Answer to the reviewers.

The authors are grateful to the 4 reviewers and the editor for their useful comments and advices. We tried to address all these comments in this answer.

Interactive comment on "Multi-model comparison of a major flood in the groundwater-fed basin of the Somme River (France)" by F. Habets et al.

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The paper deals with an interesting topic. The issues of model selection and modelling exercises considering alternative model structures (conceptualizations or conceptual models) are of current concern in many publications and application areas. The aim of this paper is to asses the ability of 4 hydrological models to reproduce a flood event in France testing them against alternative sources of information (observed discharge, piezometric heads, satellite-derived flooded areas). Authors limit themselves, however, to the comparison of the results from the 4 models whereas going a step further to obtain a combined multi-model prediction/simulation would make the potential impact of the article much higher.

Some comments that might help broaden the scope of the article beyond the study case discussed are

1. It would be helpful for the reader to find measures of model performance for the 4 models (skill, RMS, cross-validation).

Discharge error, RMSE, Efficiency and Index of agreement are given in tables 2, 3 and 4. Although no cross-validation was computed, these statistical criteria allow to discriminate the ability of the models.

2. Clearly states the periods and observed data used in the parameter optimization for each model.

Yes, this is now written that MARTHE was first calibrated on the period 1995-2002, MODCOU on the period 1995-2003, and CLSM on the period 1985-2003.

As stated in the article, the SIM model that is already applied over France was not calibrated, only one parameter was set to a low value (not fitted).

3. A quantitative distinction among the four models would help assess in better way the most plausible hydrological model. For example, different model selection criteria (e.g. AIC, AICc, BIC, KIC see Ye et al., 2008) could be used to determine the best model. These criteria are usually obtained as by-products of calibration routines, so no need for extra work.

Thank you for this reference. However, as the calibration was mostly done using trial/error simulations and not using calibration routines, only the results with the final calibrated parameters were kept. Thus, it doesn't seem possible to compute such criteria in this study. However, we'll keep in mind this reference for upcoming works.

4. The issue of missing processes in the corresponding models (p 6157, 4-5) could be reformulated to ask what are the observed data helping in identifying these missing processes. Then you could focus in collecting those data to rule out the worst model.

It was found that the models were not able to reproduce the fast increased of the piezometric level during the flood. This is almost certainly due to the transfer in the unsaturated zone, and as you suggested, an experimental network was set up to monitor what happen in the unsaturated zone. The sentence is now reformulated: '*This assumption tends to be confirmed by the results obtained in 2007 at the Flood1 experimental site that was set up to understand the processes occurring in the unsaturated zone during floods'*.

5. Why not considering an ensemble simulation of the 4 models? It is likely that the ensemble prediction/simulation will have a better predictive coverage than any single model. A suitable technique is Bayesian model averaging (BMA), which additionally allows the estimation of the predictive variance arising from the use of alternative hydrological models.
The main topic of the article was actually not to build a multi-model approach, but to make a comparison of the simulation results of several models. For the riverflow forecast of the Somme basin, a lumped model is used (Gardenia). The use of assimilation technique to estimate the initial soil moisture and piezometric head allows this model to obtain good results, even in the retrospective simulation of the flood.
Although the authors think that ensemble riverflows forecast is as powerful tools, it was not the aim of that paper. Moreover, it is probable that mutli-model prediction would be better if the models integrate a suitable physic, and the study shows that all 4 models need improving the simulation of the chalk unsaturated zone transfers.
6. The 4 hydrological models show some fundamental differences in the way the water budget is calculated, in the representation of the unsaturated zone, and in the method to obtain the saturated flows. An assessment of the uncertainty arising from these differences would significantly add to the message of the article.
That is true that the estimation of the uncertainty could add some information. However, to be able to address uncertainty issue, specific methods should be used, for instance, the realisation of several simulations to address the parameter uncertainty. That was not done in the article, because the idea was not to define the best model, but instead, to analyse the results of several modelling of the Somme basin, ie, the association of the model and its parameters.
The parameters were mostly calibrated using trial/error tests. The four models have different representation of the processes, but they obtain similar results. It is shown that fair results are obtained for the long retrospective run, but no models were able to reproduce observations during flood. The article analyses this failure as due to a missing process which acts occurs when the chalk unsaturated zone becomes almost saturated, and not to problems associated with the parameters estimation.
Although it doesn't seem possible to estimate the uncertainty in the present study, we will try to address this point when the improved representation of the unsaturated chalk will be available.

