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Hydrology and Earth System Sciences Discussions

Interactive comment on "Response to recharge variation of thin lenses and their mixing zone with underlying saline groundwater" *by* S. Eeman et al.

S. Eeman et al.

sara.eeman@wur.nl

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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.

We thank the reviewer for the thorough and careful review. We reacted to all comments below, for a better visibility, please view the attached PDF, our reactions are italic there, which however is not visible here.

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.

Your question points out that we needed some reformulations to clarify the issues you mention. Concerning the reference scenario we added, in agreement with your point, that one reference situation is needed for a specific soil- and field (geometry) type. From that, the consequences for lens and mixing zone thickness caused by different climate scenarios or other human induced recharge changes can be calculated analytically/empirically. The necessary equations are all mentioned in the right order in the conclusions and we added a description of the steps taken in the introduction of the revised ms.

- 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 impulseresponse 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)?

It is apparent that our definition given in section 3.1 was not sufficient or ambiguous. We therefore provided a clear and hopefully unambiguous definition, and gave extra explanations where doubt could occur, as you pointed out, for sections 3.2, 3.3, 4.1 and 4.3. We define the lens thickness as the depth from soil surface to the center of the mixing zone, in the middle of the field, which in practice nearly coincides with the 50% isochlor, see Eeman et al. 2011.

- 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.

We understand that our choice to put part of "methods" in the results may be a bit confusing. Our motivation for the way we separated theory from results, is that in the "theory" we provided the basic, generally accepted equations. In the "results section", however, we provide what is merely a consequence of our data-analysis, leading to assumptions or approaches which are valid for the fresh-salt soil system considered in this paper, but also quite specific for this system. As our way of interpreting the calculations is specific for our system, where we learned from each previous step, choice, and approximation, the methods-section gives results that are far more conditional than those of the theory section. We therefore think that restructuring would not lead to improved readability. Reaction on section 3.3 is added to your specific comment thereon.

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.

We completely agree that it should always be evident, which results concern analytical and which numerical approximations, and have clarified the texts, throughout the paper

In the methods section I would like to see clear steps of the performed analyses and understand why you follow these steps.

We appreciate that this clarifies our analysis, and we have added these steps in the introduction chapter, where it fits the presentation of the manuscript the best. Orders of the steps taken are, of course, kept the same throughout the paper, in introduction, theory, results and conclusions.

- 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)?

This is a good point, as we completely agree that a weekly cycle is hypothetical. Still, we considered that cycle also, for 3 reasons: âĂć To demonstrate which recharge frequencies are relevant for the types of lenses we studied, we wished to consider a range of frequencies. A methodological choice, therefore. âĂć To quantify the importance of the travelled distance (which is very much influenced by the frequency) of the center of the mixing zone for the width of this mixing zone. âĂć Although indeed weekly sinuses are hypothetical, they have a resemblance with the short-term fluctuations in rainfall that are generally observed, and therefore serve as a first step of the analysis of their impact.

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 our calculations and you would be able to study the delicate interplay of evapotranspiration and precipitation.

This is an excellent point. Particularly, it is of interest in many other papers where the upper boundary is simplified to 'net precipitation'. We agree with the reviewer that this interplay could be very interesting for a next study, to refine the results and to improve the estimation of possible crop reduction, given salt-, wetness- and drought- damage. We have already performed part of this analysis on a point scale. It did not seem feasible to put all this in one article, as the message would become too diffuse, and the logic harder to follow.

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 sce-

nario'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 mixing zone 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.

We thank the reviewer for this point, as it makes it clear to us where our text is not clear enough. What we claim is that the thickness of the mixing zone can be calculated from the recharge pattern by determining the differences in traveled distance between sinusoidal and natural recharge patterns. The ratio of these distances, is equal to the ratio of the mixing zone variances. Therefore after carrying out one simulation with a certain recharge variation, the mixing zone thickness for any other variation can be calculated using Eq. 24. The underestimation you mention is not an estimation but a comparison. We have reformulated parts of the section to make sure our message is properly understood.

- 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?

We state that lenses with a thickness between 0.8 m to 3.0 m are the scope of this study, since within the context of (agricultural) plant growth, these are the areas in which productivity is potentially high, but threatened by salinity. We made our description more precise in the text. The values of Maps as described here, would lead not only to saline water in the root zone, but even at the soil surface. Indeed, then our approach is no longer suitable. An exact estimate of the fresh water under such circumstances is, however, not all that interesting from an agricultural pint of view: there will always be saline water in the root zone, which inhibits crop growth except perhaps for halophytic species.

