

Interactive comment on “Kinetic fractionation of gases by deep air convection in polar firn” by K. Kawamura et al.

Anonymous Referee #2

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This article presents an attempt to develop a proxy indicator of past convection in firn. This issue is related to the definition of the difference in age between gases and the surrounding ice, it is thus of wide interest to the ice core, paleo environment and atmospheric sciences community. It likely involves an important experimental development to be able to precisely measure differences between isotopic ratios of several gases of about 0.03 ‰. The results on Fig. 5 show that different values of convective transport intensity and depth in firn lead to the same model results below 40 meters depth. As ice core data only trace isotopic ratios in deep firn (where most of the air is trapped), I am not optimistic about the prospect of using Krypton and Xenon

C2175

isotopes as indicators of paleo convection. The key missing element is a direct link between the measured δ values of isotopic tracers and convective zone thickness. Nevertheless, the initial idea of using inert gases having different physical properties (molecular and thermal diffusion coefficients in air) to better constrain past convection in firn is excellent and although deceiving, the results deserve to be published in the scope of Atmospheric Chemistry and Physics and its special issue on firn air.

General comments

The presentation of the theory (Section 2 and Appendix A) can be made clearer and shorter to be easier to understand for a wider audience than scientists trained in firn physics. Equation (12) can be easily derived from Equation (4) in Severinghaus et al. (2010) (replacing $D_0 e^{-z/H}$ with D_{eddy}) and two simple definitions: $Pe = D_{eddy}/D_{mol}$ and $\epsilon_k = \delta_1/\Delta m_1 - \delta_2/\Delta m_2$. The basis of the simple theory is already presented (more clearly in terms of underlying assumptions) in Severinghaus et al. (2010), thus Section 2 can be shortened and Appendix A suppressed. The first sentence of the abstract suggests that a new physical process in firn has been discovered whereas the unmodified model of Severinghaus et al. (2010) can simulate it. This is needlessly confusing.

Section 5.1: it should be stated that the simple theory as applied here (integrating $Pe(z)$ from the Severinghaus et al. (2010) model) is not applicable to ice core data in paleo-climatic conditions as the vertical profile of $Pe(z)$ is unknown. For the calculation in Section 5.1, it is explained that the Severinghaus et al. (2010) model is run with thermal fractionation set to zero. I presume that in Figures 3 and 5, δ - depth lines in the diffusive zone for the four gases are not parallel due to the non null thermal fractionation. This effect is significant and has important consequences for ice core applications, it should be discussed.

Section 5.2: I disagree with the idea that the insensitivity of kinetic fractionation in deep

C2176

firm to upper firm convection intensity and scale height ($D_{eddy,0}$ and H) indicates that the measured isotopic ratios are efficient ice core proxies of the convective zone (p7038 l1-2). Which convection related physical parameter is traced by the data? Formulating the proxy indicator problem in the simplest way (ignoring the non steady state issue in the lock-in zone), the main question is: how can the diffusive zone and convective zone thicknesses (as defined for gas dating purposes) at the Megadunes site be reconstructed from isotopic measurements at the lock-in horizon? This question is not answered or discussed.

The abstract and conclusion should provide a more precise statement about the feasibility of constraining the convective zone from ice core data.

Specific comments

Page 7023 line 25 - page 7024 line 4: the stagnant zone concept seems important for the authors (repeated three times) but is not mentioned in the references cited (Schwander et al., 1989; Sowers et al., 1989), an appropriate reference should be provided.

Page 7024 lines 4-15: in relation with Section 5.2, it should be mentioned that convection can be formulated in different ways in physical models. For example, references to Schwander (1989) and Powers et al. (1985), cited in the article, could be used in this aim.

Page 7024 lines 22-25: a third hypothesis involving the impurity content of ice has been made recently and should be mentioned (Hörhold et al., 2012; Capron et al., 2013).

Page 7026 Equation 4: the middle and right terms of Eq. 4 imply that a downward velocity term due to bubble trapping has to balance convection. This is not true. The middle term should be omitted and the text should explain that the Péclet number can be defined in different ways depending on the dominant physical processes at work (and the way they are formulated in models). At the Megadunes site, the near

C2177

zero accumulation rate implies a near-zero downward velocity and bubble trapping, whereas the effect of convection is strong in the upper firm. The role of seasonally varying thermal convection on disequilibrium should be discussed.

Page 7028 Equations 8 and 9: I see only equilibrium terms in these equations aiming at representing disequilibrium. This is needlessly confusing.

Page 7029 lines 3-5 and Table 1: This seemingly new presentation of molecular diffusion coefficient ratios raises a strong uncertainty issue: the precision of molecular diffusion coefficients is of the order of the percent (see e.g. references in Buizert et al. 2012, supplementary Table 4), whereas the ratio of diffusion coefficients between two isotopes of the same gas has to be known more precisely. Thus the presentation of diffusion coefficient ratios between isotopes of different gases is needlessly confusing. The consistency between the diffusion coefficients used here and those in Buizert et al. (2012), supplementary Table 5 and its impact on the main results should be explained.

Page 7030 lines 11-12: the precision of the approximation cannot be estimated before the comparison with a model involving less approximations is made (Section 5). The required level of precision for ϵ_k is rather $<0.01\%$ than 1% .

Page 7032 lines 13-15: the magnitude of the pressure imbalance and chemical slope corrections should be provided.

Page 7033: the word "arbitrary" is used three times to characterize the eddy diffusivity parametrisation. As convection is the main topic of the article, this is confusing and should be reformulated.

Page 7037 line 24: replace "data-based values" with "simple theory values"

Page 7039 lines 5-15: these lines contain introducing rather than concluding statements.

Technical corrections

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Page 7023 line 25: Schwander et al., 1989 - suppress "et al."

Page 7027 line 12: explain what is meant by nearly in "eddy diffusivity is nearly the same for all gases".

Page 7030 line 10: "Many approximations are made above". The main approximations should be clearly summarized.

Table 1: Fuller et al. (year?) and Reid et al. (1987) are not provided in reference list. Gas names should be really specified in the table.

Table A1: what is meant by T_{ave} should be explained. Is it the classical arithmetic mean or the more complex mean in e.g. Eq. 4 of Grachev and Severinghaus (2003a) ?

Caption of Figure 4 should define which colour shows which parameter.

Figure A1: thin and thick lines are not clearly defined. Dots are hard to see.

Reference not cited in the reviewed article

Hörhold et al., Earth and Planetary Science Letters 325-326, p93-99, 2012.

Capron et al., Climate of the Past, 9, p983-999, 2013.

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