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Interactive comment on “Beam tracking strategies for fast acquisition of solar wind velocity distribution functions with high energy and angular resolutions” *by* Johan De Keyser et al.

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Reply to Reviewer Comment #2

The authors thank the reviewer for his/her thorough revision of the manuscript and the helpful comments. Below, we respond to each of the points that was raised.

General Comments

Reviewer comment:

`The presented manuscript presents and discusses a novel approach to employ electrostatic spacecraft analyzers`

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fitted with angular deflectors. By evaluating beam parameters of the surrounding plasma, only energy- and directional bins relevant to resolving said beam need to be sampled, resulting in much faster signal acquisition and as a result, higher time resolution.

The presented method represents an instance of a sparse sampling approach, in which the sample points from a high-dimensional parameters space are deliberately constrained to certain subsets of that space in order to obtain a maximum amount of information with minimal sampling requirements. Similar techniques have been employed with great success in Biophysics (Such as compressed sensing techniques in neurosciences [1]), Astronomy (in aperture synthesis for telescopes [2]). Likewise in the same field as this manuscript, kinetic simulation approaches in space physics employ similar techniques to reduce the computational load of high-dimensional simulation spaces [3,4].

References to similar approaches from those fields, as well as overview papers of compressed sensing methods should be added, since a large body of general theoretical background work from other fields can be applied for this approach.

Specifically, the presented manuscript discusses a method to sparsely sample space plasma velocity distributions, with the intention of tracking a `"beam" and sampling it with a minimum number of required samples, to obtain an extraordinarily high temporal resolution.`

Response:
Indeed, beam tracking tries to represent a system with a maximum amount of information for a minimum sampling effort. That there is a general theoretical framework regarding “sparse sampling” is undisputable. However, we feel somewhat reluctant to expand the manuscript with a discussion of the “sparse sampling” or “compressed sensing” context for two kinds of reasons.

First: How relevant is “sparse sampling” or “compressed sensing” in this context?

- The “compressed sensing problem” deals with reconstructing a sparse vector from a reduced number of data with sparsity as a priori knowledge. The idea then is that measuring the limited data is sufficient to reconstruct the full data if one knows the sparsity properties. The practical relevance of this is very much dependent on the specific assumptions. “Beam tracking” is a very specific form of sampling, in which knowledge about smoothness and compactness of the VDFs in velocity space, and physical knowledge about the time scales involved, are all fundamental. In other words: the peculiarities of the situation at hand are responsible for the fact that one cannot learn very much from the generic theory. To put it simply: We measure data in a compact subregion of phase space (the reduced data), and from that we derive the full data by simply assuming that the VDF is zero outside that subregion (finding the full data from the known sparsity pattern). This sort of application is so trivial that we do not need the general theory. Of course, more refined approaches could be possible in which one might sample a few isolated points in phase space to derive the full VDF from that, but this would be strongly dependent on specific assumptions (for instance, you could do this efficiently if you assume that the distribution is a bi-maxwellian). However, the scientists usually do not want to make those assumptions. Alternatively, one might train a subsampling algorithm on a set of realistic data . . . but we do not have such training data – we have at present no high time cadence VDFs.

- The standard "compressed sensing" theory does not take into account the notion that there might be variable costs associated with collecting the reduced data. There is a cost (a time delay) associated with switching the spectrometer to a different energy (due to the need to set high voltages and the accompanying settling times). It makes little sense to measure at a specific elevation angle, since it is much more time-efficient to sweep the voltage between the deflector plates to acquire a contiguous set of measurements over all elevations at once. All these practical constraints limit the amount of sparsity in the problem, so that there is little to gain from the general approach.

- Compressed sensing has a lot in common with data compression, in particular with lossy compression. That is something that scientists prefer to avoid. To really understand the measurements, and to be able to reprocess data, they prefer not to work with an "indirect" representation of the sampled velocity space (the reduced data set); they simply want to know the values as measured in all the individual voxels, not any reconstructed values. Indeed, it would be very awkward (though not impossible) to update an instrument calibration and reprocess the raw data if only an indirect representation is given.

- An additional problem with an "indirect representation" is that any further statistical analysis of the data will be hampered by the strong and specific cross-correlations between the errors on the measurements in different voxels.

- For beam tracking and moment calculation, one must be able to interpret the measured data on-board in a straightforward manner and fast. It is not at all clear whether the typical optimization techniques used in compressed sensing and the required response times are within the capabilities of present-day on-board processors.

Second: How relevant is it to mention “sparse sampling” for the reader of the paper?

- Our introduction briefly reviews a number of plasma spectrometers, showing the progress of technology and actually focusing on the way in which the velocity space sampling problem has been approached. That sketches the context for the discussion of beam tracking sufficiently well to allow the reader to follow the text. We are therefore not convinced of the necessity of inserting an overview of sparse sampling techniques in the introduction.
- We want to point out that every measuring instrument is doing one form or another of sparse sampling, yet descriptions of instruments typically do not mention sparse sampling theory (none of the reference papers for the plasma spectrometers that are reviewed in the introduction does). Admittedly, it is not because nobody does it, that it could not be useful.
- We are already a bit concerned about the manuscript length and do not want to expand it unnecessarily.