7. Clearly (as shown from figures 5, 6 and 9), the predictive variance increases by expanding the modeling exercise to the model structure dimension, i.e. by considering alternative hydrological models. So, What is the relative advantage of the multi-model approach compared to the single-model approach?
The figures show some differences in the modelling results. The multi-model comparison is trying to understand what are the origins of such differences. The analysis of the soil water fluxes gives some insight. However, again, there is no multi-model approach, but only a multi-model comparison.
8. Some fundamental questions related to the multi-modeling approach: How to define a priori the ensemble of proposed conceptual models? In the light of data, How to update this ensemble once the data have been observed? How to discriminate among alternative conceptualizations when all perform equally well in a calibration stage against limited measured data?
This question seems to apply to a mutli-model approach which is not the aim of the present study. The main of the article was not to build an ensemble prediction based on several modelling approach, but to compare different modelling approach.

Anonymous Referee #1

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General comments:
This paper presents a comparison of the performances of four hydrological models applied over the Somme river basin, north of France. After a short description of the main characteristics of the river basin, the main features of the models are presented. The implementation of each model for the simulation of river discharges and piezometric levels over an 18-year period is presented and the results are compared. The paper mainly stress that the four models are unable to accurately simulate both river flows and piezometric levels during high flows, mainly due to the role of the unsaturated zone on the river (which depth variation is not dynamically taken into account in the models) and aquifer hydrodynamic. The paper concludes raising an interesting question on whether it is necessary to apply complex land surface schemes on river basins where groundwater plays a significant role instead of using groundwater models. The paper is clear, well presented and illustrated and the English seems good to this reviewer. After answering for further information requested in specific comments and corrected some technical corrections, it may be of benefit to accept this paper to publication on HESS journal.
Specific comments:
- Line 18-20 on page 6148; the authors indicate that there is a delay, but it is not clear between unsaturated flow and what? It may be SI, but this has to be detailed a bit. Also, the resulting average percolation rate is interesting but maybe providing the min and max values could describe haw variable this process simulation is between models.
The text is now modified: *'For these three models, the transfer throughout the unsaturated zone smoothes and add a delay to the SI flux. The unsaturated flow (UF) has an average percolation rate ranging from 0.5 to 1 m/day'*
The minimal and maximal values are difficult to estimate. However, the average percolation rate is already giving some insight of the differences between models.
- Line 11-15 on page 6148; an analysis of the differences between models in terms of storage in the aquifers could perhaps be intended comparing UF and BF low flows or peak flows delay?
The evolution of the storage in the aquifer is varying from year to year and is actually quite close to the evolution of the piezometric level presented in figure 6. Thus, it is partly discussed in section 4.3.

Fig 3 seems to present differences between models?
Figure 3 doesn't show the differences, but the annual average.

- Line 24-26 on page 6148; what is the geological context of the Pang-Lambourn study? Is it close to that of the Somme?
Yes, both basins are mainly chalky. It is now written: *'However, these three representations of transfers in an unsaturated chalk are quite different from the description of the Pang-Lambourn chalky basin in the UK.'*

- Line 1- 3 on page 6149; this conclusion is ok but when looking at IRZ on table 1, it appears that the simulated values are rather the same for all models, so the main differences lie in the way the models simulate the seasonal behaviour of both the unsaturated and the saturated zone.
That is correct. To put the emphasis on that point, it is now written that the transfers in the UZ and aquifers erase the temporal differences in the estimation of the surface water budget.

- Line 26 – 27 on page 6149; to facilitate the comparison with figure 3, it could be better to present the same time scale (Sept –> Aug) for graphs 5 and 9.
This modification is done

- Line 1 – 2 on page 6150; the differences noticed could also be due to geological heterogeneities not accounted for by the models or linked to their calibration processes.
Yes, it is now written *'Thus the spatial variabilities of the Somme basin are not well captured by all models, which leads to an uneven distribution of the quality of the simulated riverflows.'*
- Line 15 – 16 on page 6150; an alternative assessment could be that peak flows are over-estimated because the recession slope seems to be fairly well simulated compared to the observed one (for example in 89, 90, 91, 93, 95).
Yes, this explanation is now added: '*Thus, it may be that the models are not able to simulate a correct recession after a high flow period, or that they have some difficulties to simulate the peak flows.*'

- Line 2 - 3 on page 6152; wouldn't be much fair to write that "SIM is the model that better fit the observed river high flow during the first 50 days"? By the way, this figure demonstrates again that the recession slope is fairly well simulated: : :
Yes, it is now written: '*SIM better reproduces the observed river high flows during the first 50 days, while MARTHE, MODCOU and the CLSM underestimate the flows by 10 to 30 m3/s*'

- Line 11 on page 6153; here the Marthe model shows a less good simulation of the recession than SIM and Modcou.
Yes, it is stated that MARTHE has some problems to simulate the recession.

