We thank the reviewer for these helpful comments. Reviewer comments are listed below, along with our response to each. In some cases, we describe the proposed revisions to the manuscript (with line numbers) or refer to proposed revisions described in our responses to the other reviewers, but we recognize that the revised manuscript is requested in a subsequent step.

Comment 1:

This technical note outlines the steps for analytically deriving the values of the parameters used in the two main versions of the Budyko model; w for the Fu version and n for the Choudhury-Yang version. The authors derive these as functions of the main model variables: average E, P and Ep. Their key point is that the w/n parameters are only correctly derived as functions of three model variables and should not be expected to be physically related to any other biophysical process, except through the dependence other processes have on E, Ep and P themselves. They conclude that this derivation provides a needed solution to the question of how w/n are related to biophysical processes.

Response 1:

We thank the reviewer for the comprehensive and accurate summary.

Comment 2:

I can not comment on the maths in this article as it is beyond my expertise. I can comment on the concepts and implications of the maths. I have two main points to make. Overall, I find nothing technically wrong with the article but question the value of its contribution.

Response 2:

As noted in our responses to other reviewers, the primary aim of our technical note was to present the technical details of analytically inverting the parametric Budyko equations, with a secondary aim of providing a framework for improved mechanistic understanding of *n* and *w*. We aim to better support the value of the contribution, particularly focusing on the context and interpretation of the second aim, both in our proposed edits (see response to **Reviewer 1, Comment 3**) and in responses to the reviewer's subsequent comments.

Comment 3:

Firstly, the derivation of analytical solutions to the two parameters doesn't seem like a new contribution. Sposito [2017, Understanding the Budyko Equation, Water, 9(40)] has done similar including analytically deriving the data space of each parameter. I seek guidance from the editor on the requirements for novelty in technical notes and ask the authors to explain how their study provides an advance over what has already been done.

Response 3:

We thank the reviewer for connecting our work and that of Sposito (2017). Indeed, we were aware of Sposito (2017), and his work is cited in the companion research article to this technical note (Reaver et al., 2020) (hess-2020-584, Reinterpreting the Budyko Framework, cited on page 3, line 57). However, Sposito (2017) does not analytically invert the parametric Budyko equations as we did in this technical note, nor are we aware of any other study which does so. Since no other known study has produced the analytical inversion of either parametric Budyko equation, we can say with a high degree of certainty that these results are novel. Additionally, Sposito (2017) does not analytically derive the data space of each parameter (i.e., $n \in (0, \infty)$ and $w \in (1, \infty)$) as suggested by the reviewer, rather he reports the range of values that *n* and *w* can take that were determined in the formal derivations of the parametric Budyko equations (Yang et al., 2008;Zhang et al., 2004;Fu, 1981). We also report the values that *n* and *w* can take in our technical note (page 5, lines 112-114) and confirm that the explicit expressions for *n* and *w* (Equation 15, page 4, line 89) have the same data space (pages 5-6, lines 114-123).

The primary aim of Sposito (2017) was to show that if one postulates that the function, $f(\ )$, of the "original" Budyko hypothesis, $\bar{E} = f(\bar{P}, \overline{E_0})$, is homogeneous, then by borrowing concepts from equilibrium thermodynamics, one can derive several extant non-parametric and parametric Budyko equations (specifically, Equations 2, 4, and 5, in our technical note) from their Legendre transformations. Sposito (2017) shows that if one explicitly defines how $\bar{E}$ changes in response to changes in both $\bar{P}$ and $\overline{E_0}$ (i.e., the partial derivatives, $\left(\frac{\partial \bar{E}}{\partial \bar{P}}\right)_{\overline{E_0}}$ and $\left(\frac{\partial \bar{E}}{\partial \overline{E_0}}\right)_{\bar{P}}$), those relationships can be used to define the Legendre transformation, which can be used derived the resulting Budyko curve. Furthermore, Sposito (2017) critically reviews the current interpretations of the parametric Budyko equations and points out that there is a fundamental contradiction between the parametric equations and the postulate that solutions to the "original" Budyko hypothesis, $\bar{E} = f(\bar{P}, \overline{E_0})$, are homogeneous. This leads Sposito (2017) to state, "It must be concluded, therefore, that the current physical interpretation of the MCY and Fu model parameters [n and w, respectively] may be spurious." (page 12 of Sposito (2017), a statement we support.

Regarding how this work provides an advance over previous work, prior to this technical note, the parametric Budyko equations have only been inverted numerically; providing analytical expressions for this inversion is a novel advancement in the understanding of *n* and *w* and provides the general relationship between the catchment-specific parameters and biophysical features. An important interpretation of this relationship is that values for *n* and *w* can only be computed if $\bar{P}$, $\overline{E_0}$, and $\bar{E}$ are known *a priori* (obtained either from empirical data or models). Thus, any dependence *n* and *w* have on biophysical features must be through the dependence of $\bar{P}$, $\overline{E_0}$, and $\bar{E}$ on those same features. We address this concept briefly in the original manuscript but propose further edits to the abstract, introduction, and discussion and conclusions sections to make this point more strongly (see response to **Reviewer 1, Comment 3**).