Modifications in text:
Given all the above, we have inserted a paragraph that mentions the possibility to interpret beam tracking in the context of “sparse sampling” or “compressed sensing”, without entering into a deeper discussion that would necessitate to mention some of the points listed above. We have chosen to do so in the discussion section, rather than in the introduction. This allows us to present sparse sampling methods as a possible future avenue in the quest for even faster solar wind characterization. We have added a few general references, and one targeted toward the space plasma physics audience.

Reviewer comment:

The model assumptions going into the example analysis performed in this manuscript are a) that the "interesting" part of the particle distribution is quite compact in shape, more precisely, in this analysis it is assumed to be maxwellian b) that it’s overall shape stays the same, and only it’s parameters change. These assumptions preclude the possibility of multiple mixed plasma distributions, such as a core and beam setup in a foreshock, rings or loss cones in a fermi-type acceleration region or any other non thermally-relaxed particle distribution.

I assume that the authors only focus on the solar wind distributions’ core is motivated by their specific research interests. However, the study of kinetic physics of the solar wind, including the effects of turbulence, shocks and magnetic reconnection depends strongly on the ability to study and understand nonthermal distribution functions, that is, precisely those distribution functions that do not fulfill the assumptions going into the manuscript at hand.

Response:

There seems to be a serious misunderstanding here. We make no assumption regarding the shape of the velocity distribution function other than that it is compact, i.e., that it occupies only part of the phase space accessible by the instrument. We use Maxwellians only to test the beam tracking algorithm, since we do not possess any VDF measurements at the high cadence considered here. The shape of the distribution is allowed to change, it can be anisotropic, it can consist of a core and halo, it can be a mixture of different populations, the particle distribution does not have to be thermally relaxed . . . as long as the compactness condition is satisfied. The degree to which this must be the case depends on the choice of the phase space energy/azimuth/elevation window sizes. Obviously, this approach cannot deal with non-compact distributions. It precludes, for instance, the detection of backstreaming solar wind particles reflected from an interplanetary shock: an instrument staring at the sun cannot detect particles coming from behind.

Modifications in text:

Also in view of a comment by the other referee, we have inserted a paragraph in the conclusions that addresses the issue of populations reflected from shocks.

Reviewer comment:

While a much more thorough analysis and quantification of the detector behavior for realistic distribution functions will be required before the presented method can be employed in an actual instrument, it is probably not within the scope of this paper to perform them -- the central subject and conclusion being the presentation and motivation of a sparse sampling scheme in the first place. Still, some more reflection on the limitations of the presented analysis, and avenues to further refine the analysis should be included.

In conclusion, this manuscript presents a thoroughly novel idea that merits publication and discussion in the wider scientific community, but suffers from being too narrow in it's goals and scope. After some major revisions, in which the presented method is evaluated

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with a focus on more general kinetic-physics processes,
I consider it suitable for publication.
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Response:
We're a bit surprised that the reviewer asks us to evaluate the method with a focus on general kinetic physics requirements, as we believe that the present manuscript does exactly that. Indeed, studies of turbulence, waves, or instabilities require only two things of a plasma spectrometer: (a) obtain VDFs with high energy and angular resolution, and (b) with high time resolution. That the first goal can be achieved with the THOR-CSW design parameters, has been demonstrated by Valentini et al. (2016) as cited in the manuscript. The present study of beam tracking demonstrates that the first goal can be achieved while at the same time satisfying the second goal. That demonstration consists of

- Showing that with realistic sizes of the beam tracking window and the prescribed angular and energy resolutions, a high time resolution can be achieved.
- Showing that – and this is what we perceived as being the major concern – the beam tracking technique is able to deal with dramatic time evolution in the solar wind, as exemplified by shocks. There is no reason to worry about rapid, but less dramatic, changes at or near the centre of the solar wind beam: they will be captured as long as the beam tracking window is large enough.

The description of how beam tracking can be implemented, as well as the examples, corroborate our claim that beam tracking is indeed capable of leading to solar wind VDF measurements that enable (ion) kinetics-physics studies.

Specific Comments

Reviewer comment:

`The prediction method presented in section 3.2 and it's discussion of polynomial extrapolation overshoots is very similar in nature to the problem of flux limiters in finite volume simulation methods, such as MHD simulation. There, too, the extrapolation of a reconstruction polynomial is clamped to remain within physically realistic boundaries. This similarity could be discussed and referenced (such as [5]).`

Response:
There is indeed a certain similarity with flux limiters used in computational fluid dynamics, but the setup is different: in the CFD case, one knows the gradients at both sides of an interface, and the ratio between those two gradients is used as the argument of the flux limiter to bound the extrapolations from either side. In the case of beam tracking, one only knows the gradient from the past; nothing is known about the future. Actually, there are numerous other situations where an extrapolation (which is always risky) can be bounded by using additional heuristic knowledge. Stock market prediction is actually much more similar to the beam tracking problem that we are dealing with. However, we think that making that comparison in the manuscript would lead us too far astray.

