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The Cryosphere Discussions

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

Anonymous Referee #1

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This paper tests previous theoretical results on transmission of bed topographic variations to ice stream surfaces, using a considerable body of airborne radio echo sounding data along 3 S.W. Filchner-Ronne ice streams acquired in January 2007. The main aims are to test (i) if transmission increases with slip ratio, and (ii) if transfer amplitudes have a maximum for wavelengths in the range of several to $\sim$20 x ice thickness, as predicted by the theory. The results (Fig. 3 and 4) are intriguing, and emphatically support the main aspects of the theory. The paper goes on to discuss the comparatively minor discrepancies from the theory, and evaluates a number of plausible reasons for them, testing some of them with additional sensitivity analysis. I lack the experience to critically evaluate the data processing or spectral methods used to generate Fig. 3, but a number of references on them are given. The main source of error seems to be the estimation of internal deformation, which is treated well with error bars shown in Fig.

4. The paper is well written and clear throughout, and the results will be of particular importance in the field of inversion techniques for basal properties beneath ice bodies. The paper could benefit slightly from the minor points and suggestions below, but in my opinion it is already nearly suitable for The Cryosphere (assuming point #1 is a non-issue).

Specific Points:

1. The planar mean state with uniform velocity and thickness (section 3.2) neglects surface accumulation, that would require velocity and/or thickness to increase downstream to be in steady state. This is more likely to be a problem for the slowest (Carlson) ice stream. Assuming annual accumulation of 0.3 m/yr ice equivalent, mean velocity $\sim$30 m/yr, and profile length $\sim$100 km (pg. 4494, lines 17-22), that implies an accumulation of 1 km ice for a parcel along the entire profile, which is significant compared to the actual thickness of $\sim$2 km. Perhaps a few lines of text could be added on why this point is not serious - perhaps because the theory is still valid for a slowly-varying (longitudinally) steady state, or because it is addressed by the split between upper and lower sections of E10-12 in Fig. 4 (?)

2. Is it possible to provide a qualitative, intuitive explanation of the basic theoretical results - particularly why bed topographic variations are transmitted more readily than slipperiness variations at these wavelengths? This paper and Gudmundsson (2003) provide a few sentences in this vein, but little discussion beyond mathematical properties of the equations and solutions.

3. Perhaps show a few actual surface and bed profiles, to give readers a more concrete sense of the data, if only to illustrate the need for sophisticated spectral processing to tease out the relationships. Also this could show how justified the use of uniform slab geometry is as a mean state.

4. Similarly, perhaps a few illustrative power spectra could be shown, for both bed and surface topography. In particular, this could help to clarify the only unclear section

of the paper, in section 5.2. There, the possibility is discussed of a spectral range in bed topography with low power (Fig. 6a). But the text seems ambivalent on whether that is due to a real absence of these wavelengths, or a processing artifact caused by the limited total length of the profile. If the former, then: (i) are there physical erosive mechanisms that could preferentially smooth certain wavelengths, and (ii) why would lower basal power cause a drop-off in the transfer function for those wavelengths? Also, does Fig. 6 illustrate the former or the latter?

5. Could real basal slipperiness conceivably be spatially correlated with bedrock undulations? Perhaps by deformable sediment accumulating preferentially in bed depressions, and being absent on bedrock highs? If so, would that be problematic for the conclusions made here, and for aspects of the theory that distinguish between slipperiness and undulations (e.g., pg. 4506, lines 25-28)?.

6. In Fig. 3, a few of the panels (E9 to E12) seem to have maximum theoretical curve values of $\sim$0.3, definitely greater than 0.25. But if these have been re-scaled by a constant factor 0.25 (pg. 4497, line 16), then their pre-scaled values would be $\sim$1.2, greater than 1, which presumably is impossible for transfer amplitudes. Or maybe not? A brief explanation would help.

Technical Points:

Pg. 4491, line 25: where Fig. 1 is first mentioned, and/or in the figure caption, mention that these results are from the theory in Gudmundsson (2003) (somewhat repeating line 14).

Pg. 4496, lines 10-13: One or two additional references would help as resources for these spectral processing terms, and the techniques used here (unless it is all contained in the 3 references given on lines 26-28).

Pg. 4497, line 13 et seq. "Three-dimensional effects" are first mentioned here as possible causes of the subdued observed transfer amplitudes. But they remain mysterious,

until described in section 5.1.1. Just one line could be added here stating what they are, and referring to the later section.

Pg. 4502, lines 1-2. The theoretical test in Fig. 5 addressing 3-D effects is well taken. But the phrase "has the potential to explain" seems a little optimistic, because the suppression in transfer amplitude for even a 45-degree departure is $\sim$40%, and is only 26% for a 30-degree departure which still seems large for flowline-minus-flightline discrepancies. This compares to the 75% suppression (f=0.25) seen in Fig. 3.

Pg. 4505, lines 12-13. The phrase "increases at a rate of $\sim$350% ..." is slightly unclear, because there is no real "rate". Perhaps replace with "increases by a factor of $\sim$3.5 as slip ratios increase from $\sim$1 to several thousand".

Pg. 4505, line 24: In "systematically reduced by a factor of 60% or more", why not use the optimal least-square fit value of 75% ? (i.e., f=0.25, Fig. 3 caption).

Fig. 4 caption: Why is alpha set to .003 rad? .003 is mentioned earlier as a typical active ice stream value, but is it the (average?) value used for the mean states of these 3 particular ice streams?

Interactive comment on The Cryosphere Discuss., 6, 4485, 2012.