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Hydrology and Earth System Sciences Discussions

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 #3

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General Comments

This paper provides an assessment of the long-term (˜500 yrs) impact resulting from the use of chemically treated wood posts in vineyards in New Zealand on local and regional water quality, the latter which correspond to a six order of magnitude spatial, and four order temporal, upscaling. Of the several chemicals released from the posts to the environment, the study focuses on arsenic, as it is deemed the most mobile and toxic of the resulting pollutants. A water-capacity model was employed to simulate water flow and chemical transport dynamics for three soil types typical of the region at the "post" scale over a 500 yr period, with post replacement every 20 yrs. The forcing for this model relied on a prior 32 yr record of daily rainfall and potential evaporation as observed at a research center within the region. Model parameterization

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for sorption/desorption processes was achieved by extrapolating previous laboratory batch studies using estimates of organic carbon for the three different soils modeled in the study area and evaluating chemical leaching over a 1 yr period from submerged posts in lysimeter studies. In addition, other "soil physical and hydraulic properties were deduced from data contained in the NZ Soils Database of Landcare Research or by using...measurements made earlier at Rarangi" (the latter being one of the three soil types studied).

As a reviewer of this manuscript, I was somewhat frustrated at the lack of detail regarding the procedures. For example, did they calibrate the bare soil evaporation process for each soil type within the water capacity model framework (e.g., such as the stage 1 coefficient relating potential and actual evaporation, the accuracy with which the water capacity model, with a daily time-step, characterizes the cumulative surface flux for transitions between rainfall events, stage 1, and stage II evaporation, are there seasonal biases associated with the evaporation modeling, etc)? Since the posts are intended to support the plant canopy, how did they model the impact of the canopy on bare soil rainfall interception and evaporation adjacent to the post? Further, as demonstrated by the cited reference of Hutson and Wagenet (1993), there are markedly different chemical leaching results obtained using the water capacity model depending on the arbitrary rules employed for partioning water and chemicals between mobile and immobile regions, and redistribution in the profile. How did the authors arrive at an appropriate formulation of the model for each soil type? Is this model biased for certain types of rain events due to non-linear processes such as preferential flows? It seems that this would have required field studies at the least to calibrate the model.

When considering the magnitude of the spatial and temporal upscaling attempted in this study, I believe it is unacceptable to ignore the role of spatial and temporal variability in properties and processes, as well as uncertainty in model parameter values, with respect to interpreting model output. The very fact that this exercise represents a forecast effort that spans a 500 yr period leads one to question the objectivity of the results. However, this objectivity is brought further into question since the authors apparently ignore the uncertainty involved in the simulations. The proposed upscaling involves processes which are highly non-linear (water flow in unsaturated soil and chemical fate), and "post"-scale model parameters would naturally vary significantly both spatially and temporally within each acquifer region, and yet there is apparently little known about this variability, or at least this variability is not addressed. It seems that a more objective approach would be to follow along the lines of Finke et al. (1996), Soil Sci. Soc. Am. J., 60: 200-205, who suggest a probability sampling scheme for upscaling from a plot to a regional scale. Other suggestions are offered by Bierkens et al (2000), Upscaling and Downscaling Methods for Environmental Research, Kluwer Publishers, and include formal procedures for incorporating input uncertainty into generating output estimates. Another factor which the authors do not address is the issue of bias in their model results. Given that they do not have observations (for even a season or year) of solute flux at the exit plane below a post, how can they be confident that their model is not biased with respect to quantity and timing of chemical leaching?

In addition to the concerns cited above, I do not see what this manuscript offers in terms of new insights or observations regarding hydrology or environmental systems modeling. This work does not represent a scientific advance in our knowledge of hydrological systems. As a consequence, I recommend that the paper not be accepted for publication.

Specific Comments

Posts are used to support the grape-vine canopy, suggesting that for much of the year there exists a leafy canopy over the post, which must surely impact local water dynamics (e.g., does the water extraction pattern surrounding the grape root system result in lateral gradients in flow away from the post?). How do you partition the energy balance for evapotranspiration? You model the 5 cm ring of soil surrounding the post as a “bare” soil, but in doing so how do you account for the diminished evaporation rate resulting from the reduction in solar energy that occurs when an overlying canopy is in

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existence?

Three posts were used to assess the rate, over a 1 year period, at which chemicals are released into surrounding water. Were these posts from the same batch and same supplier? How much variation would occur if a different supplier/batch were employed?

How does the insertion of the post into the soil impact local hydrology? I assume that these crops are strictly rainfed, is that correct? Or are there supplemental irrigation systems in use? Also, when rainfall occurs, is water preferentially funneled down the sidewall of the post? It seems likely that such a phenomenon would occur, especially during rain events with windspeeds in excess of, say 4 m/s. How does canopy interception of rainfall influence the uniformity of rainfall on the soil surface? Given that the posts are rammed into the ground, is there a depression or ridge immediately adjacent to the post that would disturb the local hydrology? If so, how do you simulate this factor in your model? Also, wouldn't you expect that over a 20 year period, preferential flows would increase along the post wall within the soil (e.g., perhaps due to degradation of the wood over the 20 year lifespan)? In addition, it seems preferential flows would be further enhanced given that every 20 years the old post is removed, and a new post is mechanically rammed into the same hole for the duration of the 500 yr simulated period. Wouldn't such a procedure tend to decrease wood to soil contact?

You use only a 32 yr met record obtained at one spatial location (i.e., the research centre within the region). I would expect the annual rainfall to be spatially variable within the region of interest, possibly higher adjacent to coastal areas in contrast to more inland locales. What was the average annual rainfall for the 32 year record?

Are there possibilities to devise a relatively inexpensive system of post installation that provides a flow barrier (e.g., a mound surrounding the post, or a tight-fitting plastic shield near the soil surface, etc.) for the contaminated region, at least near the soil surface?

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