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.

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.

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?

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