Reviewer Report

Title: FDTD analysis of ELF radio wave propagation in the spherical Earth-ionosphere waveguide and its validation based on analytical solutions
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Gerneral comments

Judjing from the Title, the manuscript must be close to the paper [1] where the 2DTE snd the FDTD soultions were compared. However, this is not so. The manusript title is misleading since the text contains minor TD information and predominantly the approximate expressions in the FD.

It seems that authors do not not know of the monographs on Schumann resonance (SR), the chapters in handbooks, or recent publications in the field [2 – 7]. Content of these works is relevant to the manuscript objective, therore, comparison and discussion are necessary of similarities and departures from the published model results.

The major problem with the manuscript is the declared time domain solution while the dominant protion of material is presented in the frequency domain. It is unclear what the time domain results look like (these must be similar to waveforms shown in paper [7] or to the pulses presented in monographs [2, 3]). A reader is not imformed of how the the TD data were transformed into the specific parameters in the FD. This specific relevance is realy interesting when the solution is constructed directly in the time domain.

Particular remarks and comments

The particular drawbacks that should be addressed in revisdion are obviously seen in the Discussion of the manusript. I list the points in the correct way.

1) The complex characteristic heights being the functions of the signal frequency are discussed in the manuscript. These are used when describing the ELF radio wave propagation in a uniform 3D Earth – ionosphere cavity. Within such cavity, the field components at a particular frequency are the functions of only **single** variable – the distance θ between the source and the observer. The problem is a 3D one, and its solution is the function of a single variable. The term ‘axially symmetric cavity’ is irrelevant and actually misleading in the case. Formulas (9) and (10) are similar to the exact relations obtained in the full wave solution; however, the method remains unclear of their obtaining in the time domain.

Equations (11) and (12) actually repeat the results formulated by Greifingers (1978) and mentioned by Madden and Thompson (1965) [8]. However, Greifingers were much more cautious than the autors of this manuscript: the heights were obtained numerically using the full wave solution while the formulae were suggested for the physical interpretation of numerical results. Greifingers used the term ‘approximate’ for these equations.

There is only one analytical solution in the theory of Schumann resonance, and this is the Schumann formula for the eigen-frequencies of an ideal cavity. The resonant frequencies of a real Earth – ionosphere cavity are complex, and these are always obtained as a result of calculations. There are no closed rigorous analytical expressions for the complex resonant frequencies. We must note here that the Q - factor of Schumann resonance oscillations ranges from ~4 at the first mode to ~6 at the fifth mode, see, e.g., Madden and Thompson [8]. When discussing measurements, one turns to the peak frequencies. These are the frequencies of the maxima in the spectral density of ELF radio signal. The peak frequencies are, of course, related to the resonance frequencies, but their value depends also on the source spectrum, the source-observer distance (SOD), and on the local interferences at the observatory. The clear partition of these terms is absent in the text thus leading to confusion and misunderstanding.

2) All the stuff above is well-known, and it is widely used in literature on sub-ionospheric ELF radio wave propagation. In this context, Eq.(16) of the manuscript is a replica of the explicit formal solution for the ELF radio propagation problem. I reproduce below the zonal harmonic series representation (ZHSR) for the fields from Chapter 14 of [3]:

$$E_r(\omega) = -\frac{i\nu(\nu+1)M_C(\omega)}{4\pi a^2 h\varepsilon_0\omega}\sum_{n=0}^{\infty}\frac{(2n+1)P_n(\cos\theta)}{n(n+1)-\nu(\nu+1)} \tag{14.12}$$

$$H_\varphi(\omega) = \frac{M_C(\omega)}{4\pi ha}\sum_{n=1}^{\infty}\frac{(2n+1)P_n^1(\cos\theta)}{n(n+1)-\nu(\nu+1)} \tag{14.13}$$

The notaion is familiar to authors. Application of ZHSR is described there for estimating the SOD, source spectrum, and the ELF propagation constant. The FORTRAN listing for computing the ZHSR fields is given in the same chapter of [3].
Authors should demonstrate the relvance of their Eq.(16) to the above rigorous field expansions.

