 1600m.

P21, l. 16 …incontestable better results…: while this is literally true (because it only refers to the three stations mentioned) it give the wrong impression that potential temperature is better modeled with HR setting. When looking at Table 5, the HR setting has in 4 out of 7 cases a larger bias and in two out of 7 a larger rms. So a fair judgment is that the two (over all the 14 statistics) are about the same.

P21, l. 23 …ay be associated: how can the authors associate this to the high-resolution information? Simply because this is the only difference? How then about compensating errors?

P22, l. 2 what are the most resolute grids?

P23, l. 14 it would be extremely helpful to indicate the position of Marambaia station on the figure.

P23, l. 18 a major propagation…: the figure, however, seems to indicated that the sea breeze reaches further inland in the CTL.

P23, l. 24 Fig. 17b: in the caption of Fig. 17 the CTL run is referred to as Fig. 17a.

P24, l. 4 why not showing the cross-section as a vertical line in Fig. 17?

P24, l. 8 how can I see the TKE production? What actually is shown is TKE, not the different budget terms.

P24, l. 9 …may be associated: in fact, the budget terms could be extracted, so that this question can be resolved.

P24, l. 16 …a stably stratified…

P24, l. 17 …one can see a northerly wind: how can I ‘see’ this if only the meridional wind component is displayed? If there were a dominant zonal component that wind would not be ‘northerly’.

P24, l. 28 …of TKE increases…’

P26, l. 10 what other hours than ‘physical hours’ do we have?

P26, l. 16 ….our simulations also showed that increased resolution leads to better numerical results: I don’t think this has been demonstrated anywhere in the paper.

P26, l. 18 …HR run presents significantly lower errors: I don’t think this is an appropriate conclusion from the results presented (e.g. in Tab 5).

Fig 1 This figure serves for locating the measurement sites (among other). However, all the letters are way too small so that if one doesn’t know, one cannot find out, which is which. Furthermore a horizontal scale is needed, and also the caption should indicate which of the two domains is G5 and which G6.

Fig 3 The text IN the figures (e.g. ‘a) G5 30s USGS’) is hardly readable.

Fig 8ff the caption should refer to CTL (triangle) and HR (squares) runs explicitly.

Fig 17 the inlet (a), b)...) cannot be distinguished. The temperature labels are probably not necessary (at least they are disturbing). The caption should explicitly state that the bold solid line is the shoreline. The interpolation scheme for the temperature should be chosen such that there is not a dominating ‘high-frequency’ variability covering all the relevant information. Overall: the figure should be designed in a way, that whatever the authors want to show is visible (and does not have to be ‘searched for’).