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Interactive comment on "Footprints of climate in groundwater and precipitation" *by* A. Liebminger et al.

Anonymous Referee #1

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General comments

This manuscript presents an analysis of time series of stable isotope data from several precipitation stations in Austria as well as of drinking water from Vienna, which is used as a groundwater record. Several interesting observations are discussed, in particular:

1) Temporal trends of δ^{18}O in Vienna drinking water are related to a weighted mean of precipitation data from the recharge area (Wildalpen) and to temperature.

2) The δ^{18}O variations in precipitation at Wildalpen are correlated with the winter NAO index, and anticorrelated with the local snow/precipitation ratio and relative humidity.

3) A comparison between a mountain and nearby valley station shows the effect of sub-cloud evaporation on the stable isotope composition of precipitation.

4) The importance of sub-cloud evaporation apparently has increased over the past 30

years and is related to the precipitation amount.

Of these points, 2 and 4 are at least partly new. Point 1 presents new data but nothing unexpected. Point 3 is interesting but has already been shown in a previous paper. The paper is the third in a recent series of studies by the same authors about their analysis of stable isotope data in Austrian precipitation records (see Liebminger et al. 2006a and 2006b, as cited in the manuscript). With respect to point 3 there is a certain overlap of the present manuscript with the 2006a-paper published in Journal of Geophysical Research (referred to as JGR-paper in the following). Yet, there is a logical separation between the two studies, the JGR-paper discusses mainly the spatial variability between 12 Austrian precipitation stations, whereas the HESSD-manuscript focusses on time series from some selected stations and the drinking water record from Vienna.

The paper is very short although it treats two quite distinct issues (the groundwater - precipitation relationship and the local effects on precipitation). Upon first reading, I found it difficult to follow the logic of the manuscript and to judge the quality of some parts of the discussion. For instance, I was surprised that well-established concepts such as the amount effect or the deuterium excess are not mentioned in the manuscript. Studying the previous JGR-paper proved to be very useful to obtain a complete impression of the authors' approach. That paper contains much of the analysis that I was missing in the present manuscript, which convinced me that the authors have made a valid analysis of the data, although part of it is omitted in the present manuscript, where many details remain unclear.

The findings presented in this manuscript are certainly suitable for publication in HESS, and the overlap with the JGR-paper is acceptable. However, I think that the manuscript needs substantial revision in order to make it more accessible for readers who have not seen the previous papers as well as to clarify a number of issues. I suggest to strengthen the focus of the manuscript on temporal trends in precipitation by extending and clarifying the discussion of the main new findings and by moving back the discus-

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sion of the groundwater data. Detailed comments and suggestions for changes are given below.

Specific comments

1) Abstract: The statement “Here we show for the first time that certain interactions between different climate induced changes in local parameters prevailing during precipitation events are responsible for the observed deviation” is a bit bold. The idea that sub-cloud evaporation influences the isotopic composition is certainly not new (see below). That it is important in some Austrian precipitation records has already been shown in the JGR-paper of the same authors (although not explicitly for the time series). The paper shows that there are factors other than temperature influencing the isotopic variability (Fig. 2), but not that they are responsible for all of the variation that is not explained by temperature. In particular, the parameters shown in Fig. 2 to correlate with isotopic variation are not completely independent of temperature.

2) On p.273, line 22 it is stated that groundwater generally reflects annual mean precipitation, whereas on p.274, line 15, it is said that “recharge is not a constant process over the whole [year]”, and therefore a weighted mean should be applied. This is inconsistent.

Apparently some sort of weighting is then used to calculate an input function for stable isotopes in the recharge area for the Vienna drinking water supply, which is “calculated according to Maloszewski and Zuber (1996) applying a sinusoidal fit for the recharge factor (McGuire et al., 2002)”. These explanations should be more detailed. Maloszewski and Zuber (1996) discuss a variety of lumped parameter models that can be used for tracer data interpretation. Without a statement on the model that was used and its parameters (esp. mean residence time, MRT), the reference is quite useless. Moreover, this approach is usually applied to model the output function of a groundwater system in response to a given input, i.e., in the present example, the variation in

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Vienna drinking water as a result of the transfer of the precipitation input through the aquifer. It is unclear to me what actually has been done and is shown in Fig. 1b. The "sinusoidal fit for the recharge factor" seems more appropriate to construct an input function, but it is also rather unspecific (e.g., what is the amplitude?).

The following statement (lines 8 - 11, p.275) is unclear. Which "corresponding" temperature data explain some part of the variance in which isotopic pattern? Does this refer to Vienna or Wildalpen temperature data and to the Vienna drinking water or Wildalpen precipitation (or modified input function) isotope records? In the following discussion of Fig. 2, it is not absolutely clear to me whether the δ^{18}O record of Wildalpen precipitation refers to yearly mean or only winter mean values.

3) The entire section about the groundwater data (the last paragraph of the introduction and the first paragraph of the "results and discussion" section) appears somewhat detached from the rest of the paper and superficial. Vienna drinking water is taken as a synonym for groundwater originating at the Wildalpen, without any further discussion of the setting (is this really the only source of drinking water for Vienna, is it derived from a single source/well, how far is the recharge area from the extraction site, etc.). The importance of the MRT for the preservation of short-term fluctuations is mentioned, but no information on the MRT is given.

