On the Role Played by Magnetic Expansion Factor in the Prediction of Solar Wind Speed - Response to Reviewer

We would like to thank the reviewer for their positive, which have been thoroughly addressed in the revised version. Below are the comments from the reviewer (*italics*) and and our response to them (**bold**).

Reviewer #1 Evaluations:

Recommendation: Return to author for minor revisions

Grammar improvements needed: No

Annotated: No

Highlight: Yes

Willing to review a revision: Yes

Reviewer #1 (Comments to Author):

Major comments

Overall, very thoroughly and competently done, addresses a very sore point of confusion among many in the community.

I have always been suspicious of the Bernoulli arguments supporting the original WS concept for energy flow in holes, which was intended to explain why we have fast and slow flows on the large scale. This study really goes far toward confirming the large scale geometry as the prime factor in the fast/slow differentiation in the corona - but it is not as simple as a constant energy input being diluted along field lines across spreading holes. In open regions, at least, this paper's findings are consistent with the idea that the key energy flow cannot be confined along streamlines, there must be a strong cross-field dispersal of energy taking place in open regions, be it by waves or by the Scudder-Olbert mechanism, or whatever. That seems the only way you can get such uniform conditions in the holes and why the fast flow is so tightly related to the geometry of the hole boundaries. The slow flow, which can be viewed as leaking out between the main open regions, must be driven by some other mechanism involving non-steady physics.

I am however a bit skeptical about the reality of what may be termed the fine structure in the WSA and DCHB solutions. Given there must be some non-steady interplay in force balance across the fast/slow boundaries, I would expect the sharper structures in the solutions to either never actually exist or to be smoothed out dynamically in the corona.

The authors do not have to offer speculations along these lines, but I do think something like that would would be most welcome and add some punch to the end of the paper.

Be that as it may, this paper is an important stepping stone in building a better understanding *of the physics involved in the layout of large scale structures in the corona. Good work!*

We appreciate the positive comments from the referee, and for also demonstrating to us that we have conveyed the main points we wanted to make. The referee makes a good point about the uncertainty in the origin of the fine-scale structure. Is it due to temporal or spatial variations back at the Sun? The modeling described in this paper really only addresses the latter and really only attempts to model large-scale structure. Within these constraints, spatial and temporal variability are still mixed in the sense that the Wang-Sheeley expansion factor can be thought of as a strictly spatial modulation of solar wind speed. The interchange reconnection (or other process) that the DCHB model is presumed to capture is related to an inherently time-dependent process, which, however, may be approximated by a spatial pattern. At progressively smaller and smaller spatial scales, we envisage that temporal changes will become more important, as evidenced by remote solar observations that show structure varying more quickly on ever smaller spatial scales.

Although this is an important point, since we do not address it directly here, we would not be comfortable speculating on properties of the solar wind that we have not analyzed. However, we note that we do address the referee's point modestly in the penultimate paragraph, where we hint that expansion factor may modulate speed (on smaller spatial scales) within large polar coronal holes.

To address the referee's point more generally, we have made a few modifications to clarify that our analysis is related to the 'large-scale' or 'bulk' properties of the solar wind. For example:

"Currently, there are three principal empirical techniques in use for computing solar wind speed at some reference sphere."

now reads:

"Currently, there are three principal empirical techniques in use for computing the large-scale properties of solar wind speed at some reference sphere."

Minor comments

1) line 158/9, wording: shouldn't "that is, from a coronal hole boundary, at the photosphere" be in quotes?

To address this, which was written in a confusing way, we have changed:

"where d is the minimum, or perpendicular distance from an open-closed boundary, that is from a CH boundary, at the photosphere"

to read:

"where d is the minimum, or perpendicular distance from an open-closed (i.e.,

CH) boundary at the photosphere"

2) lines 307/8: there seems to be a notation issue here: you have four parameters (epsilon, w, Vfast, Vslow) in the DCHB model, but you show five 6s in the hypercube; same in the WSA expression, an extra "6" - "d" isnt really a model parameter, it is an input variable

Agreed. The DCHB model should have 4+1 and the WSA model should have 5+1. We have modified the description to correctly reflect this.

3) in a number of places, notation seems off:

line 467 "- 0.3 - -0.4"

line 558 "131-+"; similar for lines 596,605,610

4) line 479 - should be "WS", not SW

5) line 553 - "available FROM Stanford"

For 3) the double dash came from a LaTeX formatting error. We have corrected this. It should have read 0.3 - 0.4. Points 4) and 5) were corrected as suggested.

We have made several minor changes to improve the manuscript and also addressed the comments made by referee 2. These are all shown in the *diff* PDF file uploaded with this revision, which explicitly identifies all changes made to the document between original submission and resubmission.

Again, we thank the referee for taking their time to provide these constructive suggestions, which have improved the quality of the manuscript.