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Interactive comment on "Subglacial hydrology indicates a major shift in dynamics of the West Antarctic Ross Ice Streams within the next two centuries" *by* S. Goeller et al.

Anonymous Referee #2

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This manuscript presents results of a simple model which uses data constraining ice sheet geometry in the region draining the West Antarctic ice sheet into Ross Ice Shelf to estimate subglacial water routing and subglacial drainage basins. The authors assume that the drainage area is a proxy of subglacial water flux by assuming that the basal melting rate is spatially uniform in the study area, in spite of prior modeling results inconsistent with such an assumption (Beem et al., 2010; Joughin et al., 2003). Finally, they use the modern rates of ice surface elevation changes to estimate evolution of subglacial water routing and subglacial drainage basins 200 yeears into the future.

The methodology used in this study is simplistic compared to the current state-of-the-art in modeling ice stream evolution (Wel et al., 2013) to the point that it is hard to see

that this manuscript is actually contributing much new to the scientific understanding of Antarctic ice stream evolution. For instance, within the next 200 years Kamb Ice Stream may restart (which would switch it from rapid thickening to rapid thinning) and Whillans Ice Stream may completely stop. The simplistic assumptions made by authors would not allow an ice stream to turn on and off because a stopped ice stream will thicken and permanently drive water away from its bed towards neighboring ice streams. What would restart an ice stream in the logic of their model? Similarly, an active ice stream should be drawing down its surface elevation and 'attracting' water towards its trunk from upstream and surrounding regions.

Beyond problems with methodology, the manuscript is burdened by problems with execution, particularly with citations. The citations are mostly outdated and often their use suggests that the authors either did not read them (beyond reading the abstract) or did not understand them. On a number of occasions the authors invoke data or interpretations that have been cited by somebody else instead of going to the original papers (e.g., citing Jacobel et al. 2009 as if they would drill to the bed of Kamb ice stream).

Specific comments:

Lines 55 through 65 - Have the authors adjusted the numbers published by Retzlaff and Bentley for the fact that it has been a quarter of a century since these measurements were made?

Lines 75 through 85 - InSAR measurements made by Scheuchl et al. (2012) showing 25% of slowdown between 1997 and 2009 cannot 'confirm' the velocity loss of 23% between 1974 and 97 reported by Joughin et al., (2002). First of all, no temporal overlap between these two data sets means no 'confirmation' is possible. These are two independent and complementary data sets. Yes, the absolute magnitudes of deceleration for both time periods are similar (23% and 25%) but the lengths of the time periods are very different (25 years versus 12 years). So, in fact the deceleration rate has increased by a factor of two between the two periods. The long-term variability of

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ice flow rates on Whillans Ice Stream has been recently documented and extensively discussed by Beem et al. (2014) who should be cited here.

Lines 89 through 94 - This sentence is not well written. Furthermore, there is a logical contradiction here. When one talks about dynamic behavior of ice streams, this implies discussion of temporal ice flow variability. However, the second part of this sentence talks about ice stream location, which pertains to spatial distribution of ice streams rather than to their temporal dynamics. This sentence needs to be fixed.

Line 96 - Use 'develop' instead of 'evolve' in this sentence.

Line 97 - I think that 'numerous' is a bit of an exaggeration that is not needed to make the point that the authors are making. Either provide a number of citations to support this claim or re-phrase the sentence just to say that till has been found beneath these ice streams.

Lines 100 through 102 - This makes it seem like the whole ice stream has been shown to be underlain by a till layer with these characteristics. In fact, it's just in a limited area where the seismic survey used by Rooney et al. (1987) and Alley et al. (1986) has been conducted.

Lines 100 through 115 - Much of this discussion is dated and supported by old references. The authors should read more recent papers, not just those published in 1980s and 1990s.

Lines 110 through 115 - This appears to be another unnecessary over interpretation that the authors do not need to include to support their line of argumentation.

Lines 115 through 120 - The paper of Studinger et al. (2001) would be the best way to point out that there are widespread sedimentary basins in the region that provide a geologic template for these ice streams.

Lines 120 through 127 - Again, the authors are failing to cite more relevant and more recent papers. The term 'till delta' is not preferred anymore (use 'grounding zone wedge'

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instead). It's surprising that the authors are citing Anandakrishnan et al. (2007) to talk about stabilization of the grounding line rather than Alley et al. (2007), which was the paper on this subject. Instead, they cite Alley et al. (1989), which basically is a less up-to-date version of Alley et al. (2007). I'm starting to get the sense that the authors may not be actually reading the papers that they are citing just either reading their abstracts or skipping through them. Why otherwise would they cite what they cite? In addition, they refer to Alley et al. (1989) discovering a till delta at the grounding line of Whillans Ice Stream whereas this paper was more of a theoretical consideration of what should be happening under a soft-bedded ice stream.

Lines 129 through 132 - The relationship between subglacial water drainage and ice stream is a case of two-way coupling. Ice streams draw down ice surface and water flows then towards ice streams from upstream regions. The statement that the authors make implies that somehow there is only a one way coupling, so that water flows wherever it chooses and then ice streams form there.

