List of Responses

Response to Referee # 1 (W. K. Peterson)

This is an expanded re-analysis of an event analyzed by Strangeway now including corrected mass resolved ion outflow observations and an analysis of the nightside region. The analysis is almost completely qualitative, not quantitative. The exception is shown in Figure 5 where orbit average correlation coefficients between in situ Poynting Flux and soft electron precipitation vs solar wind inputs parameters are given and discussed without presenting the uncertainties of the correlations. The conclusion of the paper is that the data presented are consistent with what has been deduced from prior observations and modeling.

In spite of minimal quantitative analysis, the data presented and discussed here will be useful as qualitative checks for large scale magnetospheric models attempting to account for the many processes occurring in the coupled solar wind, magnetosphere, ionosphere system.

I recommend publication after the authors have considered the general and specific comments below.

Response:

The Strangeway paper and our two previous papers on this storm (Zhao et al, 2020 and Zhao et al., 2022) all addressed the local correlation between the energy input into the auroral region and the resulting O^+ and H^+ outflow flux. This paper is not a duplicate of that effort, but is taking the additional step to understand what changes in the solar wind correlated best with the energy inputs into the aurora. The point of the initial figures, and in particular Figures 3 and 4, is to show that that the changes observed in the energy input and in the outflow are changes on a storm time-scale, not just short-term ups and downs that FAST happened to catch on different orbits. Indeed, Figure 5 is the main result showing how the various SW and IMF parameters and coupling functions correlate with the energy input.

General Comments:

Comment 1: The paper presents no quantitative evaluation of the role of solar wind drivers on ion outflows. The authors should consider changing the title to: A reexamination of the drivers of auroral outflow during the September 23-26, 1998 storm.

Response:

Yes, we see your point. Since we have already shown in previous papers that the outflow correlates with the energy input, here we only address the correlations between the solar wind drivers and the energy input. We could change the title to "Solar wind driving of energy into the auroral region during a CME storm", or something similar.

Comment 2: No direct correlations were presented of ion outflow rates vs solar wind inputs. Orbit average cusp and auroral O^+ outflow rates over the latitude range could have been compared to the solar wind drivers shown in Figure 5. If such an analysis was attempted, the authors should discuss what prevented them from presenting the analysis. If the analysis had been done, comparisons could then have been made to total O^+ escape rates as a function of solar wind pressure and other solar wind inputs as presented by Ramstad and Barabash, 2021; Schillings et al, 2019; Lennartsson et al. 2004; and Peterson et al. 2024.

Response:

Yes, as explained above, since we know that the auroral outflow scales with the energy input, here we are testing how the solar wind affects the energy input. It is true that others have done empirical studies that correlate some measure of solar wind input to O^+ escape rates, and we are not trying to duplicate that effort. We are instead trying to find the causal links (or at least the best correlations) in the chain. We certainly can add the outflow correlations to the plot. The result is shown below. The results are what we expect – that the correlations general mirror what is observed for the energy input parameters, particularly the Poynting flux. We note that we actually do not find any correlation between solar wind pressure and either the energy inputs or the O^+ outflow.

