tc-2016-243 – Reply to RC1

The manuscript "Regional modeling of the Shirase Drainage Basin, East Antarctica: Full-Stokes vs. shallow-ice dynamics" by H. Seddik et al. presents simulations of the Shirase Drainage Basin …

We wish to thank the referee for his/her efforts, even though we largely disagree with the assessment. For details please see below.

I am concerned about the novelty of this paper. As stated by the authors in the paper, the Shirase glacier is "one of the fastest flowing glaciers in Antarctica" and its flow is "dominated by sliding". However, the authors also explain that the shallow-ice approximation "assumes that grounded ice flow is governed only by ice pressure and the vertical shear". Using the shallow-ice approximation for modeling such a glacier is not valid here is therefore absolutely no reason to compare full-Stokes and shallow-ice simulations for this glacier.

Note that we have not only modelled the Shirase Glacier, but the entire drainage basin all the way up to Dome Fuji (see our Fig. 1). This is a huge area of ~ 200,000 km^2, large enough to qualify as a stand-alone ice sheet if it were not connected to an even bigger ice body. In most of it, "normal", slow ice flow prevails. The regions where it is a priori clear that the shallow-ice approximation (SIA) is problematic constitute only a very small part of the drainage basin (see, e.g., our Fig. 10). Therefore, we strongly disagree with the referee's statement that using the SIA for modeling this region is not valid from the outset.

The shallow-ice approximation has been developed 30 years ago and has been extensively used …

Not everything that is old is bad! For example, Newtonian mechanics has been around for 300 years, and, even though we learned about its limitations over time, it is still a very useful tool and being used extensively. The negative ring this half-sentence conveys is therefore not appropriate.

… but is known to be valid only on slow moving areas where the motion is dominated by vertical shear. Fast flowing glaciers are dominated by basal sliding and lateral shear cannot be neglected as it provides significant resistance to the flow. It was therefore expected that the shallow-ice approximation would not perform well compared to full-Stokes on this glacier.

No, this is not correct. We would agree if we only modelled the fast-flowing Shirase Glacier itself, but, as stated above, this is not the case. Our modelling study concerns a significant part of the Antarctic ice sheet, in which the majority of the ice flows rather slowly and exhibits a slip ratio of < 0.5 (that is, the amount of basal sliding is limited; confirmed by our Figs. 8 and 9). Under such conditions, we think that it is an interesting and valid test to check how the SIA performs compared to full Stokes (FS).

The great strength of SIA is its simplicity and enormous computational efficiency. Even in connection with the shelfy stream approximation as hybrid models, SIA is often the only viable alternative in terms of computing resources for applications covering large spatial and temporal scales, such as paleoclimatic runs of an entire ice sheet. Showing by a case study, like ours for the Shirase drainage basin, where exactly the deviations of SIA results from FS exceed an acceptable margin is something that adds valuable information to potentially existing simulation challenges.

The conclusion of this paper suggesting that “careful consideration must be given to the representation of ice flow physics when attempting to model the dynamics and evolution of ice sheet areas containing ice streams and outlet glaciers” is not novel.

We are fully aware that this particular conclusion is not new, but rather a confirmation of previous findings. That’s why we started the sentence with “This confirms that…” (page 1, line 10). In order to make it even clearer, we can change it to something like “This confirms findings of earlier studies that…”, or delete the sentence altogether.

Comparing different ice flow approximations is not new, and has been studied for at least a decade, on a number of idealized geometries (Hindmars,, 2004; Gudmundsson 2008) and real glaciers (Morlighem et al., 2010; Seddik et al., 2012; Furst et al., 2013), so the domain of validity of the different stress balance approximations is well known and there is nothing new added in this paper.

We did not wish to imply that our paper is the first ever in which different force balances have been compared, and we apologize for not having mentioned more of these earlier studies in the introduction (so far, we only cite Morlighem et al. (2010)). This can be fixed in a revised version. However, the statement that “there is nothing new added in this paper” is definitively not true. As the referee points out, Hindmarsh (2004) and Gudmundsson (2008) dealt only with idealized geometries. Seddik et al. (2012) compared FS and SIA for SeaRISE-Greenland scenarios, but used two different models, namely Elmer/Ice for FS and SICOPOLIS for SIA, which limits the comparability (because differences can also arise from different numerics etc.). Fürst et al. (2013), also a study on the entire Greenland ice sheet, used only one model, but the five approximations to the force balance are between (and including) SIA and Blatter–Pattyn (aka first-order approximation), thus not including FS.

The study by Morlighem et al. (2010) is probably the most similar one to ours because it dealt with a part of Antarctica, used different force balances within one model and a control method to infer basal drag. However, there are still major differences. It dealt with a much smaller area, namely the Pine Island Glacier and its immediate vicinity. Therefore, the characteristics of their domain is very different from ours in that it contains a much larger fraction of fast-flowing ice. For such a domain, the SIA would be clearly inappropriate. Consequently the authors didn’t use it and compared FS, Blatter–Pattyn and the shelfy stream approximation. Further, the study only investigated present-day stress and velocity fields, while we also discuss time-dependent future climate scenarios.

In short, our study is (to our best knowledge) the first in which FS and SIA are compared within one model for an application to a large area that has the characteristics of an entire ice sheet. We think that this is sufficiently novel to make our paper a valuable contribution.

Finally, the simulations performed in this manuscript rely on the Elmer/Ice software, that was recently used to develop a dynamical coupling between full-Stokes and the shallow-ice approximation (Ahlkrona et al., 2016). Applying this new coupling method to the Shirase Glacier and comparing its performance and accuracy to a more traditional full-Stokes model would have been of greater interest for this study.

Frankly, we think that such a suggestion exceeds the role of a referee (which is to evaluate a given paper, rather than suggesting something more or less completely different). Anyway, we hope that our above arguments are sufficient to explain why we did what we did, and why we think that our study is interesting for the community.