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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 #1

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The paper describes a modelling case study of the release of CCA (Copper, Chromium, Arsenic) from treated timber posts in the viniculture area of Marlborough in New Zealand. Processes included are the post-to-soil release, the vertical transport through the soil to the groundwater, and the transport in some aquifers that are important in the study region. The transient modelling is based on a series of assumptions and performed with daily time steps. The predictions for the groundwater concentrations extend up to 500 years from now. The paper comes to the conclusion that it is likely that Arsenic concentrations will exceed New Zealand drinking water health standards.

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Interactive Discussion

Discussion Paper

1 Comments

The paper is well written, pleasant to read and easy to follow. It appears that it is a distillation of a recent Research Report (Vogeler et al., 2005) and a modelling extension to a recent paper by Robinson et al. (2006) where results of a soil survey on CCA are reported.

The problem of the paper is, however, that it contains no scientific progress. The modelling effort contains nothing new, and it is based on a cascade of simplifying assumptions, which are undoubtedly necessary, and which determine the results and conclusions. Furthermore, the methods to derive the parameters are insufficiently described, and many modelling parameters are not given in the paper.

1.0.1 Assumptions

Among the key assumptions are

1. The release of CCA from the timber follows a first order kinetics
2. The parameters for this kinetics can be derived from a tank experiment, and used for soil if corrected by a simple factor equal to the soil's saturated water content
3. The CCA is released equally through the whole buried surface of a post
4. The released CCA is equally distributed within a circular 5 cm around the poles
5. The transport through soil takes place in macropores, which are active in transporting as soon as there is precipitation.
6. The partitioning between water and soil is given by a linear isotherm

HESSD

3, S629–S635, 2006

7. The water balance is that of bare soil
8. Vertical transport takes place in an annular cylinder of 175 mm diameter; adsorption is therefore limited to this soil volume
9. Vertical mass flux of contaminants is perfectly mixed with the water in the aquifer over its full thickness.

While it is clear that simplifying assumptions have to be made, as in any modelling scenario, I find that almost none of these assumptions is seriously discussed nor questioned (despite good reason for doing so, see below). Furthermore, in no single case the possible consequences of simplifying the true processes in the chosen manner are discussed. Thus, it is impossible for the reader to judge on the reliability and uncertainty of the results. Actually, the fact that the paper does not address the issue of uncertainty in the predictions at all, is disqualifying. A risk assessment study that is based on a modelling scenario **must** address this issue.

Actually, quite a few of the assumptions in this modelling study must be strongly questioned. Of most critical importance among those is the assumption on the release kinetics from the timber posts to the surrounding soil. The authors propose that this kinetics is proportional to the total mass of contaminant in the post, and derive the respective coefficients from an experiment in a water tank. These data are (fortunately, and contrary to many others) shown in Fig.2, together with the fitted curves which show the modelled release. Upon inspection, one finds that the data strongly suggest a different - non-linear - release kinetics. The authors should consider that alternative release kinetics and discuss the long-term effect that arises from describing this crucial process with one or another model, including the uncertainty of the fitted parameters. I am well aware that the presented case is a worse-case scenario as compared to the alternatively one, but since this is a scientific paper the full bandwidth of probable release rates has to be shown and discussed.

As a second example, the assumption of bare-soil evaporation as an upper boundary condition must be questioned. Water transport in the vadose zone is a three-dimensional process, and plant roots will extract water, causing lateral flow towards them. This might reduce the effective net water percolation rates in the modelled annular cylinder of 175 mm diameter considerably. Again, it is clear that a much more realistic scenario can be found only with substantially higher effort, and that the given case represents again a worst case, but the kind and magnitude of the errors resulting from the simplifications in the current modelling approach must be somehow addressed.

As a third example, the assumption of the applicability of the water capacity modelling approach with a (arbitrary) slab height of 10 cm for the current process must be discussed for the transport of a sorbing chemical. This modelling depends on very strong assumptions, and leads to a mobility of sorbing substances irrespective of their sorptive behaviour. So, the chosen parameters affect the modelling result in a critical manner. I did not find any specific statement about the criteria by which these parameters were derived, nor about their values (see section "parameters").

