Re: Swift grb satelitte

Source: http://sci.tech−archive.net/Archive/sci.astro/2005−04/msg00163.html

- *From*: jaymoseley@xxxxxxxxxxx (sean)
- *Date*: 27 Apr 2005 01:49:50 −0700

Craig Markwardt <craigmnet@xxxxxxxxxxxxxxxxxxxxxxxxxxx> wrote in message news:<ony8b8v5hz.fsf@xxxxxxxxxxxxxxxxxxxx>...

From: Craig Markwardt (craigmnet@xxxxxxxxxxxxxxxxxxxxxxxxxxx)
Subject: Re: Swift grb satelitte

>> I required them to be released.

>Irrelevant. Your requirements are not part of the Swift mission.

I also note that you ignored the fact that light curves and spectra
are released via GCN notices, and in some already−published papers.

What gcn notices? I have yet to see one spectra released to the public
And what papers? It doesnt mention in the SWIFT website
that the spectra will be available in only certain papers. Let
alone which papers these will be available in.

>I note that you deleted *yet again* an example which shows the
>contradictory nature of your two claims. You could have researched
>the definition of "proportional" but did not.

Please supply the `deleted` example that supposedly shows which
of the two does *not* mean that the delay increases proportional
to wavelength. In fact it is you fails to answer the above question
I pose. Which of the two phrasings I use Does NOT mean that
the delay increases proportional to wavelength?

>> The Lyman break is a sharp break whereas there is a

>This claim is not true, therefore the conclusions you draw based on it
>are irrelevant.

Are you suggesting that the Lyman forest is seen on the long wavelength
side of 1215? In case you forgot it starts at ~1215.
And you didnt answer my question.. The Lyman break is sharp at
about 1215 whereas the observed drop off is gradual. There wouldnt
be a gradual drop off with decreasing wavelength if it were due to
the Lyman alpha forest. So please tell me why does this gradual drop

off not match a sharp Lyman break?

>http://mathworld.wolfram.com/Proportional.html

> If 'a' is (directly) proportional to 'b', then 'a/b' is
> constant. The relationship is written , which implies 'a = c b' for
> some [proportionality] constant 'c'.

>The proportionality constant between wavelength (*) and time is the
>adjustable parameter of your model, whether you realize it or not.

>(*) − or difference in wavelength, for your other contradictory claim.

So if `a` is the peak observed at wavelength `a` , lets say 20nm,
and` b` is the observed time of `a` peaking at 1 second,.. then in
another wavelength `a` of 200nm, the delay `b` would be in my model
10 seconds.
Thats 20/1 =20 and 200/10= 20.
Whats your problem? They both are the same result of 20,
or in other words constant. Your whole argument is proven wrong
by my calculation here.

>I didn't ask for a chi^2 fit to test your model. What I did suggest
>is that some kind of formal statistical test which addresses
>measurement uncertainties, upper limits, and derives parameter
>confidence regions, is required. I note your diversion to the
>"smoothing" topic.

And I ask you to be more specific. What sort of formal tests
(upper limits etc) do you require me to do? Specify please and
I will deliver. I cant read your mind.

>"Systematic bias" has a well−defined meaning, and addressing it is
>.crucial to most scientific research.

>That would be your problem, not mine. There are obvious selection
>effects which would lead to biases. For example, optical/IR
>observations can usually be done the night of the GRB, but as a
>practical matter, sensitive radio observations often take longer to
>schedule. This would naturally lead to a gamma−ray − then optical/IR
>− then radio progression of detections. But of course this only
>reflects the order of *observations*, not the presence of emission,
>nor a "peak" in the emission.

OK this is a bit more specific regarding systematic biases. Thanks.
But it doesnt seem to say anything that impinges on my model. For
instance the systematic biases cited above would only reflect
on when and how long the observations take place, and how frequently.
Yet this has no bearing on my model in that if there are less
scheduled observations in radio you say that means that
the ones that are available are not valid. Are you are suggesting that

if there are less radio observations for one grb that the ones
that have been made are less accurate?
The only relevence would be that you are saying that if there are
less observations in one wavelength the less clear the decay
rate is. Thats true but it doesnt mean that if I predict that
the decay rate will follow a precise lightcurve that these fewer
observations wont follow that lightcurve. Because in fact so far,
whether its 100 or 2 observations taken under widely different
circumstances those 2 or 100 still can be shown to fall within
the parameters set out by my predictions. And you have yet to supply
1 grb out of all in history that doesnt.

>Yes, and for a century, it has been known that electromagnetic
>radiation travels at a constant speed at all wavelengths. Thus your
>"model" is utterly erroneous.

