

# JOURNAL OF COASTAL AND HYDRAULIC STRUCTURES

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Review and rebuttal of the paper

## Quantification of Salt Intrusion Caused by Navigation Locks and Their Operation for Policy Analysis, Water Management or Salt Dispersion Modelling

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Editor handling the paper: Lydia Schulze

The reviewers remain anonymous.

## Round 1

Dear editor,

Please find attached our response to the reviewers' questions from the first round which we received on September 23<sup>rd</sup>, 2025. All remarks of the reviewers are reproduced below with authors' response in green.

We could not incorporate all text changes in our response in this document as the paper has had major revisions and many sections have been moved and/or rewritten. We provide as an additional attachment a version of the paper with tracked changes as well as a clean version. Section 2.2.4: Lock operation was moved and incorporated into a new section (Section 2.4.3) but was tracked as both deleted and inserted (as opposed to 'moved'), we accepted the deletion in this case to improve the readability.

We thank the editors for the chance to respond to the constructive remarks of the reviewers and hope that this new manuscript can again be considered for publication. We believe that this version of the manuscript is a significant improvement of the previous one and thank the reviewers and editors for their comments and efforts.

Kind regards,

The authors

## Reviewer B

*Dear authors,*

*I have reviewed the paper thoroughly and would like to congratulate you with the excellent work.*

*The paper is very relevant. Its topic on saltwater intrusion through the operation of locks is of high interest to the field, particularly in view of climate change and its related societal challenges, while existing literature is scarce. The presented model is a significant advancement, as it is more complete, flexible and general applicable compared to published models.*

*The paper is properly structured, concise, and clearly written. The abstract summarizes the work well and the introduction clearly portrays the paper's problem statement and subsequent research objective. The method is presented in detail and uses correct mathematics and physics. The paper's conclusion is supported by the model's results, which are validated by measurement data. The underlying figures are useful and of quality.*

*To further improve the clarity, completeness, and impact of the paper, I made some comments below. The authors can have a look whether they can incorporate them or disregard them.*

*Best regards.*

Dear reviewer,

Thank you for taking the time to review our paper, your kind words and thorough remarks. We are very pleased to read that you found our work to be both relevant and clearly written. Below you will find a response to each of your remarks and if applicable a description of how it was implemented in the paper.

Kind regards,

The authors

Comments

1. The abstract mentions that current methods are either calibrated on existing locks *or* are conservative assumptions about the lock operation. While various existing methods are referred to in the introduction (page 2, last paragraph: “For shipping...”), the details of the studies and their subsequent limitations are not explicitly mentioned. The subsequent conclusion that current methods do not have the predictive potential for changes in the system seems short-sighted. I therefore suggest the authors to provide a bit more detail on earlier work, cluster studies that share the same limitations, and explain more elaborately why the methods are not able to be used in forecasting.

This section has been rewritten to include more details of the studies mentioned. In short, many studies only look at a small part of the locking cycle, often only a single phase, like levelling or the lock-exchange phase, or the effectiveness measured is only applicable at the lock studied and not a general relation. The net effect of all phases of the operation is then not calculated.

Some studies just conservatively assume that the full lock chamber is exchanged each time the lock gates are opened at the canal/lake side.

Where the net effect of all lock-phases is accounted for it is generally done with a single calibration parameter for the entire locking-cycle (called an exchange coefficient). This coefficient must account for multiple sources of deviation from the assumption of full-exchange. However, this parameter cannot be generalized to locks with other patterns of operation and cannot account for varying conditions in the future and it cannot account correctly for the application of mitigation measures.

2. The introduction is generally well-structured and contains interesting background information. However, I suggest the following minor changes to further highlight the relativeness and innovativeness of the work:
  1. In the second paragraph (“Many researchers...”), the sentence about “The Netherlands” comes a bit aleatory. Also, the sentence “The Netherlands ... and canals” breaks the flow of the paragraph. I would suggest the authors to introduce

the general information on the Netherlands at the end of the first paragraph. This also holds for the general information on the Netherlands in the third paragraph (“In the Netherlands...”).

We have now grouped all information about the Netherlands together.

2. In the second paragraph (“Many researchers...”), various purposes of the numerical models for The Netherlands are claimed by the authors, but these are not specified. In the next sentences, only models of the Rhine delta are mentioned without being clear that these are used to predict the impact of drought on salt intrusion (via the Rotterdam Waterway) models. I suggest the authors to explicitly mention the model purposes for each group of models.

This has been clarified in the text. It now reads “There are 1D, 2D and 3D models of the Rhine delta in the Netherlands that have mostly been used to study salt intrusion into the open part of the estuary via the Rotterdam Waterway during times of drought.”

3. The fourth paragraph (“Efforts have...”), although interesting, in my opinion, does not contribute to the work presented in this paper. Whereas the previous paragraphs discussing numerical models to predict the effect of system-scale salt intrusion are relevant (as the presented model can be coupled to these models), the quantification of salt intrusion through groundwater seems aleatory here. Although it is important to mention that it is a problem, the extent of the paragraph distracts from the main message of the paper as it interrupts the natural flow leading to the problem statement. I suggest to either remove the paragraph, or to decrease the extent of the paragraph and shuffle its information such that the paragraphs on salt intrusion through locks are grouped. Here, the authors may want to follow the structure defined in the second sentence of the introduction to maintain a clearer structure: groundwater, surface water open estuaries, surface water closed systems (locks).