Reviewer comment:

`The same section claims that "All in all, one can expect such techniques to work reasonably well only if the energy does not change rapidly", and I agree with that statement. However, especially in shocks, discontinuities and reconnection regions, where this assumption does not hold true, is where the most interesting kinetic plasma physics effects occur.`

Response:

This sentence refers specifically to the use of higher order polynomial interpolation. The point that we make here is that linear interpolation is actually better in view of shocks or discontinuities, as explained in the preceding sentence.

Modifications in text:

We have rephrased the sentence to avoid any misunderstanding.

Reviewer comment:

Note that the sudden changes of distribution function in these events are not simply a parameter change of a maxwellian: the shape of the distribution function departs *significantly* from a maxwellian whenever kinetic physics comes into play. If the spacecraft changes it's magnetic connection to a shock, beam distributions of highly nonthermal shape can suddenly "appear" outside of the thermal velocity radius of the previous maxwellian. In reconnection regions, spitzer orbits and crescent-shaped velocity distributions additionally appear on top of any thermal background that might still be present. Additionally, nonisotropic superthermal tails can deform the solar wind distribution away from a maxwell-boltzmann shape.

Response:

We reiterate that we make no assumption regarding the shape of the velocity distribution function that has to be measured, although our tests are limited to Maxwellian distributions. We understand very well the risks of monitoring only a limited part of phase space – indeed, one can miss certain features. This is why a beam tracking

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plasma spectrometer is best used in combination with an omnidirectional spectrometer, where the former gives you very high time resolution for the core of the distribution, and the latter provides the full context (but probably at a slower pace).

Modifications in text:
A paragraph inserted in the conclusions addresses this complementarity of both types of spectrometer.

Reviewer comment:

The discussion in section 4.4, comparing internal and external beam tracking, is thus incomplete, as the asumption of a continuous change of maxwell distribution parameters won't represent reality in many interesting kinetic physics scenarios.

Response:
Maxwellian distributions are used here only to construct a test example. The emphasis of the test in section 4.4 is on achieving the necessary time resolution and being able to follow the rapid velocity jumps (jumps in energy and/or beam direction). The reviewer is of course correct in that the changing nature of the distributions themselves could play a role as well; in that sense the test is indeed incomplete. However, nobody has ever measured distributions that rapidly, so we had to construct the example artificially. An alternative could have been to use the output of a full Vlasov simulation code, but we do not have access to such simulations over a sufficiently long time period and featuring solar wind shocks. It is our opinion that “the proof of the pudding is in the eating”, i.e., a full evaluation of beam tracking is only possible by building an instrument and trying it out in space.

Modifications in text:
We have inserted a short discussion concerning the incompleteness of the test as the

last paragraph in section 4.3 (this issue is applicable to all the tests with the BMSW data, and goes beyond the internal/external beam tracking differences).

Reviewer comment:

As for the beam loss criterion itself (sections 3.3 and 3.4), it is based on the assumption that the "beam" encompasses the entire interesting part of the distribution function at time of tracking, and that the only noteworthy change at a plasma discontinuity would be a sudden loss of the beam at one spot, with reappearance at another. This is a rough oversimplification of the wide variety of foreshock distribution functions (compare [6]): in many cases, additional beam distributions will occur far outside the thermal velocity extents of the solar wind beam, thus remaining untracked by the restricted sampling process presented here. "Beam Loss" as defined in this paper is neither an appropriate, nor a sufficient criterion for re-scanning of the complete velocity space.

Response:

No, the beam tracking spectrometer can follow all sorts of changes in the shape of the distribution functions across a discontinuity, as long as these occur not too far away from the centre of the beam. We agree that this condition is not valid in the foreshock. As mentioned before, this is why a beam tracking plasma spectrometer is best used in combination with an omnidirectional spectrometer.

Modifications in text:

This is now addressed in the conclusions section.

Reviewer comment:

The presented tests inadequately asses the response of `the method to these kind of scenarios. While it is good and correct to assess the ability of this method to re-acquire the beam after a beam loss scenario with realistic dynamic timescales, this is, by far, not the only relevant measurement quantity to optimize for. I would suggest expanding section 4 with a discussion of the applicability of the presented method for the study of nonthermal kinetic effects in the distribution function. This can be rather open-ended, to initiate constructive discussion about the proposed method: estimates of dynamic timescales, angular extents and energy ranges would already allow the method to be scrutinized by experts specializing on specific phenomena.`

Response:
What the reviewer proposes here goes far beyond the demonstration of beam tracking as a viable method to operate a plasma spectrometer so as to satisfy the generic requirements for doing kinetic-scale physics. A detailed examination of what can be done by a specific spectrometer with given energy/azimuth/angular and time resolutions for a particular set of non-thermal kinetic effects (as simulated by a Vlasov code) could indeed be the subject of a follow-up paper.

Technical Corrections

Reviewer comment:

`Simulated measurement plots (figures 1, 3, 5, 7, 8 and 9) are missing an axis label on their (presumably) time axis.`

Modifications in text:
We added the axis label on all those plots.

References

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