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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A new multi-gas constrained model of trace gas non-homogeneous transport in firn: evaluation and behavior at eleven polar sites" *by* E. Witrant et al.

Anonymous Referee #2

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This paper presents a newly-developed forward model of gas movement in firn. The model is then used as part of an iterative inversion scheme to infer the diffusivity-depth profile using a multi-tracer method (developed and presented in a separate paper by Buizert et al.). Results of the inversion studies at 11 different sites are discussed, with the focus on the identification of various regions in the firn (the convective zone, the diffusive zone, the lock-in zone and close-off depth).

I found this paper exceedingly difficult to review. Scientifically, I believe the model presented here probably has real merit, and may well prove useful in future studies of firn air. To the extent that I can assess it, the model appears to be thoughtfully developed,

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and the mathematics is almost certainly sound. The modelling work presented in this paper obviously represents a great deal of effort and time.

Having said that, the small-scale organization of the paper, the inconsistent level of detail selected by the authors, and the use of the English language are all huge barriers standing between even a committed reader and the scientific content of this paper. In fact, I found them insurmountable, and after carefully reading up through section 2.7, I simply gave up on a critical assessment and started skimming.

While I know it is not the case, this paper reads a bit like a PhD dissertation (hundreds of pages in length) that has been jammed into the page restrictions of a journal. On the other hand, there is some material that seems almost irrelevant. One example is Section 2.3, where a filtration vector is introduced, but never used (as far as I can tell). Furthermore, in Section 2.3 the Dusty Gas Model is introduced as a superior tool, even though the authors (quite appropriately) choose to work with an Advection Diffusion Model instead.

I believe the underlying problems with the paper are two-fold. First, this paper often reads like a mathematical exercise, rather than a description of physical phenomena. It would benefit greatly from frequent, explicit and meaningful plain-language statements about the processes that each mathematical expression represents. For example: The introductory paragraph for section 2 (page 23034) tells us the model is built on "mass conservation and fundamental physical laws." This is nearly meaningless. All of the firn models that I am familiar with conserve mass. Furthermore, Darcy's law is not fundamental, nor is Fick's first law. These "laws" are really just phenomenological characterizations of the average behavior of many molecules. It would be far more useful to be told that (for example) "the model describes the bulk fluid motion of air in the firn using Darcy's law, and the movement of trace gases within the background air using Fick's 1st law"). Another example: A_ss (a version in roman and another in italic fonts) is/are never adequately defined. Despite having read p.23044 several times, I remain baffled by the physical meaning of these terms.

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The second problem is one of language. I have great admiration for the ability to write in a language other than one's mother tongue. Nonetheless, the reality is that this paper desperately needs close attention from a native English speaker. There are countless places where statements are roughly grammatically correct, but are meaningless, cryptic or simplly distracting. For example, in the abstract: "almost stagnant behavior described by Darcy's law (gravity effect)" In fact, gravitational fractionation arises purely from hydrostatic equilibrium in a gravitational field. On page 23038, line 13 "stratified state at equilibrium" has no meaning to me. On page 23038 lines 21-22 currently read "...as an ideal gas and that the effect of thermal flows can be neglected". This should read instead "...as an isothermal ideal gas". Page 23038 line 25: Eq. 1b is not actually needed (despite claims to the contrary). The first equation on line 23039 arises directly from Eq. 3 (in the absence of firn sinking and bubble closure.)

Finally, there are additional challenges from the notational style. For example, the authors have chosen to use subscripts of "z" and "t" rather than writing out partial derivatives explicitly. In addition, acronyms are adopted for terms that appear only a few times, such as Dusty Gas Model (DGM), Quasi Steady State (QSS), Boundary Value Problem (BVP). Mentally interpreting these notational shortcuts is yet another distraction from the task of understanding the science they describe.

To my mind, in order to be a useful contribution to the scientific literature this paper needs a major overhaul. There may be additional work needed on the model, or it may be free of errors, but with the manuscript in its present state, I simply can't make this judgement.

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