April 4th, 2014

Final Author Comments to all Referee Comments in connection with the submitted manuscript "Influence of high-resolution surface database on the modeling of local atmospheric circulation systems" – GMD-2013-141.

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Dear Sirs,

After careful examination of all reviewers' comments, we are writing this document to answer all issues raised by **Referee 1 (C2641 - Anonymus Referee) and Referee 2 (C2796 - Jehan Rihani Referee).** As shown below, all comments have been addressed adequately.

We would like to start emphasizing our belief that the proposed work is relevant to be published in the GMD journal because our two main objectives and the contents of our results are well within the scope of the Journal. Our first goal is to investigate the effects of increased resolution of surface input data on LES simulations using ARPS applied to the Metropolitan Area of Rio de Janeiro (MARJ). We point out that this type of analysis and our specific results for MARJ have not been obtained before. In addition to showing the impact of the new databases on the simulations themselves, our second goal is to develop new tools, in the form of preprocessors, that were incorporated into the ARPS model to allow the input of the information coming from the SRTM and ESA database files to generate appropriate non-homogeneous surface BC's. The development of new tools is also within the journal scope and, as observed by referee 2 (referee C2796 - Jehan Rihani), "the scientific significance and motivation behind developing new tools is clearly described within the existing state of LES modelling". We think that this point is also extremely important.

We thank both referees for their detailed reading of our paper and insightful comments and we are grateful for the opportunity to clarify the issues raised in their reviews. We also hope to have the opportunity to produce a much better document and re-submit a corrected version of the manuscript that incorporates all their comments. Based on all reviewers' comments, we were able to analyze our results more carefully and generate new arguments that highlight the considerable gain we obtain when surface databases are incorporated into the mesoscale model ARPS to simulate numerically the atmospheric circulation in large densely-populated areas that are characterized by complex terrain and heterogeneous vegetation. We also point out that, during the public discussion period, we have improved the quantity and quality of our results by including two more simulations, covering two new periods of time, in the bulk of our analysis. Additionally, we have also added more data sets from four other surface stations (totaling 11 stations) in order to improve the quality of our statistical analysis. This new set of results helped us to re-analyze our numerical data and show, in a convincing way, that the use of high-resolution databases improves significantly our ability to predict the local atmospheric circulation based on the ARPS model. We are glad to report that this re-analysis work, including more data, has remarkably improved the quality of our general results, as shown below. We believe that the results of our work and the tools that we have developed (already made available to the scientific community) will provide strong support to the scientific community in the development of general atmospheric and air quality models. Please find below our detailed reply to all reviewers' comments. Finally, please note that some new figures with improved quality (as requested by the referees) are being prepared at this moment, but they are not ready yet to be included in this document. They will definitely replace the old ones in the corrected version of the manuscript. Only the most important ones were included here.

Reply to referee 1 (C2641 - Anonymus Referee) in connection with the manuscript "Influence of high-resolution surface databases on the modeling of local atmospheric circulation systems"

By L. M. S. Paiva, G. C. R. Bodstein and L. C. G. Pimentel

General Considerations made by referee 1 (C2641 - Anonymus Referee)

"In this paper the authors report from a study, in which the surface databases in the ARPS mesoscale 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."

Reply: In relation to General Considerations made by the referee 1 (C2641 - Anonymus Referee)

We thank the referee for the general comments about our paper. In what follows we try to "make a case" and show with our analysis that the use of high-resolution topography and vegetation-type databases as input sources of data has a positive impact on the overall quality of the simulation using a mesoscale model such as ARPS. Because this is such a complex flow, the flow model carries numerous approximations, such as: the physical assumptions; the equations that actually model all the physical phenomena involved; the numerical method; the numerical grid. In addition, the measured meteorological quantities are obtained at different positions and time. As a consequence, numerical results that are calculated as a function of space at the same time are expected to present discrepancies when compared to field data. This fact cannot be forgotten when a comparison is carried out between measured and calculated data. Therefore, the difference observed between the

HR- and the CTL-resolution results do not lead necessarily to better simulation results in all variables, but we highlight that the HR simulations overcomes the CTL one, mainly for the wind speed and potential temperature. These are the most important quantities in our analysis. For the water vapor-mixing ratio, both sets of results are in fact quite similar with each other and with the field data, but results for the wind direction show clearly that at stations where there has been an improvement, this improvement was significant (SBAF and JPA). Overall, we believe that the HR results are in fact better than the CTL results. We summarize below, in this section, the main arguments that support this idea.

First, we recall that we have added more data to the bulk of our results to provide additional support to our analysis. We have run two more simulations covering the periods 6/7 February 2009 and 08 August 2011 in the MARJ region. We also included the measured data of four more surface stations (Copacabana, Vila Militar, Xerém and Jacarepaguá). With this increased data sample, it has become easier to show that our set of statistical results improved significantly. A new Table 5 has been built and is shown below, in section "**Reply 1.2**". Analysis of these statistical indexes clearly shows that the HR results are much better than the CTL results. Also, we have obtained more evidence that the HR results are better than the CTL results based on the time series from the new simulations we have performed. As you suggested, our analysis is now focused on the best results that we have obtained. Therefore, our re-analysis is based on this increased set of results, which indicates that it is very important to insert high-resolution topography and vegetation-type database in order to have a better representation of the local circulation, although it may not be enough. Additional improvements on the simulation results will also depend on other factors, such as local surface characteristics, turbulence model and other physical models associated to the mesoscale model being employed. It should be noted that this conclusion is important for advancing the development of atmospheric models too, especially for regions of the southern hemisphere, where there are few studies on the performance of these models in representing the local scale circulation and air quality modeling. Just as an example, studies have been carried out to analyze the effect of the application of high-resolution LULC data for biogenic emission and air quality simulations in the Metropolitan Areas at North hemisphere (Cheng et al, 2008 and 2003; Byun et al, 2005). However, this type of study has not yet been carried out for regions of the Southern hemisphere, where there are important tropical forests close to megacities.

References:

Byun, D. W., Kim, S., Czader, B., Nowak, D., Stetson, S., Estes, M., 2005. Estimation of biogenic emissions with satellite-derived land use and land cover data for air quality modeling of Houston-Galveston ozone nonattainment area. Journal of Environmental Management 75, 285–301.

Cheng, F. Y., Kim, S. T., Byun, D. W. 2008. Application of High Resolution Land Use and Land Cover Data for Atmospheric Modeling in the Houston-Galveston Metropolitan Area: Part II, Air Quality Simulation Results. Atmospheric Environment 42, 4853-4869.

