

Interactive comment on “Interpolation of extensive routine water pollution monitoring datasets: methodology and discussion of implications for aquifer management” by Yuval et al.

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This is our response to the review by Anonymous Referee #1 from 17 August, 2013.

We thank the Referee for the lengthy review of our manuscript. A detailed response to each of the specific comments is given below. A GENERAL response we would like to make is that science advances in different paths, sometimes parallel to each other. Any of these paths is, to our opinion, worthy of consideration as long as it follows the fundamental rules of logic and provides an understanding of nature which fits to an acceptable degree the measured observations. Nothing in the Referee's com-

C4722

ments points to any specific flaw in our logic or suggests underrated performance of our statistical approach. Beyond the Referee's fundamental disagreement with using statistical methods it was not made clear why methods which are "not physically based can lead to very dangerous decisions if applied to solve for real problems". We used real data observed in a real aquifer and gained better understanding of its real groundwater pollution problems. The performance of our methods was meticulously assessed and elaborated in length. We arrived at some simple, but important understanding regarding their usage. We believe that science is advanced by constructive criticism of a presented work (which the Referee also did and for which we are thankful). We respect the expertise of the Referee and the efforts invested in explaining his/her main point of view however, we think that a swiping rejection of a work based on a personal belief stifles scientific innovation and is counter-productive to the goals of the process of scientific publication.

Below are our responses to specific comments by the Referee. The comments were not numbered so that the numbering below is a best choice made by us which we hope reasonably divides the Referee's comments into separate sections.

1. The Referee stresses the complexity of a hydrological system. A point is made that solutes tend to be in constant movement. The Referee further contends that due to the natural heterogeneity of the sub-surface at any spatial scale "performing effective upscaling is considered today one of the major challenges in hydrology".

We thank the reviewer for highlighting this important point. Clearly one should seek a mechanistic approach to understand (i.e. model) groundwater systems, and mechanistic approach should primarily include treatment of flow, transport, and heterogeneity. However, obtaining a good grasp of those physical parameters requires extremely fine monitoring network. Unfortunately, most aquifers are not "pinched" every few meters as some research sites are (e.g. in Cape Cod, Massachusetts (http://toxics.usgs.gov/photo_gallery/capecod_page2.html), where about 10,000 sampling wells were drilled in relatively small area). As we note in page 9365 line 12, a

C4723

very dense monitoring network is not realistic. And as the reviewer notes, "from a practical perspective there is a need to provide decision-makers quick and practical maps". This is exactly what we propose in the manuscript. While it is extremely important that mechanistic tools will continue to be developed, decisions need to be taken and we believe that we provide the decision makers reasonably accurate depiction of the status of the aquifer, and in addition point to shortcomings of the existing monitoring network. The issue of dynamics is of importance too but given the slow flow rates in the aquifer that we worked with, annual or multi-annual snapshots are very reasonable for any monitoring enhancement programs or remedial efforts. Therefore, while we recognize the importance of the mechanistic approach to the problem our approach here is to bypass the complex mechanisms and to treat the observations directly, in a statistical way. We do not see how this approach is "fundamentally" wrong. After all, the observations account for all those complex processes that the Referee rightly points out. What our work does is filling the spatial gaps left in between those observations and it uses a method to assess the quality of "filling the gaps" by again resorting to the observed data through the cross-validation process. Also note that we limit ourselves to relatively small search radius in our analysis in order to avoid the "dangerous decisions" the Referee talks about.

2. The Referee points out that flow maps were generated using head gradients with no regard to hydraulic conductivity and Darcy's law.

Yes, the reviewer is correct and gradients are used as proxy for velocities, but not directly as advection. The aquifer is well known as generally horizontally homogeneous (e.g., Tolmets, 1977) and therefore gradients are used alone. Note however that we do not solve for advection but just use the gradients to delimit non-circular inclusion zones around the interpolation points. This is an expediency which may not be accurate enough and that may explain why using the elliptical zones did not prove to be better than using circular zones. However, the main points in the paper do not depend on use of the elliptical zones and our main conclusion are equally valid using only circular

C4724

ones. Thus the expediency of using gradients to estimate the flow field has no essential impact on the manuscript.