- Line 5 – 6 on page 6154; maybe the average of the piezometric levels of the wells located into the flooded areas could be plotted for an interesting comparison and a kind of validation of the inundated area detected?
Some wells are located close to the river, but none were located in the inundated area

- Line 24 on page 6157; "It was shown".. maybe it could be better to cite the fig 3 for example?
The reference to the figure is now added

Technical corrections:
- Maybe the authors should systematically use piezometric "level" instead of "head" to be precise (the piezometric head can be understood as a specific measurement of water pressure above a reference).
Yes, this was corrected.
- Line 9 on page 6137, the link between a negative (?) matrix potential and the activation of the fissure flows as an explanation for the rapid increase of the piezometric head should be detailed a bit if mentioned.
Fissure flow in the chalk is possible only when the unsaturated zone is almost saturated, which corresponds to a matrix potential close to −50 cm. This comment is added.

- Please give the internet link allowing to download the Amraoui's references (http://www.brgm.fr/publication.jsp)
This link is now added

- Figure 6 caption: “the observations are interpolated linearly between each observation”, in case of missing data?
Yes, because the frequency of the observations varies from 1 day to 10 days.

- Figure 12 caption: “saturated area simulated by CSLM” add “in yellow”?
Yes, this is corrected.

Review comments on “*Multi-model comparison of a major flood in the groundwater-fed basin of the Somme River (France)*”
By F. Habets, S. Gascoin, S. Korkmaz, D. Thiéry, M. Zribi, N. Amraoui, M. Carli, A. Ducharne, E. Leblois, E. Ledoux, E. Martin, J. Noilhan, C. Ottlé, and P. Viennot
Overview
In this manuscript, the authors compared the performance of four models for a groundwater-fed basin in France. The paper is well-organized, well-written and the topic is suitable for publication on HESS. The Somme watershed appears to be an interesting case for testing hydrologic models and land surface models, especially for those with a groundwater component. However, there are a few places in this manuscript that need to be clarified, and some grammar issue, typos, and flow of sentences to be polished. Therefore, I recommend this paper to be accepted for publication, contingent upon that the minor issues identified below be adequately addressed.
Major Comments:
1. The implementation of CLSM to the entire SOMME basin in a lumped mode looks strange to me, which might be the reason that CLSM performs worse than all the other models in this study. In other words, I don’t think it is a fair comparison because CLSM was applied at a very coarse resolution (5566 km2), while other models were applied at resolutions as fine as 1km. CLSM should be at least applied to each of the sub-basins to be fair.
The CLSM is indeed implemented in a lumped mode. However, the TOPMODEL equations in CLSM theoretically allow capturing the sub-grid distribution of soil moisture and its first-order influence on runoff and baseflow. In that sense, the CLSM is semi-distributed rather than lumped. This issue is discussed in more details by Gascoin et al. (2009) where it is mentioned that an application of CLSM to each of the subbasins was tested using the original version of CLSM, but did not allow to significantly improve the simulation. Hence, the authors decided to focus on the calibration at the scale of the entire basin.
Moreover, Pointet et al., 2001 and Amraoui et al., 2002 have shown that a lumped model (GARDENIA) is able to obtain a good estimation of the riverflows of the Somme basin (even if it shares the same difficulties as other model to reproduce the dynamic of the riverflow during the flood).

2. Based on this comparison, the authors concluded that “there is no clear benefit in using a more complex surface scheme”, “the use of complex land surface scheme is not a requirement to represent the hydrology of the Somme river basin”. I personally feel that these are overstatements because (1) only two land surface models, which are not necessarily well-known for their capability in representing hydrologic processes, were included in this comparison; (2) one of the land surface models was applied at a super coarse resolution; (3) “hydrology” here should be “flood forecasting” or something equivalent because streamflow and groundwater dynamics are just two components in the hydrological cycle. Therefore, I would like to see the authors add some discussions on the limitations of this comparison study in the revised manuscript to address the problems in items (1) and (2). For example, there are increasing number of studies in the literature where groundwater models have been implemented in land surface models to simulate groundwater table and recharge/discharge

dynamically (Liang et al. 2003; Chen and Hu 2004; Maxwell and Miller 2005; Niu et al. 2006; Fan et al. 2007; Miguez-Macho et al. 2007; Maxwell and Kollet 2008). If these models were applied to the SOMME basin, the story could be different.