We agree that this paragraph does not fully contribute to the objective of the paper. We decided to shorten it.

4. In the fifth paragraph (“The response...”), it is claimed that very few mitigation measures of salt intrusion are available in estuaries. Although I understand the statement, it took me some time to understand that redirection of river discharge is a mitigation measure rather than a (infra)structural measure. I would consider the construction of weirs and discharge sluices itself to be a structural measure, but the operation of these structures is a mitigation measure. You may want to add “by means of existing weirs and discharge sluice” to the sentence for further clarification. Also, the word “mitigation” might be highlighted by making it italic.

We implemented both of your suggestions in the text.

5. In the sixth paragraph (“For shipping”), the link between the first, second and third sentence is not clear to me and seems unnatural. The linking word “In addition” seems to indicate a continuation of the previous sentence, while there seems no relation between the sentences. I therefore suggest the authors to restructure and/or rephrase the first sentences of the paragraph, such that there is a better flow and that it is clearer that the paragraph is discussing earlier work on modelling the effect of mitigation measures at locks and their simplifications (please see also next comment).

This paragraph has been rewritten in response to the comment below, such that this comment is no longer relevant (the sentences are no longer there in this form).

6. In the sixth paragraph (“For shipping”), it would be beneficial if the mentioned literature is discussed in a bit more detail, rather than only referring to them. Now it seems that a bold claim is made that the models do not suffice in accounting for mitigation measures and that they all work implicitly. Are all the referenced models the same? Why exactly a new model had to be developed? Can the models be easily modified as such that they can also account for mitigation measures?

This is an important point and we welcome the reviewer’s call to clarify this. We believe this is a key part of the argument we make that a model of this sort is an addition to previously published models. This paragraph has been rewritten (and is now multiple paragraphs).

In short, earlier studies either only look at the effectiveness of measures on a particular phase or when accounting for all phases in the operation include assumptions about the gate-open times. Our model allows the gate-open times to be varied to account for a change in the lock operation. Without this step, we believe, it is difficult to translate the results from previous studies to other locks with other operations or to future conditions where the gate-open times may be different.

7. The seventh paragraph (“The physical”) leads in my humble opinion to a disruption of the flow of the paper. As the sixth paragraph (“For shipping”) seems to lead towards a problem statement, the seventh paragraph (“The physical”) discussed general information again that poses an additional requirement to the salt load models at locks, after which the eighth paragraph (“This paper”) discusses the objective of the paper. I suggest the authors to create a two-step structure by restructuring and rephrasing the sixth, seventh, and eighth paragraph to an elaborate discussion of earlier work that will lead into a general problem statement, after which the objective of the paper flows naturally.

This text has been moved and partially incorporated into other paragraphs.

8. The last paragraphs (“The SLF...” and “Overall, the SLF...”) read as a discussion or conclusion of the work or as additional requirements to the model (which I expected to be written before the paper objective), while I expect here the methodology of the paper, as to what to expect of the paper in terms of method, validation, and results. I suggest changing the paragraph so that it includes this methodological part, rather than a summary of the model only. Also, it might be worthwhile to discuss the requirements of the new model (i.e. computation efficient, aggregated data as input) earlier and weigh these against existing models (i.e., are the models efficient? What input do they use?). Moreover, I suggest the authors to change the verb tenses of these requirements (model *is compact* à *model should be compact, allows for* à *should allow for*). Last, I suggest the authors to put the benefits of the SLF (i.e., compact, efficient) into the discussion and conclusion (i.e., after the results as a proof that the model actually works well).

The paragraph about the purpose of this paper (the introduction of the SLF) was moved up to the first paragraph of Chapter 1: Introduction and we added a sentence that introduces the structure of the paper (methodology, validation, applications) to the reader at the very end of the chapter.

9. It might be beneficial to highlight that the SLF is open-source, and to refer to the SLF programming packages in Github.

We now emphasize the open-source aspect and availability on GitHub and as a Python package in Chapter 1: Introduction, and will later on also add this to the data access statement (which is to be inserted when the paper is accepted). The name of the repository and the python package were redacted for now to maintain the anonymity of the double-blind review process.

3. I am convinced that the method presented in this paper is a significant step forward in testing the impact of new lock operation strategies, new lock dimensions, and changing boundary conditions on the salt load (strategically and operationally) and can significantly contribute by creating boundary conditions for numerical models (strategically). However, the reasoning behind the specific choice for the Sea Lock Formulation and details on what type of model the SLF is (semi-empirical analytical?) is lacking. Moreover, the literature review (page 2, last paragraph: “For shipping...”) is limited to the same type of models. This does not elaborate on why other existing models (e.g., 1D models such as WANDA-locks zoutlekmudel) do not suffice. I also miss the reasoning step in the introduction where CFD models are being disregarded. I suggest the authors to add this to the paper.

