Checking IN

43 messages

Kiker, Thaddaeus <s024622@students.lmsd.org> Thu, Sep 17, 2020 at 6:17 PM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi!

Thanks for checking in! Since we last spoke I've been taking a deeper look into a couple articles (Alabarta et al. 2020, Xu et al. 2018, and Parikh et al. 2019) and playing around a bit with the residual fix you suggested. As tested on multiple observations with varying fit statistic contributions in the 1.6-1.8 KeV range (chi-squared contribs. ranging between 500-3000), the gaussian centered around 1.75 KeV combined with the "edge" works great!

Most importantly, I found a method to more accurately investigate the residual features: *plt>wdata*. Now that I can actually see what the chi-sq contrib. is for every point, I've been having a lot more success in approaching residuals in the 1.6-2.2 KeV range, especially when the residual patterns after 1.8 KeV differ greatly from what can quickly be ameliorated with the edge feature (it seems that for the majority of observations the <1.8 KeV excess is easily subdued by the gaussian fix, but after 1.8 KeV things often get complicated, especially when a third feature shows up between 1.86 to 2.0 KeV. This third feature resembles a sort of gaussian-like crevice about 0.13 KeV in width and often reaches a chi^2 contribution of -5331 (+) at its minima, pushing the "edge" feature out to a threshold energy of ~2+ KeV). I haven't found a definitive solution yet, but a gaussian absorption with an energy of ~1.86 KeV and a width of ~0.14 KeV is looking promising.

While I've been working with these MAXI J1535-571 residuals I've been wondering if the residuals Alabarta et al. discuss in Nicer data for MAXI J1727–203 are of similar shape—is there any chance I could procure some pipeline data from you on MAXI J1727-203 to look into this (if it's not an inconvenience for you and or you don't think it's a distraction on my part of course).

Out of curiosity, what does "parameter degeneracy" mean? My first guess would be this example: if I only told you what the slope of a line was (e.g. '2'), and then asked you for the individual values of Δy and Δx—would Δy and Δx be degenerate? Example of where I came across this: (from Xu et al. 2018) "The inner disk radius, R_{in}, and the black hole spin parameter, a*, are degenerate, as they both control the effective inner accretion disk radius. Therefore, for simplicity, during the spectral fitting we assume that the inner disk extends down to the ISCO by fixing R_{in} at the radius of the ISCO, and fit for the black hole spin as a free parameter."

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

Steiner, James <james.steiner@cfa.harvard.edu> Thu, Sep 17, 2020 at 7:59 PM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hi Thaddaeus,

Great to hear you've been digging in and exploring! I would suggest though that you avoid being too concerned about the residual features. If something is localized and only ~percent level deviation, then that's likely sufficient. *The goal should be to fit those sufficiently that they don't alter the continuum, but at some level it becomes diminishing returns to try fitting an artefact.* That said, if you wish, I can get you the data on MAXI J1727-203. Just let me know if you have a particular ObsID from Kevin's paper you want to try.

Your question about degeneracy is great. The condition of parameters being strongly correlated with one another is degeneracy. Basically, this is a very common situation where two parameters are dependent upon one another to some extent, so a probable value of one will condition a probable range of values with the other. For instance, if you want to fit x and y with the constraint x^2+y^2 = 4+/-1, you would get a ring of radius 2 with x,y values that obviously depend on one another; they are *not independent*. Speaking generally for more realistic cases, the interaction between parameters is often pretty convoluted, but commonly things like Gamma and Normalization are correlated with one another, so a contour plot of the confidence interval would look banana shaped rather than like a circle.

Best,

-Jack

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Kiker, Thaddaeus <s024622@students.lmsd.org> Fri, Sep 18, 2020 at 5:00 AM

To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi!

That's good to know. I guess what's surprising is some relative of this large residual in the 1.6-2.0 KeV is present in most of the source flux > 75% observations (I wonder if the peak feature falsely inflates the source flux for some of them?). Going back to a couple emails ago, I was checking some of my all time best fits which had source fluxes around ~4%—what allows such faint observations to fit well?

Also, I'm very intrigued by your methods in one of your 2016 papers , "STRONGER REFLECTION FROM BLACK HOLE ACCRETION DISKS IN SOFT X-RAY STATES". How did you automate fitting the 15,000 observations reliably and within a reasonable time frame?

Could I possibly have the ~86 observations between obsids 1200220101 – 1200220186?

Ah that makes sense. In that case did Xu et al. just freeze Rin at the ISCO to have less free parameters?
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Kiker, Thaddaeus <s024622@students.lmsd.org> Tue, Sep 22, 2020 at 9:14 AM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi Dr. Steiner!

Just wondering about the status of the MAXI J1727-203 data, I'm ready to keep working.

Thaddaeus

P.S. I'm taking the SAT Saturday, wish me luck!
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Steiner, James <james.steiner@cfa.harvard.edu> Tue, Sep 22, 2020 at 9:39 AM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hi Thaddaeus,

> That's good to know. I guess what's surprising is some relative of this large residual in the 1.6-2.0 KeV is present in most of the source flux > 75% observations (I wonder if the peak feature falsely inflates the source flux for some of them?). Going back to a couple emails ago, I was checking some of my all time best fits which had source fluxes around ~4%—what allows such faint observations to fit well?

Just to clarify, do you mean 75% and 4% of the Eddington limit, peak luminosity, or something else? Fainter observations, given some typical observation time, obviously have fewer counts which means larger fractional errors and less constraining power on the model. So if there is a deficiency in the calibration or model being used, it will look more prominent in brighter + long exposure time data.

> Also, I'm very intrigued by your methods in one of your 2016 papers , "STRONGER REFLECTION FROM BLACK HOLE ACCRETION DISKS IN SOFT X-RAY STATES". How did you automate fitting the 15,000 observations reliably and within a reasonable time frame?

A reasonable time frame is relative :), but I used about two dozen cores to run the fits which helped. I also stuck with computationally simplistic models, and for my runs with simpl, used lower resolution (via energies) in the run, since RXTE/PCA - the instrument used there - also has rather poor energy resolution compared to NICER.

> Could I possibly have the ~86 observations between obsids 1200220101 – 1200220186?

Spectral files are attached. Response files are too big for me to attach, so I'll keep them up here for a few days: https://nicer-web.mit.edu/nicerview/thadd_resp_m1727.tar.gz

> Ah that makes sense. In that case did Xu et al. just freeze Rin at the ISCO to have less free parameters?

That's the usual tactic, yes, when fitting for spin.
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thadd_maxi1727.tar.gz
7877K

Kiker, Thaddaeus <s024622@students.lmsd.org> Tue, Sep 22, 2020 at 10:04 AM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi!

Thanks so much!

For >75% I meant observations where source flux was more than 75% bg flux, and for 4% I meant observations where source flux was ~4 % bg flux. That makes sense—interestingly the observations with source flux ~4% bg flux were, by best fit, in the SPL state, with the highest flux and gamma for all the observations that had reasonable values for these parameters.

xD. I think it's a very impressive feat! Did you use pyxspec to loop through all the observations?

Many thanks!
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Steiner, James <james.steiner@cfa.harvard.edu> Tue, Sep 22, 2020 at 10:09 AM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hi Thaddaeus,

> For >75% I meant observations where source flux was more than 75% bg flux, and for 4% I meant observations where source flux was ~4 % bg flux. That makes sense—interestingly the observations with source flux ~4% bg flux were, by best fit, in the SPL state, with the highest flux and gamma for all the observations that had reasonable values for these parameters.

Ah, now I see. Yes, when the bg flux is very high, the noise level (N=sqrt(B+S)) where B and S are background and signal counts in a channel, then the constraining power of the data (S/N) is lowest. If the background is reasonably estimated, the fits will likely be good but poorly constrained; if the background is off, then all bets are off.

> xD. I think it's a very impressive feat! Did you use pyxspec to loop through all the observations?

Thanks. I used a different language actually, but python/pyxspec would be an equally good way to do it.

Best,

-Jack
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Kiker, Thaddaeus <s024622@students.lmsd.org> Tue, Sep 22, 2020 at 10:12 AM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Sounds good!

In that case did you operate solely in TCL?
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Steiner, James <james.steiner@cfa.harvard.edu> Tue, Sep 22, 2020 at 10:15 AM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Similar to that. I used another language (IDL) to automatically produce a series of tcl/tk scripts and then executed those distributed over a number of cores on a workstation.

-Jack
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Kiker, Thaddaeus <s024622@students.lmsd.org> Tue, Sep 22, 2020 at 10:21 AM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Interesting I'll look into that!
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Kiker, Thaddaeus <s024622@students.lmsd.org> Mon, Sep 28, 2020 at 6:16 AM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi Dr. Steiner!

Quick note on the residuals: since we last communicated I investigated the application of a systematic error (3-5%) in the < 2 KeV energy band, but this didn't improve the residuals so I'm assuming that you already accounted for this, like the binning, in your reprocessing? I also discovered that I've been unintentionally forcing an incorrect response file on multiple MAXI J1535-571 observations—the rsp I was using for all observations was `nicer_d49_55575341.rmf`, but after running a quick check (the fitsio package has been so helpful, thanks for recommending it to me!), I found that multiple observations request `nicer_d49_u100_150_55575341.rmf` in their headers.

I'm likely almost done with the SAT (took it Saturday, going to take it again this coming Saturday)*, and once I'm done I aim to return to a progress pace similar to the pace we had in the summer.

G'mar chatima tova (if you observe Yom Kippur)!
Thaddaeus

*I might take it again in November in the event that I bungle those tests.
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Steiner, James <james.steiner@cfa.harvard.edu> Mon, Sep 28, 2020 at 11:19 PM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hi Thaddaeus,

Thanks for the update.

> Quick note on the residuals: since we last communicated I investigated the application of a systematic error (3-5%) in the < 2 KeV energy band, but this didn't improve the residuals so I'm assuming that you already accounted for this, like the binning, in your reprocessing?

No, actually. But systematic errors are only incorporated with some fit statistics - chisq for instance, but I don't believe are incorporated in cstat or pgstat. The way I like to think about it is the systematic error acts as a reweighting of the data's constraint and serves to de-emphasize regions with the highest signal. I offer for information sake that that 3-5% is too large a systematic error to include as a rule. Something like 1-2% is more reasonable for a region with poorer calibration quality.

> I also discovered that I've been unintentionally forcing an incorrect response file on multiple MAXI J1535-571 observations—the rsp I was using for all observations was `nicer_d49_55575341.rmf`, but after running a quick check (the fitsio package has been so helpful, thanks for recommending it to me!), I found that multiple observations request `nicer_d49_u100_150_55575341.rmf` in their headers.

Ah! That's very good to know, and may be the culprit for some of the residuals you've been battling. Check that the arf files (keyword ANCRFILE) is also set correctly.

> I'm likely almost done with the SAT (took it Saturday, going to take it again this coming Saturday)*, and once I'm done I aim to return to a progress pace similar to the pace we had in the summer.

Congratulations - that's a nice milestone!

> G'mar chatima tova (if you observe Yom Kippur)!

Same to you.

> *I might take it again in November in the event that I bungle those tests.

I'd put dollars to donuts you did great!

My best,
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Kiker, Thaddaeus <s024622@students.lmsd.org> Thu, Oct 8, 2020 at 10:35 AM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi Dr. Steiner!

Quick note: I've been getting back into it this week and should have some interesting things to report this weekend!
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Kiker, Thaddaeus <s024622@students.lmsd.org> Fri, Oct 9, 2020 at 7:23 PM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi Dr. Steiner!

Regarding the application of systematic errors, that's good to know. I've often seen something like the "NICER team recommends the application of a systematic error of 1%" in papers that use NICER data.

I love the common.tcl file, it's so handy! I think I'm just going to use it from now on because it's simpler on the execution end (just two commands), and it shows the error strings for each parameter.

A little random: I came across an interesting statement in a paper by Plant et al. 2015 pertaining to observed disk truncation in the hard state of GX 339−4: “This result implies that the current sample of spin estimates in the hard state are inaccurate. Therefore, any distinct conclusions drawn from these estimates, such as the spin-powering of relativistic jets, may well be biased.” Since that paper was published has this issue been fully exploited and resolved, or are there still black holes with possibly incorrect spin measurements out there? Also, is physical disk truncation or changing coronal height the more widely accepted cause for profiles similar to disk truncation?

An update on the rsp/arf issue: I modified my approach and fully integrated astropy.io.fits, so when I’m producing my xspec script commands rmf and arf file names are pulled directly from the header of each observation’s fits file. Because of the two errors described below, I haven't checked how chi-square improves when the correct rmf and arf files are used though.

I’ve encountered two weird errors I’ve never encountered before in XSPEC this week: the first is frequent core dumping [SVD decomposition failed to converge…Default GSL error handler invoked…Segmentation fault (core dumped)], and the second is this odd “XSPEC Error: No variable parameters for fit” after executing “fit” for a model with *multiple* free parameters (including the usual Gamma, FracSctr, Tin, diskbb norm, etc.). My first thought was “*is this related to relxill having problems due to a low gamma*”—but these issues both happen even when Gamma is > 2.

Also a little random: this week I decided it was time to start commenting on my functions in my python files—at one point I was looking through some older files for a particular technique I used and it took a little while to find the correct version of it, so I decided that from now on I’m describing my functions for future reference.

This is preliminary, but I attached an example of a chi-scape scarred by a common morph of the residual I last sent you. The positive contribution that begins shortly after 1.6 KeV is almost always eliminated by a ~1.75 KeV gaussian of varying width for most observations. The current issue is modeling the various forms the following negative residuals take. Currently working on those. Earlier you mentioned that we could try ignoring that narrow range where these residuals occur, would ignoring channels between 1.6-2.2 KeV be ignoring too much of the data? If we decide to ignore that range, I would think it best to get >*plt wdata* outputs to exactly nail down the dimensions of this range, but for now should I keep trying to model the residuals?

I was reading through some older emails, going to look into your recommendation regarding logxi.

Excited to get back in!

Thaddaeus

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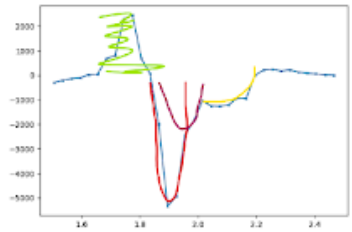
2 attachments

comments.png
170K



chiscape.png
38K

Steiner, James <james.steiner@cfa.harvard.edu> Thu, Oct 15, 2020 at 7:16 AM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hi Thaddaeus,

> Regarding the application of systematic errors, that’s good to know. I’ve often seen something like the “NICER team recommends the application of a systematic error of 1%” in papers that use NICER data.

Yes, that's a "rough" benchmark, but a reasonable one.

> I love the common.tcl file, it's so handy! I think I'm just going to use it from now on because it's simpler on the execution end (just two commands), and it shows the error strings for each parameter.

Yes, it's quite nice work by Jeremy :).

> A little random: I came across an interesting statement in a paper by Plant et al. 2015 pertaining to observed disk truncation in the hard state of GX 339−4: "This result implies that the current sample of spin estimates in the hard state are inaccurate. Therefore, any distinct conclusions drawn from these estimates, such as the spin-powering of relativistic jets, may well be biased." Since that paper was published has this issue been fully exploited and resolved, or are there still black holes with possibly incorrect spin measurements out there? Also, is physical disk truncation or changing coronal height the more widely accepted cause for profiles similar to disk truncation?

This has been a topic with a lot of controversy that is still ongoing (Garcia et al 2015 and followup papers by Wang et al. 2019 talk about this). I'd say the Plant picture is strongly disfavored at present, but not entirely out. The robust view is that allowing for the possibility of truncation, a (bright) hard-state spin measurement should be regarded as an estimated lower limit. Presently, the argument is focused around how much truncation could there be in bright hard states, with most reasonable numbers being a few times the ISCO (a factor <10). (Coronal geometry is always an important consideration, but usually they tend to fit very compact.)

> An update on the rsp/arf issue: I modified my approach and fully integrated astropy.io.fits, so when I'm producing my xspec script commands rmf and arf file names are pulled directly from the header of each observation's fits file. Because of the two errors described below, I haven't checked how chi-square improves when the correct rmf and arf files are used though.

Very good, that should make some impact when working.

> I've encountered two weird errors I've never encountered before in XSPEC this week: the first is frequent core dumping [SVD decomposition failed to converge...Default GSL error handler invoked...Segmentation fault (core dumped)], and the second is this odd "XSPEC Error: No variable parameters for fit" after executing "fit" for a model with *multiple* free parameters (including the usual Gamma, FracSctr, Tin, diskbb norm, etc.). My first thought was "*is this related to relxill having problems due to a low gamma"*—but these issues both happen even when Gamma is > 2.

The table can be sparse in a few places, and I think very large Gammas are also sparse (>~3.4, if memory serves). I'd say try freezing parameters and retrying the fit to identify the culprit parameter.

> Also a little random: this week I decided it was time to start commenting on my functions in my python files—at one point I was looking through some older files for a particular technique I used and it took a little while to find the correct version of it, so I decided that from now on I'm describing my functions for future reference.

Yes, documenting code is super important and a great habit to get used to. (I wish I was better about it!)

> This is preliminary, but I attached an example of a chi-scape scarred by a common morph of the residual I last sent you. The positive contribution that begins shortly after 1.6 KeV is almost always eliminated by a ~1.75 KeV gaussian of varying width for most observations. The current issue is modeling the various forms the following negative residuals take. Currently working on those. Earlier you mentioned that we could try ignoring that narrow range where these residuals occur, would ignoring channels between 1.6-2.2 KeV be ignoring too much of the data? If we decide to ignore that range, I would think it best to get >*plt wdata* outputs to exactly nail down the dimensions of this range, but for now should I keep trying to model the residuals?
>
> I was reading through some older emails, going to look into your recommendation regarding logxi.

No, I think 1.6-2.2 isn't crazy and you can certainly roll with that if you are having too much trouble taming the residuals with a model. Maybe do a couple test cases to see how the model parameters are affected on both fronts.

Hey, and I didn't see a description of your attachments in case you want to say anything about those.

Glad you're back in the thick of things.

Best,

-Jack

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Kiker, Thaddaeus <s024622@students.lmsd.org> Fri, Oct 16, 2020 at 12:23 PM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi!

Now that I think about it, I do recall noticing that many (if not most) of the fit failures had gammas in the 3.4 range. Good to know about the 1.6-2.2 range for the residuals, I'll play with that now.

Oh about the attachments: the screen shot from visual studio is just some descriptions I was writing about the different functions I was using, and the microsoft paint one just illustrates the positive contribution that is almost always eliminated by a ~1.75 KeV gaussian of varying width.

Over this weekend should I continue to focus on maximizing the number of observations that I can get good fits and confidence intervals for?

I'm intrigued by this disk truncation controversy, I'm going to read about it this weekend.

Many thanks!
Thaddaeus

P.S. I was reading about some dangerous linux commands the other day; there are some unnerving commands out there. Imagine if someone ran rm -rf in your terminal. Shudder.
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Steiner, James <james.steiner@cfa.harvard.edu> Thu, Oct 22, 2020 at 7:51 AM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hi Thaddaeus,

Just checking in to see how fits are progressing and if playing with those limits has allowed any headway against the crashes?

(Yes, whenever I "rm -rf " I try to take beat and check that I am not about to bilk the whole machine! With great commands-line tools comes great ...)

-Jack
[Quoted text hidden]

Kiker, Thaddaeus <s024622@students.lmsd.org> Thu, Oct 22, 2020 at 8:55 AM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi!

I just got some really great news on the crashes!

Last weekend I was dismayed because even though I was playing with the limits extensively (especially for gamma), the crashing was still occurring unabated. This made me wonder if it was a software issue, so I reached out to the HEASARC Help Desk. I found out that a very similar issue was fixed in XSPEC patch 12.11.0k: *For some complex fitting and error command cases, XSPEC may terminate with a GSL message such as 'SVD decomposition failed to converge'.* This wasn't encouraging at first because XSPEC patch 12.11.0k came out in June (I built the current version of xspec on my vm in ~July), but I checked my XSPEC version with *$version,* and it turns out that my XSPEC version is just 12.11.0! I'm working on installing the patch, I bet this will clear a lot of things up. I think I'm going to have to rebuild relxill after I execute the partial system rebuild.

The only thing that confuses me about this is that I didn't really have issues with core dumping when I was fitting relxill a lot back in August (I think it did come up for error fits back then though), so I'm confused why it really started acting up recently.

Also I don't think I've asked this previously, but out of curiosity, should I expect significant reductions in fit times with two core parallel processing?
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Steiner, James <james.steiner@cfa.harvard.edu> Thu, Oct 22, 2020 at 9:46 AM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hi Santi,

OK, that's encouraging! Let me know if the patching does the trick. Often for small patches, you won't need to recompile local models, but you should quickly see if it has trouble loading and that would be your sign.

It's hard to predict how parallel processing will impact a given task. The outcome depends on where the bottlenecks occur (if processing, then expect significant improvement, if memory swapping, then probably not). For running on multiple threads, a **rough** estimate is sqrt(N) gain, but that's just ballpark.

-Jack
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Kiker, Thaddaeus <s024622@students.lmsd.org> Thu, Oct 29, 2020 at 8:33 AM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi Dr. Steiner!

Alright I think that patching did the trick; I'm currently running XSPEC v 12.11.0m. Surprisingly I didn't have to rebuild relxill which was nice. Now that the 12.11.0k patch has been installed, I think XSPEC just "snags" and keeps fitting if I have query set to yes instead of crashing—when I was looking into this, a particular observation ran an error routine for three days nonstop in the background until I aborted it!
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Steiner, James <james.steiner@cfa.harvard.edu> Thu, Oct 29, 2020 at 9:48 AM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hi Thaddaeus,

Whoops - Santiago is my grad student and I notice my advisor brain accidentally called you Santi in my last message. Very sorry about that!

Name confusions aside, very glad the patch worked ok. Yeah, setting query to yes can cause problems sometimes as you found. I don't have good rules for avoiding or escaping those runaways, but I've had luck before tweaking the extra convergence terms in the fit command when it's stuck (the delta & beta) and the tolerance term in error.

-Jack

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Kiker, Thaddaeus <s024622@students.lmsd.org> Thu, Nov 5, 2020 at 12:30 PM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi Dr. Steiner!

No worries!

> I've had luck before tweaking the extra convergence terms in the fit command when it's stuck (the delta & beta) and the tolerance term in error.

Thanks for letting me know about this; I'm going to play with these during my next major fitting routine.

I've held off from running another major fitting routine since the weekend because after I ran that my last one I decided that it was time to write some standardized scripts for wrangling the results from all the log files. Up to this point I've been cobbling new scripts together for essentially every major fitting routine because in each fitting routine file paths, models, etc. are different. The scripts for wrangling the actual fit results aren't too different, but the script I was using to wrangle the error results with was pretty bad in general, so when I decided I was going to standardize these operations I also decided that I should just go ahead and write an improved error wrangler (one of the reasons why there was such a small number of points with error bars in that evolution plot I sent you a while back—in addition to the overly strict reduced pgstat standards—was the base script I was using to analyze the error log files wasn't able to handle all the different results that can happen (e.g. parameter getting pegged at hard upper limit and having an upper value of zero, parameter having invalid lower limit, parameter getting pegged at lower hard limit, and on occasion error still collapsing for a parameter or two in a small number of fits)).

Speaking of which, when Cuneo uses green triangles for "*parameters consistent with 0 within errors*", does this refer to situations when either the upper value of a parameter is equal to zero (because it got pegged at the hard upper limit) or when the lower limit is invalid and equal to zero?

The standardized script for wrangling the fit results—and by quick modification a standardized script to return observation dates for each data file—didn't take much time to finish and they're collectively attached as functions in the XSPECtools.py file. The error investigator script has proven to be a little more intense so I haven't fully finished it but it's nearly complete.

Here's some descriptions of the two standardized functions:

```
def returnFitResults(IDs,pathtemplate,fitstatistic,fitparameters,listnames):
```
Argument definitions:

- *IDs :*should be a list of ID strings, e.g. IDs=['1130360183','1130360184', '1130360185'].
- *pathtemplate:* should be a string that serves as the example path template for all the log files, where any ID instance is replaced with the string '+++++++++', so when iterating through the list of IDs, the function replaces '++++++++++' in the path template with the ID, and opens the file at the modified path.
- *fitstatistic*: only 'pgstat' can be passed through fitstatistic right now (since I've only been using it for my fit statistic), the reason being lines which describe the fit statistic and degrees of freedom are uniquely separated with pgstat (the index of the line that reports the model's dof is always +3 from the index of the line that gives the pgstat), and I'm pretty sure this line difference is going to be different for cstat or just chi-sq.
- *fitparameters* should be a list, but the format of each item in the list is very specific. An example parameter format: 'Gamma:simpl:5' , where the format is '[Parameter name]:[additional keywords that should be in the line]:[index of parameter value in line]'. The index of the parameter value in line refers to this: when both keywords are found in a line (this can only happen once), I use the re module to replace all instances of whitespace in the line with a single comma,* and then the line is split on ',' into a list. For every parameter there is a unique index in this 'linelist' that its fitted value is located at. Hence, fitparameters could be defined like this: fitparameters=['Gamma:simpl:5' ,'FracSctr:simpl:5','Tin:diskbb:6']. As you can see in the code, the script splits each element in the fitparameters list into three components (on ':'), and uses the first to find a candidate line, the second to confirm that it's the unique line, and the third to locate the parameter value. I *really* like this particular aspect of the function.

- *listnames* should also be a list, but the items should be list variable names that are defined prior to executing the function, and are not kept as strings in the list. For example, if I wanted to recover all the fitted gamma values, I would predefine a list as, say *Gammas*, and then pass that variable name into the list for listnames, e.g. listnames=[Gammas]. The important thing about the list names list is that the parameter lists should be in the same number that their parameter numbers appear in the fitparameters list (e.g. if Gamma is the fifth parameter in the fit routine's model, then the list index for 5 in fitparameters should be the same as the list index for the listname *Gammas* in listnames).

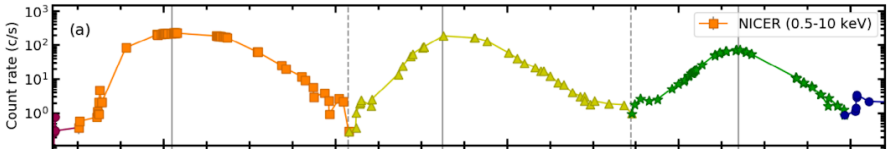
This function also returns the reduced pgstat values for each fit (by creating a globally accessible list created within the function—I'm going to change that to an argument defined list).

```
def returnDates(IDs,pathtemplate,listname):
```
Argument definitions:

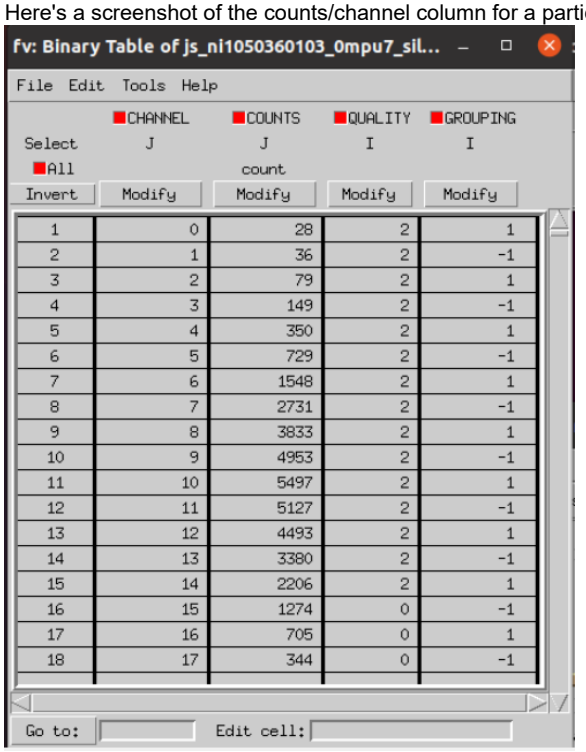
- *IDs*: same as aforementioned.
- *pathtemplate*: ditto.
- *listname*: should be a variable name which is the list into which dates should be deposited by the function.

This function was very easy to standardize, the only thing that needed to change was incorporating the flexible path template functionality.

Out of curiosity, where did Cuneo get the count rate values for his data? I don't think it's the same as the Net Count Rate that's displayed whenever you execute *$show data* in XSPEC....can Cuneo's count rate (c/s) be found by dividing the sum of the counts in the included channels by the total observation time?



Here's a screenshot of the counts/channel column for a particular data file:



1	0	28	2	1
2	1	36	2	-1
3	2	79	2	1
4	3	149	2	-1
5	4	350	2	1
6	5	729	2	-1
7	6	1548	2	1
8	7	2731	2	-1
9	8	3833	2	1
10	9	4953	2	-1
11	10	5497	2	1
12	11	5127	2	-1
13	12	4493	2	1
14	13	3380	2	-1
15	14	2206	2	1
16	15	1274	0	-1
17	16	705	0	1
18	17	344	0	-1

Also sorta random, I came across a value called "quality" when I was looking at a file in fv; are the quality values the values XSPEC refers to when I say *$ignore bad*?

```
TUNIT2  = 'count    '           / physical unit of field
TTYPE3  = 'QUALITY  '           /   Quality flag of this channel (0=good)
TFORM3  = 'I        '           / data format of field: 2-byte INTEGER
TTYPE4  = 'GROUPING'            / Grouping flag for channel (0=undefined)
TFORM4  = 'I        '           / data format of field: 2-byte INTEGER
```

Cheers!
Thaddaeus

* This is probably one of my favorite one-liner functions in Python:

```
for line in f:
   linelist = (re.sub(' +', ',', line)).split(',')
```

Where f is a file opened with: *with open(...) as f.*

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XSPECtools.py
3K

Steiner, James <james.steiner@cfa.harvard.edu> Wed, Nov 11, 2020 at 9:07 AM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hey Thaddaeus,

What a week it's been eh? (But way to go PA!)

> I've held off from running another major fitting routine since the weekend because after I ran that my last one I decided that it was time to write some standardized scripts for wrangling the results from all the log files.

Excellent!

> Speaking of which, when Cuneo uses green triangles for "*parameters consistent with 0 within errors*", does this refer to situations when either the upper value of a parameter is equal to zero (because it got pegged at the hard upper limit) or when the lower limit is invalid and equal to zero?

Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).

I *love* the outline of your routines, and "wow" is what I have to say about that. Way to go Thaddaeus, it's great you're tackling this and in a very useful manner.

> Out of curiosity, where did Cuneo get the count rate values for his data? I don't think it's the same as the Net Count Rate that's displayed whenever you execute *$show data* in XSPEC....can Cuneo's count rate (c/s) be found by dividing the sum of the counts in the included channels by the total observation time?

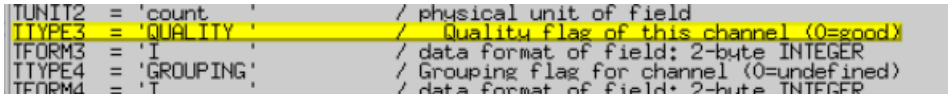
About 99% yes. "Usual" practice for NICER has become to normalize to an active set of 52 detectors. Your spectrum is a collection of all good events. So a tally of all the (good) events between 0.5-10 keV divided by the exposure time is indeed the count rate detected. However, if only using 49 or 50 detectors (also common), one would scale up by 52/49 to show the rate on the full instrument.

The one very tiny wrinkle to all this is a property called "deadtime". Each time the electronics on a detector register an X-ray event, they "latch" meaning that the detector stops listening for new signals. Effectively, the detector is turned off for a short amount of time it uses to calculate properties of event (what time it arrived, and how much charge was deposited). For NICER, this is typically ~20 microseconds.

As a concrete example of what this means in practice: when the count rate per detector reaches ~10^3/s (or in other words the full instrument light curve was at ~50,000/s), there would be ~20e-6 * 1e3 = 2% deadtime. This would mean that the *actual* count rate is ~2% higher than was detected (because the true exposure time was actually 98% the clock time). Here, most likely Cuneo didn't worry about folding in deadtime effects (I do have my pipeline correct for it automatically, so all your spectra already factor that in).

This is obviously a very small here for NICER, but I wanted to dive into it because for many missions that aren't timing beasts like NICER, deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.

> Also sorta random, I came across a value called "quality" when I was looking at a file in fv; are the quality values the values XSPEC refers to when I say *$ignore bad*?



Yes exactly this. This won't tend to come up for NICER, but e.g., for instruments that have some bad pixels and a spatially dispersed spectrum (think the rainbow from a prism), or uncalibrated energy ranges, this can be used.

> * This is probably one of my favorite one-liner functions in Python:
>
> for line in f:
> linelist = (re.sub(' +', ',', line)).split(',')
>
> Where f is a file opened with: *with open(...) as f.*

Python is a wonder for its very efficient one-lining loops!

As usual, you're knocking it out of the park Thaddaeus.

In other news, a new grad student (Santi) just joined me and he's going to start looking at some QPO data.

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Kiker, Thaddaeus <s024622@students.lmsd.org> Wed, Nov 18, 2020 at 4:14 AM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi Dr. Steiner!

> What a week it's been eh? (But way to go PA!)

It was a wild week indeed—and things seem to keep getting crazier. We were in a hybrid model since October 19th, but as of Tuesday we went indefinitely back to all online which is a two sided coin. On one hand the online schedule gives me a lot more free time, but on the other I'm going to miss the extra class time in my harder classes like mechanics and calc. Thanksgiving is around the corner though! Up to any interesting plans yourself? I think we're just going to have my mom's parents over because we already interact with them frequently, but no big gathering this year (obviously).

> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).

Would it be okay if I emailed Dr. Cuneo about this? This is very important for how I structure the confidence intervals code (and whether I make it more sensitive and able to detect more than the four generalized abnormal scenarios described later in this email).

> I *love* the outline of your routines, and "wow" is what I have to say about that. Way to go Thaddaeus, it's great you're tackling this and in a very useful manner.

Thanks so much!

> For NICER, this is typically ~20 microseconds.

Haha! I just read that NuStar's is ~2.5 *milliseconds.*

> I do have my pipeline correct for it automatically, so all your spectra already factor that in.

Cool! Does that mean I can just go ahead and calculate count rates as number of counts / exp. time and not worry about adjusting the result because you've already accounted for deadtime?

> deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.

Yes. This makes sense. I feel like I've seen something related where the center of an object was too bright in the X-Ray band (which oversaturated the detector?) so the color gradient of brightness was black in the center because the author's couldn't determine the brightness at the center. It kinda looked like an artificial coronagraph. Would that be related to a situation where the dead time was more than the event time?

> Yes exactly this. This won't tend to come up for NICER, but e.g., for instruments that have some bad pixels and a spatially dispersed spectrum (think the rainbow from a prism), or uncalibrated energy ranges, this can be used.

I'm pretty sure the RXTE data I was working with earlier this year didn't have this identifier because if I remember correctly I couldn't execute *$ignore bad* on the data in XSPEC.

> As usual, you're knocking it out of the park Thaddaeus.

:)))

> In other news, a new grad student (Santi) just joined me and he's going to start looking at some QPO data.

My salutations to Santi!

Here's a working version of the confidence interval / error results script:

```
def returnConfidenceIntervals(IDs,pathtemp,pnumbers,listnames):
```

The function can currently recognize four abnormal results: a parameter with a lower bound that makes it consistent with zero (PHLZ - Parameter Hit Lower Zero), a parameter with a higher bound that makes it consistent with zero(?)(PHUZ - Parameter Hit Upper Zero), a parameter where both of these cases are true, and a parameter for which no confidence interval was found (e.g. when error crashes and no result is logged). One thing I like about the function is its ability to handle multiple confidence intervals for a single parameter (when error starts over again and returns a new confidence interval). It searches through the files for modified lines which have their zero-th index equal to the string of the pnumber item (e.g. if gamma was the fifth fit parameter and I ran error on it) and appends the results of these lines—with flags in the appended text depending on if any of the aforementioned scenarios are true—to a temporary list which resets to temporarylist = [] for every element in pnumbers. Then there's a conditional loop which tries to return the first confidence interval for which there are no error flags (then break), but if there are none it returns the first confidence interval string from the list and breaks. The confidence interval strings are formatted

like this: *obsid:lower_error:upper_error,* with the obsid so I can exactly match confidence intervals to their fit results (because I think it wouldn't be smart to just match them by list index).

Some argument definitions:

- *IDs*: Same as usual.
- *pathtemp*: same as pathtemplate in the other functions.
- *pnumbers:* This is a list of integer parameter number values for which confidence intervals should be returned from the log files. Example: *pnumbers=[5,6,8]*.
- *listnames:* This should be a list of the names of the lists into which the confidence interval strings should be deposited. Because of the way I wrote the function, the elements in this list should correspond, in indice order, to the parameters in *pnumbers.* So for example if I was finding the confidence interval results for gammas, scatter fractions, and disk temps, I'd predefine the lists as gammas = [], etc. then order those lists in *listnames* according to how their parameter values are ordered in *pnumbers*.

I put my XSPECtools.py file which has all of these functions into a custom module in my local site-packages folder for python and it seems like it's working perfectly. I import the file like this: *from personalastropy.xspectools import XSPECtools as xt*, and I execute a particular function like this: *xt.returnFitResults().*

Happy early Thanksgiving!
Thaddaeus

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XSPECtools.py
6K

Steiner, James <james.steiner@cfa.harvard.edu> Wed, Nov 18, 2020 at 7:05 AM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hi Thaddaeus,

> It was a wild week indeed—and things seem to keep getting crazier. We were in a hybrid model since October 19th, but as of Tuesday we went indefinitely back to all online which is a two sided coin. On one hand the online schedule gives me a lot more free time, but on the other I'm going to miss the extra class time in my harder classes like mechanics and calc. Thanksgiving is around the corner though! Up to any interesting plans yourself? I think we're just going to have my mom's parents over because we already interact with them frequently, but no big gathering this year (obviously).

No kidding! Feels like we're living in a bad TV show.

Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.

A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> Would it be okay if I emailed Dr. Cuneo about this? This is very important for how I structure the confidence intervals code (and whether I make it more sensitive and able to detect more than the four generalized abnormal scenarios described later in this email).

Sure thing, email away.

> Haha! I just read that NuStar's is ~2.5 *milliseconds.*

Yeah and that's actually not too bad compared to most things out there. NICER is just an absolute timing monster.

> I do have my pipeline correct for it automatically, so all your spectra already factor that in.
>
> Cool! Does that mean I can just go ahead and calculate count rates as number of counts / exp. time and not worry about adjusting the result because you've already accounted for deadtime?

Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

> deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.
>
> Yes. This makes sense. I feel like I've seen something related where the center of an object was too bright in the X-Ray band (which oversaturated the detector?) so the color gradient of brightness was black in the center because the author's couldn't determine the brightness at the center. It kinda looked like an artificial coronagraph. Would that be related to a situation where the dead time was more than the event time?

