Reference: AA/2017/31831

Reply to the reviewer comments on “Solar wind predictions for the Parker Solar Probe orbit

Near-Sun extrapolations derived from an empirical solar wind model based on Helios and OMNI observations”, submitted by M. S. Venzmer and V. Bothmer to Astronomy and Astrophysics, 25 August 2017.

We are grateful to the referee for the thorough review of the manuscript and for the very helpful comments which we have carefully taken into account. All modifications appear as bold text in the revised version of the manuscript.

Reviewer general comments:

“This paper discusses a method of using OMNI and Helios solar wind data to estimate the solar wind parameters that might be expected during the upcoming Parker Solar Probe mission. It includes an estimate of how these parameters might vary with solar cycle, though this is a relatively small correction, and the likely sunspot number during the mission is anyway highly uncertain. The analysis appears to be thorough, retaining information on the log-normal distribution of the parameters that is used to produce „model“ representations of the distributions and their variation during the solar cycle. However, in the end, this information isn’t used in the predictions at PSP, where only median values are considered. It’s also not clear whether this type of analysis has any advantage over say a simple-minded approach where you use the radial dependence of averaged Helios values to propagate averaged 1 AU values, perhaps taken at different times in the solar cycle, down to the PSP orbit. My suspicion is that the results might not be all that different, but the authors might like to address this point. The extrapolated values do appear to be fairly consistent with previous estimates of near Sun parameters, and deviations, e.g. in the speed profile can be plausibly accounted for, so the results appear to be reasonable.”

*We have tried answering the comments made through the specific comments below and by adding a new Figure providing the predicted solar wind distributions for the first overall, and the first “closest” perihelion. We have also explained that the results would not be that different but what our motivation was (see specific comments below).*

“Some steps in the procedure could be explained more clearly in the text, as noted below, and the tables are rather cryptic since the meaning of the various parameters is not always clear. If they are fit parameters, it would be helpful to state the function that is being fitted in the table. There are conclusions and summary sections that both, yet again, go through the analysis procedure, which is unnecessary. They could be combined. There are also useful functions that give the radial dependences of the parameters that are only shown in the summary section. They could be included in the main results section.”

*We have carefully incorporated the specific comments raised below and provided more explanations. The Tables and captions have been improved for clarification. The summary and conclusion sections have been combined. The functions have been moved as suggested.*

“The English is reasonable overall, but there are a number of cases where a long phrase is used as an adjective. Suggestions have been made to rephrase these sentences. Overall, I anticipate that the paper will be acceptable for publication after the comments in this review are considered.”

Reply to the reviewer specific points:

Page 2, Results, line 5: Suggest: Since the modeled velocity and temperature values below about 20 Rs appear overestimated in comparison with existing observations, this suggests that PSP will directly measure solar wind acceleration and heating processes below 20 Rs as planned.

*We have modified the text accordingly.*

Introduction, line 8: “Until today” is unnecessary – suggest removing. SOHO is also at L1 and returns solar wind observations from CELIAS. But maybe it’s not necessary to list the spacecraft, especially as other spacecraft not listed contributed earlier observations to OMNI.

*Removed as requested. We added a reference to the SOHO measurements. Since there is no direct connection to OMNI here we kept it because we wanted to mention the current spacecraft in operation around L1 providing solar wind data.*

Line 15: Suggest: The in-situ solar wind measurements closest to the Sun to date were made by ...

*Changed as suggested.*

Line 16: Suggest: Helios 1, launched in 1974, reached distances… au. Helios 2, launched two years later, approached the Sun as close as 0.29 au.

*Changed as suggested.*

Page 6, Table 1:

Suggest writing the units together with the parameters in the first column, e.g. “Magnetic field (nT)”, rather than in a table footnote.

*Modified as suggested, also for all other Tables of the paper.*

I don’t really understand what the numbers in brackets (“estimated standard deviations”) represent. Why are they much larger than the parameter values and whole numbers?

