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Interactive comment on "An intercomparison of Large-Eddy Simulations of the Martian daytime convective boundary layer" *by* Tanguy Bertrand et al.

Tanguy Bertrand et al.

tanguy.bertrand@lmd.jussieu.fr

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Dear Mike,

Thank you for your careful and thorough reviewing of our manuscript and your insightful comments. We believe that those comments complements very well our manuscript by helping to identify the many challenges of model intercomparisons, an overarching goal truly difficult to address. Probably our submitted version reads as the definitive work on this topic, while this was absolutely not our intent. The goal of our paper is to provide the community with the report of a first attempt to thoroughly compare two LES models for Martian Planetary Boundary Layer convection. We corrected the paper accordingly to clearly reflect this modest goal and state the remaining goals.

We changed the title of the paper. We also added an entire section "Challenges of LES intercomparisons and suggestions for future studies" where we summarize the difficulties and ideas mentioned in the paper and in the reviews. We strongly believe the revised version will be a useful milestone for the community of Martian modelers, if not a definitive reference on Martian model intercomparisons, which is clearly an ambitious goal for a future study funded much more extensively than the preliminary one we performed.

We add attached as a comment a pdf document comparing the previous and the new version of the manuscript (in order to better track the changes). Note that the references are not displayed in this pdf document (made with latexdiff which does not take references into account and also get confused sometimes with the order of the sections) but of course, in the submitted paper, references and section ordering are fine.

High-level comments: Pg. 2, line 21: You identify more than two extant LES models, the present ones, plus the MarsWRF LES and the OSU LES. As you note, the need to evaluate the differences predicted by distinct martian LES are many-fold. It would seem to me, then, that doing an intercomparison with only half of the available LES models makes this study somewhat incomplete. In particular, the LMD LES is based on the WRF framework, as is the MarsWRF LES, although they have been developed independently. That seems ripe for comparison. Were these other two groups approached to contribute to the intercomparison and, if not, why not? This, to me, is a significant weakness of the manuscript. It's less an intercomparison of Mars LES, and more a simple comparison of two Mars LES.

We completely agree with this comment. Ideally, we would have compared all existing Martian LES at different seasons and local times. This would clearly be the best way to examine those models and would form a true intercomparison study. This would require far more human, time and funding resources than the one we were able to gather for this study. The more models are included, the more complex the study. In this paper, we only compare two LES, as a trade-off to extract as much information as possible from a two-model comparison. We chose to collaborate with SwRI MRAMS LES for purely non-scientific reasons (mostly related to our contractor and funding requirements). It is clear that the OSU LES and MarsWRF LES models are equivalently entitled to be part of a future intercomparison effort.

We still believe that our two-model comparison, reinforced by a discussion on remaining challenges, paving the path towards a true intercomparison, provides a strong reference and guidance for futures intercomparisons of models and EDL studies, on Mars or other planetary environments. It shall be emphasized that, despite their heavy use to design EDL sequences of Martian missions, no intercomparison study of Martian atmospheric models is published. We therefore revised our paper accordingly to emphasize the necessary (yet still imperfect) first step we would like to impulse by comparing two LES models for Mars. We changed the title: "Comparison of two Large-Eddy Simulations of the Martian daytime convective boundary layer: sensitivity study and challenges." We also discuss this matter, along with other ideas/challenges, in the section added in the revised version and entitled: "Challenges of LES intercomparisons and suggestions for future studies".

Section 5: I'm somewhat uncomfortable with this section in that you only perform sensitivity studies on one of the two models (the LMD model). This, then, becomes less of a model intercomparison and more of a sensitivity study of a single model. The two are quite different, and I would argue that the intercomparison study essentially ends in Section 4. I would like to see similar sensitivity studies for the SwRI model to evaluate whether, for example, the greater resolved TKE, or vertical wind speeds are more sensitive to parameter changes in the SwRI model than in the LMD model.

We agree that the paper would benefit from such additional data. Nevertheless, the configuration of the project did not allow us to perform a sensitivity study with the two

models; it was only possible to carry out this study with the LMD model (this is also reflected in the authorship and the leadership of this study). Given those constraints, we had to make a choice of either include or not our sensitivity study in this paper. We decided to include it because

1. We explore the behavior of the simulated boundary layer convection to changes in settings, to explain the discrepancies observed between the two models. This further helps to identify the remaining challenges and directions towards which to point further work.

2. Such a sensitivity study using Martian LES has never been published before. We are convinced that our study should be of interest to many in the Martian community – and even the modeling community. This decision to include a sensitivity study is now reflected in the new title.

Specific comments: Pg. 5, line 24: Can you expand on the validity of this assumption? In mesoscale modeling, there are 10s of km between the top of the 'good' results from the model, and the domain top. It seems that you're getting pretty close to the top of the domain when looking at the PBL, which comes in only a couple km below the model top. Are there issues with damping layers at the model top that might be affecting your results?

The choice of the model top comes from previous simulations performed with the LMD LES (Spiga et al, QJRMS 2010: 145$\times$145$\times$201, 50 m LES with 12 km model top). It had been checked in these simulations that this choice did not impact the results. We performed an additional check for this paper by running our simulations with a much higher model top (16 km), along with higher horizontal simulation (see discussion below about domain size), and results slightly changed (PBL depth 25

Figure 4: While you argue that the radiative forcing is about the same now after doing the radiative adjustment, the near-surface atmospheric temperatures are still vastly different–10 K in the nighttime and >20 K in the daytime based on

this figure. I can only imagine that is going to have a noticeable effect on the magnitude of turbulent activity at the smallest scales nearest to the surface. I don't see any discussion or acknowledgement of this difference. Surely it has to be important!

