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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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The authors explain quantitatively a hitherto not described kinetic gas fractionation in the upper firn layers resulting from competing molecular and turbulent diffusion. Ice core records of this fractionation could soon become a proxy for convective mixing in firn. This work has therefore the potential to initiate solving the long existing problem of unknown importance of paleo-convection in polar firn. Reconstructing the size of past convective layers will lead to improved gas age chronologies and thus for example help constraining phase relations between greenhouse gases and temperature within and between hemispheres. Such records could also help to improve firn densification modelling during glacial conditions. The paper is well structured and the content very well presented. The experimental data appear of excellent quality. The only minus

seem to be in some mathematical deductions and some slips, as pointed out below.

Specific comments.

p. 7028, eq. 8: It should be specified here that Delta_m is a normalized mass difference, i.e. dimensionless (in order to make epsilon_k dimensionless). Later, to cancel Delta_m in Eq. (10), you use Eq.(2) containing a real mass, leaving a non-dimensionless epsilon_k. Also in Appendix A, Delta_m has the dimension of a mass! The use of this variable should be made consistent.

p. 7028, l. 12: "Only when Deddy roughly equals Dmol will there be kinetic fractionation". As there is fractionation also for Pe far away from 1 this statement is wrong (see Fig. 2). Use a more relative formulation.

p. 7028, l. 13: "Some simplification is possible by noting that the equilibrium gradient depends on Delta_m, which then cancels out". In addition it is assumed that q1=q2=1. Some short justification would be helpful for the reader.

p. 7029, l. 13: Eq. (14) should be Eq. (12) (?)

p. 7029, Appendix A: To derive Eq. (A12) you assume Pe to be constant with depth. If you assume the same for Eq. (12), then Eq (12) becomes identical with Eq. (A12) [modulo Delta_m, but this is due to the above mentioned dimension problem]. Then, if I didn't misunderstand, it seems that the exercise of Appendix A is in fact not to present the exact treatment, but rather the (exact) derivation of Eq. (12) under special conditions. (?)

p. 7038: Insensitivity to choice of D_eddy,0 and H: If the method is to be applied as a proxy for the past convective zone, what would you suggest as meaningful parameter(s) to characterize this zone? A plot of H versus 1/(D_eddy,0)ˆ2 shows a nearly linear relation (empirical), allowing to extrapolate to infinite D_eddy,0. Would the y-intercept be a useful value? (here about 6.6 m) - just a thought..

p. 7039, l. 22: "expected value". I think this term is not adequate. In the real kinetic world we do expect kinetic fractionation. So rather call it “value without kinetic fractionation”. The following explanatory sentence is then not needed.

p. 7042, l. 5+6: Eq. (14) => Eq. (12) (?)

p. 7042, l. 5: “Note the similarity of Eq. (A12), which is exact, with Eq. (14). This similarity suggests that Eq. (14) is an excellent approximation for most practical circumstances.” A similar look does not infer similar results. This is not scientific. Please be more precise. (But see also above comments on Appendix A)

p. 7054, Fig. 2: In my PDF the dashed line is hardly recognized as dashed

p. 7056, Fig. 4: Figure needs legend

p. 7058, Fig. A1: What is the reason for some straying values in the temperature records?

References: Bender et al, 2007 in text = Bender et al., 2006 in references? Fahnestock et al, 2002 in text = Fahnestock et al., 2000 in references? No citation in text found for: Battle et al. 2011 Fabre et al. 2000 Severinghaus and Brook, 1999 Reference for Grew and Ibbs, 1954 is missing.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 7021, 2013.