Review of a manuscript "Surface undulations of Antarctic ice streams tightly controlled by bedrock topography" by J. De Rydt, G. H. Gudmundsson, H. F. J. Corr and P. Christoffersen.

General comments

This study aims to verify a theory describing the effects of the basal topographic features on ice-stream flow and its surface topography, using observations from the Rutford Ice Stream and Evans Ice Stream. Analyzing radar profiles collected along two ice streams, the authors found a "qualitative" agreement between amplitudes of the surface undulations observed along radar profiles collected on the ice streams with the amplitudes computed using the transfer theory. The major drawback of the analysis (which is stated in the manuscript) is the flowline geometry in which it is performed, i.e. the omission of the variability in the direction transverse to the ice flow direction. This drawback is caused by the limitation of the available data, and not by the quality of the analysis itself. However, this limitation impacts the drawn conclusions. In my view, some of them are overstated. Another major point that needs to be addressed is the negligence of the effects of the ice streams' lateral confinement. As figure 2 shows, the majority or radar profiles has been collected on tributaries of the Evans Ice Steam that appear fairly narrow, similar to the narrow surveyed parts of the Rutford Ice Stream (please see another comment about this figure below). However, the transfer function theory (Gudmundsson, 2003) has been developed for unconfined ice flow. It is reasonable to expect that ice flow close to the ice streams' lateral boundaries is not the same as flow along the centerline of the ice streams, and the effects of the lateral boundaries are imprinted in the collected radar profiles (e.g. R1, R4, E4, E6). Most likely, the treatment of these profiles has to be adjusted to reflect their closeness to the lateral boundaries.

Specific comments

The first sentence of the abstract is misleading. This statement is true in the case of the linear rheology. It still remains to be seen, however, whether it is true in the 3D case of the non-linear rheology. The numerical study investigating the effects of the non-linear rheology by Raymond and Gudmundsson (2005), has been done in the 2D (flowband) setting, though the 3D effects play a significant role. Another statement that basal slipperiness has no effects on the local variations in ice flow is overstated as well. At least, it cannot be drawn from the results of this analysis. There are several reasons for that. First is that the spatial variations in the slip ratio have a quantitatively similar effect on the shape of surface undulations as the basal undulations (Gudmundsson, 2003). Thus, the surface undulations represent a cumulative response of ice flow to the topographic features and slipperiness of the bed. By the way, this might be a reason for multiple peaks in the observed spectrum. Second, although the authors state that the actual value of slip ratio does not affect the results (lines 2-6, p. 4496), the theoretical transfer function has a very strong, nonlinear dependence on it. Since the authors use a constant value for each profile, its value might not reflect the dynamics of the whole ice stream. The concluding statement (lines 21 and onward, p. 4506) sounds like an unwarranted criticism of inversion studies that use forward models based on the Shallow Shelf Approximation. This approximation has been derived based on the small aspect ratio (H/L), and is valid in circumstances beyond the limits of 1/20 aspect ratio mentioned in this study. The more precise point is that the inversion results cannot be considered

on spatial scales smaller than the spatial scales for which SSA is valid, rather than the SSA is unable to adequately simulate the effects of the topographic features with short wavelengths (it cannot do that by design). It is surprising not to see a discussion of the phase shift between the bottom and surface undulations (Gudmundsson, 2003). It might be possible that it has an effect on the discrepancies in the analysis.
With respect to the manuscript presentation, my suggestion would be either to remove Section 2 or to substantially shorten it. The material presented there, appeared more than once in other papers, so there is no need to repeat it again. Similarly, the introduction section can be shortened. There is a fairly good general understanding how ice streams work, so repeating the basic ideas is unnecessary. There are no plots showing the bed and surface profiles used in the analysis. It would be interesting to see how they change along the ice streams, and how much ice thickness changes along these profiles. The fact that the ice thickness is not constant along the ice streams can be another possible source of errors. From the data description it is unclear how the surface elevation profiles are derived. Do they come from the radar data? What is the spatial resolution along the profile and how does it compare to the ice velocity resolution (900 m)? It is also worth mentioning what is the distance between the radar profiles on the same ice stream, it is difficult to get this information from figure 2, which axis labeling is confusing (10^3 km on the horizontal and 10^2 km on the vertical axes).

To summarize, this study presents an interesting analysis, however, its flowline approach is too restrictive, and the presented conclusions need to be adjusted to reflect that. The point of disregarding the effects of the ice streams' lateral confinement needs to be addressed before the manuscript can be published.