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Hydrology and Earth System Sciences Discussions

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Interactive Comment

Interactive comment on "On the importance of including vegetation dynamics in Budyko's hydrological model" *by* R. J. Donohue et al.

R. J. Donohue et al.

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RESPONSE TO ANONYMOUS REFEREE #1 COMMENTS

We agree with all the reviewer's comments and have made changes to the manuscript accordingly, except for one specific comment. This comment was that changes in rooting depth necessitate a greater catchment size in analyses. We do not agree as we do not see how rooting depth dynamics place limitations on the spatial extent of analysis.

RESPONSE TO ANONYMOUS REFEREE #2 COMMENTS

Response to General Comments.

The aim of our paper was to review Budyko's hydrological model with particular emphasis on the role of vegetation in catchment hydrology. Our paper reviews how vegetation processes have been considered within the framework by Budyko and other authors and then argues that, because vegetation is an important factor in hydrology, it would be useful to incorporate it into the framework. In general, in response to the reviewer's comments we have made changes to emphasise this aim.

This manuscript does not demonstrate how to incorporate vegetation dynamics into the Budyko framework, which is an area of active research, nor does it give an exhaustive review of the Budyko-related literature (we specifically address these two issues later). Because of this the reviewer found the manuscript disappointing. Irrespective, we believe this is an important argument that needs to be made because, even though (and as has been pointed out) the importance of the role vegetation and understanding of the mechanisms of how it effects water balances is not new, vegetation (and its dynamics in particular) continues to be neglected in most catchment scale hydrological models that we are aware of. Why this occurs is puzzling considering the wide acceptance and understanding of the role of vegetation. Our hope is that this paper will encourage research to continue exploring this area of ecohydrology.

We disagree that this paper makes no contribution to the literature. There are several specific components of the manuscript that are new:

- the explicit and quantitative inclusion of rooting depth, catchment area and analysis time-scale into the formulation of the water balance;

- the review of Budyko-related work made in the specific context of vegetation, and a discussion of how vegetation characteristics can violate Budyko's assumptions under certain circumstances;

- the collation and graphical summary of previously published data, in figure 6, emphasising the potential magnitude of changes to the water balance that vegetation can have; and

- we've developed concepts about how spatially explicit vegetation dynamics, measured by time-series remote sensing, can be incorporated into the Budyko framework.

Response to Specific Comments

1.0

Any inference made about previous researchers having applied the Budyko framework inappropriately was not intended and we thank the reviewer for bringing our attention to this. We have re-worded this section to remove any such inferences and have emphasised that the reason for revisiting Budyko's framework is to examine how vegetation can affect the inherent assumptions.

We agree that the Budyko framework has been analysed previously. In our manuscript, we have formulated the problem in an explicit way to include those things (e.g. catchment area, time scale of the analysis) that are usually implicit. This makes the discussion about including vegetation much simpler because many of the ways that vegetation can affect the mass and energy balances are explicit.

With respect to the manuscript being an incomplete review of the literature, we have modified the manuscript to make it clear that we are considering vegetation-related processes relevant to the Budyko framework. We thank the reviewer for the highlighting previous publications - some of which we were not familiar with. We have now made reference to most of these (we were unable to find one) but have kept the discussion focused on papers that deal with vegetation-related processes.

2.1

We do not understand the point being made about units. It is standard practice to specify SI units (e.g. kg, s) when formulating a problem. In practical applications, multiples of those units are usually used (e.g. Mg, day, years, etc.).

The reason we have considered changes in soil water storage in such detail compared to other terms in the water balance equation is to quantify changes in bucket depth

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resulting from changes in rooting depth. We have argued (and shown with examples) that temporal variations in the volume of the bucket (via changes in rooting depth) do alter E and Q. In fact, this is one the main points made in the manuscript (this is implicit in Eq. 9 and stated again on pages 1521 and 1532).

2.2

We do not understand the point about representing Rn in mm and removing lambda. Again, we have followed the SI scheme throughout. We recognise that in applications, one can express Rn alone as a depth if one makes the assumption of constant lambda. Note that from a physical viewpoint, lambda is not constant, but we agree that it is very conservative.

We thank the reviewer for the references and have incorporated them (Schreiber 1904, Ol'dekop 1911) when Budyko's equation of relationship is introduced in the manuscript.

Strictly speaking, Rn is a micro-climatic variable because it depends on albedo and surface temperature (amongst other things) which are themselves micro-climatic. Despite this, we agree with the reviewer's comment that Rn better defines available energy for larger catchments than it does for smaller ones. This is especially true over small time-scales when H can provide considerable energy input. Our point, however, was that, as catchments become smaller, the more important it becomes to incorporate the local catchment affects in the estimation of Rn (or any other estimate of energy). In large catchments this is not important, presumably because local effects "average out", rendering climatic estimations of energy availability to be sufficient. We have re-worded this section to emphasise this point.

3.1

This section reviews the literature with specific reference to Ac and time-scale and how vegetation has been considered previously within the Budyko framework. It is important for the discussion that follows that links (albeit qualitatively) scale, vegetation and

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Budyko. Whilst this pulls together some points already made by a variety of authors, we are not aware of a review that has specifically pointed out how the effects of vegetation can violate Budyko's assumptions in some circumstances.

3.2

We did not intend to make the inferences which reviewer has highlighted and have re-written it to remove any implicit suggestion that previous researchers have incorrectly or inadequately applied Budyko. Instead we have emphasised that it would be beneficial if we were able to apply Budyko to small spatial and time-scales and to more ecologically-oriented applications.