*The explanation for the number in brackets has been added to the Table caption, as well as to all other Tables: The numbers in parentheses are the estimated standard deviations of the fit parameters referred to the corresponding last digits of the quoted value.*

How are the mean absolute errors and sums of absolute errors defined?

*Indeed, the mean absolute errors are unnecessary in this context and are not referred to in the paper. So we removed them from the Table. The following text has been added to the caption, as well as it is mentioned in the text: For each parameter the sum of absolute residuals between data and fit (in percentage of the distribution area) is also listed.*

Page 7, Figure 1 caption, last sentence, suggest: show zoomed-in views of the high value tails of the distributions.

*Changed as suggested, also in caption of Figure 2.*

Line 5 below caption: Supporting reference or other evidence that the extreme values are associated with “CME events” (ICMEs?)?

*We have clarified this aspect by adding: “The abnormally high parameter values in the distribution functions can be attributed to shock/ICME events in agreement with the results of the OMNI solar wind investigations by Richardson and Cane (2012).”*

Line 7: Is this “overestimation” because [I]CMEs have abnormally low, not high, temperatures?

*Indeed this appears to be the case based on the comparison with the Richardson and Cane (2012) list. We have added an explaining statement to this paragraph.*

Page 8, last paragraph, line 2: Maybe avoid the word "correlated" since it is used below, and there is no correlation here. E.g., … fast solar wind occurs at times when polar coronal holes extend to lower latitudes, a typical feature of the declining phase of the solar cycle as pointed out by..... Therefore... exhibit time lags relative to the SSN maxima.

*Changed as suggested.*

Page 9, Figure 3: What does the grey shading in the top panels represent? Mention this in the caption.

*The gray shading is now explained in the caption.*

Three lines below figure caption: It's the amplitudes of the variations in the correlations that decline with increasing lag, not the amplitudes of the correlation coefficients.

*This is correct. The text has been changed accordingly.*

Next line: a period [not frequency] of about 11 years.

*Changed, also “lag time” to “time lag”.*

Page 10, Table 2: Similar comments to those for Table 1 apply.

*Has been changed, see reply to Table 1.*

Three lines below table and following: I find this step difficult to follow. It seems that the sunspot numbers are shifted according to the lags, and there is some fit to the data which is then used to produce a model of the particular parameter as a function of SSN, but the text doesn't explain the motivation (apparently to produce a "model" distribution as a function of SSN) procedure very clearly.

*Explanatory text has been added. Small changes added too:*

*- section 3, sentence with equation (9) clarified*

*- section 3, second sentence: abbreviated 'sunspot number'*

*- section 3, switched second and third paragraph*

*- section 3, added a sentence to now second paragraph*

Below equation 8: I don't understand what "with the implementation of these relations into the lognormal function" means exactly. How are the fit coefficients obtained?

*The text has been modified to explain this point and also how the coefficients have been obtained.*

Page 13, Table 3: Give the equation for the fit, to place the parameters in context.

*The reference to the fit equation is provided now, as is the case now for all other Table captions.*

Third paragraph below table, line 2: “See Figure 7”: Why not explain Figure 7 more fully here, then the coefficients d med, davg, etc. obtained from the fits will make more sense?

*Figure 7 has been explained in the text as suggested.*

Last paragraph of page: “As anticipated…” - especially since the same data are used! Also give the results in the text to make the comparison easier with the values given in this paragraph.

*We removed “As anticipated” since it is obvious and we added the exponents we obtained.*

Page 14, last paragraph, line 6: The curves are still very close together after the crossing, so is this an important feature?

*We do not consider this aspect as a special feature but have clarified the crossing aspect in the text, see the answer to the next comment.*

Line 8: But this distance (0.082 AU) is beyond the range shown in the figure, and the data, so is it reliable? I don't understand the comment "therefore the extrapolation of the magnetic field and temperature distribution frequencies to the PSP orbit by applying lognormal functions is limited. What does “limited” mean? Perhaps the comment arises because the mean is larger than the median for a log normal distribution, as noted several pages earlier, and so this crossing indicates that the distribution is no longer log normal. Maybe a few more words of explanation would help the reader understand this point, and the significance of the crossing point.

