Annales Geophysicae Discussions

Ann. Geophys. Discuss., referee comment RC1
https://doi.org/10.5194/angeo-2022-15-RC1, 2022

Comment on angeo-2022-15

Anonymous Referee #1

Referee comment on "Ionospheric effects of the 5–6 January 2019 eclipse over the People's Republic of China: results from oblique sounding" by Leonid F. Chernogor et al., Ann. Geophys. Discuss., https://doi.org/10.5194/angeo-2022-15-RC1, 2022

General comments:

This paper presents Doppler spectra from oblique ionospheric soundings over various distances during a solar eclipse, as well as some theoretical calculation to derive the electron density decrease from the data. This is interesting work and, to the best of my knowledge, not published before. Thus, it should certainly be acceptable for publication in this journal. There are only a few points which I thin the authors should address a little more carefully:

Specific comments:

1) There seems to be some confusion throughout the paper on what altitudes are involved. The general background description seems to be considering the eclipse at sea level (e.g. Figure 3 is valid for sea-level). But, In the caption to Figure 4 it is mentioned that the start and end of the eclipse are taken at 100 km altitude (which I suppose is also true in the subsequent, similar figures). On the other hand, the sunrise indicator in Figure 4 apparently is at sea-level, which is not so relevant for the ionosphere. It would make more sense to consider what is going on at the altitude of the reflection of the signals (an eclipse can progress very different and various altitudes, see e.g. doi:10.1029/2020JA028088).

In this regard, it should also be considered that the various oblique links used are of different lengths, and operate with different frequencies (as described in Table 2). Thus, some oblique reflections might come from different altitudes than others. This could be a possible explanation why some links show less clear effects than others. It would be useful to have reflection heights listed in Table 2 as well (even if simply those expected from for instance IRI climatology; the reflection altitudes during the eclipse could be affected by the event itself).

2) Further concerning the geometry of the eclipse, Table 3 lists the obscuration, magnitude and timing of the eclipse, again at 100 km altitude. However, these are presumably the obscuration in the visible light spectrum. For the ionosphere, the UV part of the spectrum is more relevant. Since the corona contributes a significant part of the solar EUV radiation, the values in Table 3 are not the most appropriate (see e.g. doi:10.1029/2017GL076771).

3) Finally, a comment concerning the introduction section. Between lines 70 and 105, a huge amount of references are given to earlier research into the ionospheric effects of solar eclipses. Obviously many such publications exist, but it is not clear what the precise relevance of each reference is to the current work. This paper is not intended to be a systematic literature review, so only those papers should be cited which are directly relevant for this work (. It should also be explained why they are relevant: e.g. in line 72 it is stated that certain papers should be noted, but why exactly are they relevant here?

A few small technical comments:

1) line 67: geospacer □ geospace

2) line 507: the maximum Doppler shift in Figure 8 appears to be over 0.5 Hz, rather than ~0.4 Hz

3) line 526: there is a deleted word “a” left here, which should read “the” instead.