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Interactive comment on "Factors controlling Slope Environmental Lapse Rate (SELR) of temperature in the monsoon and cold-arid glacio-hydrological regimes of the Himalaya" *by* R. J. Thayyen and A. P. Dimri

Anonymous Referee #1

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The authors use temperature measurements from two different valleys in the Himalayas in two different regimes (monsoon and cold-arid) to calculate temperature lapse rates along the altitude gradient. Although showing a high variability, the available data indicate seasonal cycles and distinct differences between the two regimes. In both regimes moisture seems to be the major parameter determining the strength of the lapse rate according to differences in the theoretical dry adiabatic and saturated adiabatic lapse rates. In short, more moisture reduced the lapse rates, which is observed in the monsoon regime during the summer monsoon, but also during the winter monsoon. The

impact of moisture during the winter months is also visible in the cold-arid regime. Since data from three different altitudes are available from both valleys the authors further investigated the lapse rates in different altitude bands showing that the decreases in the lapse rates are stronger in the lower altitudes. Finally, the authors propose a new equation using an empirical parameter derived from the available measurement to predict the lapse rates for the two different regimes and the investigated altitude bands. This parameterization delivers good results compared to the observed lapse rates and is recommended for further applications that require altitude-dependent temperatures. Such applications are widespread regarding for example in the modeling of the cryosphere or hydrology in this region. Therefore, such a new parameterization should certainly improves our predicting capacity in this sensitive region, where observational data is still scarce. Therefore, I recommend publication of the manuscript. Unfortunately, the manuscript contains numerous editorial and orthographical errors that need to be rectified before publication probably using the help of a native English speaker. In my comments below I raise some further issues that the authors should address before the publication of the manuscript.

Comments: Chapter 3: The authors present and use lapse rates from daily averaged temperature. However, at least at some stations and for some periods, hourly temperature records are available. What is the impact of using daily temperatures? How strong to the lapse rates vary during a 24-hour period? This certainly depends on the conditions. Nevertheless, the authors should check the high-resolution lapse rates to estimate the uncertainty of the daily lapse rates.

Chapter 4.2: The authors discuss precipitation data without any description of how these data was obtained. The measurement of precipitation in mountainous regions and in regions with a high fraction of solid precipitation is still challenging. What is the uncertainty of the presented precipitation data? Later on, the authors also show relative humidity (Fig. 7c). Again, no information on the measurement methods for the humidity is given in the manuscript. Finally, since the authors claim that moisture is

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the major driving force, why is the precipitation data necessary? Isn't the moisture a much more important parameter that should be discussed in chapter 4.2 instead of the precipitation?

Chapter 4.3 and 4.4: In many (all?) cases the ranges of the daily SELR given in the text do not correspond to the values in the corresponding figures. For example, the authors claim that in section-1A the SELR in the core winter months range from 5.8 to 7.5 °C/km. However, the SELR shown in Fig. 4a vary between 2 and 10 °C/km. The given ranges should be verified and made consistent with the data displayed in the figures.

Chapter 4.3: The authors claim that the SELR in September and November in section-1M decreases only "occasionally" to the low range of 4.9 to 5.8 °C/km observed during the summer. However, Tab. 1 shows that in September only in two years (out of six) the average SELR was higher than 5.8 °C/km, in November this was the case in only one year (out of five). This is not consistent with the statement in the text.

Chapter 4.5: The authors use the ERA-Interim data set to calculate the SELR from reanalysis data in comparison to the observed SELR. However, it is well known that the coarse-resolution re-analysis data do not well capture many features over the rough topography of the Himalayas. However, results form regional climate models for the Himalaya region are also available (e.g. M. Ménégoz et al., Hydrol.Earth Syst.Sci. 17, 3921-3936, 2013; A.J. Wiltshire, The Cryosphere 8, 941-958, 2014). Wouldn't it be better to compare the observations to the results of the RCM simulations?

Chapter 5: The authors discuss the influence of moisture on the SELR in terms of relative humidity. Wouldn't it be better to use absolute humidity? The authors claim the importance of moisture on the SELR. Did the authors correlate the SELR with the observed humidity? Do exceptional dry days during the summer period show high SELR and vice versa for humid days during the winter period? If that is the case this would support their conclusion that the moisture is a very important factor.

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Chapter 5, page 5666: The authors state that their analysis provides a “a significant advancement in our understanding of the process governing moisture–temperature interplay at the higher Himalaya and the SELR variations in two distinct glacio-hydrologic regimes of the Himalaya”. A similar statement can be found in chapter 6, page 5667. I find these statement to far-fetched. The proposed equations and coefficients certainly provide a step forward in describing the SELR in this distinct regions providing valuable information to be used in further applications. Nevertheless, I am missing a detailed discussion on how these observations have advanced our understanding. In my opinion a discussion of the validity of the derived parameter is further missing. The authors show that they represent reasonably well the SELR during the investigated periods and the two valleys. However, a conclusion of how the parameters can be extrapolated to other valleys or regimes or to other periods in the past or even in the future is absent.

Chapter 6, page 5666: The authors claim that “the single most important factor determining the temperature of the higher Himalayan mountain slopes including snow/glacier regime is the moisture.” Has this actually been tested? This is actually the only factor at which the authors have looked in detail. The manuscript gives no further information which other parameters were studied.

Figure 4: All four x-axes begin with different months making a comparison very difficult? This should be made consistent. Also in Fig. 7 and 9. In Fig. 8 the x-axes are completely missing.

Figure 7a and b: I don’t understand the claim of the authors that the SELR and LCL in Fig. 7a shows a better correlation than in Fig. 7b. This is not obvious from the graphs.

Figure 7c: This is not a good title for a figure.

Interactive comment on The Cryosphere Discuss., 8, 5645, 2014.