

## ***Interactive comment on “Pervasive diffusion of climate signals recorded in ice-vein ionic impurities” by Felix S. L. Ng***

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Initial response to Reviewer 2

Thank you for your positive and insightful review of the manuscript, and for providing a useful and engaging summary of its findings.

Your main suggestion is for the manuscript to clarify the different assumptions used in the present model and in the original model of Rempel et al. (2001), regarding the possible effect of grain size variations on the Gibbs-Thomson term in equations (7) and (8), and notably the idea that inverse coupling between grain size  $d_g$  and bulk impurity concentration  $c_B$  could make the vein face radii nearly uniform (causing the Gibbs-Thomson term to become constant) and prevent diffusion from occurring. I am happy to

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discuss this matter in the revision, and agree that such discussion can stimulate further study of the mechanisms behind grain-size variations. I plan to insert the passage and the associated discussion in the final section of the manuscript, and only briefly signpost the matter earlier, as the corresponding arguments are involved and contain intricacies that will be difficult for readers to appreciate and will disrupt the thread of the work if this discussion is delivered in the earlier sections.

I have a few reservations about the model assumption stated by you in the review for the Rempel et al. (2001) study. I think that those reservations need to be included in the new passage, in order to keep the reader informed of different viewpoints. They are given at the end.

Before describing them, I have a request. In the review, you wrote:

“... the original reasoning provided by Rempel et al. (2001, p. 570) emerges from the assumption that an anti-correlation between grain size and impurity loading makes vein radii uniform, as predicted if grain size scales inversely with the square of bulk impurity loading in equation (7) of the current work, thereby rendering the second term in equation (8) spatially uniform as well.”

This is an important clarification, as I don't think it is explicitly clear from the writing in their paper that their model had ignored the Gibbs-Thomson effect for the reason given above. The final sentences of their first paragraph on p. 570 do not relate the vein radii, via vein face curvature/radius, to the Gibbs-Thomson effect nor to depression of the melting temperature. Therefore I think that issuing this clarification will be useful to the field; conveniently, all review materials in The Cryosphere are also citable so that readers can trace its origin to this review discussion. Both the physical concept being stated, and the clarification of its role as an assumption in the Rempel et al. (2001) model, should be attributed to the originator rather than me. For this reason, can you please give me permission to reference the stated ideas to you via “personal communication”?

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The assumption that you have given for the Rempel et al. (2001) model goes as follows. It is posited that grain recrystallisation processes (at least, extraneous processes outside the formulation in section 2.1) lead to  $dg^2 c_B = \text{constant}$ , where  $dg$  is the mean grain size and  $c_B$  is the bulk ionic impurity concentration of the vein network. Then, the second part of equation (7) yields a constant vein face radius  $r_v$ , and the Gibbs-Thomson terms in equations (7) and (8) become constant at a given temperature (here I add that the same would be true for the general form described on lines 132-134 of my manuscript). The Gibbs-Thomson diffusion thus vanishes. Support is provided for the assumption, from the observed anti-correlation between grain size and impurity loading that has been reported by numerous ice core studies.

Here are my thoughts regarding this assumption:

1. The desired anti-correlation between  $dg$  and  $c_B$  needs to obey  $dg^2 c_B = \text{constant}$  exactly, for the diffusion to vanish. Moreover, an anti-correlation may not necessarily alleviate the diffusion: it could enhance the diffusion. For example, suppose that recrystallisation processes cause  $dg \propto 1/c_B$ . Equation (7) then yields  $r_v \approx \sqrt{1/c_B}$ , rather than  $r_v \approx \sqrt{c_B}$  (as indicated currently by the equation when the grain size varies slowly). Note that the dependence of  $r_v$  on  $c_B$  has not gone away. With the anti-correlation, the Gibbs-Thomson term in equation (8) now goes as  $c_B^{0.5}$ , instead of  $c_B^{-0.5}$ . This merely gives rise to a different nonlinear diffusion term on the right-hand side of equation (23). For certain values of the coefficient of proportionality in the relationship  $dg \propto 1/c_B$ , the diffusion can actually be stronger than currently predicted in my simulations (and this is true for many other kinds of negative relationships between  $dg$  and  $c_B$ ). Consequently, anti-correlation between  $dg$  and  $c_B$  does not generally support the model assumption that would prevent the Gibbs-Thomson diffusion from operating — it may support the opposite.

2. Although my numerical experiments prescribes smoothly-varying grain size profiles, the formulation in section 2.1 (equations (1) to (8)) remains general, in the sense that coupling of the grain size  $dg$  to other variables via recrystallisation processes is al-

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lowed (as indicated on line 237). I would hesitate to describe equations (1) to (8) as involving assumptions that render the present model as an “end member case”. This is especially because it is possible for inverse coupling of  $dg$  to fluctuations in  $c_B$  at short length scales to cause or even enhance the diffusion of signals (see Item 1). In contrast, I think that the specific assumption of  $dg^2 c_B = \text{constant}$  comes across as a special case in the application of the model in equations (1) to (8).

3. The bulk impurity loading shown by ice core studies to exhibit anti-correlation with the grain size is the sum of impurity contributions from the ice matrix, grain boundaries, and the vein system. However, the model variable  $c_B$ , which needs to satisfy  $dg^2 c_B = \text{constant}$  for the desired assumption to hold, refers to the ionic impurities in the vein network only. Accordingly, arguments that use the observed anti-correlation to support the assumption need to explain how the vein impurity loading relates to the total impurity loading; this explanation is missing. (As discussed in the manuscript, these quantities are not necessarily proportional to each other.)

4. It is not clear to me what physical mechanisms would enable the concentration of impurities in the veins (which make up  $c_B$ ) — as opposed to impurities at grain boundaries and in the ice matrix — to control the grain size. Existing theories that relate grain size variations to ionic impurity loading consider how dissolved impurities can reduce grain boundary mobility, through the production of drag force on grain boundaries (e.g. Alley et al., 1986, p. 422), but such mechanism refers to the impurities situated at grain boundaries, not to impurities in the vein network located at three-grain junctions.

The ideas in Items 3 and 4 indicate that for the desired assumption to hold, either some unknown/unexplained mechanism exists to allow the impurity in the veins to influence grain-scale recrystallisation processes in such a way for  $c_B$  to control the mean grain size in the right manner, or grain-boundary (maybe also matrix) impurities control the grain size  $dg$  and separately regulate the vein component  $c_B$  in just the right ways, to satisfy  $dg^2 c_B = \text{constant}$ . I think that one has to be quite hopeful for either set of interactions to yield the right behaviour to justify the assumption, especially as impurity

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concentration is only one of several factors known to affect the mean grain size. Having said this, I have not explored these theoretical possibilities (the present manuscript is not about how recrystallisation processes and impurity factors control grain size variations) so I do not rule them out, and they can be investigated in future research.

As mentioned before, I am happy to add a passage that covers your clarification of the assumption behind the Rempel et al. model, how it relates to the formulation in the present manuscript, and also the above elements.

Reference: Alley, R. B., Perepezko, J. H., and Bentley, C. R.: Grain growth in polar ice: I. Theory, *J. Glaciol.*, 32, 415–424, 1986.

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