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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.

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Dear Dan,

Many thanks for your review and 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 for the editors and reviewer 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.

1) The horizontal size of the modeling domain used in this study (a fatal problem): When performing LES studies of the convective boundary layer (CBL), it is imperative that the horizontal size of the domain is large enough so the periodic boundary conditions cannot influence/contaminate the solution computed for the domain interior. The authors suggest that they have followed the guidance of Mason (1989) to achieve this, but they haven't. For typical square LES domains, a generalized "rule of thumb" (as was used, if not clearly stated, by Mason (1989)) is to design the grid so the length of the domain side is 3x the size of the largest eddy that will be resolved by the simulation. For an afternoon Mars EDL related investigation, the size of the largest eddy scales as the maximum depth of the CBL, which can reach 10 km (thus, 30 km would be an appropriate lateral size). It's an unfortunate reality of LES for Mars that, to sufficiently resolve the range of smaller eddies and the complexity of convective structures (with the underlying desire being to capture the energy spectrum), a grid-spacing of 50 m (as was used here) is needed. For a square LES domain with a 50 m gridspacing, the number of computational locations would then be 600x600. In this work, with only 145x145 computational locations, the lateral size of the domain is just 7.25 km (this is far too small), and the problems that will be

created in this approach will contaminate the analysis and intercomparisons that could be performed at a time of day with a deep CBL. For Mars, especially once any wind profile is introduced to force the simulation, this 3x aspect ratio rule likely should be considered a minimum, and this is due to dramatic variations seen across the diurnal cycle of the scale of convective structures as a function of time and height in the domain. This poses a real problem for LES of the Martian atmosphere, and any study desiring a high-quality LES model intercomparison should err on the side of having completely eliminated the possibility of a contaminated simulation due to a very deep CBL that will interact with the periodic boundary conditions. Most certainly, the top of the modeling domain should also be well above the top of the CBL. As used here, a top of 12 km is probably sufficient, although a few km higher would be preferred. For the goals of this manuscript, the 145x145 number of computational locations, with a grid spacing of 50 m, is a fatal problem. It's easy to see that LES studies involving the CBL on the planet Mars almost certainly require the use of massively parallel architectures.

We thank the reviewer for pointing out this issue. Our choice of domain size is a tradeoff: we want the domain to be as large as possible to represent correctly the growth of the convective PBL, but we need it to run in a reasonable time and with a reasonable size of outputs to be able to carry out the intercomparison and sensitivity study. The 145x145 setting also corresponds, as imperfect as it is, a common setting employed in existing LES studies for Mars.

We slightly disagree with the reviewer on this "domain extent" issue being a fatal problem for our study. We based our LES configuration on the modeling experience reported in Spiga et al. QJRMS 2010, which shows that reasonable estimates of PBL growth, convective plumes, eddy heat flux, can be obtained with the configuration adopted (compared to LES performed with wider domains).

In addition to this, we showed in Figure 6 and 7 horizontal sections at 11:00 local time

where the PBL depth is about 2 km, which is reasonably close to the Mason 1989 rule of thumb. In those Figures, it can be observed that at least two (maybe three) convective cells are enclosed across one dimension of the LES domain.

We agree, however, with the reviewer that the domain extent remains a serious matter to be considered when performing LES. We would certainly not state that our LES setting is the perfect setting to be followed by future studies. We thus followed the reviewer's advice and we performed a LMD LES simulation with a raised model top (keeping vertical resolution similar as in our reference setting) and a much larger domain extent (250x250, progressing towards more grid points would involve computing resources that are not easily available at the time of writing). Results at local time 11:00 remained unchanged, but 25

Thus, we are convinced that our results are still valid and will be useful to the community. Still we do not want to downplay the crucial comment made by the reviewer here. We thus changed the text to better describe our choice of domain and the tradeoff, and we discuss this important comment of the reviewer in the sensitivity study (using the above-mentioned additional 250x250 LES) and in the new section Challenges.

2) dust used during the intercomparison phase (thoughts/suggestion): As described by the authors, without managing the issue of dust and its differing treatments between the two models, an intercomparison of LES models could reduce itself to an 'intercomparison of radiation schemes', not of the dynamical cores as is desired (this is also true for mesoscale and global climate model intercomparisons). The authors did put effort into getting the LMD model to show an improved agreement with the SwRI model to facilitate their intercomparison, although I wasn't fully convinced this effort was actually successful. From a perspective of do it as simply as possible, it's unclear why the authors didn't just run/compare the two models (the primary focus of this manuscript) with no dust loading at all, a dust-free atmosphere. Even with dust properties modified so ground temperatures come into far better agreement, there was no