In this study, two LSMs and two simple water balance schemes are used to compute the surface water budget. The results show that the riverflows and piezometric levels simulated by using the water fluxes from the simple water balance schemes are as good as, or even better than, the ones simulated using the surface fluxes from the LSMs. This is why it is said that the use of complex land surface scheme is not a requirement to represent the hydrology of the Somme river basin.

Such results would not be an issue if the fluxes simulated by the LSMs and by the simple water balance scheme were fairly comparable. But we show that the fluxes computed by the LSMs have a different annual evolution than those computed by the simple water balance schemes used in the hydrogeological models. However, these differences are erased when the surface fluxes are transferred in the unsaturated and saturated zones.

This is a specificity of the Somme basin, and this result is only true for the Somme basin (not only during the flood). It is due to the fact the hydrology of this basin is mostly driven by the aquifer: an aquifer spread over the whole basin at an average depth of 40m, and the baseflow represent 80% of the riverflow.

That doesn't mean that complex land surface schemes are not useful for the hydrology. We totally agree with Referee #2, who highlights the recent evolution of LSMs toward a better representation of the groundwater. We try to make a step toward this direction. Indeed, ISBA is coupled to the groundwater model MODCOU within SIM (Habets et al., 1999., Habets et al., 2008), and the CLSM version used for this study includes a groundwater reservoir and (Gascoin et al. 2009).

In the opposite, this study demonstrates the need to couple LSMs with hydrological model to simulate the Somme basin, because the impact of the transfer in the UZ and saturated zone dampened the temporal evolution of the surface fluxes.

It is true that a better calibration of the UZ and aquifer transfers may have lead to a better simulation of the riverflow with the coupled LSM-hydrogeological model. However, that doesn't mean that the use of LSMs is required to simulate the Somme basin.

One interesting aspect of the coupling between LSM and hydrogeological models is the influence of the position of the water table on the soil moisture. However, the interaction between soil moisture and watertable that is important only when the watertable is shallow (lower than 10m), is in the Somme basin, restricted to limited area, due to the shape of the Somme valley. This is probably why there is not a strong impact of the interactive coupling simulated by CLSM.

In order to take into account your comment, the conclusion is modified: *'Therefore, the use of complex land surface schemes is not a requirement to represent the hydrology of the Somme river basin. However, to simulate the Somme basin, LSMs should either be coupled to hydrogeological models or include the representation of the transfers in the unsaturated and saturated zones. This reinforce the need to include deep hydrology in LSMs which are currently increasingly developed (Yeh and Eltahie, 2005, Miguez-Macho et al., 2007, Liang et al., 2003, Maxwell and Kollet, 2008).'*

3. The terms in section 4.1.2, "soil infiltration (SI) corresponds to the flux at the bottom of the soil reservoir or root zone" and "the flux from the unsaturated zone (UF)" are confusing. Does the former refer to "percolation" and the latter refer to "recharge"? What is the difference between them in each model?

It was not easy to find names that were satisfying for these two fluxes. The term 'recharge' is not used in this article, because for one model, the SI flux corresponds to the recharge, and for

the other, the recharge is the unsaturated zone flux (UF). 'Percolation' can also be understood as the water flux that is feeding the soil moisture. The term 'Infiltration' is used in the article because it is used by the co-authors working on hydrogeological modelling, but it can also be misunderstood. This is why the definition of these fluxes is given in the article.
Some details are given to explain why the SI and UF fluxes differ: p6140 '*the deep unsaturated zone is not taken into account by the CLSM and it is represented with a simple percolation function in MARTHE, and with a simple conceptual model in MODCOU and SIM*'. It is then stated that: '*MARTHE uses a simple percolation function, with a spatial average time constant of about 3 months'*, and, *'The unsaturated zone transfer model used in MODCOU is based on a conceptual Nash cascade model (Nash, 1960). The other parameters of the Nash cascade were derived from the Seine basin application: the depth of each reservoir was set to 5m, and the drainage coefficients vary according to the geological map*.'
It is difficult to give more details while keeping the article quite short, however, additional details are given in the referenced articles.

