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Hydrology and
Earth System
Sciences
Discussions

Interactive comment on "A conceptual model of flow to the Waikoropupu Springs, NW Nelson, New Zealand, based on hydrometricand tracer (^{18}O, Cl,^{3}H and CFC) evidence" *by* M. K. Stewart and J. T. Thomas

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We would like to thank the anonymous referee #2 for his/her thoughtful general comments and many detailed comments. We consider the comments very useful. Our responses are given below.

General Comments:

Sentence 1-4. We agree with the description of our work. Sentence 5. The referee suggests (1) emphasising the hydrological/hydrogeological context of the work, and (2) presenting the physicality (physical disposition?) of our conceptual system. We think these are good ideas, which we will try to implement in the final version. Sentence 6-7. In the Results section, the referee suggests separating previous evidence (on water flows) from current results (to allow more clarification and explanation of the latter). We will move in this direction in the final version. Sentence 8. Then discuss Table 1 in the Discussion. OK. Sentence 9. Emphasise the permeabilities and porosities of the three aquifer types to undergird the dual porosity concept and the fissures dominance in the AMA. We will see what information we can gather on this, and where it leads us in terms of conclusions. Sentence 10. What are the meanings of the DP values of 0.6 and 0.12 in the context of the AMA or other karstic limestone aquifers? We will address this question.

Specific Comments:

Introduction. We agree with much of the suggested word changes and minor suggestions, and will implement them. It is probably true that pulse train analysis shows hydraulic connection, not actual flow connection. Hydrogeological Setting. Again, we agree with many of the minor suggestions and word changes. We will give typical estimates of hydraulic conductivities where appropriate.

Sampling and Methods.

Sections 3.1-3.3. The referee suggests wording changes and putting in more detail on the measurement methods, which we will do. We will move the mention of Vanessa Fox's training on CFC methodology by USGS to Acknowledgements.

Section 3.4. (Residence time determination methods) Word changes - we can do some of these. Describe how we quantify effective recharge - we can put in a sentence here, but the more detailed explanation needs to be given with the isotope or chemical in question. Put in general reference on convolution - OK. Equations 3a and 3b do not imply piston flow and exponential functions respectively, and we prefer the present text order (exponential then piston flow) because that is how we envisage it occurring in nature (i.e. recharge (exponential) followed by parallel flow in the aquifer (piston flow)). As noted by Referee #1, the parameter D/vx (from the 1-D version of the ADE) is not applicable in the lumped parameter approach. Instead, we will use the dispersion parameter (DP) defined by the referee as the mass of the variance of the dispersive distribution of the transit time. We prefer to describe the two-component model (DDM) as two dispersion models in parallel and avoid the term mixing model (which Referee #1 takes exception to), on the understanding that the components combine at or near the spring outlet and not mixed in the aquifer. We will identify the five parameters and consider changing the paragraph order.

Results. We agree with many of the minor suggestions and suggested word changes, and will implement these where appropriate. Other comments and questions are discussed below.

Section 4.1. Comment: Move previous hydrometric information from Section 4.1 to Section 2 (Hydrogeological Setting) as suggested in the General Comments. Reply: We will implement as appropriate. Question: Are there any discernible lags in the AMA recharge sources? Answer: The three types of recharge sources have different time lags, but all are small. Tritium measurements for Upper Takaka River have shown that the river water has at most a few months’ residence time in its catchment (Taylor, 2000). Valley Rainfall recharge has effectively zero time lag, because any such lag is part of the residence time estimated from the tritium measurements. It is also clear that the Karst Uplands recharge will have very little time lag because the sinking streams supplying the water arise from impervious bedrock (granite, schist) with very little water-detaining capacity. We will address this question briefly in the final paper. Q: When do the lower reaches (of streams crossing the karst uplands) run dry? A: During most of the year, only peak flows following high rainfall reach the Takaka River. Q: Is the (Lower) Takaka River therefore seasonal? A: Yes, with lowest flows in late summer, and highest flows in winter. The Upper Takaka River flow is also seasonal.