Finally, we note that the many previously proposed statistical relationships between catchment biophysical features and *n* or *w* previously proposed, e.g., (Yang et al., 2007;Donohue et al., 2012;Yang et al., 2009;Shao et al., 2012;Li et al., 2013;Xu et al., 2013;Cong et al., 2015;Yang et al., 2016;Zhang et al., 2018;Abatzoglou and Ficklin, 2017;Xing et al., 2018;Zhao et al., 2020;Ning et al., 2020b;Ning et al., 2020a;Li et al., 2020b;Li et al., 2020a;Zhang et al., 2019;Ning et al., 2019;Bai et al., 2019;Ning et al., 2017) are all incomplete, special-case approximations of the explicit expressions derived in our manuscript. Such special-case approximations are limited by the specific data and regression models used in their development. The novel contribution of the analytical expressions we derived is that $\overline{E_0}$, $\bar{P}$, and $\bar{E}$ can be first expressed in terms of biophysical features (either theoretically or empirically) and then substituted into Equation 14 to understand the explicit relationships among *n*, *w*, and proposed biophysical features.

Comment 4:

My other point is that, while the authors provide an analytical solution to how to define w and n, I am unsure of what problem it is that they are solving. They indicate the problem has been stated in the literature (L62): "Notably, there has not been an analytical derivation illustrating how n and w relate to biophysical features, though the importance of doing so has been noted many times." and (L169): ": : :the literature-identified need of an analytical expression." However, the motivation behind trying to

predict a catchment's parameter value is to be able to use Budyko to make predictions about E and Q based on Ep and P – that is, in ungauged catchments. The analytical definitions of w and n are given here as functions of E and so cannot address the need most of the literature is trying to address, which is prediction in ungauged catchments.

Response 4:
We agree with the reviewer's assessment that the utility of explicit expressions for *n* and *w* (Equation 14, page 4, line 85) is not for prediction of $\bar{E}$ and $\bar{Q}$ in ungauged catchments. Rather, these explicit expressions illustrate how the catchment-specific parameter can depend on biophysical features, but only through the dependence of $\overline{E_0}$, $\bar{P}$, and $\bar{E}$ on those same features. One of the implications of this dependence is that if only $\overline{E_0}$ and $\bar{P}$ are known for an ungauged catchment, n or w cannot be determined (Reaver et al. (2020). Additionally, the complexity and highly non-linear nature of relationship between n or w and $\overline{E_0}$, $\bar{P}$, and $\bar{E}$ highlights how the many studies purporting to develop explicit expressions for n or w in terms of biophysical features arrive at such widely divergent results, both in the functional forms of the relationships and what biophysical features are included.

Comment 5:
Further, the need stated in the literature calls for a biophysical understanding of the parameters. The authors claim their solution ": : :.thus fulfills the literature-identified need of an analytical expression for n and w in terms of biophysical features." . While they have provided an analytical solution I am not convinced it provides a solution that is any more connected to biophysical features any more than the original formulations are. Another way of saying this is, the solution doesn't provide any greater biophysical understanding of the meaning of w or n than previously existed, nor does it make Budyko any more useful. Again, how does this then address the need to be able to predict w and n ungauged catchments? I think that the authors need to rethink what is the question they are trying to address and ensure it represents an advance in the use of Budyko to make hydrological predictions.

Response 5:
Through our derivation, we provide a greater understanding of how *n* and *w* are connected to catchment biophysical features (i.e., through the dependence of $\overline{E_0}$, $\bar{P}$, and $\bar{E}$ on those same features). It is true that this result does not improve the utility of the parametric Budyko equations, but it does explain how the catchment-specific parameter is connected to biophysical features, and thus addresses the calls for a biophysical understanding of *n* and *w*. While this may not be the resolution that the literature calls for (i.e., an analytical expression for *n* and *w* in terms of biophysical features), it is nevertheless the resolution obtained through a logical and careful consideration of the parametric Budyko equations. To make this point more explicitly, we propose to add the following text to the final paragraph of the discussion and conclusions section (after the proposed text in our response to **Reviewer 1, Comment 3**):

"While this result does not improve the utility of the parametric Budyko equations, it explains how the catchment-specific parameter is connected to biophysical features, and thus addresses the calls for a better biophysical understanding of n and w. While this may not be the intended resolution called for in the literature (i.e., an analytical expression for n and w in terms of biophysical features), it is nevertheless the outcome obtained through a logical and careful consideration of the parametric Budyko equations."

To close, we note that our conclusions about the nature of *n* and *w* emerged from a genuine interest in the parametric Budyko equations and a careful study of the catchment hydrology literature in an attempt to improve the biophysical understanding of the catchment-specific parameters. We wholeheartedly agree that these explicit expressions do not make the parametric Budyko equations more useful for making hydrological predictions, however, we emphasize that their underlying “uselessness” is due to the fact that the parametric Budyko equations themselves are not useful for making hydrological predictions, a theme we elaborate on in detail in the companion research article (Reaver et al. 2020).

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