And this is the untruth I mean to expose. Because the whole
point of this theory is to show that in grbs it is the first
provable case where light can be not only shown to travel at
different speeds relative to the observor but can ONLY
be explained by c being variable. Why else would I dwell on
this if it wasnt that I felt my model was the first indisputable
evidenc of a variable c where modern physics could not offer
an equal explanation

>Of course, if as you claim, that GRBs are simply upshifted starlight,
>then the spectra in other wavelength bands should be stellar spectra
>−− BUT THEY ARE NOT. In fact, gamma−ray burst spectra have well known
>bent power law spectra (stars do not). X−ray spectra of afterglows
>are power laws (for stars they are not). The optical/IR spectra of
>GRB afterglows do not match optical/IR spectra of stars. Stellar
>spectra typically have systems of absorption and emission lines
>superimposed on a thermal continuum −− GRB spectra at all wavelengths
>are not like this. Thus your model is multiply erroneous.

You dont make sense here. For instance your first point.
1."the spectra in other wavelength bands should be stellar spectra"
what does this mean? What spectra? GRB spectra or local stars spectra?
2.You misunderstand my model. How can you critise it if you dont
understand it? For instance you say all the above grb observations
are not the same as star spectra! Of course not. Am I saying that
grbs are local stars! No. I say that grbs
are star spectra from great cosmological distances > 15 −20 billion
years at least that are being redshifted *as we watch.*
So that one minute this distant stars spectra is observable
lets say between 10−70nm and then 10 seconds later its redshifted
so much so that its strecthed to 100−700nm etc. Are local stars spectra
from < 10 billion light years being redshifted from gamma to optical in
minutes as we watch??? No of course not. Thats why grb s look different.,
thats why I call them optical illusions.
So you saying above that grb are different from local spectra only

reaffirms how correct my model is, because thats what I have been
arguing all along. In fact its actually good news in a way that
you above are actually beginning to understand the difference between
light from a nearby star and light from a very distant rapidly
redhifting star(grb) in a non BB non GR universe.
You actually admit now that grbs are not like star sources.
Obviuosly you have studied these embargoed spectra and realize
I`m right.

> >I note that you did not actually read astro−ph/0503508 or
> >astro−ph/0503521, which both show *observed* prompt optical
> >emission for GRBs 990123 and 041219a.
>
> Your two papers speculate that prompt optical emmision may
> have occured but the fact is that there is no observation
> that was made in optical at the time of trigger in gamma.

>This is an erroneous claim. In fact, for GRB 041219a, optical
>observations were taken during the peak of the gamma−ray burst, and
>optical emission was detected (see citations given). Thus, "zero"
>delay between gamma−ray and optical wavelengths is possible.

Yes but was optical seen to peak at the same time as the peak of
gamma?
How many optical observations were made during the gamma observed
part of the burst? Enough to confirm that an observation in
one filter band was seen to decay in other observations in the
same filter band while gamma was still observable?
I dont think so and if you think so supply me with those
gcn because I have already supplied gcn info that shows this is
not an observed fact (that an optical band was seen to increase
and peak and then decrease at the same time as gamma.) No such
data is available ands you cannot
supply any.

>Ironic, since in the case of 041219a, the optical emission peaked at
>the time of the gamma−ray peak. It looks like the preclusion of your
>model is incorrect.

Im not sure if we mean the same thing here or are talking across
each other. If you mean propmt optical emmision is any optical OT
seen while gamma is still being observed then I dont dispute that
nor does my theory. What Im saying is not possible is that in
041219a the optical could not be observed to peak at the same time as
gamma but always later. So it can in a unusually long burst like
041219a , be seen to overlap but never will optical peak at the
same time as gamma. And the observations support my claim.
K was seen to peak AFTER gamma was faded . K is
considered optical I assume so therefore its not
true to say that optical peaked at the same
time as gamma in 041219a. K contradicts this.

> What I am
> doing instead is showing how in a infinite non expanding model
> where c is variable one can model the way emr behaves

>This is again ironic, since you appealed to the authority of more than
>a century of research on electromagnetic research above. And that
>research demonstrates that the speed of light is constant, *not*
>variable.

Irony? Ironic? In fact I`d say its ironic you attribute
a constant c to the theoretical basis of wave only propogation
of light.
Show me one observational example where light as a wave only
was proven to travel at c regardless of the observors speed
relative to the source.

>Are you really claiming that gamma−ray bursts are heavily
>*redshifted*? Since the only way to make gamma−rays from *anything*
>is by blue−shifting, your "model" would have a serious error.