4. An explanation on piezometer measurements, meanings of piezometer heads/levels, and their relationships to water table is needed. Without such an explanation, figure 6 and section 4.3 look confusing.
There was an error in the article. All the piezometric measurements used are piezometric level, as noted by referee #1. This error is now corrected.

5. It will be helpful to list the major calibrated parameters for each model in a table, as a reference for future work. For distributed models, ranges of parameters might serve the purpose.
It is difficult to add a table with all the calibrated parameters, because then, each parameter should be defined, and this will substantially increase the length of the article. Moreover, we are not sure that it worthwhile. An idea would be to have only the value of the main hydrogeological parameters, the transmissivity and specific yield. But then, this doesn't apply for all models. Moreover, the values used by MARTHE and MODCOU (and thus SIM) are comparable, only the spatial distribution varies.
Moreover, MARTHE, MODCOU and CLSM have calibrated several parameters, and the calibration process is described in a report (Amraoui et al., 2002, available at http://www.brgm.fr/publication.jsp) and two articles (Korkmaz et al., 2009, Gascoin et al., 2009). Gascoin et al., 2009 present a table with the calibrated parameters for CLSM. In SIM, only the subgrid surface runoff was modified as stated in the text, and set to the value of 0.01.

6. Please combine sections 6 and 7 to one section entitled "Discussions and Conclusions".
This is done
Other specific comments are discussed as follows.
Specific comments:
1. Page 6137, line 4, "1.100 people", is this number wrong?
2. Page 6137, line 5, "(Deneux and Martin, 2001)."
3. Page 6137, line 22, "…evenparticularly during the floods?"
4. Page 6139, lines 10-11, "The both others ones other two models…"
5. Page 6139, line 20, "…LSM developped …"
6. Page 6141, lines 5-6, better to write the sentence as "The period used for calibration, as well as atmospheric data used for calibration, varies across the models."
7. Page 6141, line 8, what does "then only" mean?
'After this first step', this is now corrected

8. Page 6142, line 7, Shouldn't it be "…on an annual basis"?
9. Page 6142, line 13, better to write as "… with the higher resolutions associated both with…"
10. Page 6142, line 18-20, "Then, the simulated surface water budget was adjusted, as well as the groundwater parameters in a steady state.". This sentence looks confusing. Parameters for both surface fluxes and groundwater simulations were adjusted, right?
Yes, it is now written: 'Then, the parameters related with the simulation of the surface water budget were adjusted, as well as the groundwater parameters in a steady state.'
11. Page 6143, lines 6-7, "retroaction", do you mean "interaction". Also, what does "surface soil moisture" mean?
Yes, more than interaction, we mean feedback. Now it is written: '*There is not yet a feedback between the depth of the unconfined aquifer and the soil moisture simulated by the LSM'*
12. Page 6143, lines 8-9, Please change to "Thus, the surface water budget should was not be affected by the introduction of the simulated aquifer." to be more confirmative.
ok
13. Page 6143, line 9, "…in this application**,**"
14. Page 6143, line 11, better to write as "…and it was decided to use hence a 1 km resolution was used."
15. Page 6143, line 21, "it was decided to set this coefficient to a low value.", please be specific, give a number!
Done: the value is 0.01
16. Page 6144, line 3, "…the Richards equation…"
17. Page 6148, line 17, "the original baseflow from the shallow aquifer being accounts for only 27% of the total (not shown)."
18. Page 6148, line 20, "…very similar. This can be is surprising …"
19. Page 6148, lines 26-28, "Thus, for the Somme basin and on a mean annual basis, …the surface water budget on a mean annual basis."
corrected
20. Page 6151, line 11, "…and not especially on not particularly for the period of the flood."
corrected
21. Page 6153, line 28, "…water table groundwater contributes to the riverflow."
corrected
22. Page 6154, line 19, "…RMSE control point error…", what does mean, please clarify.
Now, it is written: 'Both radar and optical images are geo-referenced and superimposed, with a very slight error (the RMSE control point error is about 20 m).'
23. Page 6155, line 28, "…even if the CLSM…", not clear! I found the usage of "even if 'in this manuscript very confusing!
'*Although*' is now used instead of "*even if*"
24. Page 6156, line 17, "…exchanged quantities…", is this simply "fluxes"?
Yes, corrected
25. Page 6156, line 20, "…even if the simulated aquifer overflow…", not clear!
It is now written: *'As expected, the fluxes between the river and the aquifer are reduced at the finest resolution (due to the fact that the area in contact is reduced), but this is balanced by an increase of the aquifer overflow outside the riverbed. This extended aquifer overflow remained mostly located in the main river stream, in the bottom of the valley.'*
26. What does the shaded area represent in the lower panel of Figure 6?
The legend is modified: The number of available observation wells is plotted in the grey shaded area
27. Figure 8, "histogram" should be "bar plot" or something equivalent.
Corrected

28. Figures 8 and 9, how to read the flooding area in them? No axes correspond to it!
Yes some references are added to the figure 10.

