

Interactive comment on “A study of the climate change impacts on fluvial flood propagation in the Vietnamese Mekong Delta” by V. P. Dang Tri et al.

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We would like to thank the reviewer for taking time in reading and suggesting modification to the paper. We found all comments very useful to improve our paper.

Answers to the raised comments are as follows:

Comment 1: *General comments:*

This paper presents some interesting hydrologic modeling and impact assessment work pertaining to the potential effects that climate change may have on the Mekong Delta in Vietnam. This type of research is important to help identify appropriate adaptation measures in a region that is susceptible to climate change. The topic

certainly is relevant to Hydrol. Earth Syst. Sci. Discuss. and potentially the manuscript can make a good contribution to the literature, but before it can be accepted for publication I recommend major revisions both to content and also to the writing. I will address content issues first:

Authors' answer: We would like to thank the reviewer for taking time and evaluating the topic of the paper as interesting and as well for finding the paper useful for the HESS readers. We find all the comments of the reviewer, useful and we are following the suggested changes so that the manuscript is improved in readability and clarity.

Please find our answers to all raised issues bellow. They are addressed one by one as they are raised in the interactive discussion.

1.1: *The last paragraph on p. 7230 is not necessary..*

Authors' answer: We will eliminate that paragraph.

1.2.: *P. 7231, around l. 17 – how is flow routed and redistributed? Flood gates and control works? Please be explicit for those not familiar with the complex hydraulics of the region.*

Authors' answer: In the VMD the flows are driven by the upstream discharge entering from Cambodia to Vietnam, and by the water level of the East and West Sea. The VMD topography is quite plan and the water level differences between the East and West Sea determines that flood discharges are mainly routed to the East Sea. Historical data illustrates that discharge enters the two main branches of the VMD, Mekong and Bassac, unequally, and it is expected that this would be even more unequal in the future, when a sharp rise and drop of the flood hydrograph will happen. We will add more details and descriptions on the flood gates as well in the final version of the manuscript, so that the flow dynamics in the area is clearer.

1.3. *Section 2 – Model Setup – Is the ISIS model a 1D, 2D, or 3D model? I*

realize this is discussed a bit on p. 7239, but some discussion should be presented at the beginning, to set the stage for the reader. Why did the MRC suggest ISIS not be used for design purposes; what were the shortcomings of the model? Has the model been fully calibrated previously? Discuss this. In general, more details about the model are needed. What time step is used for the modeling? Daily? Also, daily rainfall? For how long is the model run – one year of data at a time? Is the model sensitive to start-up conditions so that there tends to be greater error near the start of the simulation period? Are 14 rain gauges enough to capture spatial variability of rainfall in this region?

Authors' answer: Thank you for pointing out to us that the hydrodynamic model is not described detailed enough, we will detail it more in this section, in the new version of the manuscript. Initially we wanted to focus on the flooding results due to SLR, rather than making long introductions to the model, but we will include the information in the new version of the manuscript. The information is as follows: The ISIS model is a 1D model. There is no need to use a 2D or 3D model in the area, because of the dense canal network that is represented in the model, and which captures well the physics in the area.

The question regarding the MRC advice, might be a misunderstanding generated by the authors. We did not intend to mention that MRC does not advice the usage of the model for design purposes, we simply stated that the ISIS model was developed by the MRC for the planning purposes at the whole Delta scale. As such it does not reflect all in detail. However, it can be used to project the trend of hydrodynamics changes in the future, especially in the context of climate change. Because the paper wanted to look at hydrodynamics trends in the delta the planning model is very good. The planning model was always used for testing different project proposals. In our case we used further the model to test the effect of the changes in climate on the VMD. There is no need for any design at this moment, nor in the future, therefore this was not needed to be addressed. This is also why there was no “shortcoming” as an issue. The model simulates very well the actual situation.

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The applied ISIS-1D model was fully calibrated by the MRC and Fig. 4 of the paper is presented to demonstrate that the ISIS-1D model could be used for the present study. In the applied ISIS-1D model, the time-step was 1 hour and covered a period from July 1st to Oct. 31st (flood period in the VMD) for different years corresponding to different scenarios.

Sensitivity to start-up conditions? This model is a hydrodynamic model and as such it is not sensitive to the start-up conditions which are the exact conditions (measured discharges and water levels) for the calibrated model. There are no errors in this respect, it is a deterministic model. However if we would like to consider uncertainties of the boundary data for the year 2100, then these will give different results. A study was done for evaluating these uncertainties, and it is prepared for a separate publication. The present paper would have been too long to cover the uncertainties as well.

We would like to point out to the reviewer that the description of the ISIS hydrodynamic model in section 3, contains the whole description of the model received from MRC, including the rainfall-runoff component in it. For the purpose of the present study only the hydrodynamic model, was used, where the rainfall gauges are not needed. The rainfall gauges were mentioned to give an idea about the overall model and the monitoring in the catchment, The hydrodynamic model uses boundary conditions upstream (i.e. discharge hydrograph) and downstream (water levels in the sea). The data is enough, because the model is calibrated based on measured discharges.

1.4. *P. 7233, around l. 25 – it is noted that with future development upstream, less water would arrive in the VMD – but no details are provided about why this is the case until p. 7234, l. 7-8. This explanation should be moved up. Also, how does the “water demand” compare to the potential impact of the upstream constructed and planned dams in China? I think the issue of the potential impacts of the dams in China really needs to be more critically discussed in this paper. I recognize it’s a sensitive topic, but it’s an important one.*

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Authors' answer: The impacts of the upstream constructed and planned dams in the Upper Mekong Basin (in China) have not been clearly identified for the following reasons: Firstly, this analysis was not at all within the scope of this study, and, secondly, data for such evaluation was not available. The aim of this paper is to analyse the impact of sea level rise on river flood dynamics. The flooding conditions may change during dry and wet periods (reflected in discharge changes at the most upstream point of the VMD) and this was analysed in the article. These variations of upstream discharge are assumed to be as in the current situation. The main assumption in this study is that in case of dam construction any of the VMD upstream discharge distributions will be similar as in the current situation, as has been agreed in the international treaties on Mekong.

