After looking through the text and through the replies to both reviews, I admit that considerable work has been done. Now, the paper could be sent for publication in its present form.

Still, I cannot help myself from a few remarks.

I had much fun when reading the materials. The major problem of authors is a poor knowledge of SR literature in spite they mention that were involved in the field for the long time. This is why they state that Greifingers (1978) used the exponential model while Greifingers applied the Pierce and Cole profile in their computations/. They introduced the both characteristic heights by using the localized approximating exponential sections in the vicinity of these two areas of high power losses in the ionosphere.

Authors apply the two layer model and do not mention that such a model was suggested by David Jones in 1964 who had found its parameters from the SR observations.

There are some odd results passed unnoticed by authors. I will mention only two of them.

Manuscript states that the conductivity profile must be postulated at heights up to a few hundreds of kilometers. Simultaneously, the left frame in Fig. 4 clearly indicates that the upper characteristic height never reaches the 100 km altitude. However, this contradiction does not bother the authors.

In the revised manuscript, the TD solutions are shown. The outline of pulses computed for the considerable source – observer distances looks rather good as well as corresponding spectra, provided that we do not go into details and compare these with observations. The real problem is hidden in the left frames of Fig. 1 where the ideal cavity is treated. I would drive attention of authors to the paper [Nickolaenko, A. P., and M. Hayakawa (2014), Spectra and waveforms of ELF transients in the Earth-ionosphere cavity with small losses, Radio Sci., 49, doi:10.1002/2013RS005281] and indicate that outline of their spectra has nothing in common with that shown in this work. It is interesting in this connection, in what a way the spectra were obtained at all? The amplitudes are infinite at the resonance frequencies by definition, and I cannot even imagine how one could initially compute the waveforms in a perfect cavity. Certainly, Figure 1 in the Response to the second Reviewer is incorrect. Look at the plot in the middle of it. The first pulse is the direct wave from a positive stroke. The second pulse must be the antipodal wave, however, what had happened to the waveform? The third pulse is the first round-the-world wave, but why the initially positive stroke had turned into the negative one?

A separate question remains, in what a way the high frequency resolution was obtained in the work while the temporal realizations do not exceed 0.2 s? One must compute the TD record of 10 s duration to have the 0.1 Hz resolution. To what an extent the pulse amplitude will be reduced in this case?

Nevertheless, I will not repeat my comments of the first review. Some of these were taken into account. The above mentioned facts tell a specialist that there are serious unresolved problems in the approach applied in the work.

I think that authors have done the best they could, so that paper might be published in its present form.

Yours sincerely,
A.P. Nickolaenko