We now mention the Zoutlekmudel and WANDA-Locks in the text with references to the (Dutch) technical reports in Chapter 1: Introduction. The Zoutlekmudel and WANDA-Locks are currently not freely available, supported or published in scientific literature.

With regard to the comment on CFD models we have now added some reasoning to the section in the introduction concerning why the SLF is necessary. It now reads:

“Hydrodynamic 3D-models of these systems require a boundary condition for the salt flux across the shipping locks because they are either incapable of capturing the necessary non-hydrostatic flow phenomena around the locks or, such as Computational Fluid Dynamics (CFD) models, are too computationally expensive to be run for realistic variations in conditions and lock operations.”

4. Regarding the methodology, I suggest the following changes to improve clarity of the work for the unknown reader:

1. Specify the model type in the first sentence in the first paragraph (“The chosen approach...”). Although it is mentioned that the model contains equations of the flows in and out of the lock for all phases of the locking cycle, it might be worthwhile to clearly state in the beginning that the model predicts salt mass fluxes based on volume balances multiplied by the lock chamber-averaged salt concentrations.

We added your suggestion to the text and also state that the approach resulted in an event-based box model.

2. In the first paragraph (“The chosen approach...”), the sentence “*Note that this phase-wise approach is not the same as a fixed time-step approach as the computation is performed per phase, regardless of its duration.*” is not clear to me. Do the authors want to say that the model uses discrete events rather than fixed time-steps such as in conventional hydrodynamic models (e.g., Delft3d)? Please clarify.

That’s correct – the SLF does not use a “ $\Delta t$ ”, but instead uses discrete events (phases of the lockage cycle) which do not have fixed durations (event-based modelling). We added clarification to the text after the sentence you cited here.

3. In the first paragraph (“The chosen approach...”), the method’s objective of calculating cycle-averaged fluxes is not clear to me. Could the authors clarify why it is necessary to calculate cycle-averaged flows of water, rather than calculating pulses of saltwater intrusion? Maybe this should also be clarified in section 2.4.

We changed the sentence such that it says that it is possible (and not necessary) to make a cycle-averaged calculation. The reasoning for the availability of this option is mentioned in the following paragraph: to be able to perform calculations despite unavailability of data on the individual cycles.

4. From the beginning of the method chapter, it seems that *salt concentration* and *salinity* are used to both express the same concept. However, these concepts are

typically different and have different units that are commonly used: salt concentration (kilograms of salt in cubic meter of water: kg/m<sup>3</sup>) and salinity (grams of salt per kilogram of water, expressed as psu or ppt). Salinity is not commonly expressed in kg/m<sup>3</sup>. This is rather called salt concentration. It seems that the SLF model uses salt concentrations as salt masses are calculated based on water volume fluxes (m<sup>3</sup>). Please clarify this in the paper and change salinity into salt concentration, and vice versa, where necessary. If both concepts are used, it might also be worthwhile to provide a methodology or references to convert salinity into salt concentration, and vice versa. Edited: later in the method chapter, it is clarified that salt concentration and salinity are used to express the same concept. Although from a method point of view I can understand this, I believe that from a scientific perspective this assumption should be further clarified. Why are conventional methods to calculate salinities into salt concentrations not used? What is the impact of this on the results?

We now consistently use 'salt concentration' in the text. We also clarify this when the symbol is first introduced in Section 2.1.3.

We use this method of calculating salt concentration as it is a simplified method which reflects the hoped for practical engineering application of our developed tool. The required accuracy is therefore not stringently high. The impact on the results is not large, as shown by the validation, which gives acceptable to good agreement with measurements.

5. In the description of variables, such as salinity and density or water level it might be useful to also include the units to prevent misinterpretation of the variables.

We added units in places where a variable is first introduced, and the paper now also includes a chapter 'Notation' with all symbols and their units and definitions to prevent misinterpretation (per the remarks of Reviewer D).

6. In the conventions and assumptions, it is not fully clear that the fluxes of levelling, vessels out, density current, and vessels in occur individually and sequentially (although later stated in the paper). Moreover, it is not fully clear that the fluxes of multiple vessels occur simultaneously and instantly. The authors may want to clarify this earlier than in section 2.3.2.

The remark about the handling of multiple vessels was added at the end of Section 2.1.4. The division of phase 2 and 4 into subphases is now also briefly introduced here, but the reader is then referred to Section 2.3.2 for further details.

7. Figure 1: it would improve clarification if the authors can indicate which number is which phase: LT1 = levelling to sea, LT2 = door open to sea, etc. Also, I am missing the fluxes that indicate the density current. For clarification, it might be useful to

include phases 2a, 2b, 2c and 4a, 4b, 4c separately in the figure to highlight that sailing out of the vessels, density current, and sailing in of the vessels occur sequentially in the model (see also previous comment).

We updated Figure 1 to include all subphases and added additional clarification in Section 2.1.4 (as suggested in your previous remark).

