**Comments on First Order Draft of Chapter 2:**
**CROSS-CUTTING GUIDANCE ON GREEN-HOUSE GAS EMISSIONS AND REMOVALS FROM ORGANIC SOILS IN ALL LAND-USE CATEGORIES.**

**Comment on Table 2.1 (p. 2.6)**
This table proposes the following $CO_2$ emission numbers for tropical / subtropical: peatlands:
*Forest: 2.31 t C ha/yr or 8.45 t $CO_2$ ha/yr*
*Rice: 8.56 t C ha/yr or 31.33 t $CO_2$ ha/yr*
*Cropland: 9.11 t C ha/yr or 33.34 t $CO_2$ ha/yr*
*Forest plantation (acacia): 11.67 t C ha/yr or 42.71 t $CO_2$ ha/yr*
*Oil palm plantation: 5.24 t C ha/yr or 19.18 t $CO_2$ ha/yr*

Unlike the numbers for other climate zones, these numbers appear to be unchanged from the IPCC 2006 guidelines. No updated references to published research are given in this Draft, despite the fact that the science base has greatly improved over the last 6 years. However in the Appendix to this Chapter, some further details on the apparent thinking of the authors of this Chapter (those that were involved in drafting the content on subtropical and tropical peatlands) are presented. This includes the statement (p. 2.33):
*"DATA AVAILABILITY IN THE SCIENTIFIC LITERATURE. There are three principal approaches in the scientific literature that are useful to our efforts to estimate the effects of land use change and management on the atmosphere: … There have been a few attempts at estimating emissions from changes to peat elevations (Kool et al., 2006; Couwenberg et al. 2010), but these methods still have high degrees of uncertainty regarding what part of this subsidence represents emissions."*

**It needs to be clear to all involved that the numbers now proposed for $CO_2$ emissions from drained (sub-) tropical peatlands, as well as the misleading statement on data availability in the appendix, are wholly unacceptable in a review that is expected to present the state of the art in an unbiased manner.**

**GENERAL COMMENTS**
A) There have been a number of recent publications specifically on $CO_2$ emissions from drained tropical peatlands converted to agriculture, especially acacia and oil palm plantations. Most of these publications are not used by the authors, including the recent review papers by Couwenberg et al. (2010), Hooijer et al. (2010), Page et al. (2011), as well as the papers representing recent field studies by Hooijer et al. (2011, 2012) and Jauhiainen et al. (2011, 2012) and several others. Recent reviews of emissions from oil palm plantations for the EU (Marelli et al., 2011), for the International Council for Clean Transportation (Page et al., 2011), and for the Round Table for Sustainable Palm Oil (RSPO-PLWG, 2012) are also completely ignored. Moreover, widely used older literature is ignored, including the enlightening studies in the (subtropical) USA Everglades (e.g. Stephens and Speir 1969, Stephens et al., 1984).

As the publications referred to above are the ones most frequently cited in most other work on this subject, this is a major oversight that is difficult to understand. Nearly all of the peer-reviewed recent publications, and reviews, conclude that emissions from oil palm and pulp plantations on tropical peatlands are above 60 t/ha/yr for typical

plantation water depths. This is at least 1.5 to 3 times higher than the numbers now proposed in this Chapter.

B) The few relevant review papers on tropical emissions that are cited in the discussion (Murdiyarso et al., 2010; Hergoualc'h et al., 2011) are co-authored by the same small group of people in Bogor that now contribute to this IPCC review. Moreover, these cited papers are controversial as they come to conclusions on low $CO_2$ emissions that are in the end supported by only one credible original source proposing low emissions (Melling et al., 2005), a study that is widely considered to be biased as it was funded by the Sarawak oil palm industry and was reported to be methodologically flawed by (e.g. by Page et al., 2011; amongst others because of the very small number of measurements: 36, vs 2300 in Jauhiainen et al. 2012). It is unacceptable in an IPCC review to selectively focus on own work and ideas alone, or on reviews based on very limited evidence, especially as these papers are limited in number and scope, and unrepresentative of the scientific mainstream.

C) There are also quite a number of studies on $CO_2$ emissions from drained and degraded tropical peatlands (e.g. Jauhiainen et al., 2008; Hirano et al., 2009). While the numbers presented from these studies do include root respiration, they can not just be ignored.

D) The emission number now provided in the Draft for oil palm is less than half that of acacia. This goes against the evidence that is available, and also against common sense. Oil palm and acacia plantations both have severely disturbed top soils during clearing of natural forest, similar water table depths (in the range of 0.5 - 1 m on average), and similar high soil temperature in the absence of a closed canopy cover (see e.g. Jauhiainen et al. 2012). The main differences are that oil palm requires more fertilization, but that acacia harvesting on a 5 year rotation causes more frequent soil disturbance. On balance, peat oxidation emissions from both plantation types is likely to be similar, and that is indeed what most studies report. Again, the Chapter fails to even indicate any of these issues.

(E) If the authors really would think they have justification to suggest that net $CO_2$ emissions from oil palm plantations (low water tables, high soil temperature, high soil disturbance, high fertilization in the case of OP) are hardly higher than those from drained natural forest (higher water tables, lower soil temperature, no soil disturbance, no fertilization), as they are now in fact doing while ignoring massive evidence to the contrary, they should provide very strong arguments.

