

Interactive comment on “Parallel algorithms for planar and spherical Delaunay construction with an application to centroidal Voronoi tessellations” by D. W. Jacobsen et al.

Anonymous Referee #2

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The paper presents a new parallel algorithm for construction of Delaunay and Voronoi tessellations on sphere by employing the overlapping domain decomposition. The presented algorithm has a potential to be of a great use for the whole community dealing with the numerical modelling on unstructured meshes. I would like to recommend this paper for publications after a minor revision which still suggests some changes.

1. The method is great but the novel technique is not clear to me from the abstract and overall through the text. Is it about overlapping domain decomposition only? Also some overview over existing algorithms is definitely missing. The authors refer them as "a few algorithms" which are limited to the planar surfaces.

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The approach employing stereographic projections for triangulations on the sphere has been used for example in Lambrechts et al., Ocean Dynamics 2008. Please indicate the novelty in MPI-SCVT going beyond the use of spherical caps for parallelisation of the approach. Also it's not clear which part of the code has been developed by the authors. The text indicates the Triangle software by J. Shewchuk for the construction of Delaunay tessellations. Is it actually used in the package?

2. What if the boundaries or continents are present? Can this technique be modified to discretize ocean only? I didn't find any comment on it in the paper (compare remark 12).

3. In line above 5th in the abstract, mentioning sphere instead, or together with Euclidean domains would be more relevant when speak about climate modelling.

4. When giving the references regarding existing unstructured approaches: is there any model which employs an unstructured mesh and goes beyond the test cases or shallow water study?

5. Chapter 2.1 can be either visualized or simplified or deleted at all. Why to provide basic material if it is not used further in the paper? Also the practical importance of this work as well as provided examples are limited to 2D and 3D cases. I would just shortly mention somewhere that the technique might be extended to R^k .

6. I fully agree with the 1st reviewer, that some discussion on the optimal choice of radius (chapter 2.2, n25) must be included.

7. In chapter 2.3/n25 giving extra formula of CE norm is irrelevant at this place. I would rather delete it or move to the other norms.

8. Is chapter 3.1 introducing stereographic projection really needed? especially second part of it? This may be found in wikipedia (compare remark 1).

9. Sorting to optimize the load balance (chapter 4.2.2) is first mentioned in the result path. Wouldn't it be proper to say something about it already in the description part.

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10. All through the chapters 4.x the total amount of different generators used is mentioned. Wouldn't it be better to introduce labels for them like 'coarse', 'medium', 'fine', for instance. Also the corresponding resolutions are first mentioned in 4.4.

11. Concerning figures, I would say, that figures 1,2,3 are too trivial, also I would suggest to combine fig. 11 and 12 into subplots

12. Since the approach aims at meshes for climate simulations it would be very desirable to include examples for a more realistic density function than the one indicated in Eq. (6). Are there results for topography based meshes and how are the regions chosen in such a general case with respect to optimal load balancing.

13. Please rephrase the final sentences in section 4.4 page 1448, starting in line 5. Expressions like "More efficiency gains ... could result in linear scaling" are highly speculative and are not supported by the current performance, especially for small generators. I agree that improvements may be obtained by optimized communication however I would suggest to be more precise in the statements.

Interactive comment on Geosci. Model Dev. Discuss., 6, 1427, 2013.