8. In 2.2.2, the second paragraph (“The lock exchange...”), it is stated that the lock exchange is often the most important process, simply because the depth of lock chamber is often larger than the lockage prism. I partially agree with this statement. However, it is not mentioned to what extent this statement depends on the vessel volume compared to the lock chamber volume. In some situations, can it be that the impact of the vessel volume is more significant than the surrounding water volume, and therefore that the vessel fluxes are more important than the density current?

It is correct that the volume of the vessels plays a role here. However, this influence is in general relatively small as the average vessel volume (total of all vessels in a lock) is often in the order of 10% or less of the volume of the lock chamber. As an example: it takes a block-shaped vessel of 80% of the length, width and depth of the lock chamber to reach 50% of its volume. Such an occupation of the lock chamber volume is very rare. Furthermore the influence works in two directions. In case of a complete lock exchange, the volume of the ship in the lock reduces the amount of water in the lock, and thereby the total amount of salt that can be transported. In case of a partial lock exchange, for instance in the case of bubble screens, the movement of vessels in and out of the lock increases the exchange of water between the lock chamber and the approach harbour at either side. This leads to an increase of the salt intrusion.

In Section 2.2.2 we have now added a reference to Section 2.2.3: Ship displacement, where we further elaborate on the effect of vessels. We also now mention the effect of ship displacement in Chapter 3: Validation.

9. In section 2.2.4, the second paragraph (“In a calculation...”), it is stated that the model is run through a number of times, taking into account constant hydraulic boundary conditions. This is not clear to me. Could the authors explain why iteration is required. Moreover, how do these constant hydraulic boundary conditions work? For example, if you have varying water levels and salt concentrations at both the sea and canalside, how would the SLF deal with these in a lock-cycle averaged setting? Could the authors elaborate on this in the paper so that the work is more reproducible?

The determination of the salt flux on an aggregated level is often initially focussed on the average value of the salt flux based on the average values of all inputs, both the

hydraulic conditions and the lock operation. For the calculation of the salt flux one has to choose a value for the initial salt concentration in the lock chamber, but the computed value of the salt flux depends on this choice. This dependency is eliminated by the iteration as described, after which one is left with an average value that is independent of the (somewhat arbitrary) choice of initial lock chamber concentration.

As a further refinement of this initial average result, one can calculate how the salt flux varies with the tide and with varying salt concentrations. The same computation is then repeated for each set of variables, for instance supplied as a timeseries of water levels and salt concentrations. This will deliver a time series of the salt flux. In doing so, computation time is reduced by using the salt concentration in the lock at the end of the iteration in the previous time step as a first guess for the next time step.

In cycle-averaged calculations the user needs to define average boundary conditions that are representative for the period of time in which the user is interested (such as a yearly average, or during drought). We added this to the text of Section 2.4 (second paragraph), and all other texts concerning the cycle averaged equations (such as the contents of Section 2.2.4: Lock Operation) have been moved to Section 2.4 for completeness.

10. Regarding to the calibration factors, could the authors elaborate in the paper how these can be estimated when applying the SLF method for a new lock or existing lock without data? Do conventional values for these calibration factors exist, or do they depend on data, and what type of data? Shipping data? CTD measurements?

The calibration factors require registrations of the lock operation or, if unavailable, simulations or future projections of the lock operation. We added this to the text of Section 2.4 (first paragraph). We also incorporated Section 2.2.4: Lock operation into a new section: Section 2.4.3 to group all information on the calibration coefficients together for more clarity and completeness. Additionally, the parameters in equation 36 (the definition of the representative gate-open time  $T_{open}$ , which was equation 10 prior to restructuring) have been elaborated upon in the text.

5. Regarding the validation, the authors could provide more information to make the research more reproducible. Could the authors share in the paper or refer to more details on the:
1. Measurement setup: the locations of the CTD lines, the depths of each CTD sensor, the measurement frequency, etc (this is also not clear in the paper to which the paper refers).

We added some additional information about the set-up to the text and a reference to a report (Uittenbogaard (2011), grey literature and in Dutch) that goes into much more detail than the corresponding journal paper by Keetels et al. (2011). Adding all details here would, we feel, add significantly to the length of an already long paper.

2. Processing methods of the data underlying the salt mass results in figure 4. Was there any kind of (moving) averaging involved over time, space, and depth? The salt mass signal looks quite smooth. How were varying water levels and salt concentrations at both ends of the lock be dealt with in determining the intruded salt mass? Is this also corrected for in the results of the SLF?

The salt mass consists of the sum of the salinities measured by each sensor times a partial chamber volume around that sensor, resulting in a chamber-averaged salt mass. Because some of the sensors were attached to floating buoys while others were stationary (which is now mentioned in the text per your previous remark), the partial chamber volumes are not constant. No filters/smoothing was applied. The SLF assumes depth-averaged salinities at the boundaries and in the lock chamber, and as such could be compared well to the chamber-averaged salt mass that resulted from the measurements.

3. Data: the salt concentrations at both the sea and canalside, and the lock could be added as a fourth plot in figure 4.

We added a fourth plot to Figure 4 with depth-averaged salinities [ppt].