(Deep dive warning:)
Yes, you're speaking about "pileup" which isn't exactly related to deadtime except that both can be important when the source is bright. Deadtime, again, is from the detector shutting down for some portion of the time. Pileup concerns are most often for an imagng type of detector - more like the camera on your phone - that collects an exposure for some amount of time and then read it off.

Chandra, XMM-Newton, Swift, Suzaku, (somewhat confusingly NuStar is in both camps), let X-rays come in from the sky, get focused onto the CCD camera, and then in some time interval, often ~1s, reads off the image and clears the CCD. The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon. So when an individual pixel gets << 1 X-ray per frame on average, you are in good shape. *But* when there is significant probability of getting >~1 X-ray per frame on a given pixel, then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

This registering of multiple X-rays incorrectly as one more energetic single event is pileup, and it is the bane of most imaging detectors for those of us working with bright sources. (Pileup can occur in instruments like NICER too, when two X-rays hit during the event-latch phase, but this is something like a <1% effect for a 100 Crab-bright source, so well in the camp of being negligible for all practical purposes.)

In the most extreme cases of pileup, the pixel digitizer, which usually records values on something like a 16-bit value (e.g., from -32768:32767), can roll over and then appears negative when too much charge hits at once.

> I'm pretty sure the RXTE data I was working with earlier this year didn't have this identifier because if I remember correctly I couldn't execute *$ignore bad* on the data in XSPEC.

Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

> Here's a working version of the confidence interval / error results script:
>
> I put my XSPECtools.py file which has all of these functions into a custom module in my local site-packages folder for python and it seems like it's working perfectly. I import the file like this: *from personalastropy.xspectools import XSPECtools as xt*, and I execute a particular function like this: *xt.returnFitResults()*.
>
> Happy early Thanksgiving!
> Thaddaeus

That's really great Thaddaeus! I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Very well done!

After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

Best,

-Jack
[Quoted text hidden]

Kiker, Thaddaeus <s024622@students.lmsd.org> Thu, Nov 19, 2020 at 5:07 PM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hey!

> Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.

Yeah, safety is (rightly so) the first priority.

> A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

Oo sounds cool, I hope you all have fun!

> Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

Perfect.

> The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon.

Interesting.

> then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

Very interesting.

I think I found the thing that was on the edge of my memory: a Chandra image of the quasar H1821+643.

So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

> Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

I should probably rephrase what I said earlier. When I ignored bad, no channels were ignored so I guess that meant no poor quality flags were raised.

> I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Agreed. I find it so enjoyable: the initial encounter with a seemingly insurmountable challenge, the brainstorming, the troubleshooting, and *especially* the successful execution of a solution. It's extremely satisfying—and to use an analogy from my youth* it feels kinda similar to when I would build things in lego.

*I say an analogy from my youth but that's somewhat misleading. I still occasionally play legos, and these days the only difference is that I have a partner: my younger brother ;).

> After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Definitely. That is exactly what I'm thinking.

> Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

:)).

Best!
Thaddaeus

Oh I almost forgot. I don't remember how/why I came across this paper (Arumugasamy et al. 2018), but after looking at the plots I've been meaning to ask you about them:

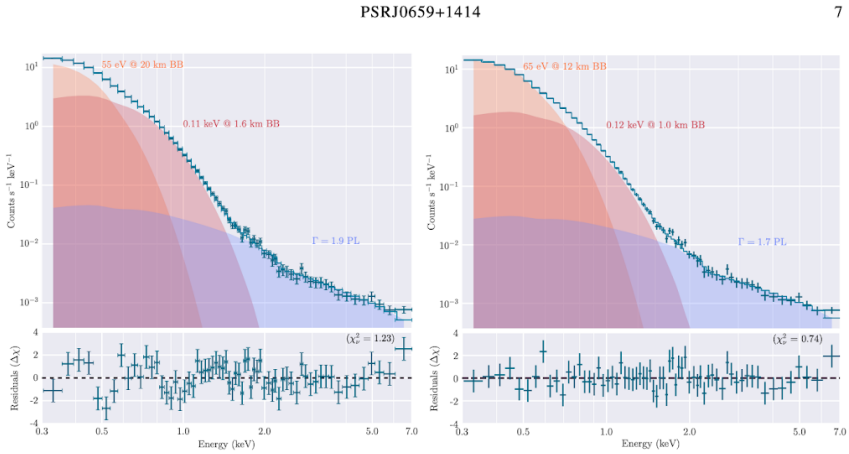

Figure 4. 2BBPL model, without (left) and with (right) `Gabs` absorption component, fit to the phase-integrated spectrum of J0659+1414. The model parameters correspond to maximum posterior probability, which in the case of G2BBPL are different from the distribution medians listed in Table 2.

I've been using the darkgrid style (like the plots above) with the rc font parameter set to Nimbus Roman in most of my plots ... but after seeing these plots I'm thinking I was to switch back to the mpl defaults—these plots are kinda distracting to me in their aesthetic.

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Kiker, Thaddaeus <s024622@students.lmsd.org> Fri, Nov 20, 2020 at 5:12 AM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Shoot! I forgot this bring this up. Would you recommend me to freeze the spin and inclination parameters to published values (that are in agreement with some of my previous best fits) to simplify the parameter space, or should I keep them free? The only benefit I can see from keeping them free would be sometimes error locks up with no variable parameters to use when free parameters get pegged due to their insignificance to the fit, and I think having them free could help prevent this...though spin and inclination seem like parameters that would be the first to get pegged during an error routine anyways.
[Quoted text hidden]
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Steiner, James <james.steiner@cfa.harvard.edu> Fri, Nov 20, 2020 at 6:16 AM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hi Thaddaeus,

Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind.

So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

And in response to your email yesterday:

> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

Yes, spot on!

As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Best,

-Jack

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Kiker, Thaddaeus <s024622@students.lmsd.org> Sun, Nov 22, 2020 at 5:00 PM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi!

> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

Yes I've been fitting with the inner radius frozen to -1. That's good to know.

> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind. So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

Sounds good.

> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Yes I agree it was nice that they showed the underlying component.

I've been running the first iterations of the reprocessing last night and today and I've had a little breakthrough—last night I quickly updated the version of XSPEC I had on the vm that I installed on my mom's computer way back in June (I also installed relxill on it), and I've been dividing the work between her computer and mine—the routines are concluding much quicker (her computer has four cores xD).

Best!
Thaddaeus

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Steiner, James <james.steiner@cfa.harvard.edu> Mon, Nov 23, 2020 at 1:30 PM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hi Thaddaeus,

Sounds good! Glad your mom's computer has some CPU cycles to share. Keep me posted when you get the results in hand. Meantime, happy Thanksgiving.

-Jack

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Kiker, Thaddaeus <s024622@students.lmsd.org> Mon, Nov 23, 2020 at 5:34 PM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Will do!
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Kiker, Thaddaeus <s024622@students.lmsd.org> Mon, Nov 30, 2020 at 7:05 AM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi Dr. Steiner!

Thanksgiving on my end was pretty nice, how was your trip?

I reprocessed the data last weekend (11/22) but the results were a little weird so I've been tweaking some things and prepping for reprocessing again this week.

I was wondering if I should take a different approach to the instrumental residuals by running a preliminary fit on all files and logging the fit statistic landscape, so I can then loop through and identify which files actually require the 1.5-2.2 keV range to be ignored and which can have it noticed (I'm wondering about this because from a preliminary check a lot of the disk temperatures were behaving weirdly, which I think could be related to having less data in the thermal band to constrain them). What do you think?

Also, I've only been fitting the silver, zero good time interval files; would you recommend me to fit some of the GTI1s, GT2s, etc. in hopes to get more good fits?

Cheers!
Thaddaeus

P.S. I wrote two new standardized functions (one calculates and returns count rates, the other calculates and returns hardness ratios) and nice documentation for all my standard functions so far.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."

-Enrico Fermi

On Mon, Nov 23, 2020 at 8:34 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Will do!
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Mon, Nov 23, 2020 at 4:30 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Sounds good! Glad your mom's computer has some CPU cycles to share. Keep me posted when you get the results in hand. Meantime, happy Thanksgiving.

-Jack

On Sun, Nov 22, 2020 at 8:01 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

Yes I've been fitting with the inner radius frozen to -1. That's good to know.

> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind. So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

Sounds good.

> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Yes I agree it was nice that they showed the underlying component.

I've been running the first iterations of the reprocessing last night and today and I've had a little breakthrough—last night I quickly updated the version of XSPEC I had on the vm that I installed on my mom's computer way back in June (I also installed relxill on it), and I've been dividing the work between her computer and mine—the routines are concluding much quicker (her computer has four cores xD).

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Fri, Nov 20, 2020 at 9:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind.

So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

And in response to your email yesterday:

> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

Yes, spot on!

As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

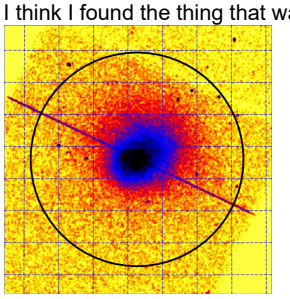
Best,

-Jack

On Fri, Nov 20, 2020 at 8:13 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Shoot! I forgot this bring this up. Would you recommend me to freeze the spin and inclination parameters to published values (that are in agreement with some of my previous best fits) to simplify the parameter space, or should I keep them free? The only benefit I can see from keeping them free would be sometimes error locks up with no variable parameters to use when free parameters get pegged due to their insignificance to the fit, and I think having them free could help prevent this...though spin and inclination seem like parameters that would be the first to get pegged during an error routine anyways.
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Thu, Nov 19, 2020 at 8:07 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hey!
>
> > Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.
>
> Yeah, safety is (rightly so) the first priority.
>
> > A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!
>
> Oo sounds cool, I hope you all have fun!
>
> > Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.
> Perfect.
>
> > The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon.
> Interesting.
>
> > then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.
> Very interesting.
>
> I think I found the thing that was on the edge of my memory: a Chandra image of the quasar H1821+643.
>
> 
>
> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?
>
> > Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.
> I should probably rephrase what I said earlier. When I ignored bad, no channels were ignored so I guess that meant no poor quality flags were raised.
>
> > I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a

> really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Agreed. I find it so enjoyable: the initial encounter with a seemingly insurmountable challenge, the brainstorming, the troubleshooting, and *especially* the successful execution of a solution. It's extremely satisfying—and to use an analogy from my youth* it feels kinda similar to when I would build things in lego.

*I say an analogy from my youth but that's somewhat misleading. I still occasionally play legos, and these days the only difference is that I have a partner: my younger brother ;).

> After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Definitely. That is exactly what I'm thinking.

> Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

:)).

Best!
Thaddaeus

Oh I almost forgot. I don't remember how/why I came across this paper (Arumugasamy et al. 2018), but after looking at the plots I've been meaning to ask you about them:

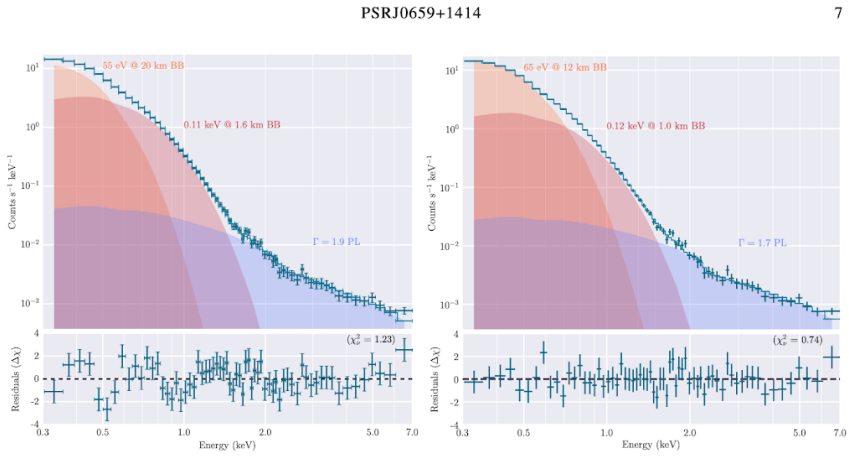


Figure 4. 2BBPL model, without (left) and with (right) `Gabs` absorption component, fit to the phase-integrated spectrum of J0659+1414. The model parameters correspond to maximum posterior probability, which in the case of G2BBPL are different from the distribution medians listed in Table 2.

I've been using the darkgrid style (like the plots above) with the rc font parameter set to Nimbus Roman in most of my plots ... but after seeing these plots I'm thinking I was to switch back to the mpl defaults—these plots are kinda distracting to me in their aesthetic.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Wed, Nov 18, 2020 at 10:05 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> > It was a wild week indeed—and things seem to keep getting crazier. We were in a hybrid model since October 19th, but as of Tuesday we went indefinitely back to all online which is a two sided coin. On one hand the online schedule gives me a lot more free time, but on the other I'm going to miss the extra class time in my harder classes like mechanics and calc. Thanksgiving is around the corner though! Up to any interesting plans yourself? I think we're just going to have my mom's parents over because we already interact with them frequently, but no big gathering this year (obviously).
>
> No kidding! Feels like we're living in a bad TV show.
>
> Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.
>
> A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> Would it be okay if I emailed Dr. Cuneo about this? This is very important for how I structure the confidence intervals code (and whether I make it more sensitive and able to detect more than the four generalized abnormal scenarios described later in this email).

Sure thing, email away.

> Haha! I just read that NuStar's is ~2.5 *milliseconds.*

Yeah and that's actually not too bad compared to most things out there. NICER is just an absolute timing monster.

> I do have my pipeline correct for it automatically, so all your spectra already factor that in.
>
> Cool! Does that mean I can just go ahead and calculate count rates as number of counts / exp. time and not worry about adjusting the result because you've already accounted for deadtime?

Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

> deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.
>
> Yes. This makes sense. I feel like I've seen something related where the center of an object was too bright in the X-Ray band (which oversaturated the detector?) so the color gradient of brightness was black in the center because the author's couldn't determine the brightness at the center. It kinda looked like an artificial coronagraph. Would that be related to a situation where the dead time was more than the event time?

(Deep dive warning:)
Yes, you're speaking about "pileup" which isn't exactly related to deadtime except that both can be important when the source is bright. Deadtime, again, is from the detector shutting down for some portion of the time. Pileup concerns are most often for an imagng type of detector - more like the camera on your phone - that collects an exposure for some amount of time and then read it off.

Chandra, XMM-Newton, Swift, Suzaku, (somewhat confusingly NuStar is in both camps), let X-rays come in from the sky, get focused onto the CCD camera, and then in some time interval, often ~1s, reads off the image and clears the CCD. The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon. So when an individual pixel gets << 1 X-ray per frame on average, you are in good shape. *But* when there is significant probability of getting >~1 X-ray per frame on a given pixel, then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

This registering of multiple X-rays incorrectly as one more energetic single event is pileup, and it is the bane of most imaging detectors for those of us working with bright sources. (Pileup can occur in instruments like NICER too, when two X-rays hit during the event-latch phase, but this is something like a <1% effect for a 100 Crab-bright source, so well in the camp of being negligible for all practical purposes.)

In the most extreme cases of pileup, the pixel digitizer, which usually records values on something like a 16-bit value (e.g., from -32768:32767), can roll over and then appears negative when too much charge hits at once.

> I'm pretty sure the RXTE data I was working with earlier this year didn't have this identifier because if I remember correctly I couldn't execute *$ignore bad* on the data in XSPEC.

Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

> Here's a working version of the confidence interval / error results script:
>
> I put my XSPECtools.py file which has all of these functions into a custom module in my local site-packages folder for python and it seems like it's working perfectly. I import the file like this: *from personalastropy.xspectools import XSPECtools as xt*, and I execute a particular function like this: *xt.returnFitResults().*
>
> Happy early Thanksgiving!
> Thaddaeus

That's really great Thaddaeus! I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Very well done!

After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

Best,

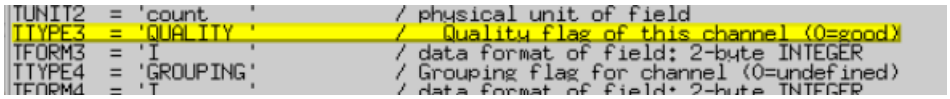
-Jack

> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Wed, Nov 11, 2020 at 12:07 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hey Thaddaeus,
>
> What a week it's been eh? (But way to go PA!)
>
> > I've held off from running another major fitting routine since the weekend because after I ran that my last one I decided that it was time to write some standardized scripts for wrangling the results from all the log files.
>
> Excellent!
>
> > Speaking of which, when Cuneo uses green triangles for "*parameters consistent with 0 within errors*", does this refer to situations when either the upper value of a parameter is equal to zero (because it got pegged at the hard upper limit) or when the lower limit is invalid and equal to zero?
>
> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> I *love* the outline of your routines, and "wow" is what I have to say about that. Way to go Thaddaeus, it's great you're tackling this and in a very useful manner.
>
> > Out of curiosity, where did Cuneo get the count rate values for his data? I don't think it's the same as the Net Count Rate that's displayed whenever you execute *$show data* in XSPEC....can Cuneo's count rate (c/s) be found by dividing the sum of the counts in the included channels by the total observation time?
>
> About 99% yes. "Usual" practice for NICER has become to normalize to an active set of 52 detectors. Your spectrum is a collection of all good events. So a tally of all the (good) events between 0.5-10 keV divided by the exposure time is indeed the count rate detected. However, if only using 49 or 50 detectors (also common), one would scale up by 52/49 to show the rate on the full instrument.
>
> The one very tiny wrinkle to all this is a property called "deadtime". Each time the electronics on a detector register an X-ray event, they "latch" meaning that the detector stops listening for new signals. Effectively, the detector is turned off for a short amount of time it uses to calculate properties of event (what time it arrived, and how much charge was deposited). For NICER, this is typically ~20 microseconds.
>
> As a concrete example of what this means in practice: when the count rate per detector reaches ~10^3/s (or in other words the full instrument light curve was at ~50,000/s), there would be ~20e-6 * 1e3 = 2% deadtime. This would mean that the *actual* count rate is ~2% higher than was detected (because the true exposure time was actually 98% the clock time). Here, most likely Cuneo didn't worry about folding in deadtime effects (I do have my pipeline correct for it automatically, so all your spectra already factor that in).
>
> This is obviously a very small here for NICER, but I wanted to dive into it because for many missions that aren't timing beasts like NICER, deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.

Also sorta random, I came across a value called "quality" when I was looking at a file in fv; are the quality values the values XSPEC refers to when I say *$ignore bad*?



Yes exactly this. This won't tend to come up for NICER, but e.g., for instruments that have some bad pixels and a spatially dispersed spectrum (think the rainbow from a prism), or uncalibrated energy ranges, this can be used.

> * This is probably one of my favorite one-liner functions in Python:
>
> for line in f:
> linelist = (re.sub(' +', ',', line)).split(',')
>
> Where f is a file opened with: *with open(...) as f.*

Python is a wonder for its very efficient one-lining loops!

As usual, you're knocking it out of the park Thaddaeus.

In other news, a new grad student (Santi) just joined me and he's going to start looking at some QPO data.

Best,

-Jack

> On Thu, Oct 29, 2020 at 12:49 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Thaddaeus,
>
> Whoops - Santiago is my grad student and I notice my advisor brain accidentally called you Santi in my last message. Very sorry about that!
>
> Name confusions aside, very glad the patch worked ok. Yeah, setting query to yes can cause problems sometimes as you found. I don't have good rules for avoiding or escaping those runaways, but I've had luck before tweaking the extra convergence terms in the fit command when it's stuck (the delta & beta) and the tolerance term in error.
>
> -Jack
>
> On Thu, Oct 29, 2020 at 11:34 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Hi Dr. Steiner!
>
> Alright I think that patching did the trick; I'm currently running XSPEC v 12.11.0m. Surprisingly I didn't have to rebuild relxill which was nice. Now that the 12.11.0k patch has been installed, I think XSPEC just "snags" and keeps fitting if I have query set to yes instead of crashing—when I was looking into this, a particular observation ran an error routine for three days nonstop in the background until I aborted it!
>
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi
>
> On Thu, Oct 22, 2020 at 12:46 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Santi,
>
> OK, that's encouraging! Let me know if the patching does the trick. Often for small patches, you won't need to recompile local models, but you should quickly see if it has trouble loading and that would be your sign.
>
> It's hard to predict how parallel processing will impact a given task. The outcome depends on where the bottlenecks occur (if processing, then expect significant improvement, if memory swapping, then probably not). For running on multiple threads, a **rough** estimate is sqrt(N) gain, but that's just ballpark.

-Jack

On Thu, Oct 22, 2020 at 11:56 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> I just got some really great news on the crashes!
>
> Last weekend I was dismayed because even though I was playing with the limits extensively (especially for gamma), the crashing was still occurring unabated. This made me wonder if it was a software issue, so I reached out to the HEASARC Help Desk. I found out that a very similar issue was fixed in XSPEC patch 12.11.0k: *For some complex fitting and error command cases, XSPEC may terminate with a GSL message such as 'SVD decomposition failed to converge'.* This wasn't encouraging at first because XSPEC patch 12.11.0k came out in June (I built the current version of xspec on my vm in ~July), but I checked my XSPEC version with *$version,* and it turns out that my XSPEC version is just 12.11.0! I'm working on installing the patch, I bet this will clear a lot of things up. I think I'm going to have to rebuild relxill after I execute the partial system rebuild.
>
> The only thing that confuses me about this is that I didn't really have issues with core dumping when I was fitting relxill a lot back in August (I think it did come up for error fits back then though), so I'm confused why it really started acting up recently.
>
> Also I don't think I've asked this previously, but out of curiosity, should I expect significant reductions in fit times with two core parallel processing?
>
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Thu, Oct 22, 2020 at 10:52 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Just checking in to see how fits are progressing and if playing with those limits has allowed any headway against the crashes?
>
> (Yes, whenever I "rm -rf " I try to take beat and check that I am not about to bilk the whole machine! With great commands-line tools comes great ...)
>
> -Jack
>
> On Fri, Oct 16, 2020 at 3:24 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
>
> > Hi!
> >
> > Now that I think about it, I do recall noticing that many (if not most) of the fit failures had gammas in the 3.4 range. Good to know about the 1.6-2.2 range for the residuals, I'll play with that now.
> >
> > Oh about the attachments: the screen shot from visual studio is just some descriptions I was writing about the different functions I was using, and the microsoft paint one just illustrates the positive contribution that is almost always eliminated by a ~1.75 KeV gaussian of varying width.
> >
> > Over this weekend should I continue to focus on maximizing the number of observations that I can get good fits and confidence intervals for?
> >
> > I'm intrigued by this disk truncation controversy, I'm going to read about it this weekend.
> >
> > Many thanks!
> > Thaddaeus
> >
> > P.S. I was reading about some dangerous linux commands the other day; there are some unnerving commands out there. Imagine if someone ran rm -rf in your terminal. Shudder.
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi

On Thu, Oct 15, 2020 at 10:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> > Regarding the application of systematic errors, that's good to know. I've often seen something like the "NICER team recommends the application of a systematic error of 1%" in papers that use NICER data.
>
> Yes, that's a "rough" benchmark, but a reasonable one.
>
> > I love the common.tcl file, it's so handy! I think I'm just going to use it from now on because it's simpler on the execution end (just two commands), and it shows the error strings for each parameter.
>
> Yes, it's quite nice work by Jeremy :).
>
> > A little random: I came across an interesting statement in a paper by Plant et al. 2015 pertaining to observed disk truncation in the hard state of GX 339−4: "This result implies that the current sample of spin estimates in the hard state are inaccurate. Therefore, any distinct conclusions drawn from these estimates, such as the spin-powering of relativistic jets, may well be biased." Since that paper was published has this issue been fully exploited and resolved, or are there still black holes with possibly incorrect spin measurements out there? Also, is physical disk truncation or changing coronal height the more widely accepted cause for profiles similar to disk truncation?
>
> This has been a topic with a lot of controversy that is still ongoing (Garcia et al 2015 and followup papers by Wang et al. 2019 talk about this). I'd say the Plant picture is strongly disfavored at present, but not entirely out. The robust view is that allowing for the possibility of truncation, a (bright) hard-state spin measurement should be regarded as an estimated lower limit. Presently, the argument is focused around how much truncation could there be in bright hard states, with most reasonable numbers being a few times the ISCO (a factor <10). (Coronal geometry is always an important consideration, but usually they tend to fit very compact.)
>
> > An update on the rsp/arf issue: I modified my approach and fully integrated astropy.io.fits, so when I'm producing my xspec script commands rmf and arf file names are pulled directly from the header of each observation's fits file. Because of the two errors described below, I haven't checked how chi-square improves when the correct rmf and arf files are used though.
>
> Very good, that should make some impact when working.
>
> > I've encountered two weird errors I've never encountered before in XSPEC this week: the first is frequent core dumping [SVD decomposition failed to converge...Default GSL error handler invoked...Segmentation fault (core dumped)], and the second is this odd "XSPEC Error: No variable parameters for fit" after executing "fit" for a model with *multiple* free parameters (including the usual Gamma, FracSctr, Tin, diskbb norm, etc.). My first thought was "*is this related to relxill having problems due to a low gamma*"—but these issues both happen even when Gamma is > 2.
>
> The table can be sparse in a few places, and I think very large Gammas are also sparse (>~3.4, if memory serves). I'd say try freezing parameters and retrying the fit to identify the culprit parameter.
>
> > Also a little random: this week I decided it was time to start commenting on my functions in my python files—at one point I was looking through some older files for a particular technique I used and it took a little while to find the correct version of it, so I decided that from now on I'm describing my functions for future reference.
>
> Yes, documenting code is super important and a great habit to get used to. (I wish I was better about it!)
>
> > This is preliminary, but I attached an example of a chi-scape scarred by a common morph of the residual I last sent you. The positive contribution that begins shortly after 1.6 KeV is almost always eliminated by a ~1.75 KeV gaussian of varying width for most observations. The current issue is modeling the various forms the following negative residuals take. Currently working on those. Earlier you mentioned that we could try ignoring that narrow range where these residuals occur, would ignoring channels between 1.6-

2.2 KeV be ignoring too much of the data? If we decide to ignore that range, I would think it best to get >*plt wdata* outputs to exactly nail down the dimensions of this range, but for now should I keep trying to model the residuals?

I was reading through some older emails, going to look into your recommendation regarding logxi.

No, I think 1.6-2.2 isn't crazy and you can certainly roll with that if you are having too much trouble taming the residuals with a model. Maybe do a couple test cases to see how the model parameters are affected on both fronts.

Hey, and I didn't see a description of your attachments in case you want to say anything about those.

Glad you're back in the thick of things.

Best,

-Jack

On Thu, Oct 8, 2020 at 1:35 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi Dr. Steiner!

Quick note: I've been getting back into it this week and should have some interesting things to report this weekend!

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Tue, Sep 29, 2020 at 2:19 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Thanks for the update.

> Quick note on the residuals: since we last communicated I investigated the application of a systematic error (3-5%) in the < 2 KeV energy band, but this didn't improve the residuals so I'm assuming that you already accounted for this, like the binning, in your reprocessing?

No, actually. But systematic errors are only incorporated with some fit statistics - chisq for instance, but I don't believe are incorporated in cstat or pgstat. The way I like to think about it is the systematic error acts as a reweighting of the data's constraint and serves to de-emphasize regions with the highest signal. I offer for information sake that that 3-5% is too large a systematic error to include as a rule. Something like 1-2% is more reasonable for a region with poorer calibration quality.

> I also discovered that I've been unintentionally forcing an incorrect response file on multiple MAXI J1535-571 observations—the rsp I was using for all observations was `nicer_d49_55575341.rmf`, but after running a quick check (the fitsio package has been so helpful, thanks for recommending it to me!), I found that multiple observations request `nicer_d49_u100_150_55575341.rmf` in their headers.

Ah! That's very good to know, and may be the culprit for some of the residuals you've been battling. Check that the arf files (keyword ANCRFILE) is also set correctly.

> I'm likely almost done with the SAT (took it Saturday, going to take it again this coming Saturday)*, and once I'm done I aim to return to a progress pace similar to the pace we had in the

summer.

Congratulations - that's a nice milestone!

G'mar chatima tova (if you observe Yom Kippur)!

Same to you.

*I might take it again in November in the event that I bungle those tests.

I'd put dollars to donuts you did great!

My best,

-Jack

On Tue, Sep 22, 2020 at 1:21 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Interesting I'll look into that!

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Tue, Sep 22, 2020 at 1:15 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Similar to that. I used another language (IDL) to automatically produce a series of tcl/tk scripts and then executed those distributed over a number of cores on a workstation.

-Jack

On Tue, Sep 22, 2020 at 1:13 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Sounds good!

In that case did you operate solely in TCL?

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Tue, Sep 22, 2020 at 1:10 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> For >75% I meant observations where source flux was more than 75% bg flux, and for 4% I meant observations where source flux was ~4 % bg flux. That makes sense—interestingly the observations with source flux ~4% bg flux were, by best fit, in the SPL state, with the highest flux and gamma for all the observations that had reasonable values for these parameters.

Ah, now I see. Yes, when the bg flux is very high, the noise level (N=sqrt(B+S)) where B and S are background and signal counts in a channel, then the constraining power of the data (S/N) is lowest. If the background is reasonably estimated, the fits will likely be good but poorly constrained; if the background is off, then all bets are off.

> xD. I think it's a very impressive feat! Did you use pyxspec to loop through all the observations?

Thanks. I used a different language actually, but python/pyxspec would be an equally good way to do it.

Best,

-Jack

On Tue, Sep 22, 2020 at 12:39 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> > That's good to know. I guess what's surprising is some relative of this large residual in the 1.6-2.0 KeV is present in most of the source flux > 75% observations (I wonder if the peak feature falsely inflates the source flux for some of them?). Going back to a couple emails ago, I was checking some of my all time best fits which had source fluxes around ~4%—what allows such faint observations to fit well?
>
> Just to clarify, do you mean 75% and 4% of the Eddington limit, peak luminosity, or something else? Fainter observations, given some typical observation time, obviously have fewer counts which means larger fractional errors and less constraining power on the model. So if there is a deficiency in the calibration or model being used, it will look more prominent in brighter + long exposure time data.
>
> > Also, I'm very intrigued by your methods in one of your 2016 papers , "STRONGER REFLECTION FROM BLACK HOLE ACCRETION DISKS IN SOFT X-RAY STATES". How did you automate fitting the 15,000 observations reliably and within a reasonable time frame?
>
> A reasonable time frame is relative :), but I used about two dozen cores to run the fits which helped. I also stuck with computationally simplistic models, and for my runs with simpl, used lower resolution (via energies) in the run, since RXTE/PCA - the instrument used there - also has rather poor energy resolution compared to NICER.
>
> > Could I possibly have the ~86 observations between obsids 1200220101 – 1200220186?
>
> Spectral files are attached. Response files are too big for me to attach, so I'll keep them up here for a few days: https://nicer-web.mit.edu/nicerview/thadd_resp_m1727.tar.gz
>
> > Ah that makes sense. In that case did Xu et al. just freeze Rin at the ISCO to have less free parameters?
>
> That's the usual tactic, yes, when fitting for spin.
>
> Best,
>
> -Jack
>
> > On Thu, Sep 17, 2020 at 11:00 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> >
> > > Hi Thaddaeus,
> > >
> > > Great to hear you've been digging in and exploring! I would suggest though that you avoid being too concerned about the residual features. If something is localized and only ~percent level deviation, then that's likely sufficient. *The goal should be to fit those sufficiently that they don't alter the continuum, but at some level it becomes diminishing returns to try fitting an artefact.* That said, if

you wish, I can get you the data on MAXI J1727-203. Just let me know if you have a particular ObsID from Kevin's paper you want to try.

Your question about degeneracy is great. The condition of parameters being strongly correlated with one another is degeneracy. Basically, this is a very common situation where two parameters are dependent upon one another to some extent, so a probable value of one will condition a probable range of values with the other. For instance, if you want to fit x and y with the constraint x^2+y^2 = 4+/-1, you would get a ring of radius 2 with x,y values that obviously depend on one another; they are *not independent*. Speaking generally for more realistic cases, the interaction between parameters is often pretty convoluted, but commonly things like Gamma and Normalization are correlated with one another, so a contour plot of the confidence interval would look banana shaped rather than like a circle.

Best,

-Jack

On Thu, Sep 17, 2020 at 9:18 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> Thanks for checking in! Since we last spoke I've been taking a deeper look into a couple articles (Alabarta et al. 2020, Xu et al. 2018, and Parikh et al. 2019) and playing around a bit with the residual fix you suggested. As tested on multiple observations with varying fit statistic contributions in the 1.6-1.8 KeV range (chi-squared contribs. ranging between 500-3000), the gaussian centered around 1.75 KeV combined with the "edge" works great!
>
> Most importantly, I found a method to more accurately investigate the residual features: *plt>wdata*. Now that I can actually see what the chi-sq contrib. is for every point, I've been having a lot more success in approaching residuals in the 1.6-2.2 KeV range, especially when the residual patterns after 1.8 KeV differ greatly from what can quickly be ameliorated with the edge feature (it seems that for the majority of observations the <1.8 KeV excess is easily subdued by the gaussian fix, but after 1.8 KeV things often get complicated, especially when a third feature shows up between 1.86 to 2.0 KeV. This third feature resembles a sort of gaussian-like crevice about 0.13 KeV in width and often reaches a chi^2 contribution of -5331 (+) at its minima, pushing the "edge" feature out to a threshold energy of ~2+ KeV). I haven't found a definitive solution yet, but a gaussian absorption with an energy of ~1.86 KeV and a width of ~0.14 KeV is looking promising.
>
> While I've been working with these MAXI J1535-571 residuals I've been wondering if the residuals Alabarta et al. discuss in Nicer data for MAXI J1727−203 are of similar shape—is there any chance I could procure some pipeline data from you on MAXI J1727-203 to look into this (if it's not an inconvenience for you and or you don't think it's a distraction on my part of course).
>
> Out of curiosity, what does "parameter degeneracy" mean? My first guess would be this example: if I only told you what the slope of a line was (e.g. '2'), and then asked you for the individual values of Δy and Δx—would Δy and Δx be degenerate? Example of where I came across this: (from Xu et al. 2018) "The inner disk radius, R_{in}, and the black hole spin parameter, a*, are degenerate, as they both control the effective inner accretion disk radius. Therefore, for simplicity, during the spectral fitting we assume that the inner disk extends down to the ISCO by fixing R_{in} at the radius of the ISCO, and fit for the black hole spin as a free parameter."
>
> Best!
> Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

2 attachments

XSPECtools.py
9K

documentation.md
8K

Steiner, James <james.steiner@cfa.harvard.edu> Mon, Nov 30, 2020 at 9:28 AM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hi Thaddaeus,

Thanksgiving was very nice, thanks! Glad yours with the grandparents was too.

> I reprocessed the data last weekend (11/22) but the results were a little weird so I've been tweaking some things and prepping for reprocessing again this week.
>
> I was wondering if I should take a different approach to the instrumental residuals by running a preliminary fit on all files and logging the fit statistic landscape, so I can then loop through and identify which files actually require the 1.5-2.2 keV range to be ignored and which can have it noticed (I'm wondering about this because from a preliminary check a lot of the disk temperatures were behaving weirdly, which I think could be related to having less data in the thermal band to constrain them). What do you think?

Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.

> Also, I've only been fitting the silver, zero good time interval files; would you recommend me to fit some of the GTI1s, GT2s, etc. in hopes to get more good fits?

YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.

> P.S. I wrote two new standardized functions (one calculates and returns count rates, the other calculates and returns hardness ratios) and nice documentation for all my standard functions so far.

Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

-Jack

> On Mon, Nov 23, 2020 at 8:34 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Will do!
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi
>
> On Mon, Nov 23, 2020 at 4:30 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Thaddaeus,
>
> Sounds good! Glad your mom's computer has some CPU cycles to share. Keep me posted when you get the results in hand. Meantime, happy Thanksgiving.
>
> -Jack
>
> On Sun, Nov 22, 2020 at 8:01 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Hi!

> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

Yes I've been fitting with the inner radius frozen to -1. That's good to know.

> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind. So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

Sounds good.

> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Yes I agree it was nice that they showed the underlying component.

I've been running the first iterations of the reprocessing last night and today and I've had a little breakthrough—last night I quickly updated the version of XSPEC I had on the vm that I installed on my mom's computer way back in June (I also installed relxill on it), and I've been dividing the work between her computer and mine—the routines are concluding much quicker (her computer has four cores xD).

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Fri, Nov 20, 2020 at 9:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.
>
> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind.
>
> So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)
>
> And in response to your email yesterday:
>
> > So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?
>
> Yes, spot on!
>
> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.
>
> Best,
>
> -Jack

On Fri, Nov 20, 2020 at 8:13 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Shoot! I forgot this bring this up. Would you recommend me to freeze the spin and inclination parameters to published values (that are in agreement with some of my previous best fits) to simplify the parameter space, or should I keep them free? The only benefit I can see from keeping them free would be sometimes error locks up with no variable parameters to use when free parameters get pegged due to their insignificance to the fit, and I think having them free could help prevent this...though spin and inclination seem like parameters that would be the first to get pegged during an error routine anyways.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Nov 19, 2020 at 8:07 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hey!
>
> > Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.
>
> Yeah, safety is (rightly so) the first priority.
>
> > A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!
>
> Oo sounds cool, I hope you all have fun!
>
> > Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.
> Perfect.
>
> > The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon.
> Interesting.
>
> > then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.
> Very interesting.
>
> I think I found the thing that was on the edge of my memory: a Chandra image of the quasar H1821+643.
>
> 
>
> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?
>
> > Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.
> I should probably rephrase what I said earlier. When I ignored bad, no channels were ignored so I guess that meant no poor quality flags were raised.
>
> > I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.
> Agreed. I find it so enjoyable: the initial encounter with a seemingly insurmountable challenge, the brainstorming, the troubleshooting, and *especially* the successful execution of a solution. It's extremely satisfying—and to use an analogy from my youth* it feels kinda similar to when I would build things in lego.
>
> *I say an analogy from my youth but that's somewhat misleading. I still occasionally play legos, and these days the only difference is that I have a partner: my younger brother ;).
>
> > After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?
> Definitely. That is exactly what I'm thinking.
>
> > Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.
> :)).

