Review of “Response to recharge variation of thin lenses and their mixing zone with underlying saline groundwater”

I would like to compliment the authors for writing a very interesting paper on the behavior of floating freshwater lenses on top of saline groundwater. As well as being a theoretical challenging subject, this research is relevant for everybody dealing with limited freshwater resources in areas with saline groundwater. Floating fresh water lenses on top of saline groundwater are common in deltas around the world. Such areas typically have high biodiversity or are used for intensive agriculture because of their fertile soils. The amount of freshwater often dictates the fate of these landscapes: precious nature reserves when little freshwater is available and intensive farming when just enough fresh water is available. This paper could lead to a relatively simple GIS tool that can indicate the amount and vulnerability of fresh water under changing precipitation and evapotranspiration regimes. The approach followed by the authors is that first many calculations of freshwater lenses as function of field characteristics are performed with a numerical 2D model. These calculations are made dimensionless and are analyzed for the lens volume, lens volume variability, and the thickness of the mixing zone. These results are then translated into simple empirical models, which can be used to regionalize the results of the numerical 2-dimensional models. As such this paper is interesting for the readers of HESS and I recommend this paper for publication after revisions of the following major and minor comments, especially with regard to the structure of the paper.

Major comments

- After reading the paper it is not entirely clear to me, if it is possible to estimate the amount and vulnerability of fresh water lenses using the empirical approximation model proposed in this paper. If I wanted to use your approach to map out freshwater lenses, their mixing zone and their sensitivity to climate change for the entire Dutch province of Zeeland, what steps should I follow and which equations should I use? In your conclusion it says that at least one reference scenario is needed to fit the analytical model (page 1457 line 28). Do you mean one reference scenario per soil type, weather type, field type or just one? Please clarify in your paper what I should do and how I can use your results.
- The paper lacks a clear definition of a freshwater lens, which confuses me throughout the paper (although reference is made to a previous paper of the same author on page 1445 line 22): However, this does not help me at page 1446, line 11, where the text refers to lenses thicker than 3m and thinner than 0.8m. How are these lenses defined? The field average thickness versus the field-center thickness. The center of the mixing zone, versus equivalent thickness of fresh water only, versus water with a concentration lower than a salinity threshold (common way)? The same problem arises in section 3.4, where you define the impulse-response function. It is not clear to me what the meaning of Δz is: “the ultimate gain for step and impulse function”: again center versus field average, and center of mixing zone versus equivalent freshwater thickness? Same for section 4.1:”We define the lens volume as the volume of pores filled with freshwater?” What do you mean with fresh water in the presence of a mixing zone? Do you have a salinity threshold for the definition of freshwater, or do you calculate an equivalent freshwater thickness (which probably is equal to the center of the mixing zone)?
- Paper structure: The paper does not contain a clear methods-section, which makes the paper unstructured. Half of the methods are presented in the Theory section (model setup for Sutra and analytical model + parameters), the other half in the results section (analyzing tools for the numerical models and derivation of the empirical model). I think the paper would benefit from a clear methods section. Theory section: 3.1, 3.4 + parts of 4.1 and 4.2; Methods section: 3.2 combined with shortened 3.3 (see later comments) + parts of 4.2. I often found myself doubting if a result was from the SUTRA model or from an analytical approximation: for example page 1455, Fig. 10: I now think that both simulations with natural and sinusoidal recharge were SUTRA calculations, but this was not immediately clear. In the methods section I would like to see clear steps of the performed analyses and understand why you follow these steps.

- If you define the frequency for recharge as illustrated in Fig. 3, I cannot see why you would vary this frequency for values between 1 week and 1 year. Clearly you are fitting an approximation for a yearly cycle. A weekly cycle does not exist. What is the value/meaning of your numerical experiments with a cycle of 1 week (page 1446, line 20, table 3)? I would have liked it better when you had split up the recharge frequency in a yearly frequency for evapotranspiration and flexible frequency for rainfall. It would have added more realism to your calculations and you would be able to study the delicate interplay of evapotranspiration and precipitation. I understand that this would make your dimensionless analysis much more complicated or even impossible. However I'm not yet convinced how good your sinusoid approximation for recharge really is: In Fig 10d for all 3 of your lens thickness scenario's the sinusoid approximation gives substantial underestimations of your mixing zone thickness. Given Fig 10a this underestimation seems likely to originate from an underestimation of the variance (amplitude and frequency) of your recharge? The authors claim in their conclusions that the sinusoid approximation reproduces the mixing zone thickness well. If I understand Figure 10D correctly I would say that the sinusoid approximation underestimates the mixingzone thickness considerably (around 40%). This however is not at all discussed in the corresponding text (page 1455, line 3 and further). I would like a clear justification for this underestimation, because the mixing zone thickness is one of the key features/innovations of your analysis.
- The paper would benefit from a clear summary of the analytical and empirical models proposed in this paper and under which conditions they are valid (see also point 1, I still don't understand how to apply your results): I read somewhere for lenses< 3m but on page 1451 it is also stated that Eq. 14 does not hold for large values of MAps, i.e. when salt water is present at the soil surface. The last bit is confusing, because that was part of your goal: to find out when and how much salt enters the rootzone and inhibits crop growth. However, here you say that your approximation is not valid under these conditions?