4. Results: which phases resulted in the most error? Why? Which processed could be improved upon in the SLF? Also: could the phases be indicated in Figure 5?

The salt transport caused by a levelling phase has a larger discrepancy with measurements than the salt transport caused by a lock exchange phase. This is now elaborated upon in Chapter 3: Validation. We also mention that research into future improvements is ongoing.

We added the phase numbers to Figure 5 but it was difficult to make it easy to read because the phases follow each other up quite quickly. Please let us know if this satisfied your request, or whether you prefer the previous version. You may note that phase 1 (levelling over the fresh lock head) often occurs both before and after phase 2. This is a result of the way in which the Stevin Lock was operated (at least at the time): the levelling valves remained open, perhaps to reduce the forces on the gate, during the gate-open time and only closed afterwards. So there is a small timeframe in which the levelling valves are open once the gate has closed. This triggers another levelling phase in the SLF, but the head difference is extremely small at that point.

6. In the application, I have a question about the following countermeasures which may suggest the authors to include more information in the paper:

1. Reducing door open times and clustering: in the first paragraph of 4.1 (“There are many...”), it is mentioned that reducing the number of lockages and door-open times can be achieved by clustering vessels close to the lock doors. However, in the

third paragraph of the conclusion (“Salt intrusion through...”), it is rightfully mentioned that with clustering the gate-open times will increase. Could the authors comment on to which countermeasure, vessel clustering or minimizing door-open time, would be more effective in what case? Is minimizing the door-open times realistic in terms of vessel safety (preventing vessels to hit a malfunctioning lock door and other vessels while manoeuvring in locks)?

Clustering multiple vessels will increase the gate-open time compared to a single vessel. But dividing multiple vessels over multiple lockages means that the time it takes to open and close the gates is included in every single lockage (and the lock exchange flow starts right away when the gates start to move), whereas clustering only requires opening/closing once. A ‘full’ lock chamber also has another benefit: any space taken up by vessels cannot be taken up by saltwater. You are right that minimizing gate-open times can influence nautical safety.

We added additional reasoning for reduced gate-open times and some examples on nautical safety to the text of Section 4.1. We also added a sentence about nautical safety to the Conclusion per the remarks of Reviewer D.

2. Sill: could the authors explain why sills are applied instead of total decrease of the depth of the lock chamber? Should there always be a significant depth in the lock chamber to improve the manoeuvrability of vessels navigating within the lock, and/or decreasing the forces on the moored vessels during levelling?

Just like a shallower lock, a lock with a sill will have a smaller depth over which the lock exchange flow can take place. A sill also “traps” the saltiest bottom layer inside the lock chamber, which then cannot be exchanged (smaller effective lock volume). Sills are often already present in locks to support the gates, but they are also potential mitigation measures that could be added to existing locks, perhaps in a way that allows (dis)assembly in dry seasons or for vessels of certain size (draught). In view of salt intrusion a lock should not be deeper than required for the vessels passing the lock. For existing locks fitting a (movable) sill can be an option. We added this to the start of the paragraph on sills.

3. Closing both lock doors in between two operations and/or two vessel arrivals: does the SLF include closing the doors in between lock operations and vessel arrivals?

The gates are only closed between phases (1, 2, 3 and 4). The subphases (2a, b and c or 4a, b and c) of the SLF occur one after another without closure of the gates in-between.

Instead of closing between vessel arrivals it should be considered to close the gates after all vessels have left the lock, and then only opening them when either a sufficient amount of vessels has arrived/are arriving, perhaps at a scheduled time. This allows clustering while avoiding the gates being open unnecessarily.

4. Length of lock: in the first paragraph of section 4.2 (“Sea level rise...”), it is mentioned that “Contrary to increasing the length or width of the lock chamber, the consequence of increasing the depth is twofold...” Would the impact of the increasing length of the lock chamber be less than the impact of increasing its width?

The effect of the width is linear and affects the lockage prism. The length affects both the lockage prism and the amount of time it takes for the density current to travel from gate to gate and back. As such the effects of the length and width aren’t equal, which may have seemed to be what we meant by the cited sentence, but are not as strong as the effect of a depth increase.

We therefore decided to remove the first part of the cited sentence and then added some clarification on these effects at the end of the paragraph.

7. Section 4.3: Could the authors explain a bit more elaborately how the SLF method can result in boundary conditions for a numerical model. As based on the operation of the lock complex, the salt concentrations at the sea and canal can change, and thereby the discharges and salt fluxes into and out of the canal. Can the SLF be applied online (in parallel) as boundary condition (updated by salt concentrations by the numerical model)? Or is it preferred to first calculate the discharges and salt fluxes and pose them offline (in series) to the model as boundary conditions?

Both are viable options as SLF calculations are very quick. We’ve added some elaboration to the text.