*These distances are calculated from both fit curves. The questions have been addressed and clarified in the text, including the significance of the crossing point: “Thus, below that distance the frequency distribution cannot well be described anymore by a lognormal function, because the mean of a lognormal function has to be larger than its median… the crossing indicates that the parameter’s distribution is not anymore of a lognormal shape thereafter.” … “The crossing points limit the regions where the distribution’s shapes can still be considered lognormal.”*

Line 10: How are the identical exponents chosen? E.g., was emed always used, or eavg, or some other value? This sentence reads as though the values were the same for all four parameters, but presumably it really means that a different exponent was used for each parameter.

*We have reworded the sentence for clarification, including a remark in the following paragraph.*

Page 15, paragraph 3, line 2: The figure is introduced twice, here and two paragraphs above. Can this be avoided?

*The figure references have been adjusted as suggested.*

Line 7, suggest: The increasing high velocity tail with distance comes from.

*Modified as suggested.*

Page 16, line below figure: Not sure what "established" means here. Remove?

*“Established” has been removed.*

Line 3: What parameter is used for the power law fits? Presumably it's the radial distance, but this could be stated specifically.

*The parameter is the radial distance and has been stated specifically now. A reference to the equation has been added.*

Page 17, figure caption: solar wind parameter power exponents for the dependence on radial distance

*Changed as suggested.*

Last sentence of paragraph, suggest: Since this separation is highly variable, and requires significant effort to calculate for an extended time series, we have ignored this aspect in this analysis.

*Modified as suggested and added 'and' between references 'Bruno et al. (1986) and Balogh et al. (1999)'.*

Section 6, line 2, suggest: predictions of the SSN during the mission are incorporated into the empirical solar wind model derived in the previous sections

*Modified as suggested.*

Next paragraph, last sentence, suggest: the perihelion distance to be reduced to a minimum of 9.86 Rs. Reverse figures 10 and 11, since figure 11 is referenced first, or just remove the mention of Figure 11 here, which isn't necessary; the statement can be understood without looking at the figure.

*Modified as suggested and switched order of figures 10 and 11. The time when the closest perihelion has been added to the first figure reference.*

Last line of page: How are the ranges of median values for different SSN shown in the figure? I'm guessing that it's the thickness of the shaded regions, but this could be stated specifically to guide the reader in interpreting the figure.

*A remark about this has been added, also to the figure caption.*

Figure 10 caption, line 1: The models obtained from Helios and OMNI measurements are …

*Modified as suggested and references to Sheeley 1997 and Wang 2000 have been ordered by year.*

Line 3: But do the extreme values really occur for min and max SSN? E.g. the highest speeds could be at intermediate SSN? How can a time lag be “noted” in this figure which there is no obvious time dependence?

*To avoid misunderstandings, we have rephrased the wording of the caption to: “The models obtained from Helios and OMNI measurements extrapolated to the PSP region for solar minimum to maximum values of 0 ≤ ssn ≤ 200.”*

*Further on, the remark about the time lag has been removed from the figure caption. For clarification, a column for the time lag has been added to Table 2.*

Line 4 below figure: “Arguably”: There's no argument here; this just seems to be a suggestion.

*Has been rephrased.*

Line 5, suggest: mentioned changing shape of the frequency distribution with heliocentric distance

*Is mentioned now as suggested.*

Line 6: But the largest difference appears to be at ~1 AU, so isn't there also a problem with the dipole/quadrapole model matching the observed fields at 1 AU?