It is clear from the above formula that the antipodal SOD is “absent” since $P_n(\cos\theta)| = (-1)^n$ when $\theta=\pi$.
The other approach is commonly used for comparing the model dispersion functions $\nu(f)$. It implies the formula for the power spectrum relevant to the field sources uniformly distributed over the globe: $|E_r(\omega)|^2 \propto \left|\frac{\nu(\nu+1)}{\omega}\right|^2 |M_C(\omega)|^2 \sum_{n=0}^{\infty}\frac{(2n+1)}{|n(n+1)-\nu(\nu+1)|^2}$. In particular, this formula was used in the Kudintseva et al. (2016) paper cited in the manuscript.

3) Equation (15) of the manuscript is a real puzzle. Authors state that they use “…electric and magnetic altitudes from FDTD model by equations (9), (10)…” In what a way the functions of frequency might be used in the time domain model?
This looks odd to me, especially, as Eq.(15) excludes the field focusing effect in a spherical cavity, which becomes especially apparent in the TD pulsed amplitude, see the last figure in paper [9].

When the wide-band signals are treated, the TD waveforms might become really complcated, see the recent paper [7] on the tweeks and the slow tails sferics.

Let us leave the Discussion section of manuscript and turn to a few particular places of the text.

- Parametrs of ELF radio propagation in the uniform isotropic cavity are insensitive to the air condictivity above the 100–110 km altitude. The full wave solution indicated that the contribution from the upper layers is less than 10^{-7}.

  Authors must indicate for what a purpose the profile was extended in height and what was the effect of this extension.
- It was shown in series of papers that the heuristic predetermined frequency dependence of characteristic heights does not correspond to a profile implying the particular heights, the conductivities and the scale heighgts [1, 10 – 14]. The full wave solution for such profiles provides the complex electric and magnetic heights departing from those postulated in the heuristic formulas.

  Do the data of Fig. 2 and Fig 3 support these results or not?
- Spectral resonance characteristics shown in the Tables raise doubts and make a reader suspicious. The resonance frequencies there are shown with an accuracy of about 10^{-3} Hz. To reach such a resolution, the TD duration must be about 1000 seconds or more than 10 min. Using of such duration is unbelievable in the TD computations.

  In addition, the SR Q-factors are equal to 4 – 6, so that the half-power width of resonance curves is about 2 Hz. How the 10^{-3} Hz resolution was obtained while the resonance curve is so wide?
- The color lines in Fig. 4 show the distance variations of the peak SR frequencies. Such curves were demonstrated for the first time by J. Galejs, and one may found them in his monograph of 1972. These variations are driven by overlapping adjacent modes. Emergence of the discontinuity in Fig. 4a is explained in detail in book [2]. Distance variations of peak frequencies, similar to Fig. 4a and Fig. 4b were used for deducing parameters of the global thunderstorm activity from the SR records. I refer only to the recent work [15] exploiting the long-term SR data from the Antarctic and Arctic observatories.

  It is not a surprise that frequency variations in Fig. 4 look quite realistic. The only question remains: does the specific FDTD data match with the known results obtained for the same conductivity profile?

The last but not the least. Authors casually mention in the text the comparing of their data with the perfect cavity model.
I doubt the prefect cavity was treated in the time domain owing to severe problems arising even in a cavity with the small finite losses, see paper [16].
In case I am wrong and authors succeed in performing TD computations for a perfect cavity, I advise them to plot the pulsed successions at the points of the source and of the source antipode.
Such a revolutionary result, if any, is worth of a separate publication.

I outlined the most obvious drawbacks of the manuscript and suggest a serious revision. The time domain and the frequency domain parts should be combined in the manuscript and treated from the unified point of view.
Since it is already known that the FDTD approach might be applied in SR modeling, the major attention should be directed to particular details. It is interesting to what an extent the novel data confirm or deviate from the published results. What are the benefits and disadvantages of formulated approach, etc.? Special attention must be paid to illustrations, which should convince a reader in the consistency of final results and conclusions.

I do not see any reason for hiding my name and I wish the authors good luck in their work.
Yours,
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