Jason Byrne <jbyrne@IfA.Hawaii.Edu> 12 April 2013 11:14
To: David Long <dml@mssl.ucl.ac.uk>, Peter Gallagher <peter.gallagher@tcd.ie>, Shaun Bloomfield <shaun.bloomfield@tcd.ie>, Shane Maloney <shane.maloney98@gmail.com>, "R. T. James McAteer" <mcateer@nmsu.edu>, "Huw Morgan" <hum2@aber.ac.uk>, Shadia Habbal <shadia@IfA.Hawaii.Edu>
Fwd: AA/2013/21223: referee report

Hi all,

I am a lot happier with this report, wherein the referee has given some in-depth feedback and comments, and understood the importance of this work.
I will go through the report in detail when I get the chance.

Cheers,
Jason.

---------- Forwarded message ----------
From: <aanda.paris@obspm.fr>
Date: 11 April 2013 23:33
Subject: AA/2013/21223: referee report
To: jbyrne@ifa.hawaii.edu

12/04/2013

Dr Jason Byrne

jbyrne@ifa.hawaii.edu

Our Ref. : AA/2013/21223

Dear Dr Byrne,

Your paper "Improved methods for determining the kinematics of coronal mass ejections and coronal waves" was submitted to a referee who recommends publication after substantial revision (see enclosed report).

Please take the referee's comments and suggestions into account in revising your work and send us the new version (in referee format and in printer format) at your earliest convenience.

Instructions for resubmission can be found at address https://mms-aanda.obspm.fr/is/aa/resubmit_a_paper.php. Your author ID number is 18133.

- In your cover letter, please indicate precisely all the changes made in the revised version. Please also include your detailed responses keyed to the items in the report.
- Mark all the changes clearly (using boldface) in your manuscript.

With best regards,

Hardi Peter
A&A Editor

Referee Report

The paper analyzes in detail some numerical issues with deriving basic kinematic data from observations of CMEs and coronal waves. It also presents some solutions to these issues. The authors are to be commended for looking into these issues, technical as they are, as few people do, despite the importance of the parameters derived from observations of CMEs and coronal waves. The Savitzky-Golay filter appears to offer a credible alternative to existing methods as a way of reducing the effect of noise in the data. However, I believe there are a number of points that should be addressed. I also encourage the authors to spell out what new information about CME and coronal wave kinematics are learned from their analysis techniques that we did not already know from previous works.

2. Sec 2., bullet point 2 - is there another objection to Eq. 1 other than the second sentence in this bullet point? If not, please re-word this bullet point. Also, "uncertainty interval" should simply be "uncertainty".

3. I don't understand why the authors use a percentage of the CME height to determine the scatter (Sec 2.1). Firstly, it implies that the height of CMEs at earlier times are better determined than later times, absolutely if not relatively. Is this really the case? Can this be justified? Surely the error in measuring a position is due to the error in the ruler and finding a point to measure, not in how far away it is from the origin. Secondly, even if there is a linear increase of error with time, the error at time t = 0 is not zero. There should be a minimum error. I would suggest examining the results of the CORPITA algorithm to assess the behavior of the measurement error as a function of time. If a suitable justification cannot be found then I suggest that the simulated data analysis be re-done with a more suitable description for the scatter.

4. 'unknown unknowns'

The authors should NOT refer to "unknown unknowns". The authors should be more accurate in what they when they use this colloquial phrase. For example, the last paragraph in Sec. 2.2 talks about

"Furthermore, the quantification of uncertainty on the physical measurements themselves is extremely non-trivial. This is due to the effect of ‘unknown unknowns’ throughout the analysis, making a robust error estimate practically impossible."

The authors do not mean that their analysis has 'unknown unknowns' running through it. I suspect that the authors are referring to at present unknown physical mechanisms operating in the phenomena under study that help create the observed signal but since they are not known, are not modeled. This happens in every scientific investigation. The point of this paper is to generate uncertainty estimates of the kinematics that can be generated by a consistent procedure that takes into account all known or estimated sources of uncertainty.

5. Please clarify the bootstrapping scheme. It looks like the authors are describing residual resampling. Step 4 in section 3 is written as

y^{*} = y + \\epsilon^{*}.

A residual resampling scheme uses

y^{*} = \\hat{y} + \\epsilon^{*}.

Is the bootstrap used to generate these results exactly as written in the paper? If so, please give the name of the bootstrap scheme. Or is there merely a typo in text and 'y' should be replaced with \\hat{y} ? In any case, the full name of the bootstrapping scheme should be given. Please justify the use of (what I think is) residual resampling when the errors are time-(height) dependent when the residual resampling scheme as described is not. Would a block bootstrap not be more appropriate in this case?

6. There are many filtering/smoothing schemes (splines, moving average) - why choose the Savitzky-Golay filter? What property of the Savitzky-Golay filter recommends itself to the authors? Would other filtering/smoothing schemes with similar properties give very similar results, say to within a standard deviation (as assessed via bootstrap)? Smoothness in the derivatives would seem to be a desirable property. It might be that almost any appropriately chosen smoothing scheme gives better results than the 3-point Lagrangian, with results similar to those derived via the Savitzky-Golay filter. The paper would have much greater weight in the community if they could show that filters with certain properties are desirable in the analysis of CME and coronal wave data, and are superior to existing techniques commonly used.

7. Sec 4.1, para.3. The authors argue that the analysis method applied towards the end of the time-series are prone to end-effects. Such end effects are also present at the start of the time-series, and so comments as to the uncertainty in derived parameters should apply to both the start and end of the time-series. Please comment in more detail about both ends of the time-series. Is it right to say that end effects must be significant when the amount of time spanned by the filter overlaps with the ends of the time-series?

8. Fig.12.
It looks like at some position angles, the wavefront is decelerating,

and at others the wavefront is accelerating and then decelerating. Could the authors comment on this?

Minor points

Abstract

In the "Results" part of the abstract, the sentence

"...then its accuracy can be examined..."

should be

"...then its precision can be examined..."

This is because the true values to the kinematic parameters are not known, and so accuracy cannot be ascertained. I encorage the authors to check for the occurence of both accuracy and precision in the text and edit accordingly.

Section 2, first sentence: AIA provides high cadence observations of EUV wavefronts

Equation (1) needs a reference.

Figs 2,3 should mention the true values of the velocity and acceleration in the figure legend for ease of reading.

Figure 4: a better way to show this result would be to plot a 2-d surface (as a function of the % scatter and the cadence) of the width of the frequency distributions shown in the lowest plot of Figure 3. The colorbar would then represent the precision of the measurement. The authors could generate the same type of plot for the accuracy of the measurement (say, the frequency distribution means as a function of the %scatter and cadcence).

Sec. 3, para 2; change "confidence" to "uncertainty"

Sec.5, para 2. "...measuring confidence intervals..."
I would suggest instead "...estimating confidence intervals...".

"Accurate uncertainty intervals...".
The paper presents a method of estimating the uncertainty in kinematic CME and coronal wave parameters. I would suggest instead "Uncertainty estimates calculated via a defined and appropriate procedure..."