

Third Response to Reviewer

We thank the referee for his/her continued effort to improve the quality of the manuscript. The referee raised two points, one concerning Figure 2 and the other related to the boundary of the slow-flow band as it relates to the EF model. Below, we respond to each comment.

Reviewer #1:

I think this paper is very close to publication, but I have two recommendations to make.

First, while I really appreciate the inclusion of Figs 3 and 10 and associated discussion, I still feel the presentation could be clarified with some work on Fig 2 and its discussion. Specifically, there are two issues to be settled:

1) I take Fig 2 as a schematic, not an actual calculation - is that correct? (it is referred to as "illustration"). If so, I find it a little puzzling, in that if you look carefully at the fieldline geometry in the near-equatorial region, they differ to the eye so little that it is hard to accept the argument that differences in expansion factor should result in slow flow in (a) and fast flow in (b). To drive home this point, I have whited-out some fieldlines in (2a and 2b) in the accompanying jpg. <(Editor): The file can be downloaded from our Editorial Manager.> Looking at the two remaining fieldlines that connect to the outer boundary, it is hard to see how the expansion factors differ by much from those in (2a), at least on the face of it - I would expect some obvious deviations in the two cases. If this pair of plots are indeed from a calculation with 2D dipolar and unipolar closed structures, then it must be that differences in expansion factor are subtle in terms of fieldline geometry and one should not try to infer much in that regard from simple plots like this (as implied much later in Fig 10). That would be an interesting point to make up front, if true.

As the referee notes, Figure 2 is a schematic (it is explicitly referred to as an illustration in the Figure caption). We addressed the referees concerns about whether it was capturing the essential geometry of the field lines by adding Figures 3 and 10 in the previous revisions which were based on MHD solutions. We believe that Figure 2 is a useful framework for introducing the two constructs of the two models. Its point is not to give an accurate profile of field lines but to illustrate how expansion factor changes as a function of latitude within the two model concepts, and we believe it does show this, at least qualitatively. It also serves to introduce the terms that are used throughout the remainder of the paper. We have successfully used this illustration at several meetings during the last year, and have informally polled colleagues about whether they found it to be confusing or ambiguous. Therefore, we continue to believe that is useful in its current form.

2) If Fig 2 is a schematic, I strongly urge you to replace it with an actual 2D calculation, for two reasons. First, I think it is important to firmly, unequivocally establish (not just hand wave) what the fieldline geometry and expansion does look around the closed structures in the two instances. You should also definitely include a plot of expansion factor, as in Fig 3. After all, this figure is at the crux of your entire presentation. Second, the fully 3D calculation in Fig 3 (and 10), currently standing alone as an actual calculation, raises an obvious question. Since the basic issue is geometry, is the low value of expansion factor in unipolar streamers a stronger effect in 3D than in 2D, or does it not matter much? Does it depend upon the length of the underlying unipolar arcade? In summary, while I understand and accept the arguments for Fig 3 (and 10), I am really uncertain how it relates to the simple arguments introduced for Fig 2 - and I expect others would be, too.

As noted above, we believe that Figure 2, which is clearly identified as an illustration, serves to introduce the concepts that are further studied in the paper. It illustrates, but does not demonstrate how expansion factor might vary as a function of latitude for the two models. Figures 3 and 10, which are based on the MHD model results provide the demonstration that the referee would like to see in Figure 2. Additionally, it would not make sense to include a plot of expansion factor, as we did in Figure 3, since the field line geometries here are illustrative, and not intended to be quantitatively correct. As a final comment, by using a schematic, we are suggesting an idea that is to be tested in what follows, and that is our intent. We do not believe it is confusing nor could it be mistaken for a numerical result. Finally, our main points are made with the 3D result, which we do not believe is overly complex. Thus, constructing an idealized 2-D simulation to test this does not appear to be warranted. Rather, had we constructed a 2-D solution, a reasonable criticism would be that this might not translate into three dimensions.

For these reasons, we believe that Figure 2, in its current form, is a useful addition to the manuscript.

Finally, one other minor point that I raised before, without much success: The sentence beginning on the bottom of pg 5, line 54 "The boundary of the slow-flow band..." - this sticks in my craw as utterly irrelevant, recommend removing it - the boundaries in the EF model actually stem from the magnetostatic expansion coupled with parameterized velocity variations derived from empirical fits, nothing more. They have nothing to do with Parker equations. One can try to impute in a backwards way some theoretical justifications for these parameterizations, but that is all there is to it.

We apologize for not addressing this in our previous response. As we point out in the paper, the WS and EF models while aligned in terms of being centered on expansion factor, are philosophically different – one is an ad hoc, empirical model and the other is a theoretical model. What the referee is objecting to would be true if we were referring to the WS model. However, the statement on page 5, line 54 is leveled at the EF model, which is based on a theoretical development. The referee may dispute the validity of this model, but the published literature (papers by Cranmer, Wang, etc. over a number of years) provide sufficient support that the EF model has at least some support in the community. Our position is to be as objective as possible; thus, we have included this statement to reflect this. Personally, one of us (PR) is not convinced that the bifurcation of the critical points provides a sound theoretical basis for selecting slow wind. And, in fact, this process does not seem to take place in our 3-D MHD calculations. However, that remains to be investigated and we have chosen to make as objective and fair of a statement as we could.

Again, we thank the referee for helping us improve the quality and clarity of the manuscript.