Best!
Thaddaeus

Oh I almost forgot. I don't remember how/why I came across this paper (Arumugasamy et al. 2018), but after looking at the plots I've been meaning to ask you about them:

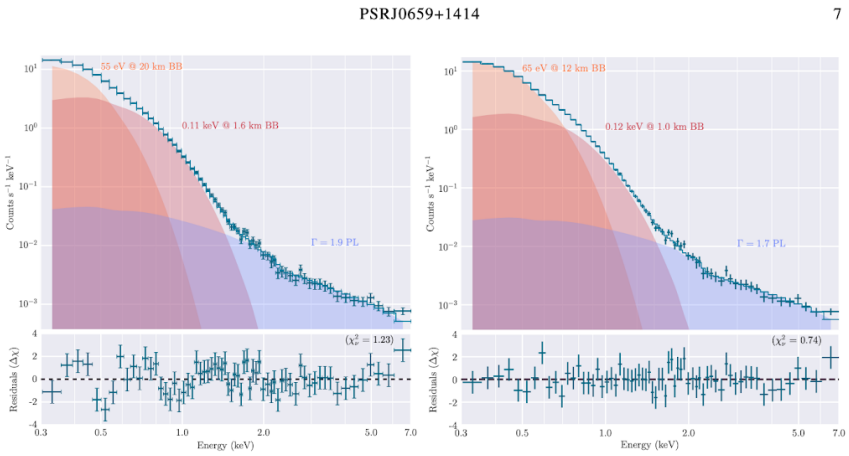


Figure 4. 2BBPL model, without (left) and with (right) `Gabs` absorption component, fit to the phase-integrated spectrum of J0659+1414. The model parameters correspond to maximum posterior probability, which in the case of G2BBPL are different from the distribution medians listed in Table 2.

I've been using the darkgrid style (like the plots above) with the rc font parameter set to Nimbus Roman in most of my plots ... but after seeing these plots I'm thinking I was to switch back to the mpl defaults—these plots are kinda distracting to me in their aesthetic.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Wed, Nov 18, 2020 at 10:05 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> It was a wild week indeed—and things seem to keep getting crazier. We were in a hybrid model since October 19th, but as of Tuesday we went indefinitely back to all online which is a two sided coin. On one hand the online schedule gives me a lot more free time, but on the other I'm going to miss the extra class time in my harder classes like mechanics and calc. Thanksgiving is around the corner though! Up to any interesting plans yourself? I think we're just going to have my mom's parents over because we already interact with them frequently, but no big gathering this year (obviously).

No kidding! Feels like we're living in a bad TV show.

Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.

A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> Would it be okay if I emailed Dr. Cuneo about this? This is very important for how I structure the confidence intervals code (and whether I make it more sensitive and able to detect more than the four generalized abnormal scenarios described later in this email).

Sure thing, email away.

> Haha! I just read that NuStar's is ~2.5 *milliseconds.*

Yeah and that's actually not too bad compared to most things out there. NICER is just an absolute timing monster.

> I do have my pipeline correct for it automatically, so all your spectra already factor that in.
>
> Cool! Does that mean I can just go ahead and calculate count rates as number of counts / exp. time and not worry about adjusting the result because you've already accounted for deadtime?

Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

> deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.
>
> Yes. This makes sense. I feel like I've seen something related where the center of an object was too bright in the X-Ray band (which oversaturated the detector?) so the color gradient of brightness was black in the center because the author's couldn't determine the brightness at the center. It kinda looked like an artificial coronagraph. Would that be related to a situation where the dead time was more than the event time?

(Deep dive warning:)
Yes, you're speaking about "pileup" which isn't exactly related to deadtime except that both can be important when the source is bright. Deadtime, again, is from the detector shutting down for some portion of the time. Pileup concerns are most often for an imagng type of detector - more like the camera on your phone - that collects an exposure for some amount of time and then read it off.

Chandra, XMM-Newton, Swift, Suzaku, (somewhat confusingly NuStar is in both camps), let X-rays come in from the sky, get focused onto the CCD camera, and then in some time interval, often ~1s, reads off the image and clears the CCD. The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon. So when an individual pixel gets << 1 X-ray per frame on average, you are in good shape. *But* when there is significant probability of getting >~1 X-ray per frame on a given pixel, then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

This registering of multiple X-rays incorrectly as one more energetic single event is pileup, and it is the bane of most imaging detectors for those of us working with bright sources. (Pileup can occur in instruments like NICER too, when two X-rays hit during the event-latch phase, but this is something like a <1% effect for a 100 Crab-bright source, so well in the camp of being negligible for all practical purposes.)

In the most extreme cases of pileup, the pixel digitizer, which usually records values on something like a 16-bit value (e.g., from -32768:32767), can roll over and then appears negative when too much charge hits at once.

> I'm pretty sure the RXTE data I was working with earlier this year didn't have this identifier because if I remember correctly I couldn't execute *$ignore bad* on the data in XSPEC.

Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

> Here's a working version of the confidence interval / error results script:
>
> I put my XSPECtools.py file which has all of these functions into a custom module in my local site-packages folder for python and it seems like it's working perfectly. I import the file like this: *from personalastropy.xspectools import XSPECtools as xt*, and I execute a particular function like this: *xt.returnFitResults()*.
>
> Happy early Thanksgiving!
> Thaddaeus

That's really great Thaddaeus! I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Very well done!

After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

Best,

-Jack

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Wed, Nov 11, 2020 at 12:07 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hey Thaddaeus,

What a week it's been eh? (But way to go PA!)

> I've held off from running another major fitting routine since the weekend because after I ran that my last one I decided that it was time to write some standardized scripts for wrangling the results from all the log files.

Excellent!

> Speaking of which, when Cuneo uses green triangles for "*parameters consistent with 0 within errors*", does this refer to situations when either the upper value of a parameter is equal to zero (because it got pegged at the hard upper limit) or when the lower limit is invalid and equal to zero?

Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).

I *love* the outline of your routines, and "wow" is what I have to say about that. Way to go Thaddaeus, it's great you're tackling this and in a very useful manner.

> Out of curiosity, where did Cuneo get the count rate values for his data? I don't think it's the same as the Net Count Rate that's displayed whenever you execute *$show data* in XSPEC....can Cuneo's count rate (c/s) be found by dividing the sum of the counts in the included channels by the total observation time?

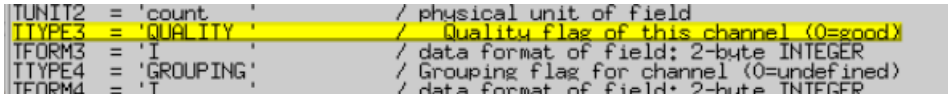
About 99% yes. "Usual" practice for NICER has become to normalize to an active set of 52 detectors. Your spectrum is a collection of all good events. So a tally of all the (good) events between 0.5-10 keV divided by the exposure time is indeed the count rate detected. However, if only using 49 or 50 detectors (also common), one would scale up by 52/49 to show the rate on the full instrument.

The one very tiny wrinkle to all this is a property called "deadtime". Each time the electronics on a detector register an X-ray event, they "latch" meaning that the detector stops listening for new signals. Effectively, the detector is turned off for a short amount of time it uses to calculate properties of event (what time it arrived, and how much charge was deposited). For NICER, this is typically ~20 microseconds.

As a concrete example of what this means in practice: when the count rate per detector reaches ~10^3/s (or in other words the full instrument light curve was at ~50,000/s), there would be ~20e-6 * 1e3 = 2% deadtime. This would mean that the *actual* count rate is ~2% higher than was detected (because the true exposure time was actually 98% the clock time). Here, most likely Cuneo didn't worry about folding in deadtime effects (I do have my pipeline correct for it automatically, so all your spectra already factor that in).

This is obviously a very small here for NICER, but I wanted to dive into it because for many missions that aren't timing beasts like NICER, deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.

> Also sorta random, I came across a value called "quality" when I was looking at a file in fv; are the quality values the values XSPEC refers to when I say *$ignore bad*?



Yes exactly this. This won't tend to come up for NICER, but e.g., for instruments that have some bad pixels and a spatially dispersed spectrum (think the rainbow from a prism), or uncalibrated energy ranges, this can be used.

> * This is probably one of my favorite one-liner functions in Python:

```
for line in f:
   linelist = (re.sub(' +', ',', line)).split(',')
```

Where f is a file opened with: *with open(...) as f*.

Python is a wonder for its very efficient one-lining loops!

As usual, you're knocking it out of the park Thaddaeus.

In other news, a new grad student (Santi) just joined me and he's going to start looking at some QPO data.

Best,

-Jack

On Thu, Oct 29, 2020 at 12:49 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Whoops - Santiago is my grad student and I notice my advisor brain accidentally called you Santi in my last message. Very sorry about that!

Name confusions aside, very glad the patch worked ok. Yeah, setting query to yes can cause problems sometimes as you found. I don't have good rules for avoiding or escaping those runaways, but I've had luck before tweaking the extra convergence terms in the fit command when it's stuck (the delta & beta) and the tolerance term in error.

-Jack

On Thu, Oct 29, 2020 at 11:34 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi Dr. Steiner!

Alright I think that patching did the trick; I'm currently running XSPEC v 12.11.0m. Surprisingly I didn't have to rebuild relxill which was nice. Now that the 12.11.0k patch has been installed, I think XSPEC just "snags" and keeps fitting if I have query set to yes instead of crashing—when I was looking into this, a particular observation ran an error routine for three days nonstop in the background until I aborted it!

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 22, 2020 at 12:46 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Santi,

OK, that's encouraging! Let me know if the patching does the trick. Often for small patches, you won't need to recompile local models, but you should quickly see if it has trouble loading and that would be your sign.

It's hard to predict how parallel processing will impact a given task. The outcome depends on where the bottlenecks occur (if processing, then expect significant improvement, if memory swapping, then probably not). For running on multiple threads, a **rough** estimate is sqrt(N) gain, but that's just ballpark.

-Jack

On Thu, Oct 22, 2020 at 11:56 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

I just got some really great news on the crashes!

Last weekend I was dismayed because even though I was playing with the limits extensively (especially for gamma), the crashing was still occurring unabated. This made me wonder if it was a software issue, so I reached out to the HEASARC Help Desk. I found out that a very similar issue was fixed in XSPEC patch 12.11.0k: *For some complex fitting and error command cases, XSPEC may terminate with a GSL message such as 'SVD decomposition failed to converge'.* This wasn't encouraging at first because XSPEC patch 12.11.0k came out

in June (I built the current version of xspec on my vm in ~July), but I checked my XSPEC version with *$version,* and it turns out that my XSPEC version is just 12.11.0! I'm working on installing the patch, I bet this will clear a lot of things up. I think I'm going to have to rebuild relxill after I execute the partial system rebuild.

The only thing that confuses me about this is that I didn't really have issues with core dumping when I was fitting relxill a lot back in August (I think it did come up for error fits back then though), so I'm confused why it really started acting up recently.

Also I don't think I've asked this previously, but out of curiosity, should I expect significant reductions in fit times with two core parallel processing?

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 22, 2020 at 10:52 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Just checking in to see how fits are progressing and if playing with those limits has allowed any headway against the crashes?
>
> (Yes, whenever I "rm -rf " I try to take beat and check that I am not about to bilk the whole machine! With great commands-line tools comes great ...)
>
> -Jack
>
> On Fri, Oct 16, 2020 at 3:24 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
>
> > Hi!
> >
> > Now that I think about it, I do recall noticing that many (if not most) of the fit failures had gammas in the 3.4 range. Good to know about the 1.6-2.2 range for the residuals, I'll play with that now.
> >
> > Oh about the attachments: the screen shot from visual studio is just some descriptions I was writing about the different functions I was using, and the microsoft paint one just illustrates the positive contribution that is almost always eliminated by a ~1.75 KeV gaussian of varying width.
> >
> > Over this weekend should I continue to focus on maximizing the number of observations that I can get good fits and confidence intervals for?
> >
> > I'm intrigued by this disk truncation controversy, I'm going to read about it this weekend.
> >
> > Many thanks!
> > Thaddaeus
> >
> > P.S. I was reading about some dangerous linux commands the other day; there are some unnerving commands out there. Imagine if someone ran rm -rf in your terminal. Shudder.
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
> >
> > On Thu, Oct 15, 2020 at 10:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> >
> > > Hi Thaddaeus,
> > >
> > > > Regarding the application of systematic errors, that's good to know. I've often seen something like the "NICER team recommends the application of a systematic error of 1%" in papers that use NICER data.
> > >
> > > Yes, that's a "rough" benchmark, but a reasonable one.

> I love the common.tcl file, it's so handy! I think I'm just going to use it from now on because it's simpler on the execution end (just two commands), and it shows the error strings for each parameter.

Yes, it's quite nice work by Jeremy :).

> A little random: I came across an interesting statement in a paper by Plant et al. 2015 pertaining to observed disk truncation in the hard state of GX 339−4: "This result implies that the current sample of spin estimates in the hard state are inaccurate. Therefore, any distinct conclusions drawn from these estimates, such as the spin-powering of relativistic jets, may well be biased." Since that paper was published has this issue been fully exploited and resolved, or are there still black holes with possibly incorrect spin measurements out there? Also, is physical disk truncation or changing coronal height the more widely accepted cause for profiles similar to disk truncation?

This has been a topic with a lot of controversy that is still ongoing (Garcia et al 2015 and followup papers by Wang et al. 2019 talk about this). I'd say the Plant picture is strongly disfavored at present, but not entirely out. The robust view is that allowing for the possibility of truncation, a (bright) hard-state spin measurement should be regarded as an estimated lower limit. Presently, the argument is focused around how much truncation could there be in bright hard states, with most reasonable numbers being a few times the ISCO (a factor <10). (Coronal geometry is always an important consideration, but usually they tend to fit very compact.)

> An update on the rsp/arf issue: I modified my approach and fully integrated astropy.io.fits, so when I'm producing my xspec script commands rmf and arf file names are pulled directly from the header of each observation's fits file. Because of the two errors described below, I haven't checked how chi-square improves when the correct rmf and arf files are used though.

Very good, that should make some impact when working.

> I've encountered two weird errors I've never encountered before in XSPEC this week: the first is frequent core dumping [SVD decomposition failed to converge...Default GSL error handler invoked...Segmentation fault (core dumped)], and the second is this odd "XSPEC Error: No variable parameters for fit" after executing "fit" for a model with *multiple* free parameters (including the usual Gamma, FracSctr, Tin, diskbb norm, etc.). My first thought was "*is this related to relxill having problems due to a low gamma"*—but these issues both happen even when Gamma is > 2.

The table can be sparse in a few places, and I think very large Gammas are also sparse (>~3.4, if memory serves). I'd say try freezing parameters and retrying the fit to identify the culprit parameter.

> Also a little random: this week I decided it was time to start commenting on my functions in my python files—at one point I was looking through some older files for a particular technique I used and it took a little while to find the correct version of it, so I decided that from now on I'm describing my functions for future reference.

Yes, documenting code is super important and a great habit to get used to. (I wish I was better about it!)

> This is preliminary, but I attached an example of a chi-scape scarred by a common morph of the residual I last sent you. The positive contribution that begins shortly after 1.6 KeV is almost always eliminated by a ~1.75 KeV gaussian of varying width for most observations. The current issue is modeling the various forms the following negative residuals take. Currently working on those. Earlier you mentioned that we could try ignoring that narrow range where these residuals occur, would ignoring channels between 1.6-2.2 KeV be ignoring too much of the data? If we decide to ignore that range, I would think it best to get >*plt wdata* outputs to exactly nail down the dimensions of this range, but for now should I keep trying to model the residuals?
>
> I was reading through some older emails, going to look into your recommendation regarding logxi.

No, I think 1.6-2.2 isn't crazy and you can certainly roll with that if you are having too much trouble taming the residuals with a model. Maybe do a couple test cases to see how the model parameters are

affected on both fronts.

Hey, and I didn't see a description of your attachments in case you want to say anything about those.

Glad you're back in the thick of things.

Best,

-Jack

On Thu, Oct 8, 2020 at 1:35 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi Dr. Steiner!
>
> Quick note: I've been getting back into it this week and should have some interesting things to report this weekend!
>
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Tue, Sep 29, 2020 at 2:19 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Thanks for the update.
>
> > Quick note on the residuals: since we last communicated I investigated the application of a systematic error (3-5%) in the < 2 KeV energy band, but this didn't improve the residuals so I'm assuming that you already accounted for this, like the binning, in your reprocessing?
>
> No, actually. But systematic errors are only incorporated with some fit statistics - chisq for instance, but I don't believe are incorporated in cstat or pgstat. The way I like to think about it is the systematic error acts as a reweighting of the data's constraint and serves to de-emphasize regions with the highest signal. I offer for information sake that that 3-5% is too large a systematic error to include as a rule. Something like 1-2% is more reasonable for a region with poorer calibration quality.
>
> > I also discovered that I've been unintentionally forcing an incorrect response file on multiple MAXI J1535-571 observations—the rsp I was using for all observations was `nicer_d49_55575341.rmf`, but after running a quick check (the fitsio package has been so helpful, thanks for recommending it to me!), I found that multiple observations request `nicer_d49_u100_150_55575341.rmf` in their headers.
>
> Ah! That's very good to know, and may be the culprit for some of the residuals you've been battling. Check that the arf files (keyword ANCRFILE) is also set correctly.
>
> > I'm likely almost done with the SAT (took it Saturday, going to take it again this coming Saturday)*, and once I'm done I aim to return to a progress pace similar to the pace we had in the summer.
>
> Congratulations - that's a nice milestone!
>
> > G'mar chatima tova (if you observe Yom Kippur)!
>
> Same to you.
>
> > *I might take it again in November in the event that I bungle those tests.

I'd put dollars to donuts you did great!

My best,

-Jack

On Tue, Sep 22, 2020 at 1:21 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Interesting I'll look into that!

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Tue, Sep 22, 2020 at 1:15 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Similar to that. I used another language (IDL) to automatically produce a series of tcl/tk scripts and then executed those distributed over a number of cores on a workstation.

-Jack

On Tue, Sep 22, 2020 at 1:13 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Sounds good!

In that case did you operate solely in TCL?

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Tue, Sep 22, 2020 at 1:10 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> For >75% I meant observations where source flux was more than 75% bg flux, and for 4% I meant observations where source flux was ~4 % bg flux. That makes sense—interestingly the observations with source flux ~4% bg flux were, by best fit, in the SPL state, with the highest flux and gamma for all the observations that had reasonable values for these parameters.

Ah, now I see. Yes, when the bg flux is very high, the noise level (N=sqrt(B+S)) where B and S are background and signal counts in a channel, then the constraining power of the data (S/N) is lowest. If the background is reasonably estimated, the fits will likely be good but poorly constrained; if the background is off, then all bets are off.

> xD. I think it's a very impressive feat! Did you use pyxspec to loop through all the observations?

Thanks. I used a different language actually, but python/pyxspec would be an equally good way to do it.

Best,

-Jack

On Tue, Sep 22, 2020 at 12:39 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> That's good to know. I guess what's surprising is some relative of this large residual in the 1.6-2.0 KeV is present in most of the source flux > 75% observations (I wonder if the peak feature falsely inflates the source flux for some of them?). Going back to a couple emails ago, I was checking some of my all time best fits which had source fluxes around ~4%—what allows such faint observations to fit well?

Just to clarify, do you mean 75% and 4% of the Eddington limit, peak luminosity, or something else? Fainter observations, given some typical observation time, obviously have fewer counts which means larger fractional errors and less constraining power on the model. So if there is a deficiency in the calibration or model being used, it will look more prominent in brighter + long exposure time data.

> Also, I'm very intrigued by your methods in one of your 2016 papers , "STRONGER REFLECTION FROM BLACK HOLE ACCRETION DISKS IN SOFT X-RAY STATES". How did you automate fitting the 15,000 observations reliably and within a reasonable time frame?

A reasonable time frame is relative :), but I used about two dozen cores to run the fits which helped. I also stuck with computationally simplistic models, and for my runs with simpl, used lower resolution (via energies) in the run, since RXTE/PCA - the instrument used there - also has rather poor energy resolution compared to NICER.

> Could I possibly have the ~86 observations between obsids 1200220101 – 1200220186?

Spectral files are attached. Response files are too big for me to attach, so I'll keep them up here for a few days: https://nicer-web.mit.edu/nicerview/thadd_resp_m1727.tar.gz

> Ah that makes sense. In that case did Xu et al. just freeze Rin at the ISCO to have less free parameters?

That's the usual tactic, yes, when fitting for spin.

Best,

-Jack

On Thu, Sep 17, 2020 at 11:00 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Great to hear you've been digging in and exploring! I would suggest though that you avoid being too concerned about the residual features. If something is localized and only ~percent level deviation, then that's likely sufficient. *The goal should be to fit those sufficiently that they don't alter the continuum, but at some level it becomes diminishing returns to try fitting an artefact.* That said, if you wish, I can get you the data on MAXI J1727-203. Just let me know if you have a particular ObsID from Kevin's paper you want to try.

Your question about degeneracy is great. The condition of parameters being strongly correlated with one another is degeneracy. Basically, this is a very common situation where two parameters are dependent upon one another to

some extent, so a probable value of one will condition a probable range of values with the other. For instance, if you want to fit x and y with the constraint x^2+y^2 = 4+/-1, you would get a ring of radius 2 with x,y values that obviously depend on one another; they are *not independent*. Speaking generally for more realistic cases, the interaction between parameters is often pretty convoluted, but commonly things like Gamma and Normalization are correlated with one another, so a contour plot of the confidence interval would look banana shaped rather than like a circle.

Best,

-Jack

On Thu, Sep 17, 2020 at 9:18 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> Thanks for checking in! Since we last spoke I've been taking a deeper look into a couple articles (Alabarta et al. 2020, Xu et al. 2018, and Parikh et al. 2019) and playing around a bit with the residual fix you suggested. As tested on multiple observations with varying fit statistic contributions in the 1.6-1.8 KeV range (chi-squared contribs. ranging between 500-3000), the gaussian centered around 1.75 KeV combined with the "edge" works great!
>
> Most importantly, I found a method to more accurately investigate the residual features: *plt>wdata*. Now that I can actually see what the chi-sq contrib. is for every point, I've been having a lot more success in approaching residuals in the 1.6-2.2 KeV range, especially when the residual patterns after 1.8 KeV differ greatly from what can quickly be ameliorated with the edge feature (it seems that for the majority of observations the <1.8 KeV excess is easily subdued by the gaussian fix, but after 1.8 KeV things often get complicated, especially when a third feature shows up between 1.86 to 2.0 KeV. This third feature resembles a sort of gaussian-like crevice about 0.13 KeV in width and often reaches a chi^2 contribution of -5331 (+) at its minima, pushing the "edge" feature out to a threshold energy of ~2+ KeV). I haven't found a definitive solution yet, but a gaussian absorption with an energy of ~1.86 KeV and a width of ~0.14 KeV is looking promising.
>
> While I've been working with these MAXI J1535-571 residuals I've been wondering if the residuals Alabarta et al. discuss in Nicer data for MAXI J1727−203 are of similar shape—is there any chance I could procure some pipeline data from you on MAXI J1727-203 to look into this (if it's not an inconvenience for you and or you don't think it's a distraction on my part of course).
>
> Out of curiosity, what does "parameter degeneracy" mean? My first guess would be this example: if I only told you what the slope of a line was (e.g. '2'), and then asked you for the individual values of Δy and Δx—would Δy and Δx be degenerate? Example of where I came across this: (from Xu et al. 2018) "The inner disk radius, R_{in}, and the black hole spin parameter, a*, are degenerate, as they both control the effective inner accretion disk radius. Therefore, for simplicity, during the spectral fitting we assume that the inner disk extends down to the ISCO by fixing R_{in} at the radius of the ISCO, and fit for the black hole spin as a free parameter."
>
> Best!
> Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

Kiker, Thaddaeus <s024622@students.lmsd.org> Mon, Nov 30, 2020 at 12:36 PM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi!

> Thanksgiving was very nice, thanks!

Yay that's great to hear!

> Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.

Sounds good.

> YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.

!!! I will do this then!

> Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

Will do.

Thanks!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 12:28 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Thanksgiving was very nice, thanks! Glad yours with the grandparents was too.
>
> > I reprocessed the data last weekend (11/22) but the results were a little weird so I've been tweaking some things and prepping for reprocessing again this week.
> >
> > I was wondering if I should take a different approach to the instrumental residuals by running a preliminary fit on all files and logging the fit statistic landscape, so I can then loop through and identify which files actually require the 1.5-2.2 keV range to be ignored and which can have it noticed (I'm wondering about this because from a preliminary check a lot of the disk temperatures were behaving weirdly, which I think could be related to having less data in the thermal band to constrain them). What do you think?
>
> Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.
>
> > Also, I've only been fitting the silver, zero good time interval files; would you recommend me to fit some of the GTI1s, GT2s, etc. in hopes to get more good fits?
>
> YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.
>
> > P.S. I wrote two new standardized functions (one calculates and returns count rates, the other calculates and returns hardness ratios) and nice documentation for all my standard functions so far.
>
> Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)
>
> -Jack
>
> > On Mon, Nov 23, 2020 at 8:34 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> > > Will do!

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 23, 2020 at 4:30 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Sounds good! Glad your mom's computer has some CPU cycles to share. Keep me posted when you get the results in hand. Meantime, happy Thanksgiving.

-Jack

On Sun, Nov 22, 2020 at 8:01 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

Yes I've been fitting with the inner radius frozen to -1. That's good to know.

> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind. So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

Sounds good.

> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Yes I agree it was nice that they showed the underlying component.

I've been running the first iterations of the reprocessing last night and today and I've had a little breakthrough—last night I quickly updated the version of XSPEC I had on the vm that I installed on my mom's computer way back in June (I also installed relxill on it), and I've been dividing the work between her computer and mine—the routines are concluding much quicker (her computer has four cores xD).

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Fri, Nov 20, 2020 at 9:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.
>
> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind.
>
> So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

And in response to your email yesterday:

> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

Yes, spot on!

As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

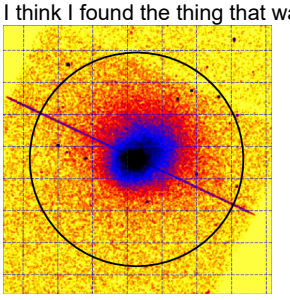
Best,

-Jack

On Fri, Nov 20, 2020 at 8:13 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Shoot! I forgot this bring this up. Would you recommend me to freeze the spin and inclination parameters to published values (that are in agreement with some of my previous best fits) to simplify the parameter space, or should I keep them free? The only benefit I can see from keeping them free would be sometimes error locks up with no variable parameters to use when free parameters get pegged due to their insignificance to the fit, and I think having them free could help prevent this...though spin and inclination seem like parameters that would be the first to get pegged during an error routine anyways.
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Thu, Nov 19, 2020 at 8:07 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hey!
>
> > Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.
>
> Yeah, safety is (rightly so) the first priority.
>
> > A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!
>
> Oo sounds cool, I hope you all have fun!
>
> > Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.
> Perfect.
>
> > The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon.
> Interesting.
>
> > then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.
> Very interesting.
>
> I think I found the thing that was on the edge of my memory: a Chandra image of the quasar H1821+643.
>
> 

So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

> Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

I should probably rephrase what I said earlier. When I ignored bad, no channels were ignored so I guess that meant no poor quality flags were raised.

> I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Agreed. I find it so enjoyable: the initial encounter with a seemingly insurmountable challenge, the brainstorming, the troubleshooting, and *especially* the successful execution of a solution. It's extremely satisfying—and to use an analogy from my youth* it feels kinda similar to when I would build things in lego.

*I say an analogy from my youth but that's somewhat misleading. I still occasionally play legos, and these days the only difference is that I have a partner: my younger brother ;).

> After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Definitely. That is exactly what I'm thinking.

> Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

:)).

Best!
Thaddaeus

Oh I almost forgot. I don't remember how/why I came across this paper (Arumugasamy et al. 2018), but after looking at the plots I've been meaning to ask you about them:

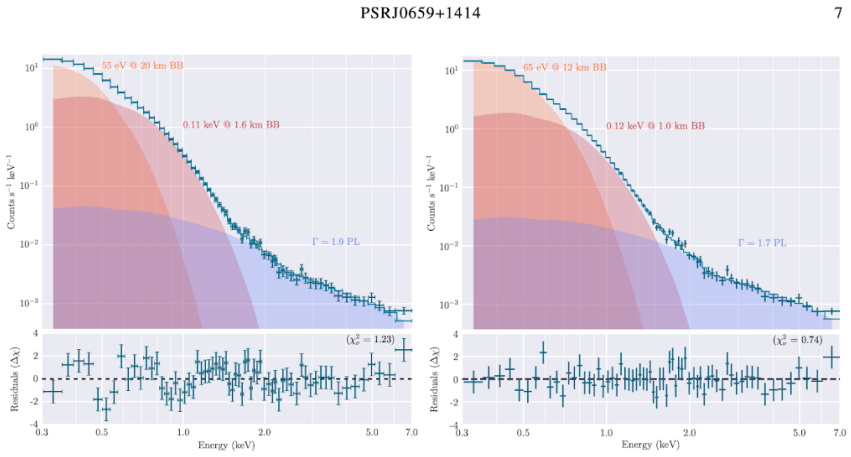


Figure 4. 2BBPL model, without (left) and with (right) `Gabs` absorption component, fit to the phase-integrated spectrum of J0659+1414. The model parameters correspond to maximum posterior probability, which in the case of G2BBPL are different from the distribution medians listed in Table 2.

I've been using the darkgrid style (like the plots above) with the rc font parameter set to Nimbus Roman in most of my plots ... but after seeing these plots I'm thinking I was to switch back to the mpl defaults—these plots are kinda distracting to me in their aesthetic.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Wed, Nov 18, 2020 at 10:05 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> > It was a wild week indeed—and things seem to keep getting crazier. We were in a hybrid model since October 19th, but as of Tuesday we went indefinitely back to all online which is a two sided coin. On one hand the online schedule gives me a lot more free time, but on the other I'm going to miss the extra class time in my harder classes like mechanics and calc. Thanksgiving is around the corner though! Up to any interesting plans yourself? I think we're just going to have my mom's parents over because we already interact with them frequently, but no big gathering this year (obviously).

No kidding! Feels like we're living in a bad TV show.

Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.

A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> Would it be okay if I emailed Dr. Cuneo about this? This is very important for how I structure the confidence intervals code (and whether I make it more sensitive and able to detect more than the four generalized abnormal scenarios described later in this email).

Sure thing, email away.

> Haha! I just read that NuStar's is ~2.5 *milliseconds.*

Yeah and that's actually not too bad compared to most things out there. NICER is just an absolute timing monster.

> I do have my pipeline correct for it automatically, so all your spectra already factor that in.
>
> Cool! Does that mean I can just go ahead and calculate count rates as number of counts / exp. time and not worry about adjusting the result because you've already accounted for deadtime?

Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

> deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.
>
> Yes. This makes sense. I feel like I've seen something related where the center of an object was too bright in the X-Ray band (which oversaturated the detector?) so the color gradient of brightness was black in the center because the author's couldn't determine the brightness at the center. It kinda looked like an artificial coronagraph. Would that be related to a situation where the dead time was more than the event time?

(Deep dive warning:)
Yes, you're speaking about "pileup" which isn't exactly related to deadtime except that both can be important when the source is bright. Deadtime, again, is from the detector shutting down for some portion of the time. Pileup concerns are most often for an imagng type of detector - more like the camera on your phone - that collects an exposure for some amount of time and then read it off.

Chandra, XMM-Newton, Swift, Suzaku, (somewhat confusingly NuStar is in both camps), let X-rays come in from the sky, get focused onto the CCD camera, and then in some time interval, often ~1s, reads off the image and clears the CCD. The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon. So when an individual pixel gets << 1 X-ray per frame on average, you are in good shape. *But* when there is significant probability of getting >~1 X-ray per frame on a given pixel, then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

This registering of multiple X-rays incorrectly as one more energetic single event is pileup, and it is the bane of most imaging detectors for those of us working with bright sources. (Pileup can occur in instruments like NICER too, when two X-rays hit during the event-latch phase, but this is something like a <1% effect for a 100 Crab-bright source, so well in the camp of being negligible for all practical purposes.)

In the most extreme cases of pileup, the pixel digitizer, which usually records values on something like a 16-bit value (e.g., from -32768:32767), can roll over and then appears negative when too much charge hits at once.

> I'm pretty sure the RXTE data I was working with earlier this year didn't have this identifier because if I remember correctly I couldn't execute *$ignore bad* on the data in XSPEC.

Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

> Here's a working version of the confidence interval / error results script:

> I put my XSPECtools.py file which has all of these functions into a custom module in my local site-packages folder for python and it seems like it's working perfectly. I import the file like this: *from personalastropy.xspectools import XSPECtools as xt*, and I execute a particular function like this: *xt.returnFitResults()*.
>
> Happy early Thanksgiving!
> Thaddaeus

That's really great Thaddaeus! I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Very well done!

After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

Best,

-Jack

> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

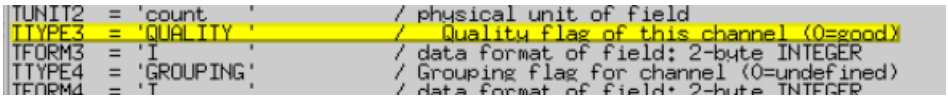
On Wed, Nov 11, 2020 at 12:07 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hey Thaddaeus,
>
> What a week it's been eh? (But way to go PA!)
>
> > I've held off from running another major fitting routine since the weekend because after I ran that my last one I decided that it was time to write some standardized scripts for wrangling the results from all the log files.
>
> Excellent!
>
> > Speaking of which, when Cuneo uses green triangles for "*parameters consistent with 0 within errors*", does this refer to situations when either the upper value of a parameter is equal to zero (because it got pegged at the hard upper limit) or when the lower limit is invalid and equal to zero?
>
> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> I *love* the outline of your routines, and "wow" is what I have to say about that. Way to go Thaddaeus, it's great you're tackling this and in a very useful manner.
>
> > Out of curiosity, where did Cuneo get the count rate values for his data? I don't think it's the same as the Net Count Rate that's displayed whenever you execute *$show data* in XSPEC....can Cuneo's count rate (c/s) be found by dividing the sum of the counts in the included channels by the total observation time?
>
> About 99% yes. "Usual" practice for NICER has become to normalize to an active set of 52 detectors. Your spectrum is a collection of all good events. So a tally of all the (good) events between 0.5-10 keV divided by the exposure time is indeed the count rate detected. However, if only using 49 or 50 detectors (also common), one would scale up by 52/49 to show the rate on the full instrument.
>
> The one very tiny wrinkle to all this is a property called "deadtime". Each time the electronics on a detector register an X-ray event, they "latch" meaning that the detector stops listening for new signals. Effectively, the detector is turned off for a short amount of time it uses to calculate properties of event (what time it arrived, and how much charge was deposited). For NICER, this is typically ~20 microseconds.
>
> As a concrete example of what this means in practice: when the count rate per detector reaches ~10^3/s (or in other words the full instrument light curve was at ~50,000/s), there would be ~20e-6 * 1e3 = 2% deadtime. This would mean that the *actual* count rate is ~2% higher than was detected (because the true exposure time was actually 98% the clock

time). Here, most likely Cuneo didn't worry about folding in deadtime effects (I do have my pipeline correct for it automatically, so all your spectra already factor that in).

This is obviously a very small here for NICER, but I wanted to dive into it because for many missions that aren't timing beasts like NICER, deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.

> Also sorta random, I came across a value called "quality" when I was looking at a file in fv; are the quality values the values XSPEC refers to when I say *$ignore bad*?



Yes exactly this. This won't tend to come up for NICER, but e.g., for instruments that have some bad pixels and a spatially dispersed spectrum (think the rainbow from a prism), or uncalibrated energy ranges, this can be used.

> * This is probably one of my favorite one-liner functions in Python:
>
> for line in f:
> linelist = (re.sub(' +', ',', line)).split(',')
>
> Where f is a file opened with: *with open(...) as f.*

Python is a wonder for its very efficient one-lining loops!

As usual, you're knocking it out of the park Thaddaeus.

In other news, a new grad student (Santi) just joined me and he's going to start looking at some QPO data.

Best,

-Jack

> On Thu, Oct 29, 2020 at 12:49 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Thaddaeus,
>
> Whoops - Santiago is my grad student and I notice my advisor brain accidentally called you Santi in my last message. Very sorry about that!
>
> Name confusions aside, very glad the patch worked ok. Yeah, setting query to yes can cause problems sometimes as you found. I don't have good rules for avoiding or escaping those runaways, but I've had luck before tweaking the extra convergence terms in the fit command when it's stuck (the delta & beta) and the tolerance term in error.
>
> -Jack
>
> On Thu, Oct 29, 2020 at 11:34 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Hi Dr. Steiner!
>
> Alright I think that patching did the trick; I'm currently running XSPEC v 12.11.0m. Surprisingly I didn't have to rebuild relxill which was nice. Now that the 12.11.0k patch has been installed, I think XSPEC just "snags" and keeps fitting if I have query set to yes instead of crashing—when I was looking into this, a particular observation ran an error routine for three days nonstop in the background until I aborted it!
>
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Thu, Oct 22, 2020 at 12:46 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Santi,

OK, that's encouraging! Let me know if the patching does the trick. Often for small patches, you won't need to recompile local models, but you should quickly see if it has trouble loading and that would be your sign.

It's hard to predict how parallel processing will impact a given task. The outcome depends on where the bottlenecks occur (if processing, then expect significant improvement, if memory swapping, then probably not). For running on multiple threads, a **rough** estimate is sqrt(N) gain, but that's just ballpark.

-Jack

On Thu, Oct 22, 2020 at 11:56 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

I just got some really great news on the crashes!

Last weekend I was dismayed because even though I was playing with the limits extensively (especially for gamma), the crashing was still occurring unabated. This made me wonder if it was a software issue, so I reached out to the HEASARC Help Desk. I found out that a very similar issue was fixed in XSPEC patch 12.11.0k: *For some complex fitting and error command cases, XSPEC may terminate with a GSL message such as 'SVD decomposition failed to converge'.* This wasn't encouraging at first because XSPEC patch 12.11.0k came out in June (I built the current version of xspec on my vm in ~July), but I checked my XSPEC version with *$version,* and it turns out that my XSPEC version is just 12.11.0! I'm working on installing the patch, I bet this will clear a lot of things up. I think I'm going to have to rebuild relxill after I execute the partial system rebuild.

The only thing that confuses me about this is that I didn't really have issues with core dumping when I was fitting relxill a lot back in August (I think it did come up for error fits back then though), so I'm confused why it really started acting up recently.

Also I don't think I've asked this previously, but out of curiosity, should I expect significant reductions in fit times with two core parallel processing?

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 22, 2020 at 10:52 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Just checking in to see how fits are progressing and if playing with those limits has allowed any headway against the crashes?

(Yes, whenever I "rm -rf " I try to take beat and check that I am not about to bilk the whole machine! With great commands-line tools comes great ...)

-Jack

On Fri, Oct 16, 2020 at 3:24 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

Now that I think about it, I do recall noticing that many (if not most) of the fit failures had gammas in the 3.4 range. Good to know about the 1.6-2.2 range for the residuals, I'll play with that now.

Oh about the attachments: the screen shot from visual studio is just some descriptions I was writing about the different functions I was using, and the microsoft paint one just illustrates the positive contribution that is almost always eliminated by a ~1.75 KeV gaussian of varying width.

Over this weekend should I continue to focus on maximizing the number of observations that I can get good fits and confidence intervals for?

I'm intrigued by this disk truncation controversy, I'm going to read about it this weekend.

Many thanks!
Thaddaeus

P.S. I was reading about some dangerous linux commands the other day; there are some unnerving commands out there. Imagine if someone ran rm -rf in your terminal. Shudder.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 15, 2020 at 10:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> Regarding the application of systematic errors, that's good to know. I've often seen something like the "NICER team recommends the application of a systematic error of 1%" in papers that use NICER data.