References:

Fan Y, Miguez-Macho G, Weaver C, Walko R, Robock A (2007) Incorporating water table dynamics in climate modeling. Part I: Water table observations and the equilibrium water table. J Geophy Res 112, D10125, doi:10.1029/2006JD008111 Chen X, Hu Q (2004) Groundwater influences on soil moisture and surface evaporation. J Hydrology 297:285-300 Maxwell RM, Kollet SJ (2008) Interdependence of groundwater dynamics and land-energy feedbacks under climate change. Nature Geoscience 1:865-869 Liang X, Xie Z, Huang M (2003) A new parameterization for groundwater and surface water interactions and its impact on water budgets with the VIC land surface model. J Geophys Res 108(D16), 8613, doi: 10.1029/2002JD003090 Maxwell RM, Miller NL (2005) Development of a coupled land surface and groundwater model. J Hydrometeorology 6(3):233-247 Miguez-Macho G, Fan Y, Weaver CP, Walko R, Robock A (2007) Incorporating water table dynamics in climate modeling: 2. Formulation, validation, and soil moisture simulation. J Geophys Res 112, D13108, doi:10.1029/2006JD008112 Niu G-Y, Yang Z-L, Dickinson RE, Gulden LE, Hua S (2006) Development of a simple groundwater model for use in climate models and evaluation with Gravity Recovery and Climate Experiment data. J Geophy Res 112 (D07103), doi:10.1029/2006JD007522

Interactive comment on "Multi-model comparison of a major flood in the groundwater-fed basin of the Somme River (France)" by F. Habets et al.

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This paper provides an excellent multi-model comparison on the hydrologic simulations at the macroscale Somme River basin in France, by using four distributed or semidistributed models (incluing two LSMs). The simulations span over a 18-year (1985- 2003), which is long enough to lead to convincing conclusions. The 4 models were tested by comparing their simulations to observed hydrographs at 5 streamflow gauges as well as groundwater level averaged from several tens of monitoring wells. Moreover, the simulation of flooded areas was also nicely investigated by comparing it to the remote sensing images. Overall, this is a fairly good paper. In my evaluation, it deserves to be published after minor revisions. There are not too many published multi-model comparison studies, perhaps due to the difficulty in performing such type of work by single research group alone. Therefore, this paper has great value to be published on HESS, particularly given that many interesting physical insights and modeling issues were revealed and reported in this paper. However, the authors are suggested to clarify some ambiguities in this paper before publication according to my following review comments. Also, the length of this paper can be shorten by more concised way of presentation.

On p.6137, line 18, modify the word "react".
'Vary' is now used
On p.6139, line 10, modify "The both other ones". How about "Another two"?
Corrected : "Other two is used" as suggested by referee 1
line 20, modify "developped" into "developed". (also p.6144, line 10)
corrected
line 21, modify "..model, that solves.." into "model that solves".
corrected
On p.6142, line 7, "on an annual basiS"

corrected
On. p.6146, line 18, unit is not correct. line 19, change "interception" into "interception loss".
Yes, corrected
On ps. 6147 and 6148, Is the unit "m/day" correct? I doubt.
Yes, the unit is correct. These are classical values for the unsaturated chalk. The observed rates in the chalk are significantly greater than estimates of the saturated conductivity of the Chalk matrix. Headworth (1972) suggested that this is due to piston flow process.

On p. 6149, line 9, How about " the coefficient of efficiency"?n
done
On p.6151, line 4, correct "diffrents" into "different".
done
On page 6149 and Fig 4: What are the reasons responsible for the biases of CLSM< in the summer of 1990-1991, and for the SIM for the summer of 1985-1989? This needs to be discussed.
1990-1991 is a dry period, and CLSM overestimates the evaporation. This tendency to overestimate evaporation over dry period was found also in two other areas in France.
The underestimation of the piezometric level by SIM at the beginning of the simulation is not well understood. SIM uses the same initial conditions as MODCOU, and there might explain the bias at the beginning of the period. But, as stated in the article, SIM has a tendency to overestimate the amplitude of the fluxes during the whole period.