Minor comments

- Abstract line 13: the "analytical approximation", if you use fitted coefficients without any physical meaning (i.e. linear approximation of Fig. 6a) isn't it an empirical approximation?
- Page 1440,line2: It is strange to say that the lens thickness influences the depth at which saline water is found: They are both part of the same system and neither one influences or causes the other, "describe" would be better than "influences"
- Nomenclature: maybe you can add here also the variables for dimensionless groups. First, I tried to find the meaning of fps, M and R in this list, but later found out that I had to be in Table 1.
- Nomenclature: Δz: ultimate gain step of what? Center of lens? (see previous comment about definition of lens)
- Page 1444 line 10: lower and upper boundary: do you mean recharge and seepage?, please state so for clarity
- Page 1444 line 11 Specific discharge<P>., according to your nomenclature, this variable is called: sinusoid average recharge
- Page 1446: I have a lot of problems understanding your reasoning in paragraph 3.3. I would suggest to skip this section entirely as it distracts from you main points, and forces you to mix method with results: For example In figure 3b you compare measured with simulated salinity profiles. However nowhere is mentioned what kind of model you have used: stationary/non-stationary, Sutra/analytical. Furthermore, Figure 4 is very difficult to understand (I still do not understand it), and the claim in the text that Fig 4 illustrates that a dispersivity of 0.25 reflects observed mixing zone thickness better than a dispersivity of 0.05, is not clear to me (or do you mean to refer to fig 3b?, if so you do not refer to Fig 4 at all? Aha Fig. 4 in text should be fig 3b, fig. 5 in text should be fig. 4, That took me a long time). Still I think it is better to skip this section and just state your chosen values for transversal and longitudal dispersivity, or move it to an appendix.
- Page 1449, line 3. Which results? Table 3 does not contain any results.

Page 1452, line11. You refer to section 2.3: 2.3 does not exist and you probably mean 3.3, but still it is not clear to me where you refer to: What are the lens thicknesses of interest?

- Page 1456, lines 6 "The little recharge still occurring…" This is a difficult sentence: and it makes the recharge sound artificial: You mean to say that the lens has its maximum thickness at the moment recharge equals discharge during a period of declining recharge.
- Page 1456: What do you mean when you state in line12 and 15: *at* >> 1.From Figure 11 is see that this can take more than 10 years, So do you mean to say that delays were derived from numerical simulations with SUTRA that ran for more than 10 year, but the delay itself is in the order of 80 days (for a yearly freq)? Or does it mean something else?
- Page 1456:As the entire goal of your paper is to derive an analytical/empirical approximation for lens dynamics. It would be nice to show a figure with lens amplitude from Sutra, against lens amplitude by Eq 13a. Now I, and presumably most other readers, miss your statement on page 1456 line 15 that differences are less than 5%. I would like to see it.
- Page 1456, line 22: Looking at figure 3A amplitudes of the natural recharge are extreme compared to your annual approximation. Will the high frequency signal of recharge significantly affect the amplitude of the lens: Looking at fig 10c. The high frequency signal does not affect the amplitude of the lens but does strongly affect the mixingzone thickness (Fig 10d):
- Somehow I miss or I do not understand your comparison between the mixingzone thickness of the SUTRA models with the mixingzone thickness of your analytical/empirical approximations: I do find a comparison for lens volume variation, and I assume that Fig. 9 compares Sutra mixingzone thickness, with model mixing zone thickness. For example, page 1453 line 17 you talk about the average variance of a mixing zone: is this the temporal variance of the mixing zone thickness, or the average mixingzone thickness? I get confused because you use both variables throughout the text. (i.e. Fig. 9 says: variance of the mixingzone thickness) But if Eq. 21 gives the temporal variance of the mixingzone thickness, which Eq. gives the average mixingzone thickness?
- Page 1457, line 13: do you mean Eq 17 instead of Eq 16. Eq 16 only gives the mean volume

Some remaining comments/questions:

To my understanding you try to find analytical and empirical approximations for: Average lens thickness/average lens volume, variation in lens volume/ amplitude of lens thickness, average mixingzone thickness, mixing zone thickness variation, and delay of the lens behavior compared to recharge. But after carefully reading your paper I still have difficulties finding the right formula for each of these variables, because it is spread over theory and results. A clearer structure would help. (see major comments)

How is the lens amplitude Q different from the change in lens volume ΔV: They must be related somehow?: Although the lens volume change does not contain *a* or *Δz*. Using Eq 16:
$\Delta V=1/4\pi LQ$??? and what about porosity?

Is the delay of the mixingzone always equal to the delay of the lens amplitude? I.e. is the mixingzone thinnest when the lens thickness is thinnest? Or can for example the delay of mixing zone thickness between 0.05 and 0.95 concentration percentile be quite different from the delay of the center of the mixing zone?

Moving now to a delta in Southern France: Saline seepage is a typical Dutch phenomenon that occurs because of land subsidence: In most deltas you do not have saline seepage, but still you have floating freshwater lenses on top of saline groundwater. This is because sea water infiltrates via a network of tidal channels during high tides. In southern France freshwater lenses form during winter which are entirely or partly depleted during summer. Can we use your approach in such situations as well? (i.e. no saline seepage, but a relatively constant saline head boundary in de ditch?) This situation is much more common around the world, than your saline seepage situation and possibly much more sensitive to precipitation and evaporation changes? How different would your analysis be?

Tables and figures

- Table 3: Bold gives De Bilt reference situation, but to my knowledge De Bilt has no saline groundwater. So it seems strange to take a De Bilt reference situation.

- Fig 5a between recharge period → between normalized recharge period.

- Figure 7 shows relation between lens thickness and lensvolume But this is just for 1 single type of field. I do not understand why the entire paper is made dimensionless, except for this figure?

- Figures 11+12 Δ → should be Δz?
- Figure 13: is change in lens thickness the same as lens amplitude? I understand that figure 11 is really a change. Throughout the paper it would be good to carefully reexamine the use of the word "change". Sometimes it really means a change driven by a change in recharge (e.g. step function, fig. 11). Most of the time "change" is use to indicate temporal variability or lens amplitude. Making this distinction more clear would make improve the paper.