Yes, that's a "rough" benchmark, but a reasonable one.

> I love the common.tcl file, it's so handy! I think I'm just going to use it from now on because it's simpler on the execution end (just two commands), and it shows the error strings for each parameter.

Yes, it's quite nice work by Jeremy :).

> A little random: I came across an interesting statement in a paper by Plant et al. 2015 pertaining to observed disk truncation in the hard state of GX 339−4: "This result implies that the current sample of spin estimates in the hard state are inaccurate. Therefore, any distinct conclusions drawn from these estimates, such as the spin-powering of relativistic jets, may well be biased." Since that paper was published has this issue been fully exploited and resolved, or are there still black holes with possibly incorrect spin measurements out there? Also, is physical disk truncation or changing coronal height the more widely accepted cause for profiles similar to disk truncation?

This has been a topic with a lot of controversy that is still ongoing (Garcia et al 2015 and followup papers by Wang et al. 2019 talk about this). I'd say the Plant picture is strongly disfavored at present, but not entirely out. The robust view is that allowing for the possibility of truncation, a (bright) hard-state spin measurement should be regarded as an estimated lower limit. Presently, the argument is focused around how much truncation could there be in bright hard states, with most reasonable numbers being a few times the ISCO (a factor <10). (Coronal geometry is always an important consideration, but usually they tend to fit very compact.)

> An update on the rsp/arf issue: I modified my approach and fully integrated astropy.io.fits, so when I'm producing my xspec script commands rmf and arf file names are pulled directly from the header of each observation's fits file. Because of the two errors described below, I haven't checked how chi-square improves when the correct rmf and arf files are used though.

Very good, that should make some impact when working.

> I've encountered two weird errors I've never encountered before in XSPEC this week: the first is frequent core dumping [SVD decomposition failed to converge...Default GSL error handler invoked...Segmentation fault (core dumped)], and the second is this odd "XSPEC Error: No variable parameters for fit" after executing "fit" for a model with *multiple* free parameters (including the usual Gamma, FracSctr, Tin, diskbb norm, etc.). My first thought was "*is this related to relxill having problems due to a low gamma*"—but these issues both happen even when Gamma is > 2.

The table can be sparse in a few places, and I think very large Gammas are also sparse (>~3.4, if memory serves). I'd say try freezing parameters and retrying the fit to identify the culprit parameter.

> Also a little random: this week I decided it was time to start commenting on my functions in my python files—at one point I was looking through some older files for a particular technique I used and it took a little while to find the correct version of it, so I decided that from now on I'm describing my functions for future reference.

Yes, documenting code is super important and a great habit to get used to. (I wish I was better about it!)

> This is preliminary, but I attached an example of a chi-scape scarred by a common morph of the residual I last sent you. The positive contribution that begins shortly after 1.6 KeV is almost always eliminated by a ~1.75 KeV gaussian of varying width for most observations. The current issue is modeling the various forms the following negative residuals take. Currently working on those. Earlier you mentioned that we could try ignoring that narrow range where these residuals occur, would ignoring channels between 1.6-2.2 KeV be ignoring too much of the data? If we decide to ignore that range, I would think it best to get >*plt wdata* outputs to exactly nail down the dimensions of this range, but for now should I keep trying to model the residuals?
>
> I was reading through some older emails, going to look into your recommendation regarding logxi.

No, I think 1.6-2.2 isn't crazy and you can certainly roll with that if you are having too much trouble taming the residuals with a model. Maybe do a couple test cases to see how the model parameters are affected on both fronts.

Hey, and I didn't see a description of your attachments in case you want to say anything about those.

Glad you're back in the thick of things.

Best,

-Jack

> On Thu, Oct 8, 2020 at 1:35 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> > Hi Dr. Steiner!
> >
> > Quick note: I've been getting back into it this week and should have some interesting things to report this weekend!
> >
> > Thaddaeus
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
>
> On Tue, Sep 29, 2020 at 2:19 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> > Hi Thaddaeus,
> >
> > Thanks for the update.
> >
> > > Quick note on the residuals: since we last communicated I investigated the application of a systematic error (3-5%) in the < 2 KeV energy band, but this didn't improve the residuals so I'm assuming that you already accounted for this, like the binning, in your reprocessing?
> >
> > No, actually. But systematic errors are only incorporated with some fit statistics - chisq for instance, but I don't believe are incorporated in cstat or pgstat. The way I like to think about it is

the systematic error acts as a reweighting of the data's constraint and serves to de-emphasize regions with the highest signal. I offer for information sake that that 3-5% is too large a systematic error to include as a rule. Something like 1-2% is more reasonable for a region with poorer calibration quality.

> I also discovered that I've been unintentionally forcing an incorrect response file on multiple MAXI J1535-571 observations—the rsp I was using for all observations was `nicer_d49_55575341.rmf`, but after running a quick check (the fitsio package has been so helpful, thanks for recommending it to me!), I found that multiple observations request `nicer_d49_u100_150_55575341.rmf` in their headers.

Ah! That's very good to know, and may be the culprit for some of the residuals you've been battling. Check that the arf files (keyword ANCRFILE) is also set correctly.

> I'm likely almost done with the SAT (took it Saturday, going to take it again this coming Saturday)*, and once I'm done I aim to return to a progress pace similar to the pace we had in the summer.

Congratulations - that's a nice milestone!

> G'mar chatima tova (if you observe Yom Kippur)!

Same to you.

> *I might take it again in November in the event that I bungle those tests.

I'd put dollars to donuts you did great!

My best,

-Jack

> On Tue, Sep 22, 2020 at 1:21 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Interesting I'll look into that!
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi
>
> On Tue, Sep 22, 2020 at 1:15 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Similar to that. I used another language (IDL) to automatically produce a series of tcl/tk scripts and then executed those distributed over a number of cores on a workstation.
>
> -Jack
>
> On Tue, Sep 22, 2020 at 1:13 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Sounds good!
>
> In that case did you operate solely in TCL?
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Tue, Sep 22, 2020 at 1:10 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> For >75% I meant observations where source flux was more than 75% bg flux, and for 4% I meant observations where source flux was ~4 % bg flux. That makes sense—interestingly the observations with source flux ~4% bg flux were, by best fit, in the SPL state, with the highest flux and gamma for all the observations that had reasonable values for these parameters.

Ah, now I see. Yes, when the bg flux is very high, the noise level (N=sqrt(B+S)) where B and S are background and signal counts in a channel, then the constraining power of the data (S/N) is lowest. If the background is reasonably estimated, the fits will likely be good but poorly constrained; if the background is off, then all bets are off.

> xD. I think it's a very impressive feat! Did you use pyxspec to loop through all the observations?

Thanks. I used a different language actually, but python/pyxspec would be an equally good way to do it.

Best,

-Jack

> On Tue, Sep 22, 2020 at 12:39 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
>
> Hi Thaddaeus,
>
> > That's good to know. I guess what's surprising is some relative of this large residual in the 1.6-2.0 KeV is present in most of the source flux > 75% observations (I wonder if the peak feature falsely inflates the source flux for some of them?). Going back to a couple emails ago, I was checking some of my all time best fits which had source fluxes around ~4%—what allows such faint observations to fit well?
>
> Just to clarify, do you mean 75% and 4% of the Eddington limit, peak luminosity, or something else? Fainter observations, given some typical observation time, obviously have fewer counts which means larger fractional errors and less constraining power on the model. So if there is a deficiency in the calibration or model being used, it will look more prominent in brighter + long exposure time data.
>
> > Also, I'm very intrigued by your methods in one of your 2016 papers , "STRONGER REFLECTION FROM BLACK HOLE ACCRETION DISKS IN SOFT X-RAY STATES". How did you automate fitting the 15,000 observations reliably and within a reasonable time frame?
>
> A reasonable time frame is relative :), but I used about two dozen cores to run the fits which helped. I also stuck with computationally simplistic models, and for my runs with simpl, used lower resolution (via energies) in the run, since RXTE/PCA - the instrument used there - also has rather poor energy resolution compared to NICER.
>
> > Could I possibly have the ~86 observations between obsids 1200220101 – 1200220186?
>
> Spectral files are attached. Response files are too big for me to attach, so I'll keep them up here for a few days: https://nicer-web.mit.edu/nicerview/thadd_resp_m1727.tar.gz
>
> > Ah that makes sense. In that case did Xu et al. just freeze Rin at the ISCO to have less free parameters?
>
> That's the usual tactic, yes, when fitting for spin.

Best,

-Jack

On Thu, Sep 17, 2020 at 11:00 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Great to hear you've been digging in and exploring! I would suggest though that you avoid being too concerned about the residual features. If something is localized and only ~percent level deviation, then that's likely sufficient. *The goal should be to fit those sufficiently that they don't alter the continuum, but at some level it becomes diminishing returns to try fitting an artefact.* That said, if you wish, I can get you the data on MAXI J1727-203. Just let me know if you have a particular ObsID from Kevin's paper you want to try.
>
> Your question about degeneracy is great. The condition of parameters being strongly correlated with one another is degeneracy. Basically, this is a very common situation where two parameters are dependent upon one another to some extent, so a probable value of one will condition a probable range of values with the other. For instance, if you want to fit x and y with the constraint x^2+y^2 = 4+/-1, you would get a ring of radius 2 with x,y values that obviously depend on one another; they are *not independent*. Speaking generally for more realistic cases, the interaction between parameters is often pretty convoluted, but commonly things like Gamma and Normalization are correlated with one another, so a contour plot of the confidence interval would look banana shaped rather than like a circle.
>
> Best,
>
> -Jack

On Thu, Sep 17, 2020 at 9:18 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> Thanks for checking in! Since we last spoke I've been taking a deeper look into a couple articles (Alabarta et al. 2020, Xu et al. 2018, and Parikh et al. 2019) and playing around a bit with the residual fix you suggested. As tested on multiple observations with varying fit statistic contributions in the 1.6-1.8 KeV range (chi-squared contribs. ranging between 500-3000), the gaussian centered around 1.75 KeV combined with the "edge" works great!
>
> Most importantly, I found a method to more accurately investigate the residual features: *plt>wdata*. Now that I can actually see what the chi-sq contrib. is for every point, I've been having a lot more success in approaching residuals in the 1.6-2.2 KeV range, especially when the residual patterns after 1.8 KeV differ greatly from what can quickly be ameliorated with the edge feature (it seems that for the majority of observations the <1.8 KeV excess is easily subdued by the gaussian fix, but after 1.8 KeV things often get complicated, especially when a third feature shows up between 1.86 to 2.0 KeV. This third feature resembles a sort of gaussian-like crevice about 0.13 KeV in width and often reaches a chi^2 contribution of -5331 (+) at its minima, pushing the "edge" feature out to a threshold energy of ~2+ KeV). I haven't found a definitive solution yet, but a gaussian absorption with an energy of ~1.86 KeV and a width of ~0.14 KeV is looking promising.
>
> While I've been working with these MAXI J1535-571 residuals I've been wondering if the residuals Alabarta et al. discuss in Nicer data for MAXI J1727−203 are of similar shape—is there any chance I could procure some pipeline data from you on MAXI J1727-203 to look into this (if it's not an

inconvenience for you and or you don't think it's a distraction on my part of course).

Out of curiosity, what does "parameter degeneracy" mean? My first guess would be this example: if I only told you what the slope of a line was (e.g. '2'), and then asked you for the individual values of Δy and Δx—would Δy and Δx be degenerate? Example of where I came across this: (from Xu et al. 2018) "The inner disk radius, R_{in}, and the black hole spin parameter, a*, are degenerate, as they both control the effective inner accretion disk radius. Therefore, for simplicity, during the spectral fitting we assume that the inner disk extends down to the ISCO by fixing R_{in} at the radius of the ISCO, and fit for the black hole spin as a free parameter."

Best!
Thaddaeus

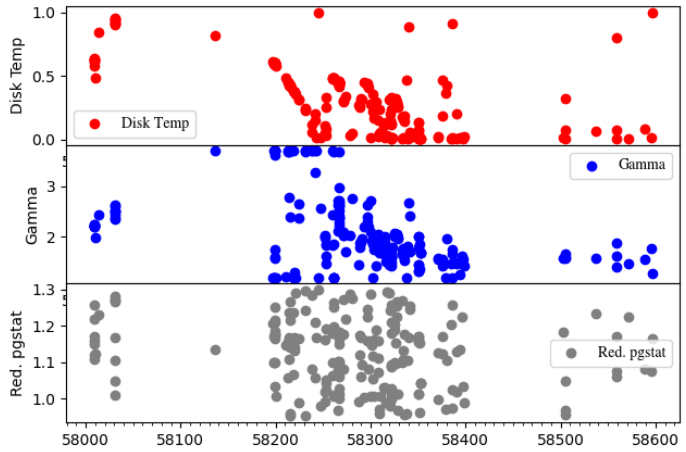
"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

Kiker, Thaddaeus <s024622@students.lmsd.org> Wed, Dec 9, 2020 at 11:39 AM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

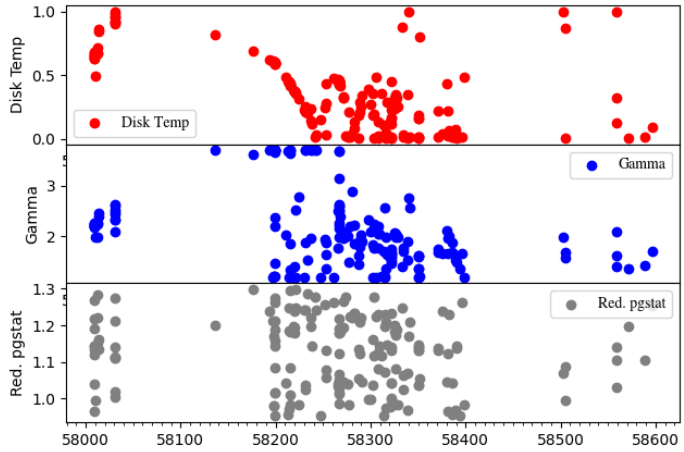
Hi Dr. Steiner!

Phew! The number of potential files for fitting has shot up to 720 from 185 now that I'm searching for and including all GTIs files.

I ran a couple fitting routines with a simple tbabs(diskbb+nthcomp) model on all the files just to get a feel for how including a lot more files would possibly change things, and the initial results from these fitting routines were quite unusual (though they were blisteringly fast compared to relxill fits regardless —with nH frozen on these routines, I was fitting all the files with 700 fit iterations each in under an hour on my mom's computer) :



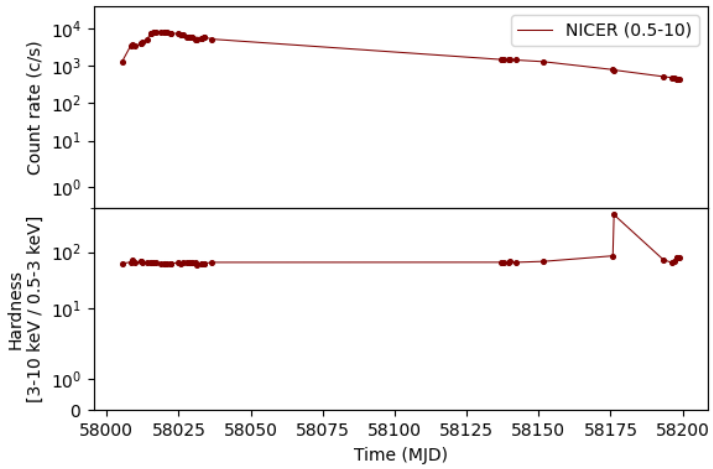
^ That routine had diskkbb Tin upper limit set to 1, gamma upper limit set to 3.7, nH frozen to 3.2, and the entire energy range from 0.5-10.0 keV noticed.



^That routine was the same except the energy interval 1.5-2.3 keV was ignored.

I think I see a little pattern of correlated variation between disk temp and gamma masked under the erratic points in both plots...I'm wondering, should I set some cutoff exposure time for the files? I recall seeing some ~22 sec exp. time data files when I was looking through other GTIs, and I'm thinking since these files would have less data to constrain the model, crazy parameters can be permissibly returned by XSPEC when these files are fit.

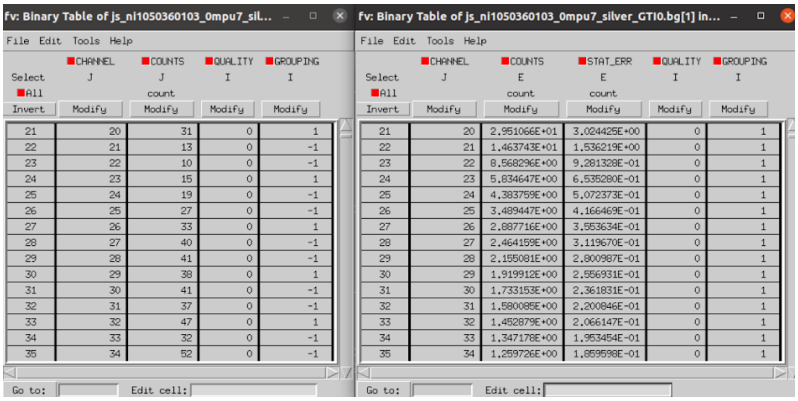
Also, could I get your advice on calculating hardness ratios? I modified my count rate function to correct for background counts and it's working pretty well (see the top subplot below):



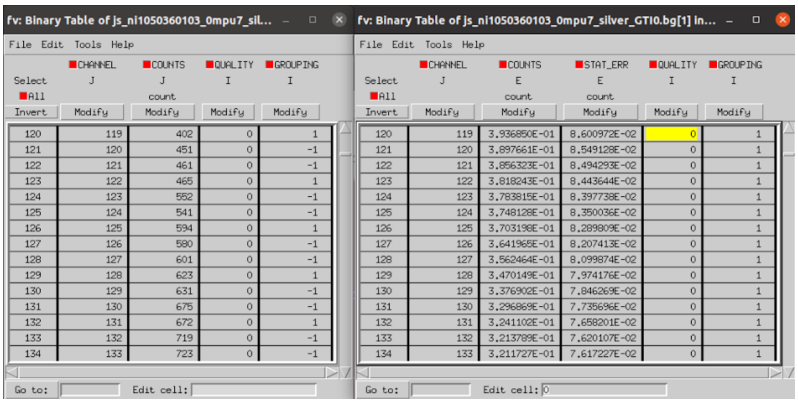
Almost identical to Cuneo's count rate plot, eh?

As you can see in the bottom subplot, the hardness ratio values are crazy, and I can't figure out how to rein them in. The way my function for hardness ratio works is I sum the number of counts in the parameter defined hard band and subtract the number of background counts in the predefined hard band from that value, giving me a net hard band counts sum. I then divide that value by (sum of soft counts-sum soft background counts). Code is attached.

I was checking with fv, and it's interesting that in this case (and what I think is true in general) the bg counts are very low in the hard band but fairly high in the thermal:



Channels 22-99 = 0.5-3 keV



Channels 101-254 = 3.0-10.0 keV

Cheers!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 3:36 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> > Thanksgiving was very nice, thanks!
> Yay that's great to hear!
>
> > Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.
> Sounds good.
>
> > YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.
> !!! I will do this then!
>
> > Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)
> Will do.
>
> Thanks!
> Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 12:28 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
Hi Thaddaeus,

Thanksgiving was very nice, thanks! Glad yours with the grandparents was too.

> I reprocessed the data last weekend (11/22) but the results were a little weird so I've been tweaking some things and prepping for reprocessing again this week.
>
> I was wondering if I should take a different approach to the instrumental residuals by running a preliminary fit on all files and logging the fit statistic landscape, so I can then loop through and identify which files actually require the 1.5-2.2 keV range to be ignored and which can have it noticed (I'm wondering about this because from a preliminary check a lot of the disk temperatures were behaving weirdly, which I think could be related to having less data in the thermal band to constrain them). What do you think?

Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.

> Also, I've only been fitting the silver, zero good time interval files; would you recommend me to fit some of the GTI1s, GT2s, etc. in hopes to get more good fits?

YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.

> P.S. I wrote two new standardized functions (one calculates and returns count rates, the other calculates and returns hardness ratios) and nice documentation for all my standard functions so far.

Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

-Jack

> On Mon, Nov 23, 2020 at 8:34 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Will do!
>
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
>
> > On Mon, Nov 23, 2020 at 4:30 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> > Hi Thaddaeus,
> >
> > Sounds good! Glad your mom's computer has some CPU cycles to share. Keep me posted when you get the results in hand. Meantime, happy Thanksgiving.
> >
> > -Jack
> >
> > On Sun, Nov 22, 2020 at 8:01 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> > Hi!
> >
> > > Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

Yes I've been fitting with the inner radius frozen to -1. That's good to know.

> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind. So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

Sounds good.

> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Yes I agree it was nice that they showed the underlying component.

I've been running the first iterations of the reprocessing last night and today and I've had a little breakthrough—last night I quickly updated the version of XSPEC I had on the vm that I installed on my mom's computer way back in June (I also installed relxill on it), and I've been dividing the work between her computer and mine—the routines are concluding much quicker (her computer has four cores xD).

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Fri, Nov 20, 2020 at 9:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.
>
> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind.
>
> So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)
>
> And in response to your email yesterday:
>
> > So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?
>
> Yes, spot on!
>
> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.
>
> Best,
>
> -Jack

On Fri, Nov 20, 2020 at 8:13 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Shoot! I forgot this bring this up. Would you recommend me to freeze the spin and inclination parameters to published values (that are in agreement with some of my previous best fits) to simplify the parameter space, or should I keep them free? The only benefit I can see from keeping them free would be sometimes error locks up with no variable parameters to use when free

parameters get pegged due to their insignificance to the fit, and I think having them free could help prevent this...though spin and inclination seem like parameters that would be the first to get pegged during an error routine anyways.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Nov 19, 2020 at 8:07 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hey!
>
> > Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.
>
> Yeah, safety is (rightly so) the first priority.
>
> > A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!
>
> Oo sounds cool, I hope you all have fun!
>
> > Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.
> Perfect.
>
> > The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon.
> Interesting.
>
> > then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.
> Very interesting.
>
> I think I found the thing that was on the edge of my memory: a Chandra image of the quasar H1821+643.
>
> 
>
> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?
>
> > Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.
> I should probably rephrase what I said earlier. When I ignored bad, no channels were ignored so I guess that meant no poor quality flags were raised.
>
> > I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.
> Agreed. I find it so enjoyable: the initial encounter with a seemingly insurmountable challenge, the brainstorming, the troubleshooting, and *especially* the successful execution of a solution. It's extremely satisfying—and to use an analogy from my youth* it feels kinda similar to when I would build things in lego.
>
> *I say an analogy from my youth but that's somewhat misleading. I still occasionally play legos, and these days the only difference is that I have a partner: my younger brother ;).
>
> > After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?
> Definitely. That is exactly what I'm thinking.
>
> > Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.
> :)).
>
> Best!

Thaddaeus

Oh I almost forgot. I don't remember how/why I came across this paper (Arumugasamy et al. 2018), but after looking at the plots I've been meaning to ask you about them:

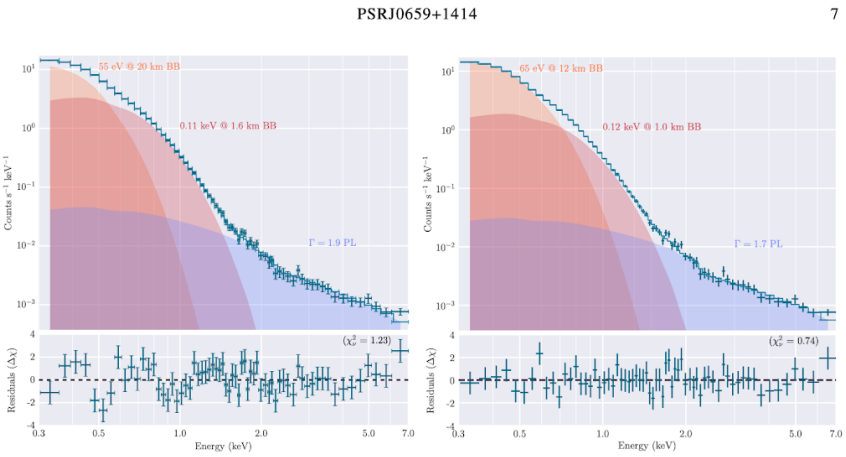


Figure 4. 2BBPL model, without (left) and with (right) `Gabs` absorption component, fit to the phase-integrated spectrum of J0659+1414. The model parameters correspond to maximum posterior probability, which in the case of G2BBPL are different from the distribution medians listed in Table 2.

I've been using the darkgrid style (like the plots above) with the rc font parameter set to Nimbus Roman in most of my plots ... but after seeing these plots I'm thinking I was to switch back to the mpl defaults—these plots are kinda distracting to me in their aesthetic.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Wed, Nov 18, 2020 at 10:05 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> > It was a wild week indeed—and things seem to keep getting crazier. We were in a hybrid model since October 19th, but as of Tuesday we went indefinitely back to all online which is a two sided coin. On one hand the online schedule gives me a lot more free time, but on the other I'm going to miss the extra class time in my harder classes like mechanics and calc. Thanksgiving is around the corner though! Up to any interesting plans yourself? I think we're just going to have my mom's parents over because we already interact with them frequently, but no big gathering this year (obviously).
>
> No kidding! Feels like we're living in a bad TV show.
>
> Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.
>
> A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!
>
> > Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
> >
> > Would it be okay if I emailed Dr. Cuneo about this? This is very important for how I structure the confidence intervals code (and whether I make it more sensitive and able to detect more than the four generalized abnormal scenarios described later in this email).
>
> Sure thing, email away.
>
> > Haha! I just read that NuStar's is ~2.5 *milliseconds.*

Yeah and that's actually not too bad compared to most things out there. NICER is just an absolute timing monster.

> I do have my pipeline correct for it automatically, so all your spectra already factor that in.
>
> Cool! Does that mean I can just go ahead and calculate count rates as number of counts / exp. time and not worry about adjusting the result because you've already accounted for deadtime?

Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

> deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.
>
> Yes. This makes sense. I feel like I've seen something related where the center of an object was too bright in the X-Ray band (which oversaturated the detector?) so the color gradient of brightness was black in the center because the author's couldn't determine the brightness at the center. It kinda looked like an artificial coronagraph. Would that be related to a situation where the dead time was more than the event time?

(Deep dive warning:)
Yes, you're speaking about "pileup" which isn't exactly related to deadtime except that both can be important when the source is bright. Deadtime, again, is from the detector shutting down for some portion of the time. Pileup concerns are most often for an imagng type of detector - more like the camera on your phone - that collects an exposure for some amount of time and then read it off.

Chandra, XMM-Newton, Swift, Suzaku, (somewhat confusingly NuStar is in both camps), let X-rays come in from the sky, get focused onto the CCD camera, and then in some time interval, often ~1s, reads off the image and clears the CCD. The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon. So when an individual pixel gets << 1 X-ray per frame on average, you are in good shape. *But* when there is significant probability of getting >~1 X-ray per frame on a given pixel, then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

This registering of multiple X-rays incorrectly as one more energetic single event is pileup, and it is the bane of most imaging detectors for those of us working with bright sources. (Pileup can occur in instruments like NICER too, when two X-rays hit during the event-latch phase, but this is something like a <1% effect for a 100 Crab-bright source, so well in the camp of being negligible for all practical purposes.)

In the most extreme cases of pileup, the pixel digitizer, which usually records values on something like a 16-bit value (e.g., from -32768:32767), can roll over and then appears negative when too much charge hits at once.

> I'm pretty sure the RXTE data I was working with earlier this year didn't have this identifier because if I remember correctly I couldn't execute *$ignore bad* on the data in XSPEC.

Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

> Here's a working version of the confidence interval / error results script:
>
> I put my XSPECtools.py file which has all of these functions into a custom module in my local site-packages folder for python and it seems like it's working perfectly. I import the file like this: *from personalastropy.xspectools import XSPECtools as xt*, and I execute a particular function like this: *xt.returnFitResults().*
>
> Happy early Thanksgiving!
> Thaddaeus

That's really great Thaddaeus! I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Very well done!

After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

Best,

-Jack

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Wed, Nov 11, 2020 at 12:07 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hey Thaddaeus,

What a week it's been eh? (But way to go PA!)

> I've held off from running another major fitting routine since the weekend because after I ran that my last one I decided that it was time to write some standardized scripts for wrangling the results from all the log files.

Excellent!

> Speaking of which, when Cuneo uses green triangles for "*parameters consistent with 0 within errors*", does this refer to situations when either the upper value of a parameter is equal to zero (because it got pegged at the hard upper limit) or when the lower limit is invalid and equal to zero?

Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).

I *love* the outline of your routines, and "wow" is what I have to say about that. Way to go Thaddaeus, it's great you're tackling this and in a very useful manner.

> Out of curiosity, where did Cuneo get the count rate values for his data? I don't think it's the same as the Net Count Rate that's displayed whenever you execute *$show data* in XSPEC....can Cuneo's count rate (c/s) be found by dividing the sum of the counts in the included channels by the total observation time?

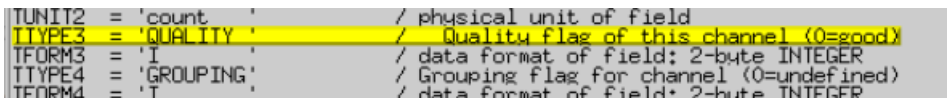
About 99% yes. "Usual" practice for NICER has become to normalize to an active set of 52 detectors. Your spectrum is a collection of all good events. So a tally of all the (good) events between 0.5-10 keV divided by the exposure time is indeed the count rate detected. However, if only using 49 or 50 detectors (also common), one would scale up by 52/49 to show the rate on the full instrument.

The one very tiny wrinkle to all this is a property called "deadtime". Each time the electronics on a detector register an X-ray event, they "latch" meaning that the detector stops listening for new signals. Effectively, the detector is turned off for a short amount of time it uses to calculate properties of event (what time it arrived, and how much charge was deposited). For NICER, this is typically ~20 microseconds.

As a concrete example of what this means in practice: when the count rate per detector reaches ~10^3/s (or in other words the full instrument light curve was at ~50,000/s), there would be ~20e-6 * 1e3 = 2% deadtime. This would mean that the *actual* count rate is ~2% higher than was detected (because the true exposure time was actually 98% the clock time). Here, most likely Cuneo didn't worry about folding in deadtime effects (I do have my pipeline correct for it automatically, so all your spectra already factor that in).

This is obviously a very small here for NICER, but I wanted to dive into it because for many missions that aren't timing beasts like NICER, deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.

> Also sorta random, I came across a value called "quality" when I was looking at a file in fv; are the quality values the values XSPEC refers to when I say *$ignore bad*?



Yes exactly this. This won't tend to come up for NICER, but e.g., for instruments that have some bad pixels and a spatially dispersed spectrum (think the rainbow from a prism), or uncalibrated energy ranges, this can be used.

> * This is probably one of my favorite one-liner functions in Python:
>
> for line in f:

linelist = (re.sub(' +', ',', line)).split(',')

Where f is a file opened with: *with open(...) as f*.

Python is a wonder for its very efficient one-lining loops!

As usual, you're knocking it out of the park Thaddaeus.

In other news, a new grad student (Santi) just joined me and he's going to start looking at some QPO data.

Best,

-Jack

On Thu, Oct 29, 2020 at 12:49 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Whoops - Santiago is my grad student and I notice my advisor brain accidentally called you Santi in my last message. Very sorry about that!

Name confusions aside, very glad the patch worked ok. Yeah, setting query to yes can cause problems sometimes as you found. I don't have good rules for avoiding or escaping those runaways, but I've had luck before tweaking the extra convergence terms in the fit command when it's stuck (the delta & beta) and the tolerance term in error.

-Jack

On Thu, Oct 29, 2020 at 11:34 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi Dr. Steiner!

Alright I think that patching did the trick; I'm currently running XSPEC v 12.11.0m. Surprisingly I didn't have to rebuild relxill which was nice. Now that the 12.11.0k patch has been installed, I think XSPEC just "snags" and keeps fitting if I have query set to yes instead of crashing—when I was looking into this, a particular observation ran an error routine for three days nonstop in the background until I aborted it!

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 22, 2020 at 12:46 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Santi,

OK, that's encouraging! Let me know if the patching does the trick. Often for small patches, you won't need to recompile local models, but you should quickly see if it has trouble loading and that would be your sign.

It's hard to predict how parallel processing will impact a given task. The outcome depends on where the bottlenecks occur (if processing, then expect significant improvement, if memory swapping, then probably not). For running on multiple threads, a **rough** estimate is sqrt(N) gain, but that's just ballpark.

-Jack

On Thu, Oct 22, 2020 at 11:56 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

I just got some really great news on the crashes!

Last weekend I was dismayed because even though I was playing with the limits extensively (especially for gamma), the crashing was still occurring unabated. This made me wonder if it was a software issue, so I reached out to the HEASARC Help Desk. I found out that a very similar issue was fixed in XSPEC patch 12.11.0k: *For some complex fitting and error command cases, XSPEC may terminate with a GSL message such as 'SVD decomposition failed to converge'.* This wasn't encouraging at first because

XSPEC patch 12.11.0k came out in June (I built the current version of xspec on my vm in ~July), but I checked my XSPEC version with *$version,* and it turns out that my XSPEC version is just 12.11.0! I'm working on installing the patch, I bet this will clear a lot of things up. I think I'm going to have to rebuild relxill after I execute the partial system rebuild.

The only thing that confuses me about this is that I didn't really have issues with core dumping when I was fitting relxill a lot back in August (I think it did come up for error fits back then though), so I'm confused why it really started acting up recently.

Also I don't think I've asked this previously, but out of curiosity, should I expect significant reductions in fit times with two core parallel processing?

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 22, 2020 at 10:52 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Just checking in to see how fits are progressing and if playing with those limits has allowed any headway against the crashes?
>
> (Yes, whenever I "rm -rf " I try to take beat and check that I am not about to bilk the whole machine! With great commands-line tools comes great ...)
>
> -Jack
>
> On Fri, Oct 16, 2020 at 3:24 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
>
> > Hi!
> >
> > Now that I think about it, I do recall noticing that many (if not most) of the fit failures had gammas in the 3.4 range. Good to know about the 1.6-2.2 range for the residuals, I'll play with that now.
> >
> > Oh about the attachments: the screen shot from visual studio is just some descriptions I was writing about the different functions I was using, and the microsoft paint one just illustrates the positive contribution that is almost always eliminated by a ~1.75 KeV gaussian of varying width.
> >
> > Over this weekend should I continue to focus on maximizing the number of observations that I can get good fits and confidence intervals for?
> >
> > I'm intrigued by this disk truncation controversy, I'm going to read about it this weekend.
> >
> > Many thanks!
> > Thaddaeus
> >
> > P.S. I was reading about some dangerous linux commands the other day; there are some unnerving commands out there. Imagine if someone ran rm -rf in your terminal. Shudder.
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
> >
> > On Thu, Oct 15, 2020 at 10:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> >
> > > Hi Thaddaeus,
> > >
> > > > Regarding the application of systematic errors, that's good to know. I've often seen something like the "NICER team recommends the application of a systematic error of 1%" in papers that use NICER data.

Yes, that's a "rough" benchmark, but a reasonable one.

> I love the common.tcl file, it's so handy! I think I'm just going to use it from now on because it's simpler on the execution end (just two commands), and it shows the error strings for each parameter.

Yes, it's quite nice work by Jeremy :).

> A little random: I came across an interesting statement in a paper by Plant et al. 2015 pertaining to observed disk truncation in the hard state of GX 339−4: "This result implies that the current sample of spin estimates in the hard state are inaccurate. Therefore, any distinct conclusions drawn from these estimates, such as the spin-powering of relativistic jets, may well be biased." Since that paper was published has this issue been fully exploited and resolved, or are there still black holes with possibly incorrect spin measurements out there? Also, is physical disk truncation or changing coronal height the more widely accepted cause for profiles similar to disk truncation?

This has been a topic with a lot of controversy that is still ongoing (Garcia et al 2015 and followup papers by Wang et al. 2019 talk about this). I'd say the Plant picture is strongly disfavored at present, but not entirely out. The robust view is that allowing for the possibility of truncation, a (bright) hard-state spin measurement should be regarded as an estimated lower limit. Presently, the argument is focused around how much truncation could there be in bright hard states, with most reasonable numbers being a few times the ISCO (a factor <10). (Coronal geometry is always an important consideration, but usually they tend to fit very compact.)

> An update on the rsp/arf issue: I modified my approach and fully integrated astropy.io.fits, so when I'm producing my xspec script commands rmf and arf file names are pulled directly from the header of each observation's fits file. Because of the two errors described below, I haven't checked how chi-square improves when the correct rmf and arf files are used though.

Very good, that should make some impact when working.

> I've encountered two weird errors I've never encountered before in XSPEC this week: the first is frequent core dumping [SVD decomposition failed to converge...Default GSL error handler invoked...Segmentation fault (core dumped)], and the second is this odd "XSPEC Error: No variable parameters for fit" after executing "fit" for a model with *multiple* free parameters (including the usual Gamma, FracSctr, Tin, diskbb norm, etc.). My first thought was "*is this related to relxill having problems due to a low gamma"*—but these issues both happen even when Gamma is > 2.

The table can be sparse in a few places, and I think very large Gammas are also sparse (>~3.4, if memory serves). I'd say try freezing parameters and retrying the fit to identify the culprit parameter.

> Also a little random: this week I decided it was time to start commenting on my functions in my python files—at one point I was looking through some older files for a particular technique I used and it took a little while to find the correct version of it, so I decided that from now on I'm describing my functions for future reference.

Yes, documenting code is super important and a great habit to get used to. (I wish I was better about it!)

> This is preliminary, but I attached an example of a chi-scape scarred by a common morph of the residual I last sent you. The positive contribution that begins shortly after 1.6 KeV is almost always eliminated by a ~1.75 KeV gaussian of varying width for most observations. The current issue is modeling the various forms the following negative residuals take. Currently working on those. Earlier you mentioned that we could try ignoring that narrow range where these residuals occur, would ignoring channels between 1.6-2.2 KeV be ignoring too much of the data? If we decide to ignore that range, I would think it best to get *>plt wdata* outputs to exactly nail down the dimensions of this range, but for now should I keep trying to model the residuals?
>
> I was reading through some older emails, going to look into your recommendation regarding logxi.

No, I think 1.6-2.2 isn't crazy and you can certainly roll with that if you are having too much trouble taming the residuals with a model. Maybe do a couple test cases to see how the model parameters are affected on both fronts.

Hey, and I didn't see a description of your attachments in case you want to say anything about those.

Glad you're back in the thick of things.