Section 4.2. Q: Where is the evidence for asides such as (Much of the excess, which flows out to Golden Bay, occurs in winter)? A: This and other comments on flows in the paper are based on extensive evidence accumulated over the years principally by the Tasman District Council (some of this data is reported in Mueller (1991, 1992) and Edgar (1998)). Qs: How are the (δ18O) averages weighted? By precipitation amount? How are the δ18O values to be corrected (cf. line 7) for the selection of winter rainfall? A: The averages given in Table 2 (partly used as the basis for the estimated δ18O values for the recharge given in the text of Section 4.2 and in Table 4) are (unweighted) averages of the unweighted yearly means. Calculated weighted mean δ18O values of recharge from the Takaka River (weighted by the amount of loss by seepage from the river) and of recharge from valley rainfall (weighted by the rainfall amount minus estimated evapotranspiration) were also considered. The calculation procedures for these are fully explained in Section 4.5 (see also Stewart et al., 2007). Q: What is the level of confidence for the Student's t-Test? A: As given in the paper, the probability of the Main and Fish Spring δ18O values not being different is 0% (actually 0.0%) according to the paired Student's t-Test (t = -13.5). Expressed differently the difference between the δ18O values of the springs is 0.31±0.05 permil (with 95% confidence limits). Q: If you are going to compare the flow-weighted means then what error bounds are you putting on the models to assess that there is a better fit and/or what goodness-of-fit criterion (cf para 4, line 1 suggesting that the more complex model better matches the observed delta-values)? A: We examined the Mueller and Edgar models to see how well they predicted the δ18O values of the springs. They led to poor matches, and therefore the models were judged to be incorrect (i.e. inadequate). They also do not account for the different compositions of the Main and Fish Springs. The estimated flows from the recharge sources were apportioned between the springs in our (more complex) model so that the model exactly reproduced the mean flows and δ18O values of the springs (except the remainder). Hence, it is not appropriate to test the fit with a goodness-of-fit criterion.

Section 4.3. Q: Why are hydrochemistries not discussed first to bolster the flow models and support then the stable isotope estimates (i.e. then moving logically through to the more sensitive system tracers)? A: The hydrochemistry was discussed briefly and references given to previous chemical studies of the springs. Q: If Ca, HCO3 and Sr show effects of interaction with carbonate rocks are these parameters also then indicators of water maturity (residence times)? A: It would certainly appear so, and it would make an interesting study, but outside the scope of the present paper. Comment: If the major ions indeed suggest (conservative) mixing with seawater then this could be shown directly on Cl-correlation plots. Reply: Yes, we originally had plots showing such conservative mixing with seawater very nicely, but took them out. The samples were from Main Spring at 3-monthly intervals. Q: What does low flow and high flow mean (in relation to m3/s)? A: Main Spring flows of about 8 and 12 m3/s. Q: Does low flow represent therefore baseflow (supported therefore by the shallow system)? A: Yes, but the term baseflow doesn't apply well to the two-component nature of the Pupu Springs. Q: Question re the chloride concentration assumed for freshwater. A: Freshwater chloride content is low (2-3 mg/l, see Table 5). Q: Do you not have δ2H values to check for evaporative concentration effects? A: No. Q: What does low Cl and high Cl mean (in relation to mg/l)? A: 2 and 124 mg/l (given in Table 6). Comment: Reflect again here why the deep system is isotopically heavier end member. Reply: The critical observation is that the δ18O values of monthly grab samples from the Main Spring become heavier (less negative) as their chloride concentration increases (Fig. 5b). This shows that two water components are involved. The deep component has a recharge source that gives it heavier δ18O values than the shallow component. Comment: Show the regression line equation and s(y,x) on Fig. 5b. Is this really a fit as the Shallow system seems to be an extrapolated end-member value? Reply: The line shown on Fig. 5b is not a regression line, but a line connecting the compositions of the deep and shallow system waters, which were estimated based on the flows and δ18O values in Table 4. The equation of the line is δ18O = 0.0057.Cl - 7.90. The regression line, calculated with δ18O as the y-variable because there is much greater relative error in the δ18O measurements than in the Cl measurements, is δ18O = (0.0046 $\pm$ 0.0008).Cl - (7.82 $\pm$ 0.08) (R2 = 0.48), which is very similar to the line in Fig. 5b. This supports the 2-component conceptual model. I don't know what is meant by s(y,x). Comment: Looking at these plots (Figs. 5a and b), I would suggest that in fact there are two slopes represented in these figures with samples Q $>$ 10m3/s and δ18O $>$ -7.5 permil on shallower slopes? Reply: No! Fig. 5a shows Springs River flow versus Main Spring chloride concentration, as explained in both the figure caption and the text. Springs River contains the flows from the Main Spring, Fish Creek springs and Fish Creek itself, so is greater than the Main Spring flow especially at higher flows. It cannot be used to examine the linearity of the response. The data in Fig. 5b are most logically interpreted as displaying a single slope, given the scatter to be expected (predominantly) from the error in the δ18O measurements. It is clear that there is one slope, it is not at all clear that there are two or more slopes.