The same comments also apply to Fig. 5: Why these four models generate quite different behaviours in five sub-basins, as authors summarized in the last paragraph of page 6149? I am particularly interested in: why the model MARTHE show lagged behaviours in AVRE sub-basin while other models do not? The authors mentioned that "This is certainly due to the spatial differences in the simulated functioning of the aquifer". More discussion to elaborate on this point would be very useful.
There are spatial variabilities in the Somme basin. For instance, the Avre basin is the subbasin where the part of the basefow is the weaker, as showned by isotopic study (Négerl and Petelet-Giraud, 2005, Amraoui et al., 2002). Although MARTHE has the best simulated evolution of the piezometric levels in the basin, and one of the best estimate of the Somme riverflows, it fails to represent the annual cycle of the Avre subbasin. This is certainly due to the fact the calibrated parameters in MARTHE doesn't represent this spatial variability. However, it is true that there is no proof that the error is due to a misrepresentation of the aquifer. The error can be in the estimation of the unsaturated zone model, or to the partition between fast and slow flows in the water balance model. The sentence is now corrected this way: '*Thus the spatial variabilities of the Somme basin are not well captured by all models, which leads to an uneven distribution of the quality of the simulated riverflows.*'

Are the two sub-plots (1998and 2001) in Figure 7 absolutely necessary? The difference between these two years is not easy to observe from the plots. I personally do not see the point to present them...
No, these sub-plots are not necessary. But, they show that MODCOU and SIM have an error in dry year (1998) for the higher piezometric levels (underestimation) and that these errors tend to be reduced during a wet year in 2001. Thus, even if the article is more focussing on the flood period, some improvements can be made to have a better estimation of the piezometric levels in the dry year.

When the groundwater contributions to riverflow during floods are discussed (for example, bottom of p. 6153 and middle of p.6156, and elsewhere throughout the manuscript), the following study is very relevant in addition to several ones that Authors have already cited: Eltahir, E.A.B, and , P. J. -F. Yeh, 1999. "On the asymmetric response of aquifer water level to droughts and floods in Illinois", Water Resources Research, 35 (4), 1199-1217. In this paper, the interactions between topography (hillsope), water table position, and base flow were discussed based on the interpretation of long-term measured data set in Illinois. It demonstrated that at the regional (macro-) scale, the dependence between shallow groundwater level and baseflow is in general non-linear due to the seasonal intersection of regional water table with local topography..

A citation in now added: *'Although the altitude of the riverbed is slightly lower than the altitude of the valley, it is obvious that the level of the aquifer is rather close to the bottom of the valley and that it can provide some aquifer overflow during the high water season, as pointed out for instance Eltahir and Yeh 1999 who then noticed that this generates an unlinear variation of the baseflow.'*

But we would like to emphasize the fact that the problems encountered to reproduce the flood seems not due to the unlinear increase of the drainage area, but to the fact that the fast increase of the piezometric head is not well estimated, because the fast response of the unsaturated chalk is not simulated.

On P. 6157, "Discussion" instead of "Discussions".

Ok, this is corrected, and this section is now called Discussion and Conclusions

There are way too many redundant "the" used throughout the paper. Instead of pointing out all of them (which is not practical!!), pardon me just use the abstract as the example: On p.6136, remove one "the" on lines 7 (the flooded), 8 (..the surface...), 11 (the observed...),13 (the deep...), 20 (the overflow...), 21 (the overflow...).

Thank you for the advice. These corrections are done…

Finally, and perhaps the most important and challenge issue to answer, how the calibration will change the simulation result and the findings? It is well-know that the problem of equi-finality in the parameters of all hydrologic models would make validation of model simulations rather difficult, if not impossible. Could the Authors please comment on this point (The "robustness" of the findings) more or less in the end of paper?

The authors agree that the results may be different with another set of calibrated parameters. However, one of the most important findings is the key role of the unsaturated zone during the flooding. Whatever the parameters, the problem is linked to the way the physical process is described. The simple representations of the unsaturated zone used don't take into account the characteristics of the chalk with its double porosity and the threshold effect when the fissure flow occurs.

