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LETTER

Hydrology, forests and precipitation recycling: a reply to van der Ent et al.

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We warmly welcome the debate our article on the relationship between forest cover and water yield has inspired. Our principal goal was to illustrate how the local scale focus on demand by trees has overshadowed the larger scale role of forest cover in supplying the atmosphere with moisture, so as to address and hopefully resolve the persistent and broad contradictions found in this literature. Furthermore, we thank the commentary authors for their strong support of the overall goal of our work. We read with general satisfaction their view that: 'Ellison *et al*. (2012a) [have] initiated an important shift in thinking of forests as water suppliers, instead of mere water users'.

On the other hand, we regret that we are required to point out and correct a number of misplaced criticisms and misrepresentations of our work. To say, for example, that we assume that evapotranspiration is 'equal to green water transpired by plants and trees' or that all of the water transpired by trees 'returns to the continents and none of it is transported back to the ocean' is simply untrue. Moreover, our interest in illustrating and defending general principles has kept us from going into some of the details raised by the commentary authors (e.g. geographic variation in the thermal envelope), details we otherwise welcome and hope to see more of in future debates.

To avoid the risk of misrepresenting the commentary authors, we include direct quotes in what follows where appropriate:

> The commentary authors repeatedly suggest our, 'general reasoning is that total continental evaporation *E* [ET in (our) paper] can be assumed to be equal to green water transpired by plants and trees and thus (that we) neglect all-important nonproductive evaporation fluxes'.

We have nowhere made this assumption and are troubled by this erroneous critique of our work. We have consciously, consistently and carefully chosen the term 'evapotranspiration' or ET. In doing so, we explicitly recognize both productive transpiration and nonproductive evaporation fluxes by insisting on the term 'ET' (as opposed to either 'E', or transpiration) throughout our text. We note explicitly at the outset of our article that ET is composed of two specific elements (transpiration and evaporation).

Moreover, we clearly indicate that trees and forests have considerable potential for increasing ET compared with other forms of productive and nonproductive evaporative flows. Table 1 (a, b) (Ellison *et al*., 2012a: 810), explicitly distinguishes different ET flux components and estimates their evaporative efficiency. As this table clearly demonstrates, our article recognizes that there are other potential sources of terrestrial E and/or ET (productive and nonproductive) besides that generated by trees and compares them to each other in terms of their potential impact on the ET regime.

Although the data we cite in Table 1a may not explicitly distinguish between the transpiration and evaporation components of ET, our sources explicitly refer to 'ET'. We fully agree with the commentary authors that interception from trees is important. And we have consistently considered interception as a component of ET.

Discussing this distinction in greater detail, however, would have required significantly more space than permitted. We had to draw a line somewhere at the degree of detail in the article. As van der Ent *et al*., 2010 themselves note, 'it would be interesting to compute the different contributions to moisture recycling… from interception, soil evaporation…', (2010: 11). We assume the commentary authors agree it is difficult to do this in

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a meaningful way without doing considerably more research and writing a completely new article.

> Even more troubling is that the Commentary authors suggest we misrepresent the role and impact of terrestrial ET by assuming, 'that all this water [ET] returns to the continents and none of it is transported back to the ocean'.

This criticism is simply untrue. At no point in our article do we state that all terrestrial ET returns to terrestrial surfaces as precipitation. It *is* true we do not explicitly state that some terrestrial ET is returned directly to oceans as precipitation. But when considering the data and examples we have cited from Bosilovich, the division of recycled precipitation falling on the continent and oceans is explicit. We considered this point so self-evident, we did not say more about it. The reader should carefully note that the major part of our analysis and the data represented in Tables 2 & 3 (Ellison *et al*., 2012a: 813–814) consider '*precipitation sources*' in the nine 'major river basins and large scale study sites' for which we have data. The measure is not where ET from these areas will go to, but rather the '*source*' of the existing precipitation (for more detail on the measurement methodology, see Bosilovich & Schubert, 2002).

Thus, by definition, our numbers *neither ignore nor fail to consider* whether ET falls back over oceans. Although the commentary authors suggest that we overestimate the total amount of recycled precipitation, there is nothing to support this view based on their criticism of our article. Thus, the commentary authors' criticism on this point is both erroneous and inattentive to detail.

