Appeal of rejection of GCB-20-1713

Dear Editor:

We would like to appeal the reject decision of our manuscript "Global patterns of forest autotrophic carbon fluxes" (GCB-20-1713) on the grounds that Reviewer 1 (of 2) was off-base in (1) providing an inaccurate assessment of the scope of our analysis relative to previous studies, (2) under-valuing high-quality synthesis that produces unsurprising results, (3) setting expectations for this type of analysis that are out of line with the state of the field, and (4) recommending a completely inappropriate alternate venue. We detail these objections below:

(1) Reviewer 1 provided an inaccurate assessment of the scope of our analysis relative to previous studies. The reviewer states, "...the findings seem not improving contemporary knowledge of forest carbon cycling, and the study appears to be a simple statistical analyses of literature data..." and "the analyses performed were too conventional that similar analyses with a smaller size of similar datasets have been performed a decade ago." These statements betray a lack of perspective on the scope of our analysis relative to previous studies. Whereas most studies consider at most a few flux variables, our considers nine. The most comprehensive similar study is Luyssaert et al. (2007), which was published in *GCB* more than a decade ago, includes much less data, does not control for effects of stand age/ disturbance history, and examines latitudinal/ global climatic trends in only # variables. This study has been cited >580 times and was highlighted as a noteworthy publication within *GCB* (Long, 2020). It would seem that something along these lines, but with an expanded database and a more carefully controlled treatment of forest age and management/disturbance history, would have high potential to become a "classic" reference in global forest carbon cycling.

(2) We feel that Reviewer 1 under-values high-quality synthesis that produces unsurprising results. The reviewer objects that the results are not surprising–e.g., that it is not a new finding that C flux declines with latitude and increases with mean annual temperature. We agree that the results are not surprising, and we recognize that surprising results tend to produce high-impact publications. However, there is also a place for high-quality synthesis that clarifies current understanding, which is the goal of this publication. Such syntheses are often published as review papers, but given that our analysis entails original analysis, we submitted it as a Primary Research Article. Perhaps it would be better suited, and reviewed with more appropriate expectations, as a Research Review (*sensu* Anderson-Teixeira et al., 2016).

(3) Reviewer 1 set an unrealistic expectation that an analysis of this nature would reveal mechanisms, which is out of line with the state of the field. The reviewer objects to the fact that the study does not "reveal the biological mechanisms lead to the detected pattern", and states, "Our contemporary knowledge on forest carbon cycling has moved well beyond using mean climatic variable to extrapolate or guess the response of forest carbon cycling to climate change. The spatial gradient do not necessarily represent the response of forests to climate change." We agree, and fully acknowledge in the paper, that this study does not reveal the biological mechanisms. Further, we do not argue that the spatial gradient represents the response of forests to climate change, as clearly discussed in the final paragraph of the discussion. However, we do argue that broad-scale patterns in C cycling across climatic gradients remain–and should remain–one of several important, complementary approaches to addressing a challenging problem [*sensu* Anderson-Teixeira et al. 2013, GCB]. We note that studies using this approach have been published this year in *Science* (Sullivan et al., 2020)... **(add some more examples)**. The reviewer's apparent wholesale rejection of this approach seems short-sighted, and the expectation that a single study attempt to explain mechanisms behind global patterns in nine flux variables seems naïve. The implied standard that studies of this type should "reveal the biological mechanisms" could be interpreted in various ways, but the best way of getting at underlying biological mechanisms would be through modeling. Integration of ForC with mechanistic ecosystem models

has yet to be acheived, but lies within the vision of leaders in the modeling community, as stated in an opinion piece that I recently reviewed a paper for *GCB* (give citation/ number). (This piece lays out a vision for improvement of community infrastructure for modeling, including one *stated the goal of linking with leading databases, specifically citing ForC.*) In the meantime, the current analysis will be at the clear forefront of studies examinging correlations between forest carbon flux and climate at the global scale.

(4) Reviewer 1's suggestion that the work be published in a data journal betrays carelessness of review and/or limited understanding of appropriate publishing standards. The reviewer suggested (twice) that we "consider a data journal like ESSD". This suggestion is completely inappropriate for two reasons. First, the hypothesis-testing analyses presented here would be completely inappropriate for a data journal. Second, the ForC database is already published (tropical portion only in Anderson-Teixeira et al., 2016, *GCB*, full database as a data paper in Anderson-Teixeira et al. 2018, *Ecology*). Many qualified reviewers would already be aware of this, and an even semi-careful review would not miss this fact, which was prominently cited in the last paragraph of the introduction and the first sentence of the methods. Thus, this suggestion betrays carelessness of review and/or limited understanding of appropriate standards for judging where scientific work should be published. Either way, it does not seem appropriate that *GCB* should put much stake in this review.

If, despite these arguments, the *GCB* Editorial Board does not feel that the paper is sufficiently competitive for publication in *GCB*, we will accept the decision and publish elsewhere. However, we believe that this manuscript has potential to become a "classic" reference on the subject of carbon cycling in forests globally, and that *GCB* would be an excellent fit, given the legacy of publishing important analyses on this theme [e.g., Luyssaert et al. (2007); Anderson-Teixeira et al. (2016); MORE]. We would like to be confident that the decision reflects a careful assessment of this manuscript's potential.

thank you,

Kristina Anderson-Teixeira (on behalf of all coauthors)

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