The authors have addressed my concerns in the previous review and the modified manuscript with the added texts and figure is now clearer and provide more insights. I recommend the paper to be published after clarifying the minor points listed below.

- Clarify "numerical consistency" in Section 2:
  Following L116 ("Numerical consistency means that budget closes not only analytically but also after discretization"), these two sentences "Eq.8 is numerically not consistent with Eq. 6" (L116) and "we search for an alternative formulation which is… numerically consistent with Eq. 6 " (L117-L118) really don't make sense. First, Eq. 8 and Eq. 6 are two mathematically equivalent, continuous differential equations, and saying that they are not numerical consistent is confusing and meaningless as numerical scheme/analysis has not come into play yet (In contrast, L160-L161 makes more logical sense as you're discussing the numerical methods used to solve the equations there). Furthermore, budget closure generally refers to the balance between the left- and right-hand sides of ONE equation. Therefore, when indicating two equations are numerically consistent or not with your given definition linking to budget closure, it is unclear whose budget closure you are implying. It appears to me that two concepts are somewhat mixed up: one is budget closure, which is theoretically possible for any form of equation as long as the applied numerical analysis of that equation showing a balance between its left- and right-hand side terms. The other concept is "numerical consistency", which I guess you meant which derived equations in the Cartesian coordinate, after discretization, can be more consistent to the discretized governing equation solved in the model (i.e., compared to Eq.8, Eq. 11 is more similar to Eq. 6 because "the coordinate metric z_η appears within the derivatives" from the authors' reply to my previous comment #6). The required modifications are minor but It is important to be precise here as these sentences are key to understand why an alternative equation is necessary and to justify your selected budget equation for the precise budget tool. I also found that these confusing wording disturb the logical flow of this article. Below are some possible changes that authors may consider (please modify the content if I misunderstood anything):

L116: "As will be pointed out in Sect. 2.3, Eq. 8 is not ideal for budget closure because the contained derivative terms cannot be discretized using consistent numerical methods with those

for the governing equation (Eq. 6) in WRF."

L128: "Using the …., one can show that Eq. 11 is mathematically equivalent to Eq. 8. For example, the horizontal flux divergence term in Eq. 10 can be expressed as:
(Eq. 12)
Dividing Eq.12 by z_η gives the same expression of the horizontal flux divergence term in Eq. 8. The left-hand side of ...analogously."

L137: "Instead of Eq.8, we select Eq.11 as the budget equation because the coordinate metric z_η appears within the derivatives as in the WRF governing equation (Eq. 6), and so the associated budget analysis can be closes more preciously in consistent with the model dynamics (see Sect. 2.3). For this, we need to …

- L269-270: "For general applications, other grid-resolved and parameterized physics terms are possible and categorized as additional budget components." I acknowledge that this sentence was my suggestion but I missed an "and" there in my previous review comment...