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Interactive Comment

Interactive comment on "CCA transport in soil from treated-timber posts: pattern dynamics from the local to regional scale" *by* B. E. Clothier et al.

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Authors' responses to commentaries on "CCA transport in soil from treated-timber posts: Pattern dynamics from the local to regional scale" by B.E. Clothier et al.

The three referees of our paper have provided critical and incisive commentaries. Whilst we disagree with their editorial conclusions, we are happy to respond to the major issues they raise. Indeed, we share their procedural and methodological concerns, for we, ourselves, were challenged by the scientific tasks set for us by the environmental protection agency of the Marlborough District Council - What is the likely long-term, regional impact on groundwater systems of the use of arrays of CCA-treated timber posts in vineyards installed at a density of 580 ha-1 and replaced on average every 20 years? This risk assessment required we develop a cascade of simplifying assump-

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tions, which the first referee notes "are undoubtedly necessary, and which determine the results and conclusions". Agreed.

The constraints of space limited our ability to describe more fully the applicability of many of the assumptions, and the uncertainties that might arise through spatial heterogeneity and modelling. Our responses here seek to redress that.

The first referee has clearly listed the 9 assumptions that we needed to enable us to carry out these long-term regional risk assessments. The reviewer rightly notes that the prime assumptions are his first 4 points relating to the prediction of the average release rate of arsenic from the post over its 20 year lifetime: T (mg-As yr-1 post-1) in our Eq. 4. We agree this is critical, and our whole approach relies on getting this right, for its role in Eq. 4 is paramount. To do this we used posts immersed in tanks to obtain what we considered a robust estimate of post leakage. It seems to us that a first-order kinetic is, ceteris paribus, a good first approximation, and our tank data do not hint at any non-linearities (Referee #1's first concern). Next, we did not consider the loss to be equal across the entire buried surface, indeed we use the literature value that flux densities out the end-grain are 40 time greater that those radially (Referee #1's 3rd point). That leaves this referee's points 2 and 4; which, we agree are critical.

Point 2: The flux measured in the tank can be modified for field use by multiplying by the 'contact area' of the saturated water content. This, we agree, is a critical assumption. First, we wished to adopt a prudent measure to provide a worst-case scenario, for in some cases shallow water tables do encroach into the top 600 mm of soil. Second, the soil around the post is in the herbicide strip, so the soil is always bare, so that the depthwise dry-down is the soil will never be that deep, so the soil is likely to remain near its saturated value. The vines are generally about 1 m away from the posts, and they are irrigated, so the vines' active roots are unlikely to venture close to the post.

Nonetheless, turning our tank observations of As flux into field estimates is a key point, and one that will have an unknown, but potentially a significant degree of uncertainty.

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Our tank observations and modelling suggests that the As loss rates from posts to be of the order of T around 5-7 mg-As month-1 post-1 (Fig 4). We sought to support this using other data. From the survey data of Robinson et al (2006), for posts of varying ages, it is possible to compare the loss of As from the below-ground portion, with that above ground to obtain an estimate of the subterranean flux. The posts in Fig 2 (II) of Robinson et al (2006) were of varying ages, but had, on average, been in the ground for around 5 years. The difference in the As between the aerial and buried portions was around 700 mg-As, and we can ascribe that difference to the loss of arsenic from the post into the soil. A rough calculation from these data gives an estimate of a blow-ground loss rate of T of about 8 mg-As month-1 post-1, although the survey estimates would be variable across the various sites. This is of the same order as that we suggest in Fig 4. Furthermore, we have been collecting and analysing leachate in bucket lysimeters (plastic rubbish bins) set up in the field with a post inserted into the repacked soil contained within the bin. These data also provide order-of-magnitude support for the value of T we predict from our tank-immersion trials.

Point 4. The first referee queries our assumption that all transport of the leaked As occurs within an annulus of 50 mm surrounding the post. This is a necessary assumption for our SPASMO modelling, and we chose 50 mm on the basis of our knowledge of the hydraulic properties of the generally coarse-textured alluvial soils of Marlborough, combined with our survey observations of As (Robinson et al. 2006) around the posts. Water flows in these soils are generally dominated by gravity, cf capillarity so lateral diffusion is likely to be weak. Again, we can derive general support for this metric using the survey data of Robinson et al. (2006). Within the errors expected from such survey data, and by assuming a simple annular geometry, the As we observed to be missing from the post (Fig 2 (II)), we recovered in the 50 mm zone around the post (Fig 3 (I)).

