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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Brief communication: Seasonal prediction of salinity intrusion in the Mekong Delta" *by* Heiko Apel et al.

Anonymous Referee #2

Received and published: 21 February 2020

The authors provided an interesting manuscript on a topic that has strong relevance for actual societal problems in Vietnam and likely beyond. A seemingly novel method for long-term forecasting of salt water intrusion in cultivated lowland areas is presented, which could provide useful early warning information for damage control in agricultural production. Statistical tests by the authors result in good confidence of model performance, leading to recommendations for wider application. However, a precise idea of the actual added value of the proposed model is not communicated clearly enough, due to several reasons. These are discussed in detail below, but can be summarised as a lack of description of similar existing models, the description of input data used, and the limited possibility for model adoption due to a limited description of the model itself and data requirements. In addition, certain aspects of style, grammar, accuracy

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of statements and embedding in literature should all be improved in order to achieve an appropriate quality for scientific publishing with this high-profile journal.

Nonetheless, the reviewer believes there is strong potential in the manuscript (especially due to the apparent societal demand for the model); and as such, a major revision is recommended with strong encouragement for follow-up by the authors. In order to allow improvements on the remarks made by the reviewer, a long but practical list of suggestions are provided in this documents (general and specific comments), as well as in the marked manuscript document (single-word suggestions).

General comments: The abstract is rather concise, and although this can be appropriate regarding the total length of the article, perhaps a few pieces of information could be inserted. For instance, the authors could improve the technical aspect of the abstract by briefly describing the type of data that predictions are nested in (i.e. drought or ENSO indices), or by providing some quantification to support the claim for "high skill".

The introduction section (chapter 1) clearly emphasises the importance of forecast models with an extended lead time (i.e. months rather than weeks). However, it is unclear whether such models already exist, and thus what is the novelty of the existing work. The authors should dedicate a few lines nested in scientific references to clarify this point, and thus to justify the relevance of their own contribution. In general, the use of literature is quite marginal in the manuscript, and embedding the proposed research in the scientific context is an integral part of scientific writing.

The description of methodology (chapter 2) deserves some critical attention to ensure an appropriate description of processes, used data, and analysis methods. The reviewer refers to the marked manuscript as well as the specific comments provided below for all of these points.

With regards to the results (chapter 3), the authors seem to present a robust set of statistical testing for findings optimal predictors. In the final lines of this chapter, an

interesting point is made about the validity of ENSO-based predictions (optimal on long-term) and SSI-based predictions (optimal for short-term). Was any performance testing done where the two indices were combined, as an “optimised predictor”? If not, the authors may discuss the possibilities for this in future explorations. In addition, chapter 3 in its current form does not provide any discussion with regards to previous scientific works (e.g. regarding other long-term forecasting models), but is mostly restricted to “results”.

In the conclusions section, potential application of the proposed model is well described and its wider use is encouraged. However, the requirements with regards to data availability are not entire clear. The authors mention that data availability should be “sufficient”, but do not specify or quantify what is the required coverage of flow data and the expected impact on prediction accuracy. This actually links back to the methodology section of the manuscript, where a quantification of data coverage in the presented study is missing as well. More clarity is required on this topic, both in described methodology and in recommendations for future applications.

Specific comments: - Page1,Line7: While acknowledging the Mekong Delta as the most important Vietnamese food production area, the value of this zone with regards to agriculture and food security could be more strongly emphasised by adding two pieces of information: (1) the fraction of rice production out of total (staple) food production in Vietnam; (2) the importance of “nationally produced” food vs. imported food with regards to food security (or possibly exported value). A second line including such information would create a more solid argument as to the context of salt water intrusion and its negative impacts.

- P1,L19: Is indeed the frequency of droughts, rather than the likely duration of the most severe drought (period), the major manifestation of climate-induced intrusion?

- P1,L20: “. . . agricultural production system and peoples livelihood developed over historical periods and thus adapted to normal intensity of salinity intrusion (. . .)”. This

sentence reads slightly unclear and could likely be simplified, e.g. as "...agricultural production systems and livelihoods over time adapted to a given intensity of salinity intrusion (...)"

- P1,L23: please specify "unprecedented high salinity intrusion"; i.e. was the 2015/2016 event characterised by the time duration of salinity issues, or rather its concentration, or the groundwater depth in which salt water was found, or measured in terms of agricultural losses, etc. etc.

- P1,L29: the current figure fails to show what are "coastal areas of the delta" or rather land-locked areas (also see multiple comments posted in the PDF version of Figure 1). Please modify the map accordingly.

- P1,L29: please provide the percentage of this economic damage in respect to total value of national agricultural production for reference.

- P2,L35: is terming saltwater intrusion as agricultural drought an original idea by the authors, or has this been defined as such before by the scientific community (if the latter, please provide appropriate referencing).

- P2.L40: please clarify what type of "flow" data is required for these hydrological models.

- P2.L44: please clarify what is meant by "rainfall anomalies deficiencies"

- P2.L45: please rephrase the following sentence while using correct usage of verbs and grammar: "Additionally sea levels around the Mekong Delta continue to rise (Smajgl et al., 2015), thus causing increasing backwater effects restricting the discharge during the dry season and consequently promote salinity intrusion."

- P2.L54: "whereas" suggests a contradiction between the previous and following sentence parts, but this is not the case. Please rephrase.

- P2.L54: in addition, this is an unnecessarily long sentence that could easily be split

into two.

- P2.L61: this statement is hydrologically disputable: if the authors are describing the long-term processes that connect monsoon rainfall and river flow that follows weeks/months later, “runoff” seems the wrong terminology. Where the latter describes the fast process of overland flow, the former is generally related to processes of infiltration, groundwater processes and surface water buffering such as retention.

- P3.L77: “The salinity intrusion in the Delta is measured by the hydro-meteorological services. . ..”: Firstly: how is this being measured (what instrumentation)? Secondly: please clarify “services” that are measuring the process.

- P3.L78: “The measurements are, however, not continuous, but typically performed for several days in a row, with some days without measurements in between.” Are there any conditions that determine whether measurements are taken (such as high expected intrusion)? This may create a bias in measurements, which should be discussed by the authors.

- P3.L81: “The salinity measurements covered the time span 1996 – 2016”: please specify whether this was a 100% coverage or provide another grounded estimate.

- P3.L85: what is the reference for this salinity threshold? Please clarify sources (same in L90).

- P4.L96: what do these ENSOxx indices represent, and how is the selection of these particular indices justified?

- P4.L109: please provide adequate sources for reference with regards to statistical methods applied (throughout L106-109).

- P6.L174: the meaning of the final part of the final sentence is not very evident. This vague statement contrasts the practical and specific recommendations made in the previous sentences. Please rephrase for clarity.

- Figure 1: major revision recommended with regards to style, intuitiveness and clarity, see marked manuscript document for all comments.

Technical corrections: See marked manuscript attached for any technical corrections related to wording or style.

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-276/nhess-2019-276-RC2-supplement.pdf

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