The following statement is now added in the discussion: *"Thus, the problem encountered during the flood is not only due to a poor calibration of the parameters, but to the use of an unsaturated model not adapted to the chalk matrix."*

On the last paragraph of p. 6157, the Authors stated that "as the temporal evolution of the water fluxes is deeply modified by the transfer in the unsaturated and saturated zone, the impact of the surface schemes is mostly hidden by the calibration of the UZ and groundwater parameters. This is an interesting finding, but this statement may be limited to the basins where groundwater outflows dominates such as the Somme basin here. In this regards, the following paper might be relevant to cite: Gulden, L. E., E. Rosero, Z. L. Yang, M. Rodell, C.

S. Jackson, G. Y. Niu, P. J. -F. Yeh, and J. Famiglietti, 2007, “Improving land-surface model hydrology: Is an explicit aquifer model better than a deeper soil profile?” Geophys. Res. Lett. 34, L09402, doi:10.1029/2007GL029804.

Sure, this statement is limited to the Somme basin, and because the unsaturated zone is rather deep (40m on average).

Additional Reference: Yeh, P. J.-F., and E. A. B. Eltahir, 2005, “Representation of water table dynamics in a land surface scheme: 2. Subgrid heterogeneity”, Journal of Climate, Vol. 18, No. 12, pages 1881-1901

Interactive comment on “Multi-model comparison of a major flood in the groundwater-fed basin of the Somme River (France)” by F. Habets et al.

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Overall, the paper has received excellent comments from 4 reviewers, which the authors should individually respond, and in the process improve the presentation of their manuscript. Most of the comments are about presentation, which should be relatively easy to handle. Overall, the reviewers are supportive of eventual publication of a revised manuscript. As editor, I feel that the authors can and should go beyond addressing the detailed comments of the reviewers. The phenomenon that they are trying to model – groundwater dominated floods - is a very interesting one. The authors have approached the problem from a multi-model comparative approach, and the reviewers have also approached it in the same manner, and the analysis and discussion have a certain "beauty contest" flavor. In my opinion what is missing is a synthesis of the results of the various model applications. What actually happens during these flood events?

Some details on the flood event are given in Hubert, 2001, Pointet et al., 2003 and Négrel and Pételet-Giraud 2005. The flood is due to large precipitation over several months. But the unexpected aspect of this flood is the fast increase of the water table level which is due to a rapid saturation of the unsaturated zone, and that causes inundation.

Is there some consensus about it? If not, why not?

Yes, there is now a consensus on this aspect; especially owing to the Flood1 experiment that was conducted after the flooding, but whose results are not published yet. References to this experiment are given in the introduction and in the conclusion. However, although the physical processes are now better understood, they are not yet integrated in the distributed hydrological modelling, and some additional works are under progress.

To better emphasis this aspect, a comment is now added in the conclusion: *“According to these conclusions, studies aiming at the improvement in MARTHE and MODCOU of the simulation of the water transfer in the Chalk unsaturated zone are in progress by taking into account the fissure flow (Thiéry et al., 2008) and by integrating a dynamical unsaturated zone depth (Philippe et al., 2009). The application of these developments in the distributed modelling of the Somme basin should help to improve the modelling of the riverflows and piezometric head during the 2001 flood.”*

One cannot take the "blind men and the elephant" attitude to it. I like to see some answers that go beyond how well each model does or does not. Could not one come up with a conceptual model of what happens on the basis of a top-down, data based study that provides some illumination of the dominant processes at work?

This study has identified the difficulty of the models to reproduce the evolution of the piezometric level during the flood, and has deduced from the analysis of the results that the error is mainly due to an incorrect estimation of the transfer in the chalk unsaturated zone

when it becomes almost saturated. Of course it is known since a long time that the chalk is characterised by a double porosity, and that fissure flow occurs only at a high saturation level. However, such process does not occur on a large scale very frequently, and it was therefore neglected for the simulation of the Somme basin. But, not surprisingly, the impact of this double porosity should be taken into account to study the flooding. It is fairly possible that a conceptual model taking into account this threshold effect could have a fair representation of the flooding. However, the authors have not tried to develop such conceptual model, but are trying to improve the distributed modelling. Some improvements of the unsaturated zone models are developed. A comment on such topic is added in the conclusion (see comment above)

I would like a thorough revision of the manuscript that brings out these hydrological issues, rather than merely reporting on just the model inter-comparisons. I look forward to reading a revised manuscript.

We have tried to take into account all the comments in the revised manuscript.

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