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Interactive comment on "Regional seesaw between North Atlantic and Nordic Seas during the last glacial abrupt climate events" *by* Mélanie Wary et al.

Anonymous Referee #3

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Wary et al present reconstruction of surface water conditions in the North Atlantic region during stage three, mainly derived from dinocyst assemblages using transfer functions. The results are compared with climate model hosing experiments and show evidence of a inverse relationship between temperatures in the Nordic Seas and the North Atlantic Ocean/Greenland. The manuscript is well written but I there are some issues that need to be addressed before it should be accepted.

When I read a manuscript that uses transfer functions, I like to start with the raw assemblage data. I was disappointed that the mansuscript does not include these, but I found data for two of the four cores examined in Eynaud et al (2002) and partial assemblage data for a further core in Wary et al (2016). Although both of these papers

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Discussion paper

are cited for information about foraminifera assemblages and chronologies, neither is apparently cited for the dinocyst assemblage data. Both these publication also include transfer function derived estimates of sea surface conditions and make similar findings to the present manuscript. The lack of citations to this earlier, overlapping work makes the present manuscript appear more novel than is justified: this must be rectified by citing the authors' previous work appropriately and explicitly stating which parts of the proxy data in the present manuscript are new. The dinocyst stratigraphies that are not already published should be included in the supplementary material.

From the dinocyst assemblages, the manuscript reconstructs summer and winter sea surface temperature and salinities, and sea ice duration using transfer functions. Seasonality is inferred from the difference between summer and winter temperatures. The reported transfer function performances are all impressive, however, these are leave-one-out estimates which, as has been shown repeatedly (Telford 2006; Telford & Birks 2005, 2009, 2011), severely underestimate the true uncertainty in the reconstruction. This is because the environmental variables in the dinocyst calibration set are spatially autocorrelated, violating one of the basic assumptions of transfer functions (Birks et al 2010). There are cross-validation schemes that are more robust to spatial autocorrelation (Trachsel and Telford 2016): performance statistics from these should be used instead.

It is likely that with a robust cross-validation scheme, the transfer function performance statistics will appear worse and some variables will have little or no skill. I suspect that salinity models are the weakest and that it will be difficult to make independent reconstructions of sea ice duration or winter temperature as both have strong non-linear relationships with summer temperature. Without knowing how large the uncertainty is, the reader cannot evaluate how meaningful the stadial-interstadial difference temperature is.

Neither the manuscript nor the precursor papers include any reconstruction diagnostics, such as distance to nearest analogue, which would help the reader evaluate

whether the reconstructions can be relied upon.

The manuscript needs to make the inclusion criteria for the hosing models explicit. Swingedouw et al (2013) includes six models, but only five are used now. The omitted model is BCM2, which has the opposite temperature response in the Nordic Seas to the other models.

The combination of the warm dinocyst-inferred surface temperatures and cold planktic foraminifera inferred sub-surface temperatures in the stadials raise some questions. Firstly, why do sub-polar planktic foraminifera not inhabit the surface layer. Secondly, do the models suggest such a thin surface layer.

Minor points

Tables 2 and S4 claim to present anomalies, but appear to be the actual reconstructions. If they are anomalies, the baseline needs to be specified.

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