*This is correct and we have added an explaining sentence to the text: “It should be noted, that one cannot straightforward compare the results of our study with the values obtained by Banaszkiewicz et al. because the DQCS model assumes solar wind originating from coronal holes at higher heliographic latitudes only, neglecting the slow solar wind belt.”*

Line 7: “Whereas”: Combine the sentences; whereas isn’t used at the beginning of a sentence. Since the peak speeds are on the declining phase of the cycle, wouldn't the peak predicted speeds occur for some intermediate sunspot number, not at SSN=200?

*The sentences have been combined. The comment is correct, it is a shifted SSN. We added the value of the time lag for each solar wind parameter.*

Last line: New sentence at “Thus”.

*Has been modified.*

Page 19, line 6: What is the relevance of the comment on assuming a helium abundance? It's not clear how it relates to the comparison of the density profiles.

*The statement about the helium abundance has been reworded and is explaining the relation now.*

Line 10: overestimates

*Corrected.*

Paragraph 2, line 2: There is an abrupt change of topic here, and it's not immediately obvious why short term SSN predictions are relevant. There could be an introductory sentence saying that since the solar wind predictions depend on SSN, they may be refined using predictions of the SSN during the PSP mission.

*Introductory sentences have been incorporated, addressing short-term SSN predictions relevance as well as an additional argument on the prediction for the first perihelion.*

Line 5: Why should the last cycle be used rather than say a previous one? Is this based on expectations that the next cycle will also be small?

*The comment is valid. We explained that we used the assumptions of Hathaway and Upton 2016.*

Line 8, suggest: the SSN during the closest perihelia, which will commence at the end of 2024 at the likely maximum phase of cycle 25, cannot be predicted at this time. Suggest moving this up to before "For the prediction of the next solar cycle..."

*Has been modified as suggested.*

Paragraph 3: How are the error bands in Figures 12 and 13 calculated? E.g., do they use different assumptions for the predicted SSN?

*We have explained now how the error bands have been calculated and which SSN the predictions are based on.*

Figure 11, lower legend in figure: “Cycle 24” amplitude rather than "its".

*Changed.*

Page 21, line 4, suggest: au, deriving power law fit functions that were used

*Modified as suggested.*

Line 6, suggest: -2025, encompassing the prime mission up to the closest approach of 9.86 Rs.

*Modified as suggested. Replaced solar radii with Rs.*

Paragraph 2, line 3, suggest: Differences from the results of these other studies suggest that below about 20 Rs, PSP may dive into the region where the acceleration and heating of the solar wind is expected to occur.

*Modified as suggested and a remark has been added.*

Last line on page, suggest: because the solar wind is believed to be accelerated up to the height of the Alfvenic critical surface, which is predicted to lie on average around 17 Rs, with a range of between 15 Rs at solar minimum and 30 Rs at solar maximum.

*Modified as suggested and a remark has been added.*

Page 22, paragraph 2, line 5, suggest: that due to solar wind drag, the speeds

*Modified as suggested.*

Summary, paragraph 2: It's not really necessary to go though the procedure yet again; there is already a synopsis in the conclusions. Is it necessary to have both a conclusions and a summary section? They could be combined.

*We have combined discussion and summary sections by integrating the first paragraph of the summary into the beginning of the discussion section and have removed the second description of the procedure. The section is now “Discussion and Summary”.*

Equation 12 and following equations: The formulas presented here have not been shown above, but are much easier to interpret than the results above. I'd suggest moving them into the results section above.

*We have moved the equations to the end of section 5. The statement about slow/fast wind balance has been implemented into the statement about the velocity dependencies.*

Page 23, last but one paragraph: Again, there are points here that have already been said twice above.

*We have removed the paragraph.*

Additional changes applied:

* *We have added a referee acknowledgment.*
* *We found mismatching density values and corrected a typo in our density calculation. The error affected the densities for the first and nearest perihelia and the curves for the densities in figures 12 and 13. Corrections have been made to the abstract, section 6 and summary section: values 4015 to 214 and 9733 to 2951 cm-3.*
* *We have removed a duplicate 'during' in the caption of figure 13 and have replaced “nearest” with “first closest”.*