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Interactive comment on "Climatology of intermediate descending layers (150 km) over the equatorial and low latitude regions of Brazil during the deep solar minimum of 2009" *by* Ângela M. Santos et al.

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Response letter to Reviewer #2

Reviewer comments on the paper entitled " Climatology of intermediate descending layers (150 km) over the equatorial and low latitude regions of Brazil during the deep solar minimum of 2009" by Santos et al. and our responses.

Dear Editor, The paper titled "Climatology of intermediate descending layers (150 km) over the equatorial and low latitude regions of Brazil during the deep solar min-

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Discussion paper

imum of 2009" presents a statistical study of intermediate layers (ILs) using digisondes observations. The statistical results are new, interesting and can be helpful to understand the formation and dynamics of the ILs. Therefore, the paper should be considered for publication after revision. This way, I have listed below some comments/questions/suggestions.

Our answers: We thank the reviewer for taking the time to review our manuscript and provide important comments and questions. Our responses are given below:

Discussion paper 1. P20, L1-3: I cannot see a relation between the day-to-day variability shown in Figures 10a and 10b and gravity waves. Figures 10a and 10b show a wave like perturbation with a periodicity of some days. Gravity waves periodicity varies from some minutes to few hours. It is not possible to afïňĄrm the inïňĆuence of gravity waves with one point per day. This discussion needs some improvements. We thank the reviewer for bringing out this important point. We agree with the reviewer that it is not possible to verify the gravity waves influence with one point per day. We corrected this part of the manuscript. See P34, L9-12 and P35, L1-2 (also please note that the illustrations numbering have changed in the revised version).

2. P29, Figure 14 and its discussion: a) In the label of Figure 14, h'F line is yellow and h'IL line is blue; Ok, corrected (please note that the illustrations numbering have changed in the revised version).

b) How can h'IL be higher than hmF2? As showed in the ionograms of Figure 8, the ionosphere behavior was very peculiar during this day (05 October). It is possible to check in the ionogram at 1020 UT, the IL was located at ∼133 km. Until 1120 UT, the variation of the IL height was very clear in the ionograms, but in the next times, a complex behavior was observed. As discussed in P24, L5-14, the IL appeared to have merged with the F1 layer at 1130 UT. After this junction, the h'IL was considered based on the perturbation of the extra ionization at the high frequency end of the F1 layer until this perturbation attained the F2 layer. The virtual height (h'IL) at a given frequency is higher than the true height at the same frequency. In the present case, from 12 UT the h'IL was higher than the hmF2 as the reviewer can noted. This can be easily verified in the ionograms show below in which the grey curve represents the true height derived from the ionogram:

c) hmF2 is not intensiïňĄed at 1115UT,it has a smooth upward movement starting at around 0930 UT. Apparently, not related with h'IL intensiïňĄcation; Yes, the variations in the hmF2 were very smooth when compared with the variations in IL. We believe that a PPEF can have influenced the IL movement during this event, however the reason for the distinct responses of the ionospheric parameters to the penetration electric field is not completed understood and would need more investigation. An explanation on this matter was included in the P25, L1-5.

d) It is not possible to observe gravity wave activity in the F layer true height at ïňĄxed plasma frequencies. Characteristics of gravity waves as downward phase propagation cannot be seen, only a modulation that could be related to prompt penetration electric ïňĄeld. Would be helpful if the authors presented others plasma frequencies, e. g., 5, 6, 7, 8 MHz, in order to see a vertical propagation of gravity waves in the ionosphere. 4.1, 4.2, 4.3, 4.4 MHz represent basically the same ionospheric height, which makes difïňĄcult to see any gravity wave propagation; We prepared a new figure following the suggestion of the referee using the plasma frequencies of 3, 4 and 5 MHz. It was not possible to analyze the 6 and 7 MHz frequencies due to data gaps in the interval of interest. Now a downward phase propagation can be noted in Fig. 9 (P26). Although not very clear, we also can observe in the first peak oscillation, between 10 and 1130 UT, some kinks (mainly in the 3 and 4 MHz), that can indicate some perturbation caused by a gravity wave. A discussion about this new figure was included in P24, L5-14.

e) It is also difïňĄcult to address a possible cause to gravity wave when we have a magnetic disturbance. The author should choose a case without any magnetic disturbance and try to improve this discussion, even the authors do not believing that the uplifting of the layer was caused by a penetration of electric ïňĄeld. We agree with the reviewer. We include now a new case study (Figure 10b, P30) in which we believe that the magnetic disturbance does not affect the behavior of the intermediate layers. See the discussion about this in P28, L1-15.