NO. Grbs are not heavily redshifted, they dont exist except at point
of observatuion. GRbs are optical illusions caused by emr that is being
redshifted while we observe .
And there is no restriction on emr in a non gr non bb universe that
prevents light from being first initially observed as heavily blue
shifted.The serious error is yours.
I`ve already excplained this to Bjoern but he was unable to grasp
it.
If in a infinite non GR universe light from a star x travels across
the aether like so in all directions but below is an example of
what happens
in just one of those directions .But x is also travelling left
relative to hypothetical
point z in the aether at 2*c

z <−−− x>>>>>>>light>>>>>>>>>>>
Then lets say 30 billion light years later the light is still
travelling across space like so but x no longer existsbut because of
xs initial speed relaitve to z the light (relative to z is actually
moving backwards
towards z)

z <−−− >>>>>>>light>>>>>>>>>>>
Now imagine that in those 30 years a galaxy with earth formed
that was moving right relative to z, in the aether, at 2* c
z <−−− >>>>>>>light>>>>>>>>>>>
Earth −−−−−>

Its obvious that Earth is moving right
at 2*c and the light from x is moving left at 2*c.,

the net result is that earth sees x`s light backwards
so that after a while the above drawing becomes the below
drawing
z<−−− >>>>>>>light>>>>>>>>>>>
Earth −−−−−>
In other words we see the light from x backwards.
And seeing as the rate between x`s old light and earth is
never possibly constant then Earth either has to be deccelerating
or accelerating relative to x`s light. And following Newton,
I asssume energy is always lost between two `objects` that interact
so it seems only possible that Earth must slow RELATIVE to x`s
light as it travels to the right.
Hence we first see x`s light in gamma and then as we slow down
we see it in longer wavelengths.
You speculate as to how it gets blue shifted?
Well, if earth forms travelling at speeds lets say 100 times
c relative to x then whats the upper limit in an infinite universe.
200* 500* Or infinite *?
And you can never state an upper limit so there will
always be blue shifted light from old stars in a infinite
universe being redshifted into our visible spectrum. *Always*
Thats what grbs are.
And furthermore I speculate that its this emr that IS the aether!
And it is the emr that has slowed down to almost 0 and 0 that is
the atom. The vibrating node in a contained system I argued with
you in the last few years.

>> Redshifts from ground based spectra are always made post burst after
>> the optical peak and at mag similar to the so called host galaxy

>Irrelevant. The imprint of redshifted absorption features will appear
>on both the afterglow spectrum and the host galaxy spectrum (if any).
>See "lyman alpha forest" again.

Really? Then show me a SWIFT spectra taken when the
OT is at peak 15 mag ~ that shows this. You cant because
this evidence either doesnt exist or your team has embargoed the data.

>The spectral features of the earth's atmosphere are well known and do
>not vary in wavelength. Thus, these features can be identified and
>ignored. The careful study of redshifted absorption features has a
>long history and the techniques are well understood.

Have you tried comparing 2003 grbs well known spectra
with earths spectra to see if there are common features
in all different era spectra taken of that grb?
Ive matched up the two and shown how one clearly contaminates
the other. Especially early day spectra that are brighter.
Its on my website page 2

>> How can you even pretend that where there is no data between

>> exposures that the decay rate is a straight
>> line between two adjacent datapoints when in many cases the
>> decay rate between any three exposures is variable.

>Pretense is not needed. Your claim is that the optical light curve
>should be a stretched version of the gamma−ray light curve. In fact,
>most of the light curves shown in the Berger paper are very well
>sampled −− sampled enough to show a stretched version of the gamma−ray
>light curve −− and look nothing like the gamma−ray light curve.

I dont agree . By well sampled I mean every 10−100 seconds not
1 a day.
And exposure times should be 1−10 seconds not 500 as they seem to
usually be.
What happens if in optical it is known already and I predict that
the peak usually occurs over 1−10 hours? Most observations in
any `1 band are made every day or every second day! Youll never
get any detail.,THats why I Have made
such a fuss about SWIFT. Because I assumed it would give
observations in the first few hours in 3 or so different wavelengths
and with each band having at least 5−10 seperate 16mag observations
.. So far it seems unfortunately that SWIFT hasnt done this.
You could though if next bright grb you took rotating exposures of
10 seconds in each band followed by 10 seconds in the next like
so... 10 s U, 10 s B, 10 s V, 10 s, U 10 s, B 10 s, etc
so that in the firts minute you have 2 U, 2 B and 2 V etc etc .
Then co add them all.Then coadd the first 8 V, etc, then co add
the first 4 V etc then the first 2 V etc.
Rather than 200 s U or 500 s V . Its too slow! The movies over
before you even started watching it.

