Letter to the Editor

Creative Carbon Accounting—A Reply to “The Wood, the Trees, or the Forest? Carbon in Trees in Tasmanian State Forest: A Response to Comments”

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Moroni et al. (2012) made forty claims which misrepresent my earlier reply to their work (Dean, 2011) and if left unrefuted, might mislead all but the most expert reader—I cover seven of the most important ones here. Firstly, in my earlier paper I had calculated a conservative carbon deficit in State forests due to logging of the most-targeted forest types—mature wet-eucalypt—by clearfell, burn and sow to yield even-aged eucalypt regeneration. That deficit was conservative as a range of stand ages were used even though most carbon flux through logging has been from the old-growth subset. It was additionally conservative at the landscape-scale as inclusion of conversion to plantation and logging of other primary-forest types would have yielded a larger carbon deficit, not a smaller one, as implied in Moroni et al. (2012). Secondly, their claim that I applied “carbon saturation” at the landscape-scale is incorrect. Instead I applied carbon carrying capacity at that scale and included different stands ages in its calculation (by definition). Conversely, Moroni et al. (2012) produce the “confusion” which they claim to observe by advocating the use of “carbon saturation” at the landscape-scale, which can have no practical usage.

Moroni et al. [1] in their reply to my reply [2] have made numerous claims that if left unrefuted might mislead all but the most expert reader [1]. There are forty claims which misrepresent my reply to their earlier paper. I cover seven of the most important of these below.

Moroni et al. ([1], page 2) unexceptionally state that “...Dean [2] acknowledges wildfire may be “intrinsic to both the standard definition of CCC and to the time-based average method of determining carbon budgets for forest activities...”” However they follow this with the claim: “this is then ignored in the incorrect generalisation of CCC values across the landscape.” I calculated [2] the estate-wide (i.e. landscape-level) carbon deficit due to the logging of a particular forest type and age class (that which has been most sought after—mature wet-eucalypt forest, especially the tall, old-growth stage) and compared it with an independent

value of the estate-wide deficit (from a report commissioned by Forestry Tasmania [3]). The calculated deficit was then used to reveal a conservative value for the estate-wide CCC (conservative as other forest types were also logged). The method was correct because it used the two actual states of the forest (with and without logging) as observed. If any wildfire had occurred since logging, the regenerating stands would have been removed from the particular data set presented in Moroni et al. [4] and hence the deficit I calculated using these figures was due to logging alone.

I used a range of mature stand ages in my calculation of the prelogging carbon and hence also in the carbon deficit and in the calculation of CCC [2]. Note though that using a range of stand ages as I did for the pre-logging state of wet-eucalypt forests yielded a conservative value of the carbon deficit for those forest types, because the logging

has predominantly been of the old-growth subset of mature stands (e.g. [5–9]) which can have 50% higher biomass than early-mature stands [2].

Moroni et al. ([1], page 2) state: “...Dean [2] appears to agree when he criticises Moroni et al. [1] for “their unnecessary redefinition of CCC and their use of C saturation at a landscape-scale rather than its usual usage over a specific duration for a specific forest stand....Dean himself applies CCC at the landscape scale when he writes “the estimated C deficit in Tasmanian State forests (the amount below its CCC) due to commercial forestry is currently 29 (± 4) Tg.”” Moroni et al. [1] thus inferred that “the best example of the flaw in the logic” of my paper was the application of the concept of C saturation at the landscape scale. I did not apply “C saturation” at the landscape-scale. I used CCC at the landscape-scale.

Moroni et al. [1] refer to a “confusion between site-level CCC and attainable landscape-level C stocks” but after reviewing the literature I could only find such a “confusion” in work by Moroni et al. [4], Moroni et al. [1], and Moroni [10]. For example, in Moroni et al. [1]: “The dynamic nature of these ecosystems and their C stocks in time and space prevents all stands across a landscape reaching stand-level CCC at the same moment, prevents any stand from remaining at its CCC indefinitely, and makes a nonsense of the goal of managing for stand-level CCC at the landscape scale (19). It would be preferable to use the term theoretical carbon saturation *sensu* Nabuurs et al. (20) at the landscape level, restricting CCC to the stand or site level.” No one would ever suggest that forest stands stay at CCC at a fine time scale, neither individually nor in unison. Nevertheless, by the common definition of CCC, both stands and landscapes can, over appropriate time periods, maintain CCC—that is how any value of CCC is calculated. It is a spatio-temporal average. In their second sentence cited here they suggest using C saturation at the landscape level—which would surely be a purely abstract concept only, and of little use, even theoretically.

Moroni et al. ([1], page 3) make two claims about my use of forest class data: “Dean [2] overestimates CCC primarily by basing his estimate on Forest Classes 1–17.” For my CCC estimate (as stated in Dean [2]) I used a combination of: (a) data for those forest classes 1–6 and 12–14, (b) for their regrowth stage (classes 50–74), (c) an estimate of old-growth carbon typical of that present in those stands prior to State-wide high grading, and (d) an estimate from an independent publication [3]. I restricted the classes used to even-aged regrowth and wet-eucalypt forests because they have been the most targeted to date [2] and required supplementary data for them is more readily available. If I had included an estimate of CCC and the carbon deficit for other logged forest types such as dry-sclerophyll and rainforest then the calculated carbon deficit would have been larger, rather than smaller, and the CCC higher.

Moroni et al. ([1], page 3) suggest that carbon accounting would be better improved in agricultural and urban lands than in forests. However, the carbon balance of native forest industries remains very relevant to climate change science, as explained in my earlier reply [2].

While I did say that “the Tasmanian forest industry has received repeated economic support...” (Moroni et al. [1], page 4), I did not make the “suggestion” that the industry is financially unviable. I stated that the industry had received repeated economic support from the federal government in the context of showing how it related to other States and the broader society over a long time span. I was referring to the financing of industry restructuring to the harvesting of plantations but also to such things as the diesel fuel rebate, federal government support for investment through compulsory, Australia-wide superannuation schemes, and the \$2 million to develop alternatives to clearfelling old-growth forest [11].

Moroni et al. ([1], page 4) imputed a 30% drop in my calculation of changes in soil carbon, with each logging event. The default change in soil organic carbon used with each clearfell, burn, and sow event was 2.5% [12]. They also made a point of stating that this publication was “unpublished,” But it is common practice in science to present findings at conferences in order to garner feedback from peers. No complaints were received and those calculations have since been through double-blind peer review and published in a reputable journal. Although the initial findings were presented at a conference they were publicly available on the internet, like several reports by Forestry Tasmania and Moroni, referred to by Moroni et al. [4] and Moroni et al. [1].

Moroni et al. ([1], page 4) imply that I believe the carbon in wood-products to be irrelevant. I specifically referred to some of our publications where the wood-product pool is an intrinsic part of the context, some of which were cited in Moroni et al. [4] and Moroni et al. [1]. The carbon deficit I calculated in Dean [2] was carbon that could be sequestered in the forest. Wood-products cannot sequester carbon. I did not mention wood-products at length in Dean [2] as the paper was about how to properly calculate the carbon carrying capacity of the State forests, rather than about the carbon budget as a whole.

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