Cheng., F. Y., Byun, D. W., Kim, S. B. Sensitivity study of the effects of land surface characteristics on meteorological simulations during the TexAQS 2000 period in the Houston-Galveston area, Extended Abstract. The 13th Penn State/NCAR MM5 User's Workshop, Boulder, Co. June, 2003.

Major comments made by referee 1 (C2641 - Anonymus Referee)

(1) "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)."

Reply: In relation to the First Major comments made by the Referee 1 (C2641 - Anonymus

Referee) – Note that we split the major comments into small questions.

Question 1.1) "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)."

Reply 1.1: We agree with the comments of referee 1. We removed Figs. 5a-b from the manuscript, since it is related to Figs 4a-b through of the information available on Table 3, and Fig. 6a-b because it is related to Figs. 4a-b and 7a-b. We think that the other figures are important in order for the reader to observe the differences between the original databases and the high-resolution databases.

Question 1.2) "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."

Reply 1.2: Motivated by the comments from both referees, we, as mentioned above, carried out two more simulations corresponding to the periods 6/7 February 2009 and 08 August 2011 in the MARJ region and also added data from four new meteorological stations (Copacabana, Vila Militar, Xerém and Jacarepaguá) to the measured data that we were using in our previous analysis described in the submitted manuscript. Our intention is to increase our numerical and field databank to aid in the process of demonstrating, in a more convincing way, that the HR results are significantly better than the CTL results.

Based on this increased databank, we rebuild Table 5, which summarizes that the HR statistics indexes are, overall, much better now. From the data displayed in Table 5, we see that the HR statistics are worse only in 6 out of the 22 statistics for potential temperature, and only in 2 out of 22 statistics for the wind speed. We consider these statistical indexes to be very appealing. Specifically in the case of the wind speed, which is a very important quantity, the improvement obtained in its calculation can be quantified by looking, for example, at the Marambaia and Ecologia Agrícola stations (located in the west region). At Marambaia, Table 5 shows that there is a decrease of the bias from 2.32 to 1.66, whereas at Ecologia Agrícola the decrease goes from 3.18 m/s to 2.69 m/s. Also, the RSME goes from 3.02 m/s to 2.42 m/s at Marambaia, and from 4.26 m/s to 3.59 m/s at Ecologia Agrícola. To give support to this line of reasoning, we built a second table, shown below as Table 6, which summarizes the statistics by classifying the stations into three regions: west region (Marambaia, Ecologia Agrícola and SBSC stations), center-south region (SBAF, Vila Militar, SBJR, JPA), and east region (SBRJ, SBGL, Copacabana, Xerém). Table 6 shows that the wind speed results are better for the HR runs in the west and center-south regions, which adds up to 14 improvements out of 14 statistics.

From Tables 5 and 6, the statistical indexes for the potential temperature show significant improvement over the CTL results when HR databases are employed. All cases are better for the HR runs in the east region, and 4 out of 6 are better in the west region. Considering all regions, only 6 cases out of 22 presented worse results with HR databases. Although we had 4 worse cases out of 8 in the center-south region, the calculated bias for both the CTL and the HR runs are small (less than 1.8 K for the west and center-south regions) compared to the average values of the measured potential temperature (about 300.0 K). In the east region, where we had 8 improvements out of 8 statistics, the bias values are also small (in the range 1.5-2.6 K). This set of results indicate that ARPS, overall, is doing a good job in the prediction of the the time and space variation of this quantity in the simulation.

When we consider the statistical indexes for the vapor-mixing ratio, we see from Table 5 that there is no significant difference between the bias values calculated from the HR and the CTL runs. Although this result indicates that there is no clear advantage of using HR databases over low-resolution databases, we point out that the bias values are small (less than 1.7 g/kg) compared to measured values of the order of 14 g/kg (which sets the scale for this quantity), except at the SBJR and SBRJ stations. In other words, the flow model is doing a good job in predicting the time and space variation of this quantity in ARPS simulations, and there is relatively little to improve on the calculation of the vapor-mixing ratio from a statistical point of view. Therefore, the statistical indexes that compare HR and CTL results should not be used directly to assess the advantage of the HR simulation over the CTL simulation.

Table 5: Mean errors (bias) and Root-Mean-Square Errors (RMSE) for potential temperature θ, water-vapor mixing ratio q_v, wind direction and speed.

Weather stations and runs \ Variables and statistics		θ (K)		q_v (g/kg)		Direction (arc-deg)		Speed (m s^{-1})	
		bias	RMSE	bias	RMSE	bias	RMSE	bias	RMSE
Marambaia	CTL	-0.44	2.46	-1.29	1.77	13.57	76.67	2.32	3.02
	HR	0.31	2.69	-1.27	1.77	20.22	76.75	1.66	2.42
Ecologia Agricola	CTL	-0.23	2.20	-1.69	2.29	-36.16	69.50	3.18	4.26
	HR	-0.09	2.28	-1.75	2.32	-36.47	72.90	2.69	3.59
SBSC	CTL	-0.29	2.60	-1.04	1.92	-25.35	79.48	0.71	2.60
	HR	-0.07	2.53	-1.06	1.94	-23.96	81.77	0.73	2.50
SBAF	CTL	-0.08	3.33	-1.07	2.29	-16.17	89.74	0.37	1.79
	HR	-0.83	3.14	-0.93	2.23	2.56	93.40	-0.13	1.68
Vila Militar	CTL	-0.28	2.48	-0.87	1.87	-13.14	102.51	0.48	1.13
	HR	-0.86	2.34	-0.82	1.87	16.06	101.04	0.39	1.12
SBJR	CTL	-1.74	3.01	-3.70	3.40	-3.60	60.04	1.43	2.31
	HR	-1.87	2.87	-3.58	3.85	5.74	62.42	1.30	2.08
Jpa	CTL	0.04	1.92	-0.70	1.46	-17.38	59.73	1.29	2.36
	HR	-0.08	1.85	-0.65	1.46	-6.36	67.48	1.00	1.90
SBRJ	CTL	-1.53	2.00	-2.15	2.42	10.36	84.30	0.03	1.67
	HR	-1.33	1.89	-2.19	2.46	-8.79	84.94	0.22	1.86
SBGL	CTL	-1.44	2.59	-0.45	1.75	11.22	90.45	-0.16	1.66
	HR	-1.22	2.59	-0.48	1.77	14.54	84.50	-0.16	1.64
Copacabana	CTL	-1.49	2.92	-1.43	2.00	-3.09	86.16	-0.32	2.13
	HR	-1.26	2.75	-1.44	2.04	-4.78	93.01	-0.15	2.43
Xerém	CTL	2.62	5.00	-1.56	2.30	-26.49	105.11	2.35	3.68

	HR	2.22	4.38	-1.49	2.33	-20.50	104.13	1.80	2.99

Table 6. Summary of the statistical indexes: cases where HR is worse than CTL (with respect to the total number of cases).

