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Hydrology and Earth System Sciences Discussions

Interactive comment on "Downscaling ERA-Interim temperature data in complex terrain" *by* L. Gao et al.

Anonymous Referee #2

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The manuscript presents and compares different elevation corrections of ERA-Interim temperatures. Temperatures are estimated for Garmisch-Partenkirchen, Zugspitzplatt and Zugspitze. The estimates are based on 2m or 850hPa temperatures from an interpolated version of ERA-Interim and different estimates for lapse rates based on literature values, observed lapse rates between Garmisch and Zugspitze, and different versions of ERA-Interim lapse rates. The key findings are that observed lapse rates are in all months of the year slightly to moderately lower than those in ERA-Interim and substantially lower than the published lapse rates. The validation of the estimates against the observations at the three stations is based on RMSE, mean absolute error and the Nash-Sutcliffe Efficiency coefficient. It is found that the estimates based on the ERA-Interim lapse rates have similar errors to those based on observed lapse rates,

which both are substantially smaller than those based on published lapse rates. It is claimed that these results can be expected to be valid in other regions as well and that therefore an elevation correction of ERA-Interim temperatures based on ERA-Interim lapse rates can be expected to yield useful results in general.

Although the comparison of observed, ERA-Interim and published lapse rates is interesting the paper lacks conceptual clarity and the claims that ERA-Interim lapse rates can be expected to be realistic in general is a very strong and unjustified generalisation of the results based just on the Zugspitze area. There are many problems and open questions that have been already rather comprehensively listed by reviewer A. I would have made a similar list but as there is no benefit from repeating the same points I only want to reemphasize some aspects and add a few comments. In my opinion the manuscript needs major revisions before it can be considered for publication.

Specific points:

1.) As pointed out by reviewer A a key problem is that the ERA-Interim lapse rates might be close to the observed lapse rates because of the assimilation of temperature and other data from the area. No information on this is given in the paper and instead it is claimed that the relatively realistic lapse rates indicate that ERA-Interim parameterisations that are relevant for the lapse rate work well and that this good skill can be expected also in other areas. The claims made are not supported by any evidence and a revised version should include a systematic discussion of the potential reasons for errors in lapse rates in reanalyses, of what is known about these errors already, and of what is known and not known about the spatial variability of the errors.

2.) As stated in eqn.1 the temperature estimate for a specific location is based on a temperature estimate for a reference level plus an estimate for the lapse rate times the elevation difference between the reference level and the target location. As the error in the elevation difference is negligible the error for the estimate at the target location is thus the sum of the error for the temperature at the reference level plus the elevation

difference times the error in the lapse rate.

The validation of the temperature estimates in the manuscript quantifies the total error, The discussion implicitly suggest that the total error is dominated by the error in the lapse rate but no evidence for this is given.

This point is linked to the points raised by reviewer A about the large-scale biases in ERA-Interim temperatures, as the reference level error can be thought of as a sum of a large-scale, temporal mean bias and time-dependent, small-scale errors. This point is also related to reviewer A's question on how the interpolation on the 0.25 deg grid has been performed, in particular how the varying elevations are dealt with in the interpolation.

A revised version should include a systematic discussion of the various error components and clarify which errors are addressed in a particular part of the analysis. It should also clarify the purpose of the three different skill measures (in particular the NSE coefficient needs explanation and the wording should be made consistent with respect to the use of 'MAE' and 'bias', which are the same.

3.) I agree with reviewer A that the title should use 'elevation correction' rather than 'downscaling'. Although an elevation correction is a form of downscaling if high-resolution elevation information is used, many other effects that introduce small-scale horizontal variability and that are usually approximately represented by downscaling models are not included in the elevation correction. It thus seems better not to use the phrase downscaling in the title to avoid misunderstandings. The link between elevation correction and downscaling could then be discussed in the introduction. Please note in this context that the elevation corrections are conceptually close to Model Output Statistics approaches but differ from those (and from Perfect Prog Downscaling) as no observations of the predictand variable are used to fit a statistical model (for a recent review on downscaling see Maraun et al., Geophys. Rev. Lett, 2010).

4.) A minor point that should be clarified is why for the second half of the day the

forecasts initialised with the 00:00 UTC analysis have been used despite the fact that forecasts initialised at 12:00 UTC are available.

5.) How can the MAE in Table 3 be different for 3h and daily resolution? Are the daily values not simply the mean of the 3h values and thus the MAE should be identical?

6.) The units used in Fig.1 should be 'hPa' rather than 'mb'.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 5931, 2012.