

## ***Interactive comment on “Isovolumetric replacement and aeolian deposition contributed to Terrae calcis genesis in Franconia (central Germany)” by Bernhard Lucke et al.***

### **Anonymous Referee #1**

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The paper entitled “Isovolumetric replacement and aeolian deposition contributed to Terrae calcis genesis in Franconia (central Germany)” is an article that supplies to the debate about the origin of Terra Rossa and its soil formation processes. This research support the theory wide discussed since the first publication of Merino and Banerjee (2008) about an allochthonous material and its weathering that contributes to replace carbonates in the bedrock to generate soil. The article is well written in English, however, many aspects of the technical vocabulary associated to mineralogy, petrography and soil micromorphology are confusing generating speculation (this will be mentioned in the corresponding section). With respect the Introduction, in the section 3.1 (Metasomatism in Terrae calcis genesis), a more profound revision of the concept of meta-

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somatism is necessary, for example, considering the publications of Putnis (2002), Putnis & Austrheim (2010) and Kondratiuk et al. (2015). In the section two, Genesis of Terra calcis in Franconia (central Germany), is notable the descriptions of the “crack fills”. Many of these soil infillings have been described as pedosediments, pitfalls, even palaeokarst, in tropical environments of Yucatan, Bahamas and Bermuda (e.g. Carew & Mylroie, 1991);at respect, the figure 2, in particular 2a and 2c generates questions about its morphology. The description denotes the homogeneous aspect of the infilling and very abrupt and vertical limits of crack, even its continuity to the deepest part of the profile...could be possible to suggest the presence of a fault plane and fault gouge material? I think is necessary more detail in the geology description of the study sites, including lithology and major structures must be showed in the location map (figure 1). The map must be developed under cartography qualities. The section fourth (methods) denotes the most controversial sections of the paper, unfortunately this affect the results and its discussion: the observation scale. The authors state that micromorphology was applied to observe the clay neof ormation and its relation with carbonate bedrock. That’s inexact, in any case what was used were ultramicroscopy techniques. The preview step to use ultramicroscopy is a medium scale study with thin sections. The authors, apparently, used a thin section to apply EDS, however, they never make reference to the general description of the samples under the petrographic microscope to localize a potential area with isovolumetric replacement. The authors base much of their work in the propose of Merino and Banerjee (2008) which defend the use of petrography descriptions to identify isovolumetric features. Additional to this omission, is the decision to not made petrography of the “unaltered” limestone and describe its components. This aspect is the key to compare with the “altered” sections in the infillings. Another aspect necessary for revision is the decision to concentrate the study of isovolumetric replacement in the microfossils; is well know the different processes associated to authigenic clay minerals in carbonates in diverse sedimentary deposits (e.g. Bristow et al., 2012; Deocampo, 2015; Ifandis et al., 2015). The authors never contrast information about these possibilities in the discussion section. The general

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impression is that the authors pretend to see the same evidence and processes that Merino and Banerjee (2008) in Indiana, biasing and even forcing their results. The section five, the results, are confusing in the descriptions, beginning with the “micro-morphology section”, with descriptions as “clay films”, “allocthonous clays”, “micro-clay beddings” (particularly in the manuscript lines 385-391), vocabulary used without context and not used in the technical terminology of micromorphology. Part of the results is more close to a discussion section, even “conclusions” as shown the lines 391-394. Specially, expressions as “amorphous structure” or “amorphous gel” generates several confusions in the reader: do you mean “low crystallinity” or “low birrefringence” (if you really observe thin sections under petrographic microscope)? I don’t know if try to describe “clays aggregates” or “clay coatings with low birrefringence” or even “isotropic clay coatings”. The paper needs an important revision of vocabulary concerning to soil micromorphology and carbonates petrography. In the section 5.2 (bulk soil analysis), again the paper present a mix of results and discussion; I think is recommendable to prepare just one section with the title of “Results and Discussion” . Is necessary to revise the assertion that v-shape marks on the quartz grains are typical of wind transport (please, check publications as Reineck and Sing (1986) and the recent work of Woronko and Pisarska-JamroÅijy (2016) associated to frost weathering); if you work with “real” micromorphology, probably you can observe some evidences of the sedimentary processes related with the input or transport of the “infilling” in the cracks. A category of Mineralogical results is essential, contrasting the mineral phases in lime-free residue, cracks infillings and soils. The mineralogical interpretation is vague and poor understood; the same is with the major and trace elements, never used in the paper to interpret processes, just a part with the EDS results and Ti/Zr ratios (it is obvious the dissolution effect of these elements in the limestone and its concentration in the soil and infillings). An important question is to know which mineral phases are concentrated in these elements ( zircon, rutile, epidote group minerals, others? ) I think that much of the discussion concerning to Parent Materials (6.1) must be related to mineralogy and geochemistry. The Discussion section denotes weakness in the un-

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derstood of metasomatism concept background. Again, the major question is : do you really have concrete evidence of neoformed clay minerals beyond photomicrographs by SEM and the use of explanations as “clay gel like-material”? Descriptions as “no crystallinity” and/or “amorphous clays” are very ambiguous in the text and scarce informative and discussed. I recommend consulting the publication of Velde and Meunier (2008). The petrographic aspects of the carbonates, specially related to needle calcite forms can be discussed with the help of publications as Verrechia (2011) and his review of pedogenic carbonates. With respect the bulk soil analyses, there is a poor exploitation of the geochemical data (major and traces elements); the paper discuss Ti/Zr ratios without references to the possibility to use elements for provenance studies by comparison with other published data. The loess influence must be traced with the help of REE patterns and the use of Y, Th, La, Zr among others elements in ternary diagrams. Aspects as the speculative presence of “pseudosand structures” and the “shrink-swell” in the soils must be verified by micromorphology in the soil groundmass and its b-fabric.

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