

The paper is improved by the addition of more detail of the datasets used, and I commend the authors on their careful and considered conclusions, which rightly highlight the complexity of this phenomena. While I still have some reservations about the dataset due to the fundamental importance of the lapse rates used to create the dataset in determining EDW, I understand the difficulties in validating such a dataset, and the limitations are well discussed in the paper now. Some comments are discussed below.

I am clearer now about the purpose of looking at regional warming amplification, however I think the distinction between regional warming amplification and altitude warming amplification could still be made clearer. Unless the terminology of 'regional warming amplification' and 'altitude warming amplification' are used elsewhere in the literature, it might be clearer to stick to the terminology used in Rangwala and Miller (2012), and only use 'elevation dependent warming' to describe altitude warming amplification. It could be made clearer that section 3.1 is considering whether the Chinese Tianshan Mountains are warming faster than the surrounding lowland areas as a whole, and then that 3.2 and 3.3 are looking at elevation dependence within these mountains.

Table 3 and 4: There still seem to be some instances where the warming trends are larger in both  $T_{min}$  and  $T_{max}$  than in  $T_{mean}$ . This may be what the data show, but I think it needs some discussion. It suggests either a fundamental change to the diurnal cycle, or that the results may be overly dependent on the hours chosen.

Table 5: It is very useful to have all these put numbers in one table and makes a good addition to the paper. However, the method used to determine the trends is suggesting startling differences between the trends, which are being exasperated by elevation bands used to determine the trend.

For example, April in table 5, there is a suggestion of increased warming with elevation in  $T_{min}$  and  $T_{max}$ , but decreased in  $T_{mean}$ . This discrepancy seems to be due to the authors taking the gradient of the slope for minimum and mean temperature from all the elevation bands, but the gradient of the slope for the mean temperature only from 2500 m upward (Fig S4). Could you explain why you chose a different method for  $T_{max}$  and  $T_{mean}$ ? I think the values in table 5 should compare similar slopes, otherwise they are somewhat confusing. Fig S6 is also somewhat surprising, in that in the highest elevation band, the trends for minimum and maximum daily temperature are both smaller than the trend for mean daily temperature.

Figure 5: While the subplots are added are striking, I am not wholly convinced that they are representative of the whole subregion being examined. For example, figure 5 b, in zone 2, if you took a similar transect at the very northern region of zone 2, would you see the opposite results? These subplots would be better based on average temperatures with elevation within each zone, rather than unique transects.

### **Minor comments**

Line 75: please provide some references relating to the Alps, Andes and Rockies.

Line 80: is this trend in minimum and maximum temperature differences a worldwide phenomena?

Line 137: some words missing in this sentence 'for example, the lapse rates of ERA\_Interim are greater than those from September to December'.

### **References**

Rangwala, I., and Miller, J. R.: Climate change in mountains: a review of elevation-dependent warming and its possible causes, *Climatic Change*, 114, 527-547, 2012.