As a side note, I do not agree with the statement that the non-linear sorption isotherms of the metals can be taken linear at a low concentration level. The authors write "*At our somewhat low values of arsenic on the soil solution, a linear isotherm is nonetheless reasonable*." (page 7, line 16). A Freundlich isotherm is always non-linear, regardless of the concentration being high or low. A linear approximation can only be justified if the concentration range of interest is not too wide.

Finally, some of the modelling assumptions remain unclear. An example is the modelling of the process of the distribution of the chemical, which is released from the timber post to the surrounding soil. Why do the authors speak of "... *we limit lateral chemical movement to within 50 mm of the post...*", if their modelling is indeed 1D vertical? Is the released CCA mass instantaneously and equally distributed in the annular cylinder? If so, why within 50mm, and not within 25 or 100 mm, (which would alter the conclusions with respect to the exceedance of guideline values for soils)? Also, it is not

HESSD

3, S629–S635, 2006

Interactive Comment

clear whether in this calculation the pole's face releases of CCA, with a 40-fold higher rate as compared to its sides, is considered this calculation considered?

1.0.2 Methods and Parameters

In a technical sense, the used methods are not described in a sufficient manner. As an example, the crucial release-experiment for CCA in a water tank is only marginally described; we do not know any or the experimental conditions, such as the size of the tank, the temperature, the ionic composition of the water, and whether there was continuous mixing in the tank, etc. An assessment of the authors' assumption that these release rates can be set equal to those expected in soil, is not possible for a reader. This, however, touches the core of the analysis, because the whole modelling effort is, on a long term, more or less a simple mass balance calculation driven by the release kinetics for the timber posts.

Almost none of the parameter values used in the modeling study are numerically given (with the exception of the release rates of CCA from the posts). Often, the reference to parameters is embedded is somewhat diffuse statements, such as

- "*Parameters of the modelling were taken from data collected during laboratory studies on a range of Marlborough soils* "....(p6, l)
- " *A standard crop-factor approach was used to related soil evaporation to the prevailing weather*"
- "K_D *were measured, using batch methods* ..."
- " *the detailed analytical results can be found in our report to the Marlborough District Counil* ..."(p8,l7)

HESSD

3, S629–S635, 2006

Interactive Comment

- “ *the soil physical and hydraulic properties were deduced from data contained in the NZ Soils Database of Landcare Research* . . . (p9): which properties, where are the numbers?

As an example, for the short term effects, the vertical contaminant transport in the soil is modelled by taking place through macropores. Any release to the soil during rain phases is assumed to be transported immediately in macropores. During the precipitation-free periods a redistribution will take place (the authors speak of “*lateral chemical movement*”, but their model is 1D, so I guess it’s just a even distribution, calculated by a mass balance). The result of this conceptual model will depend crucially on the assumed parameters, but we do not learn how the parameters are estimated or determined, how uncertain they are, and how this uncertainty may affect the modelling results.

From this paper, the reader does not get information on even the most basic soil physical and soil chemical properties, nor on the values of some other important parameters, such as the equilibrium distribution coefficients between water and soil (and their uncertainty), and the water capacity and transmissivity parameters (and their uncertainty). This makes it impossible to re-calculate the modeling study, thus failing to match a requirement that is basic to any scientific paper

To summarize, this is a scenario that provides some insight into the modelling approach that is appropriate to tackle the given tasks and to answer the questions of the regulatory agency. It is further illustrative to see this kind of simple mass balance modelling. However, scientifically, it is rather a student’s effort than a paper that contributes to the advancement of science,.

It is in particular the missing discussion of

- the applicability of the many assumptions involved and of the possible implications on the results given by these (necessary) simplifications, and

- the uncertainties that may arise from model errors and parameter uncertainties, and
- the uncertainties that may result from spatial heterogeneitiy

which leads me to the conclusion that the presented study dos not contribute a significant progress with respect to the issue of *pattern dynamics* of CCA contaminations in soils and aquifers. Hence, I recommend a publication in a local journal rather than in HESS.

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HESSD
3, S629–S635, 2006
Interactive Comment