Reviewer #1 (Anonymous)

I appreciate the authors responding to the comments and addressing the concerns raised. The manuscript is distinctly clearer in those areas. I remain ambivalent about whether the proposed approach solves the issue of the stress exponent in ice, given the uncertainty in the parameters inherent in the analysis (e.g., grain growth), the assumptions around grain shape and its measurement, and the lack of community consensus about grain size being a better piezometer or wattmeter. Despite that ambivalence, this paper adds to the discussion, and I think the community will benefit from its perspective.

I have only small recommended changes (line #s reference the track changes document):

Line 36: "report" to "reported"

Line 522: Duvall to Duval

Fig 6 caption (line 847) "Black curves show calculates constant..." to "Black curves show constant..."

Fig 6 caption (line 849): "(e-h) Same as panels a-d, but comparing..." to "(e-h) Same panel axes as a-d, comparing..."

We thank Reviewer #1 for their careful reading of our revised manuscript. All minor points have been corrected.

Reviewer #2 (Paul Bons)

I thank the authors for the extensive replies to the comments of both reviewers. The authors did acknowledge that there were issues that needed to be addressed and made changes to the text accordingly. This definitively did improve the manuscript. However, changes in the manuscript are at times rather brief and do not quite reflect the more extensive discussion in the comments & replies. I will only focus on two issues: the actual value of the stress exponent and the effect of a crystallographic preferred orientation (CPO).

The authors now do acknowledge (lines 35-44 in document with show changes) that although n=3 is normally used, actual observations do not always agree with it, referring to Cuffey & Kavanaugh (2011) and Budd & Jacka (1989). There are, of course, more papers (not cited) that suggest that n is unequal to 3, like n=4. The origin of n=3 is not really acknowledged: taking the minimum strain rate or maximum stress point, which is at only a few per cent of strain. This paper is about higher strains. Having added the few sentences, the manuscript continues as it was, i.e. effectively based on the assumption that n=3 is not only commonly used, but indeed correct. The suggestion from the original review to "include in

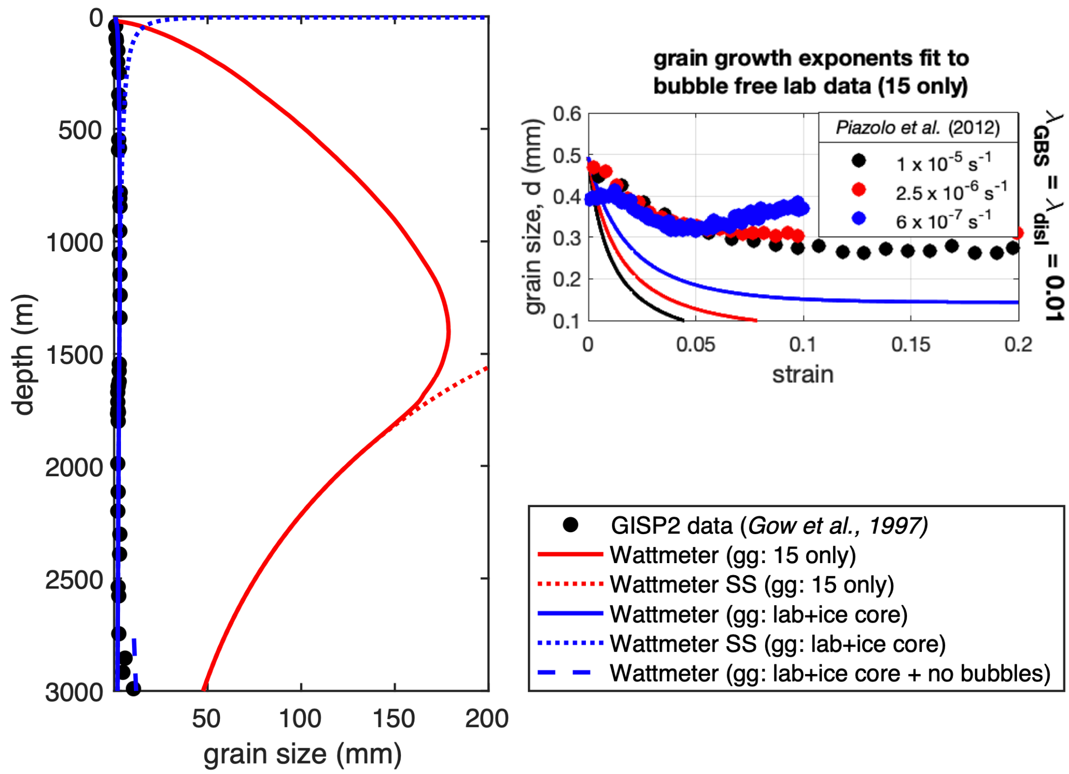
their following analysis what the consequences would be if n for natural flow is not 3, but perhaps indeed 4 as some claim to have measured in nature. Would this, for example, mean no contribution of GBS? Would the wattmeter work and give reasonable results?" is not considered.
On the contrary: In line 432, the authors write: " This provides an additional argument against applying the small grain growth exponents for bubble-free ice in the laboratory to natural settings. For example, if p = 2 the effective stress exponent for GBS-limited creep becomes 4.25. In this scenario, neither dislocation creep nor GBS-limited creep would result in an effective stress exponent that is consistent with the Glen law value." The authors argue thus that p should not be small, because the results would then not fit with n=3 of Glen's law, which is apparently taken as correct. If one acknowledges that n could be 4 (as I would say the actual velocity field of the Greenland ice sheet indicates), the conclusion would clearly be that p should be small, because it nicely fits the observations. This again shows that the growth law is a big uncertainty of the model.