Variables / Grid Geographic Zone	θ	Speed
West	2/6	0/6
South-Central	4/8	0/8
East	0/8	2/8
Total	6/22	2/22

In the case of the wind direction, large deviations between the ARPS runs are observed against observational data. However the HR run shows significant improvement over the CTL run at the SBAF and JPA stations. At the SBAF station, the wind direction bias discrepancy goes from -16° to 2°, approximately, whereas at the JPA station, the bias discrepancy goes from -17° to 6°, approximately.

We now follow referee 1's suggestion to reduce the number of figures that show the time cross-section data (Figs. 8-14) for the seven stations considered in the original manuscript. However, we have now a larger set of results, since we have two more periods of time and four more surface stations. Based on the analysis above, we will concentrate only on a few surface stations where it can be seen that the HR run is better than the CTL run. The analysis above also demonstrates that we can use as reference only the potential temperature and the wind speed results to compare the HR and the CTL runs. The corrected version of the manuscript will present a selected (and smaller) set of figures that shows, in a more convincing way, the analysis described above. As an example, we show below two sets of results in their new format: one for the Marambaia station, and the other for the Jacarepaguá (JPA) station. This latter case comes from one of our new simulations.

The results for the vertical profiles (Figs. 15 and 16) are analyzed in the reply to the comment on P.20, l.26.

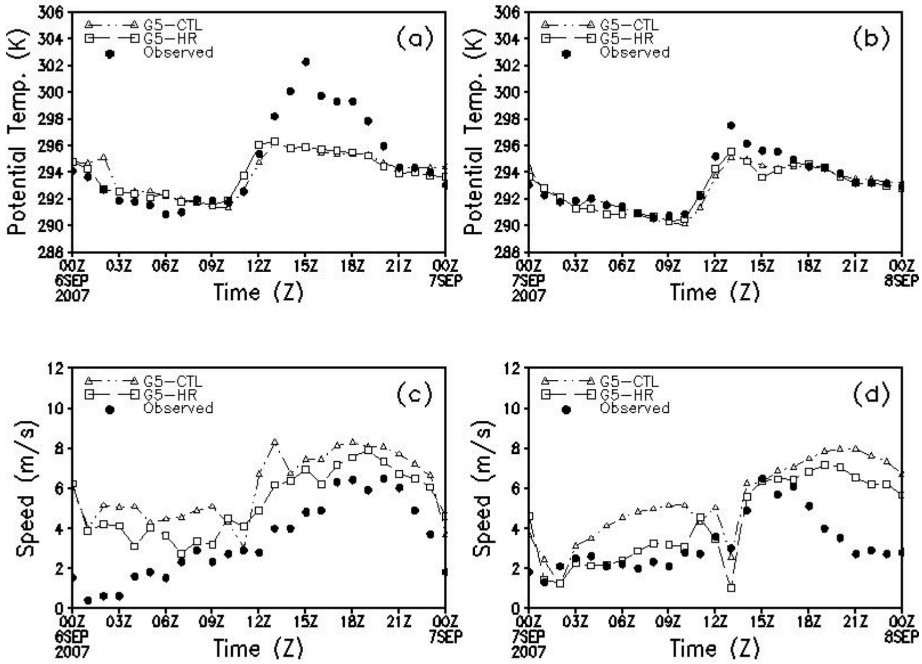

Figure 8: HR and CTL potential temperature and wind speed against observational data from Marambaia station.

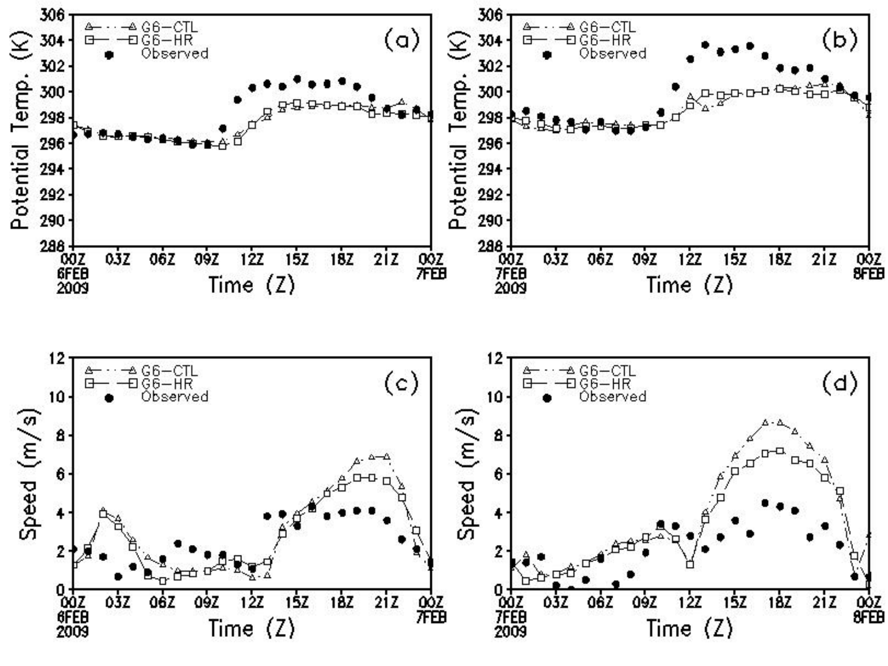

Figure 9: HR and CTL potential temperature and wind speed against observational data from the Jacarepaguá (JPA) station.

Question 1.3) "… 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)."
Reply 1.3: We argued in the reply 1.2 above in an attempt to make a case, as requested. Because there are no surface stations in the northwest region on G5, we concentrated our reply on the three regions we have defined above. **(See reply 1.2).**

Question 1.4) "… 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)."
Reply 1.4: Following the comments from referee 1, we have done a diagnosing of the observational and model results at the same height. So we extrapolated the results from 10 m to 2 m for the water-vapor mixing ratio. This information has been inserted into the corrected version of the manuscript.

(2) "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)."

Question 2.1) "… First of all, the central figure (Fig. 17) has a very bad quality (see detailed comments)."