The gap in the nighttime is actually less, as we can see on the new figure (the previous figure was biased because the levels used close to the surface were not the same than those in the SwRI model: this has been corrected). The daytime gap of near surface temperatures is, indeed, as much as 20 K. The near- surface schemes in the two models are very different and, despite our extensive efforts with matching the radiative heating rates, it has been impossible to fill this gap between the two SwRI and LMD 1D models with our methodology. We acknowledge this interesting difference in the 1D comparison and discuss it as a challenge in section 6.

Pg. 11, line 28: You discuss 'quantitative discrepancies' between the models as being responsible for some of the differences between LMD and SwRI, and then refer forward to Section 6. I think it needs to be stated here what these quantitative differences are for the reader to understand and interpret the results of this section.

Agreed. We modified the text to improve clarity.

Pg. 14, line 2: To be honest, I don't think you've done any investigation of the discrepancies between the two models to this point. You've identified what they are, but you haven't done any interpretation of what is causing those discrepancies, or provided any insight into how they might be resolved.

We agree with the reviewer. Now, interpreting the causes for discrepancies and providing insights into how they might be resolved is a very ambitious goal we are not able to completely fulfill with our study (although we have some clues, e.g. diffusive schemes, near surface scheme...). No studies published in the literature ever attempted to at least identify possible discrepancies between models sharing a common methodological path (which was not the case in one of the only existing study comparing Martian mesoscale models, Kass et al. 2003). This is why we believe our study, imperfect as it is, forms a necessary first step to reach through future studies the overarching goal mentioned by the reviewer. We address this comment by changing the text at the beginning of section 5 and adding details in the section "Discussion".

Pg. 16, line 21: This in an incomplete comparison–what were the model parameters in this 'other' LMD LES simulation? It's peculiar to say that the current results are comparable to past results that you don't show, because I, as a reader, have no objective way to assess that statement. What defines 'good agreement', for example?

We changed the text: "The simulation remains typical compared to previous studies performed with the LMD LES. As an example, the PBL height obtained is in the 5-7 km range of what has been obtained for similar surface pressure values (see Spiga et al, 2010, Figure 2 case b and i)."

Pg. 16, line 28: This is tied into the 'quantitative discrepancies' comment on Pg. 11, above. The difference in subgrid-scale diffusion scheme seems like a key difference that has gone unexplored. You've already gone to the effort to match as many physical parameters of the two models as you can, so why not the subgrid-scale diffusion scheme? It's somewhat of a cop-out to say that you see differences between the models, and then speculate on what might cause those differences (subgrid-scale diffusion) without trying to actually determine if it is, indeed, a cause. I think this study is incomplete because of this.

See our previous comment. We agree our study is incomplete (although we prefer to qualify it as a "necessary first step"), but matching the subgrid-scale diffusion schemes between the two models is an ambitious work which would have required additional time, funding, and human resources to heavily modify the codes of one or the other model involved in our intercomparison. However, we completely agree this is an interesting and crucial point to address for future intercomparison studies. We discuss this point in the section “Challenges”.

Figure 5: Can you explain why there are far more points in the LMD curves than in the SwRI curves if both models have the same timestep? Is it as simple as more frequent output in LMD vs. SwRI? If so, why was that not coordinated? The higher frequency output of LMD gives the impression it is ’noisier’ than SwRI, and it should probably be reduced to the same output frequency for plotting, if this is a rigorous intercomparison.

Both simulations have the same physical timestep. It is indeed (the reviewer guessed right) a matter of output frequency. We corrected the figure so that both curves have the same output frequency. We deleted the figures showing the incident flux for sake of simplicity.

Figures 10, 11, 12, 13, 16: Can you explain why in all of these figures, the SwRI LES output is truncated before the end of the time period under investigation? Also, in Figure 10, the LMD data is truncated at 17:00 as well. These figures need to be complete, or else an explanation given for their incompleteness. If it is due to something like a model crash, then this needs to be investigated and explained. I would not feel confident at all in model results that derived from a simulation that crashed. If it’s just because the model was stopped because the interesting results had finished at a particular time, then this also needs to be explained and/or made consistent across all panels.

The turbulent convection is active until the end of the afternoon (typically 16:30-17:00) and suddenly stops (the PBL collapses) when the surface becomes colder than the atmosphere above it (nighttime inversion). This occurs before 17:00 for some of the simulations. A comment has been added in the text.

Typographical/minor issues: Figure 5 goes from 09:00-17:00. Figure 10 goes from 08:00-17:00. Figure 11 goes from 07:00-19:00. Figures 12 and 13 go from

11:00-17:00. Figures 16-21 go from 08:00-17:00. Why not plot everything on the same temporal axis? Consistency makes the reader happy, and the manuscript easier to follow

OK this has been corrected for all figures. All other typographical/minor issues have been taken into account in the text. We carefully read the manuscript again to correct all typos.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/gmd-2016-241/gmd-2016-241-AC1-supplement.pdf

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