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

Anonymous Referee #1

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This manuscript deals with a new procedure able to interpolate data about groundwater solutes from sparse measurements and to provide effective maps depicting the distribution of these pollutants in the subsurface. As an example of application, the authors illustrate the data from the Coastal Plain in Israel. Overall the article is well written and stylistically I have very few concerns (some minor details which the authors could readily change).

However, my recommendation for this submission is rejection. The GENERAL and main reason is that I fundamentally disagree with this type of approaches used to simulate the distribution of pollutants in the hydrodynamic subsurface. They are basically

not physically based and they can lead to very dangerous decisions if applied to solve for real problems.

Aquifer are heterogeneous and especially "hydrodynamic" systems by definition (Latin roots of the word). If solutes are introduced in pristine conditions (i.e. when anthropogenic effects like pumping wells or artificial recharge are not present), subsequent contaminant plumes tends to be in constant movement, with evolving and unpredictable shapes and times.

In addition, if anthropogenic effects exist, the boundary conditions applied to the contaminant movements become very hard to be accurately describe. Since this spatio-temporal variability is triggered by natural heterogeneity (ubiquitous at all spatial scales in geological environments, from pore scale to regional aquifers), performing effective upscaling is considered today one of the major challenging in hydrogeology. Effective upscaling means also that resulting maps depicting the distribution of pollutants are useful to (quoting the authors) " Delineating polluted areas with high level of accuracy focuses the remediation efforts" and enabling " an efficient management of the water resources both in terms of water production and pollution remediation"

What presented here by the authors does not account many of these aspects for. Authors are treating the aquifer as a static system (not even the Darcy's law is considered to estimate the advection velocities). I can understand that from a practical perspective there is a need to provide decision-makers quick and practical maps. As such, one may want to take "snapshots" of the distribution of contaminants and treat them as static variables. However, one needs to clearly change its perspective since this is intrinsically not correct from a physical perspective.

SPECIFIC aspects are listed as follows:

- One of my points of disagreement starts directly from the selection of the water head data (Sec. 2.2). I quote the authors: "For each well, the median of its water level observations during 2009–2010 was considered. These median values were projected to the

regular grid used for the pollution data interpolation using linear Delaunay triangulation method implemented by the Matlab$^{\circledR}$ griddata function (The MathWorks Inc., 2010) calculated for each point 25 on that regular grid using a finite differences scheme." Are the authors generating a maps of flow velocities -thus advection - as the gradients between two points?? What about the hydraulic conductivity and the Darcy law? What about the heterogeneity and connectivity in this property, which is one of the controlling factor for the migration of contaminants at the local scale, and thus on larger evolving scales? (See Renard and Allard 2013 AWR for a review of these concepts).

- In Sec. 3.1 the authors illustrate the main core of their method. The interpolation is based on three parameters (N,d and p) and the distance between points. None of these parameters depend on the actual value of the concentration measured at the points. What mainly matters in this approach is if the "subset (N) should include only observations at locations which given the advection and dispersion time scales may be associated with the concentration at the interpolation grid point". First, advection is ill-defined (see my previous point). Second, dispersion is chosen arbitrarily as a costant normalizing value, while this is in reality another most important parameters controlling the hydromechanical transport of solutes. At line 8 of page 9370 the authors say (without referencing it) that "The value of R is set a-priory based on the transverse dispersion expected in the aquifer. For example, in the coastal aquifer the transverse dispersion is assumed usually to be around 2". Dispersivity (rather than dispersion) is a physical value with units, and it is found highly dependent on the scale of the problem. (See Gelhar et al 1992 WRR, Dagan 1989, Rubin 2003).

- The selection of the area influencing the selection of points N is not accounting for the fact that around pumping well the capture zone is not elliptical as the avreage uniform flow patterns (assuming no heterogeneity). I expect that most of the pollutants are collected in pumping zones, thus this approach is additionally not correct. At page 9371, the authors illustrate the validation method, which is based on the leave-one-out cross-testing scheme. While first I don't understand then if SR (Eq. 3) is the average

Success Rate for all the M used in the validation, what puzzles me more is actually the selection of M. In this study M is the "number of observations for which interpolation are produced". But M is also chosen according to the criteria defined in Sec. 3.1; thus, SR is biased by the selection of the M points.

- In addition, from a decision-making perspective one would probably expect the quality of an estimation of the concentration also where the data are missing, not just in the measurement location. This is of course not possible to be performed with a static interpolator but requires another approach (such as a flow and transport stochastic model).

- At page 9373 the authors indicate that in the Coastal aquifer solute are not expected to move from more than 1km from the source. Referencing is again missing here and it is fundamental.

- Fig. 3: If the purpose is to show that only a few points show very high concentrations, hence it is better to put a table indicating the CDF percentiles. In this sense, one also gets an idea about the distribution of the data. In addition heavily-tailed distribution are poorly represented by histograms. The reason relied in the selection of the bin size which should be optimal and not arbitrary. Otherwise one can bias the interpretation (see the books by Silverman 1986 on Density estimations, for instance).

- Fig. 6 is considering as "exceedance" only to the number of interpolated values M, which is subject to the selection of the radius, in turn ill-defined. This is thus not correct.

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