We agree that this sentence could be misleading, and have modified it accordingly on lines 399–409 of the revised text.

A sceptical or malicious reader could interpret this as a circular argument: first fit the parameters to get n=3 and then claim success of the model, because it fits n=3. One could also come to the conclusion that if the model works for n=3 while ignoring mechanical anisotropy (see below) it must be wrong, since n=3 only applies to circa 3% strain and because ice is anisotropic. To avoid such unwanted interpretations, it would be good (or even imperative) if the authors would consider how well the wattmeter would work if n would be different, for example about 4.

Using the composite flow law employed in our study, there are two ways to arrive at an effective stress exponent of ~4. The first is to have creep via grain size insensitive dislocation creep. The second is to have grain size sensitive GBS creep with a grain growth exponent $p = \sim 2$, consistent with bubble-free ice. The reason that we do not favor grain size sensitive creep with a grain growth exponent of 2 is that with these parameters the model cannot simultaneously fit the laboratory data and the ice core data. The figure below shows an application of the wattmeter using the grain growth parameters for Exp. 15 from Azuma et al. (2012) for bubble-free ice. Note that while this model under-predicts grain size in the laboratory experiment, it greatly overpredicts the grain size in the ice core. This is consistent with the extrapolation of the grain growth data shown in Figure 2 (blue curve corresponding to Exp. 15). Further tuning of the wattmeter parameters (e.g., λ) may improve the fit to one data set, but will degrade the fit to the other. We argue that this further supports the use of a higher grain growth exponent in the wattmeter, and that ice flow characterized by $n = 4$ mostly likely corresponds to dislocation creep.

We have described this result on lines 399–409 of the revised text. For the moment, we have chosen not to add this new figure to the manuscript. However, if either the reviewer or editor feels that it would be beneficial to include it in the final version, we would be happy to do so.



The model focuses strongly (completely) on grain size as modifier of the effective viscosity. A big issue, raised in the review process, is actually the mechanical anisotropy of ice, which can greatly change the effective viscosity. After the review, the authors added only 2 short sentences (lines 522-524) addressing this elephant in the room. This I find rather meagre. What would be the effect of further grain size reduction that is mentioned? A lowering of the effective n for the whole ice sheet, if at the base where most shearing happens we may expect strong CPOs? Does that fit with some observations that grain size increases near the base? Or do other parameters need to be readjusted to refit the model to observations?

This comment led us to go back and reassess our model for enhanced grain growth at the base of the ice sheet. Originally, we had noted that using the grain growth law for bubble-free ice ($p \sim 2$) resulted in basal grain sizes that were too large, and thus we settled on an intermediate p value of 4 for basal ice in the pre-melting regime (Section 4.2). Following the reviewer's suggestion, we re-ran a model using the grain growth parameters for bubble-free ice, while simultaneously enhancing dislocation creep by a factor of 10 to simulate fabric development (based on the enhancement factor in Table 3.6 of Cuffey & Paterson, 2010). This does indeed result in a good fit to the grain sizes in the lowermost 200 m of ice (see dashed line in revised version of Figure 7). We have modified the text on lines 440–448 to describe these results.

Additional self-consistent modeling of fabric development coupled to grain size evolution is beyond the scope of the current study, but we do agree with the reviewer that this is an important avenue for future research.

The wattmeter approach is of interest, as has already been proven for rocks other than ice, and therefore the paper could make a valuable contribution to glaciology. Without truly addressing alternatives to the isotropic Glen's law model, I am afraid it may be dismissed by those that acknowledge that that model is not realistic.
I therefore suggest publication after addressing these comments.
Kind regards, Paul Bons