Statement of the referee on Ph.D. Thesis of Mgr. Kateřina Zajícová

THE VARIABILITY OF SOIL ORGANIC CARBON POOL AND THE POTENTIAL OF GROUND PENETRATING RADAR IN ITS ESTIMATING

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The Ph.D. Thesis is based on a set of four publications, which follow each other well thematically and together cover a determined topic. One of the articles represents a review study that provides comprehensive information on the state-of-the-art use of GPR in soil science. Other articles are case studies with their own scientific findings. At the time of thesis submission, three studies were published in respected journals classified on Web of Science within the Q1 category Soil Science and Forestry. These were Geoderma, European Journal of Soil Science and European Journal of Forest Research (Geoderma even currently belongs to the 1st decile of the field). The fourth study was under review at the time of Ph.D. Thesis submission, however, it is now published in the Geoderma. It is clear from the above that the results of the scientific work of Mgr. Kateřina Zajícová underwent a demanding review procedure and was effectively shared with the scientific community.

Mgr. Kateřina Zajícová participated limitedly in the first study published in the European Journal of Soil Science, namely by data processing and helping to write a text. In other studies, however, the Ph.D. student was the first author and played a key role in their creation. I consider the degree of the student's participation in individual studies to be above standard for the purposes of defending the dissertation, her author's share is a significant. Three publications in prestigious journals from the position of a key author are a nice result of doctoral studies. This testifies to its scientific maturity in the given issue.

Formally, the Ph.D. Thesis is processed as an overview of the most important methodological steps and research findings in the length of 52 pages, which are related to the four mentioned articles. These form a separate appendix to the thesis, which refers to them frequently. The dissertation has a uniform format and works consistently, which was probably the main motivation of this organization.

I comment below and *ask questions* about the main sections

Topic

In all articles, I appreciate the nicely structured introduction, which progresses to determination the science gap and outlining the goals of the study. It is clear from the text how research will enrich scientific knowledge. Some of the studies also have a significant methodological contribution. I consider a deeper knowledge of carbon cycle, even with the use of new geophysical procedures, to be a highly urgent topic at a time of ongoing climate change.

Data collection

The thesis is based on a broad database of mainly geochemical and pedological analyzes from 14 forested catchments of the GEOMON network managed by the Czech Geological Survey. I really appreciate such a robust database, as it greatly stabilizes the research of beginning scientists. However, in this context, I would welcome more information on the Ph.D.-student's share of data collection. For example, the first study works with a respectable set of outputs from 112 soil profiles. ***What data did Mgr. Kateřina Zajícová have available and what did she personally collect in the field?***

Data processing

For specific methodological steps, the authors use an adequate apparatus of mathematical models and statistical tests. The use of a linear mixed effect model, geographically weighted regression models, Moran's *I*, or Ordinary Kriging seems entirely appropriate. The chosen procedures are sophisticated, suitable and testify to the team's excellent orientation in working with data and statistical analysis. Sophisticated approaches enabled the separation of individual studied factors and the finding of causal dependencies.

Results and Discussion

In general, I consider the research results of Mgr. Kateřina Zajícová to be very interesting, novel and supported by data.

Article I: Soil properties here are related to the depths, not to soil horizons, as in other articles. I wonder what general recommendation you would make for the quantification of soil organic C. For example, would you recommend to work on horizons basis and even address their uneven boundaries using 3D models, or use rather geochemical approach that favors depth? ***Is it possible to somehow combine the approaches in order to achieve maximum profit from the advantages of both concepts?***

Interesting are the findings about the impact of air pollutions in soil carbon, up to a depth of 40 cm. A significant forest type impact also contains an anthropogenic footprint. The study talks about a carbon balance. ***How dynamic do you think organic matter is in the soil and how fast does it transform between soil horizons? Would the inclusion of humus forms and forms of organic horizons*** (e.g. according to Klinka, Jabiol, Zanella) ***help to better understand the dynamics of organic matter in the soil?***

Article II: The high nugget is, of course, a reflection of the studied spatial level. The minimum distances between the samples seem to be relatively high, on scale of meters or even tens of meters. This skips the important level of the most local spatial variability (e.g. publications by Ponge, Klinka). The application of nested stratified randomized sampling could potentially lead to more effective research on spatial variability.

Article III: In the last two decades, more reviews have targeted the use of geophysical methods and specifically GPR in soil science and geomorphology. Some of these studies are cited in the article. Despite this fact, I consider the new review study to be very interesting, it is clearly organized, readable, informative, it contains a number of very recent reports. This is also evidenced by the fact that the article has been cited 29 times on WoS since 2019.

Article IV: The results are based on a comparison of two regions, not just two soil units. Focus on soil unit would require more replications from multiple regions, for example from places of a higher spatial soil complexity, where there is little heterogeneity of some soil-forming factors (e.g. bedrock). For this reason, I consider the achieved results to be difficult to generalize at the level of soil units. However, this article has significant methodological benefits.

Minor points (response is not required)

Some pedological terms and symbols used in the included studies could be an English translation of the terms from the Czech soil taxonomy (Němeček et al. 2011) rather than part of an international standard, although this is repeatedly referred (IUSS-WRB 2014, 2015). These are, for example, the mark of soil horizons (Ah, Ev), the definition of the A horizon as *organomineral* (instead of upper mineral), the talk of *soil type* (the system does not have this unit) or using of Podzosols. This is not a big mistake, but it can make communication more difficult. I see a clearer possibility of communication as an essential reason for the use of international taxonomies.

I understand the reason why in some studies the humification horizon was merged with the upper mineral horizon A. Personally, however, I do not prefer such a procedure, because it abolishes the basic division into organic and mineral horizons and makes it difficult to compare with studies where there is 0 on the surface of the horizon A. However, this is a decision fully within the competence of the authors of the study.

Kap. 2.2., Tab 1: As a potential driving factor I would mention also a soil disturbance regime and time.

After a successful defense, I gladly recommend to Mgr. Kateřina Zajícová the title of Doctor of Philosophy, Ph.D.

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