



Interactive comment on “Synoptic climate change as a driver of late Quaternary glaciations in the mid-latitudes of the Southern Hemisphere” by H. Rother and J. Shulmeister

H. Rother and J. Shulmeister

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The critical comments by Anderson and Mackintosh on our paper (Rother & Shulmeister, 2005) provide a valuable contribution to the debate and we welcome their input. We would like to respond to the issues raised. Point numbers follow Anderson's and Mackintosh's comment.

1) Anderson and Mackintosh state that our conclusion that substantial glacial advances during the LGIT occurred without evidence for a marked cooling is due to a selective reading of the pollen record. We acknowledge that a few papers do suggest significant cooling during the LGIT but the vast majority of pollen records do not. Selective reading is an odd charge as the main record we cite is Matt McGlone's review of all the then available LGIT pollen records examining the possibility of a late glacial climate reversal (McGlone, 1995). Instead the charge of selective reading could be directed

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at Anderson and Mackintosh. The key site in the Turney et al (2003) paper to which they refer is the Kettlehole Bog record from Cass Basin. McGlone et al (2004) [the same research team but different lead author] work up this record in detail and indeed note a reversal during the LGIT. However, McGlone et al specifically discount a large scale temperature decline as the driving factor. They state “If the glacial advances are seen as a response to a substantial, prolonged cooling during the YDC [Younger Dryas Chron] they cannot be reconciled with the pollen record” (p277, col 2, para 1, lines 16-19). They continue “A late-glacial switch to more humid and persistent westerly winds across southern New Zealand would have increased snow accumulation on the Main Divide and in the spillover zone to the east and in combination with lower summer insolation and generally lower mean annual temperatures, would have kept the potential for glacial advances high” (McGlone et al., 2004 p277, col 2, para 2, lines 8-14). They are in fact invoking the exact conditions we need to trigger our precipitation conversion model - modest cooling and high precipitation. The Newnham and Lowe (2000) record from Kaipo Bog, to which Anderson and Mackintosh refer, comes from the top half of the North Island (some 500 km north of the northernmost Southern Alps snow fields). While it may show a climate change in northern New Zealand during the LGIT, it is unclear how it represents a better record to compare to glacial histories than the Singer et al (1998) paper from tree line and near tree-line sites in a South Island formerly glaciated valley. Furthermore the Cobb Valley region has multiple records extending through the LGIT to the LGM (Shulmeister et al., 2003) and all show a consistent story. Anderson and Mackintosh mention a non-quantified LGIT temperature decline from the speleothem based oxygen isotope record in Williams et al (2005). We do not challenge the presence of an LGIT cooling in this record but we note that Williams et al. also observe a variety of positive isotope excursions during the LGIT and Holocene but they choose not to interpret them. They state “Marked positive excursions occurred at 11.14, 6.47, 3.43 and 0.57 ka, but they cannot be explained simply in terms of temperature because numerous precipitation-related factors that affect $\delta^{18}O_p$ can also have an influence.” (Williams et al., 2005, p311). Finally, Anderson and Mackintosh fail to

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mention other proxy records such as the elegant seawater uranium isotope work of Robinson et al (2004) which explicitly attributes LGIT glacial advances in New Zealand to increased rainfall instead of cooling.

Point 2) We agree entirely with this comment.

Points 3+5) Anderson and Mackintosh reject our interpretation that LGM scale glacial advances in the Southern Alps can occur with only moderate cooling. In their comment they refer to a coupled mass balance - ice flow model for Franz Josef Glacier (FJG) (Anderson & Mackintosh, in press) which they use to suggest that a cooling of 5 degrees C is needed to drive this glacier to its LGIT re-advance position (which is far less extensive than the LGM ice position). We believe that while their model may be useful to describe conditions at FJG it is unlikely that the results apply to conditions in the wider Southern Alps. Topographical constraints of FJG and its névé make it virtually impossible to add new ice accumulation areas with moderate cooling, which is a critical aspect in our conceptual model. All of FJG's possible glacial catchment is already glaciated and the surrounding mountain slopes are far too steep to add new accumulation areas. Therefore, FJG advances are produced by the thickening of its ice field and not by the enlargement of its glacial catchment. This can only change if ice from neighbouring névé fields became thick enough to overflow into the catchment. However, Anderson and Mackintosh state that there is no evidence that the ridges separating the catchments were overtopped by ice (point 3). Since moderate cooling would not cause a significant increase in FJG's glacial accumulation area we do not expect this glacier to respond dramatically to moderate cooling. Conditions are different, however, for most parts of the Southern Alps where existing or potential glacial catchments are topographically far less confined (compared to FJG). Our DEM based hypsometrical calculation for a 60 km long sector in the central alps (Fig. 6 in Rother & Shulmeister) shows that a 300 m ELA drop would more than double the surface area of low angle slopes (less than 15 degrees) which would cause a vast increase of potential ice accumulation areas. High-mass-balance (HMB) feedbacks, in particular in the less steeply

descending eastern valleys would further increase the extent of new ice accumulation areas. None of this applies to FJG where the possibility of adding new ice accumulation areas is extremely limited. We conclude that although the confined catchment of FJG may make this glacier more suitable for a quantitative model approach, the very conditions make FJG unsuitable for understanding mechanisms of ice accumulation during large LGM style glaciations in the wider Southern Alps. Furthermore, if temperature decrease is the main force responsible for driving FJG to its LGM position, and no ice cap existed to add ice from neighbouring catchments, we would like to know what the minimum required cooling would be to drive FJG to near its LGM position. We expect this temperature to be substantially more than the 5 degrees C; cooling needed to drive FJG to its LGIT position. We note that although there is some debate about the precise amount of cooling in NZ during the LGM, ~5 degrees C is the typical estimate of cooling for the LGM and not the LGIT (e.g. Soons 1979).

Point 4) The ELA we provide is not arbitrary and the precipitation is not hypothetical. The ELA data come from measurements by Chinn and Whitehouse (1980) and the precipitation data come from measurements by Griffiths and McSaveney (1983).

Point 6) We agree with this point in general but an ice flow model is not a panacea. It is part of a bigger jigsaw puzzle.

Point 7) HMB feedback actually accentuates the effect of our model.

Point 8) We disagree. High mass balance sensitivity implies short ice response times especially in valley glacier systems.

Point 9) We have not yet converted our mass balance to glacier length and volume estimates. Watch this space for developments. It is difficult for us to respond to an as yet unpublished ice flow model without being able to evaluate either the input data or the model itself.

Point 10) see point 9.

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Point 11) We agree. Our model is an alternative forcing of some NZ glacial advances. It does NOT preclude glaciation by Northern Hemisphere forcing. It simply means that NH forcing is not required to explain all advances.

We appreciate the opportunity for this dialogue and hope our response stimulates further discussion.

Sincerely, Henrik Rother and James Shulmeister

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