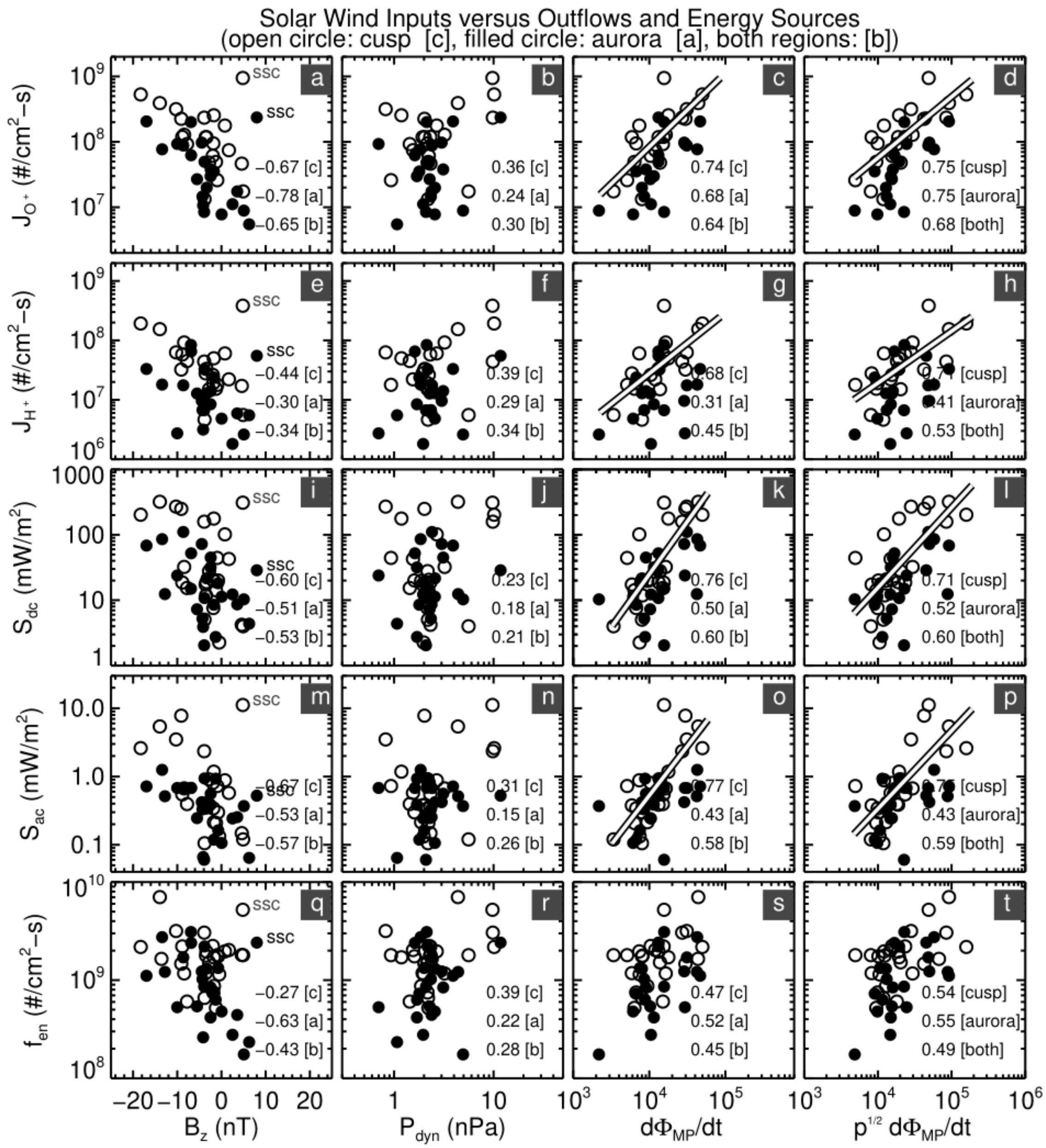

Figure a1 (revised Figure 5). Scatter plots of the solar wind-magnetosphere coupling functions, Bz (leftmost column), P_{dyn} (second column from the left), $d\Phi_{\mathrm{MP}}/dt$ (third column from the left) and $p^{1/2}d\Phi_{\mathrm{MP}}/dt$ (rightmost column) versus the outflow fluxes, including J_{O^+} and J_{H^+}, and the energy inputs, including S_{dc}, S_{ac}, and f_{en}.

Comment 3: O^+ outflowing fluxes are a strong function of solar activity. Please state somewhere the F10.7 index range for the orbits analyzed.

Response:

We will add the following statement to the end of the second paragraph in Section 2.1:

"The solar radio flux at 10.7 cm wavelength is between 136.2 and 144.1 s.f.u. (10^{-22} W/(m^2-Hz)) in this storm from September 23 to 26, 1998."

Specific Comments:

Comment 4: Line 149: The paper should explicitly state that the TEAMS data have been corrected for spacecraft potential.

Response:

Yes, we will add that statement in the revision.

Comment 5: Line 192: Significant fluxes of photoelectrons are observed below 60 eV as reported by Peterson 2021, Front. Astron. Space Sci. and others.

Response:

In the revision, we will explain that we are following Strangeway's study (Strangeway et al., 2005) and we added that significant fluxes of photoelectrons are observed below 60 eV in the FAST altitudes (Peterson et al., 2021).

