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## ***Interactive comment on* “Soil-atmosphere exchange of nitrous oxide, methane and carbon dioxide in a gradient of elevation in the coastal Brazilian Atlantic forest” by E. Sousa Neto et al.**

**E. Sousa Neto et al.**

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### Specific comments

p. 5229, l. 8-14: Here and throughout the manuscript, you use three different units for the three different gases, i.e.  $\text{ng N}_2\text{O-N cm}^{-1} \text{ h}^{-1}$ ,  $\text{mg CH}_4 \text{ m}^{-2} \text{ d}^{-1}$ , and  $\mu\text{mol CO}_2 \text{ m}^{-2} \text{ s}^{-1}$ . This does not help to compare the fluxes with each other and with literature. I suggest choosing a common area and time basis (e.g.  $\text{m}^{-2} \text{ h}^{-1}$ ) and deciding whether a mass unit (g) or an amount unit (mol) should be used, but please be consistent.

Author’s comment: The selection of units for reporting gas fluxes has not been standardized in the broad literature of this topic. We are neither aware of any broad stan-

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dard nor of any standard employed by Biogeosciences. We have selected units that are commonly employed and we have employed these same units in many previous papers for nearly 20 years without ever receiving a similar comment. We disagree with the reviewer comment. Comparison among the fluxes requires only simple arithmetic and comparison to the literature is easily done with the commonly employed units that we have selected.

p. 5231, l. 18: The study was conducted in humid subtropical climate, but throughout the paper you dwell extensively on the importance of tropical forests and their soils in the global GHG budget, and the scarcity of data from tropical regions. Strictly speaking, this work does not contribute to reducing the scarcity of knowledge about tropical regions, as it was not conducted in the tropical climate zone.

Author's comment: Contrary to the reviewer's undocumented comment, the region under study has been previously described as tropical moist forest (Oliveira-Filho and Fontes, 2000; Alves et al. 2010) based on quantitative geographical and climatic data. The study areas are geographically and climatically characteristic of the tropical region even though they occur near the boundaries of that region.

p. 5231, l. 20: "historical": please give the averaging period here (e., 1971-2000).

Author's comment: The averaging period was inserted into the text (1973-2004). See line 96 in the manuscript

p.5232, l. 2: Could you specify "nutrient contents" and "nutritional reserve"?

Author's comment: We removed "nutritional reserve" and rewrote the sentence for clarity (see lines 103-105 in the manuscript).

p. 5235, l. 13-14: In Table 2 I find the opposite, i.e. greater fine root biomass in the dry season.

Author's comment: The referee is absolutely right. In fact, words were out of order. Greater fine root biomass was found in the dry season. Corrections were made in the

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text (see lines 197-198 in the manuscript).

p. 5237, l. 22f and conclusions: The hole-in-the-pipe conceptual model relates fluxes through the “pipes” of nitrification and denitrification to gaseous losses of NO, N<sub>2</sub>O and N<sub>2</sub>. Thus, it requires gross rates of nitrification and denitrification for proper consideration. In this work, only net rates of N-mineralization and nitrification were determined, making the relation of the findings to the hole-in-the-pipe model invalid or at least weak. Greater nitrogen (e.g. nitrate) availability always occurs, when consumption is lower than production, irrespective of the magnitude of the fluxes. If one assumes that N<sub>2</sub>O losses are proportional to the amount of nitrogen converted, e.g. from ammonium to nitrate or from nitrate to molecular nitrogen, and hence correspond to the gross rates of nitrification or denitrification, the findings of the present work don’t necessarily have to be contradictory to the hole-in-the-pipe concept. The same applies to carbon stocks in the soil. Why do we find the largest C stocks especially in colder and drier places (also in this study)? Because the decomposition activity is lower than the production rate, despite the lower absolute productivity of the ecosystem. In other words, we have to expect lower soil respiration and N<sub>2</sub>O emission rates there despite the high soil organic matter content. It all depends on the fluxes, not on the stocks and their availability

