

## ***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.***

**Anonymous Referee #2**

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This paper assesses the potential contamination of groundwater by heavy metals that leach out of treated-timber posts. Treated-timber posts are widely used in wine growing areas but I suppose that there exist other important examples of a wide spread application of such posts as well. Therefore, I think that this study is a nice example of applying soil plant models and hydrogeological models to solve practical problems. Important issues are the quantification of the heavy metal flux from the timber posts to the surrounding soil, the mass flux of the heavy metals from the bottom of the field towards the upper aquifer, and the dilution of this mass flux with uncontaminated groundwater. The authors present a set of modelling approaches to assess these different fluxes. However, a few crucial aspects are missing or insufficiently discussed in this paper. At the field-scale, I think that is important to present a field scale water balance since

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the amount of water that drains to the groundwater or 'leaves the system' directly determines the long term heavy metal concentration in the water that percolates to the groundwater since it is assumed that the amount of heavy metals that are released from the posts depends only on the heavy metal content of the posts. To estimate the concentration in the water that recharges the groundwater, the amount of heavy metals released per year should be divided by the amount of water that percolates to the groundwater. This amount is the rainfall minus runoff, soil evaporation and plant transpiration. In their model calculations, the authors only consider soil evaporation. This may be justified for the simulation of heavy metal concentrations in the vicinity of the posts when it can be assumed that there are no plant roots there. First, it must be questioned whether this assumption holds true. Second, at the field-scale, plant transpiration and root water uptake must not be neglected. If root water uptake is neglected, the amount of water leaching to ground water is over estimated and as a consequence, the heavy metal concentrations are under estimated. Even if the concentrations are correctly estimated in the water leaching from the cylindrical soil volume around the post, the amount of water leaching from this column is larger than the amount of water leaching from a similar column with root water uptake. Therefore, the water leaching from the cylinder with a post will be diluted to a lesser extend by non-contaminated water when plant water uptake is considered. Thus, the field scale heavy metal concentration will be under estimated if plant water uptake is neglected. The assumption that heavy metal flux from a field can be simply obtained by multiplying the concentration leaching from a bare soil cylinder around the post by the fraction of the area of these cylinders to the total area of the field is to my opinion not justified. To assess the dilution of the water leached from a contaminated field plot by non-contaminated water in the aquifer, the recharge from the contaminated area should be compared with the recharge from the non-contaminated area into the aquifer. For the Wairau aquifer, the dilution factor is about 20. That means that the area which recharges to the aquifer is about a factor 20 larger than the area of the vineyards. Looking at figure 8, the percentage of the area grown with vineyards seems to be much larger than 5% in the Wairau aquifer.

Therefore, I guess that the area from which this aquifer receives its recharge is much larger than the area shown in Figure 8. On the other hand, assuming an As concentration under the vineyard of 12.5 ppb and an exit concentration of 13 ppb in Brancott area, it seems that the Brancott aquifer is only recharged from the vineyards. Rather than considering recharge from different areas to calculate the dilution, the authors use information about the water flow in the aquifer and assume that the input concentration is 0. This assumption may hold true when there are no vineyards outside the area outside for which the flow is calculated. Looking at Figure 8, I wonder whether this may be assumed in this particular situation. A second assumption which is implicitly made when the Darcy flux is estimated from the water velocity, which I assume is derived from some kind of tracer test, is that water is flowing through the entire pore space. There are several examples and situations where this is not the case and water is flowing rapidly through only a small fraction of the pore space, or the effective porosity. In that case, the Darcy flux is overestimated when the water velocity is multiplied by the total porosity and not by the effective porosity and this overestimation of the Darcy flux leads to an overestimation of the dilution.

Summarizing, I think that the authors should revise their calculations of field scale heavy metal leaching taking root water uptake into consideration and should discuss the uncertainty in their simplified hydrogeological model calculations.

Detailed comments:

I feel that there is a need to give some more details about the water flow modelling. For instance, bare soil evaporation is modelled using the approach presented in Allen et al (1999). In this approach, the bare soil evaporation is coupled to the potential evapotranspiration using a 'crop factor' or Kc factor. The Kc itself depends on the available water in the top soil layer. Up to a certain water content, evaporation is controlled by the atmospheric demand (stage I) and the Kc factor is kept constant. When the critical water content is reached, the Kc factor is assumed to decrease linearly with decreasing soil water content until the total available water for evaporation is depleted. Critical

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parameters in this model of bare soil evaporation are the depth of the soil layer from which water can be lost through bare soil evaporation and the amount of water that can be lost during stage I evaporation. These parameters are also soil dependent. I would suggest to briefly give this information in the paper. I also suggest to refer to electronic version of the Allen et al. FAO report, which is accessible via the internet.

I think that there should also information about the parameters used in the water-capacity model. For instance, what were the values for the mobile and immobile water contents, what was the exchange rate between the mobile and immobile zones, was it modelled assuming a first-order exchange rate concept or was a sorptivity approach used? I would also like to know how the water redistribution within the mobile zone after a rainfall event was modelled. Besides the exchange with the immobile zone, the vertical redistribution after rainfall within the mobile region may also be important.

P 2043 In 13-14: I do not directly understand how the factors  $1/3$  and  $2/3$  for the end and side faces are linked to the difference in diffusivity of 40. I guess also the areas of the end and side faces play a role.

P 2045 In 5: Using a linear fit to the concentration-rise data, it is assumed that  $P_i$  remains almost constant with time.

P 2047 In 16-18: I do not fully understand what the authors mean by a ‘quenched soil sorptive capacity’. But, if a linear sorption isotherm is assumed, then the sorption capacity of the soil is assumed to be infinite and can never be ‘quenched’.

P 2049 Eq. 5: I think there is a  $v$  missing in the second equation.

P 2050: I think that the authors should give some information where the estimates of  $v$  and  $\epsilon$  come from.

P 2051 In 7-9. I think that it is wrong to say that the concentrations in the leachate are affected by the soil chemistry. If linear sorption is assumed, then soil chemistry has no effect at all on the steady-state concentration. The steady state concentration is only

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determined by the rate of release of the heavy metal concentrations and the water flux in the soil.

Figure 6: I have serious questions about Figure 6. It seems unrealistic to me that breakthrough at 1 and 2 m occur almost at the same time.

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