Line 135 - Actually, the paper of Alley et al. (1986) is not based on radar data. The only place where radar data are mentioned in this paper is in their abstract, where they refer to Robin et al. (1970). This just serves to re-affirm my suspicion that the authors are actually not reading the papers they cite. The statements they make in the next two sentences after these 'radar' citations are not supported by any 'radar' data in Alley et al. (1986).

Lines 143 through 146 - This statement has been made before already in this manuscript and represents a repetition.

Lines 169 through 172 - The authors missed a recent observational constraint on basal melt rate beneath the lower part of Whillans Ice Stream in Fisher et al. (2015)

Lines 172 through 175 - This citation of the results from Beem et al. (2010) is incorrect. Beem and others reported that the basal melt rate is 20-50 mm/yr beneath shear margins and their range of 3-7 mm/yr is averaged across ice stream width. In fact,

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there is no 'contrast' between their results and those of Joughin et al. (2003). The two models agree, except in shear margins where Beem et al. use a method that treats shear margin dynamics at a higher resolution.

Lines 188 through 199 - The assumption of a spatially uniform basal melt rate is not justified by the data. This assumption arises from the authors' flawed interpretation of the results of Beem et al. (2010). In reality, these past modeling results are very much inconsistent with this assumption. Calculated basal melt rates vary by at least an order of magnitude. The assumption made by authors basically pre-determines the later result they get.

Equation 1 makes no sense to me. I see no difference between the variable 'p' and 'pw'. I presume that this is a confused interpretation of Equation 4 from Shreve 1972.

The discussion above equation 2 - It is not as obvious as the authors would like to make it appear that subglacial water pressure can be taken to be equal to the ice overburden pressure. Yes, effective stresses tend to be small but they are within the range of pressure differences that can significantly shift the direction of subglacial water flow (see Carter et al., 2013). Between the errors in ice surface slope, ice thickness, the average density of an ice column, and the assumption that effective stress is zero any routing of subglacial water can be quite uncertain. The series of recent papers by Carter et al. should be cited here since they represent the state-of-the-art in hydrological routing models for this study region.

Lines 257 through 259 - Again, one could argue as well that ice streams control where subglacial water flows because they draw down the ice thickness and ice surface elevation, thereby creating areas of low subglacial water pressure towards which water flows from upstream areas.

Lines 265 through 268 - This is an artifact of their assumption that there is spatially uniform melt rate in the whole study area.

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Lines 278 through 280 - Jacobel et al. (2009) did not drill any boreholes to the bed of the sticky spot. Engelhard (2004) did report on the boreholes drilled by him and colleagues. And Jacobel et al. (2009) cite this work. The authors should read Jacobel et al. carefully and refer to the original publication.

Lines 317 through 319 - This is, again, the artifact of the assumption that there is a uniform basal melt rate throughout the study region.

Section 3.3 - The assumption of constant ice surface elevation changes into the future is a pretty significant one. The authors should at least discuss why this simplifying assumption may fail and what would be the consequences. The complete lack of ice dynamics and ice-water flow coupling in their model really makes it difficult to have much trust in the results. The level of sophistication in numerical ice stream models is increasing and I am not sure that there is still scientific value in doing simple extrapolations from the modern state (compare this manuscript to Wel et al., 2013).

Alley, Richard B., Sridhar Anandakrishnan, Todd K. Dupont, Byron R. Parizek, and David Pollard. "Effect of sedimentation on ice-sheet grounding-line stability." Science 315, no. 5820 (2007): 1838-1841.

Beem, L. H., S. M. Tulaczyk, M. A. King, M. Bougamont, H. A. Fricker, and P. Christoffersen. "Variable deceleration of Whillans Ice Stream, West Antarctica." Journal of Geophysical Research: Earth Surface 119, no. 2 (2014): 212-224.

Carter, Sasha P., Helen A. Fricker, and Matthew R. Siegfried. "Evidence of rapid subglacial water piracy under Whillans Ice Stream, West Antarctica." Journal of Glaciology 59, no. 218 (2013): 1147-1162.

Engelhardt, Hermann. "Thermal regime and dynamics of the West Antarctic ice sheet." Annals of Glaciology 39, no. 1 (2004): 85-92.

Fisher, Andrew T., Kenneth D. Mankoff, Slawek M. Tulaczyk, Scott W. Tyler, and Neil Foley. "High geothermal heat flux measured below the West Antarctic Ice Sheet."

Science Advances 1, no. 6 (2015): e1500093.

Studinger, Michael, Robin E. Bell, Donald D. Blankenship, Carol A. Finn, Robert A. Arko, David L. Morse, and Ian Joughin. "Subglacial sediments: a regional geological template for ice flow in West Antarctica." Geophysical Research Letters 28, no. 18 (2001): 3493-3496.

Wel, Narelle, Poul Christoffersen, and Marion Bougamont. "The influence of subglacial hydrology on the flow of Kamb Ice Stream, West Antarctica." Journal of Geophysical Research: Earth Surface 118, no. 1 (2013): 97-110.

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