Comment 6: Lines: 197-200: Why are averages of some quantities compared to maxima of others? The median of all quantities would be consistent as well as a good filter of extreme values. If different methods of selecting values of quantities presented in the figures are used, this fact should be noted in the figure caption.

Response:

To explain why the maxima of the Poynting flux are used in this study, we will add the following statements in the main text:

"…In this study, averages are calculated for the outflow fluxes, the electron number flux, and the amplitude of ELF waves, while maxima are computed for the Poynting fluxes since we found that the Poynting flux is concentrated spatially in a narrow region compared to the selected wide range for outflows…"

This is similar to the data analysis in Zhao et al. (2020) and Zhao et al. (2022). The figure captions will be updated.

Comment 7: Line 207: This reads like the H^+ number flux on orbit 8284 wasn't what we expected so we ignored it. The readers deserve a better explanation. In the reviewer's opinion, presenting the median H^+ number flux for all intervals, even if orbit 8284 it is low would be better and would reflect the variability of the night side outflows as discussed in the paper.

Response:

In the orbit 8285 (not 8284), which was mentioned by the reviewer, FAST observed very small population of H^+ outflows and thus we also excluded it from our analysis. Based on the comment, we calculated the flux in this orbit, and will include it in our study. It makes only minor changes to the correlations.

Following the referee's suggestion, we tried calculating the median outflow flux, as well as the maximum outflow flux, in each selected outflowing time periods. The trends are the the same. Since the median values show similar evolution to the averages so we still use the averaged values instead of the medians.

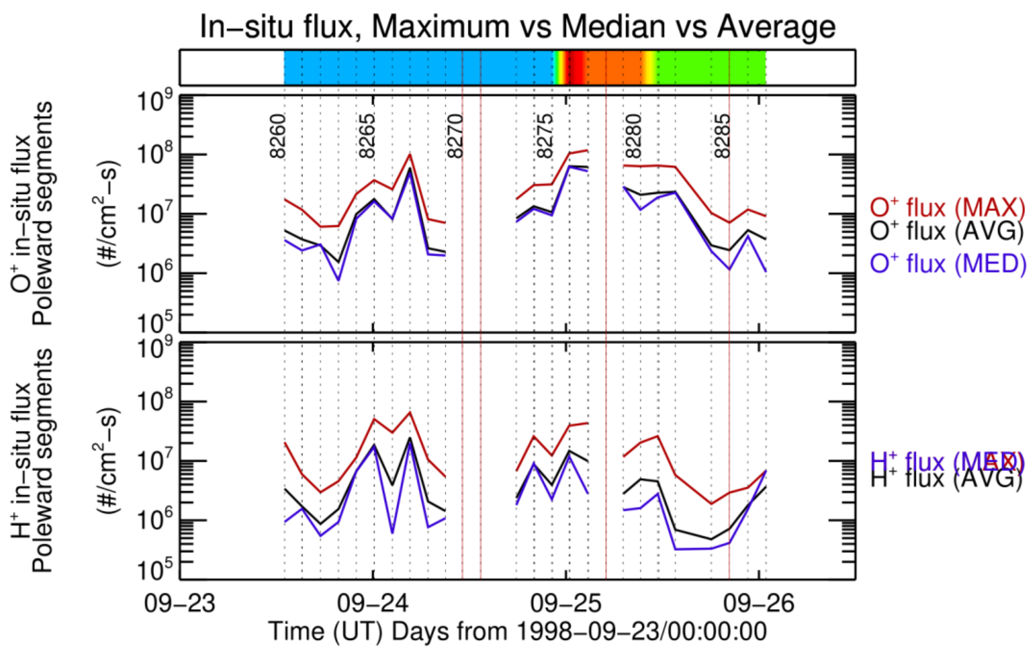

Figure a2. Comparison of the nightside outflow flux (O^+ in the top panel and H^+ in the bottom panel) between the maximum (red), the median (blue) and the averaged (black) values over the outflow time periods in the nightside auroral region. Vertical red lines indicate the three orbits with large data gaps or the orbit 8285 with low flux. Orbit 8285 is included in this plot.

Comment 8: Line 304: The O^+ and total fluxes track very well indicating what?