Reply 2.1: A new Figure 17, shown below, will replace the old one in the corrected version of the manuscript. It presents the difference between the HR and CTL potential temperature field on grid G5. The figure bellow shows the comparison between the HR/CTL simulation results and it highlights better the areas where there are visible discrepancies between the results. We inserted a vertical line in the figures below to indicate the vertical cross section we considered in the analysis of the TKE in the flow.

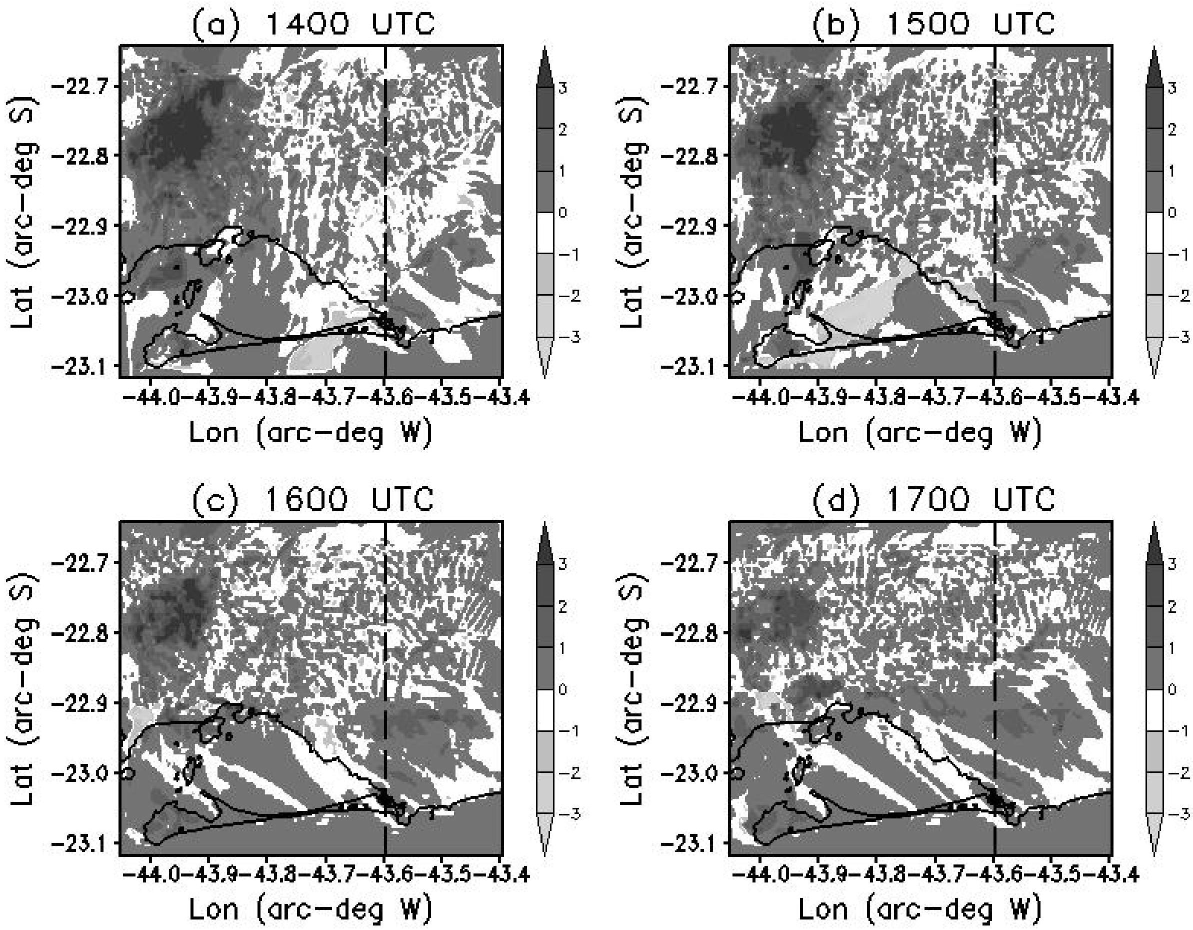

Figure 17: Difference between HR/CTL Potential Temperature Results

Question 2.2) "… 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."

Reply 2.2: Figure 18 will be suppressed in the corrected version of the manuscript because, following the comments of referee 1, we prefer to focus the discussion on the development of the sea breeze front based on the TKE analysis, considering that the HR simulation overcomes the CTL one, as argued in **reply 1.2**. In order to improve Figure 19, we changed the scale to 0 – 1,000 m above the ground level and the color scale now ranges between 0.05 - 3.0 m^2s^{-2}. Also, we inserted a line in Figs. 17 (see figure presented in the **reply 2.1**) and more information on the new TKE figure to indicate where the land and sea cross-sections are located in order to support the TKE/Breeze sea front analysis. The new Fig. 19 is shown below.

