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Biogeosciences Discussions

Interactive comment on "Soil properties determine the elevational patterns of base cations and micronutrients in plant-soil system up to the upper limits of trees and shrubs" *by* Ruzhen Wang et al.

Anonymous Referee #1

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The submitted manuscript reports a study of the possible effects of elevation on base cations and micronutrients in soils and in plants. The question of the role of elevation on biogeochemical processes has been debated –and is still discussed– for years. Just recently, several papers were published on this topic (Reed et al., 2012; Sundqvist et al., 2014; Nottingham et al., 2015; Asner et al., 2016; He et al., 2016; Augusto et al., 2017). But those papers generally focus on nitrogen and phosphorus. That is why this study, which focusses on cations and micronutrients, is interesting. As a whole, the manuscript is well written, clear, and easy to read. In addition, the study design is fairly convincing (but see below) and the methods are standard method. Based on this, I believe that this manuscript should be published. My main concerns are as follows: -

Sites are not sufficiently described. It is not clear to me to which extent the different sites of a given elevation gradients are comparable or not. The flora, soils, parent material, seem quite different among altitudes. It is possible that initial heterogeneities among altitudes are the cause of the inconsistent relationships between elevation and ecosystem properties. - The manuscript is too long. Indeed, it reports results that are already obvious and well-established relationships (for instance: lines 251-257; line 258; lines 262-267; Figure S2; lines 362-364). I recommend to shorten the manuscript and to focus it on what is new (for instance lines 279-281): that is to say, the weak relationship between elevation on the one hand, and base cations and micronutrients on the other hand.

*** Other comments

* lines 29-32: these patterns are not restricted to mountains but are relevant for all soils.

* lines 43-44: not useful.

* lines 45-46: I disagree. See for instance (Naples & Fisk, 2010; Baribault et al., 2012; Sardans & Penuelas, 2015).

* lines 49-51: This applies also to Mn and Zn (see the book of Marschner, and the book of Graham et al.; both cited by the authors).

* lines 54-59: these relationships are well-known.

* line 148: this kind of analysis should be made on fresh samples (not dried samples).

* line 159: replace "slurry" by "soil-solution suspension".

* lines 293-297: It depends of the range of pH values in each site (small ranges are unable to put such relationships into evidence). I recommend to use different symbols in the Figure S2 (one symbol per study area).

* line 300: replace "decomposed" by "decomposable".

* lines 302-303: This reference is about croplands. It seems to be not relevant.

* lines 333-336 and 343-345: another explanation could be that initial differences in biogeochemical properties among elevation positions were larger than the effects of elevation. In other words, an elevation effect might exist, but it is of a too small magnitude to be detected with this study design.

* lines 353-354: I disagree. See for instance the compilation in Marschner's book.

* Table 1 should be merged with Table 2.

* Table 4: these relationships are not new. Please move this table to supplementary materials.

* Table S1: - MAP values are probably along the elevation gradient in Balang and in Qilian (such as in Changbai). Therefore, please indicate the range of MAP values for all sites. - MAT: please indicate range of values for temperature. - Soil parent materials are important drivers of soil biogeochemistry (Castle & Neff, 2009; Augusto et al., 2017). This information should be provided.

* Figure S2: in panels c and e, the relationships are clearly non-linear. Hence, why using a linear fitting?

* I suggest to change site names. For instance "Balang" by "Subtropical mountain".

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