Jason Byrne <jbyrne6@gmail.com>

Error

10 messages

Jason Byrne <jbyrne6@gmail.com> **24 June 2008 17:46**
To: Peter Gallagher <peter.gallagher@tcd.ie>
Cc: James McAteer <james.mcateer@tcd.ie>

So,
If I take the filter at what I've been calling scale 5 (or labeled in reverse order from the eight computed scales it's scale 3) so we've filter size 2^3=8 pixels,
so max effective sampling by Nyquist says 2*8=16 pixels,
and converting this by arcsec/pixel through to solar radii for a typical C3 image gives approximately 0.9 - 1 R_Sun as the minimum error introduced at this scale. Sounds about right to me looking at C3 images, and certainly by eye I was pulling out FWHMs ranging from 8 to 40+ pixels for the fronts across C3 fov.

Hardcoding min error of +/- 8 pixels (from error range of 16pix) will increase the error bars of concerned event 2000-Apr-23 significantly (new plots attached).
CDAW quotes a speed 1100 km/s which is bang on our fit, and a negative accel - 48.5 m/s/s which is very plausible from our scatter.

I can't find better reasoning than derivsig.pro propagation for unevenly spaced samples. Looking at deriv.pro again, it's that a Lagrange interpolation is performed before the derivative is taken, so the weighting between points is based upon how a fit would sit between them, and derivsig goes hand-in-hand with this assumption, *but* does appear to be determining the propagated error at the midpoint of the interval.

20000423_min_err.pdf
23K

Peter Gallagher <gallagpt@tcd.ie> **24 June 2008 18:43**
Reply-To: peter.gallagher@tcd.ie
To: Jason Byrne <jbyrne6@gmail.com>
Cc: James McAteer <james.mcateer@tcd.ie>

Thanks, Jason. This looks much more reasonable all right. Could you make sure
David and Shane are aware of these ideas?

Could you apply the same methodology to the April 21 event?

Cheers,

Peter.

Peter Gallagher PhD

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Jason Byrne <jbyrne6@gmail.com> **25 June 2008 10:57**
To: peter.gallagher@tcd.ie
Cc: James McAteer <james.mcateer@tcd.ie>

Morning,

Here's the 21-Apr-2002 event. The minimum error for C2 is about 0.1 of a Solar radius. So while the decreasing profile of the acceleration may not be wholly justified in such a small data set, in this event it is still apparent, and within the error bars could show the expected trend of acceleration decreasing to zero.

The velocity fit ranges within 2250 - 2450 km/s (where Gallagher et al quote ~2500 km/s at heights beyond 3.4 R_Sun) and the acceleration fit gives 24 m/s/s.

I'm a lot happier with this minimum coded error now. It was always a concern that the ellipse could be more confident than I was by eye!

Jason.

2008/6/24 Peter Gallagher <gallagpt@tcd.ie>:

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Peter Gallagher <gallagpt@tcd.ie> **25 June 2008 18:31**

Reply-To: peter.gallagher@tcd.ie
To: Jason Byrne <jbyrne6@gmail.com>
Cc: James McAteer <james.mcateer@tcd.ie>

Hmm, that looks interesting. I agree with you that the errors are not much more believable. I know I've said it a million times without providing a solution, but the start and end points in v(t) and a(t) still bother me. Even though I now understand what derivsig is doing, I don't think it is correct. For example, the uncertainties in the first velocity data-point are a factor of 3-4 greater that the mean of the non-endpoint values. Statistically, this can't be the case can it? Also, the uncertainties in the first and last data-points are making the simple a=const or 0 model consistent with the data, which I don't believe is the case, particularly for the April 21 event. Same for acceleration, the uncertainties in the first data-points are a factor of ~2 greater than the mean of all the other non-endpoint values. Reducing the uncertainty in the first and last data-points of a(t) to the mean of all other values would imply completely different kinematics.

What do you guys reckon?

Peter.