3. The Referee claims that the core of our method does not use the actual observed concentrations.

This is not true. We estimate the concentration at an interpolation point using a weighted mean of the observed data (the Z_j in equation 1) that fall within an inclusion zone.

4. We said in the manuscript that the subset of observations used in the interpolation for each point "...should only include observations at locations which given the advection and dispersion time scales may be associated with the concentration at the interpolation grid point." The Referee claims that the term "advection" was ill-defined and that references to the way which we treated the "dispersion" are required.

We thanks the Referee for the comment and will clear these issues in future versions.

5. The Referee assumes that most of the wells in which our data were observed are production wells and that the capture zone around such wells is not elliptical.

First, only a fraction of the wells in which the data are collected are production wells. Many others are purely monitoring ones. However, in general we agree with the Referee. If flow were uniform the capture zone would probably be somewhat conic. Note however that a) flow is not uniform due to temporal changes in gradients and b) what matters is not only the "up-gradient" direction (that probably indicates future concentrations in a grid point) but the down-gradient (that better represents the past flow). Therefore we believe that an ellipse is a reasonable first order approximation. As we also show, the shape of the inclusion zone does not matter that much and in most cases circular inclusion zones produced similar results.

6. The Referee asks about the parameter M and wonders about the consideration for its selection.

C4725

As we define in section 3.2, M is the number of observations to whose locations interpolation could be carried out in the process of leave-one-out cross-validation. In calculating the Success Rate (SR), which is a function of M, we debated whether to set M as the nominal number of observations or instead consider M to be the number of observation locations for which interpolation was actually carried out (there are observation locations with no nearby other observations to which interpolations were impossible for given inclusion zones shape and dimensions). We chose the latter option which, as explained in the manuscript, gives a feeling for the accuracy of the interpolation that was actually carried out. This choice practically assigns the observation locations to which interpolation was not carried out an infinite uncertainty, as should be the case given that they are too far from any other observation.

7. Regarding the choice of M, the Referee say that "... from a decision-making point of view one would probably expect the quality of an estimation of the concentration also where the data are missing...".

We can assess the quality of the interpolation ONLY where we have observations. How otherwise can we honestly estimate the accuracy? By carrying out the leave-one-out cross-validation we estimated the performance at the observation locations but in such a way so that each of them WAS NOT used in the interpolation process. Thus the quality of the interpolation is indeed estimated at points of observations but in a completely independent process. Given the large number and fairly wide distribution of observation locations we believe that the reported interpolation performance (based on estimation in the data locations) represents well the performance in locations where data do not exist (i.e., over the interpolation grid).

8. At page 9373 we indicated that solutes are not expected to be found at a distance of more than 1km from their source without providing a reference.

As the flow rate in the aquifer is relatively slow (gradients of the order of 1/1000; The Hydrological Service, 2011) and the hydraulic conductivity of the order of 10 m/day

C4726

(Averbach, 1958), it is clear that advection can not result in solute movement of more than a few tens of metres per year. This is also evident in the size of the saline plumes in the aquifer (e.g. Shavit and Furman, 2001).

9. The Referee comments about Figure 3 and the suitability (or not) of histograms to present long-tailed distributions.

We believe that the comment is meant for Figure 4, not Figure 3. Assuming that this is indeed the case, we agree with the comment. We will look for a better way to demonstrate the traits of the concentration distributions which we had to deal with (although clearly the Referee was able to understand our point very well with the figure as it is).

10. The Referee claims that the "exceedance" which we plot in Figure 6 depends on the value of M, which according to the Referee is ill-defined.

The plots in Figure 6 present results of the full interpolation (i.e., using all the grid points) and thus have nothing to do with the value of the M interpolation points used for the cross-validation.

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Shavit, U., Furman, A., 2001. The Location of Deep Salinity Sources in the Israeli Coastal Aquifer, *Journal of Hydrology*, 250: 63-77.

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C4727