>Your analogy is irrelevant. In fact, the exposure times are short
>compared to the time span of the overall light curves. This is
>especially true on a logarithmic time axis. The optical light curves
>are well enough sampled to detect a stretched version of the gamma−ray
>light curve. With the existing sampling, there is really no way for
>an optical light curve which decays by many orders of magnitude to be
>mistaken for a gamma ray burst light curve.

I dont agree. Where have you an example of a single band, lets say
600nm band where you have 20 seperate exposures of 30 seconds each
made starting 5 minutes post burst with each 20 second exposure
starting immediately after the last? For that matter where have you
any example of the same frequency and exposure length at any time
post burst in one band width?
In fact the only one I know of is one by the AAVSO observor Arno
(ithink his name is or arto)and he did exactly this in two bands
for 303329 and saw a peak clearly in one wavelength
and it appeared delayed by a few minutes in a slightly longer
wavelength observed band.
This observtion has still to be explained by anyone except me.

>Again irrelevant. The whole point is for the IPN to make an
>independent solution with the timing data only. Professional science
>is not elementary school, and there is no need to manipulate the
>results. The physics and geometry that underlie the IPN technique are
>very simple.

OK it shoulnt be a problem for you to give me those 21
grb numbers if you think I`m wrong. Let me analyse them and Ill
show you how the success had larger overlaps than the failures
on average.

>I note your continued lack of substantiation on your manipulation
>claim. You could have analyzed the Laros data to substantiate your
>claims of manipulation, but did not.

>> And as far as I am aware
>> no OT was ever made of a grb till after these dates so how can
>> this paper supply time of arrival locations that can be verified
>> seperately by Optical or radio observations?

>Irrelevant. Both Laros et al. papers show that IPN solutions are
>consistent with GRO error boxes, and show that IPN solutions are
>consistent with each other (in cases where multiple satellites are
>involved). Your "model" would predict neither of those facts.
>The locations of the IPN spacecraft are provided in each Laros et
>al. paper, and they are given in the same coordinate system as the GRB
>localizations. Thus your preconditions have already been met. The
>rest is your problem.

Ive looked at a couple grbs data from one of your cited papers and it
seems there is enough to recalculate annulus . Although it would take
a bit of study or help and probably a powerful computer (seeing as the
98 paper points out the method was initially devised by a Los Alamos team
of physisists presumably using more than an ipod) to correctly
calculate an annulus. I`d like to point out that the 98 paper
actually states that part of the process involves correcting IPN
localizations by EYE(!) to make them work.
(Laros quote)
...."Furthermore, all of the quantitatively obtained results are checked
for compatibility with eye estimates..."

Not what I would call areassuring statement considering I have
always claimed they adjusted the annulus to fit other verifiable
localizations
Anyways seperate from a detailed annulus calculation I found I can
actuallly do a rough check by working out which is the closest satelite
and the farthest of the three by comparing the 3 RA Dec positions of the
3 satelittes and then doing a rough calculation where Ulysses and pvo
are similar to see if Ulysses is closer to the IPN localization. A
couple I tried did roughly affirm the annulus calculations ~.

The problem is, I cant verify if the IPN position is correct without
a small alternative non IPN error box solution or preferably a known
OT location with exact coordinates.
The two papers are pre 96 data only when I believe the first OT
was discovered for any GRB. So for me the papers are useless without
seperate non IPN verifiable localizations to check against.
What I need is as a Laros paper that has satelitte
ephemeris for GRB`s where there are also verified OT localizations
or at least small HETE/ibis type error boxes.
I havent downloaded the 97 paper yet but I imagine its similar
sort of data to 98 and as far as I can see the 98 paper offers
NO seperate error boxes even though it hints that Batse does
give them . Is it because Batse error box/circle was very large
(like 1/2 the sky?)
Anyways as I have mentioned I now can double check whether
an accurate non IPN localization, preferably OT coordinates,
is consistent with time of arrival ephemeris which is probably
more than anyone on this newsgroup is able to do with the possible
exception of you(are you able to calculate this without calculating
an annulus? Can you calculate an annulus?)
So if I can find ephemeris data AND seperate non IPN localizations
for at least 4 grbs,I will be able to settle this .
Sean
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- ***References***:
  - ♦ ***Re: Swift grb satelitte***
    - ◊ *From:* sean
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    - ◊ *From:* Craig Markwardt
  - ♦ ***Re: Swift grb satelitte***
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  - ♦ ***Re: Swift grb satelitte***
    - ◊ *From:* Craig Markwardt

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