

Interactive comment on “Apsu: a wireless multichannel receiver system for surface-NMR groundwater investigations” by Lichao Liu et al.

PhD Irons (Referee)

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The authors present a fairly comprehensive description of a new modular surface NMR receiving instrument. The wireless nature of the system is novel and should open up survey design. Additionally, a more open instrument description is welcome compared to commercial ‘black-box’ systems. As such, this paper presents a relevant and important contribution to the surface NMR literature.

1. *Does the paper address relevant scientific questions within the scope of GI? Yes, the topic is very relevant to Geoscientific Instrumentation.*
2. *Does the paper present novel concepts, ideas, tools, or data? Yes, this paper*

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- describes the first wireless distributed receiver sensor network for surface NMR surveys.
3. *Are substantial conclusions reached?* Yes, the authors have successfully designed, built, and deployed a seemingly field-rugged system utilizing GPS timing synchronisation. This is no small feat. Bravo!
 4. *Are the scientific methods and assumptions valid and clearly outlined?* Yes, the system is described in a fair degree of detail. A better description or citation of differential coils should be included. I would also like to see a description of the power requirements of the receivers, and how long data collection can be performed on a single charge.
 5. *Are the results sufficient to support the interpretations and conclusions?* The authors claim to have improved upon the SNR of the measurements. However, the field example does not demonstrate a reduced noise floor compared to other available instrumentation. If the authors should substantiate this statement, or remove it from the manuscript.
 6. *Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?* This is a tricky question. This manuscript would not be sufficient to replicate their design. However, the authors do not elude to this being an open-source design. That said, the description is sufficient for *users* of the instrument to gain a much better understanding of system noise, response, and related issues. This information is invaluable for calibration, as such, I find this aspect of the paper acceptable and beyond what many instrument manufacturers provide.
 7. *Do the authors give proper credit to related work and clearly indicate their own new/original contribution?* On page 2, line 10 a description of the first surface

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NMR instruments cites Legchenko and Valla, 2002 which describes the Iris NUMIS. However, the first surface NMR instrument was the Hydroscope:

```
@INPROCEEDINGS{Semenov1987,  
  author = {A. G. Semenov},  
  title = {{NMR} hydroscope for water prospecting},  
  booktitle = {Expanded Abstracts},  
  year = {1987},  
  pages = {66--67},  
  organization = {Indian geophysical Union},  
  note = {Proceedings of the Seminar on Geotomography, Hyderabad}  
}
```

In this circumstance, it would be appropriate to cite the first instrument in addition to the NUMIS.

8. *Does the title clearly reflect the contents of the paper?* It does, however the acronym(?) 'Apsu' is never defined. If it has some sort of meaning, please define it in the copy somewhere.
9. *Does the abstract provide a concise and complete summary?* It does, however the discussion of SNR improvements either need to be substantiated or removed from the abstract as well.
10. *Is the overall presentation well structured and clear?* The paper is well structured, with the exception of §4.3 §4.4 and §5. The dead time discussion and (to some extent) field noise synthetics follows from the field examples. It would be more clear to introduce the field cites as e.g. 'Field Validations' with subsections dedicated to dead time realizations and noise synthetics. As it stands Schillerslage is introduced twice and Silkeborg once. If the authors want to keep the Silkeborg examples in §4 that would be fine, but I would still recommend moving the

dead time to §5 with a new §5.2 describing the data comparisons. If the phase is presented (discussed below), this could be a separate section as well.

11. *Is the language fluent and precise?* The manuscript is well written. On page 10 line 5 a trailing apostrophe (') is used where a leading apostrophe (‘) should be.
12. *Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?* Yes.
13. *Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?* See above discussion of §4 and §5.
14. *Are the number and quality of references appropriate?* While the use of GPS timing is novel in surface NMR, it is common in the MT/CSEM community. For example the Zonge Zen system. A citation of this prior art would be appropriate and also affirm that GPS timing can reliably be used. Seismic nodal systems also use GPS timing and can be cited.
15. *Is the amount and quality of supplementary material appropriate?* N/A

In addition to the points above, I offer a few additional suggestions for consideration.

- The use of the word ‘identical’ on P. 3 line 5 to describe the recorded NMR signals should be avoided. This description gives the impression that the two signals have no discernable measure between. ‘Practically equivalent’ or some similar verbiage would be preferable.
- The jet colourmap in figure 11 should be replaced with a perceptually uniform one. Additionally, the colormap clips at 0, but the quadrature detection should result in negative values as well. A diverging colormap centred around 0 is highly encouraged for the top two subfigures.

- Complex inversion is an important consideration in surface NMR, especially with separated transmitters and receivers. Data phase comparisons (or real/imaginary plots) with the GMR are highly encouraged and will confirm that the developed instrumentation is at the 'bleeding edge' of surface NMR instrumentation.

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