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Interactive Comment

Interactive comment on "Strong increases in flood frequency and discharge of the River Meuse over the late Holocene: impacts of long-term anthropogenic land use change and climate variability" *by* P. J. Ward et al.

P. J. Ward et al.

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We are very thankful for the positive remarks from the reviewer, and for the detailed reading and useful suggestions. Below we respond to the more detailed comments.

1. The reviewer raises a valid point, namely that our conclusion states that during the 20th Century climate change has had the most impact on discharge (rather than land use), but that this is logical since we assumed a constant land use over the course of the 20th Century. In fact, the conclusion should state that if we compare the discharge characteristics of the 20th Century to those of the preceding (19th) century, this

EGU

increase in discharge and flood frequency has been mainly caused by changes in the climate. Between these two centuries we did simulate a land use change (namely an increase in forest and urban area, and a decrease in agricultural area). We will amend the conclusion in the revised manuscript accordingly.

Furthermore, the reviewer states that various studies reveal that significant land use changes took place in the Belgian Ardennes until the late 1950s, and that it would be interesting to look at the effects of these changes on runoff. We agree that this would provide an interesting research question. However, in our paper we only examined long term (centennial) land use changes (i.e. we made a land use map for each century, and not sub-centennial). The main reason for not using sub-centennial maps is that data pertaining to land use changes at this time-scale (let alone centennial) are not available pre-1800. Hence, we feel that it would be inconsistent to use sub-centennial land use maps for the last century, when this was not the case for the preceding centuries. We will add a sentence to the revised manuscript to clarify this. Nevertheless, if we compare our land use maps for the 19th and 20th Centuries, we can see a clear increase in forest at the expense of agriculture in the Ardennes region. Hence, the long-term land use change in the Ardennes between the situation in the 19th and 20th Centuries has in fact been included in the simulations, although the land use change in the rest of the basin also shows a change over this period. Given that out model is mainly designed for assessing discharge at the basin scale, we feel that it would be more appropriate to set-up a higher-resolution model if the aim was to look at the specific effects of change in land use in the Ardennes region only.

2. For our model study we have assumed that the basin was fully forested (except for some wetland areas) during the period 4000-3000 BP. The reviewer correctly points out that in reality this was not the case, since many archaeological sites have been found in the Belgian Ardennes region dating from the Late Mesolithic and early Neolithic. In fact we also state in the original manuscript that the assumption of full forest cover is not entirely correct, and give examples of other studies which have found evidence of

HESSD

4, S1509–S1513, 2007

Interactive Comment

human presence in the basin during the period 4000-3000 BP. We thank the reviewer for providing the reference of Henrard (2003), which we will add to the new manuscript.

The second point made here by the reviewer is that whilst the impact of this human presence would indeed have been negligible compared to today, it may still have played an important role, and that our assumption that the impact was too small to have had any significant influence on hydrological processes may be incorrect. This is a valid point since this assumption is based on conjecture on our part. The reviewer suggests that it may be useful to carry out sensitivity experiments for the period 4000-3000 BP, but instead of assuming full forest cover, assuming different percentages of deforestation to have occurred. We agree that this provides an excellent way to test our assumption. Hence, over the last month we have re-run the model using climate data for the period 4000-3000 BP, but with land use maps showing 5% and 10% deforestation respectively (forest cells replaced at random by agricultural cells). If we compare the control results with these new runs we find that there is no statistically significant change in mean annual or maximum discharge for a deforestation of 5% (t-test), and that the frequency distribution curve shows hardly any change. For a deforestation of 10%, mean annual discharge increases by ca. 1.6%, which is significant according to a 2-tailed t-test. Nevertheless, whilst statistically significant this increase is minor and the resultant increase in high-flows is even smaller. We will include the results of this sensitivity analysis in section 3.4 of the new manuscript, and thank the reviewer for the suggestion, which we feel has strengthened the basis of our mid-Holocene land use assumptions.

3. The third comment relates to comments 1 and 2, and suggests that a sensitivity analysis of the effects of a change in land use by a few percent would be beneficial. This has been dealt with in the second point of our reply. We feel that the results of the sensitivity analysis outlined in point two indicate that very detailed land use maps are not necessary, and that our generalised maps of land use change are therefore robust for the stated aims.

4. The reviewer suggests that given the large uncertainties associated with the estimates of soil water holding capacity (WHC), it may be more useful to simply use a constant value for this parameter over time. It is true that there are uncertainties involved with our estimates of WHC. However, we view the approach used here as a first step, which can be developed as more information on the relationship between WHC and land use becomes available. Furthermore, we feel that it is important that the sign of change in WHC is represented in our input data. Numerous studies have shown that the conversion of forest soils to agriculture can lead to reduced WHC, and whilst the spatial distribution of change in our input maps only provides a rough approximation, the basin average decrease in WHC over the late Holocene will be more accurate. We agree that the use of a constant value could also be considered, but since the effects of the parameter on discharge were shown to be insignificant in our sensitivity studies, there would be almost no effect on the final results. Since both our method and the method suggested by the reviewer are valid, we do not feel that the time required to re-run the model for all time-slices, scenarios, and ensemble members (ca. 9 months) can be justified given that there will be no improvement in the final results. However, we will include a sentence in section 3.2.3 acknowledging this point.

5. The reviewer states that the difference between modelled and observed PE is not trivial. We agree with this comment and feel that the original manuscript did not provide a detailed enough discussion of this issue. One problem is that the so-called observed values are in fact also estimates, but estimates based on many more variables than were used in our simulations. However, this problem notwithstanding, the discrepancy remains significant. In fact, the amount of water actually leaving the basin through evapotranspiration is not given by PE, but rather by AE. Hence, it would have been more meaningful to compare our modelled AE with more accurate estimates of AE: unfortunately these are not available for the Meuse basin. Nevertheless, we can infer that the annual values of AE calculated in our model are reasonable since there is no statistical difference between our modelled values of precipitation and discharge. For the period 1901-2000 we simulated a basin-wide precipitation of 912.7 mm.a-1,

HESSD
4, S1509–S1513, 2007

Interactive Comment

compared to 913.3 mm.a-1 in the observed (CRU) record; for mean annual discharge at Borgharen for the period 1921-2000 we simulated 268.4 m3s-1, against 270.4 m3s-1 in the observed record. Given that the annual values for soil storage, groundwater storage, snow storage (i.e. the storage compartments of STREAM) remain constant over these periods, the simulated value of annual AE must also be correct. We will strengthen the discussion of evapotranspiration in section 3.3 to reflect and expand on the points discussed here.

6. We agree that it would be useful to also look at the mean discharge and flood frequencies in the period 1000-1900 AD: these results have been added to the discussion. Whilst mean discharge and flood frequency showed an increase between the period 4000-3000 BP and 1000-1900 AD (even when the 20th Century was not considered), the same was not the case for the variability. The standard deviation of mean annual discharge increased between the period 4000-3000 BP and 1000-1900 AD, but not between 1000-1900AD and the 20th Century. Hence, we believe that our interpretation as to the cause of the increase in variability remains valid.

7. The crop factor is dimensionless. It is a factor by which reference PE is multiplied to account for empirically derived differences in PE above different land use types. We will clarify this in the revised manuscript. The equations used in the STREAM model can be found in the report by Ward (2007, p. 39-40), available online at http://ivm5.ivm.vu.nl/adaptation/project/files/File/Documents/Report_Meusepalaeo_Methods.pdf.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 2521, 2007.