

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

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This paper presents new data obtained along approximate flowlines in the Ronne Filchner sector of the West Antarctic Ice Sheet (WAIS). The ice surface is shown to exhibit a stronger response to bedrock features in regions with high slip ratio (i.e. ice streams, especially approaching grounding lines). Transfer functions relating surface features to bedrock irregularities are calculated and compared to theoretical transfer functions predicted by a full Stokes ice dynamical model. Qualitative agreement between the theoretical and observational transfer functions is demonstrated. Possible reasons for quantitative disagreement are well discussed although a precise quantification of the observational-theoretical discrepancy can not yet be given.

The paper is very clearly written and well laid out. The results are useful and interesting

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in their own right, though it would be interesting to see discussion on the possible relevance of this work to ice sheet mass balance taken a little further. I recommend the paper for publication with minor modifications.

Given the high quality of the text and figures in this initial submission my comments below should be mostly considered as suggestions rather than corrections as such.

Specific comments:

On more than one occasion the phrase “independent on” is used. I think it is more common to say “independent of”?

In more than one place the word “whith” needs to be corrected for spelling to “with”

Maybe I missed it, but I didn’t find a justification for saying that variations in C (normalised slipperiness) should be $\ll 1$. Justification for ranges of $C(0)$ are given on page 4495. In the abstract (last line) it is claimed that variations in basal slipperiness are not relevant at the scales discussed, but this is only a robust statement if you have justified that your range of values for slipperiness variations cover the full likely range.

Page 4487, lines 21-24. I would think that back stress due to shelf buttressing would count as an “other resistive stress” that could undergo rapid dynamic change due to shelf thinning or large calving events.

Page 4490, lines 1-5. Expressing a quantity in terms of deviations around a mean isn’t an assumption. The key assumption is in the following paragraph, that the deviations are small. Perhaps clearer to say something like “we express bed topography (b) [etc] in terms of deviations around a mean value” and the following paragraph would be something like “The perturbative study [etc] relies on the assumption that the deviations from the mean values are small. . .”

Page 4490, lines 8 and 9. Maybe a good idea here to reference the later section of the paper where you justify some of these assumptions using observations?

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Page 4491 equation 4. If the theoretical transfer functions are not to be described here it would be good for them to be precisely referenced. I think equations in 19 and 20 in Gudmundsson 2003 are the relevant equations?

Page 4494 line 10. I'm not sure that an amplitude can have a qualitative behaviour. Perhaps better say "...not influence the qualitative aspects of the relationship between transfer amplitude and λ_x/H "

Page 4496, line 9. Can you clarify what is meant by detrended? Did you remove a linear trend?

Page 4496, lines 10-13. There are concepts here that might not be familiar to all glaciologists. In particular it is not intuitively obvious to me that the transfer amplitudes can be calculated as the quotient of the mentioned power spectral densities. I am not suggesting this is wrong, simply that a few more lines of explanation and or a reference would be helpful to glaciologists who want to understand this method but who are not so familiar as the authors with this approach.

Page 4497, line 17. The physical origin of the scaling factor has not yet been made clear at this point. I suggest adding a reference to section 5.1.1 which goes into this in more detail.

Page 4499, lines 25-26. If one wanted to be really pedantic it should be noted that "data" is plural for "datum" and here it used in the singular.

Page 4505, line 12. 350% is not a "rate". Maybe it would be better to say "increases by ~350% over the range of slip ratio values used here"

Conclusions/discussion. It is noted that the qualitative agreement between modelled and observed expression of bedrock irregularities in upper surface features increases confidence in the model (at least for the case of the full Stokes model). But the more pressing issues relate to model ability to simulate changes in large scale ice dynamics and ice sheet mass balance. The inability of a model to agree with observations re-

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garding expression of bedrock irregularities at the surface doesn't prove that the model is not capable of giving useful predictions of ice sheet mass balance. Conversely, a good agreement between model and observations in this respect doesn't prove that the model will be a good simulator of ice sheet mass balance. Can the authors say anything about the relevance of the model validation provided by this study to large scale model based predictions? I realise this is a difficult question, and personally I do not know how I would answer it. But there is no harm in asking difficult questions!

Fig 5 b) the legend box needs to be slightly bigger.

Fig 6. I think it would be nice to see one of the actual profiles (2 or 3) used here and also one of the observed profiles somewhere.

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

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