Nonlin. Processes Geophys. Discuss.,
https://doi.org/10.5194/npg-2020-8-RC1, 2020

Nonlinear Processes in Geophysics
Discussions
Open Access

Interactive comment on "An enhanced correlation identification algorithm and its application on spread spectrum induced polarization data" *by* Siming He et al.

Anonymous Referee #1

Received and published: 14 September 2020

The manuscript titled "An enhanced correlation identification algorithm and its application on spread spectrum induced polarization data" by authors He et al discusses a new statistical method for noise suppression of electrical IP data, captured in the time-domain, but analyzed in the frequency domain.

While I'm familiar with spectral induced polarization methods, I have no first-hand experience with the discussed spread-spectrum induced polarization methods. Therefore my comments should be interpreted as either real issues to be dealt with, or (hopefully the case) just misunderstandings on my side (for which I apologize). In any case, I think a thorough rework of the _representation_ of the manuscript can remediate all

issues.

My major concern with the manuscript is the total lack of multi-frequency complex data, be it in the form of resistivity magnitude and phase values, or real and imaginary parts (spectra). My understanding of the method is that this is exactly the goal here: Capture multi-frequency complex impedance data using very fast time-domain-alike injection/measurement schemes. As such I strongly suggest to:

1) show extracted measured spectra (i.e., the resulting "data" after noise removal and conversion to the frequency domain): |R(f)|, Phase(f)

2) show phase results for the inversion results (if not easily included into the main text, additional magnitude/phase results for different frequencies could be supplied in a supplement)

From an outside view this is important to actually judge the method and the achievements presented in this study.

Other comments:

* Please employ a proper notation for complex entities. From my point of view most equations contain complex values.

* page 4,line 17: "ZW-CMDSII" not defined - please provide a reference here as this seems to be related to the construction of a measurement system?

* page 4/ECI approach: My understanding of the main point of the ECI approach is that you assume uncorrelated noise for applied voltage (n1), measured voltage (n2), and measured injected current (n3) (otherwise the cross-correlations between those quantities shouldn't be zero, as stated in page 4, line 19). If this reading of mine is correct, I would like to see more discussion on this: Is this always the case? What about electronic noise (e.g., from the ADCs involved) - shouldn't this superimpose on all three noise components? I understand that this is probably not an issue here, but it would be nice if you could guide the reader into the right direction. Also, this main

assumption should be more prominently presented in your manuscript.

* Is NT specified? What interval was used for the synthetic and the experimental cases?

* Fig 4: i(t) is the injected current, if I'm not mistaken. As such it should have a unit of Ampere, not Volts (same with the corresponding noise n3). If i(t) somehow has units of volts, please explain correspondingly (also in the previous text passages).

* Synthetic case: It is not clear to me why you only add noise to the injected current and not to the measured voltage (n2) and the applied voltage (n1). Isn't that the whole point of your study? It would be nice if you could show that also for the synthetic case the cross-correlation of those noise components reduces to zero (p4, line19). I suppose this entails generating suitably uncorrelated random ensembles.

While I think this step was taken to simplify the discussion, I think the simplification does not represent the problem at hand. As you stated in page 4, you also expect significant noise levels in n2 ("n2(t) and n3(t) may possess more massive energy..."), you should at least add suitable noise levels to n2 to test you algorithm. I still wonder why the current measurement entails such large noise components, given that this measurement is usually just a voltage measurement over a shunt resistor...

* Eq. 14/15: Is M defined in the text?

* Inversion results (Fig. 9: You do not discuss any error parameterization. However, I think this is crucial here in order to properly judge and understand the results: Did you account for the remaining noise components in each of the three subplots differently, or did you assume similar data noise estimates for the inversions? What were the final RMS values?

* Please use the same colorbar limits for all plots in Fig. 9. Otherwise a proper comparison is not possible.

* It would be nice if you could conduct a residual analysis of the inversions, comparing

the forward response of the final inversion model to the actual data. Does this analysis also follow the observed noise levels?

* Just to be sure (and perhaps encourage a slight extension of the last paragraph of the introduction to better clarify): The major point of this manuscript is that it takes into account also noise from the current measurement, which is not commonly done, right? For example Liu et al 2017, (10.1190/GEO2016-0109.1) seem to only assume noise on the primary potential measurements (The geophysics-paper also nicely shows pseudosections of both magnitude and phase - this would also be interesting here).

In conclusion, I suggest to improve the presentation of the manuscript and to better work out the novel contribution of the ECI algorithm in comparison to the various other correlation-based noise-reduction algorithms out there, as well as to make sure your test cases compare to those of other studies (i.e., current and voltage noise).

Looking forward to reading the published paper!

Best regards

Interactive comment on Nonlin. Processes Geophys. Discuss., https://doi.org/10.5194/npg-2020-8, 2020.