8. Discussion before the conclusion and recommendations: the conclusion is clear. I however recommend the authors to include some part of discussion on the strengths and weaknesses of the model. What can be further improved upon given the method? Or is a 7-10% underestimation good enough? Can the same accuracy be reached when applying the SLF in cycle-averaged mode? What would be the difference between the cycle-averaged and phase-wise application in terms of accuracy? Can the authors comment on the expected error at other (types of) lock complexes? What are the causes of the underestimation? Are there any recommendations for further research and updates of the model? What is further needed to apply the model to Panama? Is there current work going on?

The user’s choice of boundary conditions is key when using the cycle-averaged SLF. Calculations for a different lock in the Netherlands showed that good accuracy (within 10%) can be achieved with cycle-averaged calculations if representative boundary conditions are chosen. We plan to publish another paper in the future that details such calculations.

Because of the choice for a cumulative parameter as the validation parameter, it is expected that a longer continuous series of lockages (multiple days or even weeks instead of single days)

would yield a more accurate result as the over- and underestimations continue to cancel each other out with every cycle and will eventually be more stable. We have now included this remark in Chapter 3: Validation.

The salt transport caused by a levelling phase has a larger discrepancy with measurements than the salt transport caused by a lock exchange phase. For the Netherlands, the lockage prism is generally much smaller than the lock exchange volume (which depends on lock dimensions and gate-open time), and so the contribution of this discrepancy to the total salt transport is relatively small. This may be different in case of very large head differences combined with short/narrow locks or very short gate-open times. We have also included this remark in the Validation chapter now. Also, the discrepancies in levelling phases are both underestimations and overestimations which cancel each other out to some extent.

We are currently in the early stages of exploring how the SLF can be utilized for locks in series such as the Panama locks. We expect that some modifications are needed to capture the various ways of salt transport (between lock chambers, via cross-filling between parallel lanes, or with water saving basins).

We are also currently working on the implementation of the SLF in decision support systems for operational saltwater management and in hydrodynamic models (D-HYDRO suite). We added a remark on this to the Conclusion.

## Reviewer D

### General

- This paper clearly describes a model which simulates the salt transport through a shipping lock, to be used as a boundary condition for a large scale detailed dispersion model. Thus, the salt intrusion into the greater water system can be determined.

Dear reviewer,

Thank you for taking the time to review our paper. In particular your thorough look at the equations and parameter definitions as it allowed us to fix some inconsistencies.

You will see a tick mark after some of your remarks – these mostly relate to punctuation or word order and were implemented directly (with tracked changes) without further explanation by us. For all remaining remarks we briefly describe how we implemented your suggestions. A few of your suggestions were related to the use of another word for what is already an established variable name in the SLF. We chose not to change those.

Kind regards,

The authors

- The line numbers are missing. Then, the following comments are more difficult to locate in the text.

Line numbers have been added for more convenience during the next round of reviews.

- The novelty treated in this paper is not well substantiated: in Keetels (2011): ‘By using a conceptual model for salt transport through a shiplock, we can compute salinity transport through the shiplock if a reliable estimate for the effectiveness of the different measures (indicated as the salt transmission factor) is available. The description and validation of this model will be published elsewhere.’ It seems that this type of model was already available in 2011 and probably published elsewhere. What is known in literature about this earlier version of the model. Why is not referred to this model. What are the novel aspects of the present model compared to the earlier model?

To our knowledge, Keetels et al. did not end up publishing the described model or the validation. The model that Keetels et al. refer to is the so-called “Zoutlekmudel”, which is also the subject of the report by Uittenbogaard (2010) that you mention in your next remark. This model and its validation remains only available in (Dutch) technical reports. The model presented in our paper works along the same lines as that of earlier models and we use the same dataset for validation. We do not therefore claim that this aspect is novel. We submitted an earlier version of this paper to this journal without the validation section and were explicitly asked to add it for this submission. We think this adds to the robustness of the work presented here because it is the first time that such a validation is published in the scientific literature. We have added some references to conference proceedings where the application of these models is described without giving details of the model.

A novel aspect of the model is that it is open-source and freely available via GitHub. The SLF is generally applicable and wrapped in a user-friendly Python package with online documentation and examples. The Sea Lock Formulation’s predecessors, the ‘Zoutlekmudel’ and ‘WANDA-Locks’ (both now mentioned in the paper) are currently not freely available or supported. It has become clear now that the open-source aspect was not evident from the first version of the paper and has now been emphasized. The name of the repository and the python package were redacted for now to maintain the anonymity of the double-blind review process. This will also be included in the data access statement under Acknowledgements (which is to be inserted when the paper is accepted).

Another novel aspect is the focus on lock operation and with that its applicability in salt intrusion management. The use of a calibration factor (the  $c_{DOT}$ ) to determine the representative gate-open times was not part of previous models, which averaged out the operation of the lock.