3. a) P1, L12: “São Luís (2 âŮę S; 44 âŮę W, I: -5.7âŮę)” should be “São Luís (2âŮę S; 44âŮę W, I: -5.7âŮę)”; Ok. Done.

b) P3, Methodology and data presentation: Some details about the digisonde used could be helpful for those who don’t know about it (e. g., model, time resolution, etc) or, at least, some references; A brief introduction about the Digisonde was included in P4, L1-10.

c) P5, Figure 1: Would be nice know the time of occurrence of the ionogram, even in an example; The ionogram in Figure 1 was registered at 1630 UT. This new information is included in the Figure. See P5.

d) P5, L10: What“60%”means? 60% of the days analyzed for each month or 60% of the ionograms? General information about the statistic (e.g., number of day with data and number of day with ILs), as did in Table 1, could be summarized in this section; The percentage of occurrence of the intermediate layers was calculated considering the number of days with intermediate layers and the number of days of available data. For example: in March (SL) the calculation considered 21 days with available data, being 15 of them with the presence of IL. This give a percentage of occurrence of $\sim$ 70%. A table containing the number of data used in the calculation for all months was included in the manuscript as suggested by the reviewer. See P6.

e) P6, Figure 2: What kind of mean have done in Figure 2? Does it actually a monthly mean? As explained in previous item (d), the data presented in Figure 2 correspond to the monthly percentage occurrence of the ILs and not a “monthly mean percentage occurrence” as we wrote in the legend of the Figure 2. The word “mean” was inappropriately used and was removed.

f) P8, Figure 3: Standardize (0 to 24h) both x-axis (São Luis and Cachoeira); The x-axis was standardized.

g) P8, L6: It is not possible to distinguish the days in this plot. We cannot check the number of days with or without ILs; The reviewer is right. This part was removed from the manuscript.

h) P9, L1: Why have you not seen ILs in March, April, and September (very small occurrence) around 15 UT? This ILs behavior was very interesting and occurred only over SL. In March, the lack of data (10 days) may have influenced this result, but in the other months (April and September) the number of days without data was very small or none during this time. In these months, we verify that during the interval between 12 and 15 UT, a perturbation in the F1 layer was present, but for some reason, the IL formation from a detachment in the F1 layer base (as we observe in many other cases) did not occur, or occurred only later on. A detailed investigation about this point needs to be done, but in this moment, this analysis is out of scope in this work.

i) P20, L11-12: “SL/CP” should be “CP/SL”; After the inclusion of the standard deviation in Figure 15, we think better remove this text of the manuscript.

j) P24, Figure 12: The number of ILs doesn’t match with the information given in Table 1. The number of ILs does not match with the information given in former Table 1, because in the case of Figure 12 (Figure 5 in the new version of the manuscript), all the simultaneous ILs observed were considered in the calculation of the occurrence probability. In Figure 2, we consider only the occurrence or non-occurrence of the IL at each day, independently if the ILs were observed one or more times during a given day. In the equinox (March-April, September – October), for example, the occurrence probability of the ILs over SL in Figure 2 (using the data from the new Table 1) was calculated considering 85 days with the presence of IL. In the calculation of Figure 5 (former Figure 12), we considered 86 IL events, because on 17 April, we observed the presence of two ILs occurring at the same time (two events in the same day). An explanation of this was included in P13, L14-18.

Please also note the supplement to this comment:
https://www.ann-geophys-discuss.net/angeo-2019-74/angeo-2019-74-AC2-supplement.pdf

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