For the long-term, the radial distance we assume is not that important. It is the flux of As quitting the post that is crucial: T in Eq. 4. The metric might influence the time taken to quench the adsorptive capacity within that annular volume, but since that is

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likely to occur on the timescale of 100-200 years (see Fig. 6), it would not likely affect our 500-year results.

Thus in response to the first four points of referee #1, and in part raised by the two other reviewers, we consider that the our predictions in the long-term, and at the regional scale are robust, for we have identified the key role of T at the local scale, and through our tank experiments, supported by other observations, we have been able to provide predictions of value to policy agents.

All the reviewers, especially the second referee, have queried our assumption of bare-soil conditions, and these suggest that we should have considered root water uptake. We do not agree, for the reasons we hinted at above. The posts are aligned within the 1 m wide herbicided strip underneath the vines. Thus there is no surface vegetation. Further, the nearest vine is about 1 m away, and since these vines are irrigated, the vines would preferentially extract the lion's share of their water uptake from within the zone wetted by the dripper.

Another major concern was our use of our model SPASMO and the 'Ě applicability of the water capacity modelling approach with a (arbitrary) slab height of 10cm for the current process must be discussed for the transport of a sorbing chemical.' We have used, and verified, SPASMO across a range of field conditions, including its use with sorbing chemicals. We did cite Rosen et al. (2004) for that contains a description of the model's structure. We were remiss, however, in not citing its very recent use with tracers and pesticides, and its comparison with field observations on two very different soils in New Zealand. These papers are Sarmah et al. (2005) (Australian Journal of Soil Research 43:471-489) and Sarmah et al. (2006) (Australian Journal of Soil Research 44:581-597). SPASMO was compared with LEACHM, HYDRUS-1D, and GLEAMS, and the models sought to predict the field behaviour of bromide, plus the sorbing (and degrading) pesticides of atrazine, diazinon, hexazinone, procymidone and terbuthylazine. Sarmah et al. (2005) concluded that "Ě SPASMO simulations were reasonable considering [unlike the other models which were 'calibrated'] that only

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uncalibrated parameter values were used”. The simulations were compared with field measurements over 550 days following the application of a cocktail of pesticides to field soils.

In our application of SPASMO to the leaching of CCA from the posts, we did however have the advantage of an estimate of the source strength T, and field measurements of the strength of As adsorption - albeit by assuming a linear isotherm, which for our range of concentrations we consider to be reasonable.

Another concern raised by the reviewers was our consideration of preferential flow in macropores. Over the years, we have carried out campaigns of measuring the soil’s saturated and near-saturated hydraulic properties in Marlborough using disc permeameters. We have good data on the soil’s macroporosity and mobile water contents, and these values were used in SPASMO. The third referee raises an interesting point about the possible role of the post itself in intercepting rainfall and creating ‘post’ throughflow. Certainly, this would be a factor during winter when the vines had no leaves. The viticultural area of Marlborough is within the 700 mm isohyet, and because it is a Mediterranean climate of winter-wet/summer dry, post flow could be a problem, however, as yet we have little idea of its impact. Minor, we would suggest. Our bin-lysimeter trials could help with assessing its impact.

Finally, another concern raised by the first referee was our assumption that the aquifers were well mixed (our Fig 7). This is a little beyond the ambit of our remit from the policy agency, but is of course a scientific issue. The groundwater contamination concerns were mainly centred on the potential As contamination of those surface waters resulting from the efflux of the Wairau when it ‘hits’ an aquiclude halfway down the Wairau Valley (the base of the isosceles triangle in Fig. 7). Some 90% of the aquifer rises to feed surface streams. It would seem that this rising would be quite an effective mixer. The other concern was the coastal Rarangi aquifer which eventually wells up into the sea - another effective mixer. The Southern Valley aquifers eventually feed into the Wairau system, where they will eventually become mixed. So for those fates there is probably

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effective mixing. Nonetheless, if the concern were the use of the aquifers as sources of water, the referees' concerns would stand.

We thank the referees for their perspicacious comments, and we think that their published concerns, and our responses here, will merit publication of our work.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 2037, 2006.