

Interactive comment on “Surface undulations of Antarctic ice streams tightly controlled by bedrock topography” by J. De Rydt et al.

J. De Rydt et al.

janryd69@bas.ac.uk

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1. Re *“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 Stream 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*

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adjusted to reflect their closeness to the lateral boundaries.”

We agree that all models suffer from a series of shortcomings at various levels of importance, including the negligence of side drag. However, the main aim of our work is to test the simple qualitative properties of the local surface response, in particular the enhanced transfer of information about local bedrock undulations to the surface. Discarded effects such as side drag (and several others mentioned in Sect. 5) have an impact on the amplitude and wavelength of the peak amplitude, but do not alter the fundamental characteristics of the transfer function. In particular the inclusion of a lateral drag parameterization will have an effect on the mean flow state, decreasing the mean slipperiness $C^{(0)}$. A remark in this respect will be added to the discussion in section 5.

2. Re *“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.”*

The work of Raymond and Gudmundsson (2005) is indeed done in a flowline setting, and a 3D treatment of transfer functions for a non-linear rheology has not been published. However we think such a treatment is beyond the scope of the present work, and the abstract will be modified to reflect this point.

3. Re *“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*

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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.”

The statement that “spatial variations in the slip ratio have a quantitatively similar effect on the shape of surface undulations as basal undulations” is incorrect. The important differences are summarized in Figures 1 and 2 of (Gudmundsson, 2003), where the flow over a Gaussian bedrock and slipperiness perturbation is simulated. Generally speaking, the response is much larger in the former case, intuitively because the ice has to physically flow over and around the obstacle. This results in a more localized response and horizontal and vertical speeds that are generally an order of magnitude larger than for the basal slipperiness perturbation. In the latter case, the response tends to spread out more, changes in velocities are lower, and as a result, the surface response is much less pronounced. In terms of transfer functions from the bed to the surface, this translates into an amplitude which is much reduced for basal slipperiness perturbations as compared to bedrock perturbations, intuitively explaining the absence of a maximum in the former case.

4. Re “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.”

Most profiles (C1-C5 and E2, E5 and E6 being exceptions) show a significant large-scale variability in surface speed, with speeds gradually increasing towards the grounding line. As a result, the mean slip ratio varies significantly along most profiles, and the use of one mean value along the entire profile is indeed question-

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able. However, the main aim of our work is to show that for high enough values of the slip ratio (i.e., $C^{(0)} \gg 1$), the transfer function develops a local maximum, as predicted by theory. Despite the large variability of $C^{(0)}$ along individual radar profiles, its value is always significantly larger than unity along the entire length of the fast flowing Rutford and Evans ice streams, and hence a local maximum is expected. This is confirmed by the observations. The nonlinear dependence of the transfer function on the mean slip ratio has been addressed in Fig. 4, in particular through the splitting of profiles E10-E12 into an upper and lower sections. It was found that the slower flowing upper parts have a significantly reduced transfer as compared to the fast-flowing region closer to the grounding line.

5. Re *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).*

With this remark we wanted to emphasize exactly what the reviewer points out, namely that SSA models cannot be used to obtain reliable information about the bed for wavelengths less than $20H$. This part of the conclusions will be reformulated to avoid misunderstanding.

6. Re “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

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an effect on the discrepancies in the analysis.”

The phase shifts have been considered, but the results were inconclusive and have hence not been incorporated in this study. A remark in this respect will be added to the manuscript.

7. Re *“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.”*

We find it appropriate to give a concise introduction to transfer functions for readers that are less familiar with this concept. In addition it makes the manuscript self-contained, as section 2 also introduces the notations that are used throughout the remainder of the manuscript, and it provides a description of the transfer characteristics that are important for comparison with observations later on. The introduction will be shortened and paragraphs on p4486 and p4487 will be made more concise.

8. Re *“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.”*

A plot with the radar profiles for the bed and surface will be added to the manuscript (see Figure 1 included here). We also depict the plane slab approximations used in this study by dashed blue lines. It can be seen that the mean

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state with constant mean ice thickness along the entire profile is a reasonable approximation for all profiles. The mean amplitude of the deviations Δ_s is smaller than $0.01H$ and the mean amplitude of Δ_b lies between $0.035H$ and $0.24H$. Locally Δ_b can be up to $0.6H$ for E10, and $0.7H$ for E11 and E12, but only for a small fraction of the profile length.

9. Re *“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)?*

The surface data was acquired during the radar mission described in Sec. 3.1. The typical spatial distance between successive radar points is 20m, the surface velocity field is sampled on a 1km by 1km grid. The surface velocity at each point along the radar track is chosen to be the value of the nearest point on the velocity grid. This information will be added to the manuscript

10. Re *“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 (103 km on the horizontal and 102 km on the vertical axes).”*

The typical distance between radar profiles ranges from about 5km to 10km. A new Fig. 2 will be produced which has equal units on the x and y axis.

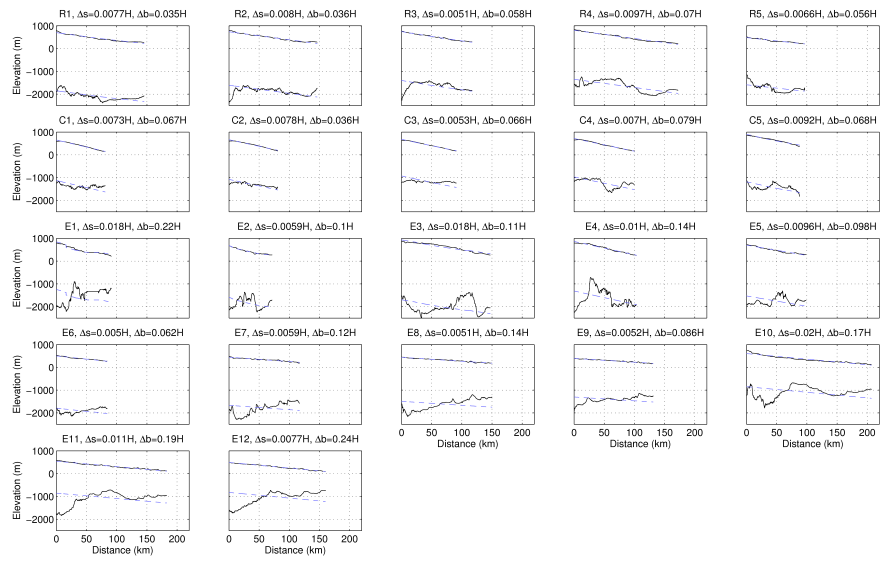


Fig. 1. Measured bed and surface radar profiles for all section in this study. The dashed blue lines determine the plane slab approximations.