

Interactive comment on “Characterization of soil organic matter by near infrared spectroscopy – determination of glomalin in different soils” by J. Zbírál et al.

J. Zbírál et al.

jiri.zbiral@ukzuz.cz

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Reply to RC 2 comments A. General remarks 1) Suitability of NIRS It is true that soils have very broad peaks generally not suitable for assessing individual soil organic matter components. Soils generally have very similar reflectance spectra in the 1100 to 2500 nm range (FIGURE 3 shows that differences can be seen between soils from arable land and forest soils). But even if the absorption peaks for soils in the near-infrared region are difficult to assign to specific chemical components, the spectra still have complex information that can be used for analytical purposes. ISO 17184 clearly shows that even if the individual peaks cannot be assigned to any individual compound the spectral information reflects organic matter content in soil. Glomalin is not defined

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as an individual compound but as an operational parameter GRSP. The situation is exactly the same as if for example fibre is determined by NIRS in feed. 2) GRSP and Cox correlation As Cox is (GRSP+other SOM components) there is really a correlation between the parameters. But this correlation is not so strong that we could say that GRSP is not bringing any new information. We suppose that mainly the ratio GRSP/Cox could be probably valuable qualitative indicator of SOM. But it was not intended to discuss this issue in our paper. Only for your information we add an Excel sheet with regression results as you proposed. It can be seen that we cannot say that GRSP could be easily calculated from Cox. The discussion is maybe for other paper and it was not in the scope of our paper. We intended mainly to propose a new and effective analytical tool for simultaneous determination of Cox and GRPS (and maybe more SOM parameters in future). 3) Results and discussion The discussion part was improved in the section 3.

B. Specific comments 1) In the abstract and the introduction, glomalin is repeatedly described as a promising indicator of SOM quality (or soil carbon changes, page 2 - line 20). Given the substantial difficulty in measuring glomalin, it is not very suitable as an indicator: many other proxies for SOM quality/dynamics exist, which are much easier to obtain. Hence, the authors should clearly indicate the additional value of globalin content for e.g. large scale surveys (page 1 -line 13) as compared to existing indicators. In the references we found that authors suppose that GRSP can be a valuable SOM quality indicator and these citations were used in the Introduction part. In fact it was not our task (at least not in this paper) to show how useful or not GRSP is. However, we assume that the value of this parameter as an indicator of soil quality can rely in its contribution to aggregate stability (and related physical soil properties) which results from slow decomposition of GRPS. Most analytical methods for estimation of various fractions of SOM are rather focused on labile fractions which can be used as early indicators of changes in dynamics of SOM (e.g. water extractable C, light fraction, particulate SOM). We showed that there is a way how to determine this “difficult to measure indicator” in a substantially easier way. And this may facilitate further re-

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search about usefulness of this indicator. We fully agree that GRSP cannot be used instead of other SOM indicators but we suppose that it can be used as an additional parameter. In the large scale surveys any indicator that can be determined by NIRS simultaneously with e.g. Cox or Ctot is extremely valuable because it is very cheap. The possibility of this simultaneous determination was emphasized in the revised text. 2) The rest of the introduction is mainly a methodological state of the art, towards the procedures to be followed to extract and process the GRSP. For a reader not too familiar with the protocol, it is quite confusing. Many problems are listed, but in the end it is concluded that the method is nevertheless valid, yet no strong arguments for that point are provided. We wanted to show that there was some effort to improve extraction and/or measurement step of the former analytical method for GRSP determination but most authors finally agreed that the method published by Wright and Upadhyaya is acceptable. And this method was used in our work as a reference method.

3 (more points aggregated) For NIRS: the introduction is very general. Some critical reflections about the possibilities and limitations of the technique are in order, either in the introduction or in the conclusion - Introduction and materials and methods: There are many types of NIRS, which one did you use (DRIFT?) Section 2.3: specify the type of NIR (diffuse reflected? Fourier transformed? Etc). Much more interesting than the type of software is the type of statistical analysis it performs. Specify further the kind of model that was used and how optimisations were done. Were all wavelengths included in the final model. Accepted. The text will be improved in this way. FT diffuse reflectance was used. 4) Mention which soil classification system you use and use correct terms (e.g. Fluvisol, Cambisol etc is written with a capital in WRB) Accepted, corrected 5) Section 3.3: Not clear, description of the method rather than a result. In fact, the main result of our work was a new analytical method and this method is described in this section. We can put it in the "conclusion" but we suppose that as a final result of our work it should remain in the Result part. 6) Table 1, 2 and 3: why are different soil properties listed in the tables? Sometimes CeS is measured, sometimes Cox and sometimes both Cox and Ctot. I do not see a rationale in it. Also, these results

are not described nor discussed. The available data for arable land and grasslands and for forest soils were different, but it is really confusing therefore we adjusted the tables to have only the same parameters. We did not discuss the parameters. They were shown only to support part 2.1 where we stated that the sample set covered wide range of different soils. The text was corrected.

7) Two observations clearly have a higher value for both the measurements and the prediction, with a big gap between them and the rest. How good is your model if you repeat it without these two observations? Also, indicate what the line depicts. A regression plotted between the two? Indicate an R2 value if this is the case. Idem for figure 2.

Accepted and corrected. Calculation was done and the results are discussed in the Part 3. 8) Why the difference in baseline? 1 sample or an average? Specify We do not fully understand the first part of the comment. In Figure 3 there are examples of a typical soil spectra for a soil from arable land and a forest soil. It is an example (1 soil). Description of the Figure was changed to be clear.

C. Technical comments The UKZUZ is a national institute, this should be specified more clearly – We do not fully understand this comment. Page 4, lines 19-25: structure is confusing as first it seems that there should be 92 samples in table 1, etc. The total number of soils from arable land and grasslands was 92, Out of them 84 (Table 1) was chosen for calibration of the NIRS instrument and the remaining 8 (Table 2) was used for external validation. The total number of forest soils was 81. 75 for calibration of NIRS (Table 1) and 6 for external validation (Table 3).

- In the results section, you often mention using “a reference method” – do you mean the one specified in the materials and methods section? Then clearly indicate so. – Yes, it is the method described in the part 2.2 “Reference method.” The text was changed to avoid confusion.

Many typo's regarding units – consistently leave a space between the value and the

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unit and do not leave spaces in values (e.g. page 6 - line 5; 2500 in stead of 2 500
The space was deleted (we have found only this one). We cannot see any space that
should be added between number and unit. Only in page.5, line 10, but there is 30ml
as an adjective and we are not sure if a space is possible in this case.

Please also note the supplement to this comment:

<http://www.soil-discuss.net/soil-2016-9/soil-2016-9-AC2-supplement.pdf>

Interactive comment on SOIL Discuss., doi:10.5194/soil-2016-9, 2016.

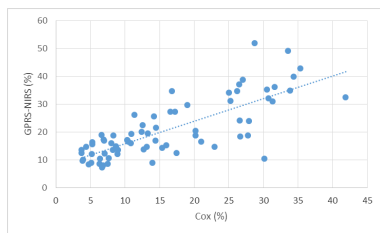
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Relationship between Cox and GRSP.

Fig. 1. reply to RC 2 comments - Cox vs. GRSP