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Hydrology and
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Discussions

Interactive comment on "Spatial and temporal connections in groundwater contribution to evaporation" *by* A. Lam et al.

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We thank you for your compliments as well as for your criticisms, that certainly will improve our paper. However, to accomodate referee $\sharp 2$, we will strive to minimize elaboration of the introduction and methods sections.

Specific Comments:

RC: (1) The modeling approach used here appears insufficient to cap- ture the complex interaction between evapotranspiration and lateral surface and sub- surface flows that is of interest in this study. The ET formulation appears to be much less sophisticated than those used in the land surface components of many GCMs, LSMs, and distributed

hydrologic models in particular, the authors do not specify how moisture stress is applied to ET. Is it assumed that evaporative demand (Eo) is independent of soil moisture (or suction)?

Authors reply: The E_0 formulation by Penman-Monteith is a standard. The approach of meeting the demand preferentially by the different stores is present in many GCM/LSM formulations (e.g. canopy stores are the first to supply to E), but a sophisticated allocation algorithm of stores is indeed absent in our model. Evaporative demand E_0 in the Penman-Monteith formulation does not depend on soil moisture. Moisture stress is handled by representing a limited supply, which involves the suction of soil moisture (Eqs 4, 7). This is essentially similar to the approach in most land surface schemes used in GCMs.

RC: The method of coupling between surface water and groundwater in the "flat" regions is also unclear, as are the boundary conditions of the 2D MODFLOW model used in these portions of the domain. Are boundary conditions implemented as constant head or no-flow? Either way, it appears that no consideration is given to recharge into these areas. The choice of boundary conditions for the 2D MODFLOW model are likely to affect lateral flow throughout the "flat" regions, and thus are likely to affect results regarding the contribution of lateral groundwater flow to ET.

Authors reply: We mentioned the coupling between surface water and groundwater only briefly, and will augment our paper in this respect. The coupling is a flux, described with the Darcy equation $R = -k_{sat}\frac{\Delta H}{L}$ where ΔH is the difference between river level and groundwater level, L is a calibrated river bed thickness, and R is the surface water – groundwater interaction per unit of river length in a cell. This equation also dictates the recharge at the aquifer boundaries: the groundwater reservoirs in the 'steep' areas deliver water to the rivers, and the rivers deliver this water to the boundaries of the aquifer, where it recharges the groundwater. This model concept mimics the role of alluvial fans and similar geomorphological structures at the edge of large basins. By setting boundary conditions of MODFLOW to 'no-flow' we preserve the mass balance.

RC: More importantly, the authors have chosen an arbitrary threshold of -5m for groundwater-surface water interaction. This seems to imply that groundwater transpires freely and without resistance when the water table is less than 5m from the surface, and not at all when the water table falls below this threshold. I suspect that the results presented here are strongly dependent on this threshold formulation and the choice of threshold depth.

Authors reply: Groundwater transpires not without resistance, and only when E_0 exceeds potential canopy and soil supply (Equation 5, 8). The maximal contribution of groundwater to E_0 diminishes linearly with groundwater depth and becomes zero at the threshold depth, so the change in behaviour from coupled to decoupled and vice versa, is not abrupt. The threshold is for groundwater–soil water interaction only, and does not apply to the aquifer–river interaction (which, of course, is also a groundwater–surface water interaction). We concede that the choice of -5 as threshold is more or less arbitrary, but we observe some constraints. A much shallower table (say, 2m) would forbid interaction even when the GW level is still in the root zone. Our choice of -5 is well below the local rooting depth of dominant vegetation types in our region of interest (Masson et al., 2003). On the other hand, successively deeper thresholds (say, to 15m) would increase the possibilities of interaction by ever smaller increments. Our choice is in good accordance with the 'extinction depths' found by Shah et al. (2007). We address this issue in the revised discussion.

RC: Lastly, the grid resolution used here (5km) is very course for simulation of a transient groundwater flow problem, and is too course to resolve the "critical zone" of groundwater-land surface interaction as identified in previous studies cited by the authors. Similarly, I doubt the ability of a 5-day timestep to adequately simulate ET, which undergoes important diurnal fluctuations and short-term (temporal) variability at similar timescales to weather events.

