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Interactive comment on "The Parallelized Large-Eddy Simulation Model (PALM) version 4.0 for atmospheric and oceanic flows: model formulation, recent developments, and future perspectives" *by* B. Maronga et al.

Anonymous Referee #1

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1 General Comments

Overall, this manuscript is a very nice overview of the PALM 4.0 model. The model components are presented in impressive detail. In some cases, I found certain passages overly wordy or comprised of overly colloquial verbiage. However, I think the authors offer a good balance between approachability and detail. I especially appreciate the concluding sections where past work is shown in conjunction with future uses. This is of high value to a user new to the code. I recommend the manuscript be ac-

cepted, pending a few minor revisions and technical edits.

2 Specific Comments

- Perhaps it is a matter of semantics, but I would suggest the authors rethink the casual use of resolution to describe the numerical grid spacing. In my view, using the word "resolution" implies to users and non-experts that the model is capable of resolving features on the order of the grid scale. Spectral analysis of course shows that the effective resolution is several times larger than the grid spacing due to many factors. I understand what the authors are conveying, but I suggest that they change "resolution" to "spacing" when describing explicit values (*e.g.*, 5-m spacing, not 5-m resolution). Resolution is broad terms is still reasonable (*e.g.* we increased the model resolution).

- I do not think the authors explicitly say why, but I am curious why 5th-order advection was chosen for LES applications. The scheme, coupled with 3rd-order RK time stepping is known to be overly dissipative, even beyond the grid scale (see, for example, Gibbs and Fedorovich 2014 "Comparison of Convective Boundary Layer Velocity Spectra Retrieved from Large- Eddy-Simulation and Weather Research and Forecasting Model Data"). I'm not sure it needs to be justified, but several other model documents discuss why particular numerics are used. It is but one data point, but I would be interested.

- The authors employ the ever popular Deardorff 1.5-order TKE closure. They also note that future uses of the code will almost certainly extend to stable boundary layers. Are the authors considering an update to the closure? For instance, the scheme was never really designed for stable boundary layers, per se. The formulation for Kh will almost certainly lead to values larger than Km for realistic values of N. This can overestimate the effects of stratification (as shown by Schumann

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1991). Another byproduct is the possibility of a near step-jump in Kh values between adjoining grid cells in transition periods. This patchy Prandtl pattern can modify local patterns of temperature gradients in a way that can lead to false conclusions about the flow. Do the authors use PALM much for stable conditions? If so, do they have a feeling for how well the scheme behaves in these situations?

- On page 1552 line 12, the authors state: "The model is initialized by horizontally homogeneous vertical profiles of potential temperature, specific humidity (or a passive scalar), and the horizontal wind velocities." I am wondering if the code uses a specific method to generate turbulence initially. For instance by randomly perturbing the first model level values of potential temperature.

- The document describes a very robust system of available components (terrain, oceans, canopy, etc). I wonder if the authors have any test/ideal cases that might demonstrate the code's performance on famous general cases and those that might benefit from the new additions? It might be reassuring to readers to see visual proof of physical capabilities of the model.

- Perhaps it is my own stylistic preference, but I do not like the inclusion of code samples. I am not sure if it is required with this journal, but if not I would suggest removing them. The authors are talented in describing the procedures using words. In my estimation, a general new user might not care about the literal outlay of code that was used to make efficient loops. I suggest using an approach like that in the WRF technical documentation (see Skamarock 2008, "A Description of the Advanced Research WRF Version 3"), where procedures are described in text (and charts when necessary).

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3 Technical Correction

- On Page 1540, line 16, I suggest changing the sentence to read, "**the** first key features of LES **were** studied by Lilly (1967) and Deardorff (1973, 1974)
- Page 1540, line 24: I suggest changing Turbulent to Turbulence.
- Page 1541, lines 19-20: I suggest changing the sentence to read, "Thus, Raasch and Schroter (2001) can no longer ..."
- In several instance (for example Equation 8) the authors use multiple parentheses in a single expression. I find it more readable if the interior parenthesis are brackets, or some other more easily differentiated brace.

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