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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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We thank the reviewer (Alex West) for the comprehensive and constructive review. We very much appreciate the time and effort to understand and digest the proposed method to improve stability. The reviewer finds the method convincing, but feels that the clarity of the presentation can be improved. The comments provided are very helpful.

Point by point

1 Although the solution of the tri-diagonal matrix by Gaussian elimination is standard, it is worth adding a few lines in the Appendix. This suggestion was also made by reviewer 1.

2 The availability of the forcing temperature at the new time level is a simplification for this study. However, it is not a limitation. The coupling scheme to an atmospheric model (i.e. not just a forcing level) as described by the reviewer, is not what we have in mind. Although it is not the topic of the paper, it is obviously a shortcoming of the paper not to discuss it. Thanks for raising this issue.

The way it can be done in a fully implicit way is by doing the elimination phase of the tri-diagonal matrix for the turbulent diffusion in the atmosphere (from top to surface) exactly in same way as is done for the surface heat diffusion in this paper. This procedure leads also to a linear relation between temperature at the lowest atmospheric model level and the heat flux into the surface. This relation replaces the imposed temperature at the new time level. This is precisely the coupling procedure followed in the ECMWF model and is compatible with the Best et al. (2004) approach. It is equivalent to solving a single tri-diagonal matrix that handles the entire atmosphere and surface as a single implicit problem.

We propose to add a paragraph in the paper to discuss how the method can be applied in a fully coupled atmosphere / surface model.

3 The reviewer raises an interesting point on the temporal evolution of temperature as the result of a perturbation at the surface. It is argued that the final scaling relation can be achieved in a more logical way. Reading the manuscript again, we agree that the transition from the continuous to the discrete problem is vague, which does not help clarity of the manuscript.

The simplest way of deriving a similarity relation for α is by doing a basic dimension analysis, which leads directly to equation (20) of the manuscript for the continuous problem (given time scale Δt). The discrete problem adds a new length scale resulting in a functional dependence on $(\delta/\Delta z)$, i.e. equation (22) of the manuscript. We opted to start with the traditional scaling relation for the diffusion equation and then to add the complexity of the discretization.

The derivation by the reviewer is attractive, but after thinking about it more carefully, I am not sure any more. The attractive aspect is the simplicity and the fact that function h (eq. 20 of the manuscripts) comes back as function f (eq. 22). It is important to realize that the meaning is not necessarily the same. Function h represents the shape of the temperature profile, whereas function f is an empirical function that describes the transition from one asymptotic scaling regime (the non-discretized problem) to another (the extremely coarse vertical resolution regime). The two functions are related and perhaps even close, but I am not sure they are the same. To demonstrate the potential difference, it is necessary to consider the average temperature (or average h) over the discretization intervals, instead of using midpoint values, otherwise conservation is lost (I think).

The beauty of similarity theory is that we don't need to answer this question. Function f is an empirical function and we can derive it from numerical experiments.

In view of the discussion above, we propose to modify the manuscript such that it is clear where the transition is made from the continuous to the discrete problem.