

Referee Comments

for V.P. Dang Tri et al. "A Study of the climate change impacts on fluvial flood propagation in the Vietnamese Mekong Delta"

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:

1. The last paragraph on p. 7230 is not necessary.
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.
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?
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.
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.
6. It is odd that Figure 4 is referred to after Figures 5 and 6 in the text (p. 7235).
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.
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.

9. P. 7242 – you make an interesting point regarding water storage in backswamp areas. Have these areas been drained for agricultural and/or urban development/settlement?
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”.
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.
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?

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:

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 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).

Or:

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.

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