We would like to thank the reviewer for their useful comments which we believe have helped to improve the manuscript. Below we address each of comments these in detail.

**Reviewer’s comments**

1. *The premise is that we would expect congruence between indicators, but is this fair? What are the objectives and underlying assumptions of the indicators that might lead us to expect (or not expect) congruence? Martin et al. should address this, but possibly Newbold as well (though I note they stay clear of discussing other indicators and focus on their own, which is a very reasonable decision).*

**Author’s response**

The motivations behind the two indicators we compare in our manuscript are remarkably similar. The Biodiversity Intactness Index (BII) estimates the average abundance of originally present species relative to abundance in an undisturbed habitat1. Biomass intactness (BMI) estimates the current above- and below-ground biomass of vegetation relative to the same location without human disturbance2.

The estimations of BII and BMI are produced using very different datasets – BII uses a large dataset of field estimates of biodiversity while BMI uses a combination of national-level data, forest inventories, remote-sensed data, and data from previous syntheses of forest biomass. Despite the differences in the types of data used, given that the aims of BII and BMI are very similar we would expect them to be positively correlated and congruent in most locations. We now mention in our manuscript that these two metrics measure different aspects of biodiversity, and that while in some degraded forests it is possible that BII exceeds BMI, more generally we expect BII to be lower than BMI because biodiversity faces greater threats than land-use change alone (lines 32–38).

1. *It is not clear which data are used in the analyse by Martin – from the 2016 Newbold paper (based presumably on 2015 data), or updates on BIP or other websites? Give the almost doubling of data between Newbold’s 2015 and 2016 papers, this may influence patterns.*

**Author’s response**

The data we used in the analysis were taken from the 2016 paper by Newbold *et al* which used a version of the PREDICTS database from April 2015. Having looked at the BIP website it appears that this version of the BII map is the only one that is currently available. We have now clarified which data we used on line 40.

1. *I think this will be a useful and interesting exchange – though Martin et al. should probably try to be a little more constructive in their tone and message. They don’t offer many solutions, or suggestions about how these indicators can be used together to give greater inference about change in biodiversity, and what should be done to alter the trajectory. The question with something like the BII is not whether it has flaws (yes, it does), but rather how can the flaws be minimised, how can we fill data/knowledge gaps to make it more useful, and how can we use it to support decisions or inference about change that minimise the impacts of the flaws on decisions.*

**Author’s response**

Based on the reviewer’s comment we have revised the final paragraph of our manuscript (lines 58–71), which now identifies a set of criteria that we think the BII should meet. We also now recommend that modelled BII values are ‘ground-truthed’ by comparing them with data from new field surveys for a stratified random sample of plots with a global distribution (lines 67–69).

**References**

1. Newbold, T. *et al.* Has land use pushed terrestrial biodiversity beyond the planetary boundary? A global assessment. *Science* **353**, 288–291 (2016).
2. Erb, K.-H. *et al.* Unexpectedly large impact of forest management and grazing on global vegetation biomass. *Nature* **553**, 73–76 (2018).