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Interactive comment on "Using stable isotope tracers to identify hydrological flow paths, residence times and landscape controls in a mesoscale catchment" *by* P. Rodgers et al.

Anonymous Referee #1

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Review of Rodgers et al., Using stable isotope tracers to identify hydrological flow paths, residence times and landscape controls in a mesoscale catchment, submitted to HESS.

General Comments: This manuscript describes a nested catchment study of the landscape level controls on flow paths and residence times. The authors present reasonable arguments for a study of this type, suggesting that isotopic geochemical tracers observed at multiple catchment scales provide valuable insight to elucidate hydrological processes in mesoscale basins. Studies such as this are novel, lacking in the literature, and necessary to better understand the internal behavior of larger scale catchments.

I find this manuscript acceptable for publication in Hydrology and Earth System Sciences, provided that the authors satisfactorily address my criticisms and concerns that are intended to improve the quality of the manuscript. This manuscript is publishable barring moderate revisions and some minor reanalysis.

1) The nature of the isotopic input signature: Even though the authors use a simple approach to estimate residence times, they neglect several important assumptions with respect to the input function. First, this is a lumped parameter model and the authors should demonstrate that precipitation isotopic composition is approximately uniform over these catchments and consistent with the observed precipitation data. For example, the differences in mean baseflow compositions may be an artifact of more depleted precipitation for higher elevation catchments (see Clark and Fritz altitude effect). Second, the input time series is rather short compared to the estimated residence times. It is typical in studies such as this to approximate past inputs using regression with air temperature or other monitoring stations (see Vitvar et al., 1997 and Uhlenbrook et al., 2002). If the authors do not estimate past inputs, then they should discuss this problem since it appears to effect their stream observations at the beginning of the study. If one assumes an exponential residence time distribution, then how far into past are observations needed? Also, the input sine wave could vary from year to year; therefore, longer time series allow for a better estimate of the average input sine wave. It is an important to highlight in the revised text that this sine wave is assumed to hold for the past. The most important issue, however, is that the authors do not take into account the fraction of the precipitation that becomes recharge. It is clear that isotopic mass balance is not achieved (Table 3, weighted precipitation = -9.36L' and streams range from -9.06 to -8.55L'). This shows the importance of weighting the precipitation signal according to the recharge flux of the catchment (i.e., removing the time variation of ET and estimating the proportion of precipitation that infiltrates contributes to catchment turnover) and documenting potential isotopic elevation effects. Also, related to this issue is how the authors deal with snowmelt. See Vitvar et al., 1997; 1999; McGuire et al., 2002; and Uhlenbrook et al., 2002 for examples of recharge estimation. All of these

HESSD

2, S1–S7, 2005

Interactive Comment

assumptions should be addressed in the revised manuscript.

2) Mixing terminology: The use of the term mixing with regards to residence time distributions is unfortunate. In the subsurface, it is general assumed that flow lines cannot cross and therefore mixing cannot occur until that water has discharged to a stream, well, or spring (see discussion in Maloszewski and Zuber, 1998). The term mixing stems from the simple conceptual model of a continuously stirred reactor or reservoir model, which yields an exponential distribution of residence times (see the classic papers: Danckwerts, 1953 and Eriksson, 1971). If the catchment residence time distribution is exponential, it does not mean that the subsurface water is well-mixed, but rather that the flowlines are exponentially distributed (related to catchment/aquifer geometry, soil depth type, etc.) It is acceptable to qualify the term mixing as "effective" or "conceptual." 3) Model selection: It is not clear why the authors use only an exponential distribution of residence times, when other distributions may fit the authors' conceptual model better (e.g., the exponential-piston flow model, EPM can represent the delayed piston-like behavior cited on page 11). Estimating the residence time is significant part of this study; thus, the authors need to address the choice in the model. The EPM has been shown to provide reasonable fits to observed isotopic data in several catchment studies (e.g., Vitvar and Balderer, 1997; McGuire et al., 2002). See Comment 7 below. 4) Discussion: The organization of the manuscript could be improved. The results section is mislabeled since there is much discussion in the results section (perhaps: Results Discussion) and section 5 serves more like a conclusions and implications section. 5) It is interesting to me that mean slope is the most important factor controlling residence time. It explains more variation than any of the other variables, and yet it is perhaps the easiest variable to obtain. How much variance can be explained in a multivariate analysis? Relating these factors to explain what might control residence time and how catchments might organize would be a big step in hydrology. In my opinion, these results (e.g., page 17, lines 15-18) deserve more discussion and are truly the unique feature of this manuscript.