Finally, in this paper we use this model to draw conclusions about the likely effect of climate change (sea level rise and reduced freshwater availability during droughts) on salt water

management of fresh water reservoirs with shipping locks. To our knowledge these insights are novel in the scientific literature and can only be reached with a model built along the principles outlined in our paper,

- How does the presented model relate to the following report:

Uittenbogaard, R.E. (2010): Voorstudie: Ontwerpstudie en Praktijkproef Zoutlekbepierking Volkeraksluizen, Model voor Zoutvrachtberekeningen, Deltares, 2010a, Report 1201226-011. (in Dutch)

The ‘Zoutlekmudel’ from Uittenbogaard (2010) was developed at the time of the field measurements as described in Keetels et al. (2011) and is a partial inspiration for the model presented in this paper. The validation data also originates from that same project (Uittenbogaard (2011), report 1201226-005), a reference to that report (grey literature, in Dutch) has now been added in addition to Keetels et al. (2011), which is also related to that same measurement campaign.

Another model came after the Zoutlekmudel, WANDA-Locks, which was developed for a specific project (renovation of the Krammer Locks) to include flow dynamics in culverts, etc. WANDA-Locks is a lot heavier computationally, and requires a lot of detail of the exact inner workings of the levelling system. Subsequently, it was assessed that the essence of the processes could be captured in an event-based box model like the one presented in this paper. The Sea Lock Formulation was then developed, which is computationally efficient, generally applicable and does not require as much detailed input. The authors believe that this latest model incorporates the important understanding of the issues concerning salt intrusion at locks developed over many years of projects and the development of these earlier models and that the scientific literature on this subject would benefit from its publication in this form.

### Abstract

‘...canals, protected by coastal dams.’ Not clear.

We chose to omit ‘protected by coastal dams’.

### Keywords

Add a keyword related to the model.

We added ‘event-based model’ and ‘box model’. We avoided using ‘event-based box model’ all together as a keyword as this does not appear to be an established term, but we do use it in several other locations now per the remarks of Reviewer B.

### Introduction

- Show consistent use of The Netherlands or the Netherlands throughout the text.

‘the’ Netherlands is now used consistently throughout the text.

- P2: ‘The Netherlands is a highly densely populated country located in a low-lying delta which can experience saltwater intrusion into groundwater, open estuaries and into freshwater lakes and canals. Specifically for the Netherlands, models have been developed for a variety of purposes.’ Change the sequence of the sentences. ✓
- P2: ‘The exceptions include, the redirection of river discharge...’ without comma. ✓
- P2: ‘For shipping locks at the interface between the sea and lakes and canals...’ Or canals. ✓
- P3: This paper details a calculation method, a set of formulae, that is capable of ‘...’ This should be mentioned earlier in the text. Reshuffle the introduction for a more clear order.

The section you cite here was moved to the end of the first paragraph of the Introduction. This is also where some remarks are made on novelty based on your earlier remark under General. We did some additional reshuffling also per the remarks of Reviewer B, for example grouping together all information that is specific to the Netherlands.

## Methodology

- P3, 2: define the ‘...locking cycle...’

Definition added.

- P5, 2.1.4: ‘...in Figure 1 for the discharge: positive in the downstream direction, meaning towards the sea.’ In Figure 1, during high tide, the levelling flows are directed inland (‘downstream’ associated with a river?).

That’s right – we chose the dominant flow direction of rivers and deltas, i.e. downstream, towards the sea, from fresh to salt, as the positive direction for the SLF. We added some clarification to the text.

- P5, 2.1.5, Equation 3/Equation 4: in this way the always occurring water level differences between both sides of the active gate at the end of levelling are neglected.

We added a footnote in the line that introduces Equation 3 and 4 that touches on this.

- P7, 2.2.3: ‘..., water is pushed out of said lock chamber.’ Said?

‘said’ refers to the previously mentioned lock chamber. We replaced ‘said’ by ‘the’ for more clarity.

- P7, 2.2.3: ‘This value can differ for both directions: upstream and downstream. Do note that these two values do not have to be equal.’ These sentences say the same.

Replaced by a single (different) sentence.

- P8, 2.2.4: ‘This will generally not be the case: spreading...’ In reality, this will usually not be the case; ... ✓

- P8, 2.2.4: ‘...that lock gates are open, and during which the lock exchange flow can take place.’ ...when the lock exchange flow takes place. ✓
- P9, 2.2.4, Equation 11/Equation 12: these equations seem invalid;  $[s] = [-]/[s]$ .

You are correct – it should have been multiplication, not division. We updated the equations. Due to restructuring per reviewer B’s remarks, these equations now have numbers 37 and 38.

- P11, 2.4.1, Equation 33:  $M_{F,4}$  has not been introduced. Thus, two exchanges at the fresh side are included? Explain.

Great observation.  $M_{F,4}$  is the salt mass transport over the lock head on the *freshwater* side (suffix F), during phase 4 (suffix 4), which is the lock exchange over the lock head on the *saltwater* side. The only circumstance under which  $M_{F,4}$  is non-zero is when there is a flushing discharge over the lock head at the freshwater side during phase 4. We decided that we will not include flushing discharges in this paper (briefly mentioned in Chapter 1: Introduction), but we overlooked this parameter in the equations. The same holds for  $M_{S,2}$  in equation 38. Both have been removed. Please note that due to restructuring per reviewer B’s remarks, these equations (33 and 38) now have numbers 30 and 35.

