Atmos. Chem. Phys. Discuss., 11, C14196–C14206, 2012
www.atmos-chem-phys-discuss.net/11/C14196/2012/

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.

E. Witrant et al.

emmanuel.witrant@ujf-grenoble.fr

Received and published: 13 January 2012

Answers to Referee 2

The authors would like to thank the referee for her/his constructive remarks. We tried our best to answer all the questions raised, as detailed below.

RC: 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.).*

Our multi-gas inversion method is only briefly summarized in Buizert et al. (focused on the intercomparison of 6 forward models applied to the NEEM site). Buizert et al. refer to this ACPD paper for LGGE-GIPSA model description. The inversion method is presented in Section 3.

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

AC: Our intent was to emphasize the multidisciplinary aspects of the firn air modeling problem, which typically renders the review process particularly difficult. Major revisions will be made to sim-

ACPD

11, C14196–C14206, 2012

plify the presentation and take into account comments by the two referees.

RC: 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.

AC: Some precise suggestions on the small-scale organization could have been helpful. A major rewriting and shortening of Section 2 focusing on the original aspects of this model will be performed. The proposed new structure is detailed in our answer to Referee 1. Other sections will be screened for unnecessary details. The writing style will be revised and Copernicus copy-editing for English service could be used if necessary after revision.

RC: 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.

AC: Considering the existing literature in firn modeling and poromechanics, the authors did not foresee the difficulty met by

ACPD

11, C14196–C14206, 2012

Interactive Comment

the referees to evaluate Section 2. Section 2 was intended to contain a multi-disciplinary analysis of firn physics, especially pointing some advances in poromechanics which could lead to future advances in firn modeling. We agree that it is complex in nature and a major rewriting will shorten, simplify and focus it on the LGGE-GIPSA model description.

Sections 3 and 4 are shorter and less complex in nature. Efforts will be made to improve their conciseness and writing style. However, the absence of precise scientific comments/suggestions from the referees on these sections will limit our ability to improve them.

RC: 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).

AC: Both referees converge on suggesting to remove this discussion. It will be done in the revised manuscript.

RC: 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.

AC: The Dusty Gas Model is more complete and frequently used

ACPD

11, C14196–C14206, 2012

Interactive Comment

to model transport in unsaturated media, compared to the simpler Advection Diffusion Model, which neglects the interactions between the different gases to obtain a decoupled model. DGM was introduced as a mean to discuss the limitation of neglecting interactions between gases in our model. This discussion will be shortened and clarified, although mention of the DGM is needed to clarify the meaning of our multi trace-gas approach in relation with a comment from Referee 1.

First major problems

RC: 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.

AC: The mathematical exercise has been quite essential to analyze and update the initial LGGE model (based on the work of Rommelaere 1997) for multi-gas optimization objectives. We agree with the referee on the fact that its description complexifies the manuscript. Revised Section 2 will essentially focus on the treatment of Equation (1b), which describes gas transport in open

ACPD

11, C14196–C14206, 2012

Interactive Comment

porosity. The physical meaning of its terms will be described. The representation of molecular diffusion, eddy diffusion and gravitational fractionation in the term $[\rho_f^o f w_f]_z$ of Equation (1b) will be emphasized.

RC: 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").

AC: We agree with the referee on the fact that the term “fundamental” is confusing. It is used only once in the manuscript and not explained/justified. We also agree on the fact that mass conservation has to be ensured in all firn models. The introductory paragraph for Section 2 will be suppressed.

Concerning the differences between firn models, we would like to mention that previous models have been formulated in terms of

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

gas quantity (involving open porosity in the state variable), concentration (involving air density distribution) or isotopic ratios. The different models are mostly equivalent (see supplement) but considering the fluid (trace gas or air) mass (or density) as the state variable has the advantage of allowing a more direct use of the laws of physics and of numerous results established in poromechanics. This is not directly discussed in the introduction of Section 2 though.

The revised manuscript will emphasize the modeled physical processes.

RC: 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.

AC: A_{ss} results from an analysis of the steady-state behavior of the trace gases (p 23043 l. 19-20). This too quick statement will be explained in the revised manuscript. Special attention will be brought to the phenomenological description of this term in the paper update.

ACPD

11, C14196–C14206, 2012

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Second major problems

RC: 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.

AC: The writing style will be revised and Copernicus copy-editing for English service could be used if necessary after revision. The examples provided below also reflect scientific understanding issues, and effort will be made to introduce and clarify concepts not usually used in the glaciology community.

RC: 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.

AC: In the abstract, this can be rephrased as "the treatment of gravitational fractionation". We view the issues related to gravity as complex and two folds. First, it is introduced in the mass conservation equation as an external force with Darcy's law: a

ACPD

11, C14196–C14206, 2012

simplification of the external force impact on the momentum conservation (classical Euler or Navier-Stokes equations, e.g. see Anderson Ch. 2). Including the momentum equation while avoiding its dynamics is a major problem when deriving simplified fluid models. Second, hydrostatic equilibrium is not always reached, especially in the convective zone due to fast transport processes (e.g. wind and pressure variations) that are not fully modeled, and in the lock-in zone due to a dominant effect of advective flows and bubble closure.

RC: On page 23038, line 13 "stratified state at equilibrium" has no meaning to me.

AC: This relates to a similarity with the relative gravity impact (buoyancy-driven volume flux) on a stratified flow (e.g. B.R. Morton, "Forced plumes", Journal of Fluid Mechanics, 1959). It will be removed in the paper update.

RC: 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".

AC: It is not necessary to consider a temperature that is constant in time for equation 3 (e.g. depth-averaged temperature variations in time could be considered), thus the isothermal hypothesis is

ACPD
11, C14196–C14206, 2012

somewhat stronger. As the isothermal hypothesis is done at the beginning of the section, such subtle distinction is not necessary and will be removed in the revised version.

RC: 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.

AC: It is true that the first equation in p 23039 does not need Eq. 1b. Equation 1b is only needed to understand the text just after: "taking into account firn sinking and gas trapping, the hydrostatic equilibrium is not reached". The derivation of hydrostatic equilibrium from equation (3) will be suppressed as proposed by Referee 1.

RC: 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.

AC: Such notations are often used in other fields of science but can easily be changed.

RC: In addition, acronyms are adopted for terms that appear only a few times, such as Dusty Gas Model (DGM), Quasi Steady

ACPD

11, C14196–C14206, 2012

Interactive Comment

State (QSS), Boundary Value Problem (BVP). Mentally interpreting these notational shortcuts is yet another distraction from the task of understanding the science they describe.

AC: This will be changed too.

RC: 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.

AC: The authors will propose a major rewriting of the forward model description (Section 2) and improve the writing style of other sections.

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