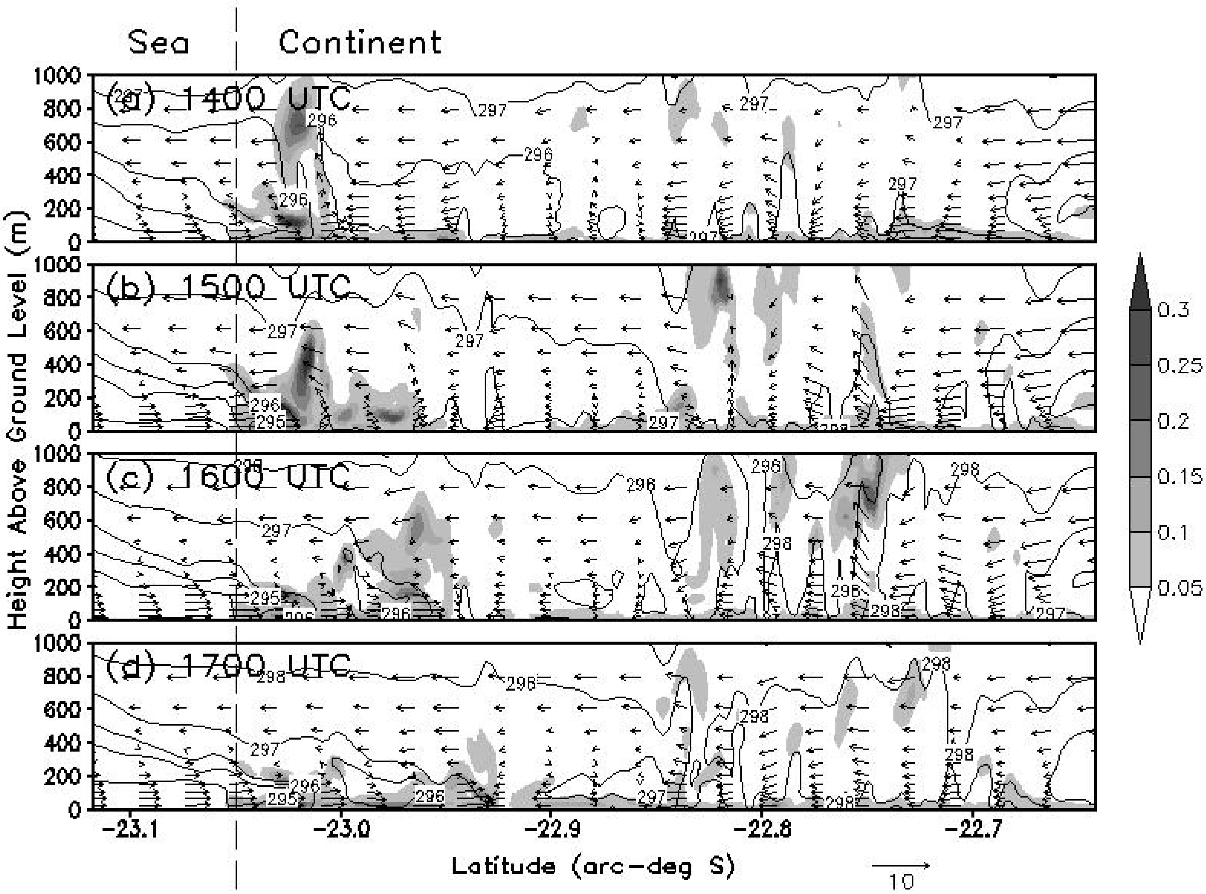

Figure 19: Vertical-latitudinal cross-section of Turbulent Kinetic Energy (TKE – m^2 s^{-2} - shaded), potential temperature (K – solid line) and meridional-vertical wind vector components (m s^{-1} - vectors) up to 1.5 km AGL simulated by G5 of HR run for (a) 1400, (b) 1500, (c) 1600 and (d) 1700 UTC 07 September 2007, at -43.60 arc-deg west-longitude (Marambaia longitude location).

Question 2.3) "**…** 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?”

Reply: The sum of the two, resolved-scale TKE and sub-grid scale.

Question 2.4) “ … 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).”

Reply 2.4: As suggested, we will focus the discussion (in the corrected version of the manuscript) on the development of the sea breeze front by using the HR results because there are no TKE measurements in the MARJ and because our analysis based on statistical indexes (new Table 5), described in reply 1.2, shows that the HR run is better than the CTL run for the wind speed and potential temperature.

In relation to Detailed comments made by the Referee 1 (C2641 - Anonymus Referee)

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

Reply: We agree with the referee 1 and changed the sentence in the corrected version of the manuscript to read: “Topographic shading is turned on and two soil layers are used to compute the soil temperature and moisture budgets in all runs.”

“P3, l. 23 …ARPS allows significant….”

Reply: We corrected the highlighted word: … 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?”

Reply: …. The sentence was changed according to: “In this case, the coarse spatial resolution of the topographic database does not add any additional information to the simulations, since fine numerical grids require 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’.”

Reply: We apologize to the Dr. Chow and to reviewer 1 for this regretful typo and we thank referee

1 for pointing it out. We have changed the text to read “her simulations”.

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

Reply: Chow et al. (2006) used five one-way nested grids to simulate flow in the Riviera Valley at horizontal resolutions of 9 km, 3 km, 1 km, 350 m, and 150 m. We inserted this information into the corrected version of the manuscript that we are preparing.

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

Reply: We have already inserted the word “to” into this sentence in the version of the manuscript that we are preparing. Thank you for catching it.

“P6, l. 8 …occur on the subgrid scales”

Reply: Corrected. Thank you.

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

Reply: The new sentence reads: “ … to run it in LES mode”.

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

Reply: We changed the sentence to read“… to input …”

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

Reply: Thanks. We’ll input the comma in the sentence.

“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).”

Reply: The problem occurs only when we compare the vapor-mixing ratio measured at 2 m WMO standard at the stations with the value calculated by the model at the first grid point (10 m). We corrected this problem by extrapolating the calculated results from 10 m to 2 m, as suggested.

“P9, l. 6 …as seen in Fig. 1 …”

Reply: We thank referee 1 and corrected the sentence.

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

Response: We extracted the following data from METAR (the METeorological Aerodrome Report): wind speed and direction, visibility, air absolute temperature, dew point temperature, and atmospheric pressure. This explanation was included in the corrected version of the manuscript.

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

Reply: We meant to say only "numerical method employed on ARPS". We withdrew the expression "high-order" of the corrected version of the manuscript.

"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)?"

Reply: The schemes selected can be found on P15, l. 16-20. They are reproduced here for convenience: Colette et al. (2003) topography shading scheme; Chou (1990) and Chou and Suarez (1994) short and long-wave radiation schemes; Kain and Fritsch (1990, 1993) microphysics scheme; and the 1.5-Turbulent Kinetic Energy (TKE) Moeng and Wyngaard (1989) turbulence model. The Kessler (1969) and Lin 20 et al. (1983) cumulus scheme is turned on only for the G1 and G2 synoptic-grids. We re-wrote the entire sentence on P9, l. 20 to read:

… "The steps taken include the setup of the numerical method of ARPS, the structure of a high-resolution one-way nested grid, the incorporation of detailed and updated topography and land-use 20 databases on ARPS, and the adequate selection of radiation, turbulence closure, microphysics and cumulus parameterizations based on ARPS user's guide (Xue et al., 1995). …"

"P10, l. 2 We set ARPS up to …"

Reply: We meant: "We set up ARPS to employ …". We corrected that in the new version of the manuscript.

"P10, l. 11 ... is set up …"

Reply: Corrected.

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

Reply: They are now clearly indicated in the new Figure 1, shown below.