discussion about heating rate profiles, that they had also come into better agreement as a result. It is much easier (and more straightforward) to use a dust-free atmosphere for the intercomparison focus of this effort, eliminating the complication of the radiative properties of dust in the atmosphere, and the probable non-linear response to this change the the heating rates as a function of height. For the secondary aspect of this effort (the prediction of the EDL environment for the Sciaparelli spacecraft), dust would be reintroduced in both models, and LES results would need to be compared with mesoscale model results (presumably form both of the parent models). Most certainly, a primary reason to use LES is to both improve and qualify our confidence in (and understanding of) the results from mesoscale models, specifically the performance of PBL schemes being used (since there is no PBL scheme in LES). Moreover, and I believe this is important, the use of mesoscale model results would allow the ability to characterize the larger-scale environment in which the LES was being performed. It's important because the larger-scale regional/local circulation can dramatically affect the evolution of the CBL. Careful consideration of this for site-specific LES is not addressed by any authors to date, whereas local strong subsidence has been shown by Tyler and Barnes (2015) to be very important to the development of the CBL, with actual evidence of this (Moores et al., 2015). How can this reality be incorporated into LES, and is it even possible? For Mars, LES has unique challenges, and a manuscript sufficiently well thought-out, that addresses some of these issues head-on with some new approaches would be most-welcomed and likely important to the community.

About performing dust-free atmosphere: we agree with the reviewer that this can be a simpler method to investigate the differences between the LMD and SwRI LES dynamical cores (respectively the WRF and RAMS terrestrial models). Our comparison study is, however, intended to provide an assessment of the spread of results of Martian LES as realistic as possible to be used for EDL studies. This makes the dust-free LES difficult to justify in this context, where realistic temperature profiles accounting for

the radiative impact of dust (which is strong on Mars) are needed.

About performing LES with mesoscale models: we agree with the reviewer that this shall be an overarching objective for future LES studies. Our goal here is, nevertheless, to assess differences between existing LES. To the extent of our knowledge, there is only one LES study which accounts for evolving background conditions following a diurnal cycle [Tyler et al. 2008]; and no published Martian LES study features the use of predictions from mesoscale modeling to represent time-evolving conditions for LES. Instead of trying innovative LES settings, we based our comparisons on the usual settings adopted in most LES studies thus far. We certainly agree, though, with the reviewer that LES studies in the future shall try and incorporate a better account for the diurnal variability in the regional and global dimension, which is expected to be strong on Mars.

We tried to revise our manuscript accordingly, to reflect those perspectives and challenges, although we unfortunately cannot change our paper to make it as groundbreaking as what the Reviewer is suggesting. The scope of our study is perhaps less ambitious than the community is hoping for; yet we still believe we made a significant first step that is worth being reported to inspire better studies of this kind in the future.

We added a discussion about this point in our new section “Challenges”, based on this comment provided by the reviewer.

3) subgrid mixing parameterizations (thoughts/suggestion): Upon looking at the results provided, it's easy to agree with the authors that subgrid mixing in the LMD model is much stronger than it is in the SwRI model. A short section in the manuscript suggested the authors did experiment with the subgrid mixing strength in the LMD model. Unfortunately no results were shown to indicate the degree of change seen towards what I would expect to be a much more 'noisy' solution (more like that in the SwRI model). Were such changes seen in that exercise, and if not did the authors try to completely disable the subgrid mixing

to insure it had indeed been modified?

We tested the model sensitivity to the subgrid mixing coefficient but it has little effect on the results. By decreasing the coefficient by a factor of 1000, we obtained a slightly more noisy solution and but no change of PBL height. Decreasing more the coefficient does not affect the results more than that. Actually, it is not trivial to completely disable the subgrid-scale mixing in the LMD LES based on WRF, because the WRF dynamical scheme possesses a discretization term which is naturally diffusive. It would certainly help to disable this term completely, but this remains a difficult task.

We detailed more this point in section 5.1 and discuss it in section Challenges. All in all, we would like to show in our paper that even when the radiative transfer is matched between two Martian LES models, the predictions of both models can be remarkably different for reasons related to the choice of dynamical core itself.

Analogously, subgrid mixing schemes are to LES as PBL schemes are to mesoscale models; and, both are fundamentally untested in regard to being used in atmospheric modeling for Mars (designed for and tested in terrestrial modeling). I believe we should have a bit more trust in the subgrid mixing scheme of LES (as being fundamental) than the PBL scheme of a mesoscale model, although doubt must still be raised in regard to the isotropic nature of vertical mixing compared to horizontal in a deep and energetic CBL. How do individual schemes address this? The results here serve to elucidate the central importance of subgrid mixing schemes in LES model intercomparisons, where a thorough intercomparison would investigate a wide range of strengths of subgrid mixing. We should be able to show that results become 'smooth' on one hand (as in the LMD results) and very 'noisy' on the other hand (as in the SwRI results) with variations in the strength of this mixing. The idea would be to investigate the point at which the energy spectrum became sufficiently corrupted and/or modified. Possibly, a balancing of the subgrid mixing schemes between the two models, in a nodust scenario such that the energy spectra became more

similar, would be a logical framework from which to begin this intercomparison. This has never even been tried by investigators, and as so clear in the results presented, it's a central question that was not sufficiently addressed.