Best,

-Jack

On Thu, Oct 8, 2020 at 1:35 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi Dr. Steiner!
>
> Quick note: I've been getting back into it this week and should have some interesting things to report this weekend!
>
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Tue, Sep 29, 2020 at 2:19 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Thanks for the update.
>
> > Quick note on the residuals: since we last communicated I investigated the application of a systematic error (3-5%) in the < 2 KeV energy band, but this didn't improve the residuals so I'm assuming that you already accounted for this, like the binning, in your reprocessing?
>
> No, actually. But systematic errors are only incorporated with some fit statistics - chisq for instance, but I don't believe are incorporated in cstat or pgstat. The way I like to think about it is the systematic error acts as a reweighting of the data's constraint and serves to de-emphasize regions with the highest signal. I offer for information sake that that 3-5% is too large a systematic error to include as a rule. Something like 1-2% is more reasonable for a region with poorer calibration quality.
>
> > I also discovered that I've been unintentionally forcing an incorrect response file on multiple MAXI J1535-571 observations—the rsp I was using for all observations was `nicer_d49_55575341.rmf`, but after running a quick check (the fitsio package has been so helpful, thanks for recommending it to me!), I found that multiple observations request `nicer_d49_u100_150_55575341.rmf` in their headers.
>
> Ah! That's very good to know, and may be the culprit for some of the residuals you've been battling. Check that the arf files (keyword ANCRFILE) is also set correctly.
>
> > I'm likely almost done with the SAT (took it Saturday, going to take it again this coming Saturday)*, and once I'm done I aim to return to a progress pace similar to the pace we had in the summer.
>
> Congratulations - that's a nice milestone!
>
> > G'mar chatima tova (if you observe Yom Kippur)!

Same to you.

> *I might take it again in November in the event that I bungle those tests.

I'd put dollars to donuts you did great!

My best,

-Jack

> On Tue, Sep 22, 2020 at 1:21 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Interesting I'll look into that!
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi
>
> On Tue, Sep 22, 2020 at 1:15 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Similar to that. I used another language (IDL) to automatically produce a series of tcl/tk scripts and then executed those distributed over a number of cores on a workstation.
>
> -Jack
>
> On Tue, Sep 22, 2020 at 1:13 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Sounds good!
>
> In that case did you operate solely in TCL?
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi
>
> On Tue, Sep 22, 2020 at 1:10 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Thaddaeus,
>
> > For >75% I meant observations where source flux was more than 75% bg flux, and for 4% I meant observations where source flux was ~4 % bg flux. That makes sense—interestingly the observations with source flux ~4% bg flux were, by best fit, in the SPL state, with the highest flux and gamma for all the observations that had reasonable values for these parameters.
>
> Ah, now I see. Yes, when the bg flux is very high, the noise level (N=sqrt(B+S)) where B and S are background and signal counts in a channel, then the constraining power of the data (S/N) is lowest. If the background is reasonably estimated, the fits will likely be good but poorly constrained; if the background is off, then all bets are off.

> xD. I think it's a very impressive feat! Did you use pyxspec to loop through all the observations?

Thanks. I used a different language actually, but python/pyxspec would be an equally good way to do it.

Best,

-Jack

> On Tue, Sep 22, 2020 at 12:39 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Thaddaeus,
>
> > That's good to know. I guess what's surprising is some relative of this large residual in the 1.6-2.0 KeV is present in most of the source flux > 75% observations (I wonder if the peak feature falsely inflates the source flux for some of them?). Going back to a couple emails ago, I was checking some of my all time best fits which had source fluxes around ~4%—what allows such faint observations to fit well?
>
> Just to clarify, do you mean 75% and 4% of the Eddington limit, peak luminosity, or something else? Fainter observations, given some typical observation time, obviously have fewer counts which means larger fractional errors and less constraining power on the model. So if there is a deficiency in the calibration or model being used, it will look more prominent in brighter + long exposure time data.
>
> > Also, I'm very intrigued by your methods in one of your 2016 papers , "STRONGER REFLECTION FROM BLACK HOLE ACCRETION DISKS IN SOFT X-RAY STATES". How did you automate fitting the 15,000 observations reliably and within a reasonable time frame?
>
> A reasonable time frame is relative :), but I used about two dozen cores to run the fits which helped. I also stuck with computationally simplistic models, and for my runs with simpl, used lower resolution (via energies) in the run, since RXTE/PCA - the instrument used there - also has rather poor energy resolution compared to NICER.
>
> > Could I possibly have the ~86 observations between obsids 1200220101 – 1200220186?
>
> Spectral files are attached. Response files are too big for me to attach, so I'll keep them up here for a few days: https://nicer-web.mit.edu/nicerview/thadd_resp_m1727.tar.gz
>
> > Ah that makes sense. In that case did Xu et al. just freeze Rin at the ISCO to have less free parameters?
>
> That's the usual tactic, yes, when fitting for spin.
>
> Best,
>
> -Jack
>
> > > > On Thu, Sep 17, 2020 at 11:00 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> > > > Hi Thaddaeus,
> > > >
> > > > Great to hear you've been digging in and exploring! I would suggest though that you avoid being too concerned about the residual features. If something is localized and only ~percent level deviation, then that's likely

sufficient. *The goal should be to fit those sufficiently that they don't alter the continuum, but at some level it becomes diminishing returns to try fitting an artefact.* That said, if you wish, I can get you the data on MAXI J1727-203. Just let me know if you have a particular ObsID from Kevin's paper you want to try.

Your question about degeneracy is great. The condition of parameters being strongly correlated with one another is degeneracy. Basically, this is a very common situation where two parameters are dependent upon one another to some extent, so a probable value of one will condition a probable range of values with the other. For instance, if you want to fit x and y with the constraint x^2+y^2 = 4+/-1, you would get a ring of radius 2 with x,y values that obviously depend on one another; they are *not independent*. Speaking generally for more realistic cases, the interaction between parameters is often pretty convoluted, but commonly things like Gamma and Normalization are correlated with one another, so a contour plot of the confidence interval would look banana shaped rather than like a circle.

Best,

-Jack

On Thu, Sep 17, 2020 at 9:18 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> Thanks for checking in! Since we last spoke I've been taking a deeper look into a couple articles (Alabarta et al. 2020, Xu et al. 2018, and Parikh et al. 2019) and playing around a bit with the residual fix you suggested. As tested on multiple observations with varying fit statistic contributions in the 1.6-1.8 KeV range (chi-squared contribs. ranging between 500-3000), the gaussian centered around 1.75 KeV combined with the "edge" works great!
>
> Most importantly, I found a method to more accurately investigate the residual features: *plt>wdata*. Now that I can actually see what the chi-sq contrib. is for every point, I've been having a lot more success in approaching residuals in the 1.6-2.2 KeV range, especially when the residual patterns after 1.8 KeV differ greatly from what can quickly be ameliorated with the edge feature (it seems that for the majority of observations the <1.8 KeV excess is easily subdued by the gaussian fix, but after 1.8 KeV things often get complicated, especially when a third feature shows up between 1.86 to 2.0 KeV. This third feature resembles a sort of gaussian-like crevice about 0.13 KeV in width and often reaches a chi^2 contribution of -5331 (+) at its minima, pushing the "edge" feature out to a threshold energy of ~2+ KeV). I haven't found a definitive solution yet, but a gaussian absorption with an energy of ~1.86 KeV and a width of ~0.14 KeV is looking promising.
>
> While I've been working with these MAXI J1535-571 residuals I've been wondering if the residuals Alabarta et al. discuss in Nicer data for MAXI J1727−203 are of similar shape—is there any chance I could procure some pipeline data from you on MAXI J1727-203 to look into this (if it's not an inconvenience for you and or you don't think it's a distraction on my part of course).
>
> Out of curiosity, what does "parameter degeneracy" mean? My first guess would be this example: if I only told you what the slope of a line was (e.g. '2'), and then asked you for the individual values of Δy and Δx —would Δy and Δx be degenerate? Example of where I came across this: (from Xu et al. 2018) "The inner disk radius, R_{in}, and the black hole spin parameter, a*, are degenerate, as they both control the effective inner accretion disk radius. Therefore, for simplicity, during the spectral fitting we assume that the inner disk extends down to the ISCO by fixing R_{in} at the radius of the ISCO, and fit for the black hole spin as a free parameter."
>
> Best!
> Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

returnHardnessRatios.py
3K

Steiner, James <james.steiner@cfa.harvard.edu> Wed, Dec 9, 2020 at 11:57 AM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hey Thaddaeus,

Those parameter plots look great! Can you add in the diskbb and compTT normalizations as panels as well?

It looks to me like things are probably going wrong because you are probably using bin-number rather than channel number. Sometimes these two get conflated, but here by bin I mean groups of one or more channels, where a channel is the thing that is hard-coded in the spectral file (and PI-bins correspond to 10 eV increments). So you'd want to use channels 50-299 and 300-999 for your two bands.

The other thing to check is that the background is scaled properly. So you want to make sure that to background you subtract is:
BG_Spectrum_MatchedToData = BG_Counts_FromBGFile / BGFile_ExpTime * DataFile_ExpTime

Hopefully my shorthand there is clear enough.

Best,

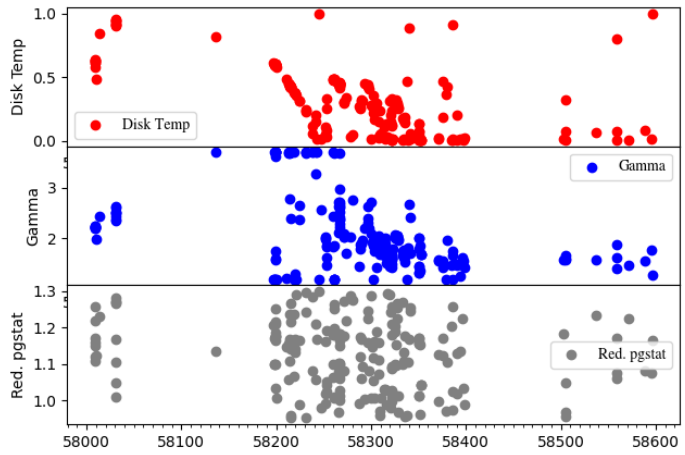
-Jack

On Wed, Dec 9, 2020 at 2:39 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

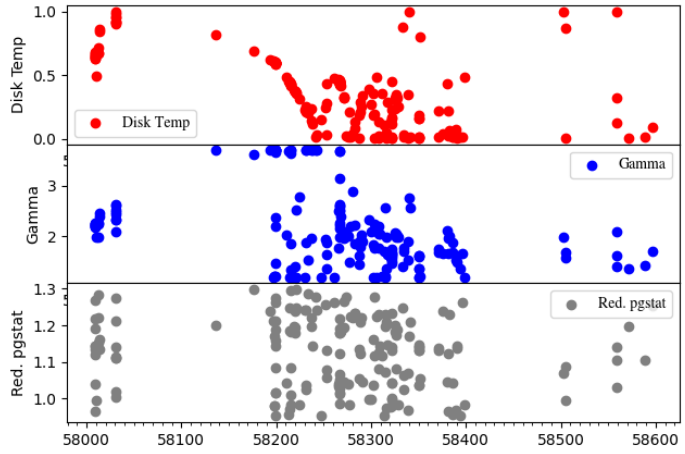
Hi Dr. Steiner!

Phew! The number of potential files for fitting has shot up to 720 from 185 now that I'm searching for and including all GTIs files.

I ran a couple fitting routines with a simple tbabs(diskbb+nthcomp) model on all the files just to get a feel for how including a lot more files would possibly change things, and the initial results from these fitting routines were quite unusual (though they were blisteringly fast compared to relxill fits regardless —with nH frozen on these routines, I was fitting all the files with 700 fit iterations each in under an hour on my mom's computer) :



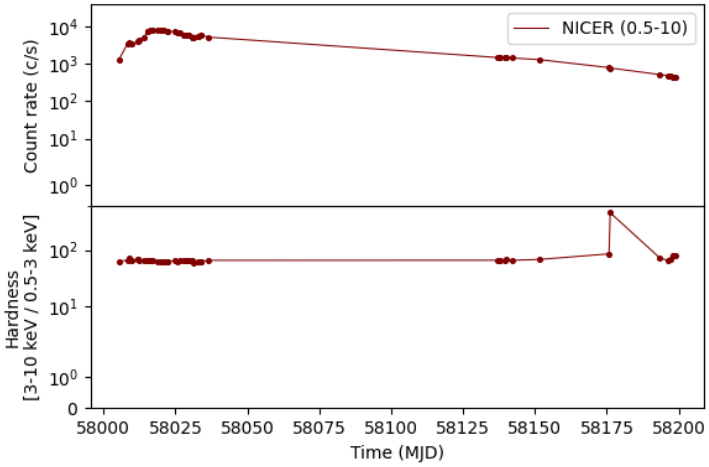
^ That routine had diskkbb Tin upper limit set to 1, gamma upper limit set to 3.7, nH frozen to 3.2, and the entire energy range from 0.5-10.0 keV noticed.



^That routine was the same except the energy interval 1.5-2.3 keV was ignored.

I think I see a little pattern of correlated variation between disk temp and gamma masked under the erratic points in both plots...I'm wondering, should I set some cutoff exposure time for the files? I recall seeing some ~22 sec exp. time data files when I was looking through other GTIs, and I'm thinking since these files would have less data to constrain the model, crazy parameters can be permissibly returned by XSPEC when these files are fit.

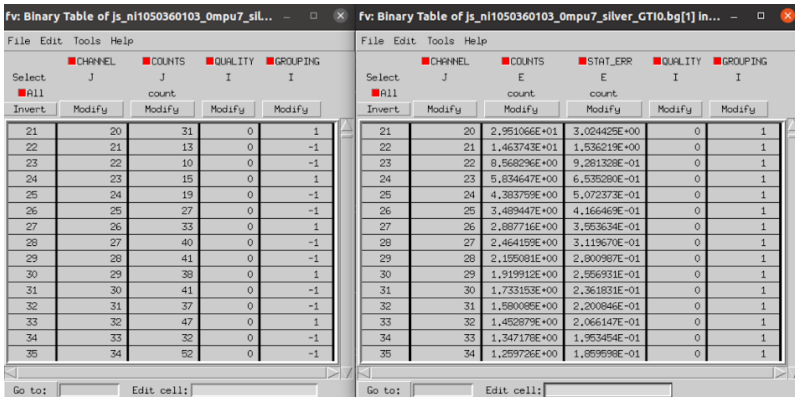
Also, could I get your advice on calculating hardness ratios? I modified my count rate function to correct for background counts and it's working pretty well (see the top subplot below):



Almost identical to Cuneo's count rate plot, eh?

As you can see in the bottom subplot, the hardness ratio values are crazy, and I can't figure out how to rein them in. The way my function for hardness ratio works is I sum the number of counts in the parameter defined hard band and subtract the number of background counts in the predefined hard band from that value, giving me a net hard band counts sum. I then divide that value by (sum of soft counts-sum soft background counts). Code is attached.

I was checking with fv, and it's interesting that in this case (and what I think is true in general) the bg counts are very low in the hard band but fairly high in the thermal:



Channels 22-99 = 0.5-3 keV

Channels 101-254 = 3.0-10.0 keV

Cheers!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 3:36 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> > Thanksgiving was very nice, thanks!
> Yay that's great to hear!
>
> > Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.
> Sounds good.
>
> > YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.
> !!! I will do this then!
>
> > Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)
> Will do.
>
> Thanks!
> Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 12:28 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Thanksgiving was very nice, thanks! Glad yours with the grandparents was too.

> I reprocessed the data last weekend (11/22) but the results were a little weird so I've been tweaking some things and prepping for reprocessing again this week.
>
> I was wondering if I should take a different approach to the instrumental residuals by running a preliminary fit on all files and logging the fit statistic landscape, so I can then loop through and identify which files actually require the 1.5-2.2 keV range to be ignored and which can have it noticed (I'm wondering about this because from a preliminary check a lot of the disk temperatures were behaving weirdly, which I think could be related to having less data in the thermal band to constrain them). What do you think?

Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.

> Also, I've only been fitting the silver, zero good time interval files; would you recommend me to fit some of the GTI1s, GT2s, etc. in hopes to get more good fits?

YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.

> P.S. I wrote two new standardized functions (one calculates and returns count rates, the other calculates and returns hardness ratios) and nice documentation for all my standard functions so far.

Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

-Jack

On Mon, Nov 23, 2020 at 8:34 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Will do!

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 23, 2020 at 4:30 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Sounds good! Glad your mom's computer has some CPU cycles to share. Keep me posted when you get the results in hand. Meantime, happy Thanksgiving.

-Jack

On Sun, Nov 22, 2020 at 8:01 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

Yes I've been fitting with the inner radius frozen to -1. That's good to know.

> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind. So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

Sounds good.

> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Yes I agree it was nice that they showed the underlying component.

I've been running the first iterations of the reprocessing last night and today and I've had a little breakthrough—last night I quickly updated the version of XSPEC I had on the vm that I installed on my mom's computer way back in June (I also installed relxill on it), and I've been dividing the work between her computer and mine—the routines are concluding much quicker (her computer has four cores xD).

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Fri, Nov 20, 2020 at 9:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.
>
> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind.
>
> So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)
>
> And in response to your email yesterday:
>
> > So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?
>
> Yes, spot on!
>
> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.
>
> Best,
>
> -Jack

On Fri, Nov 20, 2020 at 8:13 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Shoot! I forgot this bring this up. Would you recommend me to freeze the spin and inclination parameters to published values (that are in agreement with some of my previous best fits) to simplify the parameter space, or should I keep them free? The only benefit I can see from keeping them free would be sometimes error locks up with no variable parameters to use when free

parameters get pegged due to their insignificance to the fit, and I think having them free could help prevent this...though spin and inclination seem like parameters that would be the first to get pegged during an error routine anyways.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Nov 19, 2020 at 8:07 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hey!
>
> > Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.
>
> Yeah, safety is (rightly so) the first priority.
>
> > A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!
>
> Oo sounds cool, I hope you all have fun!
>
> > Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.
> Perfect.
>
> > The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon.
> Interesting.
>
> > then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.
> Very interesting.
>
> I think I found the thing that was on the edge of my memory: a Chandra image of the quasar H1821+643.
>
> 
>
> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?
>
> > Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.
> I should probably rephrase what I said earlier. When I ignored bad, no channels were ignored so I guess that meant no poor quality flags were raised.
>
> > I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.
> Agreed. I find it so enjoyable: the initial encounter with a seemingly insurmountable challenge, the brainstorming, the troubleshooting, and *especially* the successful execution of a solution. It's extremely satisfying—and to use an analogy from my youth* it feels kinda similar to when I would build things in lego.
>
> *I say an analogy from my youth but that's somewhat misleading. I still occasionally play legos, and these days the only difference is that I have a partner: my younger brother ;).
>
> > After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?
> Definitely. That is exactly what I'm thinking.
>
> > Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.
> :)).
>
> Best!

Thaddaeus

Oh I almost forgot. I don't remember how/why I came across this paper (Arumugasamy et al. 2018), but after looking at the plots I've been meaning to ask you about them:

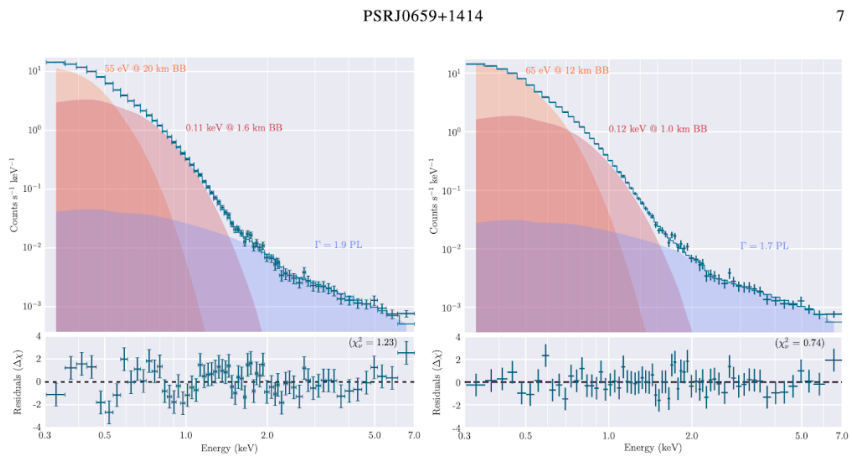


Figure 4. 2BBPL model, without (left) and with (right) `Gabs` absorption component, fit to the phase-integrated spectrum of J0659+1414. The model parameters correspond to maximum posterior probability, which in the case of G2BBPL are different from the distribution medians listed in Table 2.

I've been using the darkgrid style (like the plots above) with the rc font parameter set to Nimbus Roman in most of my plots ... but after seeing these plots I'm thinking I was to switch back to the mpl defaults—these plots are kinda distracting to me in their aesthetic.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Wed, Nov 18, 2020 at 10:05 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> It was a wild week indeed—and things seem to keep getting crazier. We were in a hybrid model since October 19th, but as of Tuesday we went indefinitely back to all online which is a two sided coin. On one hand the online schedule gives me a lot more free time, but on the other I'm going to miss the extra class time in my harder classes like mechanics and calc. Thanksgiving is around the corner though! Up to any interesting plans yourself? I think we're just going to have my mom's parents over because we already interact with them frequently, but no big gathering this year (obviously).

No kidding! Feels like we're living in a bad TV show.

Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.

A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> Would it be okay if I emailed Dr. Cuneo about this? This is very important for how I structure the confidence intervals code (and whether I make it more sensitive and able to detect more than the four generalized abnormal scenarios described later in this email).

Sure thing, email away.

> Haha! I just read that NuStar's is ~2.5 *milliseconds.*

Yeah and that's actually not too bad compared to most things out there. NICER is just an absolute timing monster.

> I do have my pipeline correct for it automatically, so all your spectra already factor that in.
>
> Cool! Does that mean I can just go ahead and calculate count rates as number of counts / exp. time and not worry about adjusting the result because you've already accounted for deadtime?

Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

> deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.
>
> Yes. This makes sense. I feel like I've seen something related where the center of an object was too bright in the X-Ray band (which oversaturated the detector?) so the color gradient of brightness was black in the center because the author's couldn't determine the brightness at the center. It kinda looked like an artificial coronagraph. Would that be related to a situation where the dead time was more than the event time?

(Deep dive warning:)
Yes, you're speaking about "pileup" which isn't exactly related to deadtime except that both can be important when the source is bright. Deadtime, again, is from the detector shutting down for some portion of the time. Pileup concerns are most often for an imagng type of detector - more like the camera on your phone - that collects an exposure for some amount of time and then read it off.

Chandra, XMM-Newton, Swift, Suzaku, (somewhat confusingly NuStar is in both camps), let X-rays come in from the sky, get focused onto the CCD camera, and then in some time interval, often ~1s, reads off the image and clears the CCD. The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon. So when an individual pixel gets << 1 X-ray per frame on average, you are in good shape. *But* when there is significant probability of getting >~1 X-ray per frame on a given pixel, then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

This registering of multiple X-rays incorrectly as one more energetic single event is pileup, and it is the bane of most imaging detectors for those of us working with bright sources. (Pileup can occur in instruments like NICER too, when two X-rays hit during the event-latch phase, but this is something like a <1% effect for a 100 Crab-bright source, so well in the camp of being negligible for all practical purposes.)

In the most extreme cases of pileup, the pixel digitizer, which usually records values on something like a 16-bit value (e.g., from -32768:32767), can roll over and then appears negative when too much charge hits at once.

> I'm pretty sure the RXTE data I was working with earlier this year didn't have this identifier because if I remember correctly I couldn't execute *$ignore bad* on the data in XSPEC.

Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

> Here's a working version of the confidence interval / error results script:
>
> I put my XSPECtools.py file which has all of these functions into a custom module in my local site-packages folder for python and it seems like it's working perfectly. I import the file like this: *from personalastropy.xspectools import XSPECtools as xt*, and I execute a particular function like this: *xt.returnFitResults().*
>
> Happy early Thanksgiving!
> Thaddaeus

That's really great Thaddaeus! I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Very well done!

After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

Best,

-Jack

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Wed, Nov 11, 2020 at 12:07 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hey Thaddaeus,

What a week it's been eh? (But way to go PA!)

> I've held off from running another major fitting routine since the weekend because after I ran that my last one I decided that it was time to write some standardized scripts for wrangling the results from all the log files.

Excellent!

> Speaking of which, when Cuneo uses green triangles for "*parameters consistent with 0 within errors*", does this refer to situations when either the upper value of a parameter is equal to zero (because it got pegged at the hard upper limit) or when the lower limit is invalid and equal to zero?

Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).

I *love* the outline of your routines, and "wow" is what I have to say about that. Way to go Thaddaeus, it's great you're tackling this and in a very useful manner.

> Out of curiosity, where did Cuneo get the count rate values for his data? I don't think it's the same as the Net Count Rate that's displayed whenever you execute *$show data* in XSPEC....can Cuneo's count rate (c/s) be found by dividing the sum of the counts in the included channels by the total observation time?

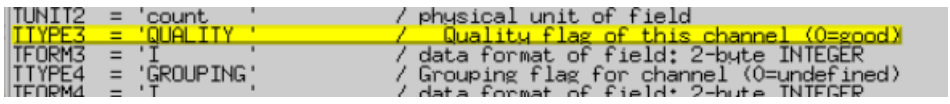
About 99% yes. "Usual" practice for NICER has become to normalize to an active set of 52 detectors. Your spectrum is a collection of all good events. So a tally of all the (good) events between 0.5-10 keV divided by the exposure time is indeed the count rate detected. However, if only using 49 or 50 detectors (also common), one would scale up by 52/49 to show the rate on the full instrument.

The one very tiny wrinkle to all this is a property called "deadtime". Each time the electronics on a detector register an X-ray event, they "latch" meaning that the detector stops listening for new signals. Effectively, the detector is turned off for a short amount of time it uses to calculate properties of event (what time it arrived, and how much charge was deposited). For NICER, this is typically ~20 microseconds.

As a concrete example of what this means in practice: when the count rate per detector reaches ~10^3/s (or in other words the full instrument light curve was at ~50,000/s), there would be ~20e-6 * 1e3 = 2% deadtime. This would mean that the *actual* count rate is ~2% higher than was detected (because the true exposure time was actually 98% the clock time). Here, most likely Cuneo didn't worry about folding in deadtime effects (I do have my pipeline correct for it automatically, so all your spectra already factor that in).

This is obviously a very small here for NICER, but I wanted to dive into it because for many missions that aren't timing beasts like NICER, deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.

> Also sorta random, I came across a value called "quality" when I was looking at a file in fv; are the quality values the values XSPEC refers to when I say *$ignore bad*?



Yes exactly this. This won't tend to come up for NICER, but e.g., for instruments that have some bad pixels and a spatially dispersed spectrum (think the rainbow from a prism), or uncalibrated energy ranges, this can be used.

> * This is probably one of my favorite one-liner functions in Python:

for line in f:
  linelist = (re.sub(' +', ',', line)).split(',')

Where f is a file opened with: *with open(...) as f.*

Python is a wonder for its very efficient one-lining loops!

As usual, you're knocking it out of the park Thaddaeus.

In other news, a new grad student (Santi) just joined me and he's going to start looking at some QPO data.

Best,

-Jack

On Thu, Oct 29, 2020 at 12:49 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Whoops - Santiago is my grad student and I notice my advisor brain accidentally called you Santi in my last message. Very sorry about that!

Name confusions aside, very glad the patch worked ok. Yeah, setting query to yes can cause problems sometimes as you found. I don't have good rules for avoiding or escaping those runaways, but I've had luck before tweaking the extra convergence terms in the fit command when it's stuck (the delta & beta) and the tolerance term in error.

-Jack

On Thu, Oct 29, 2020 at 11:34 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi Dr. Steiner!

Alright I think that patching did the trick; I'm currently running XSPEC v 12.11.0m. Surprisingly I didn't have to rebuild relxill which was nice. Now that the 12.11.0k patch has been installed, I think XSPEC just "snags" and keeps fitting if I have query set to yes instead of crashing—when I was looking into this, a particular observation ran an error routine for three days nonstop in the background until I aborted it!

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 22, 2020 at 12:46 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Santi,

OK, that's encouraging! Let me know if the patching does the trick. Often for small patches, you won't need to recompile local models, but you should quickly see if it has trouble loading and that would be your sign.

It's hard to predict how parallel processing will impact a given task. The outcome depends on where the bottlenecks occur (if processing, then expect significant improvement, if memory swapping, then probably not). For running on multiple threads, a **rough** estimate is sqrt(N) gain, but that's just ballpark.

-Jack

On Thu, Oct 22, 2020 at 11:56 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

I just got some really great news on the crashes!

Last weekend I was dismayed because even though I was playing with the limits extensively (especially for gamma), the crashing was still occurring unabated. This made me wonder if it was a software issue, so I reached out to the HEASARC Help Desk. I found out that a very similar issue was fixed in XSPEC patch 12.11.0k: *For some complex fitting and error command cases, XSPEC may terminate with a GSL*

message such as 'SVD decomposition failed to converge'. This wasn't encouraging at first because XSPEC patch 12.11.0k came out in June (I built the current version of xspec on my vm in ~July), but I checked my XSPEC version with *$version,* and it turns out that my XSPEC version is just 12.11.0! I'm working on installing the patch, I bet this will clear a lot of things up. I think I'm going to have to rebuild relxill after I execute the partial system rebuild.

The only thing that confuses me about this is that I didn't really have issues with core dumping when I was fitting relxill a lot back in August (I think it did come up for error fits back then though), so I'm confused why it really started acting up recently.

Also I don't think I've asked this previously, but out of curiosity, should I expect significant reductions in fit times with two core parallel processing?

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 22, 2020 at 10:52 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Just checking in to see how fits are progressing and if playing with those limits has allowed any headway against the crashes?
>
> (Yes, whenever I "rm -rf " I try to take beat and check that I am not about to bilk the whole machine! With great commands-line tools comes great ...)
>
> -Jack
>
> On Fri, Oct 16, 2020 at 3:24 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
>
> > Hi!
> >
> > Now that I think about it, I do recall noticing that many (if not most) of the fit failures had gammas in the 3.4 range. Good to know about the 1.6-2.2 range for the residuals, I'll play with that now.
> >
> > Oh about the attachments: the screen shot from visual studio is just some descriptions I was writing about the different functions I was using, and the microsoft paint one just illustrates the positive contribution that is almost always eliminated by a ~1.75 KeV gaussian of varying width.
> >
> > Over this weekend should I continue to focus on maximizing the number of observations that I can get good fits and confidence intervals for?
> >
> > I'm intrigued by this disk truncation controversy, I'm going to read about it this weekend.
> >
> > Many thanks!
> > Thaddaeus
> >
> > P.S. I was reading about some dangerous linux commands the other day; there are some unnerving commands out there. Imagine if someone ran rm -rf in your terminal. Shudder.
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
> >
> > On Thu, Oct 15, 2020 at 10:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> >
> > > Hi Thaddaeus,
> > >
> > > > Regarding the application of systematic errors, that's good to know. I've often seen something like the "NICER team recommends the application of a systematic error of 1%" in papers that use NICER data.

Yes, that's a "rough" benchmark, but a reasonable one.

> I love the common.tcl file, it's so handy! I think I'm just going to use it from now on because it's simpler on the execution end (just two commands), and it shows the error strings for each parameter.

Yes, it's quite nice work by Jeremy :).

> A little random: I came across an interesting statement in a paper by Plant et al. 2015 pertaining to observed disk truncation in the hard state of GX 339−4: "This result implies that the current sample of spin estimates in the hard state are inaccurate. Therefore, any distinct conclusions drawn from these estimates, such as the spin-powering of relativistic jets, may well be biased." Since that paper was published has this issue been fully exploited and resolved, or are there still black holes with possibly incorrect spin measurements out there? Also, is physical disk truncation or changing coronal height the more widely accepted cause for profiles similar to disk truncation?

This has been a topic with a lot of controversy that is still ongoing (Garcia et al 2015 and followup papers by Wang et al. 2019 talk about this). I'd say the Plant picture is strongly disfavored at present, but not entirely out. The robust view is that allowing for the possibility of truncation, a (bright) hard-state spin measurement should be regarded as an estimated lower limit. Presently, the argument is focused around how much truncation could there be in bright hard states, with most reasonable numbers being a few times the ISCO (a factor <10). (Coronal geometry is always an important consideration, but usually they tend to fit very compact.)

> An update on the rsp/arf issue: I modified my approach and fully integrated astropy.io.fits, so when I'm producing my xspec script commands rmf and arf file names are pulled directly from the header of each observation's fits file. Because of the two errors described below, I haven't checked how chi-square improves when the correct rmf and arf files are used though.

Very good, that should make some impact when working.

> I've encountered two weird errors I've never encountered before in XSPEC this week: the first is frequent core dumping [SVD decomposition failed to converge...Default GSL error handler invoked...Segmentation fault (core dumped)], and the second is this odd "XSPEC Error: No variable parameters for fit" after executing "fit" for a model with *multiple* free parameters (including the usual Gamma, FracSctr, Tin, diskbb norm, etc.). My first thought was "*is this related to relxill having problems due to a low gamma"*—but these issues both happen even when Gamma is > 2.

The table can be sparse in a few places, and I think very large Gammas are also sparse (>~3.4, if memory serves). I'd say try freezing parameters and retrying the fit to identify the culprit parameter.

> Also a little random: this week I decided it was time to start commenting on my functions in my python files—at one point I was looking through some older files for a particular technique I used and it took a little while to find the correct version of it, so I decided that from now on I'm describing my functions for future reference.

Yes, documenting code is super important and a great habit to get used to. (I wish I was better about it!)

> This is preliminary, but I attached an example of a chi-scape scarred by a common morph of the residual I last sent you. The positive contribution that begins shortly after 1.6 KeV is almost always eliminated by a ~1.75 KeV gaussian of varying width for most observations. The current issue is modeling the various forms the following negative residuals take. Currently working on those. Earlier you mentioned that we could try ignoring that narrow range where these residuals occur, would ignoring channels between 1.6-2.2 KeV be ignoring too much of the data? If we decide to ignore that range, I would think it best to get >*plt wdata* outputs to exactly nail down the dimensions of this range, but for now should I keep trying to model the residuals?

> I was reading through some older emails, going to look into your recommendation regarding logxi.

No, I think 1.6-2.2 isn't crazy and you can certainly roll with that if you are having too much trouble taming the residuals with a model. Maybe do a couple test cases to see how the model parameters are affected on both fronts.

Hey, and I didn't see a description of your attachments in case you want to say anything about those.

Glad you're back in the thick of things.

Best,

-Jack

> On Thu, Oct 8, 2020 at 1:35 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> > Hi Dr. Steiner!
> >
> > Quick note: I've been getting back into it this week and should have some interesting things to report this weekend!
> >
> > Thaddaeus
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
>
> On Tue, Sep 29, 2020 at 2:19 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> > Hi Thaddaeus,
> >
> > Thanks for the update.
> >
> > > Quick note on the residuals: since we last communicated I investigated the application of a systematic error (3-5%) in the < 2 KeV energy band, but this didn't improve the residuals so I'm assuming that you already accounted for this, like the binning, in your reprocessing?
> >
> > No, actually. But systematic errors are only incorporated with some fit statistics - chisq for instance, but I don't believe are incorporated in cstat or pgstat. The way I like to think about it is the systematic error acts as a reweighting of the data's constraint and serves to de-emphasize regions with the highest signal. I offer for information sake that that 3-5% is too large a systematic error to include as a rule. Something like 1-2% is more reasonable for a region with poorer calibration quality.
> >
> > > I also discovered that I've been unintentionally forcing an incorrect response file on multiple MAXI J1535-571 observations—the rsp I was using for all observations was `nicer_d49_55575341.rmf`, but after running a quick check (the fitsio package has been so helpful, thanks for recommending it to me!), I found that multiple observations request `nicer_d49_u100_150_55575341.rmf` in their headers.
> >
> > Ah! That's very good to know, and may be the culprit for some of the residuals you've been battling. Check that the arf files (keyword ANCRFILE) is also set correctly.
> >
> > > I'm likely almost done with the SAT (took it Saturday, going to take it again this coming Saturday)*, and once I'm done I aim to return to a progress pace similar to the pace we

> had in the summer.

Congratulations - that's a nice milestone!

> G'mar chatima tova (if you observe Yom Kippur)!

Same to you.

> *I might take it again in November in the event that I bungle those tests.

I'd put dollars to donuts you did great!

My best,

-Jack

On Tue, Sep 22, 2020 at 1:21 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Interesting I'll look into that!
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Tue, Sep 22, 2020 at 1:15 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Similar to that. I used another language (IDL) to automatically produce a series of tcl/tk scripts and then executed those distributed over a number of cores on a workstation.
>
> -Jack
>
> On Tue, Sep 22, 2020 at 1:13 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
>
> > Sounds good!
> >
> > In that case did you operate solely in TCL?
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
> >
> > On Tue, Sep 22, 2020 at 1:10 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> >
> > > Hi Thaddaeus,
> > >
> > > For >75% I meant observations where source flux was more than 75% bg flux, and for 4% I meant observations where source flux was ~4 % bg flux. That makes sense—interestingly the observations with source flux ~4% bg flux were, by best fit, in the SPL state, with the highest flux and gamma for all the observations that had reasonable values for these parameters.

Ah, now I see. Yes, when the bg flux is very high, the noise level (N=sqrt(B+S)) where B and S are background and signal counts in a channel, then the constraining power of the data (S/N) is lowest. If the background is reasonably estimated, the fits will likely be good but poorly constrained; if the background is off, then all bets are off.

> xD. I think it's a very impressive feat! Did you use pyxspec to loop through all the observations?

Thanks. I used a different language actually, but python/pyxspec would be an equally good way to do it.

