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Interactive comment on "On the numerical stability of surface-atmosphere coupling in weather and climate models" *by* Anton Beljaars et al.

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[gmd, manuscript]copernicus We thank the reviewer (Florian Remarie) for the extensive review, the constructive comments, and for providing a stability analysis to complement the paper. The practical relevance of this paper is clearly recognized and we appreciate the encouragement to publish after revision.

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Discussion paper

Response to the general comments

1 The paper is about stability of the numerical coupling and we recognize that a formal stability analysis is missing. The reason for not including such an analysis is that the proposed scheme is equivalent to a fully implicit scheme with known stability characteristics. However, we feel that the stability analysis is a welcome addition to the paper.

Using 1 mm thick snow layers is unusual in large scale models. Many models still use a single slab of snow where the layer thickness is controlled by snow mass and density. There are two reasons why the stability issue was urgent in the ECMWF model: (i) in snow accumulation and snow melt conditions the layer thickness can be very small, and (ii) there is evidence that thin layers are needed because the temperature response of the snow skin temperature is rather fast and currently not captured by models. Stability is an absolute requirement for all model points. So far, accuracy implications have been seen from the vertical discretization, but not from the time stepping. Some discussion will be added to the manuscript.

2 It is indeed interesting that the instability can occur for constant forcing, and the paper might give the wrong impression. We will revise the paper to make sure that this point is clear.

3 We agree that the statement about long time steps is not very precise. It is of course about the time step in relation to the physical time scale of the discretized problem (which depends on vertical discretization and diffusion coefficients). We will revise the manuscript accordingly.

4 The statement on p.6 line 5 about accuracy is indeed not very general and limited to the case that is presented. We will modify the text to clarify. It is worth pointing out that the paper is not about accuracy. Numerical accuracy is secondary to stability for practical applications. Furthermore, we feel that the accuracy of all formulations is good compared to the knowledge about the parametrized equations.

5 We agree that conclusions on stability can not be generalized on the basis of limited empirical numerical experimentation. However, as pointed out under 1, we were not surprised because the scheme is equivalent to a fully implicit formulation. It is good to see this confirmed by a formal stability analysis.

6 We agree that it is of interest to consider a non-uniform grid and flow dependent diffusion coefficients. The conclusion already discusses the case of non-uniform diffusion coefficients, and there is no reason why a non-uniform grid would behave differently from non-uniform diffusion coefficients. This point will be added in the discussion.

Temperature dependent diffusion coefficients is a completely different story because it potentially introduces a non-linear instability which is classic in atmospheric boundary layer schemes (see e.g. Kalnay and Kanamitsu 1988: "Time schemes for strongly nonlinear damping equations", Monthly Weather Review, 116, 1945-1958). Discussion of this issue is beyond the scope of the paper.

7 The benefit for real models is stability (which is an absolute requirement) and that advantage can be taken from finer vertical discretization resulting in a faster response of the surface temperature.

Response to the technical corrections

We thank the reviewer for his careful reading. The corrections will be included in the revised manuscript. In line with the request by reviewer 2 we will add a paragraph on the tri-diagonal solver in Appendix A. It is a standard Gaussian elimination procedure, which I think, is also called the Thomas algorithm.