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Interactive Comment

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

Anonymous Referee #1

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General Comments: The authors build on recent research regarding the influence of groundwater processes, including lateral groundwater convergence, on water and energy fluxes at the land surface. The authors use a somewhat ad hoc modeling approach at relatively course spatial and temporal resolution to estimate the groundwater contribution to evapotranspiration in (topographically) flat regions of the Danube River basin. Their model results suggest that while groundwater constitutes a significant fraction of total evapotranspiration during hot, dry summer conditions, local (vertical) groundwater processes provide the dominant contribution, with negligible contribution from lateral groundwater convergence. The context and importance of the current study are clearly and concisely detailed in the Introduction, and overall the manuscript is clear, succinct, and well written. However, I suspect the results presented here are strongly dependent on the specific areas considered as well as the modeling approach, including model formulation and spatial/temporal resolution.

Specific Comments: (1) The modeling approach used here appears insufficient to capture the complex interaction between evapotranspiration and lateral surface and subsurface 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)?

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.

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.

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.

(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.

(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.

(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).

(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).

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Minor/Editorial Comments: (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".

(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.

(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.

(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.

(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)?

(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).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 1541, 2011.