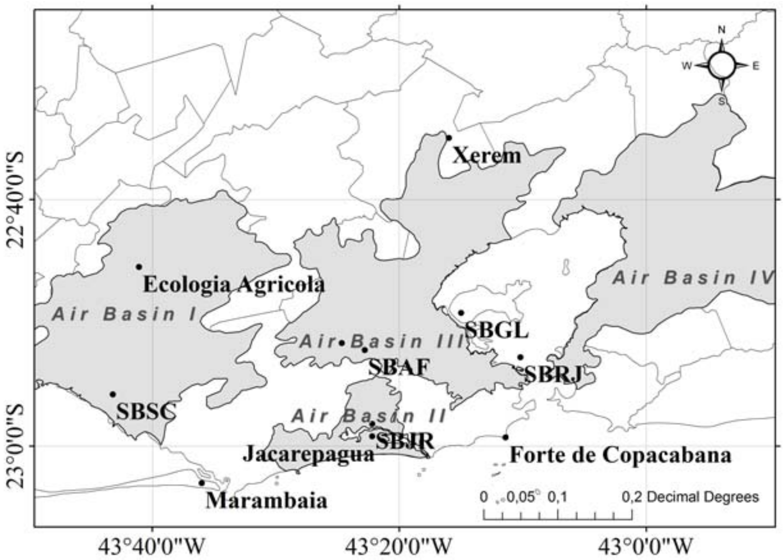

Figure: Map of the Site

"P12, l. 11 n_z should be defined"

Reply: The parameter n_z is the number of the vertical grid points. The text was corrected to include this information.

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

Reply: Our judgment was based on the comparison of the synoptic fields (synoptic chart based on observational data) with the computed fields obtained from our modeling, especially the atmospheric pressure field. This information was included the corrected version of the manuscript.

"P12, l. 28 coarser not coaser …"

Reply: Corrected.

"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?"

Reply: Thank you for pointing that out. We changed the text in the corrected version of the manuscript to read:

"This scheme is in function of the soil and vegetation types, vegetation cover fraction and the Normalized Difference Vegetation Index (NDVI) and/or Leaf Area Index (LAI)".

“P14, l. 17 …in our runs ...”

Reply: Corrected.

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

Reply: ESA is a well-known international database. Its resolution is 300 meter and the USGS database has a resolution of 1 degree. The expression "seems better" was replaced by "is better".

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

Reply: We rewrote the manuscript following the discussion that we presented to answer referee’s comments in the section “***General Considerations made by referee 1 (C2641 - Anonymus Referee)***” of this document and **reply 1.2**.

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

Reply: As written above, the stations’ measurements for the mixture ratio are collected at 2 m agl, according to the WMO standard, and we now extrapolated our model results, calculated at 10m agl, down to 2 m agl. We added this information to the corrected version of the manuscript.

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

Reply: Our belief is based on similar results obtained by Chow et al. (2006) for other regions. We rephrased that to read: “Based on similar results obtained by Chow et al. (2006) for other regions, we believe that …”

“P17, l. 28 ….differences are found when…”

Reply: Corrected.

“P20, l. 10 …collected at the Galeao airport: where is this airport? I don’t think it has been introduced earlier.”

Reply: Sorry about that. The Galeão airport is the place where the SBGL station is located. We changed the sentence to: “… collected at the SBGL station.”

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

Reply: Corrected.

"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."

Reply: We agree with referee 1 that the simulations did not predict the occurrence of the sharp decrease of the vapor-mixing ratio that tops the mixing layer. In the corrected version of the manuscript we will focus the analysis on the region limited by the surface and the sharp decrease of the vapor-mixing ratio around 1600m. The simulation results present reasonable agreement with the measured data in this region, which comprises most of the ABL (our main interest). Above this level, the discrepancies increase.

"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."

Reply: As discussed extensively in **reply 1.2**, the new Table 5 that was rebuilt with new data and the analysis based on Table 5 indicates that the HR statistics provide support to conclude that the HR run present better results than the CTL run.

"P21, l. 23 … may 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?"

Reply: In the analysis of reply 1.2 we demonstrated that the HR run is better than the CTL run using arguments based on global statistical indexes and detailed results for two surface stations. We do recognize that there may be some compensating errors that are implicit to global statistical indexes. However, if we consider the results of the three surface stations located in the west region, shown in Figs. 8, 9 and 10 of the original manuscript, there is clearly a decrease of the wind speed that we believe are associated to the increase in the surface roughness of these stations (Fig. 5a-b) that appear when a high-resolution database is used instead of the low-resolution database. Whereas a low-resolution database indicates a low value of the surface roughness, the high-resolution database provide more detailed information and shows that the surface roughness is actually higher than the low-resolution database indicates. We will rewrite the text accordingly.

“P23, l. 2 what are the most resolute grids?”

Reply: They are G5 and G6 grids as described in the manuscript. We rewrote the text as follows: “…in the most resolute grids of this work (G5 and G6), based on …”.

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

Reply: As requested, we indicated the position of Marambaia station in the new figures (see figures presented in the reply of sections 1.2, 2.1 and 2.2) that that replace the old ones in the corrected version of the manuscript.

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

Reply: We improved the figure quality and it became more evident that the sea breeze penetrates faster in the case of the HR run. However, Figure 18 (TKE of the CTL run) has been removed from the corrected version of the manuscript, because the previous comments of referee 1. We prefer to focus the discussion on the development of the sea breeze front through the TKE analysis, considering that the HR simulation overcomes the CTL one in one of the regions, such as the west region (where the Marambaia Station is located, for example).

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

Reply: The CTL run is actually in Fig. 17a. However, in the corrected version of the manuscript we will present only the HR run analysis **(see reply 2.2)**

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

Reply: Thank you. We did that in the corrected version of the manuscript.

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

Reply: Thank you again. The focus of the analysis is on the TKE distribution, not the TKE production. We removed the word "production" from the sentence.

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

Reply: As explained above, the focus of the analysis is on the TKE distribution. We removed the word "production" from the sentence.

“P24, l. 16 …a stably stratified…”

Reply: Corrected.

“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’.”

Reply: Correct. We changed the text to read: “a northerly wind component”

“P24, l. 28 …of TKE increases…”

Reply: Corrected.

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

Reply: Here we intended to avoid confusion with hours of simulation (CPU time). We modified the text to read just "hours".

“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.”

Reply: This sentence has been removed from the manuscript in light of the new discussion we are presenting here and in the corrected version of the manuscript.

"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).

Reply: As shown above, we rebuilt Table 5, including two more simulation periods and data from four new meteorological stations (Copacabana, Vila Militar, Xerém and Jacarepaguá) to make the results and conclusions more convincing. Table 5 summarizes that the HR statistics indexes are better for potential temperature in 15 out of the 22 stations and better for wind velocity in 17 out of the 22 stations. Thus, it is easier now to see that the HR run is better than the CTL run with respect to the wind velocity and potential temperature globally. Furthermore, we show that the main improvement occurs in the west region (Air Basin 1 and Marambaia) in the figure bellow, which was inserted in the corrected version of the manuscript.

"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."

Reply: We redid figure 1 and split it in two. The first one contains the measurement sites and the air basins. We added a horizontal scale. The second one, we improved the figure and increased the size of the lettering. These new figures were inserted in the corrected version of the manuscript.