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? We comply.

- 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" The sentence was rephrased.

- 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. We agree and put the contents of the table n the nomenclature section.

- Nomenclature: Δz: ultimate gain step of what? Center of lens? (see previous comment about definition of lens). Indeed center of lens, this was added.

- Page 1444 line 10: lower and upper boundary: do you mean recharge and seepage?, please state so for clarity. We complied.

- Page 1444 line 11 Specific discharge<P>., according to your nomenclature, this variable is called: sinusoid average recharge. Sorry, thank you for discovering this mistake.

- 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/ nonstationary, 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). Very sorry about that!! Renumbering at the end has apparently lead to insufficient checking from our side. Corrected.

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)? This has been explained above.

Still I think it is better to skip thsection and just state your chosen values for transversal and longitudal dispersivity, or move itto an appendix. We complied and put this part into an appendix.

- Page 1449, line 3. Which results? Table 3 does not contain any results. We rephrased the sentence to prevent misunderstanding. It was results from simulations based on input data from table 3.

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? This should have been section 3.2, where parameter values are specified and explained.

- 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. Thank you for the suggestion, we complied.

- 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? The step function is used to obtain the parameters needed to perform a convolution. It requires a steady state to determine the ultimate change in lens thickness, which indeed takes a very long time. However, this does not mean that the system reacts on this same time scale! It starts reacting much faster, as is concluded (and also visible in fig.11), and keeps on following external changes. This is not the same as equilibrium! One of the discussion points of our previous paper on this was that equilibrium of such a system takes so long that it will probably never occur under natural circumstances. For these variable-recharge-input cases, equilibrium certainly is not to be expected!

- 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. We comply in a sense, we already showed a result as mentioned in Eeman et al. (2011). Showing a related figure in this paper, with such small differences (<5%), requires much space, yet has little added value (compared with our assertion in text). For this reason, we decided not to add such a figure.

- 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): This is true and was made more explicit.

- Somehow I miss or I do not understand your comparison between the mixingzone thickness ofthe SUTRA models with the mixingzone thickness of your analyti-

cal/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. This concerns lens volume/thickness, and not mixing zone thickness. This entire section concerns only lens thickness, the mixing zone is treated in the next section.

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? We have corrected this, and use a more constant terminology in this section, with a few extra explanations.

- Page 1457, line 13: do you mean Eq 17 instead of Eq 16. Eq 16 only gives the mean volume Indeed, we corrected this.

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) As stated earlier, we hope to have solved this issue in the revised manuscript.

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? The results chapter starts with the definition of lens volume, which already includes porosity. Your question concerning the relation between Q and ΔV is answered at the end of section 4.1, and also treated in section 4.3.

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? The thickness of the mixing zone is related to lens movement rather than lens thickness. This is elaborated in section 4.2, and was also partly explained in our previous article.

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? Thank you for this interesting question. We have thought about the differences in boundary conditions and the generally larger systems as we carried out this study. Colleagues are currently working on this, using parts of the Dutch coast that have similar regions concerning size and boundary conditions. They use an adaptation from the dimension analysis we have used here. Though not a topic for our present paper, we think that 2-3 dimensional aspects of flow become more important, as well as the dynamics of the water-unsaturated zone. In view of these complications, our analysis will not be transferable directly to the more complicated channel network problem. However, they may help in understanding such more complicated systems.

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. You are right that there is no saline groundwater in De Bilt. However, this saline groundwater does not influence the recharge very much. As explained before, this is only a reference situation. We could have chosen any other signal. Given the small differences between recharge in De Bilt and 40 km westward, where saline groundwater is present at very shallow depth, we did not use a reference that is far beyond a realistic situation.

- Fig 5a between recharge period $\rightarrow$ between normalized recharge period. The recharge periods have been made dimensionless (Eq. 6a), the volumes were normalized. Not exactly the same. We made the caption more precise.

- 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? It is for all simulations used to make figures 4-6, the dots are rather close to one another. Analytically this is equation 16, not for 1 specific field, only the length is kept constant (as in the numerical simulations).We could only show the validity of the linear relationship for the range of lens thicknesses we are interested in showing the actual lens volumes, that is the reason this picture is not dimensionless.

- Figures 11+12 $\Delta \rightarrow$ should be Δz? Indeed, corrected.

- Figure 13: is change in lens thickness the same as lens amplitude? I understand that figure 11is 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. Thank you for this suggestion. We replaced quite some “changes” in the article accordingly.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/9/C3656/2012/hessd-9-C3656-2012-

supplement.pdf

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