> The Commentary authors suggest that we calculate our ET-multiplier as follows:
>
> ET-multiplier = E / (P – E) (or) (P – Q) / Q

The reader should take careful note that we have explicitly focused attention on 'ET' and have clearly distinguished this from E (or what we have labeled oceanic evaporation, OE). We prefer the following definition of the ET multiplier: ET/OE, or what turns out to be the same: ET/R (where R stands for runoff). Thus, while the commentary authors suggest that we are using E to calculate the ET multiplier, we have in fact explicitly chosen numbers for ET (or what is equivalent to 'E_c' in the Commentary authors text).

While differences remain between the Commentary authors' and our estimates of ET, this cannot be ascribed to our failure to understand ET and where it falls. Furthermore, we point out that not enough is known about the share of terrestrial ET that falls over oceans and there is inadequate observational data on which to base a more quantitative and explicit conclusion.

In this respect, we hope that future research will endeavor to decide which measure of the ET impact on precipitation is more accurate. Unfortunately there is no scope within a Commentary or a Reply to adequately address alternative formulations.

> Our 'numbers about evaporative potential are simply based on the current global distribution of land cover, where forests are mainly present in the tropics'.

Although we do use 'global' numbers for estimating the evaporative potential of different types of land cover (p. 810), we do not say anything about their distribution, nor was this appropriate in the context of this demonstration. The statement that most forests are currently present in the tropics is simply untrue [according to the FAO (2001:329, Table 47-4), the global forest distribution is 47% tropical, 33% boreal, 11% temperate and 9% subtropical] and historically is, at best, an anachronism (see Kaplan *et al*., 2009).

Moreover, no substantive claims made in our paper are attached to the commentary authors' criticism. We assume the commentary authors wish to suggest reduced forest cover in other areas of the world means forests will produce less moisture vapor. But for us, this is of course exactly the point. Historically, the spatial extent of forests was greater in many parts of the world. Moreover as some of the deforestation literature cited in our article suggests, where forests have disappeared this is presumably related to declining rates of precipitation and/or the emergence of drought cycles, whether these have to-date been adequately measured or not.

Although we welcome the observation that evaporative potential will differ across geographic regions along with changes in the thermal envelope, we look askance at the suggestion that the evaporative efficiency and potential of forests will be lower than that of other forms of land cover. In fact, the only significant counter-argument offered by the commentary authors (the remaining evidence discussed is generally supportive of our findings) is an example from India, where 'due to the large-scale irrigation, *E* has actually increased compared to natural vegetation'. However, for one, the commentary authors say nothing about the type of natural vegetation they are comparing to irrigated agriculture, and, for another, since this example is dependent on the anthropogenic addition of 'irrigation', it represents something of an exception to the general argument we have provided. Finally, the authors fail to point out the potential advantages of the natural vegetation state.

We are likewise puzzled by the suggestion that irrigated agriculture is somehow similar in impact to natural forest cover. In a forthcoming paper, Ellison *et al.* (2012b) point out why this might not be true. The crux of the argument is related to the differences that can (but may not always) be created between the *natural* ET regime and the *anthropogenically* impacted ET regime.

The likely consequence of increased ET from irrigated agriculture is presumably the overuse of available groundwater and/or freshwater resources. Furthermore, we suggest that *both* the extensive loss of natural or semi-natural forest cover *and* the transition to irrigated (and nonirrigated) agriculture is hypothetically related to the pervasive loss of invaluable fresh groundwater resources in that part of the world. Neglecting the role and importance of such relationships, in particular in the context of progressive and potentially catastrophic climate change, is both unwise and irresponsible.

We think our GCB article provides the foundation for shifting the focus of forest-water interactions away from an over-emphasis on demand-side relationships and suggests the need for greater consideration and inclusion of supply-side relationships, the impact of land conversions on the ET regime and the related geographic distribution of precipitation and further water-recycling regimes.

We fully agree with the Commentary authors that removing all forest cover would not eliminate moisture vapor flows or precipitation events from terrestrial surfaces. However, we suggest that land use change, deforestation and change in the ET regime represent highly important, even crucial environmental and hydrological considerations.

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