Response:

It indicates the consistency of the outflow fluxes measured by the two distinct instruments on board FAST satellite. Because of cross-calibration between TEAMS and iESA, we expect the outflow flux of O^+, which is the dominant outflowing species in this event, to be close the iESA flux and their temporal evolution of the flux shows a similar pattern.

We have modified this sentence to,

"The fluxes track very well indicating good cross-calibration between the two instruments."

Comment 9: Line 321: are fen in the text here and in other places and Fen on Figure 4 the same parameter? If so choose one for consistency.

Response:

We thank the reviewer for his careful reading of our manuscript. We will use f_{en} throughout the manuscript for consistency.

Comment 10: Figure 5: The reader can and should be better informed about the quality of the correlation coefficients shown in Figure 5. This could be done by moving the coefficients to a separate table and reporting their uncertainties. There would then be room to put lines showing the best fits for the cusp intervals which show the best, but still poor, correlations of ~0.8.

Response:

This is a good suggestion. We will add the table in the resubmission.

Response to Referee # 2 (Spencer Hatch)

In this study the authors revisit FAST measurements during the CME storm that occurred September 23–26, 1998, including relatively recently recalibrated mass spectrometer measurements made by the TEAMS instrument. This storm and the corresponding FAST measurements have been the subject of a number of studies now spanning two decades. I am a little confused about the purpose of this study, because the territory covered by the authors has already been covered elsewhere, including in their own studies (Zhao et al, 2020, 2022; Nowrouzi et al, 2023). There are also plenty of studies describing qualitative correlations between so-called solar wind drivers and various measures of local energy input, so I struggle to see what's new here.

Beyond not really seeing what's new, I have a hard time with the statement in the abstract that the energy inputs are "strongly correlated with the solar wind - magnetosphere coupling functions". The correlation is generally low quite low, with correlation coefficients of 0.5 or less in Figure 5. (The highest value is 0.75 for the correlation between dayside DC Poynting flux and the Newell coupling function.) I also find the results in Figure 5 somewhat misleading, because as far as I can tell, the authors do not calculate the general correlation between the quantities in Figure 5; they specifically calculate statistics of these quantities during observations of ion outflows. Given that ion outflows are the final result of multi-stage processes on day- and nightside, and that the geometry of the magnetic field is such that energy inputs are not necessarily collocated with the ion outflows they induce (e.g., Sánchez and Strømme, 2014, doi: 10.1002/2013JA019096; Gorney et al, 1985, doi: 10.1029/ja090ia05p04205), the connection is tenuous at best. Sánchez and Strømme (2014) have a good discussion of the problem with trying to correlate nightside outflows with auroral drivers, if we may call them that.

In other words, I have a problem with the methodology. It doesn't make sense to me to identify short periods of ion outflow at altitudes of a few thousand kilometers, and to then calculate (for example) the average Bz during these short periods as though there should be a relationship between Bz at the bow shock and intense ion outflow that takes at least a few minutes to make it out of the ionosphere. This problem is even worse on the nightside, where there is really no reason to expect that observations of outflows should have anything to do with what is happening at the bow shock at that moment.

Given these difficulties with the methodology, I unfortunately don't think the authors succeed in answering the second question they pose in the last paragraph of the introduction: "(2) Which solar wind drivers are responsible for the enhanced energy input?" The authors are looking for causation, and the dataset they use just doesn't put them in a position to do this.

On a related note, the first question the authors pose ("How do the energy input and the outflow flux change with time during the storm?") has been addressed by a number of studies, the first that come to mind being the work of Yau et al (1985, doi: 10.1029/JA090iA09p08417) Audrey Schillings (https://ltu.diva-portal.org/smash/record.jsf?pid=diva2%3A1203208&dswid=3966), and my own work (Hatch et al, 2016, doi:10.1002/2016JA022805), and there are many others that I think several coauthors of this study must be aware of.

Conclusion

In summary, I unfortunately cannot recommend this study for publication. The first question that the authors pose has been answered in many previous studies (just do a forward citation search on the several hundreds of citations of Yau et al, 1985), and I don't think the dataset or methodology are appropriate for answering the second question.