(G) Furthermore, the numbers now presented for tropical peatlands are out of line with those presented for drained temperate peatlands. It is undisputed that biological oxidation is a temperature-dependent process that proceeds much faster in hot climates, and that the resulting carbon emissions are therefore much higher in the (sub-)tropics. For that reason alone, it is clearly impossible that emissions from temperate oil palm plantations would be lower than from temperate cropland as is now indicated in Table 2.1 (p. 2.6). It appears that the authors have chosen to consider emissions from drained tropical peatlands in isolation, resulting in inconsistency and not befitting the broad integrated review that this is supposed to be.

(F) Finally, the decision by the authors to systematically ignore all studies that determine carbon loss from peat soils by determining subsidence and changes in peat characteristics (bulk density, carbon content) goes against nearly 100 years of international peat science. The only two papers referred to in this respect (Kool et al., 2006; Couwenberg et al. 2010; see above) are only a very small sample and moreover are misrepresented: e.g. Kool et al. (2006) did NOT unsuccessfully "attempt to estimate emissions from changes to peat elevations" as the authors indicate, but rather tried (and failed, that is widely agreed) to reconstruct historical subsidence from changes in peat characteristics.

Numerous peer-reviewed papers report subsidence rates and the contribution that oxidation makes to that subsidence, which allows calculation of carbon loss if carbon content and bulk density are also known. It goes too far to discuss this in-depth here, but there are good reviews available for tropical peatlands in e.g. Andriesse (1988), Couwenberg et al. (2010) and Hooijer et al. (2012), not to mention the many studies that have applied this concept in temperate climates (e.g. Leifeld et al. 2011, to name a recent example). The numerous studies done in the Everglades in the USA from 1912 onwards, as summarized in Stephens and Speir (1969) and Stephens et al. (1984), are of specific interest to this IPCC Chapter as they apply to a subtropical peatland as is the subject of this Chapter. The broad conclusions of this body of work are that i) subsidence rates are constant at 2.5 – 3 cm/yr on average excluding higher rates in the first few years, and that ii) this is caused for at least 78% by oxidation as is evident from $CO_2$ emission studies and soil studies in the area. Similar subsidence and emission rates are also reported for the Sacramento Delta peatlands in California in a number of publications. If the authors of the IPCC guidelines chapter feel they have good arguments to reject nearly the entire international body of work on the relation between subsidence and soil carbon loss, they should present these arguments in full, and allow the wider scientific community to respond. It should then also be acknowledged by IPCC that this rejection of the subsidence method applies not only to tropical peatlands but to all peatland science, as it is surely not acceptable to accept a method for most of the world but to reject it for one region.

**SUMMARY COIMMENTS AND CONCLUSION**

In short, the authors of the inputs to subtropocal and tropical inputs have failed in the following ways:

- Nearly all recent and most older peer-reviewed papers presenting CO2 emissions from drained tropical peatlands (plantations and degraded forest) are not referenced. The few references made are co-authored by the authors, and do not present original findings but biased reviews.
- All emission estimates derived from subsidence studies, the main tool in peatland carbon loss studies for many decades, are rejected on the basis of misrepresentation of 2 studies.
- Studies from subtropical regions (Florida), and from regions that are hot part of the year (California, Israel) from which lessons can also be learnt, are ignored altogether.
- No attempt is made to place (sub-)tropical emission numbers in an international context, leading to the impossible conclusion that emissions from tropical oil palm plantations are lower than from temperate croplands on peat.
- It is not even attempted to explain how emissions from tropical oil palm and acacia emissions relate to eachother. There is no reason at all to believe that

the latter would be more than double the first, nor is this suggested by recent literature.

Based on the above, it must unfortunately be concluded that the authors contributing to the tropical peat emission section of this IPCC Chapter have done an extremely poor job. Considering the importance of the subject, it is therefore strongly recommended that IPCC objectively assesses the current status of this Chapter, regarding $CO_2$ emissions from subtropical and tropical peatlands, and to consider consulting additional independent authors on this subject. The authors who are now presumably responsible for this input (Louis Verchot, Fahmuddin Agus and Supiandi Sabiham) are known to be a small and isolated group (all are based and working in Bogor, Indonesia), who hardly have a track record in peatland science and who seem either unaware of most of the published science, or strongly biased in their selective use of science to show low $CO_2$ emissions. It is especially worrying that there are allegations of close links of some involved with the oil palm and acacia industries, which are very actively lobbying to reduce the numbers used to calculate emissions from their plantations, and who are funding dubious research programmes as part of that campaign. In our view, the IPCC should not allow itself to be controlled by sectoral interests, and should be aware of the risk of being abused as a platform to bring out scientific falsifications.

It is clear that unless the numbers and justification now proposed in this Draft for (sub-)tropical peatlands are greatly improved, very few in the international independent science community will accept them. Scientific and public controversy should be expected, the independence and transparency of the process would be questioned, and the credibility of the IPCC would be damaged.

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