- P12, 2.4.1: ‘The discharge and salinity which is transport...’ Transported. ✓
- P12, 2.4.2, Equation 38: idem Equation 33. Explain.

Please see our response to your remark on Equation 33.

- P12: Stevin Lock instead of Stevin lock.

We now consistently use ‘Stevin Lock’ throughout the paper.

- P12, Tabel 1: lock (chamber) floor instead of lock bottom.

‘lock\_bottom’ is the name of a variable in the (already in-use) model and we have therefore decided to not change it here.

- P13, 3: ‘levelling gates’, levelling valves.

We replaced the use of ‘gates’ by ‘valves’ in all places concerning the levelling system, including Figure 4.

- P13, 3: ‘...to later on be able to reconstruct...’ To be able to reconstruct later. ✓
- P13, Figure 4: how are the mitigation measures included in the validation, or, how should the performance of the mitigation measure be valued? Reference?

We did not have long series of lockages with only a single applied mitigation measure available to us. The measured data of the Stevin Lock mostly contains combinations of mitigation measures from which we would then not be able to distinguish which measure caused the

deviation. We therefore decided to only present validation of the base equations of the SLF for now. The equations that describe the mitigation measures in the SLF, however, are based on studies such as Keetels et al. (2011) and therefore do have a basis in field and laboratory observations.

- P14, Tabel 2: considering a lock length of 200 m, this is a relatively wide lock.

You are correct that locks of similar width are usually longer (Noordersluis IJmuiden, Nieuwe Sluis Terneuzen). We do not expect this to have a negative or unexpected influence on the results, particularly because we only compare the result to other calculations of that same lock and do not make any statements about the influence of this particular aspect ratio.

It was our intention to really use a ‘fictive’ lock as to not make any statements about existing locks. One way of interpreting the fictive lock would be to view it as two side-by-side locks that each have typical dimensions for inland shipping.

- P14, Tabel 2: ‘leveling time’, levelling.

‘levelling\_time’ is the name of a variable in the (already in-use) model and we have therefore decided to not change it here.

- P14, Tabel 2: ‘calibration coefficient’, which calibration coefficient?

‘calibration\_coefficient’ is the name of a variable in the model. It refers to the calibration coefficient on the gate-open time that allows the user to take the irregular distribution of locking cycles throughout the day into account in cycle-averaged calculations ( $c_{DOT}$ ). We added a footnote for clarification.

- P15, 4.1: ‘In practice, this may mean that ships might need to wait...’ In practice, this could mean that ships will have to wait... ✓
- P16, Figure 7: to put this figure in perspective, in The Netherlands the sea level rise until 2100 according to the KNMI-2023-Climate-Scenario’s is somewhat more than 0.5 m.

We added the ranges (lower limit of the ‘low’ scenario up to the higher limit of the ‘high’ scenario) of predicted sea level rise for the Netherlands to Figure 7 to aid in the interpretation of the figure.

- P17, 4.3: ‘...have differing salinities.’ Have different salinities? ✓
- P17, 4.3: ‘...i.e. close the background salinity.’ Close to the... ✓
- P17, 4.3: “... (orange dash-dotted line in Figure 6).’ Figure 8. ✓
- P17, 5: ‘...broken up into different distinct phases, ...’ Remove ‘different’. ✓
- P18, 5: ‘...each of these relations...’ Start with a new sentence.

We restructured the previous sentence instead.

- P18, 5: ‘...will be a mixture of the salinities of the outer and inner harbour.’ ...shows an intermediate value due to the influence of both outer and inner harbour waters. ✓
- P18, 5: ‘...if as a consequence of...’ if as a result of. ✓
- P18, 5: ‘However, shortening the gate-open times is a very effective measure to reduce salt intrusion.’ In the case of large sea locks, used for the passage of large sea-going vessels, it is often required that the gate is open for a longer period because it takes time for the large vessels to leave and enter the lock and the lock approaches, and it could be unsafe for the large vessels to enter the lock during the lock-exchange flow. Another cause for a longer gate-open time is the minimization of the collision risk when a large vessel manoeuvres towards the lock; in the event of an accidental gate failure, the vessel must retain sufficient remaining sailing length to ensure safe stopping.

We added the examples you used here to Section 4.1 (also per Reviewer B’s remarks) and added a generic sentence about the importance of nautical safety in Conclusions (directly after the sentence you cited).

- P18: include a statement on the use of AI.

We did not use AI in the creation of the model or the paper. We added a subheader to Acknowledgments to make this statement.

- P18: include a notation of the symbols which are used in the equations and formulas.

A chapter Notation was added with a list of all symbols with units and definitions.

Some inconsistencies that came to light while making this list were fixed with tracked changes throughout the paper (such as the symbol for Equation 22, and the consistent use of the suffix ‘Lev’ instead of ‘Level’ or ‘level’).

- P18, References: follow the formatting of the template.

The references use Word’s built-in APA format. The journal only suggests the use of author-date style on [this page](#), please let us know if a style other than APA format is more suitable. We also updated the font from JCHS\_maintext to JCHS\_references.

Please let us know in the next round if we misunderstood your remark and this is not what you meant by formatting.