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Climate of the Past Discussions

Interactive comment on "Thermal signal propagation in soils in Romania: conductive and non-conductive processes" *by* C. Demetrescu et al.

Anonymous Referee #1

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General Comments

This is a potentially promising paper introducing data from Romania that include meteorological variables and subsurface temperature measurements down to 1 m. The data are analyzed for the purpose of understanding relationships between air and ground temperatures, with the ultimate goal of evaluating the assumptions of borehole paleoclimatology, i.e. conduction is the dominant heat transport mechanism in the subsurface over decadal and longer timescales and air and ground temperatures track each other over similarly long timescales. The validation (or refutation) of these assumptions is the subject of ongoing investigations, and studies of observational data such as this

work by Demetrescu et al. are important contributions to these efforts. Additionally, temperature measurements that extend below the first several centimeters of the subsurface are quite rare and the addition of this new dataset is a notable achievement. As such, the manuscript has the potential to be a valuable contribution to Climate of the Past and should eventually be published. As it stands, however, the paper suffers from lack of focus and organization and the writing needs to be substantially revised. I suggest that the paper be published only after significant revisions have been made.

Specific Comments

As the paper stands, there are several digressions that are not necessary and make the paper heavy with figures (and light on analyses in some sections). Section 3 is perhaps the best example of this. While thermal orbit analyses (Figures 5-7) have the potential to be insightful, the qualitative manner in which they are presented in the paper does little to support the authors' arguments. This section, along with the corresponding 3 figures, could be removed from the paper without losing much of its overall content and arguments.

On page 473, lines 8-9 the authors suggest that they will provide analyses of one station, Bistrita, and only briefly discuss the other stations. This isn't entirely accurate, given the focus of the paper, and it highlights another problem, namely a back-and-forth between analyses at a single site and those from the group of sites. I understand the authors' motivation to include all of the stations and there are certainly insights to be gained from comparisons between all available observations. Nevertheless, the present manuscript gives neither an in-depth analysis from a single location or a satisfactory spatial synthesis of the multiple sites. I suggest the authors focus on one or the other in the present manuscript. For instance, if the authors choose to focus on Bistrita only, they should consider a general presentation of all sites and then proceed to analyze the data from the single location. This would obviate the need for Table 2 and Figure 13 (although I find this latter figure quite interesting). Alternatively, a spatial synthesis of the data might look at the variable effects of snow cover at the sites or

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investigate how the timing of rainfall events at different sites affect their ideas about the influence of precipitation on signal transport.

In keeping with my above statements, I find the analyses of the Bistrita data incomplete and the corresponding conclusions somewhat weak. It is easiest to provide my arguments by stepping through several of the figures:

Figures 8 and 9. The authors attempt to model the 5 cm soil temperature using different thermal diffusivities and the air temperature as the surface forcing. One should first notice that the range of diffusivities has little impact on the derived 5 cm series. This is not surprising given the fact that the signal is only diffusing through 5 cm of the subsurface. Only very large differences in diffusivity would yield significantly different results and this type of sensitivity analysis is not very instructive. By contrast, Figure 9 demonstrates a greater sensitivity because the signal is modeled over a depth range that is ten times larger. One also wonders why the authors don't attempt these modeling experiments using the estimated effective thermal diffusivity from Figure 12, in which case the deviations at each depth may be more insightful (see Figures 4 and 5 in Smerdon et al. 2003 or Figure 4 in Smerdon et al. 2006). I am also confused by the authors' suggestion that Figure 8 indicates the absence of "pure conduction" (Page 476, Line2 14-15). The differences between measured and observed temperatures in Figure 8 are undoubtedly a snow cover effect and have little to do with the conductive or non-conductive nature of the subsurface. It therefore seems important to point out that this is a question of coupling between the air and ground surface and not the product of any transport one would observe in the first 5 cm of the subsurface.

Figures 9 and 10. The relationship between Figures 9 and 10 are indeed interesting. The authors suggest that increased precipitation gave rise to the increasing differences between measured and modeled temperatures. This would be very interesting to investigate further. Does such a separation exist between other depths? Does the separation really coincide with the onset of increased precipitation (it looks like the precipitation starts a little later)? The authors suggest that either increases in soil wa-

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ter content or increased evapotransporative cooling might be the physical reason why this occurred. Can they rule out one or the other? One particular curiosity is that the differences increase into the fall, at which time the available energy to drive evapotranspiration is also reducing. Would that help rule out this particular effect as an explanation?

Figures 14 and 15: These figures come under the banner of conductive vs. non-conductive processes, but I again feel that the authors are mixing above-ground coupling with heat transport processes that occur in the subsurface. Most of what is presented in these two figures is the result of snow cover, i.e. the differences between the air temperature and ground surface temperature. These are instructive figures, but do not necessarily fit within the context that the authors present them. If the authors wish to analyze the coupling between the air and ground, they should focus more on the differences between air and ground temperatures and try to explain them in terms of the meteorological data that they have, i.e. precipitation and snow cover. By contrast, the authors' attempts to conductively model signal transport within the subsurface addresses the behavior of the signal once it is in the ground. I am not suggesting that above and below ground processes are not coupled, but the authors could do more to try to disentangle them.

Technical Corrections

Writing: There are many places within the text where the writing could be improved. While I have not taken the time to suggest corrections, I am happy to see that Reviewer 2 has indeed made many suggests. I strongly support these language corrections.

Page 470, Line17: If the authors wish to provide a list of geothermal reconstructions they should include other relevant studies such as: Beltrami, 2002; Harris and Chapman, 2001; Huang et al. 2000; Roy et al. 2002; Pollack et al. 2003 etc.

Page 471, Line 9: Additional references on air and ground temperature relationships are applicable: Zhang et al. 2001; Smerdon et al. 2004, 2006; Bartlett et al. 2005,

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2006; Schmidt et al. 2001; Sokratov and Barry 2002; Stieglitz et al. 2003; Lin et al. 2003; Putnam and Chapman 1996; etc.

Page 471, Line 13: Additional references are again warranted: Briffa and Osborn 2002; Mann et al. 2003; Pollack and Smerdon 2004; Rutherford and Mann 2004; North et al. 2006; Mann and Schmidt 2003; Schmidt and Mann 2004; Chapman et al. 2004; Moberg et al. 2006; Esper et al. 2004; Hegerl et al. 2007.

Page 473, Line 5: I do not understand the sentence beginning with “HoweverĚ”

Page 472, Line 11: “Romanian National Meteorological Agency”?

Page 477, Lines 5-8: The authors mention two ways of extracting the annual signal from the data: Fourier analysis and a least-squares fit of the data to a cosine function. While both of these methods should give similar results in this case for the annual signal, the authors should be clear about which one they have actually used, as they are two different methods.

Figure 1: This map should include a legend or the caption should describe the symbols used. It looks like black squares denote observational sites and red circles denote cities. This should be clarified.

Figure 3: For consistency, why not plot the Adamclisi data to 1 m and leave the 50-100 cm blank as in the other figures where data is missing?

Figure 8 and Figure 9 (top): Temperature should be labeled degrees C here, not K.

Figure axes: Most of the figure axes are labeled as the number of days from a specific start day. Many of the start days change from figure to figure, making it very hard to compare figures or to line up discussions in the text (which mention specific days or months) with specific points on the figures. The axes labels should all be changed to the actual dates.

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