Author’s comment: The reviewer has correctly interpreted the “hole-in-the-pipe” (HIP) theory. We have altered the discussion in our manuscript in order to accommodate the reviewer’s comments. We note that although our results can be interpreted in terms of the HIP theory as the reviewer indicates, there are still some anomalies compared to previous studies that deserve note. Please, see lines 278 ff and conclusions in the revised manuscript.

p. 5238, l. 15-20: The reasoning on oxygen availability is indeed obvious, but purely speculative. It would have been nice to have data on O<sub>2</sub> concentration profiles. As this is not the case, the authors should use a more cautious wording. Author’s comment: We have no data on O<sub>2</sub>, thus, we rewrote the sentence to reflect the speculative nature of this discussion. See lines 298-301.

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p. 5238, l. 23f: You compare your results from a sub-tropical location with data from tropical forests. Is this justified? If you think so, you should give a reason why.

Author's comment: See the comment for p. 5231, l. 18

p. 5239, l. 7f: Again, you bring forward the role of tropical forests as methane sinks, but your experimental sites are located in sub-tropical climate.

Author's comment: See the comment for p. 5231, l. 18

p. 5240, l. 11: "within the range of other tropical forests of the world": maybe I am too picky, but again, your study sites are not tropical.

Author's comment: See the comment as for p. 5231, l. 18,

p. 5240, l. 15-18, and last sentence of the Abstract: Your data do not form the basis for this statement, as it is contradictory to your own statement on p. 5231, l. 3-4, that you "expected soil gas emissions to vary with altitude responding to combinations of the factors described above", i.e. differences in climatic conditions, species composition and structure, nutrient supply and soil physical and chemical properties. Therefore, it is not justified to assume that a temperature increase alone will lead to an increase in N<sub>2</sub>O and CO<sub>2</sub> emission and in CH<sub>4</sub> uptake rates. The interactions between the factors mentioned above in terrestrial ecosystems are too complex, as that one could assume that, if one is changed, the others will remain constant.

Author's comments: We accept the reviewer's comment. We reformulated the abstract and the conclusions to reflect greater caution. See lines 35-38 in the abstract and lines 298-302 in the discussion of the manuscript, and lines 352-364 in the conclusion.

Minor points

p. 5229, l. 5: use lowercase for nitrous oxide and carbon dioxide

Author's comment: suggestion accepted, see line 25.

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p. 5229, l. 19: omit “in” after “increased”

Author’s comment: The sentence was rewritten, see lines 37.

p. 5231, l. 3: omit “that” after “expected”

Author’s comment: The text was changed according to the reviewer’s suggestion, see line 75.

p. 5231, l. 5: omit “the” before “global”

Author’s comment: The text was changed according to the reviewer’s suggestion, see line 77.

p. 5231, l. 6: use singular for “gradient elevation”

Author’s comment: The text was changed according to the reviewer’s suggestion, see line 78.

p.5231, l. 7: period after parentheses

Author’s comment: The text was changed according to the reviewer’s suggestion.

p. 5231, l. 7-9: the last sentence of this paragraph appears like an appendix. I suggest moving it further up to the beginning of the paragraph.

Author’s comment: The sentence was moved to lines 67-69.

p. 5231, l. 12: change to “on the northern coast”

Author’s comment: The text was changed according to the reviewer’s suggestion, see line 83.

p. 5235, l. 26: use singular for “litter decay”

Author’s comment: The text was changed according to the reviewer’s suggestion, see line 210.

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p. 5236, l. 7: omit “at” before “than”

Author’s comment: The text was changed according to the reviewer’s suggestion, see line 218.

p. 5240, l. 17: omit “in” after “increased”

Author’s comment: The sentence was rewritten; see lines 349f.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/7/C4800/2011/bgd-7-C4800-2011-supplement.pdf>

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Interactive comment on Biogeosciences Discuss., 7, 5227, 2010.

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