Authors reply: The relations between appropriate scale in space and time, and the scope and scale of the problem at hand, and the scale and granularity of model inputs,

are important. But the not clear-cut. We would agree with the statement that 5km is too coarse to find phenomena that others found at their much finer scale of investigation. But 5km is very fine in comparison with the climate inputs, and also in comparison with GCMs and RCMs. Similarly, the 7-day timestep is too long, we agree, to mimic diurnal fluctuations in ET (which are not under investigation, and for which we would need a much more detailed climate dataset). For the description of the seasonal variation, including onset and length of dry spells, we found that the averaging step did not reduce variability much, when compared with computational gain. In comparison with the seasonal variation of interaction with the slowly reacting groundwater store that we investigate here, our choice is appropriate for the scale and variability of the processes in question.

RC: (2) In section 4.1, the authors write: "...where rivers are incised, the groundwater levels are more likely to stay below the interaction level of 5m below the land surface." This statement does not appear justified from the research, and personal experience suggests that incised channels are often gaining reaches (i.e., baseflow contributes to these reaches), which would indicate groundwater levels close to the surface. Recent work on bank erosion also suggests that channel erosion and incision is accelerated in areas of bank seepage. This statement should be clarified and/or relevant references should be cited.

Authors reply: We agree to the observations of the referee, but argue that they are, contrary to her/his opinion, in support of our argument. Incised channels cause steeper groundwater head gradients, with two implications: 1) more groundwater flow towards the channel and 2) lower average groundwater head. We attach a simple diagram here to illustrate this (Fig. 1), and will provide a better description in the revised paper.

RC: (3) Section 4.2 and the conclusion (section 5) draw conclusions about the soil moisture feedbacks on climate. The modeling system here does not include a dynamic atmosphere, and therefore cannot gage feedbacks on atmospheric processes. These statements should be revised to refer to surface fluxes, surface water/energy balance, etc.

Authors reply: We agree, and rephrase this in the revision.

RC: (4) The authors should emphasize throughout the discussion and conclusion that their results suggesting lateral groundwater flow does not contribute significantly to ET is strictly limited to flat areas at the course resolution simulated here – the case is likely to be very different over "flat" areas if finer resolution is considered, and in areas of greater topographic relief even at 5km resolution. This conclusion strongly contradicts previous studies on this topic. Theory and model results supporting the importance of lateral groundwater flow in ET as a function of topography and climate are clearly layed out in several of the references cited here (Kollet and Maxwell 2008 and Anyah et al. 2008 in particular).

Authors reply: We will augment the discussion in our paper to reflect these valuable comments, especially with regard to scale. Indeed, the studies mentioned here already show the importance of lateral flow at finer resolution and steeper slopes. We expect that finer resolution alone will make lateral groundwater flow much more important, firstly because slopes will be steeper (see the latest papers of fan and miguez macho) and secondly because much more flow will be lateral, allbeit on the finer scale. We expect that a finer scale in the spatial sense may imply a larger effect on short-term memory, and a smaller effect on long-term memory of the land surface model in these areas. Areas of greater topographic relief are also much better drained (more intricate and deeper incised river channels), so we expect that the large-scale lateral flow will not be voluminous. The elements of scale and of fractional dimensionality of the topography and of the groundwater level have to be taken into account when comparing results from different studies. In this view, the contradiction between our conclusion and previous results may not be as large as the referee surmises. This will be reflected in our revised discussion.

RC: (5) In the conclusions, the authors claim that inclusion of a groundwater component

in GCMs will help to close the water and energy budget of these models; this may be the case, but it is not demonstrated in this study. This conclusion thus seems unsupported (particularly since not all groundwater modules used in LSMs have good water balance closure).

Authors reply: The referee is right. We do not demonstrate this. Let us rephrase then as a suggestion, that at least it would fit LSMs to describe all aspects of terrestrial water storage and transport, such that even when the water balance is not closed, the water cycle is.

Minor/Editorial comments RC: (6) Page1543, Line 17 it should be noted that LSMs used in GCMs suffer from a short memory bias because their soil columns are only ± 2m deep, thus they don't include deep enough storage to account for slower processes that result in "soil moisture memory".