Best,

-Jack

> On Tue, Sep 22, 2020 at 12:39 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Thaddaeus,
>
> > That's good to know. I guess what's surprising is some relative of this large residual in the 1.6-2.0 KeV is present in most of the source flux > 75% observations (I wonder if the peak feature falsely inflates the source flux for some of them?). Going back to a couple emails ago, I was checking some of my all time best fits which had source fluxes around ~4%—what allows such faint observations to fit well?
>
> Just to clarify, do you mean 75% and 4% of the Eddington limit, peak luminosity, or something else? Fainter observations, given some typical observation time, obviously have fewer counts which means larger fractional errors and less constraining power on the model. So if there is a deficiency in the calibration or model being used, it will look more prominent in brighter + long exposure time data.
>
> > Also, I'm very intrigued by your methods in one of your 2016 papers , "STRONGER REFLECTION FROM BLACK HOLE ACCRETION DISKS IN SOFT X-RAY STATES". How did you automate fitting the 15,000 observations reliably and within a reasonable time frame?
>
> A reasonable time frame is relative :), but I used about two dozen cores to run the fits which helped. I also stuck with computationally simplistic models, and for my runs with simpl, used lower resolution (via energies) in the run, since RXTE/PCA - the instrument used there - also has rather poor energy resolution compared to NICER.
>
> > Could I possibly have the ~86 observations between obsids 1200220101 – 1200220186?
>
> Spectral files are attached. Response files are too big for me to attach, so I'll keep them up here for a few days: https://nicer-web.mit.edu/nicerview/thadd_resp_m1727.tar.gz
>
> > Ah that makes sense. In that case did Xu et al. just freeze Rin at the ISCO to have less free parameters?
>
> That's the usual tactic, yes, when fitting for spin.
>
> Best,
>
> -Jack

On Thu, Sep 17, 2020 at 11:00 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Great to hear you've been digging in and exploring! I would suggest though that you avoid being too concerned about the residual features. If something is localized and only ~percent level deviation, then that's likely sufficient. *The goal should be to fit those sufficiently that they don't alter the continuum, but at some level it becomes diminishing returns to try fitting an artefact.* That said, if you wish, I can get you the data on MAXI J1727-203. Just let me know if you have a particular ObsID from Kevin's paper you want to try.
>
> Your question about degeneracy is great. The condition of parameters being strongly correlated with one another is degeneracy. Basically, this is a very common situation where two parameters are dependent upon one another to some extent, so a probable value of one will condition a probable range of values with the other. For instance, if you want to fit x and y with the constraint x^2+y^2 = 4+/-1, you would get a ring of radius 2 with x,y values that obviously depend on one another; they are *not independent*. Speaking generally for more realistic cases, the interaction between parameters is often pretty convoluted, but commonly things like Gamma and Normalization are correlated with one another, so a contour plot of the confidence interval would look banana shaped rather than like a circle.
>
> Best,
>
> -Jack

On Thu, Sep 17, 2020 at 9:18 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> Thanks for checking in! Since we last spoke I've been taking a deeper look into a couple articles (Alabarta et al. 2020, Xu et al. 2018, and Parikh et al. 2019) and playing around a bit with the residual fix you suggested. As tested on multiple observations with varying fit statistic contributions in the 1.6-1.8 KeV range (chi-squared contribs. ranging between 500-3000), the gaussian centered around 1.75 KeV combined with the "edge" works great!
>
> Most importantly, I found a method to more accurately investigate the residual features: *plt>wdata*. Now that I can actually see what the chi-sq contrib. is for every point, I've been having a lot more success in approaching residuals in the 1.6-2.2 KeV range, especially when the residual patterns after 1.8 KeV differ greatly from what can quickly be ameliorated with the edge feature (it seems that for the majority of observations the <1.8 KeV excess is easily subdued by the gaussian fix, but after 1.8 KeV things often get complicated, especially when a third feature shows up between 1.86 to 2.0 KeV. This third feature resembles a sort of gaussian-like crevice about 0.13 KeV in width and often reaches a chi^2 contribution of -5331 (+) at its minima, pushing the "edge" feature out to a threshold energy of ~2+ KeV). I haven't found a definitive solution yet, but a gaussian absorption with an energy of ~1.86 KeV and a width of ~0.14 KeV is looking promising.
>
> While I've been working with these MAXI J1535-571 residuals I've been wondering if the residuals Alabarta et al. discuss in Nicer data for MAXI J1727−203 are of similar shape—is there any chance I could procure some pipeline data from you on MAXI J1727-203 to look into this (if it's not an inconvenience for you and or you don't think it's a distraction on my part of course).
>
> Out of curiosity, what does "parameter degeneracy" mean? My first guess would be this example: if I only told you what the slope of a line was (e.g. '2'), and then asked you for the individual values of Δy and Δx—would Δy and Δx be degenerate? Example of where I came across this: (from Xu et al. 2018) "The inner disk radius, R_{in}, and the black hole spin parameter, a*, are degenerate, as they both control the effective inner accretion disk radius. Therefore, for simplicity, during the spectral fitting we assume that the inner disk

extends down to the ISCO by fixing R_{in} at the radius of the ISCO, and fit for the black hole spin as a free parameter."

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

Steiner, James <james.steiner@cfa.harvard.edu> Thu, Dec 31, 2020 at 11:38 AM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

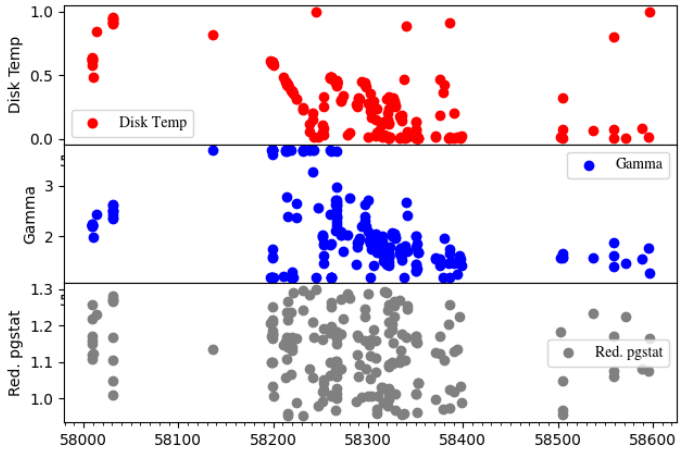
Hi Thaddaeus,

It's been quite a month everywhere! Hope you had a nice holiday season and that you and your family have been doing well. Wishing you a happy and better New Year ahead.

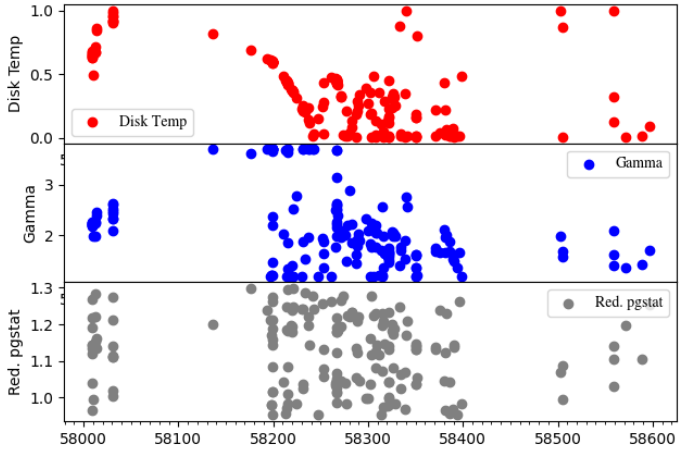
-Jack

On Wed, Dec 9, 2020 at 2:57 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hey Thaddaeus,
>
> Those parameter plots look great! Can you add in the diskbb and compTT normalizations as panels as well?
>
> It looks to me like things are probably going wrong because you are probably using bin-number rather than channel number. Sometimes these two get conflated, but here by bin I mean groups of one or more channels, where a channel is the thing that is hard-coded in the spectral file (and PI-bins correspond to 10 eV increments). So you'd want to use channels 50-299 and 300-999 for your two bands.
>
> The other thing to check is that the background is scaled properly. So you want to make sure that to background you subtract is:
> BG_Spectrum_MatchedToData = BG_Counts_FromBGFile / BGFile_ExpTime * DataFile_ExpTime
>
> Hopefully my shorthand there is clear enough.
>
> Best,
>
> -Jack
>
> On Wed, Dec 9, 2020 at 2:39 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
>
> > Hi Dr. Steiner!
> >
> > Phew! The number of potential files for fitting has shot up to 720 from 185 now that I'm searching for and including all GTIs files.
> >
> > I ran a couple fitting routines with a simple tbabs(diskbb+nthcomp) model on all the files just to get a feel for how including a lot more files would possibly change things, and the initial results from these fitting routines were quite unusual (though they were blisteringly fast compared to relxill fits regardless —with nH frozen on these routines, I was fitting all the files with 700 fit iterations each in under an hour on my mom's computer) :



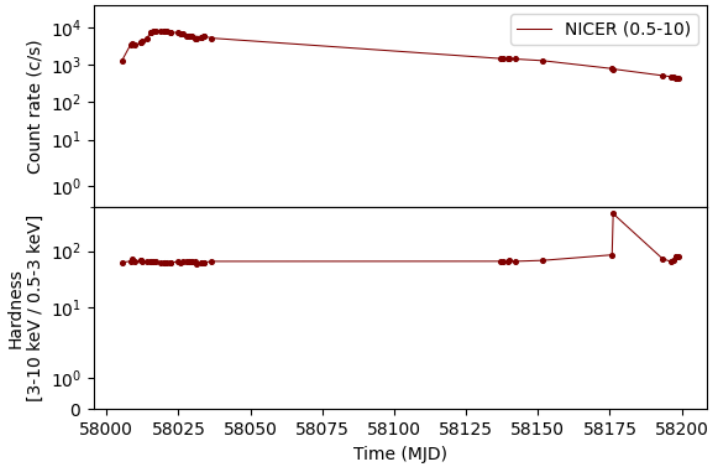
^ That routine had diskkbb Tin upper limit set to 1, gamma upper limit set to 3.7, nH frozen to 3.2, and the entire energy range from 0.5-10.0 keV noticed.



^That routine was the same except the energy interval 1.5-2.3 keV was ignored.

I think I see a little pattern of correlated variation between disk temp and gamma masked under the erratic points in both plots...I'm wondering, should I set some cutoff exposure time for the files? I recall seeing some ~22 sec exp. time data files when I was looking through other GTIs, and I'm thinking since these files would have less data to constrain the model, crazy parameters can be permissibly returned by XSPEC when these files are fit.

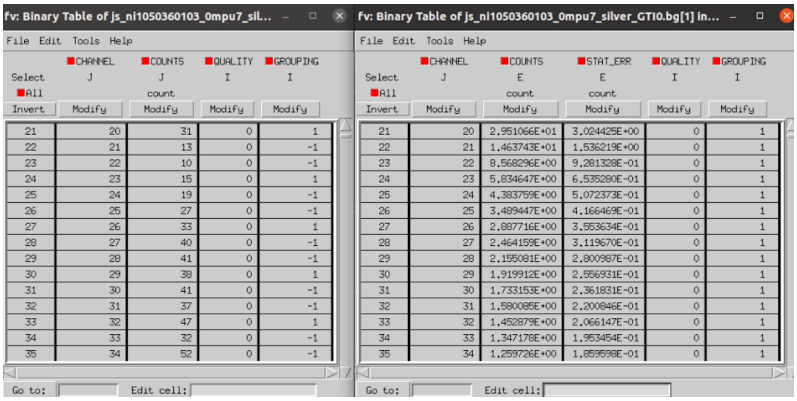
Also, could I get your advice on calculating hardness ratios? I modified my count rate function to correct for background counts and it's working pretty well (see the top subplot below):



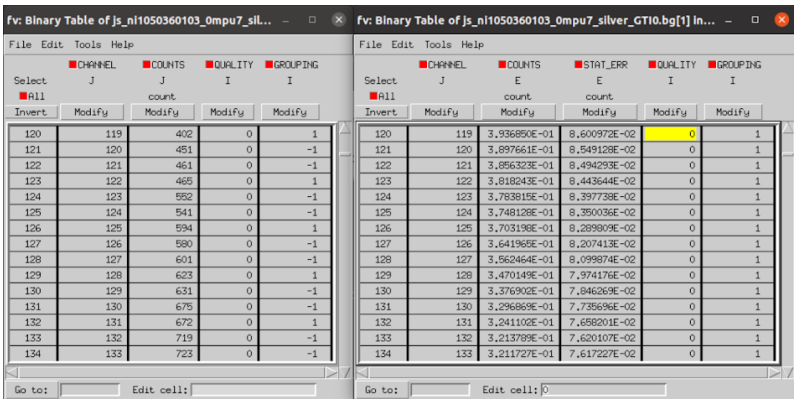
Almost identical to Cuneo's count rate plot, eh?

As you can see in the bottom subplot, the hardness ratio values are crazy, and I can't figure out how to rein them in. The way my function for hardness ratio works is I sum the number of counts in the parameter defined hard band and subtract the number of background counts in the predefined hard band from that value, giving me a net hard band counts sum. I then divide that value by (sum of soft counts-sum soft background counts). Code is attached.

I was checking with fv, and it's interesting that in this case (and what I think is true in general) the bg counts are very low in the hard band but fairly high in the thermal:



Channels 22-99 = 0.5-3 keV



Channels 101-254 = 3.0-10.0 keV

Cheers!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 3:36 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> > Thanksgiving was very nice, thanks!
> Yay that's great to hear!
>
> > Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.
> Sounds good.
>
> > YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.
> !!! I will do this then!
>
> > Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)
> Will do.

Thanks!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 12:28 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Thanksgiving was very nice, thanks! Glad yours with the grandparents was too.

> I reprocessed the data last weekend (11/22) but the results were a little weird so I've been tweaking some things and prepping for reprocessing again this week.
>
> I was wondering if I should take a different approach to the instrumental residuals by running a preliminary fit on all files and logging the fit statistic landscape, so I can then loop through and identify which files actually require the 1.5-2.2 keV range to be ignored and which can have it noticed (I'm wondering about this because from a preliminary check a lot of the disk temperatures were behaving weirdly, which I think could be related to having less data in the thermal band to constrain them). What do you think?

Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.

> Also, I've only been fitting the silver, zero good time interval files; would you recommend me to fit some of the GTI1s, GT2s, etc. in hopes to get more good fits?

YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.

> P.S. I wrote two new standardized functions (one calculates and returns count rates, the other calculates and returns hardness ratios) and nice documentation for all my standard functions so far.

Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

-Jack

On Mon, Nov 23, 2020 at 8:34 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Will do!

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 23, 2020 at 4:30 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Sounds good! Glad your mom's computer has some CPU cycles to share. Keep me posted when you get the results in hand. Meantime, happy Thanksgiving.

-Jack

On Sun, Nov 22, 2020 at 8:01 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

Yes I've been fitting with the inner radius frozen to -1. That's good to know.

> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind. So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

Sounds good.

> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Yes I agree it was nice that they showed the underlying component.

I've been running the first iterations of the reprocessing last night and today and I've had a little breakthrough—last night I quickly updated the version of XSPEC I had on the vm that I installed on my mom's computer way back in June (I also installed relxill on it), and I've been dividing the work between her computer and mine—the routines are concluding much quicker (her computer has four cores xD).

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Fri, Nov 20, 2020 at 9:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind.

So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

And in response to your email yesterday:

> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

Yes, spot on!

As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Best,

-Jack

On Fri, Nov 20, 2020 at 8:13 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Shoot! I forgot this bring this up. Would you recommend me to freeze the spin and inclination parameters to published values (that are in agreement with some of my previous best fits) to simplify the parameter space, or should I keep them free? The only benefit I can see from keeping them free would be sometimes error locks up with no variable parameters to use when free parameters get pegged due to their insignificance to the fit, and I think having them free could help prevent this...though spin and inclination seem like parameters that would be the first to get pegged during an error routine anyways.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Nov 19, 2020 at 8:07 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hey!
>
> > Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.
>
> Yeah, safety is (rightly so) the first priority.
>
> > A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!
>
> Oo sounds cool, I hope you all have fun!
>
> > Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.
> Perfect.
>
> > The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon.
> Interesting.
>
> > then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.
> Very interesting.
>
> I think I found the thing that was on the edge of my memory: a Chandra image of the quasar H1821+643.
>
> 
>
> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?
>
> > Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.
> I should probably rephrase what I said earlier. When I ignored bad, no channels were ignored so I guess that meant no poor quality flags were raised.
>
> > I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.
> Agreed. I find it so enjoyable: the initial encounter with a seemingly insurmountable challenge, the brainstorming, the troubleshooting, and *especially* the successful execution of a solution. It's extremely satisfying—and to use an analogy from my youth* it feels kinda similar to when I would build things in lego.

*I say an analogy from my youth but that's somewhat misleading. I still occasionally play legos, and these days the only difference is that I have a partner: my younger brother ;).

> After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Definitely. That is exactly what I'm thinking.

> Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

:)).

Best!
Thaddaeus

Oh I almost forgot. I don't remember how/why I came across this paper (Arumugasamy et al. 2018), but after looking at the plots I've been meaning to ask you about them:

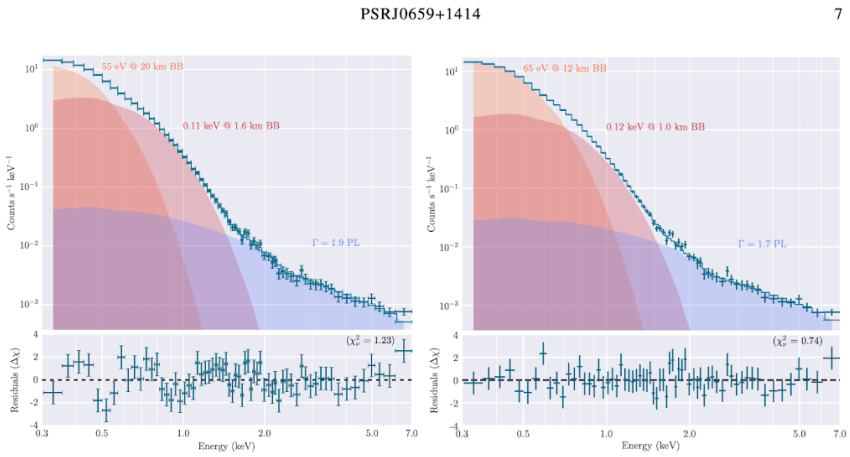


Figure 4. 2BBPL model, without (left) and with (right) `Gabs` absorption component, fit to the phase-integrated spectrum of J0659+1414. The model parameters correspond to maximum posterior probability, which in the case of G2BBPL are different from the distribution medians listed in Table 2.

I've been using the darkgrid style (like the plots above) with the rc font parameter set to Nimbus Roman in most of my plots ... but after seeing these plots I'm thinking I was to switch back to the mpl defaults—these plots are kinda distracting to me in their aesthetic.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Wed, Nov 18, 2020 at 10:05 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> > It was a wild week indeed—and things seem to keep getting crazier. We were in a hybrid model since October 19th, but as of Tuesday we went indefinitely back to all online which is a two sided coin. On one hand the online schedule gives me a lot more free time, but on the other I'm going to miss the extra class time in my harder classes like mechanics and calc. Thanksgiving is around the corner though! Up to any interesting plans yourself? I think we're just going to have my mom's parents over because we already interact with them frequently, but no big gathering this year (obviously).
>
> No kidding! Feels like we're living in a bad TV show.
>
> Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.
>
> A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> Would it be okay if I emailed Dr. Cuneo about this? This is very important for how I structure the confidence intervals code (and whether I make it more sensitive and able to detect more than the four generalized abnormal scenarios described later in this email).

Sure thing, email away.

> Haha! I just read that NuStar's is ~2.5 *milliseconds.*

Yeah and that's actually not too bad compared to most things out there. NICER is just an absolute timing monster.

> I do have my pipeline correct for it automatically, so all your spectra already factor that in.
>
> Cool! Does that mean I can just go ahead and calculate count rates as number of counts / exp. time and not worry about adjusting the result because you've already accounted for deadtime?

Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

> deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.
>
> Yes. This makes sense. I feel like I've seen something related where the center of an object was too bright in the X-Ray band (which oversaturated the detector?) so the color gradient of brightness was black in the center because the author's couldn't determine the brightness at the center. It kinda looked like an artificial coronagraph. Would that be related to a situation where the dead time was more than the event time?

(Deep dive warning:)
Yes, you're speaking about "pileup" which isn't exactly related to deadtime except that both can be important when the source is bright. Deadtime, again, is from the detector shutting down for some portion of the time. Pileup concerns are most often for an imagng type of detector - more like the camera on your phone - that collects an exposure for some amount of time and then read it off.

Chandra, XMM-Newton, Swift, Suzaku, (somewhat confusingly NuStar is in both camps), let X-rays come in from the sky, get focused onto the CCD camera, and then in some time interval, often ~1s, reads off the image and clears the CCD. The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon. So when an individual pixel gets << 1 X-ray per frame on average, you are in good shape. *But* when there is significant probability of getting >~1 X-ray per frame on a given pixel, then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

This registering of multiple X-rays incorrectly as one more energetic single event is pileup, and it is the bane of most imaging detectors for those of us working with bright sources. (Pileup can occur in instruments like NICER too, when two X-rays hit during the event-latch phase, but this is something like a <1% effect for a 100 Crab-bright source, so well in the camp of being negligible for all practical purposes.)

In the most extreme cases of pileup, the pixel digitizer, which usually records values on something like a 16-bit value (e.g., from -32768:32767), can roll over and then appears negative when too much charge hits at once.

> I'm pretty sure the RXTE data I was working with earlier this year didn't have this identifier because if I remember correctly I couldn't execute *$ignore bad* on the data in XSPEC.

Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

> Here's a working version of the confidence interval / error results script:
>
> I put my XSPECtools.py file which has all of these functions into a custom module in my local site-packages folder for python and it seems like it's working perfectly. I import the file like this: *from personalastropy.xspectools import XSPECtools as xt*, and I execute a particular function like this: *xt.returnFitResults().*
>
> Happy early Thanksgiving!
> Thaddaeus

That's really great Thaddaeus! I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Very well done!

After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

Best,

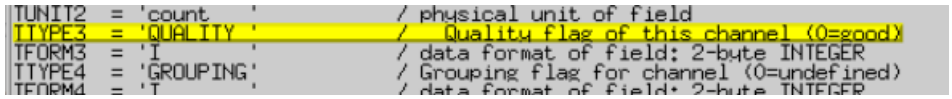
-Jack

> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Wed, Nov 11, 2020 at 12:07 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hey Thaddaeus,
>
> What a week it's been eh? (But way to go PA!)
>
> > I've held off from running another major fitting routine since the weekend because after I ran that my last one I decided that it was time to write some standardized scripts for wrangling the results from all the log files.
>
> Excellent!
>
> > Speaking of which, when Cuneo uses green triangles for "*parameters consistent with 0 within errors*", does this refer to situations when either the upper value of a parameter is equal to zero (because it got pegged at the hard upper limit) or when the lower limit is invalid and equal to zero?
>
> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> I *love* the outline of your routines, and "wow" is what I have to say about that. Way to go Thaddaeus, it's great you're tackling this and in a very useful manner.
>
> > Out of curiosity, where did Cuneo get the count rate values for his data? I don't think it's the same as the Net Count Rate that's displayed whenever you execute *$show data* in XSPEC....can Cuneo's count rate (c/s) be found by dividing the sum of the counts in the included channels by the total observation time?
>
> About 99% yes. "Usual" practice for NICER has become to normalize to an active set of 52 detectors. Your spectrum is a collection of all good events. So a tally of all the (good) events between 0.5-10 keV divided by the exposure time is indeed the count rate detected. However, if only using 49 or 50 detectors (also common), one would scale up by 52/49 to show the rate on the full instrument.
>
> The one very tiny wrinkle to all this is a property called "deadtime". Each time the electronics on a detector register an X-ray event, they "latch" meaning that the detector stops listening for new signals. Effectively, the detector is turned off for a short amount of time it uses to calculate properties of event (what time it arrived, and how much charge was deposited). For NICER, this is typically ~20 microseconds.
>
> As a concrete example of what this means in practice: when the count rate per detector reaches ~10^3/s (or in other words the full instrument light curve was at ~50,000/s), there would be ~20e-6 * 1e3 = 2% deadtime. This would mean that the *actual* count rate is ~2% higher than was detected (because the true exposure time was actually 98% the clock time). Here, most likely Cuneo didn't worry about folding in deadtime effects (I do have my pipeline correct for it automatically, so all your spectra already factor that in).
>
> This is obviously a very small here for NICER, but I wanted to dive into it because for many missions that aren't timing beasts like NICER, deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events.
> Instruments like NuSTAR deal with this for bright sources.

Also sorta random, I came across a value called "quality" when I was looking at a file in fv; are the quality values the values XSPEC refers to when I say *$ignore bad*?



Yes exactly this. This won't tend to come up for NICER, but e.g., for instruments that have some bad pixels and a spatially dispersed spectrum (think the rainbow from a prism), or uncalibrated energy ranges, this can be used.

> * This is probably one of my favorite one-liner functions in Python:
>
> for line in f:
> linelist = (re.sub(' +', ',', line)).split(',')
>
> Where f is a file opened with: *with open(...) as f*.

Python is a wonder for its very efficient one-lining loops!

As usual, you're knocking it out of the park Thaddaeus.

In other news, a new grad student (Santi) just joined me and he's going to start looking at some QPO data.

Best,

-Jack

> On Thu, Oct 29, 2020 at 12:49 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Thaddaeus,
>
> Whoops - Santiago is my grad student and I notice my advisor brain accidentally called you Santi in my last message. Very sorry about that!
>
> Name confusions aside, very glad the patch worked ok. Yeah, setting query to yes can cause problems sometimes as you found. I don't have good rules for avoiding or escaping those runaways, but I've had luck before tweaking the extra convergence terms in the fit command when it's stuck (the delta & beta) and the tolerance term in error.
>
> -Jack
>
> On Thu, Oct 29, 2020 at 11:34 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Hi Dr. Steiner!
>
> Alright I think that patching did the trick; I'm currently running XSPEC v 12.11.0m. Surprisingly I didn't have to rebuild relxill which was nice. Now that the 12.11.0k patch has been installed, I think XSPEC just "snags" and keeps fitting if I have query set to yes instead of crashing—when I was looking into this, a particular observation ran an error routine for three days nonstop in the background until I aborted it!
>
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi
>
> On Thu, Oct 22, 2020 at 12:46 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Santi,
>
> OK, that's encouraging! Let me know if the patching does the trick. Often for small patches, you won't need to recompile local models, but you should quickly see if it has trouble loading and that would be your sign.

It's hard to predict how parallel processing will impact a given task. The outcome depends on where the bottlenecks occur (if processing, then expect significant improvement, if memory swapping, then probably not). For running on multiple threads, a **rough** estimate is sqrt(N) gain, but that's just ballpark.

-Jack

On Thu, Oct 22, 2020 at 11:56 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> I just got some really great news on the crashes!
>
> Last weekend I was dismayed because even though I was playing with the limits extensively (especially for gamma), the crashing was still occurring unabated. This made me wonder if it was a software issue, so I reached out to the HEASARC Help Desk. I found out that a very similar issue was fixed in XSPEC patch 12.11.0k: *For some complex fitting and error command cases, XSPEC may terminate with a GSL message such as 'SVD decomposition failed to converge'.* This wasn't encouraging at first because XSPEC patch 12.11.0k came out in June (I built the current version of xspec on my vm in ~July), but I checked my XSPEC version with *$version,* and it turns out that my XSPEC version is just 12.11.0! I'm working on installing the patch, I bet this will clear a lot of things up. I think I'm going to have to rebuild relxill after I execute the partial system rebuild.
>
> The only thing that confuses me about this is that I didn't really have issues with core dumping when I was fitting relxill a lot back in August (I think it did come up for error fits back then though), so I'm confused why it really started acting up recently.
>
> Also I don't think I've asked this previously, but out of curiosity, should I expect significant reductions in fit times with two core parallel processing?
>
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Thu, Oct 22, 2020 at 10:52 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Just checking in to see how fits are progressing and if playing with those limits has allowed any headway against the crashes?
>
> (Yes, whenever I "rm -rf " I try to take beat and check that I am not about to bilk the whole machine! With great commands-line tools comes great ...)
>
> -Jack
>
> On Fri, Oct 16, 2020 at 3:24 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
>
> > Hi!
> >
> > Now that I think about it, I do recall noticing that many (if not most) of the fit failures had gammas in the 3.4 range. Good to know about the 1.6-2.2 range for the residuals, I'll play with that now.
> >
> > Oh about the attachments: the screen shot from visual studio is just some descriptions I was writing about the different functions I was using, and the microsoft paint one just illustrates the positive contribution that is almost always eliminated by a ~1.75 KeV gaussian of varying width.
> >
> > Over this weekend should I continue to focus on maximizing the number of observations that I can get good fits and confidence intervals for?
> >
> > I'm intrigued by this disk truncation controversy, I'm going to read about it this weekend.
> >
> > Many thanks!
> > Thaddaeus
> >
> > P.S. I was reading about some dangerous linux commands the other day; there are some unnerving commands out there. Imagine if someone ran rm -rf in your terminal. Shudder.
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*

-Enrico Fermi

On Thu, Oct 15, 2020 at 10:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> > Regarding the application of systematic errors, that's good to know. I've often seen something like the "NICER team recommends the application of a systematic error of 1%" in papers that use NICER data.
>
> Yes, that's a "rough" benchmark, but a reasonable one.
>
> > I love the common.tcl file, it's so handy! I think I'm just going to use it from now on because it's simpler on the execution end (just two commands), and it shows the error strings for each parameter.
>
> Yes, it's quite nice work by Jeremy :).
>
> > A little random: I came across an interesting statement in a paper by Plant et al. 2015 pertaining to observed disk truncation in the hard state of GX 339–4: "This result implies that the current sample of spin estimates in the hard state are inaccurate. Therefore, any distinct conclusions drawn from these estimates, such as the spin-powering of relativistic jets, may well be biased." Since that paper was published has this issue been fully exploited and resolved, or are there still black holes with possibly incorrect spin measurements out there? Also, is physical disk truncation or changing coronal height the more widely accepted cause for profiles similar to disk truncation?
>
> This has been a topic with a lot of controversy that is still ongoing (Garcia et al 2015 and followup papers by Wang et al. 2019 talk about this). I'd say the Plant picture is strongly disfavored at present, but not entirely out. The robust view is that allowing for the possibility of truncation, a (bright) hard-state spin measurement should be regarded as an estimated lower limit. Presently, the argument is focused around how much truncation could there be in bright hard states, with most reasonable numbers being a few times the ISCO (a factor <10). (Coronal geometry is always an important consideration, but usually they tend to fit very compact.)
>
> > An update on the rsp/arf issue: I modified my approach and fully integrated astropy.io.fits, so when I'm producing my xspec script commands rmf and arf file names are pulled directly from the header of each observation's fits file. Because of the two errors described below, I haven't checked how chi-square improves when the correct rmf and arf files are used though.
>
> Very good, that should make some impact when working.
>
> > I've encountered two weird errors I've never encountered before in XSPEC this week: the first is frequent core dumping [SVD decomposition failed to converge...Default GSL error handler invoked...Segmentation fault (core dumped)], and the second is this odd "XSPEC Error: No variable parameters for fit" after executing "fit" for a model with *multiple* free parameters (including the usual Gamma, FracSctr, Tin, diskbb norm, etc.). My first thought was "*is this related to relxill having problems due to a low gamma*"—but these issues both happen even when Gamma is > 2.
>
> The table can be sparse in a few places, and I think very large Gammas are also sparse (>~3.4, if memory serves). I'd say try freezing parameters and retrying the fit to identify the culprit parameter.
>
> > Also a little random: this week I decided it was time to start commenting on my functions in my python files—at one point I was looking through some older files for a particular technique I used and it took a little while to find the correct version of it, so I decided that from now on I'm describing my functions for future reference.

Yes, documenting code is super important and a great habit to get used to. (I wish I was better about it!)

> This is preliminary, but I attached an example of a chi-scape scarred by a common morph of the residual I last sent you. The positive contribution that begins shortly after 1.6 KeV is almost always eliminated by a ~1.75 KeV gaussian of varying width for most observations. The current issue is modeling the various forms the following negative residuals take. Currently working on those. Earlier you mentioned that we could try ignoring that narrow range where these residuals occur, would ignoring channels between 1.6-2.2 KeV be ignoring too much of the data? If we decide to ignore that range, I would think it best to get *>plt wdata* outputs to exactly nail down the dimensions of this range, but for now should I keep trying to model the residuals?
>
> I was reading through some older emails, going to look into your recommendation regarding logxi.

No, I think 1.6-2.2 isn't crazy and you can certainly roll with that if you are having too much trouble taming the residuals with a model. Maybe do a couple test cases to see how the model parameters are affected on both fronts.

Hey, and I didn't see a description of your attachments in case you want to say anything about those.

Glad you're back in the thick of things.

Best,

-Jack

> On Thu, Oct 8, 2020 at 1:35 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> > Hi Dr. Steiner!
> >
> > Quick note: I've been getting back into it this week and should have some interesting things to report this weekend!
> >
> > Thaddaeus
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
>
> On Tue, Sep 29, 2020 at 2:19 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> > Hi Thaddaeus,
> >
> > Thanks for the update.
> >
> > > Quick note on the residuals: since we last communicated I investigated the application of a systematic error (3-5%) in the < 2 KeV energy band, but this didn't improve the residuals so I'm assuming that you already accounted for this, like the binning, in your reprocessing?
> >
> > No, actually. But systematic errors are only incorporated with some fit statistics - chisq for instance, but I don't believe are incorporated in cstat or pgstat. The way I like to think about it is the systematic error acts as a reweighting of the data's constraint and serves to de-emphasize regions with the highest signal. I offer for information sake that that 3-5%

is too large a systematic error to include as a rule. Something like 1-2% is more reasonable for a region with poorer calibration quality.

> I also discovered that I've been unintentionally forcing an incorrect response file on multiple MAXI J1535-571 observations—the rsp I was using for all observations was `nicer_d49_55575341.rmf`, but after running a quick check (the fitsio package has been so helpful, thanks for recommending it to me!), I found that multiple observations request `nicer_d49_u100_150_55575341.rmf` in their headers.

Ah! That's very good to know, and may be the culprit for some of the residuals you've been battling. Check that the arf files (keyword ANCRFILE) is also set correctly.

> I'm likely almost done with the SAT (took it Saturday, going to take it again this coming Saturday)*, and once I'm done I aim to return to a progress pace similar to the pace we had in the summer.

Congratulations - that's a nice milestone!

> G'mar chatima tova (if you observe Yom Kippur)!

Same to you.

> *I might take it again in November in the event that I bungle those tests.

I'd put dollars to donuts you did great!

My best,

-Jack

> On Tue, Sep 22, 2020 at 1:21 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> > Interesting I'll look into that!
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
> >
> > On Tue, Sep 22, 2020 at 1:15 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> > > Similar to that. I used another language (IDL) to automatically produce a series of tcl/tk scripts and then executed those distributed over a number of cores on a workstation.
> > >
> > > -Jack
> > >
> > > On Tue, Sep 22, 2020 at 1:13 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> > > > Sounds good!
> > > >
> > > > In that case did you operate solely in TCL?
> > > >
> > > > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > > > -Enrico Fermi

On Tue, Sep 22, 2020 at 1:10 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> For >75% I meant observations where source flux was more than 75% bg flux, and for 4% I meant observations where source flux was ~4 % bg flux. That makes sense—interestingly the observations with source flux ~4% bg flux were, by best fit, in the SPL state, with the highest flux and gamma for all the observations that had reasonable values for these parameters.

Ah, now I see. Yes, when the bg flux is very high, the noise level (N=sqrt(B+S)) where B and S are background and signal counts in a channel, then the constraining power of the data (S/N) is lowest. If the background is reasonably estimated, the fits will likely be good but poorly constrained; if the background is off, then all bets are off.

> xD. I think it's a very impressive feat! Did you use pyxspec to loop through all the observations?

Thanks. I used a different language actually, but python/pyxspec would be an equally good way to do it.

Best,

-Jack

> On Tue, Sep 22, 2020 at 12:39 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
>
> Hi Thaddaeus,
>
> > That's good to know. I guess what's surprising is some relative of this large residual in the 1.6-2.0 KeV is present in most of the source flux > 75% observations (I wonder if the peak feature falsely inflates the source flux for some of them?). Going back to a couple emails ago, I was checking some of my all time best fits which had source fluxes around ~4%—what allows such faint observations to fit well?
>
> Just to clarify, do you mean 75% and 4% of the Eddington limit, peak luminosity, or something else? Fainter observations, given some typical observation time, obviously have fewer counts which means larger fractional errors and less constraining power on the model. So if there is a deficiency in the calibration or model being used, it will look more prominent in brighter + long exposure time data.
>
> > Also, I'm very intrigued by your methods in one of your 2016 papers , "STRONGER REFLECTION FROM BLACK HOLE ACCRETION DISKS IN SOFT X-RAY STATES". How did you automate fitting the 15,000 observations reliably and within a reasonable time frame?
>
> A reasonable time frame is relative :), but I used about two dozen cores to run the fits which helped. I also stuck with computationally simplistic models, and for my runs with simpl, used lower resolution (via energies) in the run, since RXTE/PCA - the instrument used there - also has rather poor energy resolution compared to NICER.
>
> > Could I possibly have the ~86 observations between obsids 1200220101 – 1200220186?
>
> Spectral files are attached. Response files are too big for me to attach, so I'll keep them up here for a few days: https://nicer-web.mit.edu/nicerview/thadd_resp_m1727.tar.gz
>
> > Ah that makes sense. In that case did Xu et al. just freeze Rin at the ISCO to have less free parameters?

That's the usual tactic, yes, when fitting for spin.

Best,

-Jack

On Thu, Sep 17, 2020 at 11:00 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Great to hear you've been digging in and exploring! I would suggest though that you avoid being too concerned about the residual features. If something is localized and only ~percent level deviation, then that's likely sufficient. *The goal should be to fit those sufficiently that they don't alter the continuum, but at some level it becomes diminishing returns to try fitting an artefact.* That said, if you wish, I can get you the data on MAXI J1727-203. Just let me know if you have a particular ObsID from Kevin's paper you want to try.

Your question about degeneracy is great. The condition of parameters being strongly correlated with one another is degeneracy. Basically, this is a very common situation where two parameters are dependent upon one another to some extent, so a probable value of one will condition a probable range of values with the other. For instance, if you want to fit x and y with the constraint x^2+y^2 = 4+/-1, you would get a ring of radius 2 with x,y values that obviously depend on one another; they are *not independent*. Speaking generally for more realistic cases, the interaction between parameters is often pretty convoluted, but commonly things like Gamma and Normalization are correlated with one another, so a contour plot of the confidence interval would look banana shaped rather than like a circle.

Best,

-Jack

On Thu, Sep 17, 2020 at 9:18 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

Thanks for checking in! Since we last spoke I've been taking a deeper look into a couple articles (Alabarta et al. 2020, Xu et al. 2018, and Parikh et al. 2019) and playing around a bit with the residual fix you suggested. As tested on multiple observations with varying fit statistic contributions in the 1.6-1.8 KeV range (chi-squared contribs. ranging between 500-3000), the gaussian centered around 1.75 KeV combined with the "edge" works great!

Most importantly, I found a method to more accurately investigate the residual features: *plt>wdata*. Now that I can actually see what the chi-sq contrib. is for every point, I've been having a lot more success in approaching residuals in the 1.6-2.2 KeV range, especially when the residual patterns after 1.8 KeV differ greatly from what can quickly be ameliorated with the edge feature (it seems that for the majority of observations the <1.8 KeV excess is easily subdued by the gaussian fix, but after 1.8 KeV things often get complicated, especially when a third feature shows up between 1.86 to 2.0 KeV. This third feature resembles a sort of gaussian-like crevice about 0.13 KeV in width and often reaches a chi^2 contribution of -5331 (+) at its minima, pushing the "edge" feature out to a threshold energy of ~2+ KeV). I haven't found a definitive solution yet, but a gaussian absorption with an energy of ~1.86 KeV and a width of ~0.14 KeV is looking promising.

While I've been working with these MAXI J1535-571 residuals I've been wondering if the residuals Alabarta et al. discuss in Nicer data for MAXI J1727−203 are of similar shape—is there any chance I could procure some pipeline data from you on MAXI J1727-203 to look into this (if it's not an inconvenience for you and or you don't think it's a distraction on my part of course).