1.5. *Section 3.1.1 – what are the major sources of model error? Some of the discussion presented Section 3.4 (e.g. around lines 20-25) should be moved here.*

Authors' answer: Thank you. We will make the correction.

1.6. *It is odd that Figure 4 is referred to after Figures 5 and 6 in the text (p. 7235).*

Authors' answer: Thank you for pointing this out. We will make re-numbering of figures so that they are also referred in text in the order of numbering.

1.7. *P. 7238, around l. 15-17 – it seems here that you conclude there are relatively small impacts between Scenario 1 and 2 (development vs. no development), yet earlier you said development would result in lower flows. Please clarify this. Part of the confusion, I think, stems from the fact that Scenario Numbers vs. Run Numbers are difficult to keep straight, as you present in Table 1. Maybe just call them model runs 1, 2, 3 and 4? Also, Table 1 does not make the difference between Scenario 1 and 2 clear – the land use differences should be summarized in Table 1.*

Authors' answer: We will make the requested modifications, so that things are

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clearer between scenarios and runs and their order.

11.8. *Section 3.4 could be edited and eliminated. Much of this section reads like a conclusion, so a shorter, edited version could be put in the Conclusion section. The exception to this comment is that the discussion of the 1D/2D modeling could be placed more appropriately in Section 3.1.1 – Comparison of the Results to Other Studies.*

Authors' answer: We will revise this and change the manuscript accordingly.

1.9. *P. 7242 – you make an interesting point regarding water storage in back-swamp areas. Have these areas been drained for agricultural and/or urban development/settlement?*

Authors' answer: In the last 3 decades, flood water was kept in the back-swamp areas during the flood period and later on the stored water was routed downstream during the dry season. In this way the routed discharge was used for agricultural activities and in the same time helped in eliminating the saline intrusion in the coastal areas. Such the room-for-water existed before 1980s, as well, but converted in rice fields, which leads to the great concern that there is no room-for-water any more in the VMD, and the VMD would be more sensitive to the annual floods. These areas were not transformed in urban areas. We will make it more clear in the paper.

1.10. *For clarification (I realize it's also noted on p. 7236), I think for all duration difference maps it should be noted in the figure caption that a negative difference represents a longer flood in 2050 vs. 2000. It also is not clear what the terms "low" and "high" in the duration difference map actually mean. For example, in Figure 7, -1 is listed as "high" and -3020 is listed as "low".*

Authors' answer: Thank you for making this clear to us, we will improve the descriptions.

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1.11. *Figures 16-20 are small and difficult to read. Given that the manuscript has an excessive number of figures (21!), possibly figures 17-20 could be removed, without major loss of information.*

Authors' answer: We will revise the paper and see what can be reduced in the new version of the manuscript. We will try to reduce as much as possible, as suggested by the reviewer.

1.12. *P. 7242 – it is noted that adaptation to climate change should be integrated in social economic planning at all scales. This is a nice guiding principle, but I suspect it easier said than done. What are the barriers to implementing such a principle in Vietnam? How can these barriers be overcome?*

Authors' answer: All the mentioned adaptation strategies were studied in detail by different authors (i.e. ADB, 2010; Mainuddin, 2010; Nguyen et al., 2006, Belder et al., 2004, Hoanh et al., 2003 – as they have been mentioned in the paper). These studies are presenting both positive and negative aspects and give suggestions on the conditions that the adaptation strategies can be applied. We are pointing them to the reader, because they were not within the scope of our study. E will make it clear in the revised version of the paper.

Comment 2: *Writing: I recognize the difficulty of writing in English when it is not the author's first language. However, for a publication in an international, English-language journal, it is important that the writing is clear and as free of grammatical errors as possible. This is not to say that the authors should not have some latitude in using expressions that are not specifically "North American" or "European". However, passages such as the following (in yellow highlight) are difficult to understand:*

Authors' answer: We will carefully revise the manuscript from English point of view.

2.1. *In the VMD, the future sea level rise might result in a large inundated area (mainly along the east and west coast; Fig. 1) (WWF, 2009) leading to the*

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significant lost (should be loss) of mangrove forest and agricultural lands while the livelihoods of about 1.9 million local residents 20 would be at risk and extended from 2000 to 2050 (Ericson et al., 2006).

Authors' answer: Thank you for pointing this out, we will revise the manuscript.

2.2. *An important element in conducting a good study based on a flood modelling for the VMD and its vulnerability to floods (Delgado et al., 2010; Dinh et al., 2012), consists of the effects in the economies that are in relation to the land use og the agro-ecological zones that were derived from a land use map of the year 2006 (Fig. 3). I think this could be more clearly and succinctly worded as: To be of greatest benefit, flood studies in the VMD should consider socio-economic impacts related to land use in the agro-ecological zones, in addition to the basic hydrologic/hydraulic modeling.*

Authors' answer: Thank you, we will revise it.

2.3. *There are a number of typographical errors as well. A careful proof read and consultation with an English language editor is needed.*

Authors' answer: Thank you, as mentioned we will carefully revise the manuscript for typing errors and English.

Authors' final remark: All our responses to the questions raised by the reviewer will be included in the revised version of the manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 7227, 2012.

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