Authors reply: The depth of the soil column is certainly no proven remedy (see Gulden 2007) as there are LSMs in GCMs and RCMs with soils deeper than 2 meter that still show the 'memory loss' (HTESSEL is one example). And even if it were a remedy, we would still be more interested in modelling these slower physical processes than in accounting for their effects by employing ever-growing stacks of leaking buckets.

RC: (7) Page 1544, Line 16 this paragraph seems defensive regarding the novelty of the current study; revising the paragraph to contrast the advances of previous studies with the questions presented previously seems like a better way to emphasize the novelty and importance of this work.

Authors reply: Thanks, we will do this.

RC: (8) Page 1545, Line 10 " a basic description of the climate gradient such as plots of average annual precip, temp, and ET over the study region would be more helpful than citing references regarding climatology.

Authors reply: Thanks, but in view of referee $\sharp 2$'s comments, we will not elaborate on background information.

RC: (9) Is the term q in Equation (2) the same as Qr in Equation (1)? This term needs to be defined, and if q=Qr then the notation should be changed to be consistent.

Authors reply: They are not, and we see room for improvement here. Qr, surface runoff, is the resultant of baseflow in steep areas, and aquifer-river interactions. The term q is groundwater recharge from the soil. We will revise this.

RC: (10) Page 1549, Line 4 – what do the authors mean by "capillary rise that is not immediately consumed for evaporation is added to the soil"? Is capillary rise the term q in Equation (2) (if moving upward)?

Authors reply: Capillary rise is the term $E_{gw.pot}$. If, in line 3 of equation 5, there is a surplus of supply, then $E_{gw.pot} - (E_0 - E_{c.pot} - E_{s.pot})$ is added to the soil store, i.e. it is not thrown back into the aquifer or into runoff.

RC: (11) The discussion of calibration in section 3.6 suggests that the model was not so much calibrated as manually tuned until simulations behaved reasonably, based on someone's professional judgment. This is also suggested by the low R2 values in Figure 4. If this is the case, a detailed discussion of calibration really isn't necessary – a simple statement that the model was manually calibrated is fine, with emphasis on the fact that calibration is not the goal of the study (which the authors clearly state in section 3.6 already).

Authors reply: Thanks, we will do that.

References:

Masson, Valery, Jean-Louis Champeaux, Fabrice Chauvin, Christelle Meriguet, Roselyne Lacaze, 2003: A Global Database of Land Surface Parameters at 1-km Resolution in Meteorological and Climate Models. J. Climate, 16, 1261–1282. doi: 10.1175/1520-0442-16.9.1261

Nirjhar Shah, Mahmood Nachabe, and Mark Ross, 2007: Extinction Depth and Evapotranspiration from Ground Water under Selected Land Covers. Ground Water, 45, 3, 329–338

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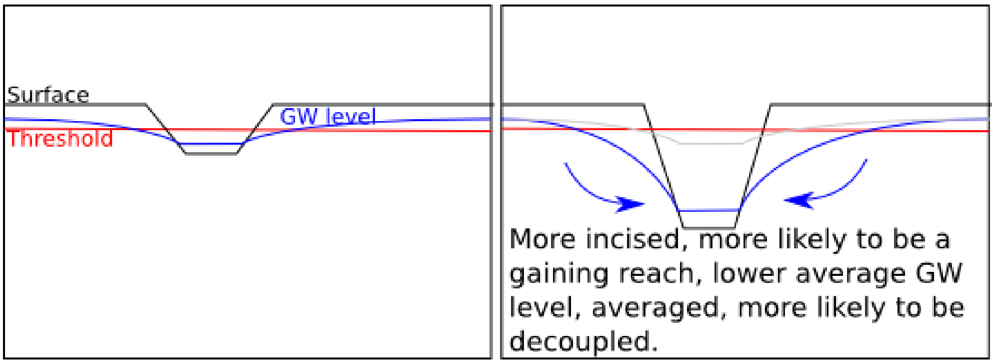

Fig. 1. Deeply incised channels and their relation with groundwater levels.