Out of curiosity, what does "parameter degeneracy" mean? My first guess would be this example: if I only told you what the slope of a line was (e.g. '2'), and then asked you for the individual values of Δy and Δx—would Δy and Δx be degenerate? Example of where I came across this: (from Xu et al. 2018) "The inner disk radius, R_{in}, and the black hole spin parameter, a*, are degenerate, as they both control the effective inner accretion disk radius. Therefore, for simplicity, during the spectral fitting we assume that the inner disk extends down to the ISCO by fixing R_{in} at the radius of the ISCO, and fit for the black hole spin as a free parameter."

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

Kiker, Thaddaeus <s024622@students.lmsd.org> Sun, Jan 3, 2021 at 11:22 AM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

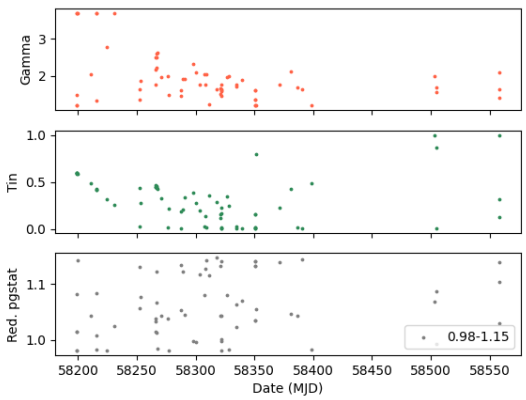
Hi Dr. Steiner!

Thank you for your well wishes! I hope the same for you and your family as well!

To start the new year off well, I'm excited to share some very good news with you. I've finished the nthcomp look at all the MAXI J1535-571 data! It's been a sprint of sorts since Tuesday when I started working on it intensely, so I wouldn't say I'm completely done (I want to standardize a couple functions I wrote up), but otherwise I have some great things to share!

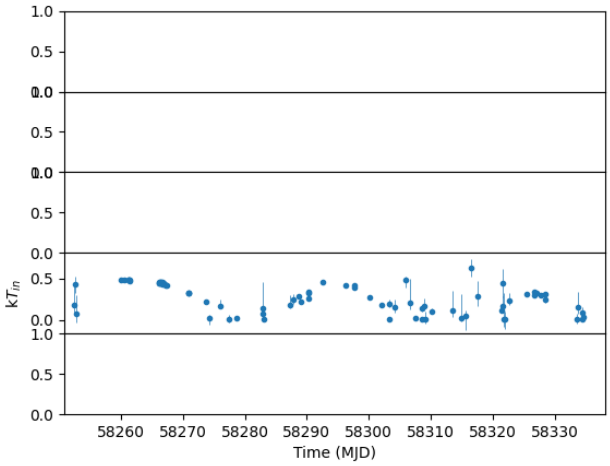
First, a thought back on the relxill models: don't nthcomp and simpl produce different values for gamma and hence they should not be mixed with each other? This thought came to me this morning when I was tidying things up. Since I was tieing relxill's gamma to simpl's, I wonder if that threw anything off. Now that I think about it more, I don't think it would catastrophically derail things, but it would still be inappropriate to tie gammas from different models right?

The first hint of a breakthrough happened on Tuesday when I realized I forgot to filter for background fraction. I looped through all the data files that contributed to the last plot I sent you and only plotted fits from files where background was < 50% source, giving me this, much neater, plot (where gamma and inner disk temp are actually somewhat correlated:



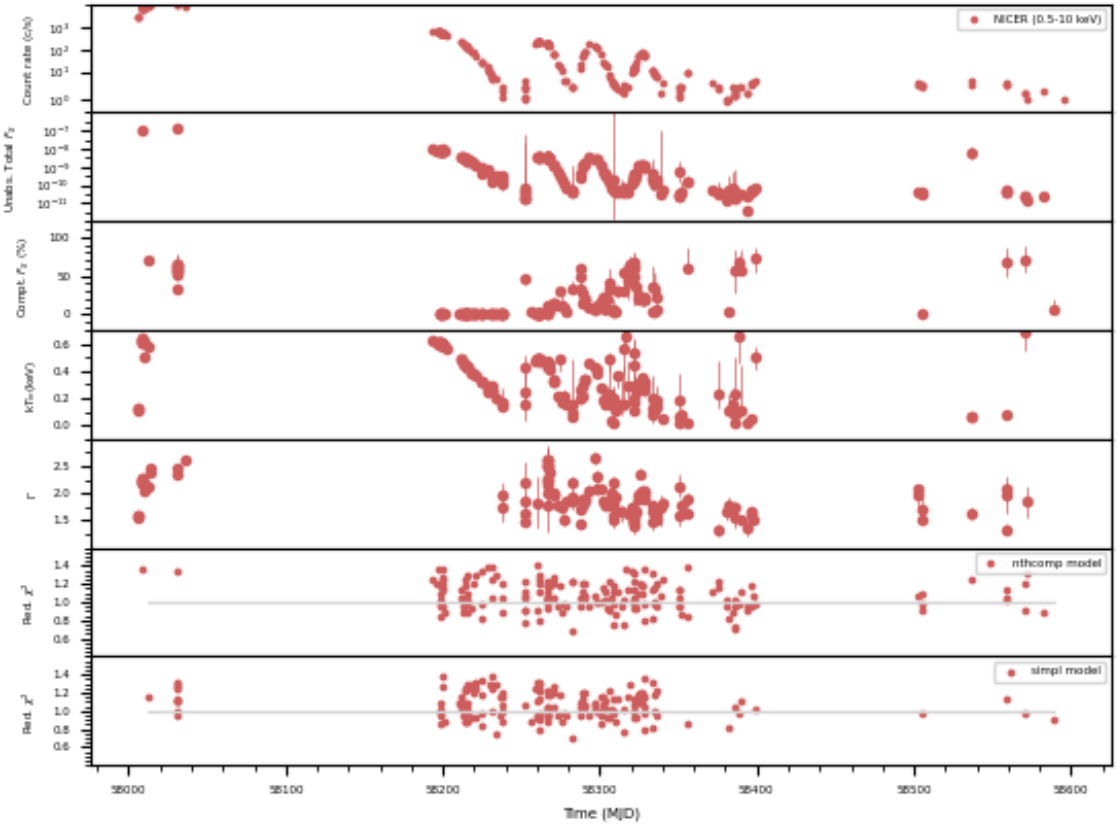
This cleaned things up pretty well, but I noticed there were only a couple points with gamma > 3 (all of which were right on 3.5), so I ran another routine with the upper limits on gamma set to 3. The points that were right on 3.5 were on 3.0 for gamma in this routine, so I wondered if it wasn't an issue of the upper limit but an issue of insensitivity. When I tried to estimate 1-σ errors on a couple of the fits that had 3.5 upper limits, XSPEC returned "***Warning: Parameter pegged at hard limit: 3.5"—which I take as being caused by gamma's insensitivity to the fit in the observations.

Then I plotted Tin measurements from that fit routine with gamma's upper limit set to 3.0 with one sigma (all confidence levels I bring up are one sigma unless otherwise noted, as in Cuneo's paper; though I must say, one sigma sounds so much better than 68% confidence even though they're the same thing lol), here's how that turned out:



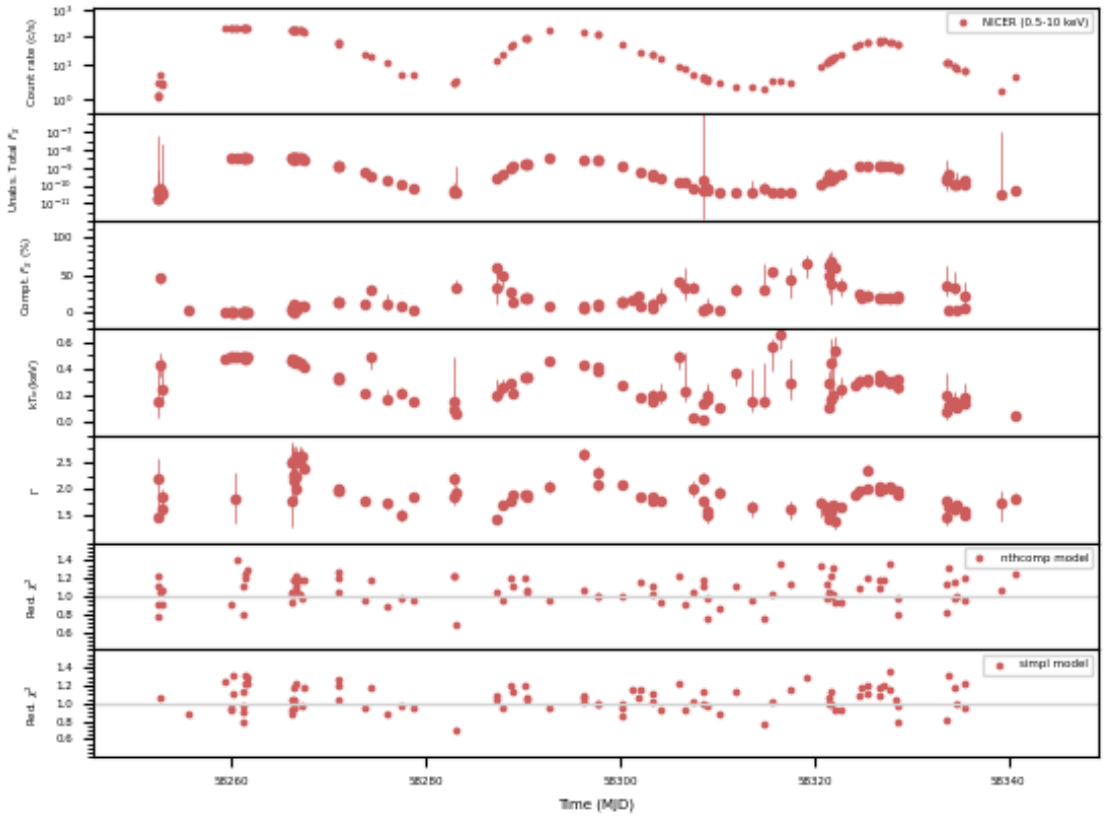
Pretty nice, eh?

So fast forward (and a bunch of partial fitting routines later), I'm proud to present these:



This is my final plot for the MAXI data. The top panel is background subtracted count rate (a note on this later), the second is unabsorbed total flux on the range 0.5-10.0 keV, the third panel is comptonized flux percentage (calculated with a separate simpl model, a note on this later), the fourth inner disk temp, the fifth gamma, the sixth reduced pgstat for the nthcomp model, and the seventh the reduced pgstat for the simpl model.

Here's what my plot looks like during the reflaring period investigated by Cuneo:



All parameters were calculated with the nthcomp model except for comptonized fraction. Model descriptions below:

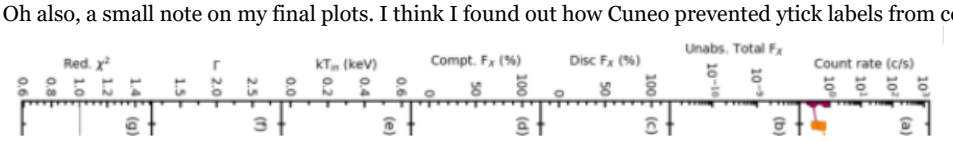
##Model One##
Model: tbabs(diskbb+nthcomp)
Delta fit statistic: 1.
Noticed Ranges: 0.5-1.5 2.3-10.0 keV
nH: 3.2 (frozen)
Tin lower limit: 0.01
Tin upper limit: 1.0
Gamma lower limit: 1.2
Gamma upper limit: 3.0
High energy rollover: 100 keV
inp_type: 1
Redshift: 0

##Model Two##
Model: tbabs(simpl(diskbb))
Delta fit statistic: 1.
Noticed Ranges: 0.5-1.5 2.3-10.0 keV
nH: 3.2 (frozen)
Tin lower limit: 0.01
Tin upper limit: 1.0
Gamma lower limit: 1.4 #I adopted this limit from your paper in which you used simpl to fit those 15,000-odd observations.
Gamma upper limit: 3.4 #I adopted this limit from your paper in which you used simpl to fit those 15,000-odd observations.
UpScOnly value: 1

****
Per the background subtracted count rates, I corrected my code to account for your pointers; the important snippet is below:

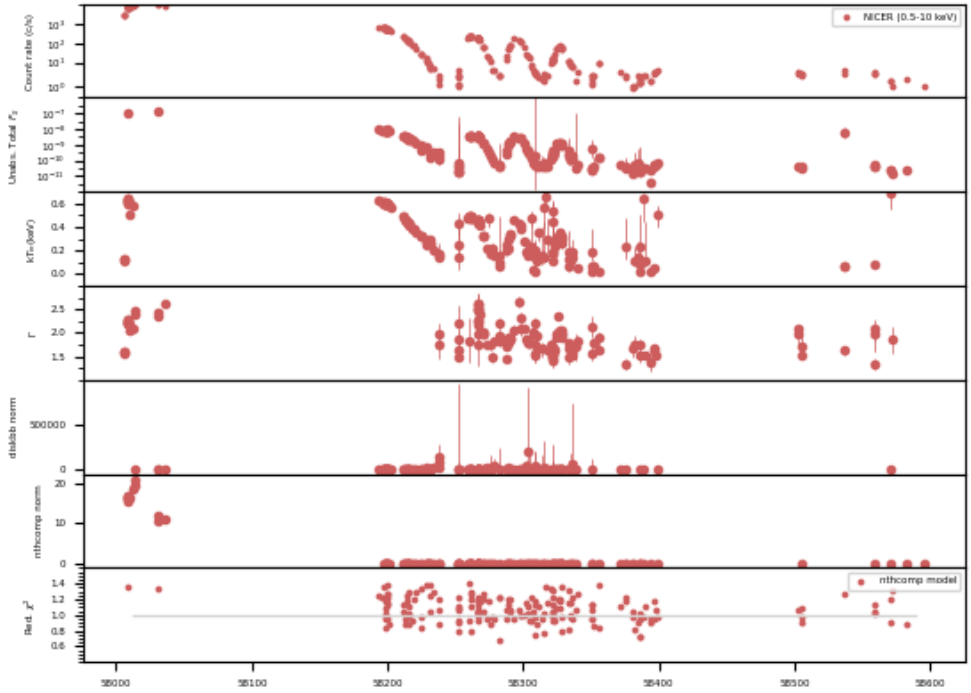
```
scaled_bg_counts = (sum(restricted_bg_counts_list)/bg_exp_time)*exp_time
count_rate = sum(restricted_data_counts_list)-scaled_bg_counts
count_rate = count_rate/exp_time
os.remove(temp_data_file)
os.remove(temp_bg_file)

out_list.append((seg_id+':'+str(count_rate)))
```

Oh also, a small note on my final plots. I think I found out how Cuneo prevented ytick labels from colliding:

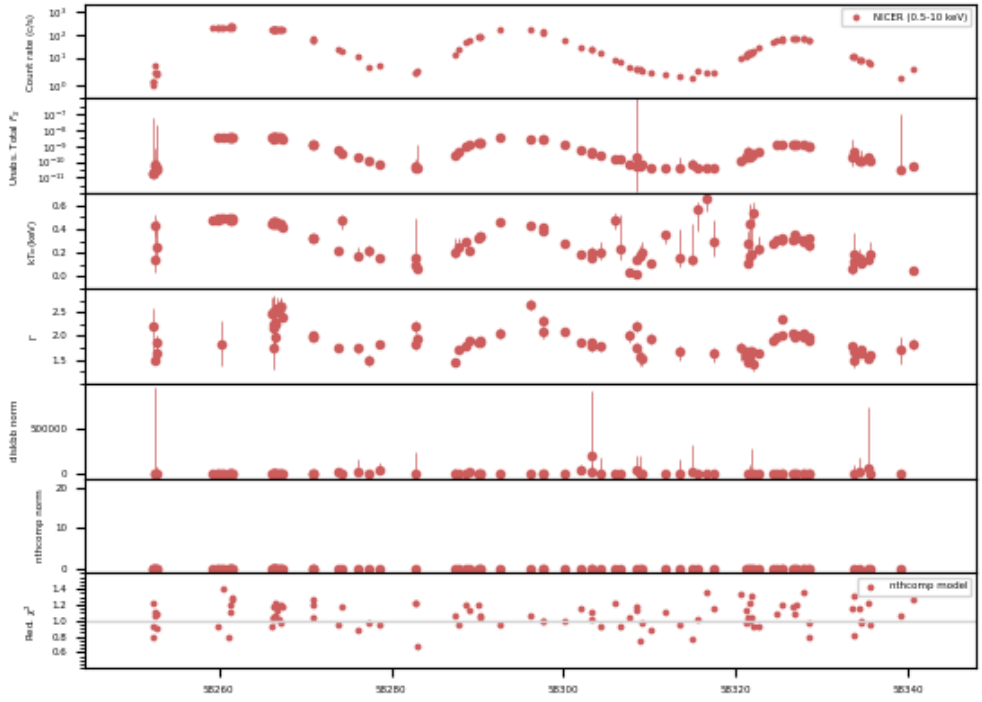


I think she set the ylim bounds somewhat lesser and somewhat more than the min(y) and max(y) vals—so I did this and it improves the aesthetics significantly in my opinion.

As you requested, here's a plot with the diskbb and nthcomp norm components:



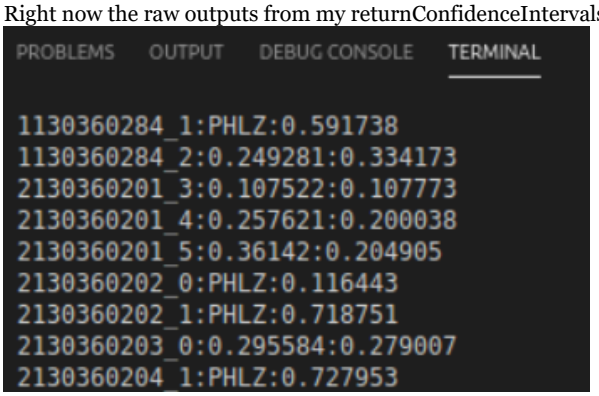
(Full range)



(cuneo date range)

I didn't include values consistent with zero within errors in any of these plots, but I can if you'd like.

Right now the raw outputs from my returnConfidenceIntervals() function look like this:



(where PHLZ stands for Parameter Hit Lower Zero and PHUZ stands for Parameter Hit Upper Zero)

Early in the week I thought about changing the output of this function to some kind of multidimensional numpy array like this:

```
[['seg_id_1','seg_id_2'],[lower_1,lower_2],[upper_1,upper_2]]
```

But I didn't really feel like it because the current function works fast enough for me (I don't ~~see the need to vectorize~~ (more accurate statement: feel like vectorizing) all my functions yet; unless you would advise me to in which case I'll get right to it lol). *

I did write a standard function for returning float values of value date, value and a two dimensional array for yerr for parameters based on an input x=the parameters output from returnConfidenceIntervals(), y=theoutput from a returnDates() function I wrote a bit ago, and z = an output from the returnFitResults() function I wrote a bit ago. Lol reading that last sentence, it looks a little confusing so I'll add this function to my package and document it in markdown and show it to you in a later email.

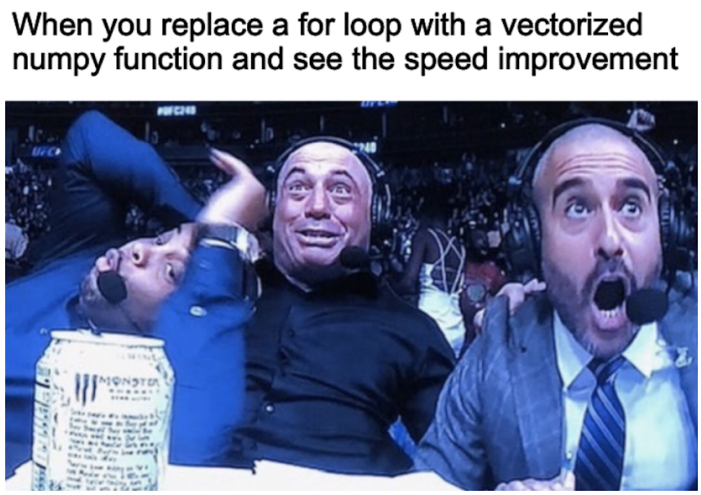
I still have to iron out my hardness ratios function, will get to that soon.

A random, last thought: in compsci club at school we've recently been discussing algorithmic efficiency in the context of things like sorting algorithms, which inspired me to think about this in the context of my code. So far I'd say a noticeable improvement that has resulted from this is that I'm limiting the number of computationally expensive tasks I put under for and conditional loops as long as I can execute them outside the loop. An example of this would be declaring a scipy.interpolate.interp1d object outside of a for loop rather than declaring it for every data value in a set (this came up in something else I was working on, but I brought it up because I think it's a good example).

All data referenced in the plots, the python file I wrote for creating these plots, and the python file I used to write the fit commands are all attached in the email_attachments .xz tarball.

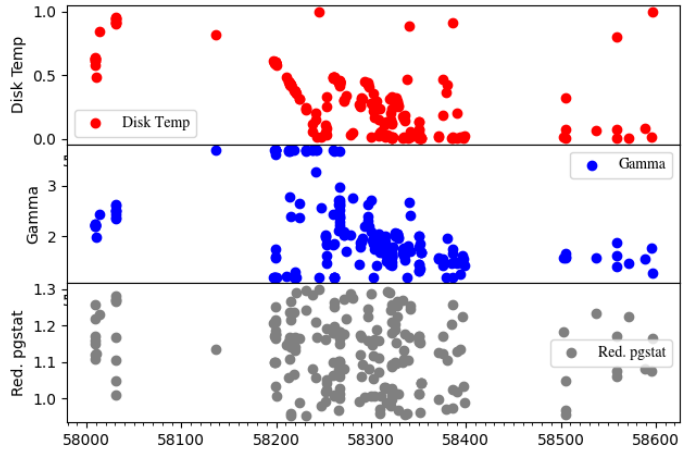
Cheers (and Happy New Year)!
Thaddaeus

*

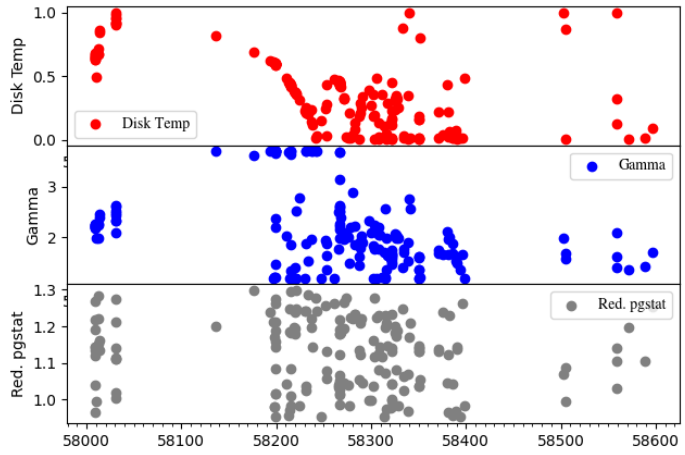


"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Dec 31, 2020 at 2:38 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Thaddaeus,
>
> It's been quite a month everywhere! Hope you had a nice holiday season and that you and your family have been doing well. Wishing you a happy and better New Year ahead.
>
> -Jack
>
> On Wed, Dec 9, 2020 at 2:57 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> > Hey Thaddaeus,
> >
> > Those parameter plots look great! Can you add in the diskbb and compTT normalizations as panels as well?
> >
> > It looks to me like things are probably going wrong because you are probably using bin-number rather than channel number. Sometimes these two get conflated, but here by bin I mean groups of one or more channels, where a channel is the thing that is hard-coded in the spectral file (and PI-bins correspond to 10 eV increments). So you'd want to use channels 50-299 and 300-999 for your two bands.
> >
> > The other thing to check is that the background is scaled properly. So you want to make sure that to background you subtract is:
> > BG_Spectrum_MatchedToData = BG_Counts_FromBGFile / BGFile_ExpTime * DataFile_ExpTime
> >
> > Hopefully my shorthand there is clear enough.
> >
> > Best,
> >
> > -Jack
> >
> > On Wed, Dec 9, 2020 at 2:39 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> > > Hi Dr. Steiner!
> > >
> > > Phew! The number of potential files for fitting has shot up to 720 from 185 now that I'm searching for and including all GTIs files.
> > >
> > > I ran a couple fitting routines with a simple tbabs(diskbb+nthcomp) model on all the files just to get a feel for how including a lot more files would possibly change things, and the initial results from these fitting routines were quite unusual (though they were blisteringly fast compared to relxill fits regardless —with nH frozen on these routines, I was fitting all the files with 700 fit iterations each in under an hour on my mom's computer) :



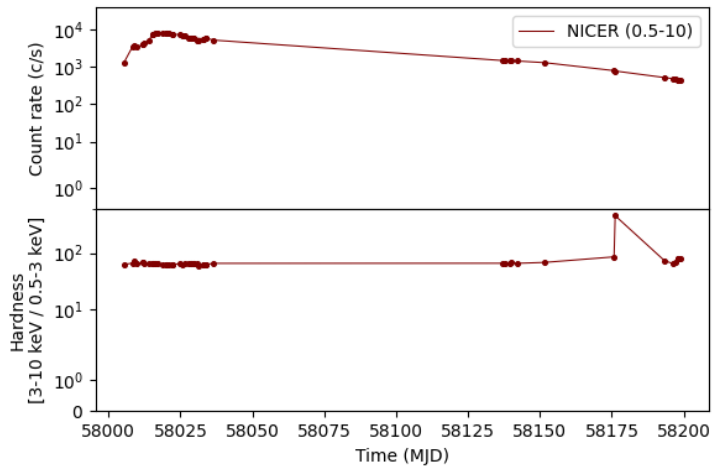
^ That routine had diskkbb Tin upper limit set to 1, gamma upper limit set to 3.7, nH frozen to 3.2, and the entire energy range from 0.5-10.0 keV noticed.



^That routine was the same except the energy interval 1.5-2.3 keV was ignored.

I think I see a little pattern of correlated variation between disk temp and gamma masked under the erratic points in both plots...I'm wondering, should I set some cutoff exposure time for the files? I recall seeing some ~22 sec exp. time data files when I was looking through other GTIs, and I'm thinking since these files would have less data to constrain the model, crazy parameters can be permissibly returned by XSPEC when these files are fit.

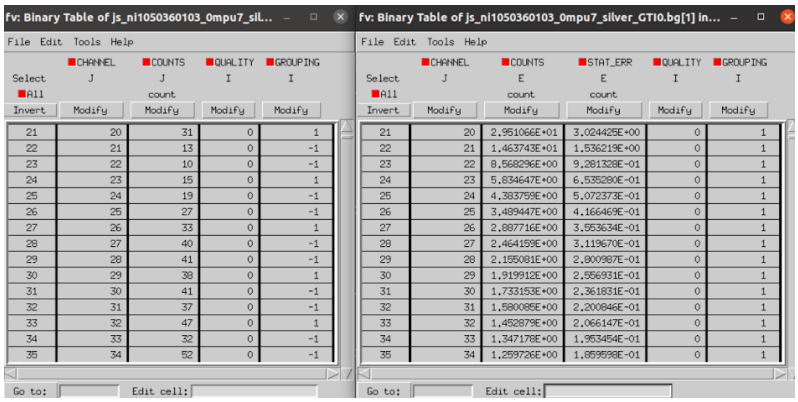
Also, could I get your advice on calculating hardness ratios? I modified my count rate function to correct for background counts and it's working pretty well (see the top subplot below):



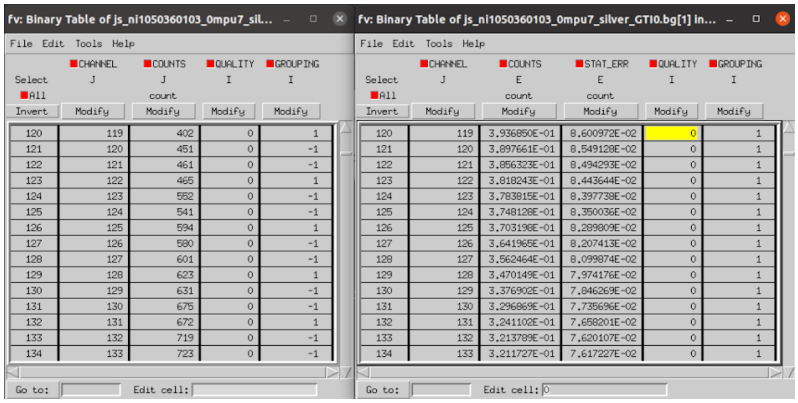
Almost identical to Cuneo's count rate plot, eh?

As you can see in the bottom subplot, the hardness ratio values are crazy, and I can't figure out how to rein them in. The way my function for hardness ratio works is I sum the number of counts in the parameter defined hard band and subtract the number of background counts in the predefined hard band from that value, giving me a net hard band counts sum. I then divide that value by (sum of soft counts-sum soft background counts). Code is attached.

I was checking with fv, and it's interesting that in this case (and what I think is true in general) the bg counts are very low in the hard band but fairly high in the thermal:



Channels 22-99 = 0.5-3 keV



Channels 101-254 = 3.0-10.0 keV

Cheers!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 3:36 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> > Thanksgiving was very nice, thanks!
> Yay that's great to hear!
>
> > Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.
> Sounds good.
>
> > YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.
> !!! I will do this then!
>
> > Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

Will do.

Thanks!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 12:28 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Thanksgiving was very nice, thanks! Glad yours with the grandparents was too.

> I reprocessed the data last weekend (11/22) but the results were a little weird so I've been tweaking some things and prepping for reprocessing again this week.
>
> I was wondering if I should take a different approach to the instrumental residuals by running a preliminary fit on all files and logging the fit statistic landscape, so I can then loop through and identify which files actually require the 1.5-2.2 keV range to be ignored and which can have it noticed (I'm wondering about this because from a preliminary check a lot of the disk temperatures were behaving weirdly, which I think could be related to having less data in the thermal band to constrain them). What do you think?

Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.

> Also, I've only been fitting the silver, zero good time interval files; would you recommend me to fit some of the GTI1s, GT2s, etc. in hopes to get more good fits?

YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.

> P.S. I wrote two new standardized functions (one calculates and returns count rates, the other calculates and returns hardness ratios) and nice documentation for all my standard functions so far.

Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

-Jack

On Mon, Nov 23, 2020 at 8:34 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Will do!

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 23, 2020 at 4:30 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Sounds good! Glad your mom's computer has some CPU cycles to share. Keep me posted when you get the results in hand. Meantime, happy Thanksgiving.

-Jack

On Sun, Nov 22, 2020 at 8:01 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> > Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.
>
> Yes I've been fitting with the inner radius frozen to -1. That's good to know.
>
> > As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind. So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)
>
> Sounds good.
>
> > As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.
>
> Yes I agree it was nice that they showed the underlying component.
>
> I've been running the first iterations of the reprocessing last night and today and I've had a little breakthrough—last night I quickly updated the version of XSPEC I had on the vm that I installed on my mom's computer way back in June (I also installed relxill on it), and I've been dividing the work between her computer and mine—the routines are concluding much quicker (her computer has four cores xD).
>
> Best!
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Fri, Nov 20, 2020 at 9:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.
>
> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind.
>
> So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)
>
> And in response to your email yesterday:
>
> > So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?
>
> Yes, spot on!
>
> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

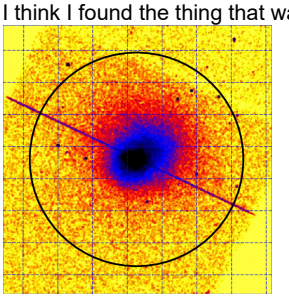
Best,

-Jack

On Fri, Nov 20, 2020 at 8:13 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Shoot! I forgot this bring this up. Would you recommend me to freeze the spin and inclination parameters to published values (that are in agreement with some of my previous best fits) to simplify the parameter space, or should I keep them free? The only benefit I can see from keeping them free would be sometimes error locks up with no variable parameters to use when free parameters get pegged due to their insignificance to the fit, and I think having them free could help prevent this...though spin and inclination seem like parameters that would be the first to get pegged during an error routine anyways.
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Thu, Nov 19, 2020 at 8:07 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hey!
>
> > Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.
>
> Yeah, safety is (rightly so) the first priority.
>
> > A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!
>
> Oo sounds cool, I hope you all have fun!
>
> > Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.
> Perfect.
>
> > The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon.
> Interesting.
>
> > then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.
> Very interesting.
>
> I think I found the thing that was on the edge of my memory: a Chandra image of the quasar H1821+643.
>
> 
>
> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?
>
> > Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.
> I should probably rephrase what I said earlier. When I ignored bad, no channels were ignored so I guess that meant no poor quality flags were raised.
>
> > I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then

> getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Agreed. I find it so enjoyable: the initial encounter with a seemingly insurmountable challenge, the brainstorming, the troubleshooting, and *especially* the successful execution of a solution. It's extremely satisfying—and to use an analogy from my youth* it feels kinda similar to when I would build things in lego.

*I say an analogy from my youth but that's somewhat misleading. I still occasionally play legos, and these days the only difference is that I have a partner: my younger brother ;).

> After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Definitely. That is exactly what I'm thinking.

> Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

:)).

Best!
Thaddaeus

Oh I almost forgot. I don't remember how/why I came across this paper (Arumugasamy et al. 2018), but after looking at the plots I've been meaning to ask you about them:

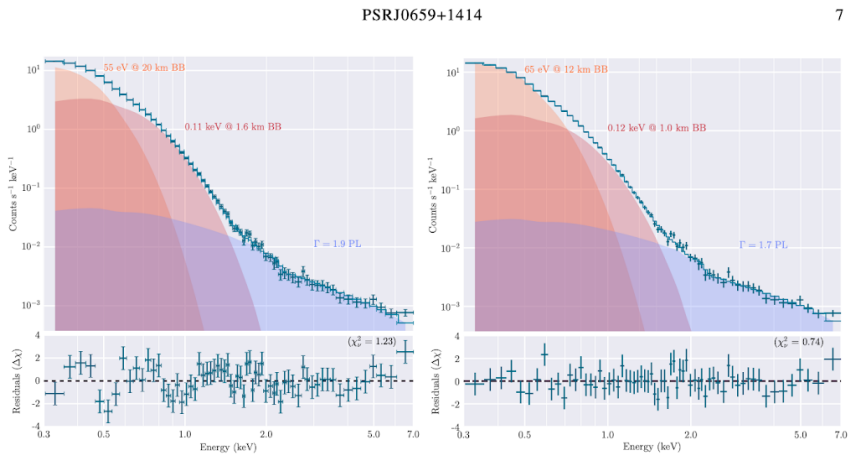


Figure 4. 2BBPL model, without (left) and with (right) `Gabs` absorption component, fit to the phase-integrated spectrum of J0659+1414. The model parameters correspond to maximum posterior probability, which in the case of G2BBPL are different from the distribution medians listed in Table 2.

I've been using the darkgrid style (like the plots above) with the rc font parameter set to Nimbus Roman in most of my plots ... but after seeing these plots I'm thinking I was to switch back to the mpl defaults—these plots are kinda distracting to me in their aesthetic.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Wed, Nov 18, 2020 at 10:05 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> > It was a wild week indeed—and things seem to keep getting crazier. We were in a hybrid model since October 19th, but as of Tuesday we went indefinitely back to all online which is a two sided coin. On one hand the online schedule gives me a lot more free time, but on the other I'm going to miss the extra class time in my harder classes like mechanics and calc. Thanksgiving is around the corner though! Up to any interesting plans yourself? I think we're just going to have my mom's parents over because we already interact with them frequently, but no big gathering this year (obviously).
>
> No kidding! Feels like we're living in a bad TV show.
>
> Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.

A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> Would it be okay if I emailed Dr. Cuneo about this? This is very important for how I structure the confidence intervals code (and whether I make it more sensitive and able to detect more than the four generalized abnormal scenarios described later in this email).

Sure thing, email away.

> Haha! I just read that NuStar's is ~2.5 *milliseconds*.

Yeah and that's actually not too bad compared to most things out there. NICER is just an absolute timing monster.

> I do have my pipeline correct for it automatically, so all your spectra already factor that in.
>
> Cool! Does that mean I can just go ahead and calculate count rates as number of counts / exp. time and not worry about adjusting the result because you've already accounted for deadtime?

Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

> deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.
>
> Yes. This makes sense. I feel like I've seen something related where the center of an object was too bright in the X-Ray band (which oversaturated the detector?) so the color gradient of brightness was black in the center because the author's couldn't determine the brightness at the center. It kinda looked like an artificial coronagraph. Would that be related to a situation where the dead time was more than the event time?

(Deep dive warning:)
Yes, you're speaking about "pileup" which isn't exactly related to deadtime except that both can be important when the source is bright. Deadtime, again, is from the detector shutting down for some portion of the time. Pileup concerns are most often for an imagng type of detector - more like the camera on your phone - that collects an exposure for some amount of time and then read it off.

Chandra, XMM-Newton, Swift, Suzaku, (somewhat confusingly NuStar is in both camps), let X-rays come in from the sky, get focused onto the CCD camera, and then in some time interval, often ~1s, reads off the image and clears the CCD. The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon. So when an individual pixel gets << 1 X-ray per frame on average, you are in good shape. *But* when there is significant probability of getting >~1 X-ray per frame on a given pixel, then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

This registering of multiple X-rays incorrectly as one more energetic single event is pileup, and it is the bane of most imaging detectors for those of us working with bright sources. (Pileup can occur in instruments like NICER too, when two X-rays hit during the event-latch phase, but this is something like a <1% effect for a 100 Crab-bright source, so well in the camp of being negligible for all practical purposes.)

In the most extreme cases of pileup, the pixel digitizer, which usually records values on something like a 16-bit value (e.g., from -32768:32767), can roll over and then appears negative when too much charge hits at once.

> I'm pretty sure the RXTE data I was working with earlier this year didn't have this identifier because if I remember correctly I couldn't execute *$ignore bad* on the data in XSPEC.

Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

> Here's a working version of the confidence interval / error results script:
>
> I put my XSPECtools.py file which has all of these functions into a custom module in my local site-packages folder for python and it seems like it's working perfectly. I import the file like this: *from personalastropy.xspectools import XSPECtools as xt*, and I execute a particular function like this: *xt.returnFitResults()*.

> Happy early Thanksgiving!
> Thaddaeus

That's really great Thaddaeus! I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Very well done!