Specific Comments: 1) Page 5, lines 1-2: How is the snowmelt isotopic composition sampled and considered in determining the input? Snowmelt isotopic composition is typically different than fresh snowfall.

2) Page 6, line 2: Was burning factored into the hydrologic process analysis? Burned areas often contain hydrophobic soils and thus may show more responsive hydrology.

3) Page 6, line 25: Were samples collected independent of flow regime (i.e., storm vs. non-storm flows)? This has important consequences for the residence time analysis. It is important to remember that the residence time estimate is for steady-state conditions, which has been recently dealt with using flow volume corrected time (see Rodhe et al., 1996; Kirchner et al., 2001). Steady state is problematic since most systems are not in steady state, but have a time-varying residence time distribution. If a catchment has a more or less even distribution of high and low flows (not substantial seasonality), the steady state treatment may provide some averaged residence time distribution, which is probably adequate. A related problem is the disparity between the 2-3 week precipitation averaging (Page 7, line 8) and the weekly stream sampling. This could lead to a bias in the stream water isotopic composition that will not be indicative of the average residence time distribution, but rather reflect the contribution from stormflow, which is generally shallow and much younger. For instance, a stream sample could be collected on the tail-end of a storm, but the corresponding averaged precipitation may not manifest that storm composition (i.e., it depends on the amount and composition of other storms included in the averaging), then the input will not reflect that stream sample. These issues should be discussed as related to the residence time modeling assumptions, since flow regime is a significant point of discussion later in the manuscript.

4) Page 7, lines 2-3: “A further two sampling sites” should read, “Two additional sample sites”

5) Page 7, line 25: It’s my understanding that X is a regression coefficient equal to

HESSD

2, S1–S7, 2005

Interactive Comment

the y-intercept of the equation and thus, may not be equivalent to the mean annual measured d18O, although it will be close. The reference given in DeWalle et al. (1997) (i.e., Bliss, 1970) shows that X is determined as such: d18O = a*sin(ct) + b*cos(ct) + c where a, b, and c are regression coefficients and c = X. The amplitude and phase are then computed from a and b.

6) Page 8, lines 8-9: Delete extra “and” and change “is” to “was”

7) Page 8, line 10-19: The author did not provide adequate justification for only using the exponential distribution in the sine wave residence time analysis. Sine wave solutions exist for the dispersion model (Maloszewski et al., 1983) and the exponential-piston flow model (Asano et al., 2002). The latter seems appropriate given the authors’ discussion on page 11, lines 18-22. Also, it is not clear what the authors mean by “complex” in line 18. Kirchner et al. (2000) present one other potential model (a gamma function with ? 0.5); however, the models I listed above are equally complex (i.e., they all have two parameters). It is recommended if the authors justify the use of only the exponential model, then Maloszewski and Zuber (1982) be added as a reference in line 19. The additional analysis using the models proposed above would not be excessive considering that it could easily be done using Excel (or other spreadsheet) and the current amplitude ratios from the manuscript.

8) Page 8, line 23-24: The precipitation means should be weighted means for comparison to the stream compositions, which are naturally volumetrically weighted. These values should approximately balance if recharge is considered.

9) Page 11, line 1: Switch Figure 3 order with Figure 4, since the discussion of Figure 4 is first in the text.

10) Page 13, line 29: should read “notable seasonal differences in d18O”

11) Page 14, line 27 to Page 15, lines 1-2: It is not clear how the authors “optimise” precipitation to weight the seasonal extremes. This is very important to describe the

method used here since it relates to the recharge concept discussed above. It seems as though the authors correct their data to represent recharge. Typically, precipitation amounts are weighted by an empirical infiltration coefficient that adjusts the input composition represent to recharge (see Maloszewski et al., 1992; Vitvar and Balderer, 1997; McGuire et al., 2002).

12) Page 15, line 24-25: What effect does elevation have here? Also, this highlights the problem of mass balance.

13) Page 16, line 28 to Page 17, lines 1-3: Some description of how these characteristics (e.g., responsive and freely drained soils) were determined is necessary for the reader evaluate the significance.

All references should be doubled checked. Page 4, line 14 should specify Rodgers et al., 2004a or 2004b. The references Darling et al., 2003 and Boorman et al., 1995 are missing from the reference list. Page 24, Vitvar and Balderer should be 1997 not 1998.

References:

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HESSD

2, S1–S7, 2005

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Vitvar, T., et al. (1999), Application of GIS-based distributed hydrological modelling for estimation of water residence times in the small Swiss pre-alpine catchment Rietholzbach, in Integrated Methods in Catchment Hydrology, IAHS Redbook No. 258, pp. 241-248.

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