Response:

As noted in response to the first referee, the Strangeway paper and our two previous papers on this storm (Zhao et al, 2020 and Zhao et al., 2022) all addressed the local correlation between the energy input into the auroral region and the resulting O^+ and H^+ outflow flux. This paper is not a duplicate of that effort, but is taking the additional step to understand what changes in the solar wind correlated best with the energy inputs into the aurora. The point of the initial figures, and in particular Figures 3 and 4, is to show that the changes observed in the energy input and in the outflow are changes on a storm time-scales, not just short-term ups and downs that FAST happened to catch on different orbits. And Figure 5 then examines the correlation between the SW and IMF parameters and the energy input into the aurora.

Our assumed chain of events is that solar wind/IMF is driving the energy input that comes into the aurora region. The energy input then drives outflow (as discussed in many papers). That outflow then flows along the field and, depending on convection, may end up closer or further down the field. Papers have looked the correlations between the input (e.g. solar wind pressure or solar wind energy) and the ultimate output (e.g. net O^+ flux down the tail), and between the energy input and the outflow. But there are still questions on the direct connection between the solar wind parameters (pressure, Bz, etc) and the resulting energy flux, both Poynting and precipitation, into the auroral zone.

That said, we certainly agree that this study of one storm is not the final answer on this question, but we do think that the set of measurements during this storm that gives snapshots of energy input and outflow every 2 hours from before the storm into the recovery phase moves us towards better understanding.

Regarding the methodology, we completely agree with the referee's point that there is difficulty in trying to compare instantaneous solar wind measurements with instantaneous outflow. However, the point we are making particularly in Figures 1 and 4, is that a storm is not just a random set of ups and downs in the driving parameters. The storm is driven by a structure that moves across the magnetosphere, and the changes are large scale and long term. Therefore, our results are not sensitive to the exact sampling time of the solar wind. Similarly, the FAST measurements also are assumed to be representative of long term changes, not just indicative of the few minutes that the outflow is observed during each individual orbit. As Nowrouzi et al. (2023) showed, there are large-scale changes to the outflow during a storm. Because of its orbit, the FAST satellite samples the changes every couple of hours. But the outflow and energy input change during the storm occur on longer time scales. Therefore, time delays between the energy input and the outflow won't matter significantly in this analysis. To show this, we have tested the correlations shown in Figure 5 using different time delays between the measurements and the solar wind parameters, and using the solar wind parameters averaged over the previous hour. This gives a more integrated picture of the energy input, and has a similar result. We find that there are only small changes in the correlation coefficients both with time delays up to an hour and when using the hourly averages. When shifting the time up to an hour, the correlations $r(S_{ac\,[cusp]}, d\Phi_{MP}/dt)$, $r(S_{dc\,[c]}, d\Phi_{MP}/dt)$, $r(J_{O^+\,[c]}, d\Phi_{MP}/dt)$, $r(J_{O^+\,[aurora]}, d\Phi_{MP}/dt)$ that were greater than 0.6 are always greater than 0.6. The correlations become to decrease when the delay is about 100 minutes and the averaged correlation drops to less than 0.2 when the delay is 3 hours. The correlations between the Poynting flux and the coupling function are at the level of 0.7 when the time delay is within an hour. When using the hourly averages, the correlations that were greater than 0.7 between the Poynting flux and the coupling function are still greater than 0.7, from 0.74 to 0.76 for S_{dc}, and from 0.74 to 0.77 for S_{ac}. The correlation between the O^+ flux and the coupling function changes from 0.74 to 0.68. Based on the referees comments, we will change to using the hourly averaged values, and discuss more clearly how we are looking for solar wind drivers on storm time-scales.

Minor comments

205–210: I, like the other reviewer, was confused by the statement on these lines that H^+ fluxes were too low during orbit 8285. Does this mean that the measurements were bad in some way? If the measurements were not bad, it seems to me data from that orbit should be included. As Jesper Gjerloev likes to say (in paraphrase), the measurement that doesn't match your expectations is often where the discovery is waiting to be made.

Response:

Yes, we have added that point back in. It does not change the conclusions.

Section 3.4: "To find the driving factors …" Again, I don't think the dataset or the methodology puts the authors in a position to positively identify driving factors (i.e., causation) of variation in energy inputs. What they present are correlations, and as we hear so often, correlation just isn't causation.

Response:

While it is true that correlation isn't causation, the correlations do give us the best clues about what the actual driving parameters are. We will reword this statement to be less definitive: "To find the **likely** driving factors of the energy inputs…"