Section 4.4. Q: How does applying the recharge model of Table 4 get you the precipitation-weighted mean altitudes? Has an altitude regression been applied to precipitation and/or δ18O of precipitation (is this implied in para 3)? A: The recharge model of Table 4 gives the percentage of water from each source; e.g. the Main Spring receives 74% of water from karst uplands (with mean recharge altitude of 460 m), 18.5% from Upper Takaka River (1100 m), and 7.5% from valley rainfall (100 m). The weighted altitude of recharge is then 546 m. Q: How is it known that recharge is predominantly from sinks unaffected by evaporation? A: All workers have agreed that recharge to the AMA is predominantly from sinks (especially recharge from the Upper Takaka River and Karst Uplands) (Mueller, Edgar). The waters flowing into the sinks are affected by evaporation while being collected in their runoff channels (i.e. Takaka River and Upland streams). Comment: Perhaps state here the (atmospheric) half lives of the CFCs. Reply: OK. Q: What temperatures and atmospheric pressures of recharge have been used? A: Temperature 12.5 oC and atmospheric pressure equivalent to 100 m altitude. Q: How is it known that CFCs have not been subject to degradation or contamination? Or are these presumptions for modelling purposes? A: These are assumptions. However, we have only found degraded CFCs in waters showing obvious signs of chemical reduction, and the Takaka waters are not reduced (Michaelis, 1976). And when we have found contamination, the CFC-12 concentrations have generally been markedly enriched. There is no obvious evidence of that here. Comment: It is likely that any subsurface samples (although less so for surface waters) do have an excess air component. The USGS approach using N2/Ar to define the excess air component (and RT if you can presume the altitude/atmos pressure of recharge) is fine if you can preclude any denitrification sources for N2. Is the argument then that because the CFC-11 ages are similar to 3H ages, then this presumption is reasonable? Note that Herzberg & Mazor suggest that sinks and sinkholes might be associated with excess air entrainment. Reply: We agree that excess air entrainment is quite common in groundwaters and reasonably likely in this situation. However, we have taken the fact that the 3H and CFC-11 ages agree for the Main Spring with the 2-component model as an indication that excess air, degradation and contamination problems have not affected the results. As noted above, we think it likely that the latter two are not affecting the results. We have applied the USGS approach (using N2/Ar to define the excess air component) in some later studies. Q: re Fig. 6b caption Function. A: Function is the response function (h(τ)) from equation 2 (otherwise known as the residence time distribution or RTD). The 2-component model is the sum of the two DMs, which if plotted separately would overlap only in the area where there is the abrupt change of slope. The peak at short residence time comes from the DM with MRT of 1.2 yr, while the long tail comes from the DM with MRT of 10.2 yr. The SD expresses the least squares fit of the data about the various models. Q: re summary of model details. A: We agree that the summary details of the models should be tabulated. Q: Does the result that CFC-12 has younger age estimate suggest that there might indeed be an excess air component? A: Possibly, however this frequently occurs as we noted. We have felt that maybe our atmospheric input data for CFC-12 is incorrect. Q: Why is an error of ±0.2 assigned to the estimate of b based on the tritium measurement for Fish Spring? Likewise ±0.1 for CFC-11? A: The errors are estimated based on the relatively weak discrimination given by the goodness-of-fit criterion, in order to give the reader a feel for the likely accuracy of the determination of b. No statistical protocol has been applied. Q: Why would zero tritium and CFCs imply 100-years residence time (at least) for the deep component? A: This depends on calculations assuming that the tritium/CFC concentrations are $<=$0.05 TU or 8 pptv (i.e. twice the measurement error for zero concentration). For example, with the EPM model, the estimated age would be 100 years or more for any EPM model with $f<0.58$ (tritium), $f<0.74$ (CFC-11) and $f<0.95$ (CFC-12). Q: Why is it reasonable to presume that the older water component comes from the porous matrix (how permeable is this)? A: This presumption comes from the greater age of this water. We need to get more data on the porosity and permeability of the matrix to understand this better. Q: Are you implicating the primary fissures distribution/apertures at depth as the source/restricted permeability of the AMA at this depth. Is there other evidence of this (other than reference to the drawdown)? A: order.