We admit that the reviewer is, again, perfectly right. This is a difficult question to address, though (that could easily fill an entire paper). We might be wrong, but we thought such detailed analysis of the energy spectrum simulated by both dynamical cores would take our paper too far from its initial goal which is a preliminary step towards a comparison of Martian mesoscale and LES models – a topic seldom, if not never, discussed in the literature. Reading this comment makes us realize that our initial submission may have seemed too ambitious in its intent, whereas our goal is to offer a preliminary LES comparison to the community, with a clear identification of the remaining challenges to perform a truly complete intercomparison of Martian models. These comments have thus been mentioned in our section "Challenges".

MINOR THOUGHTS/ISSUES:

The vertical grid of the SwRI model is actually quite different, with heights fixed above ground in a sigma z (not sigma p) formulation, quite different form the vertical grid used in the LMD model. It actually leads to a "pressure cooker" effect in MRAMS. For comparison of these two models, it is worth acknowledging that there shouldn't be any surface pressure variation in the LMD model, while there should be in the SwRI model.

It is indeed the case, although the pressure variations in the SwRI model are relatively small. This comment has been mentioned in our section "Challenges".

Without a doubt, spacecraft payloads are only going to get more massive, which renders the sensitivity of any spacecraft to atmospheric turbulence quantified during the EDL phase by LES in the CBL (such as for Phoenix or Insight) a moot point. The LES modeling that was done for Phoenix was driven by the possibility of oscillations in the parachute/lander system that (according to my understand-

ing) are not problematic for either the MSL or the Mars 2020 spacecraft. I'm unaware of any LES modeling being requested/performed for Insight. In the near future, SpaceX and others will be landing massive payloads; and, the EDL trajectories for such payloads will cause the spatial variation of the CBL depth (as modified by topographic circulations) to be far more important during the final phase of 'flight' (quite low for large horizontal distances to make the greatest use of larger air densities). Results from LES are likely to provide the turbulence spectra that turns out to be crucial for autonomous navigation software, etc: : : My sense at this point is that the reason for high-resolution LES modeling may have much more to do with autonomous robotic flying exploration vehicles, such as Mars Helicopter: http://www.jpl.nasa.gov/news/news.php?feature=4457

This is a very good point, especially since our paper discusses LES modeling that was performed in the context of an EDL analysis. We added in the revised text a few sentences about this discussion.

The manuscript was not clear on whether the surface layer scheme truly only acts on the lowest model layer acting with the ground for turbulence closure. I know this is the case for the OSU Mars LES model and believe it must also be so for the LMD and SwRI LES model. It's a point worth making, especially since the authors see differences in the near surface wind and/or temperature profiles. The structure near the surface is quite possibly another means through which the subgrid mixing can be examined in an intercomparison.

Indeed, what the Reviewer describes is a standard way to account for surface layers in our model too. We completely agree that surface layer is another plausible source for the observed discrepancies between the LMD and SwRI LES models. We mention this possibility in our new section "Challenges".

The authors certainly recognize that systematic intercomparisons between mesoscale and/or LES models have not been carried out, possibly because no

funding entity seems to think that such intercomparisons are important. I acknowledge and appreciate that this work has tried to remedy this situation. Since atmospheric dynamics are actually so much more important on Mars in relation to physical processes than they are on the Earth, raising awareness here is important. Possibly, we are stuck in a terrestrial modeling paradigm that we need to escape to improve our mesoscale and LES modeling of the Martian atmosphere.

This is a comment that is impossible to include in a scientific paper, but we fully agree with the reviewer. This is exactly what we intend to do with our paper (and this is why we submit a revised version reformulated towards identifying all remaining challenges): provide a starting point to further intercomparison studies to try to remedy a situation (even a paradox shall we say) where mesoscale modeling are being extensively used to assess the conditions for EDL landing, yet 1) observations to validate those models are still scarce and 2) it is difficult to find funding, time, and human resources to make the necessary intercomparisons studies for such important tools. This is why we believe our study, albeit admittedly imperfect, is a necessary first step which is essential to publish.

Finally, even though it only leads to greater computational burden, it is definitely worth considering the value of running the LES model through one full diurnal cycle before examining results from the second full diurnal cycle. Such 'spin-up' does affect the results, with the second diurnal period quite different. When background forcing is added, the second diurnal cycle can be quite different from the first, where the morning air temperature profiles (before the onset of convection) differ significantly, which noticeably affects the growth/evolution of the CBL. In the diurnal cycle, there is another weakness of LES (the highly stable regime, even more stable in the atmosphere of Mars) to investigate and better understand. We strive for the most realistic/honest results from our models; and, in this, intercomparison is an art form that I hope efforts such as this convince us to practice more enthusiastically.

Addressing this question is beyond the scope of our paper in which we only focus on the daytime convective boundary layer, because this is the focus of the vast majority of the published Mars LES studies. There is actually another reason: simulating the small-scale turbulence in highly stable conditions is still challenging also on the Earth.

The reviewer makes an interesting point though: differences may be found when comparing the second LES day with the first LES day. We left this discussion apart because we felt it has already been published elsewhere [cf. Tyler et al. 2008], and we would not have been able to provide new elements on this question.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/gmd-2016-241/gmd-2016-241-AC2-supplement.pdf

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