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

Reply: We improved that.

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

Reply: We included this information in the caption.

"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')."

Reply: Figure 17 will be replaced in the corrected version of the manuscript by a new one, shown in **reply 2.1** of this document, that presents more clearly the difference between the HR and the CTL potential temperature fields on G5. In this new form, the figure shows a comparison between the HR and the CTL simulation results and highlights better the discrepancies between the results across the entire area **(reply 2.1).**

Reply to referee 2 (C2796 - Jehan Rihani Referee) in connection with the manuscript

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

By L. M. S. Paiva, G. C. R. Bodstein and L. C. G. Pimentel

General Comments made by referee 2 (C2796 - Jehan Rihani Referee)

"The authors develop subroutines to incorporate improved, high-resolution, land surface databases into the ARPS model. Their goal is to investigate effects of increased resolution of surface input fields on LES simulation results for the Metropolitan Area of Rio de Janeiro (MARJ). Six one-way nested LES simulations are performed with varying vertical and lateral resolutions and parameterizations. The authors recommend improved representation of land surface characteristics and input fields as these can dramatically influence the exchange of moisture, momentum and energy between the surface and the atmosphere.

The scientific significance and motivation behind developing the new tools is clearly described within the existing state of LES modelling. The authors do a good job in presenting the setup and performed simulations. The presented results and discussion however are not enough to support the derived conclusions and convince the reader of significant improvements in the higher resolution simulations versus the control run when compared to observations. **I recommend the paper be resubmitted once the authors address this major issue as well as the minor points listed below."**

Reply: We thank referee 2 for the insightful comments about our paper. The main point emphasized by referee 2 is that we need to improve our discussion to support the derived conclusions and convince the reader of significant improvements in the higher resolution simulations versus the control run when compared to observations. As requested, we address this major point in detail in the reply to referee 1, above, and we believe that the arguments we used presents a complete answer to this comment. We, therefore, kindly ask referee 1 to read the first part of this document. In what follows, we address in detail the remaining comments made by referee 2.

Specific Comments made by referee 2 (C2796 - Jehan Rihani Referee)

"- Page 5, Line 9: "… although sensitive the soil temperature and moisture initialization": Do you mean that their simulation results were sensitive to soil temperature and moisture initializations? This sentence is not very clear."

Reply: Yes. This was a conclusion of Chow et al. (2006)'s work. We rephrased the text to read: "Chow et al. (2006) concluded that, although sensitive to the soil temperature and moisture initialization, their numerical results were in good agreement with the field data recorded during the 1999 campaign of the Mesoscale Alpine Programme (MAP Riviera Project; Rotach et al.,2004).".

"- Page 10, Line 25: "air basins I and II–III" are not previously defined or shown in Figure 1."

Reply: They are now clearly indicated in the new Figure 1 showed below. Air basins are regions delimited as a function of the homogeneity of the areas, the topography formation, the soil-type cover, the climate characteristics, the pollutants dispersion mechanisms and the airspace regions. We inserted this definition in the corrected version of the manuscript.

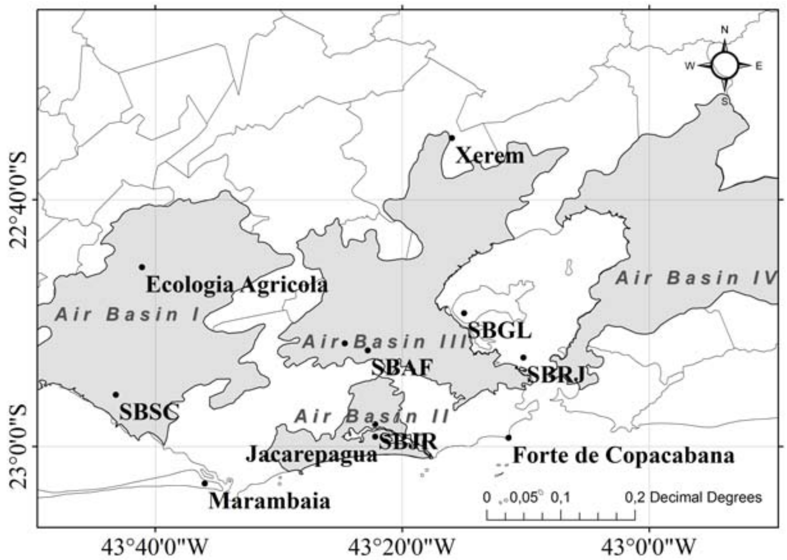

Figure: Map of the Site

“- Page 15, Line 9: “The ESA land-use database seems better than the USGS” should be rephrased to indicate how the ESA database is better (e.g. “The ESA land-use database is more detailed than the USGS”).”

Reply: We agree with referee 2. We rephrased that to read “The ESA land-use database is more detailed than the USGS”, as suggested.

“- Figure 1:”

“- labels are difficult to read on this figure. A higher quality figure is needed to follow the description of observation stations. One suggestion here is to move the middle figure (showing locations of the nested domains) above the main two domains to provide space needed to make these larger and more legible.”

Reply: We improved figure 1 and split it in two. These two new figures replaced the old figure 1 in the corrected version of the manuscript. The first one, which is shown in the reply to the ***General Comments made by referee 2***, contains the measurement sites and the air basins. The second one, which shows the nested grids, was improved and the lettering size was increased, as shown below.

