Anonymous Referee #2

General comments: This study is interesting since not too many data and knowledge exists about evapotranspiration in Alpine environments. The authors worked hard and made a good job to generate new results from few available data. However, I do not see sufficient novelty and innovative potential in the analysis in order that it should be published in an international, highly ranked journal. The main drawbacks of this study are: (a) There already exist evapotranspiration maps for Austria and other countries in the European Alps, some of them including greater detail than the study presented here (b) Applications of the Hargreaves method and its adjustment with respect to accepted, physically based methods already exist (c) The physical background of the presented methodology does not exist or is questionable.

Questions and comments to item (a): Why was a new mapping of evapotranspiration necessary for Austria? Why didn't the authors compare their results with data from existing studies? There are evapotranspiration maps available for Austria: - Hydrological Atlas of Austria: Plate 3.2 (Mean annual potential evapotranspiration) and Plate 3.3 (Mean annual areal actual evapotranspiration using water balance data) Besides, there is an evaporation map for Switzerland which is based on the Penman-Monteith equation (reference period 1973-1992): - Hydrological Atlas of Switzerland: Plate 4.1 (Mean annual actual evaporation).
It is strongly recommended to analyse and to explain existing agreements or differences with the Austrian and possibly the Swiss map (e.g., different elevation gradients, mean annual data of evapotranspiration for different elevation zones etc.). Based on these analyses the authors should explain why a new product was necessary for Austria. What is the real novelty and in which fields was new knowledge generated with regard to the existing products? Why was a modified version of the Hargreaves equation applied when products exist which are based on more accepted methods?

There are two main reasons for the compilation of a new ET0 dataset:
(i) to create a long-term dataset of reference evapotranspiration from 1961 onwards on a DAILY time step. The intention of this study was not to calculate new maps of climatological mean values. There are of course maps of ET0 in the hydrological atlases of Austria and Switzerland. Hence, they are compiled based on more physically representations of ET0 (for Austria based on Penman-Monteith). But it is much easier to get gridded climatological mean values of all the input data needed for calculating Penman-Monteith ET0 than it is for daily mean fields. Daily fields of wind speed, humidity, and radiation are unfortunately not available before the 1980s or 1990s, so there is no chance for calculating daily Penman-Monteith ET0 before the 90s.
(ii) We intended to compile a dataset with high spatial and temporal resolution stretching back as far as possible. Since daily fields of Tmin and Tmax are now available from 1961 onwards, we decided to use the Hargreaves method. This method is of course not physically based, it is a parameterization. But it is widely used and, as is shown by the references cited in our manuscript, there are approaches to calibrate this method to physically meaningful formulations (Gavilán et al. 2006, Pandey et al. 2014, Aguilar and Polo 2011, Bautista et al. 2009). The reasons for these attempts are always lack of data to calculate e.g. Penman-Monteith ET0 and the intention to stretch further into space and/or time by using a simpler method.

Comments to items (b) and (c): The authors apply the simple Hargreaves method (HM) with a standard correction factor C (0.0023) to 42 stations in Austria. They compare the performance of the HM method with the modified Penman-Monteith method (PM) to express the reference evapotranspiration ET0. Then, "in order to achieve a meaningful representation of ET0 by HM" (page 5061, line 25) they adjust the calibration parameter Cadj to optimize the agreement between HM-derived ET0 estimates with those calculated with PM. The authors apply a simple method which was developed earlier, thus this step is not new. The results show that Cadj at individual stations varies over the time. Finally, the monthly Cadj parameters are first linearly interpolated to daily data which are then interpolated on a daily 1x1 km grid over Austria. The interpolation from 42 stations to the individual grid cells is carried out through monthly fitting of a third-order polynomial curve against altitude (the monthly shapes of the curves greatly differ). Result is a gridded dataset of Cadj for every day of a year. In a final step, ET0 is computed for the individual grid cells by use of the HM method and the Cadj values. All the steps described above lack conceptual clarity, the procedure just consists of a number of optimization steps which introduce fuzziness regarding any physical meaning. Therefore, any physically-based explanation regarding the temporal and spatial variation (including altitude dependencies) of Cadj or the HM-derived ET0 estimates is not given.

Thanks for your comments; we actually we don´t actually know where this temporal and altitude dependence is emerging from. We will add an additional paragraph to the Discussion section where we will address this feature in detail, since this might be also be relevant for a broader audience and will raise the significance of the paper.
The whole approach is indeed an optimization and merging of existing methods. But we still think that this new optimization method is valuable, since it is worldwide applicable, not only for the Eastern Alpine Area.

Hence, analysis given in section 4 (results) remains obscure. Moreover, time series analysis with respect to climate change impacts on evapotranspiration seems not trustworthy and should be avoided.

True. We will avoid these kinds of analysis and also statements on the usage of the dataset to assess climate change impacts on reference evapotranspiration evolution.

As ET0 refers to the evapotranspiration from a well-watered grass cover neglecting the impact of soil properties how would you rate the applicability of this concept to high alpine areas? What is the meaningfulness of the ET0 concept for such conditions? Is ET0 a realistic approach for e.g. dwarf shrub communities on shallow initial soils, bare rock or snow/ice cover? Don't you think that ET0 overestimates evapotranspiration for such conditions?

