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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 #1

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General: This paper could be an important contribution to the firn air modeling efforts. However the work is presented in a cumbersome style and not easy to follow even for a scientist with good background knowledge in this subject. It is too lengthy and contains substantial misconceptions. After struggling through the first 12 pages I decided to stop my review, because I found the status of the paper unacceptable. I do not question the value of the results at this point, but I would wait for such judgment until the paper is in a better condition. I suggest to focus on the new aspects of this study, to use references for established ideas and results (e.g. it is not necessary to derive the barometric formula) and to discard unnecessary/unused sections. It should be relatively easy for the many expert co-authors to give good advices.

My decision is based on the following:

(1) One main problem is that the authors do not present the work in a goal-oriented way. Rather the first 12 pages contain a number of effects, thoughts and statements, subjects partly presented with completely wrong weight, while relevant parts are mentioned in a sub-clause. For example there is a lengthy discussion about hydrostatic equilibrium that is not reached due to viscous flow. There is no discussion that the velocities discussed here are in the order of cm per year, which should be compared to time constants of viscous flows. There is no discussion on the effects of temperature and surface pressure variations that are neglected. On the other hand on page 23041, first line, the most important trapping rate of air bubbles is given in an in-line equation, without any comments. This trapping rate is very debatable.

(2) Reaching eq. 3 after 10 lengthy pages, the discussion on viscous non-equilibrium is suddenly terminated by "A direct approach to this problem would require a knowledge of the firn permeability (scaling laws such as those proposed by Schwander (1989) or Freitag et al. (2002) could be used for this purpose) and would require solution of a nonlinear BVP, which 10 would necessitate a dedicated mathematical analysis (our tests using classical commercial solvers failed to provide satisfactory results)." I guess most readers will be confused here if not earlier.

(3) Then follows a variation approach (deviation from hydrostatic equilibrium). Does the superscript –o mean hydrostatic equilibrium? (it is not well explained!). If so, is the almost trivial solution for w(air) given at the bottom of page 23041 the outcome of the first 10 pages?

(4) There are a number of misconceptions and errors:

Page 23031, line 12: "..and an almost-stagnant behavior described by Darcy's law (gravity effect)." A stagnant behavior is not described by Darcy's law.

Page 23037, line 11: What is the purpose of the filtration vector? It is not used further in the manuscript. It is rather unusual to speak of a flow speed w(x) in a diffusive mixing environment.

line 16: Eq. 2 does not follow from the previous equation. This can be check e.g. by substituting the variables with suitable numbers. The final eq. is not a solution for phi(ai) as suggested by “The specific filtration vector is then..” (phi(ai) is still on both sides of the equation).

Page 23039, line 12: This equation is only valid for a single trace gas in air. This should be stated, since previous equations and the title of the paper suggest a multi-trace gas discussion.

Page 23040, line 6: “Note that, while Fick’s law is reasonably accurate to describe the molecular diffusive flux for a nonstagnant gas, it was found to be totally inadequate for a stagnant gas (considered as a gas without source or sink, thus at steady state), as discussed by Thorstenson and Pollock (1989). Note also that Fick’s Law is adequate for a trace gas, but for a major gas the Stefan-Maxwell equations (diffusion in a multi-component 10 system) must be used. This implies, in the framework of trace gas transport in firns, that Fick’s law is suitable to describe the dynamics of gases with large atmospheric variations in nonstagnant firn regions but should be used with care otherwise (small concentration gradients or limited airflow in deep firn)”. There is a serious misconception here. Stagnant gases described in Thorstenson are not the same as stagnant firn regions. Fick’s law is precisely the physics to describe (trace gas!) fluxes in a stagnant environment. This has been proven valid in many studies. If Fick’s law is inadequate e.g. in the close-off zone then it is not due to stagnant conditions but rather due to flows induced by compaction of almost close firn layers.

(5) The manuscript is loaded with “empty phrases”, which is another reason that makes the paper too long. E.g. page 23034, line 16: “Several issues associated with the transport characterization are detailed, as well as simplifying hypotheses that allow the use of this model for multigas diffusivity optimization.”???

(6) By checking further sections of the manuscript this cumbersome style unfortunately carries on. E.g. page 23046, line 7: “The choice of Ass = 0 below zlid implies that the internal and external forces have a negligible impact on trace gases in the vertical direction, which is the case if the surface stress tensor or non-vertical pressure gradients dominate the gravitational force in the LIZ. The stress tensor is induced by the firn deformation gradient (Coussy, 2003) and local pressure gradients that may appear if thin walls enclosing a pressurized bubble break, two phenomena associated with the bubble closure process.” It is difficult to guess the meaning of this if one does not already have a good background in the matter.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 23029, 2011.