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Hydrology and Earth System Sciences Discussions

Interactive comment on "Biotic pump of atmospheric moisture as driver of the hydrological cycle on land" *by* A. M. Makarieva and V. G. Gorshkov

A. Nobre

anobre@ltid.inpe.br

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Living amidst a very active international community of meteorologists dedicated to the understanding of terrestrial biosphere-atmosphere interactions, and having spent 20 years conducting field observations and experiments in Amazonia, I was struck by the biotic pump proposition developed in this paper. Not because I think such a mechanism lacks physical substance, but rather because the source of what the authors call evaporative force appears to me, after reading the explanations in the paper and the debate in this discussion, as self evident, a realization stemming on well known and well established principles of gas physics. Therefore, how could mainstream meteorology, having developed highly sophisticated and powerful numerical representations of the atmosphere, plainly ignore such a key physical force? After some investigation, motivated by the reading of this paper, I found some clues. Although not written in papers, it is not uncommon to hear references to the fact that the representation of tropical convection is poor (understood as having poor physics) and somewhat controversial in virtually all atmospheric models. As a result, the representation of rainfall in the tropical areas tend to be poor. Some NCEP reanalysis data, for example, places more rainfall on Maranhão to the East of the Amazon, a transition to a semi-arid zone, than on some areas within Amazonia to the West. Problems of grid scale, sub-grid phenomena (like cloud representation), and other complexity and non-linearity issues are usually blamed for these mismatches. From our research we know that tropical rainforest trees are extremely efficient evaporators (Tomasella et al, subm, Cuartas et al, 2006). Because of this fact (already known for many decades), most of the available energy in such system is consumed by evaporation, being converted at the surface to latent heat. Resulting from this, surface temperature is drastically lowered if compared to a drier surface elsewhere at the same latitude. Even when the surrounding tropical Atlantic average SSTs are compared to average rainforest surface temperatures it becomes apparent that the Amazon *green ocean&* (sensu Andrea et al, 2004) is in average consistently cooler than the tropical Atlantic. If I get it right, cool temperatures at the surface tend to be associated to higher atmospheric pressures, while hotter surface temperatures tend to be associated with lower atmospheric pressures. Following down pressure gradients, wind tend to blow from high to low pressures. But in the Amazon-Atlantic coupling the winds blow consistently from sea to land, precisely the contrary to what the surface temperature (and associated buoyancies) would entail. Therefore, conventional meteorological wisdom, as listed out by Dr. Dovgaluk, produces in Amazonia what looks like a paradox. When confronted with this indication, conventional meteorological wisdom is quick to point either to planetary circulation forcing or, mainly, to convection in Amazonia itself (?) as a source of lower pressure at the

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surface that then drives winds inland. But, isn't precisely convection that is poorly represented in the models? I am then left with the impending sensation that meteorological models have captured convection using a greater degree of parameterization and a smaller degree of representation for the physics, a numerical mimics of sorts. I can see that the logic of the evaporative force not only would resolve the paradox enunciated above, it also would give a good clue in the understanding of long term climate stability in Amazonia. Paleoclimatologists as well as paleontologists have suggested that South America enjoyed sufficiently stable and humid climate for at least tens of thousands of years (Baker et al, 2001), maybe even millions of years (Hooghiemstra et al, 2002). Given the fact that climate forcing over such long spans of time would hardly justify stable climates on land, due to profound changes in oceans, ice caps and inferred atmospheric circulation patterns, it ensues that South American forests must have enjoyed some special mechanisms to guarantee the availability of moisture on land, even when circulation was unfavorable if it was the case. Revisiting these inferences, now illuminated by the mechanism proposed in the biotic moisture pump, I can find a defensible hypothesis on South America paleoclimate stability, although it still remains a hypothesis in need of proof.

Questions 1) I reckon that during the time of the Pangea, circa 200 Myears ago, the single continent had vast interior regions completely arid, an aridity that has evolved after the continent earlier had vegetation cover. This aridity extended across the equatorial belt, something unthinkable today due to the inter tropical convergence zone that produces rainfall all around the globe. According with your formulations, and ignoring for now the complexities of atmospheric circulation changes, this ancient aridity in the single continent would be easily explained by the length-scale, distance from the mega-ocean and lack of extensive forests. With the continental splits and drifts, leading to the fragmentation of the huge land mass and also with the appearance of smaller and fragmented oceans, the relative distance of any given land point to the oceans was significantly reduced, leading to changes in rainfall distribution on land. Coastal lands in these fragmented continents, depending of prevailing winds, received geophysical

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fluxes of precipitable water, even with the interior areas still arid or desert. I previously thought that this availability of moisture in the coastal zones lead to the development of luxuriant forests there, which then started to condition the land hydrology, as you so well have formulated, which on its turn created conditions inland for further expansion of the coastal forests. But in your paper you suggest that deforestation inland could compromise coastal zone forests as well, giving a mechanism through which interior deserts could overcome narrow strips of coastal forests. How then to solve the paradox of a coastal forest evolving into the interior (as one supposes it happened at least once over the course of evolution) with your explanation that a narrow forest bands cannot survive to the effect of an interior desert?

2) I found your explanations on the generation and maintenance of the Hadley circulation quite interesting. Then I started to imagine scenarios without forests and I felt like not able to solve some puzzles. For example Africa, where the Sahara lies precisely on the desert band (30o N) associated with subsiding dry air from the Hadley circulation. It is well known that the Sahara has had forests sometime in the past. If that was the case, what happened then with the Hadley circulation over that area? Using your logic, with a forested Sahara there would be evaporative force rising in both middle latitudes and the equatorial zone. What kind of circulation would have existed then? In the same line, most of the present day deserts lie in the 30 degree latitude North or South. Someone might contend your association of human deforestation with desertification by arguing that humans settled everywhere, not only in these two bands, so why deserts did not develop elsewhere (thinking of the Gobi...)?

3) In South America there are no coastal savannas, but in the inner continent they are vast, and have a typical monsoon climate. In your paper you seem to suggest that, theoretically, a dense forest present no limitation in terms of extension of cover on big continents, giving the examples of Siberia and the Amazon. Why would you think then there are savannas far inland in South America? As mentioned in the introduction of this comment, there are other evidences that South America indeed have been covered

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by almost continuous forests, but why is not the case now? If humans are to blame, why their effect only appeared in central SA?

4) Even though evapotranspiration in the Amazonian forest continues full force throughout the year, most of the basin experiences strong seasonality associated with the north south oscillation of the inter tropical convergence zone. Therefore the ITCZ is a purely geophysical periodic force that I suppose is superimposed by the biotic pump in its effects on moisture transport and rainfall. As you might know, many observational studies have suggested that SA has a typical monsoon climate, even though the massive forest sits there. Do you think this could conflict with your suggestions in the paper that the Amazon does not have a monsoon climate?

Conclusion: I repute your paper as a potential groundbreaker, a rare and welcome contribution to the advancement of the understanding of how land vegetation can be of key importance to the hydrological cycle on land.

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