Jason Byrne <jbyrne6@gmail.com>

COR1/COR2 fits

6 messages

Peter Gallagher <peter.gallagher@tcd.ie> **18 August 2009 04:10**

To: Shane Maloney <shane.maloney98@gmail.com>, Jason Byrne <jbyrne6@gmail.com>, james mcateer <james.mcateer@tcd.ie>
Cc: David Long <dlong@tcd.ie>

Hi guys,

I've spent today playing with the COR1 and COR2 heigh-time data from the December CME. Been trying mainly to reduce the scatter in the velocity and acceleration profiles using a variety of interpolation and smoothing routines, but to no avail (copying you Dave because its about the on-going kinematics debate).

Attached are three figs showing constant acceleration fits to the CME's height-time evolution through COR1 and COR2.

Each of the attached figs are laid out as follows:

1st panel: Height vs time and const acceleration fit. Also shown are the best-fit param values to the height data together with their uncertainties and the corresponding chi-squared.

2nd panel: Data minus model. Also shown are +/-3 COR1 or COR2 pixels (in white dashes) and the 1-sigma uncertainty in the fitted model params (in red dots) from MPFITEXPR.

3rd panel: Velocity vs time using DERIV and DERIVSIG. Also show is the model velocity from the height fit and its 1-sigma uncertainty.

4th panel: Similar to 3rd panel, but for acceleration.

Now some comments on the figs.

cor1-cor2.tiff: The fit to the COR1/2 height-time data in the top panel looks okay, but there is a lot of structure in the residual plot (2nd panel). The early portion of the data (COR1) shows a distinct peak, which implies that the constant acceleration model does not fit well to entire COR1/2 height range. The velocity and acceleration profiles derived using DERIV also show a peaked feature (as we've seen before). This suggests to me that this early peak in acceleration is real, as it results from two independent methods. The latter part of the data appears to do be ok approximated by a constant acceleration model.

cor1.tiff: This is a fit to the COR1 data-points only. Again, both the residuals from the const acceleration fit to the height-time data and the velocity and acceleration profiles from DERIV show structure in the kinematics in the second half of the COR1 data => Peak in acceleration is real.

cor2.tiff: Fit to COR2 heights. The residuals don't show any real structure, and the majority of the data-points in the residual lie within the instrumental uncertainty (+/- 3 pixels) => constant acceleration model fits. There is a large discrepancy between the const acceleration velocity profile and the DERIV velocity profile, which I don't understand (will check my code again tomorrow). The DERIV acceleration points are pretty well scattered about the constant acceleration model, so this might also suggest that a constant acceleration model is sufficient to explain the height-time evolution in COR2.

Going to have a look at HI tomorrow (Tuesday), but please do send me any comments. Would be particularly interested in hearing how these results compare with some of the previous analysis that you guys have done.

Cheers,

Peter.

Peter T Gallagher
School of Physics, Trinity College Dublin

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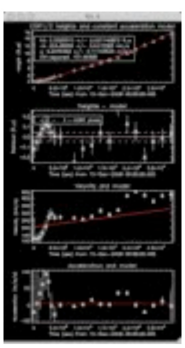
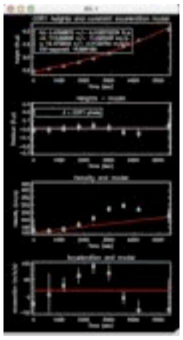
Web: www.physics.tcd.ie/astrophysics

3 attachments



cor1-cor2.tiff
35K



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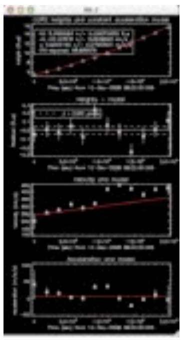

cor2.tiff
33K

Jason Byrne <jbyrne6@gmail.com> **18 August 2009 13:55**
To: Peter Gallagher <peter.gallagher@tcd.ie>
Cc: Shane Maloney <shane.maloney98@gmail.com>, james mcateer <james.mcateer@tcd.ie>, David Long <dlong@tcd.ie>

