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Interactive comment on "Numerical simulations of the Cordilleran ice sheet through the last glacial cycle" *by* J. Seguinot et al.

S.J. Marshall (Referee)

shawn.marshall@ucalgary.ca

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Review of J. Seguinot et al., Numerical simulations of the Cordilleran ice sheet through the last glacial cycle

Summary Comments

Seguinot and colleagues provide the first detailed glaciological modelling of that I am aware of for the Cordilleran Ice Sheet in western North America, making this a novel and long overdue contribution. The authors have not only made new advances with this contribution, they have done so in an impressive leap forward. This is an excellent and carefully-presented study which is likely to rejuvenate interest and debate in Cordilleran Ice Sheet reconstructions. The balance between numerical modelling and

glacial geological/geomorphological considerations is unusually strong, and the authors can be commended for this emphasis. This adds tremendous value to the results and increases confidence in the modelling, and I also appreciate that the authors point out areas where the numerical model is not in accord with the geological record.

The manuscript is well-written and beautifully illustrated, and I have very few substantive comments. The choices made by the authors are logical and well-explained, and they reach several well-substantiated conclusions: a two-phase Cordilleran glaciation, a reasonably robust estimate of CIS volume at LGM, the general model of CIS growth through multiple alpine icefields, and the importance of the Skeena Mountain inception centre. One can always quibble with specific aspects of the model design and climate scenarios, but the authors have explored a reasonable span of 'solution space' and these aspects of the Cordilleran ice sheet history appear to be robust features of the simulations.

The modelling strategy and results presented here stand to be widely cited, and I expect that it will serve as a springboard for additional studies from others in the international community. I recommend this manuscript for publication in The Cryosphere without reservations.

Specific Comments

The Cordilleran Ice Sheet is difficult to model due to its complex topography and multiple inception centres (and possibly multiple domes/divides), strong regional climatic gradients, which require relatively high-resolution climate input fields, and a dearth of paleoclimate proxies for western North America to inform spatial and temporal variations in climate conditions during the glacial period. The authors confront these challenges well, with a adequate ice sheet model resolution (5 to 10 km) and ice physics, carefully calibrated 'control' climatology (published in Seguinot et al., 2014), and a good exploration of different paleoclimate time series histories in this contribution.

Nevertheless, it is not clear that ice-core based paleoclimate proxies from Greenland

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or Antarctica are appropriate for western North America. This may be particularly true of Greenland proxies, where the amplitude of D-O (millennial) climate variations is exceptionally strong and is likely to be regional. Because these remote ice core records are 'scaled' based on only one constraint, producing a CIS maximum configuration that resembles the geological record, it is difficult to assess the pre-LGM simulations or the details of the modelled ice divide structure, ice thickness, etc. The robustness of the conclusion that Greenland and Antarctic ice core records are good proxies of glacial climate variability in western North America is therefore not so clear, but it is admittedly hard to do better at this time. I do wonder if there is any hope from more regional climate proxies such as the Logan ice cores or the off-shore Vancouver Island sediment records that are cited from Cosma et al. This is worth a short discussion.

Similarly it is difficult to know the errors and uncertainties associated with the assumption of fixed modern-day spatial patterns for temperature and precipitation. I suspect that the sensitivity of this assumption far exceeds that associated with the different paleoclimate proxies. Such that, for example, one could readily imagine different assumptions, such as a maritime effect that gives reduced glacial cooling near the coast vs. in the interior, that is a stronger effect than the difference between different paleoclimate proxies with respect to the timing of LGM, ice divide structure, etc. But this is a very reasonable start, what the authors have done – there is always going to be more parameter space to explore in future studies. As above, I would perhaps just suggest a small discussion of the authors' opinion on this question, the uncertainty or possible influence of this assumption of modern-day climate patterns.

Several minor points and grammatical corrections are included in the attached text. Nothing that will require much thought – this is a really impressive piece of research, overall, and I am hard-pressed to find any criticism of it. It is one of the easiest reviews I have ever done. Congratulations to the authors and thanks for this fine work.

Please also note the supplement to this comment:

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http://www.the-cryosphere-discuss.net/9/C1528/2015/tcd-9-C1528-2015-supplement.pdf

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