The meaningfulness of ET0 is of course shrinking going to higher elevations where bare rock and snow/ice is dominating. However, the concept of ET0 serving as a reference (well-watered grass cover), is that there is a "starting point", from which actual evapotranspiration can be derived by using hydrological or land surface models. These models consider the "real" land surface cover, may it be forest, agricultural land or pasture, the soil conditions and actual soil wetness.

Specific comments:
The article requires English language editing. There occur quite a number of spelling and grammatical errors and there are ways to say things more clearly or using fewer words. Some sections, including the abstract, read complicated.

English language editing will be accomplished, as well as clearer formulations throughout the manuscript.

Confusing notations: In the first sections of their article, the authors term the reference evaporation as ET0. In section 3.1 they term the ET0 following the (modified) PM method as E (equation 1) which they also define as reference evapotranspiration. Then, in the same section they apply the terms ET0_p for the reference evapotranspiration based on the (modified) PM equation and ET0_h for the ET0 derived from the original HM equation. In section 3.2 (equation 3) EH is "the original ET0 from HM" and EP "is the ET0 from PM" (page 5062, lines 3/4). This change in terminology is really confusing.

We will change the terminology of the different ET0-types to be consistent throughout the manuscript.

There are several repetitions in the text regarding the statement that the modified PM method is seen as the reference (see e.g., page 5058, line 6 or page 5061, line 6)

We will go through the manuscript and change/delete redundant parts of the text.

Repetitions of ET0 definition: There are at least two definitions of ET0, and they seem quite different which confuses the reader. See for example page 5057, lines 6/7 and page 5059, lines 21/22

In principle these two text passages state the same thing, but in rather different words. It is true that these contradicting formulations may confuse the reader, so we will change the passages to be more coherent.

Page 5060: line 2 says that the PM method requires global radiation. In equation (1) however and on line 6 net radiation is mentioned as necessary input.

The global radiation is used to calculate the net radiation. We will add some text to clarify that issue.

Regarding the formulation of the PM equation on page 5060 please mention that this is a modified version of PM, with the original form (to calculate actual evapotranspiration) including a resistance network.

We will add some text on the differences between the original and the FAO Penman-Monteith formulation. Thank you for your suggestion.

Page 5060, lines 11/12: It is simply not practicable / physically allowable to set the soil heat flux to zero on a daily time step! Please see standard textbooks on micrometeorology about the radiation balance. Or would you set the change in daily soil water storage to zero as well?

In the FAO Penman-Monteith method, the soil heat flux on a daily time step (not on a shorter time step) is set to zero based on the following statements extracted from Allen et al. 1998:

Complex models are available to describe soil heat flux. Because soil heat flux is small compared to Rn, particularly when the surface is covered by vegetation and calculation time steps are 24 hours or longer, a simple calculation procedure is presented here for long time steps, based on the idea that the soil temperature follows air temperature.

For day and ten-day periods:

As the magnitude of the day or ten-day soil heat flux beneath the grass reference surface is relatively small, it may be ignored and thus:
Gday ≈ 0

For us it seems appropriate to follow this guideline, since it is a worldwide accepted and widely used framework.

Page 5060: please explain how you calculated Ra for the Austrian stations / the individual grid cells from extra-terrestrial radiation and give an example (in water equivalent). Don't you think that this involves high uncertainty in the whole calculation process?

Ra (extra-terrestrial radiation at the top of the atmosphere given in MJ m^{-2} day^{-1}) can be calculated for every station by using latitude and Julian day as input variables. By multiplying the result by a conversion factor of 0.408 the Ra [MJ m^{-2} day^{-1}] is converted to Ra [mm day^{-1}]. The calculation steps are given in Allen et al. 1998 in detail.

Example:

Station at latitude 48° North, 22nd of April which is 112th day of year.

J = 112
lat = 48°
latr = 48/ 57.2957795 -> latitude in Radians

delta = 0.409 * sin(0.0172 * J - 1.39)
dr = 1 + 0.033 * cos(0.0172 * J)
omega = acos(-tan(latr) * tan(delta))

Ra = 37.6 * dr * (omega * sin(latr) * sin(delta) + cos(latr) * cos(delta) * sin(omega))

Ra = 34.01004 [MJ m^{-2} day^{-1}]
Ra = 34.01004 * 0.408 = 13.87609 [mm day^{-1}]

There is of course uncertainty in the calculation, since this conversion factor applies to water at 20°C. Nevertheless, we followed the FAO guidelines in all of the calculation steps and think that this is an appropriate way of calculating ET0.

Why are there separate Discussion and Conclusion sections? In the Discussion, any critical analysis is missing, while the Conclusion is just another summary of the work.

From your previous comment we will add some critical analysis regarding the altitude dependence of the calibration parameter and the uncertainty involved in the calculation process. This will additionally justify a separate Discussion and Conclusions section.

Figure 5: They grey shaded area as well as the black line in Fig. 5a seems to be identical with the ones in Fig. 3b. Please avoid redundancy

We thought it would support the reader if the original RMSE is added to the graph, to actually see the improvements.

Page 5057, line 10: why “also recommended by FAO”?

We will delete “also” for clarification.