Hi,

I like that this shows the acceleration to be real since it can be hard to trust the deriv procedure. The use of the inversion code provides acceleration of ~70m/s/s while we see deriv gets upto ~100m/s/s and they agree within errorbars.
I'm not convinced that the constant acceleration model is appropriate in COR2 where my eye wants to draw a negative x^2 fit. (On your fitting, I found in some cases sometimes the velocity had to be fit directly and integrated up for heights.)
Eitherway the question then remains do we fit the data sets simultaneously across all FoVs with some form of drag or the inversion because we see different regimes of propagation? The first attempt at inversion starts off with the high acceleration immediately while the deriv procedure maintains a growth phase of the acceleration upto its peak.

Have you seen this paper (attached) about the kinematics of CMEs leading edges compared to their prominence eruption? They just do a simple derivative on a "cubic-spline smoothing of the distance-time data" but mention how the acceleration data "often shows several 'oscillations' around a=0 at the end of the acceleration stage". I don't think we could include analysis of the prominence but it's good to see their direct comparison.

I also still wonder what errors we should be considering for our data. You highlight +/-3pixels here, but since errors are additive there would be more to it than that throughout the reconstruction methods. Is a residual threshold sufficient (e.g. 1 sigma) in this case?

Jason.

2009/8/18 Peter Gallagher <peter.gallagher@tcd.ie>

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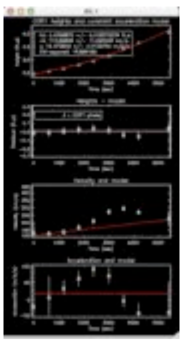
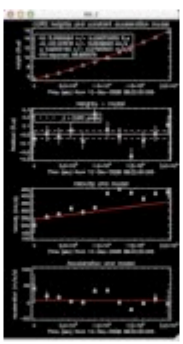
4 attachments



cor1.tiff
32K



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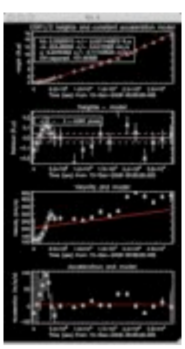
33K



cor1-cor2.tiff
35K

Maricic_etal_ProminenceCMEkins_SolPhys2009.pdf
344K

Peter Gallagher <peter.gallagher@tcd.ie> **18 August 2009 14:19**
To: Jason Byrne <jbyrne6@gmail.com>
Cc: Shane Maloney <shane.maloney98@gmail.com>, james mcateer <james.mcateer@tcd.ie>, David Long <dlong@tcd.ie>

Hi,

On 18 Aug 2009, at 07:55, Jason Byrne wrote:

> Hi,
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> I like that this shows the acceleration to be real since it can be hard to trust the deriv procedure. The use of the inversion code provides acceleration of ~70m/s/s while we see deriv gets upto ~100m/s/s and they agree within errorbars.

Good. We now have three independent approaches giving a peak in the acceleration at the start of the CME's evolution.

> I'm not convinced that the constant acceleration model is appropriate in COR2 where my eye wants to draw a negative x^2 fit. (On your fitting, I found in some cases sometimes the velocity had to be fit directly and integrated up for heights.)

Do you mean that the constant, a, in the a * t^2 term, is negative? I can have a shot at fitting by forcing a negative a.

I didn't fit the velocity profile because of the features that DERIV sometimes introduces.

> Eitherway the question then remains do we fit the data sets simultaneously across all FoVs with some form of drag or the inversion because we see different regimes of propagation? The first attempt at inversion starts off with the high acceleration immediately while the deriv procedure maintains a growth phase of the acceleration upto its peak.

I wonder if the inversion starting velocity could be fixed to a particular value in order to better constrain the inversion.

My preference would be to find a model which fits as much of the data as possible, but this may not work out in practice. I'll have a shot at some other fitting today.