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Jason Byrne <jbyrne6@gmail.com> **26 June 2008 10:57**
To: peter.gallagher@tcd.ie
Cc: James McAteer <james.mcateer@tcd.ie>

Morning,
I've just been going through Bevington again and I can't quite work out exactly where derivsig gets its equation from, and I think it's important that I do. But most importantly on the point of edge effects (while I would always argue that the errors must be larger at the edges) the actual weightings taken of 9:16:1 for computing the edge points' errors in derivsig comes from squaring the weightings used in deriv (3:4:1) for performing 3-point interpolation for derivative at the edges.
ie: adding the variances means adding the squares of the standard deviations which are weighted by these constants.
I don't see that there's any other way around it, if this is the correct statistical approach. We may simply lack enough data points.
Eg: looking at the attached (tentative) results for Stereo event where there's a lot of data points for this slow CME the middle points are able to constrain the fit while the edge errors become large.
Jason.

2008/6/25 Peter Gallagher <gallagpt@tcd.ie>:

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plot.pdf
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Jason Byrne <jbyrne6@gmail.com> **26 June 2008 18:04**

To: peter.gallagher@tcd.ie
Cc: James McAteer <james.mcateer@tcd.ie>

I've gone through all the maths behind deriv/derivsig/Bevington and accounted for each step in the codes. I have it step-by-step (ready for my thesis) and it's very convincing!

2008/6/26 Jason Byrne <jbyrne6@gmail.com>:
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Peter Gallagher <gallagpt@tcd.ie> **26 June 2008 18:21**
Reply-To: peter.gallagher@tcd.ie
To: Jason Byrne <jbyrne6@gmail.com>
Cc: James McAteer <james.mcateer@tcd.ie>

Hi Jason,

I agree with you regarding the end points; these uncertainties should of course be larger than the other points, but is a factor of ~4 reasonable? Even if it does agree with Bevington, do we believe agree with the method and its assumptions? I think we should meet up on Friday to go through the maths. What about 2 on Friday?

Those STEREO kinematic curves are very interesting. We should compare the kinematics derived using LASCO to those derived from STEREO. This would be a real test for how good are uncertainties actually are.

Talk tomorrow,
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> > <http://www.physics.tcd.ie/Astrophysics>
>

Jason Byrne <jbyrne6@gmail.com> **26 June 2008 18:21**
To: peter.gallagher@tcd.ie
Cc: James McAteer <james.mcateer@tcd.ie>

Also here's another Lasco event I analysed (and am going to analyse Stereo event 16-Nov-07 once I've the data properly prepped).

This event is one which I showed movies at during meeting illustrating how chaining through scales using angular info can mask out streamer deflections. Again the codes are all separate bits and pulling them together is tricky so this is just analysed by eye same as the rest.

http://www.maths.tcd.ie/~jaydog/Solar/CME_ellipse_movies/20000211/20000211_ellipse.html

The kinematics are attached. They show constant acceleration = 10.7 m/s/s (CDAW gives 10.2 m/s/s) and speed 300 - 630 km/s (CDAW gives 498 km/s).

Hints of structure in the kinematics may not be real since in C2 the max height jumps significantly. I think I

have a code that will take heights along a specific angle I can look at...

Jason.

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Jason Byrne,
Astrophysics Research Group,
School of Physics,
Trinity College,

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Jason Byrne <jbyrne6@gmail.com> **26 June 2008 18:33**
To: peter.gallagher@tcd.ie
Cc: James McAteer <james.mcateer@tcd.ie>

I know what you mean - we could assume a next/previous data point at small time gap to be in some way similar to the trend of the end point thus the error is lessened... the maths is an interpretation of not knowing what the next data point will do!
I'm free all day; 2's grrrand.
ps Did you see my last email - they just crossed in cyber-space.

2008/6/26 Peter Gallagher <gallagpt@tcd.ie>:

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james mcateer <james.mcateer@tcd.ie> **26 June 2008 22:28**
To: Jason Byrne <jbyrne6@gmail.com>
Cc: peter.gallagher@tcd.ie

also around all day.
see you 2pm then.

ps> will work on the stereo problems in the morning.

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R. T. James McAteer

james.mcateer@tcd.ie
Skype: rtjmca
