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Interactive comment

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

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Dear reviewer #1, dear colleagues,

thanks very much for your efforts invested in our paper. We agree that some of the terminology should be improved, and that it is a good idea to include the actual results and discussion in a joint chapter "Results and discussion". However, some recommendations seem a little out of the scope of this article. We do neither intend to present a complete review of the literature on Terra rossa genesis (which would require many more pages describing the state of the art), nor a full-scale petrographic investigation of the studied profiles. As well, we do not intend to quantify the contribution of possible individual parent materials such as loess, replacement, or bedrock dissolution, and

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Discussion paper

think this has been made clear in the introduction.

The scope of this paper is intentionally limited to the qualitative reporting and discussion of features in the rock-soil transition zone that might be explained by isovolumetric replacement, and to some pedogenic parameters, in particular the sand content, that might be attributed to deposition of allochthonous material. The cited literature has been limited to the sources that we assume essential for offering a thorough interpretation in the context of the current state of the art. We consider this legitimate since the vast literature on Terra rossa genesis is more or less impossible to summarize in a paper presenting original data – a selection has to be made unless one aims to write a review paper. For example, sources like the mentioned papers by e.g. Putnings and Kondriatuk et al. are very specific. They focus on models of linear kinetics that could provide an analytical expression of chemical mechanisms by which a tight coupling between precipitation and dissolution fronts can arise that lead to volume-preserving replacement. They can be cited, but we consider that not necessary since these contributions deal with very specific details of the possible replacement process that might not really matter for our argumentation.

Similarly, it might currently be impossible to calculate the contributions of various possible parent materials from elemental compositions: since we do not know the extent and origin of possibly neo-formed clay minerals, such statistical estimates must remain to some degree speculative, distract from the qualitative reporting of the observed features, and would greatly extend the length of the paper. This intentional limitation explains our 'poor' use of the available geochemical data. Similarly, we do not agree that the mineralogical analysis of the studied profiles is be needed within our scope. That comparisons of clay mineralogy are of limited use has been showed by recent studies by Sandler et al. (2015) who found that weathering can be reversible and similar clay mineralogies are thus not suited to prove inheritance. Regarding the sand contents, it would certainly be of interest to better understand the mineralogy of the grains for tracing their origin, but already the available data allow for sufficient conclusions in the context of the scope of our paper.

It is possible to provide better maps and microphotographs of the limestone. However, we are not certain whether this matters for the reported evidence. As described in detail in the paper, the possible replacement features were only visible at the rock-soil transition zone, while the limestones consisted of calcite or dolomite with microfossil inclusions. Such a microphotograph might be presented, but then as evidence that there is nothing new to see, and we are not sure how important that would be. In addition, areas of unaltered limestone that border the observed replacement features are already visible in the provided microphotographs.

What is true is that the paper presents only results of ultramicromorphology – a method never applied by Merino and colleagues. As explained above, we focused on the possible replacement features which could only be observed by SEM and EDS and did not present results of micromorphology as the latter provided no new information. We therefore agree that some revision of the terminology and description is indicated. However, the review provides no recommendations on the observed key features, and we consider the question "do you really have concrete evidence of neoformed clay minerals beyond photomicrographs by SEM" as not fully fair. Photomicrographs are the only possible evidence... the difference between the features reported in our paper and the ones observed by Merino and Banerjee (2008) is the use of SEM, and even though the features that we observed might not fully match the theoretical model proposed by Merino and Banerjee (2008), we see no reason to discard them just because they could not be observed by optical micromorphology.

Unfortunately the review does not provide an alternative explanation for the observed features, but is in a way discussing them away by demanding more methods, quotations, and additional data. But the observed features are very similar to the ones observed by Lucke et al. (2012), although that study dealt with soil development in semi-arid Mediterranean climate. One motivation to conduct our research was to check whether similar features can be observed in temperate climates, and we consider the fact that this is the case as already highly relevant for various questions of soil science even though some aspects cannot yet be explained or quantified.

Regarding the suggested map of geological faults and fractures, it is well-possible that the studied limestone cracks are connected to regional fracture systems. But again, the question is whether this matters for the studied clay fill and its rock-soil transition zone. While it is certainly possible that transport of allochthonous material might have proceeded via regional fracture systems, it would be out of the scope of our paper to attempt tracing such movements. Even with a geological map, we cannot prove or disprove whether fault plane and fault gouge material is present. We think not, since there is a strong similarity to the covering lower Terra rossa material and sand grains of possible glacial loess origin, which will be added in the discussion. But even if fault plane material is present, that would not change anything since the questions of its origin and formation remain.

What can be stated is that an increase of sand content by 1300% compared to the bedrock residue, and by 300% compared to the covering loess, is very likely connected with the deposition of allochthonous material, and v-marks on the quartz grains are a strong hint to a glacial origin of such grains (frost weathering would even more associate them with the loess, since aeolian transport might also have occurred in the reef environment where the limestones formed). A standard reference is Mahaney (2002): aeolian transport of the grains is unambigious, and a connection with loess deposition very likely – although, as stated in our discussion, that is only one possible source of the sand and the question of transport mechanism into the cracks remains.

In this context, grains that appear as sand size during bulk soil analysis, which show under the microscope to be hard rounded aggregates of finer particles, are by definition 'pseudosand' – as well as macroscopic prismatic aggregate structures in clay-rich soils are known to result from shrink-swell processes. We are thus not 'speculating' about pseudosand. It is there, and part of the key question how to explain the increase of the sand content compared to the bedrock residue.

With regard to the key evidence of our paper, i.e. the features partially consisting of clay and calcite observed by ultramicromorphology, and regarding the strong increase of sand content, the review did in our opinion not provide substantial suggestions. We therefore agree to improve the terminology and description and unite the results and discussion in one chapter, but would prefer to leave the scope and extent of the paper as it is.

Thanks and best regards Bernhard Lucke, Helga Kemnitz, and Stephan Vitzethum

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