Hydrol. Earth Syst. Sci. Discuss., 7, C2205–C2208, 2010
www.hydrol-earth-syst-sci-discuss.net/7/C2205/2010/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Potential groundwater contribution to Amazon evapotranspiration" *by* Y. Fan and G. Miguez-Macho

Dr. Teuling (Referee)

ryan.teuling@wur.nl

Received and published: 9 September 2010

General comments

The current manuscript addresses the important issue of groundwater and its role in the terrestrial water cycle in one of the key regions that has a potential to impact future global climate through large-scale vegetation-hydrology feedback's. The paper is well written and well illustrated. The authors have gone through a large effort in collecting groundwater data from various sources, and run their high-resolution equilibrium groundwater model on a fine grid on a continental scale. This, by itself, could merit publication if the main goal of the paper was to show groundwater distributions can be simulated with reasonable accuracy at the continental scale, and if similar work is not

yet published (or submitted) elsewhere.

However, the goal of the paper is more specific, namely to show how the presence of a groundwater table can potentially impact hydrological fluxes across the land surface in the Amazon. This is a topic of high relevance, as is also claimed by the authors. Since the paper is likely to make a significant contribution to this area of research, it is important that the conclusions that are drawn from the study are robust and valid over the relevant timescales over which hydrological budgets are usually evaluated in the Amazon (i.e. months to seasons). I feel, however, that the methodology used in the study does not provide such robust estimates, and that it needs revisions before possible publication. While I do not dispute the role that groundwater can have in the terrestrial hydrological cycle, I feel the potential values that are given in the manuscript will be significantly reduced when dynamical effects are taken into account. I think that not only the quality of the manuscript, but moreover the potential impact, will be improved whenever dynamical effects (i.e., impacts of alternation between wet and dry seasons) are considered jointly with the mean depth to the water table (an aspect that is covered well in the manuscript). This can for instance be done by running a Richards' equation-based model with seasonal forcing and map the capillary flux during the dry season to the mean depth of the water table. Only by doing so the authors can claim that groundwater can "sustain" fluxes (this implies a temporal dimension).

In the present manuscript, the authors defend their approach of taking a steady-state Richards' equation-based model to determine capillary fluxes by citing two papers that they claim show that groundwater levels in the Amazon lag behind the rainfall, and are in fact highest in the dry season. However if this in really true, it is also open for a different interpretation. I would interpret a long lag in the response of groundwater to input from the unsaturated zone as a sign of week coupling between the two; not as a sign of strong coupling as claimed by the authors. In addition, the papers cited by the authors do not support the claims about delayed response of the groundwater table. For 3 sites with shallow groundwater table (so where the impact of capillary rise is potentially largest) Jirka et al. report decreasing groundwater tables throughout the dry season (their Figure 4), and not increasing ones such as claimed by the authors. This is a serious error. It is consistent though with the "classical" view that any capillary rise leads to a rapid reduction in groundwater levels due to the small specific yield value (Tomasella et al. report a specific yield value of 0.17), effectively leading to a new equilibrium between the groundwater and unsaturated zone with reduced capillary rise. Tomasella et al do report a "4-month travel time of recharge" for sites on a plateau, but groundwater tables here are deep and likely have little effect on surface fluxes (as also shown by the authors). The possible dynamical effects and the role of the small specific yield in controlling groundwater fluctuations should be discussed.

In addition to this, I have a problem with the authors' claims that there estimates are conservative. While I agree with most of their arguments, they don't provide straightforward arguments as to why their estimates might in fact over- rather than underestimate the capillary rise. For instance, they claim that by taking the most conservative (lowest) recharge estimate, groundwater tables are over- rather than underestimated in the simulations. However, even the land surface model with the lowest recharge would calculate even lower recharge when the effect of a groundwater table would be considered in this step already (since it effectively lowers the flux from the unsaturated zone to the groundwater by reducing pressure gradients). This effect is not discussed, even though I am aware that the approach is common practice in hydrology. Another effect which increases the estimates is the choice of the upper boundary condition in the Richards' equation-model simulations. Choosing wilting point suction as a boundary condition results in large gradients and large fluxes, especially for situations where high suction at the (dry) surface coincides with shallow groundwater tables. I doubt that such a combination will occur in nature, especially beneath dense canopy cover in the Amazon. Different upper boundary conditions can have a large impact on the calculated capillary rise with Richards' equation (see e.g., Bogaart et al., WRR, 2008). In my opinion, the fact that capillary rise values are calculated that exceed the potential ET is a direct result of the two effects mentioned above and proof of the fact that the

estimates are over- rather than underestimated.

In summary, the manuscript by Fan and Miguez-Macho is clearly written, structured and illustrated. The first part about the equilibrium groundwater tables is excellent. In the second part, a proper justification for the large reported values for capillary flow is lacking, so that I cannot recommend publishing the manuscript in its current form. I am convinced though that a revised manuscript including a rough quantification of dynamical effects can make a significant impact in the field, and I encourage the authors to provide such a revision.

Minor comments

The paper by Saleska et al (2007) has recently been criticized. In this light it would be good to also cite other papers, for instance Samanta et al. (2010).

Page 5133, Line 16/17: "... root system does not seem to take water beyond 2 m depth". Others have reported root water uptake to occur much deeper than that, up to at least 10 m (Bruno et al., 2006).

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 5131, 2010.