5. Discussion.

Section 5.1. Comment: The information in Section 5.1 could be moved to the Introduction. Reply: We will consider this option further in revising the manuscript, but our initial preference is to retain the comparison between the two conceptual models (as in the status quo). Section 5.2. Comment/Reply: Our ideas as to why the two-component system has developed are given later in the Discussion, but agree it could be useful to lead up to them with a sentence or two here. Q: Do you mean unlike that illustrated in Fig 9a etc. A: No, we meant like that illustrated in Fig. 9a as written, but will try to make our meaning clearer. Q: The two following sentences etc. A: Yes, we can tidy up what is in each section. Q: I am not entirely clear why conduits (major solution channels) are invoked specifically, unless it is to invoke focussed transport towards the springs complex etc.? A: We think a major conduit or conduits must be involved in channelling flow to the Pupu Springs (as well as the presence of the diorite sill), because of the very substantial flow (7.4 m3/s on average from the deep system at the Main Spring) and because such a large proportion of the known recharge of the deep system emerges at the springs (80% at Main Spring). However, we think such major conduits probably act as collecting vessels (or short-circuits through to the springs) from smaller channels/fissures and are not present throughout the AMA system. Q: But how is this seen in the modelling then? A: The modelling only goes so far. At present, we are not clear about the relative roles of fissured-porous matrix and karstic channels in the deep system. The modelling may not tell us if the porous matrix is almost unseen, at least by water feeding the Pupu Springs, because of very low permeability/porosity in the matrix. Q: High piezometric levels implies artesian conditions? A: What was meant here was that the piezometric level was above the level of the AMA and within the capping low-permeability Motupipi Coal Measures. The level may be artesian at some places, and certainly is at the Pupu Springs locality. Q: Do you mean that for the dispersion system diffusive exchange is invoked between the (mobile water) fissures and the (relatively immobile water) porous matrix? Or is it diffusion from channels (secondary fissures)? How is this adapted in your modelling? A: This is a question we are struggling with and will address further in the revised version. Q: Why is it possible that older water is resident in less accessible parts of the porous matrix? Surely the porous matrix is low-permeability and any exchange is then diffusively controlled rather than head-potential controlled? A: Yes, and this is a problem for the age modelling, because the porous matrix could act to suck the age (i.e. tritium/CFCs) out of the fissure water, if the porous matrix volume that is accessible is significant. Conclusions. Q: Does the system really burst upwards at the Main Spring complex? A: Perhaps surge is a better term, considering that the main pool shows a doming of about 0.5 m, the flow is large and water in the AMA is drawn powerfully towards the springs. References, Tables, Figures. Thanks for the useful suggestions.

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Interactive Discussion

Discussion Paper