We have removed the line "Integration of vegetation into Budyko framework is expected to increase its reliability in predicting E and Q when Ac is small..." on page 1528. This also serves to reduce repetition.

4.1

This section outlines how dynamics in L, Ag and Zr each affect plant water use and does not make any proposal for how to integrate these vegetation attributes into the Budyko framework. Instead, an approach for doing this is proposed in section 5.2 where we argue that it is only practical to measure L (because it is an above-ground, structural characteristic) and that measures of L will be somewhat correlated to both Ag and Zr. Then in 5.3 we further state that L is a difficult characteristic to measure repeatedly across space and time and so the functionally-similar fPAR, which is easily measured via remote sensing, it is an excellent alternative.

4.2

We did not intend to confuse readers by using the term raingreen and have modified this section. We can see that the term raingreen is confusing the way we have used it, which was meant to encompass all vegetation types that have dramatic growing periods triggered by the sudden availability of some limiting resource, one of which

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is water. We have renamed evergreen and raingreen to be persistent and recurrent functional vegetation types, respectively.

We disagree that the introduction suggests that one objective of the paper is to test the vegetation-in-Budyko hypothesis. Instead, the stated aim is to review Budyko in relation to vegetation and to present an argument for why it is important to incorporate vegetation.

4.3

We do not understand the point being made because we did not say that raingreen Ag is greater than evergreen Ag. We agree that the Ag of annuals and deciduous trees can be higher than that of evergreens and have specifically stated this in section 4.1. What we have said in section 4.3 is that clearing perennial vegetation instantly reduces Zr, Ag and L - a statement made on the assumption that, after clearing, the landscape would have very little vegetation of any type remaining. After that, when raingreen vegetation re-establishes, E will be lower overall. This is because the total water use of raingreen vegetation is generally less than that of evergreen vegetation as has been argued with references in section 4.2.

4.4

The comment about the summary seems to be a matter of personal style because the offending "summary" is only one paragraph long. Some people like summaries at the end of sections and others don't. Personally, we would prefer a short sharp (single paragraph) summary at the end of this section, but we can remove it if necessary.

5.1

Whilst the work of Eagleson in the late 70s has been very influential in promoting the concept of equilibrium, some of his assumptions have been shown to be biologically unrealistic (Kerkhoff, A. J., Martens, S. N., and Milne, B. T.: An ecological evaluation of Eagleson's optimality hypotheses, Functional Ecology, 18, 404-413, 2004). In particu-

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lar, the hypothesis that natural vegetation systems develop a growth equilibrium state in which average soil moisture is maximised and transpiration is minimised. Specht in his seminal 1972 paper demonstrated that vegetation in water-limited environments uses the maximum possible amount of soil water (i.e., minimises Sw) without completely depleting supplies. This is the opposite of Eagleson's assumption. We have modified this section to cite the pioneering work by Eagleson, but in view of the above-noted problems, it is difficult to build on that work.

We have stated that L may provide a surrogate measure of Zr (as opposed to "a reliable predictor of") and only in some circumstances. This idea originally comes from Specht (1972) which we indicated by citing Specht's paper. That L may also provide a surrogate for Ag is implied only, and is a conclusion that flows from the same reasoning as that for Zr.

We do not understand the point made by the reviewer that the relationships between L and Zr (or Ag) only hold at large spatial or time-scales. L is a sensitive indicator of water availability and has shown to lag behind changes in water availability only by about one month (see Pook's series of papers in the Australian Journal of Botany, 1984, 1985 and 1986). That L is sensitive to changes in water availability over fine spatial scales is readily observed by contrasting riparian vegetation with adjacent hill slope vegetation.

5.2

We agree with the reviewer that sensing L from space is a research topic in its own right. The purpose of the section on remote sensing was to highlight the potential of this technology for gathering spatially and temporarily dense vegetation information relevant to hydrology. This section is intentionally succinct as this purpose can be achieved with a very focussed literature review. We hadn't discussed in detail the relationship between NDVI and L, even though there has been extensive work in this area, simply because the relationship is inherently non-linear and saturating and using NDVI to estimate L remains problematic. Considering that the relationship between fPAR and NDVI does not suffer from this problem, we highlighted the potential of using fPAR without entering into discussions of NDVI-L relations. We have now, however, changed the section slightly to make the above argument more explicit.

6.0

Again, the summary style seems to be a matter of personal preference. For example, two of the three reviewers thought that this paper was well written.

We agree that testing of this hypothesis is needed. However, we feel it would take an in-depth study rather than a case study to provide a convincing proof of concept and that this would be outside the scope of this paper. We believe that the current paper is still useful without a case study. Other reviewers have agreed saying that the analysis presented is useful and thorough. The principal issue is that while most hydrologists (and ecologists) accept that we need to put vegetation into catchment water balance models, that is not as yet a routine practice in hydrological research. Our aim is to stimulate that research.

RESPONSE TO ANONYMOUS REFEREE #4 COMMENTS

We do not entirely understand the point being made about the definition of steady-state. We don't understand how the description outlined by the reviewer is markedly different from what we have presented on page 1520. However, we have added the following to try to clarify this point in the manuscript: "In reality, dSw/dt is almost continually varying due to fluctuations in P and E and steady-state conditions are typically established in analyses by integrating Eq. (1) over a finite time period that is larger than the time-scale of fluctuations in Sw."

We thank the reviewer for pointing out the error in our reference to the Rodriguez-Iturbe and Porporato paper. We have made the appropriate changes.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 3, 1517, 2006.

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