> Have you seen this paper (attached) about the kinematics of CMEs leading edges compared to their prominence eruption? They just do a simple

> derivative on a "cubic-spline smoothing of the distance-time data" but mention how the acceleration data "often shows several 'oscillations' around a=0 at the end of the acceleration stage". I don't think we could include analysis of the prominence but it's good to see their direct comparison.

Yes, I tried this kind of think yesterday. It has the effect of removing the early peak in acceleration, but that's ok I think. I think this method could have value after the peak. I'll see how I get on with this again today.

Incidentally, could you send me:

Mariˇci´c, D., Vršnak, B., Stanger, A.L., Veronig, A.: 2004, *Solar Phys.* **225**, 337.

This appears to give more details on their smoothing approach.

> I also still wonder what errors we should be considering for our data. You highlight +/-3pixels here, but since errors are additive there would be more to it than that throughout the reconstruction methods. Is a residual threshold sufficient (e.g. 1 sigma) in this case?

Yes, this was just a guess. From a consideration of the front width, etc., what do you think an appropriate value would be?

Talk later,

Peter.

P.S. Could everyone read the paper and cover letter before the telecon so that we can all comment on the state of the paper?

> Jason.
>
> 2009/8/18 Peter Gallagher <peter.gallagher@tcd.ie>
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<cor1-cor2.tiff>

<cor1.tiff><cor2.tiff>

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<cor1.tiff><cor2.tiff><cor1-cor2.tiff><Maricic_etal_
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Shane Maloney <shane.maloney98@gmail.com> **18 August 2009 14:24**
To: Peter Gallagher <peter.gallagher@tcd.ie>
Cc: Jason Byrne <jbyrne6@gmail.com>, james mcateer <james.mcateer@tcd.ie>, David Long <dlong@tcd.ie>

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> I didn't fit the velocity profile because of the features that DERIV sometimes introduces.

Just to note that the velocity is not high enough from the fits (~350) if we use the arrival time at ACE as constraint.
I also think that fitting in velocity space just fits the artifact of the deriv method. The corresponding fit to height data is more accurate I would think.

> Yes, I tried this kind of think yesterday. It has the effect of removing the early peak in acceleration, but that's ok I think. I think this method could have value after the peak. I'll see how I get on with this again today.

Yea I tried this as well didn't get very far, get getting oscillations between the data points, or messed up kinematics.

> Yes, this was just a guess. From a consideration of the front width, etc., what do you think an appropriate value would be?

If you were to fit two of the instruments with the same model, we could probably estimate the error from the stats of the residuals, still low numbers but would give some thing.

Shane

Peter Gallagher <peter.gallagher@tcd.ie> **18 August 2009 14:36**
To: Shane Maloney <shane.maloney98@gmail.com>
Cc: Jason Byrne <jbyrne6@gmail.com>, james mcateer <james.mcateer@tcd.ie>, David Long <dlong@tcd.ie>

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I agree.

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> Yea I tried this as well didn't get very far, get getting oscillations between the data points, or messed up kinematics.

Yes, me too. I think the problem that I was having was because I was interpolating over an increased number of data-points. Say for example that the original data had measurements at 10 times. I then interpolated over 20 or more times. This gave me the oscillations that you talk about.

I actually think that the correct approach is to downsample the data to less times. This will smooth the data. DSPLINE should be able to do a good cubic-spline interpolation, but the key is fiddling with the "elasticity" of the fit.

PG

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Jason Byrne <jbyrne6@gmail.com> **18 August 2009 14:47**
To: Peter Gallagher <peter.gallagher@tcd.ie>
Cc: Shane Maloney <shane.maloney98@gmail.com>, james mcateer <james.mcateer@tcd.ie>

Maricic etal. 2004 paper attached.

I also just threw the deriv velocity profile on top of the inversion profile. I didn't realise the inversion was chopping the early times off for some reason, and the points don't all lie within the errorbars, though the deriv errors might overlap them if included. I'm really not sure about what errors to take, mostly because I don't know what a referee will accept. If I was to go through method errors I think they would boom!

[Quoted text hidden]

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2 attachments

fulltext-12.pdf
687K

inv_deriv_vel.pdf
15K