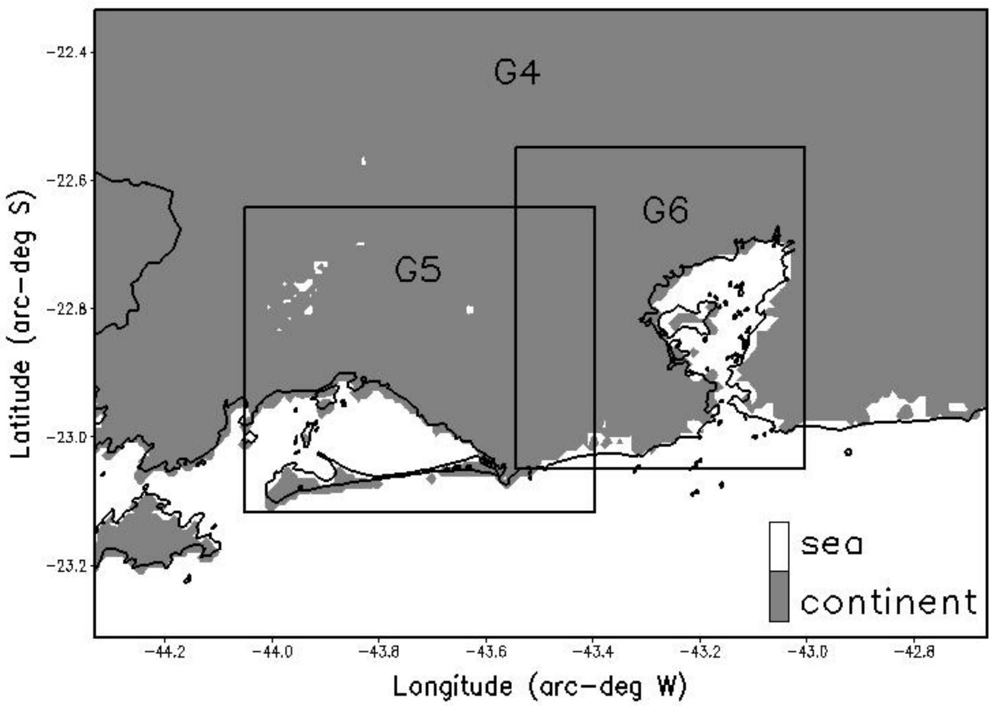

Figure: Nested Grid

“- The title of Figure 1 mentions G4-6 before they are described in the main text (Page 9 line6). Perhaps dividing the figure into parts a and b and referring to each respective part in its correct location in the main text would make things more clear.”

Reply: We agree with referee 2 and we split the figure as discussed above.

“- It would be good to label which of the domains is G5 and which is G6 and mention clearly whether the middle domain represents G4 or not (if not, then the authors should consider adding a figure which shows G4 relative to G5 and G6).”

Reply: We believe that the new figure 2 (see above) meets these suggestions.

“- Figures 8-14: The quality and legibility of the figures need to be improved.”

Reply: We improved the quality and legibility of all these figures in the corrected version of the manuscript. As suggested by **referee 1**, we present the potential temperature and wind speed only at the sites where significant improvements were observed (see figure below as an example).

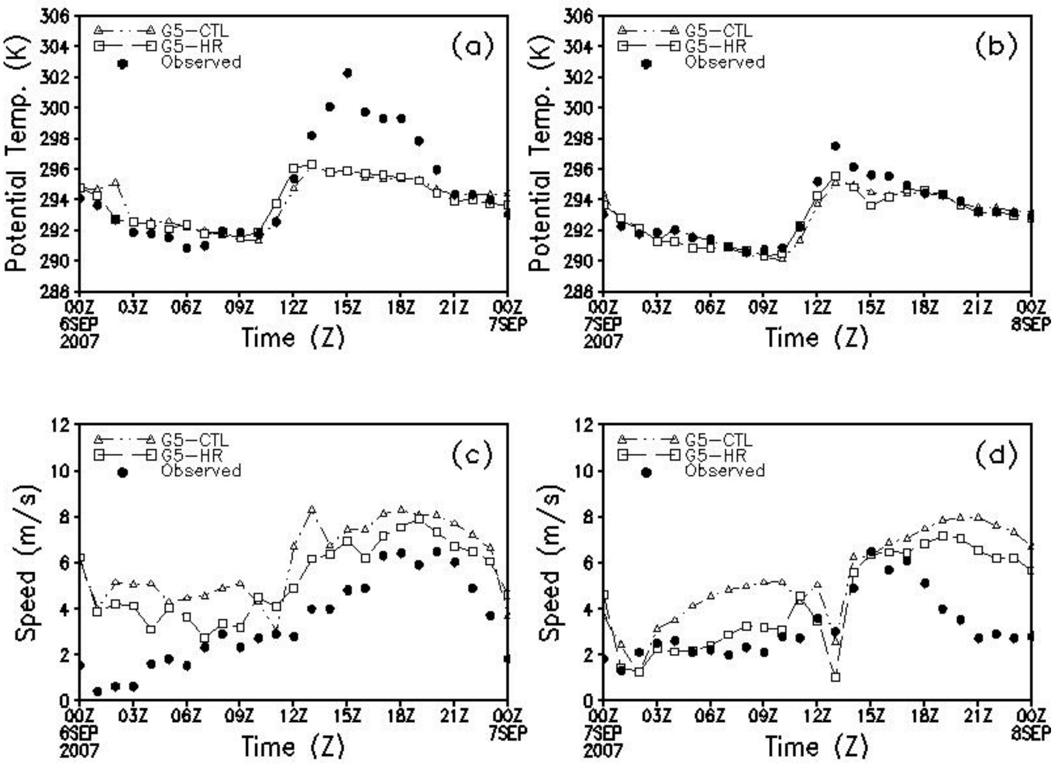

Figure: HR and CTL Potential Temperature and Wind Speed Against Observational data from Marambaia Station.

“- Figures 8-14 can be reduced to a much smaller selection to demonstrate the main points the authors want to convey in comparing simulated high-resolution and control runs to observations. It is not necessary to show the timelines of all stations particularly since Table 5 provides a nice accompanying summary.”

Reply: We thank referee 2 for the suggestion. As mentioned above, we present timelines of the potential temperature and wind speed only for the sites where significant improvement occurred.

Technical Corrections made by referee 2 (C2796 - Jehan Rihani)

Reply: We thank referee 2 for catching all these typos and grammar errors. We have already implemented all these corrections in the improved version of the manuscript.

Suggestions for the Authors’ Consideration made by referee 2 (C2796 - Jehan Rihani Referee)

“- Page 3, Lines 25-26: “We chose the LES-ARPS model as our main tool because it is based on a 1.5-order TKE scheme and the Moeng and Wyngaard (1989) turbulence model …”

It might be helpful for people from different fields to know why you chose these schemes.”

Reply: We agree with referee 2. We rewrote these lines according to:

“We chose the LES-ARPS model as our main tool because it is based on a 1.5-order TKE scheme and the Moeng and Wyngaard (1989) turbulence model and because it has been thoroughly tested (Chow, 2004; Chow et al., 2006) and used as a reference for the assessment of state-of-the-art mesoscale models such as WRF (Gasperoni, 2013). Among all turbulence parameterization schemes available in ARPS, the 1.5-order TKE scheme and the Moeng and Wyngaard (1989) turbulence model is the best for the type of simulation”.

“- Page 4, Lines 17-19: “Usually, high-resolution numerical grids are often employed for simulations in small areas, since the number of grid points grows excessively as the resolution increases, which implies in a high computation cost.”

Suggested rephrase: “Usually, high-resolution numerical grids are often employed for simulations in small areas due to high computation cost.” …”

Reply: We thank again referee 2. We corrected the text as suggested.