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Geoscientific Instrumentation Methods and Data Systems Discussions

Interactive comment on "Non-destructive evaluation of moisture content in wood by using Ground Penetrating Radar" *by* Hamza Reci et al.

Anonymous Referee #2

Received and published: 9 September 2016

This is an interesting experiment, which includes a brief introduction and a theoretical background of GPR followed by laboratory measurements and a brief summary. The whole paper gives the impression of a very quick and not very consistent work. From the beginning to the end there are many more or less serious inaccuracies. Considering those facts, the comments below, missing satisfactory explanations of the results, and a missing novelty of the paper, I regret not to recommend this paper for publishing.

Specifically: English should be improved (ideally checked by a native speaker).

GPR image of some measurement would be welcome. In this paper, we can see only A-scans.

In Table I there are no symbols (εr) and units ([-]) for the relative permittivity.

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In Figure 4 we can see a variable R (R2=0.9028, R2=0.9981) without any description of its meaning. Is it supposed to be a measure of uncertainty/goodness of fit (sometimes also denoted as ˆ2)?

Captions and axis labels in figures should be of the same size and should start with a capital letter.

Brackets in all axis labels should be unified.

The captions in figures should be unified with the text, sometimes the important information is only in the captions and sometimes only in the text.

Not many measurements were done and so the data cannot be considered as decisive.

Page1: Line 26: In this paper, there is a sentence “For example, it is known that the minimum value of moisture content for the development of wood degrading fungi is 17% by mass, whereas the optimum values range from 30% to 70% depending on the fungi and wood type (Mai et al., 2015).” which was basically copied from the cited article of Mai et al., 2015. Unfortunately, this statement is not proved by any reference in that paper. Could the authors back up those statements? Myself, I found different values of moisture which are optimal for fungi development. The formulation “it is known” is then not very well chosen in this case. The authors claim, that the moisture content is the most common cause of wood deterioration. Do they know other factors influencing biological degradation of wood caused by fungi and insects? Out of curiosity, do the authors know, how can be the moisture of wood changed by the fungi attack?

Page 3: Line 1: Speed of light in vacuum is approximately equal to 0.3 m/ns (more precisely 299 792 458 m/s), it is not equal to 0.3 m/ns and it is also not speed of light in the air (On Earth, Mars, or Titan?).

Page 3: Line 3: There is a mismatch in terminology. Symbol εr is the relative permittivity (called also as the dielectric constant), it is confusing to call it “dielectric relative permittivity” or “dielectric permittivity” in the line 13 of the same page, etc.

Page 3: Line 6: There is, again, a mismatch in terminology. Symbol ε is the permittivity or absolute permittivity, it is not "the dielectric permittivity" claimed by authors.

Page 3: Line 6: The vacuum permittivity $\varepsilon 0$ (or electric constant, or permittivity of free space) is not equal but is approximately equal to 8.854187*10-12 F/m. Nevertheless, I assume that the "ε0=8.854*1012 F/m" is just a typo.

Page 3: Line 7: The authors claim, that wood is a low-lossy medium. Is it true for all humidity levels of wood? Is it also true for living trees? In what range do the authors define low-lossy media and what are high-lossy media according to them?

Page 3: Line 16: The Equation (2) does not seem to be correct. Furthermore, it is missing in the reference given by authors, Neal, 2004. Could authors provide more information about the equation derivation?

Page 3: Line 27: The authors chose the initial humidity level equal to 12%. Why did they choose this value of humidity? How did they measure that?

Page 3: Line 30: The Equation (3) given by Moron et al., 2016 is strictly determined for samples in the anhydrous state for W0. In spite of that, authors used the sample with the humidity level of 12%, which is not correct and could (and probably did) highly influence their further data.

Page 4: Line 2: "The weight of the sample is measured with a balance having a sensitivity to the gram." It is a useless information if we do not know the mass of the measured sample in the anhydrous state. For example, if the sample has 3 grams, such a measurement will be entirely insufficient.

Page 4. Line 4: "...an almost curvilinear increase occurs." The authors do not have enough data points to say anything about the shape of the curve. It appears to me more like linear.

Page 4. Line 20: In the Equation (4), v0 is not defined.

Page 4. Line26: It would be much clearer if the authors provided a simple drawing of the disposition of the measurement. It is not immediately obvious what dR, dTR, and h mean.

Page 4. Line29: If the Equations (4) and (5) are exact, then the Equation (7) is exact as well. There is equality, not estimation.

Page 4. Line29: Why the velocity v in Equation (7) has a different style than previously (italic, non-italic)?

Page 5. Line1: There is a note about the humidity level of the sample “ranging from 12% to 64.65%” which is incomprehensibly precise. The humidity levels are calculated from a hydrated sample and then, the humidity is basically estimated and precision like that is not required.

Page 5. Line6: Could authors explain why only the reflected wave with the parallel polarization shows such a different increasing of the relative permittivity than the others?

Page 5. Line 8: “The increase of relative permittivity versus moisture content is piecewise linear, with a slope change occurring when the humidity level is about 18%.” This statement is based on single measurement when the humidity was around 18%. There is no measurement error on the point, the measurement at 18% can be just a small upward fluctuation.

Page 5. Line 8 and 9: ”Moisture content is piecewise linear.” Of course, it is piecewise linear, if the graph is made by connecting data points from isolated measurements. Why did the authors not make a fit instead of connecting points?

Page 5. Line 19: ”Apparently, the propagation paths are similar in the two cases.” Do the authors have some explanation, why is it like that? Did they try to adapt their equations for two cases?

Page 5. Line 25: The authors claim that the last possible humidity level which can be measured is 43%. Did the authors try the measurement with the humidity of 44% or

45%? Or did they just tested 43% as the last point where it worked, and then 52% and it did not work?

Page 5. Line 32: Why did the authors mix the distances of the measurement in Figure 7? We cannot really compare two different measurements when the distance (and so the attenuation) is greater.

Page 6. Line 3: ”. . .this may be due to a superposition of direct and direct air waves.” Did the authors try other calculations or simulation to fully describe the phenomenon? Why does it not occur in the other case? Do they have any explanation why does the amplitude firstly increase and then decrease?

Page 6. Line 25: The authors claim that the measurement is effective to estimate the permittivity behavior regardless of different results by two different techniques (especially the reflective wave method). Do they have some error tolerance for the permittivity determination?

Please also note the supplement to this comment:
http://www.geosci-instrum-method-data-syst-discuss.net/gi-2016-24/gi-2016-24-RC2-supplement.pdf

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