

Ann. Geophys. Discuss., referee comment RC2
<https://doi.org/10.5194/angeo-2020-68-RC2>, 2021
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Comment on **angeo-2020-68**

Anonymous Referee #2

Referee comment on "Simulation of Gravity Wave D-region disturbance and its effect on the LWPC simulated VLF signal" by Abdellatif Benchafla et al., Ann. Geophys. Discuss., <https://doi.org/10.5194/angeo-2020-68-RC2>, 2021

Summary:

This paper describes simulations of the D-region ionosphere response to a simulated gravity wave perturbation, and with the VLF response via simulation of the sub-ionospheric VLF signal. Results show that the imposed disturbance modifies both the neutral density and electron density, and specifically, when the neutral density increases, the electron density decreases, and vice versa. The VLF signal response is then simulated, and shows measurable changes in amplitude and phase of about 0.5 dB in amplitude and 5-10 degrees of phase.

Overall Impression:

I do not feel that this paper is sufficient for publication in Annales Geophysicae. First, the paper simply does not provide a significant result worthy of publication. Second, what is described in the paper has some serious issues or missing information. One of the main issues is that the neutral density and electron density are negative in Figures 2 and 3, which is not physical and therefore highly suspect. There are various other issues that I comment on below.

On the question of significance, the paper provides simulations and model predictions of the D-region response to a gravity wave, and the VLF signal response. But these simulations are not grounded in any real data. There are no examples provided of VLF

observations of gravity waves to compare the simulations to. The gravity wave perturbation that is imposed on the D-region (Equation 1) is not backed up with any data. In fact, there is no simulation of the gravity wave from the ground to the D-region, similar to Marshall and Snively (2015); here, the disturbance is imposed directly at 80-90 km altitude. Without justification of the size, shape, and evolution of this disturbance, it is not really a believable gravity wave. I will address these and other issues in more detail in the line-by-line comments below.

Line-by-line Comments:

Line 13: the "ionization rate" should not be affected by chemistry. The ionization rate is q , also called production, which at nighttime is just the cosmic ray ionization source. I think you mean "electron density" here.

Line 21: the ellipsis makes it seem like this sentence is incomplete. Ellipses are not used in formal writing. Same on line 31.

Line 25: The citation to NaitAmor (2010) is fine, but there are many more earlier papers that show similar results. It is a disservice to previous authors to leave those out and suggest that NaitAmor (2010) is the first to show the geometry of VLF perturbations. See numerous papers by the Stanford VLF group on early/fast and LEP events going back to the early 1990s.

Line 28: Similarly, it has long been known that an increase in N_e below the reflection height will lead a lower reflection height. This paper did not discover this phenomenon.

Line 29: "people are attracted" - this statement is unsubstantiated; references are needed to justify this claim.

Line 35: Of critical importance, Marshall and Snively did not just conduct simulations, but used simulations to explain real data of a VLF perturbation associated with a thunderstorm. This is much more powerful than simulation alone, without data.

Line 42: It is not clear which paper the "wavelet analysis" is referring to.

Line 45: Why is the ambient reflection height of 87 km chosen?

Line 55-56: Equation 1 describes the disturbance to the D-region at 80-90 km altitude. The equation is similar (nearly identical) to the gravity wave simulated in Marshall and Snively; however, in that paper, the disturbance was at 10 km, where the storm occurs. What is the justification for the same analytical form of the disturbance at 80-90 km altitude? Most simulations of gravity waves show considerably different structure once the disturbance reaches the mesosphere. See numerous papers by Snively, Heale, and many others.

Line 58: How is the wind speed imposed, mathematically? I would expect this to be incorporated into equation 1.

Line 65: Most uses of the GPI model now use the 5-species version, updated by Lehtinen and Inan (2007), which includes heavy negative ions. The authors should justify the use of the four-species model, which is somewhat outdated.

Line 100-101: The LWPC simulation setup is not clear. Is the new reference height, h' , used everywhere along the LWPC path, or only in the disturbance region? Only the latter makes physical sense, but it's not clear from the paper.

Line 110-112: It makes sense that as neutral density increases, collisions increase, and therefore electron loss by recombination goes up, reducing electron density. This will track the gravity wave disturbance if the gravity wave variation (frequency ω) is slow compared to the electron recombination time. The paper should compare these two timescales.

Figure 1: The top panel should show a map, or at the very least a scale in km. No dimensions are given.

The second and third panels show variations in the neutral density which at some times become negative. How can the neutral density be negative?? From the figures and the text, it seems clear that this plot shows the neutral density and not the change in neutral density; so it makes no physical sense to be negative. This makes the entire simulation seem unbelievable, since this neutral density is critical to the entire paper.

Figure 2: Again, the electron density becomes negative at various points. The chemistry model should even be able to output negative values!

Figure 3: h' values are good to show vs time and distance, but the authors should also show changes to β . The D-region electron density disturbance can be fit to give a best estimate of β near the reflection height, which would be valuable information.

Figure 5: the authors should comment on the 360-degree phase difference after 2400 km. This likely requires a phase-wrapping correction.