Apparently the point of the groundwater section is that the isotopic variations seen in the groundwater essentially reflect those of the winter precipitation, which in turn are related to NAO and other climatic parameters. Maybe this would become clearer if first the patterns in the precipitation and their relationship with the winter NAO index were discussed, and then it would be shown that this winter signal is still visible in the Vienna drinking water. The groundwater data would then appear as an excursion from the main topic of the isotopic patterns observed in precipitation, which seems to reflect more appropriately the main thrust of the paper.

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4) On p. 276, the influence of sub-cloud evaporation is discussed by comparing data from several stations at different orographic positions. This is hard to follow without the more detailed information that is given in the JGR-paper. For instance, from the text it does not become clear that the stations Weyregg and Kufstein are located in front of the mountains, and there is no information on the altitude of these stations and of Innsbruck. Part of this information is given in the caption of Fig. 3 (but no altitudes for Weyregg and Kufstein).

Furthermore, the text is simply hard to read. The long sentence stretching from lines 2 to 6 at least needs some commas. It ends with "of increased importance", but compared to what? This statement is essentially a conclusion from the JGR-paper, and needs more explanation to become clear to those who have not read that paper. The sentence beginning on line 11 is also difficult to interpret: "Considering a lower relative humidity at these winter days with rain (Fig. 2c; R^2=25%; p=0.010) ... " seems to imply that Fig. 2c shows a relationship between humidity and S/P ratio, but it shows humidity and δ^{18}O and apparently this is what R^2 and p refer to. It remains unclear whether the "lower relative humidity" is assumed or observed. The next sentence refers to "quite different" trends between Innsbruck and Patscherkofel "as well as quite similar trends" at Weyregg and Kufstein. Are the latter now similar to Innsbruck or Patscherkofel? Is the "comparable lanscape profile" comparable to Innsbruck or Patscherkofel? The next sentence "For both examples the overall year to year δ^{18}O variation remains quite similar overriding an essential part of the information which does not come directly from temperature" remains a mystery to me. No information is overridden, it just may be more difficult to extract without the nice possibility to compare nearby valley and mountain stations.

5) A way how to extract the "hidden" information on evaporation is mentioned shortly afterwards, namely to compare δ^{18}O and δD. One might expect to see the classical δD versus δ^{18}O plots (such as in Fig. 4c,d for Vienna) and a discussion of the deuterium excess here. At first, I was puzzled by this omission, but then I found that this discus-

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sion is contained in the JGR-paper. The main results of that discussion and a reference to the JGR-paper should be included in the present manuscript.

The new aspect presented here (and not in the JGR-paper) is that there is a difference in the temporal trends of δ^{18}O and δD at least for Innsbruck and Kufstein. This is an interesting observation that would deserve more discussion. How significant is the difference in the slopes? Is there an explanation as to why evaporation should have become more important (are there trends in temperature and/or humidity)? Are there alternative explanations, e.g. a change in moisture source, which is a common interpretation for changes in the deuterium excess? Such questions should be addressed in more detail. The issue comes up again later in the paper with respect to Vienna precipitation data, but again without substantial discussion.

6) On p. 277, lines 2 - 7, the influence of the amount of precipitation on the importance of sub-cloud evaporation is discussed. These relationships are well-known under the name amount effect, which, as correctly cited in the JGR-paper, has already been described by Dansgaard (1964) and many later authors (see for example Rozanski et al., Isotopic patterns in modern global precipitation, in Climate Change in Continental Isotopic Records, pp. 1-36, Geophys. Monograph 78, AGU, 1993, for a review of this and other well-known effects). I wonder why the amount effect is not mentioned here and no literature is cited.

The amount effect is usually observed as an inverse relationship between monthly mean values of δ^{18}O (or δD) and precipitation amount at tropical stations, and sub-cloud evaporation is one (partial) explanation for it. The interesting point of the present study is that it appears to be (increasingly) important in the mid-latitude stations of Austria. This would deserve more discussion, if it really can be substantiated. However, I am not convinced by Fig. 4a, which is supposed to demonstrate the inverse relationship between δ^{18}O and amount for Vienna precipitation. The reason is that it is unclear what is shown there. From the temporal resolution it seems that yearly mean values

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are shown, but the precipitation heights of some 50 mm rather seem to be monthly values. If these were yearly mean data, I would be really surprised to find such a strong relationship, and it would be difficult to relate the figure to the text, which seems to refer to individual precipitation events. This absolutely needs to be clarified.

7) In the conclusions the authors again make a rather bold statement that they "show for the first time that younger groundwater due to special recharge conditions is a potential reservoir of climate information bearing just more than temperature." That groundwater is an archive for climate information via stable isotopes is certainly not new, nor that stable isotopes contain more than merely temperature information. An example for the use of young groundwater to detect recent changes not only in temperature could be: Rademacher et al., Temporal changes in stable isotope composition of spring waters: Implications for recent changes in climate and atmospheric circulation, Geology, 30, 139-142, 2002.

The really new aspects of this manuscript, as far as I can judge, are the correlation of the NAO index with stable isotopes in precipitation (not directly shown for groundwater) and the temporal changes in the deuterium excess. This should be worked out more clearly.

Technical corrections

p. 274, line 15: replace "season" by "year"

p. 275, line 7: "... colder months are main**ly** responsible ..."

p. 277, first sentence of Conclusion: "... reflecting the influence of temperature but also that of **other** local meteorological parameters ..." (temperature is also a local meteorological parameter). Avoid the exclamation mark.

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