After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

Best,

-Jack

> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Wed, Nov 11, 2020 at 12:07 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hey Thaddaeus,
>
> What a week it's been eh? (But way to go PA!)
>
> > I've held off from running another major fitting routine since the weekend because after I ran that my last one I decided that it was time to write some standardized scripts for wrangling the results from all the log files.
>
> Excellent!
>
> > Speaking of which, when Cuneo uses green triangles for "*parameters consistent with 0 within errors*", does this refer to situations when either the upper value of a parameter is equal to zero (because it got pegged at the hard upper limit) or when the lower limit is invalid and equal to zero?
>
> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> I *love* the outline of your routines, and "wow" is what I have to say about that. Way to go Thaddaeus, it's great you're tackling this and in a very useful manner.
>
> > Out of curiosity, where did Cuneo get the count rate values for his data? I don't think it's the same as the Net Count Rate that's displayed whenever you execute *$show data* in XSPEC....can Cuneo's count rate (c/s) be found by dividing the sum of the counts in the included channels by the total observation time?
>
> About 99% yes. "Usual" practice for NICER has become to normalize to an active set of 52 detectors. Your spectrum is a collection of all good events. So a tally of all the (good) events between 0.5-10 keV divided by the exposure time is indeed the count rate detected. However, if only using 49 or 50 detectors (also common), one would scale up by 52/49 to show the rate on the full instrument.
>
> The one very tiny wrinkle to all this is a property called "deadtime". Each time the electronics on a detector register an X-ray event, they "latch" meaning that the detector stops listening for new signals. Effectively, the detector is turned off for a short amount of time it uses to calculate properties of event (what time it arrived, and how much charge was deposited). For NICER, this is typically ~20 microseconds.
>
> As a concrete example of what this means in practice: when the count rate per detector reaches ~10^3/s (or in other words the full instrument light curve was at ~50,000/s), there would be ~20e-6 * 1e3 = 2% deadtime. This would mean that the *actual* count rate is ~2% higher than was detected (because the true exposure time was actually 98% the clock time). Here, most likely Cuneo didn't worry about folding in deadtime effects (I do have my pipeline correct for it automatically, so all your spectra already factor that in).

This is obviously a very small here for NICER, but I wanted to dive into it because for many missions that aren't timing beasts like NICER, deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.

> Also sorta random, I came across a value called "quality" when I was looking at a file in fv; are the quality values the values XSPEC refers to when I say *$ignore bad*?

```
TUNIT2  = 'count    '           / physical unit of field
TTYPE3  = 'QUALITY  '           /   Quality flag of this channel (0=good)
TFORM3  = 'I        '           / data format of field: 2-byte INTEGER
TTYPE4  = 'GROUPING '           / Grouping flag for channel (0=undefined)
TFORM4  = 'I        '           / data format of field: 2-byte INTEGER
```

Yes exactly this. This won't tend to come up for NICER, but e.g., for instruments that have some bad pixels and a spatially dispersed spectrum (think the rainbow from a prism), or uncalibrated energy ranges, this can be used.

> * This is probably one of my favorite one-liner functions in Python:
>
> for line in f:
> linelist = (re.sub(' +', ',', line)).split(',')
>
> Where f is a file opened with: *with open(...) as f.*

Python is a wonder for its very efficient one-lining loops!

As usual, you're knocking it out of the park Thaddaeus.

In other news, a new grad student (Santi) just joined me and he's going to start looking at some QPO data.

Best,

-Jack

> On Thu, Oct 29, 2020 at 12:49 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Thaddaeus,
>
> Whoops - Santiago is my grad student and I notice my advisor brain accidentally called you Santi in my last message. Very sorry about that!
>
> Name confusions aside, very glad the patch worked ok. Yeah, setting query to yes can cause problems sometimes as you found. I don't have good rules for avoiding or escaping those runaways, but I've had luck before tweaking the extra convergence terms in the fit command when it's stuck (the delta & beta) and the tolerance term in error.
>
> -Jack
>
> On Thu, Oct 29, 2020 at 11:34 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Hi Dr. Steiner!
>
> Alright I think that patching did the trick; I'm currently running XSPEC v 12.11.0m. Surprisingly I didn't have to rebuild relxill which was nice. Now that the 12.11.0k patch has been installed, I think XSPEC just "snags" and keeps fitting if I have query set to yes instead of crashing—when I was looking into this, a particular observation ran an error routine for three days nonstop in the background until I aborted it!
>
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi
>
> On Thu, Oct 22, 2020 at 12:46 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Santi,

OK, that's encouraging! Let me know if the patching does the trick. Often for small patches, you won't need to recompile local models, but you should quickly see if it has trouble loading and that would be your sign.

It's hard to predict how parallel processing will impact a given task. The outcome depends on where the bottlenecks occur (if processing, then expect significant improvement, if memory swapping, then probably not). For running on multiple threads, a **rough** estimate is sqrt(N) gain, but that's just ballpark.

-Jack

On Thu, Oct 22, 2020 at 11:56 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> I just got some really great news on the crashes!
>
> Last weekend I was dismayed because even though I was playing with the limits extensively (especially for gamma), the crashing was still occurring unabated. This made me wonder if it was a software issue, so I reached out to the HEASARC Help Desk. I found out that a very similar issue was fixed in XSPEC patch 12.11.0k: *For some complex fitting and error command cases, XSPEC may terminate with a GSL message such as 'SVD decomposition failed to converge'.* This wasn't encouraging at first because XSPEC patch 12.11.0k came out in June (I built the current version of xspec on my vm in ~July), but I checked my XSPEC version with *$version,* and it turns out that my XSPEC version is just 12.11.0! I'm working on installing the patch, I bet this will clear a lot of things up. I think I'm going to have to rebuild relxill after I execute the partial system rebuild.
>
> The only thing that confuses me about this is that I didn't really have issues with core dumping when I was fitting relxill a lot back in August (I think it did come up for error fits back then though), so I'm confused why it really started acting up recently.
>
> Also I don't think I've asked this previously, but out of curiosity, should I expect significant reductions in fit times with two core parallel processing?
>
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Thu, Oct 22, 2020 at 10:52 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Just checking in to see how fits are progressing and if playing with those limits has allowed any headway against the crashes?
>
> (Yes, whenever I "rm -rf " I try to take beat and check that I am not about to bilk the whole machine! With great commands-line tools comes great ...)
>
> -Jack
>
> On Fri, Oct 16, 2020 at 3:24 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
>
> > Hi!
> >
> > Now that I think about it, I do recall noticing that many (if not most) of the fit failures had gammas in the 3.4 range. Good to know about the 1.6-2.2 range for the residuals, I'll play with that now.
> >
> > Oh about the attachments: the screen shot from visual studio is just some descriptions I was writing about the different functions I was using, and the microsoft paint one just illustrates the positive contribution that is almost always eliminated by a ~1.75 KeV gaussian of varying width.
> >
> > Over this weekend should I continue to focus on maximizing the number of observations that I can get good fits and confidence intervals for?
> >
> > I'm intrigued by this disk truncation controversy, I'm going to read about it this weekend.
> >
> > Many thanks!
> > Thaddaeus

P.S. I was reading about some dangerous linux commands the other day; there are some unnerving commands out there. Imagine if someone ran rm -rf in your terminal. Shudder.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 15, 2020 at 10:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> Regarding the application of systematic errors, that's good to know. I've often seen something like the "NICER team recommends the application of a systematic error of 1%" in papers that use NICER data.

Yes, that's a "rough" benchmark, but a reasonable one.

> I love the common.tcl file, it's so handy! I think I'm just going to use it from now on because it's simpler on the execution end (just two commands), and it shows the error strings for each parameter.

Yes, it's quite nice work by Jeremy :).

> A little random: I came across an interesting statement in a paper by Plant et al. 2015 pertaining to observed disk truncation in the hard state of GX 339−4: "This result implies that the current sample of spin estimates in the hard state are inaccurate. Therefore, any distinct conclusions drawn from these estimates, such as the spin-powering of relativistic jets, may well be biased." Since that paper was published has this issue been fully exploited and resolved, or are there still black holes with possibly incorrect spin measurements out there? Also, is physical disk truncation or changing coronal height the more widely accepted cause for profiles similar to disk truncation?

This has been a topic with a lot of controversy that is still ongoing (Garcia et al 2015 and followup papers by Wang et al. 2019 talk about this). I'd say the Plant picture is strongly disfavored at present, but not entirely out. The robust view is that allowing for the possibility of truncation, a (bright) hard-state spin measurement should be regarded as an estimated lower limit. Presently, the argument is focused around how much truncation could there be in bright hard states, with most reasonable numbers being a few times the ISCO (a factor <10). (Coronal geometry is always an important consideration, but usually they tend to fit very compact.)

> An update on the rsp/arf issue: I modified my approach and fully integrated astropy.io.fits, so when I'm producing my xspec script commands rmf and arf file names are pulled directly from the header of each observation's fits file. Because of the two errors described below, I haven't checked how chi-square improves when the correct rmf and arf files are used though.

Very good, that should make some impact when working.

> I've encountered two weird errors I've never encountered before in XSPEC this week: the first is frequent core dumping [SVD decomposition failed to converge...Default GSL error handler invoked...Segmentation fault (core dumped)], and the second is this odd "XSPEC Error: No variable parameters for fit" after executing "fit" for a model with *multiple* free parameters (including the usual Gamma, FracSctr, Tin, diskbb norm, etc.). My first thought was "*is this related to relxill having problems due to a low gamma*"—but these issues both happen even when Gamma is > 2.

The table can be sparse in a few places, and I think very large Gammas are also sparse (>~3.4, if memory serves). I'd say try freezing parameters and retrying the fit to identify the culprit parameter.

> Also a little random: this week I decided it was time to start commenting on my functions in my python files—at one point I was looking through some older files for a particular technique I used and it took a little while to find the correct version of it, so I decided that from now on I'm describing my functions for future reference.

Yes, documenting code is super important and a great habit to get used to. (I wish I was better about it!)

> This is preliminary, but I attached an example of a chi-scape scarred by a common morph of the residual I last sent you. The positive contribution that begins shortly after 1.6 KeV is almost always eliminated by a ~1.75 KeV gaussian of varying width for most observations. The current issue is modeling the various forms the following negative residuals take. Currently working on those. Earlier you mentioned that we could try ignoring that narrow range where these residuals occur, would ignoring channels between 1.6-2.2 KeV be ignoring too much of the data? If we decide to ignore that range, I would think it best to get *>plt wdata* outputs to exactly nail down the dimensions of this range, but for now should I keep trying to model the residuals?
>
> I was reading through some older emails, going to look into your recommendation regarding logxi.

No, I think 1.6-2.2 isn't crazy and you can certainly roll with that if you are having too much trouble taming the residuals with a model. Maybe do a couple test cases to see how the model parameters are affected on both fronts.

Hey, and I didn't see a description of your attachments in case you want to say anything about those.

Glad you're back in the thick of things.

Best,

-Jack

> On Thu, Oct 8, 2020 at 1:35 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> > Hi Dr. Steiner!
> >
> > Quick note: I've been getting back into it this week and should have some interesting things to report this weekend!
> >
> > Thaddaeus
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
>
> On Tue, Sep 29, 2020 at 2:19 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Thanks for the update.

> Quick note on the residuals: since we last communicated I investigated the application of a systematic error (3-5%) in the < 2 KeV energy band, but this didn't improve the residuals so I'm assuming that you already accounted for this, like the binning, in your reprocessing?

No, actually. But systematic errors are only incorporated with some fit statistics - chisq for instance, but I don't believe are incorporated in cstat or pgstat. The way I like to think about it is the systematic error acts as a reweighting of the data's constraint and serves to de-emphasize regions with the highest signal. I offer for information sake that that 3-5% is too large a systematic error to include as a rule. Something like 1-2% is more reasonable for a region with poorer calibration quality.

> I also discovered that I've been unintentionally forcing an incorrect response file on multiple MAXI J1535-571 observations—the rsp I was using for all observations was `nicer_d49_55575341.rmf`, but after running a quick check (the fitsio package has been so helpful, thanks for recommending it to me!), I found that multiple observations request `nicer_d49_u100_150_55575341.rmf` in their headers.

Ah! That's very good to know, and may be the culprit for some of the residuals you've been battling. Check that the arf files (keyword ANCRFILE) is also set correctly.

> I'm likely almost done with the SAT (took it Saturday, going to take it again this coming Saturday)*, and once I'm done I aim to return to a progress pace similar to the pace we had in the summer.

Congratulations - that's a nice milestone!

> G'mar chatima tova (if you observe Yom Kippur)!

Same to you.

> *I might take it again in November in the event that I bungle those tests.

I'd put dollars to donuts you did great!

My best,

-Jack

> On Tue, Sep 22, 2020 at 1:21 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> > Interesting I'll look into that!
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
> >
> > On Tue, Sep 22, 2020 at 1:15 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> > > Similar to that. I used another language (IDL) to automatically produce a series of tcl/tk scripts and then executed those distributed over a number of cores on a workstation.
> > >
> > > -Jack

On Tue, Sep 22, 2020 at 1:13 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Sounds good!
>
> In that case did you operate solely in TCL?

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Tue, Sep 22, 2020 at 1:10 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> > For >75% I meant observations where source flux was more than 75% bg flux, and for 4% I meant observations where source flux was ~4 % bg flux. That makes sense—interestingly the observations with source flux ~4% bg flux were, by best fit, in the SPL state, with the highest flux and gamma for all the observations that had reasonable values for these parameters.
>
> Ah, now I see. Yes, when the bg flux is very high, the noise level (N=sqrt(B+S)) where B and S are background and signal counts in a channel, then the constraining power of the data (S/N) is lowest. If the background is reasonably estimated, the fits will likely be good but poorly constrained; if the background is off, then all bets are off.
>
> > xD. I think it's a very impressive feat! Did you use pyxspec to loop through all the observations?
>
> Thanks. I used a different language actually, but python/pyxspec would be an equally good way to do it.
>
> Best,
>
> -Jack
>
> > On Tue, Sep 22, 2020 at 12:39 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> >
> > Hi Thaddaeus,
> >
> > > That's good to know. I guess what's surprising is some relative of this large residual in the 1.6-2.0 KeV is present in most of the source flux > 75% observations (I wonder if the peak feature falsely inflates the source flux for some of them?). Going back to a couple emails ago, I was checking some of my all time best fits which had source fluxes around ~4%—what allows such faint observations to fit well?
> >
> > Just to clarify, do you mean 75% and 4% of the Eddington limit, peak luminosity, or something else? Fainter observations, given some typical observation time, obviously have fewer counts which means larger fractional errors and less constraining power on the model. So if there is a deficiency in the calibration or model being used, it will look more prominent in brighter + long exposure time data.
> >
> > > Also, I'm very intrigued by your methods in one of your 2016 papers , "STRONGER REFLECTION FROM BLACK HOLE ACCRETION DISKS IN SOFT X-RAY STATES". How did you automate fitting the 15,000 observations reliably and within a reasonable time frame?
> >
> > A reasonable time frame is relative :), but I used about two dozen cores to run the fits which helped. I also stuck with computationally simplistic

models, and for my runs with simpl, used lower resolution (via energies) in the run, since RXTE/PCA - the instrument used there - also has rather poor energy resolution compared to NICER.

> Could I possibly have the ~86 observations between obsids 1200220101 – 1200220186?

Spectral files are attached. Response files are too big for me to attach, so I'll keep them up here for a few days: https://nicer-web.mit.edu/nicerview/thadd_resp_m1727.tar.gz

> Ah that makes sense. In that case did Xu et al. just freeze Rin at the ISCO to have less free parameters?

That's the usual tactic, yes, when fitting for spin.

Best,

-Jack

On Thu, Sep 17, 2020 at 11:00 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Great to hear you've been digging in and exploring! I would suggest though that you avoid being too concerned about the residual features. If something is localized and only ~percent level deviation, then that's likely sufficient. *The goal should be to fit those sufficiently that they don't alter the continuum, but at some level it becomes diminishing returns to try fitting an artefact.* That said, if you wish, I can get you the data on MAXI J1727-203. Just let me know if you have a particular ObsID from Kevin's paper you want to try.
>
> Your question about degeneracy is great. The condition of parameters being strongly correlated with one another is degeneracy. Basically, this is a very common situation where two parameters are dependent upon one another to some extent, so a probable value of one will condition a probable range of values with the other. For instance, if you want to fit x and y with the constraint x^2+y^2 = 4+/-1, you would get a ring of radius 2 with x,y values that obviously depend on one another; they are *not independent*. Speaking generally for more realistic cases, the interaction between parameters is often pretty convoluted, but commonly things like Gamma and Normalization are correlated with one another, so a contour plot of the confidence interval would look banana shaped rather than like a circle.
>
> Best,
>
> -Jack

On Thu, Sep 17, 2020 at 9:18 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> Thanks for checking in! Since we last spoke I've been taking a deeper look into a couple articles (Alabarta et al. 2020, Xu et al. 2018, and Parikh et al. 2019) and playing around a bit with the residual fix you suggested. As tested on multiple observations with varying fit statistic contributions in the 1.6-1.8 KeV range (chi-squared contribs. ranging between 500-3000), the gaussian centered around 1.75 KeV combined with the "edge" works great!

Most importantly, I found a method to more accurately investigate the residual features: *plt>wdata*. Now that I can actually see what the chi-sq contrib. is for every point, I've been having a lot more success in approaching residuals in the 1.6-2.2 KeV range, especially when the residual patterns after 1.8 KeV differ greatly from what can quickly be ameliorated with the edge feature (it seems that for the majority of observations the <1.8 KeV excess is easily subdued by the gaussian fix, but after 1.8 KeV things often get complicated, especially when a third feature shows up between 1.86 to 2.0 KeV. This third feature resembles a sort of gaussian-like crevice about 0.13 KeV in width and often reaches a chi^2 contribution of -5331 (+) at its minima, pushing the "edge" feature out to a threshold energy of ~2+ KeV). I haven't found a definitive solution yet, but a gaussian absorption with an energy of ~1.86 KeV and a width of ~0.14 KeV is looking promising.

While I've been working with these MAXI J1535-571 residuals I've been wondering if the residuals Alabarta et al. discuss in Nicer data for MAXI J1727−203 are of similar shape—is there any chance I could procure some pipeline data from you on MAXI J1727-203 to look into this (if it's not an inconvenience for you and or you don't think it's a distraction on my part of course).

Out of curiosity, what does "parameter degeneracy" mean? My first guess would be this example: if I only told you what the slope of a line was (e.g. '2'), and then asked you for the individual values of Δy and Δx—would Δy and Δx be degenerate? Example of where I came across this: (from Xu et al. 2018) "The inner disk radius, R_{in}, and the black hole spin parameter, a*, are degenerate, as they both control the effective inner accretion disk radius. Therefore, for simplicity, during the spectral fitting we assume that the inner disk extends down to the ISCO by fixing R_{in} at the radius of the ISCO, and fit for the black hole spin as a free parameter."

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

email_attachments.tar.xz
3061K

Steiner, James <james.steiner@cfa.harvard.edu> Tue, Jan 5, 2021 at 2:23 PM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hey Thaddaeus,

Wow, that's an impressive and sweet-looking delivery!

> Thank you for your well wishes! I hope the same for you and your family as well!

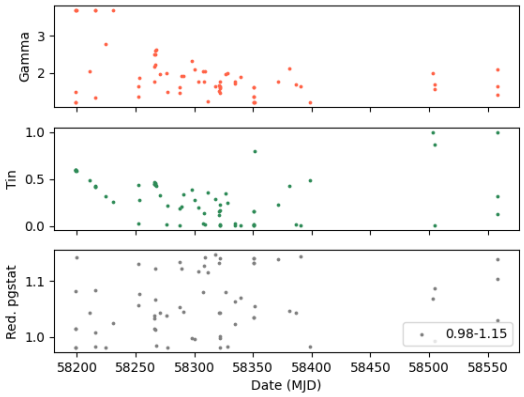
Thanks Thaddaeus, we're doing just fine and couldn't be happier to turn the page on 2020.

> First, a thought back on the relxill models: don't nthcomp and simpl produce different values for gamma and hence they should not be mixed with each other? This thought came to me this morning when I was tidying things up. Since I was tieing relxill's gamma to simpl's, I wonder if that threw anything off. Now that I think about it more, I don't think it would catastrophically derail things, but it would still be inappropriate to tie gammas from different models right?

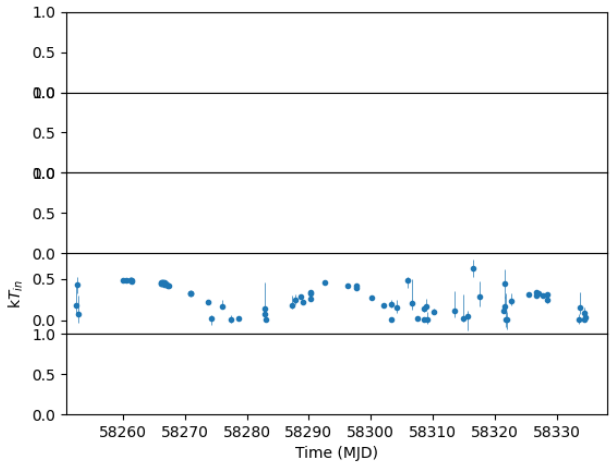
The default answer is that these should be identical - they both describe the shape of the Comptonized power-law component, and in the same way. For completeness, I should mention there has been some small exploration about decoupling these things with some loose physical arguments, but basically that's a stretch and I would absolutely keep them linked up as you've done.

> The first hint of a breakthrough happened on Tuesday when I realized I forgot to filter for background fraction. I looped through all the data files that contributed to the last plot I sent you and only plotted fits from files where background was < 50% source, giving me this, much neater, plot (where gamma and inner disk temp are actually somewhat correlated:

Ah, yes! Glad you caught that; keeping to the cleanest data is a very good idea, and 50% is reasonable; if things look at all funny, even down to ~20% would be a reasonable place to cut.

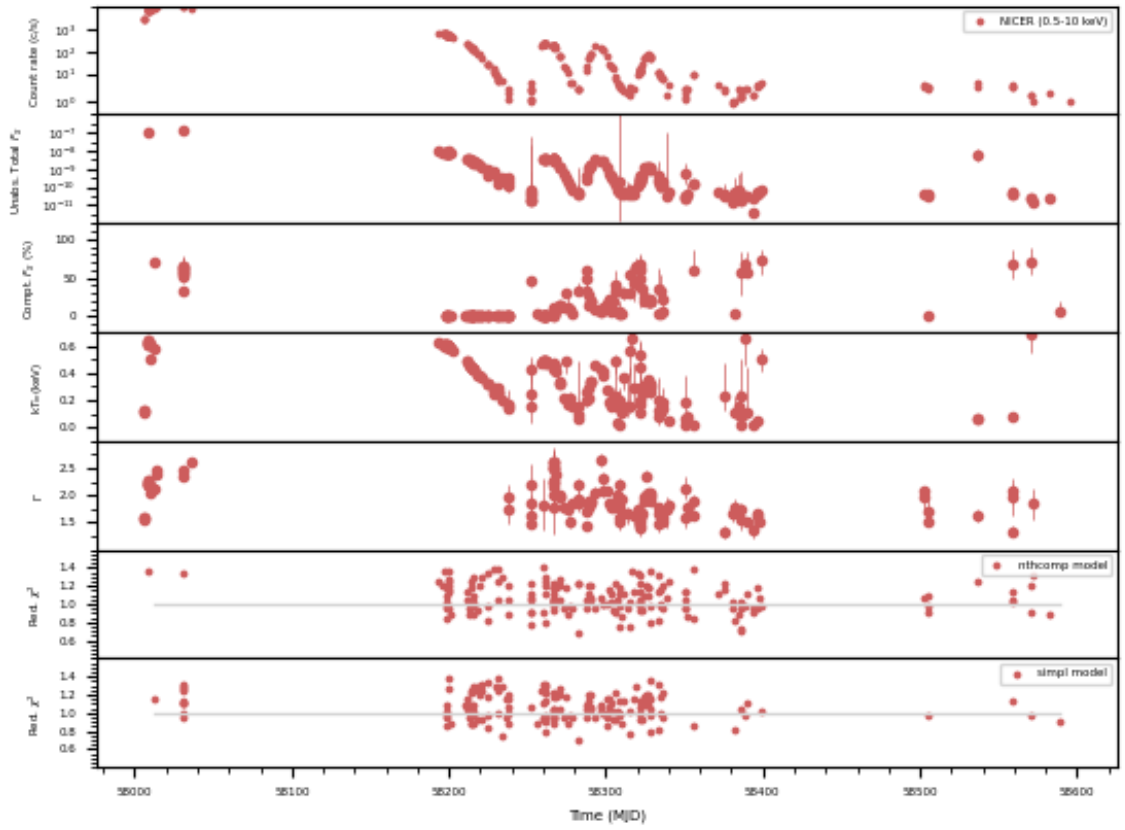
> 
>
>
> This cleaned things up pretty well, but I noticed there were only a couple points with gamma > 3 (all of which were right on 3.5), so I ran another routine with the upper limits on gamma set to 3. The points that were right on 3.5 were on 3.0 for gamma in this routine, so I wondered if it wasn't an issue of the upper limit but an issue of insensitivity. When I tried to estimate 1-σ errors on a couple of the fits that had 3.5 upper limits, XSPEC returned "***Warning: Parameter pegged at hard limit: 3.5"—which I take as being caused by gamma's insensitivity to the fit in the observations.

It is possible this is because the fit isn't sensitive to Gamma. (You could check this out by comparing fit statistics to the upper-limit 3.5 vs the upper-limit 3 fits, or also looking at the f_sc values - <~ 0.01 being an indication that isn't sensitive.) But alternatively, it's also possible that the data are sensitive to Gamma and either (i) the spectrum is very soft, wanting high Gamma in which case lowering the upper-limit would still cause it to peg but the fit would be markedly worse or (ii) *my guess for most likely for these data*, the background subtraction is off in those cases and oversubtracts, creating an artificial appearance of a very soft spectrum.

> Then I plotted Tin measurements from that fit routine with gamma's upper limit set to 3.0 with one sigma (all confidence levels I bring up are one sigma unless otherwise noted, as in Cuneo's paper; though I must say, one sigma sounds so much better than 68% confidence even though they're the same thing lol), here's how that turned out:
>
> 
>
>
> Pretty nice, eh?

Yes, this looks *very nice*!

> So fast forward (and a bunch of partial fitting routines later), I'm proud to present these:

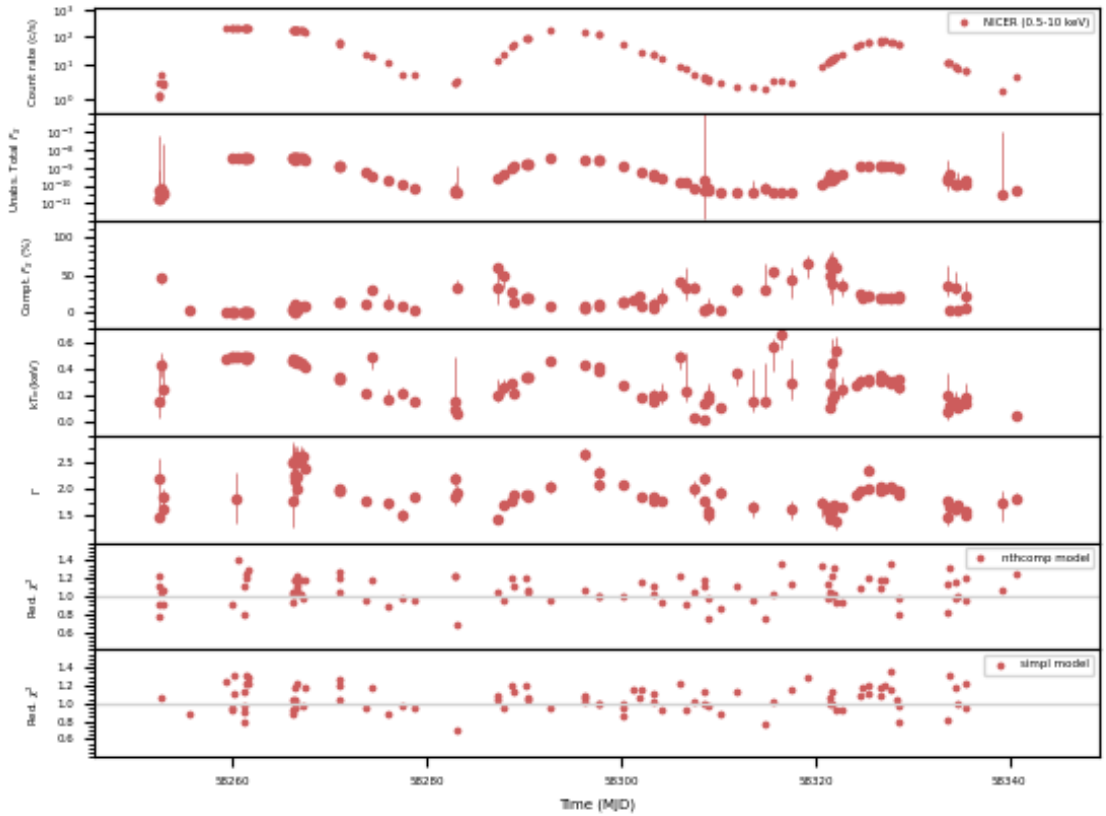


This is my final plot for the MAXI data. The top panel is background subtracted count rate (a note on this later), the second is unabsorbed total flux on the range 0.5-10.0 keV, the third panel is comptonized flux percentage (calculated with a separate simpl model, a note on this later), the fourth inner disk temp, the fifth gamma, the sixth reduced pgstat for the nthcomp model, and the seventh the reduced pgstat for the simpl model.

Excellent - these look really great. It appears to me that there are some points shown in the simpl panel that aren't shown in the nthcomp panels, and vice-versa. Can you mention what the screening difference is? (e.g., around day 55030)

For a disk-blackbody, the expectation is that L ~ T^4, so you can plot L vs T on a log-log plot and look at that scaling.

Here's what my plot looks like during the reflaring period investigated by Cuneo:



All parameters were calculated with the nthcomp model except for comptonized fraction. Model descriptions below:

##Model One##
Model: tbabs(diskbb+nthcomp)
Delta fit statistic: 1.
Noticed Ranges: 0.5-1.5 2.3-10.0 keV
nH: 3.2 (frozen)
Tin lower limit: 0.01
Tin upper limit: 1.0
Gamma lower limit: 1.2
Gamma upper limit: 3.0
High energy rollover: 100 keV
inp_type: 1
Redshift: 0

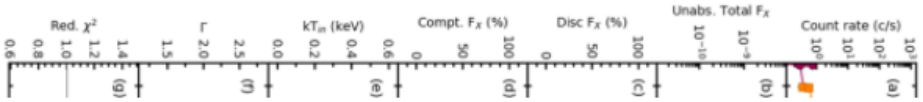
##Model Two##
Model: tbabs(simpl(diskbb))
Delta fit statistic: 1.
Noticed Ranges: 0.5-1.5 2.3-10.0 keV
nH: 3.2 (frozen)
Tin lower limit: 0.01
Tin upper limit: 1.0
Gamma lower limit: 1.4 #I adopted this limit from your paper in which you used simpl to fit those 15,000-odd observations.
Gamma upper limit: 3.4 #I adopted this limit from your paper in which you used simpl to fit those 15,000-odd observations.
UpScOnly value: 1

Looks good.

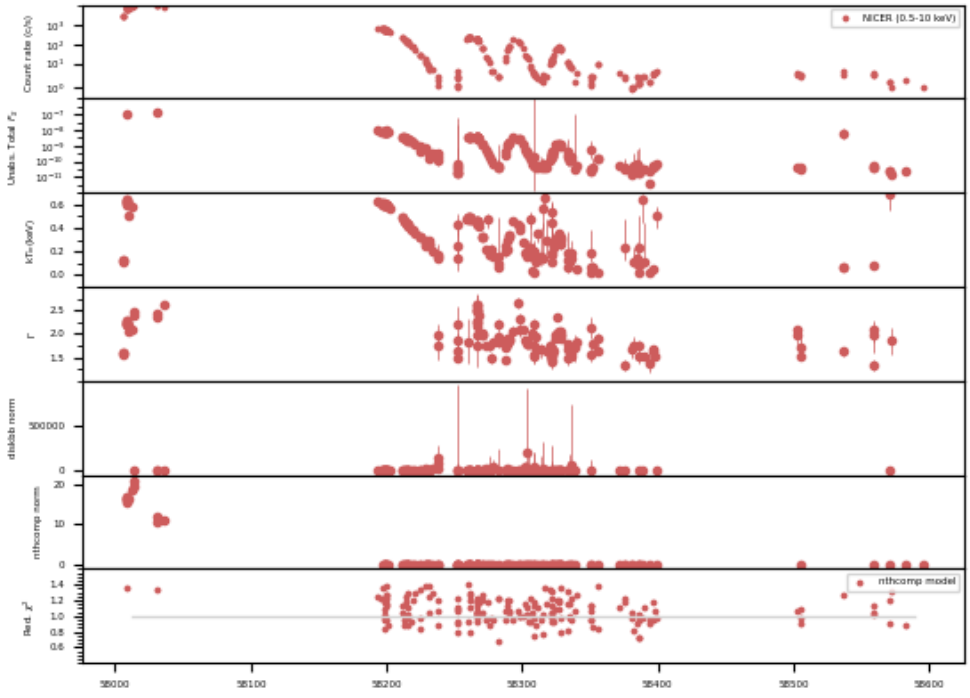
****
Per the background subtracted count rates, I corrected my code to account for your pointers; the important snippet is below:

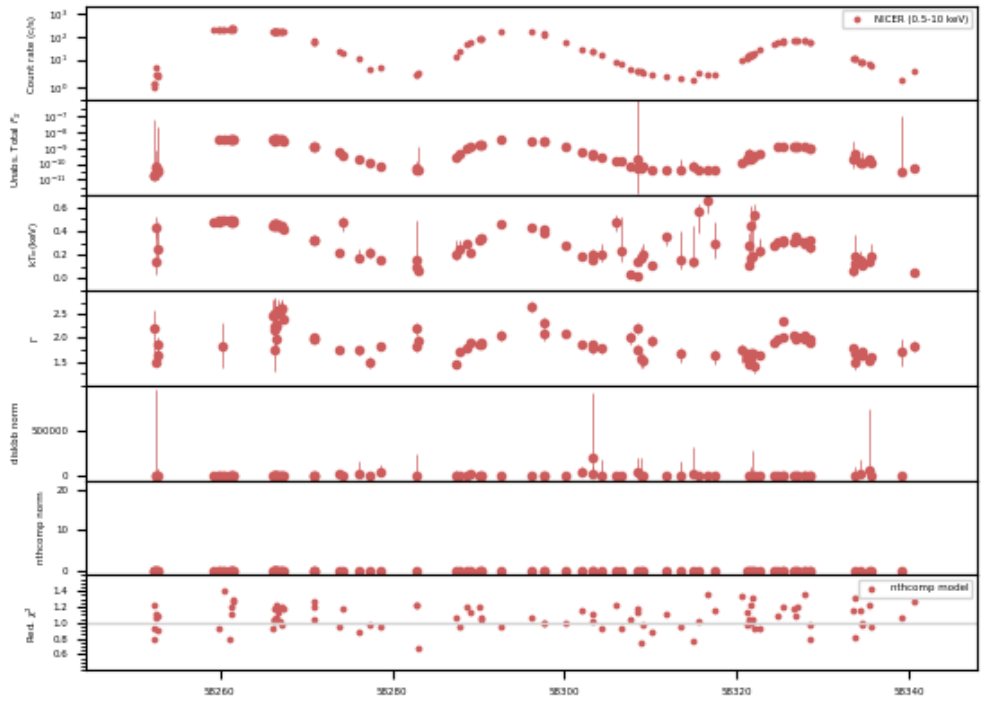
```
scaled_bg_counts = (sum(restricted_bg_counts_list)/bg_exp_time)*exp_time
count_rate = sum(restricted_data_counts_list)-scaled_bg_counts
count_rate = count_rate/exp_time
os.remove(temp_data_file)
os.remove(temp_bg_file)

out_list.append((seg_id+':'+str(count_rate)))
```

To check: I take it restriced_bg_counts_list, and analogous for the data are only using the noticed ranges? If so, all looks good!

> Oh also, a small note on my final plots. I think I found out how Cuneo prevented ytick labels from colliding:
>
> 
>
>
> I think she set the ylim bounds somewhat lesser and somewhat more than the min(y) and max(y) vals—so I did this and it improves the aesthetics significantly in my opinion.

Yes, this is a nice way to keep the plot clean. (And for x-ranges too.)

> As you requested, here's a plot with the diskbb and nthcomp norm components:
>
> 
>
>
> (Full range)



(cuneo date range)

This is great, but I do have one request: it would be helpful to have the normalizations plotted in log-scale as well (to better see their dynamic change).

> I didn't include values consistent with zero within errors in any of these plots, but I can if you'd like.

Sounds reasonable to me. Can also try plotting them with a different (less prominent) color or symbol as an alternative.

> Right now the raw outputs from my returnConfidenceIntervals() function look like this:

```
PROBLEMS  OUTPUT  DEBUG CONSOLE  TERMINAL

1130360284_1:PHLZ:0.591738
1130360284_2:0.249281:0.334173
2130360201_3:0.107522:0.107773
2130360201_4:0.257621:0.200038
2130360201_5:0.36142:0.204905
2130360202_0:PHLZ:0.116443
2130360202_1:PHLZ:0.718751
2130360203_0:0.295584:0.279007
2130360204_1:PHLZ:0.727953
```

> (where PHLZ stands for Parameter Hit Lower Zero and PHUZ stands for Parameter Hit Upper Zero)
>
> Early in the week I thought about changing the output of this function to some kind of multidimensional numpy array like this:
> `[['seg_id_1','seg_id_2'],[lower_1,lower_2],[upper_1,upper_2]]`
> But I didn't really feel like it because the current function works fast enough for me (I don't ~~see the need to vectorize~~ (more accurate statement: feel like vectorizing) all my functions yet; unless you would advise me to in which case I'll get right to it lol). *

Ha - while it can absolutely make a big difference in some cases, it sounds like from below you've found those cases where it really matters. Usually when memory juggling big arrays you want to do that once and avoid repeated calls in that vein, and otherwise small transformations of scalars there will be plenty of situations where it looks much nicer but won't make much practical impact.

> I did write a standard function for returning float values of value date, value and a two dimensional array for yerr for parameters based on an input x=the parameters output from returnConfidenceIntervals(), y=theoutput from a returnDates() function I wrote a bit ago, and z = an output

> from the returnFitResults() function I wrote a bit ago. Lol reading that last sentence, it looks a little confusing so I'll add this function to my package and document it in markdown and show it to you in a later email.
>
> I still have to iron out my hardness ratios function, will get to that soon.
>
> A random, last thought: in compsci club at school we've recently been discussing algorithmic efficiency in the context of things like sorting algorithms, which inspired me to think about this in the context of my code. So far I'd say a noticeable improvement that has resulted from this is that I'm limiting the number of computationally expensive tasks I put under for and conditional loops as long as I can execute them outside the loop. An example of this would be declaring a scipy.interpolate.interp1d object outside of a for loop rather than declaring it for every data value in a set (this came up in something else I was working on, but I brought it up because I think it's a good example).

Absolutely, this is just the kind of case you want to call once only.

> All data referenced in the plots, the python file I wrote for creating these plots, and the python file I used to write the fit commands are all attached in the email_attachments .xz tarball.
>
> Cheers (and Happy New Year)!
> Thaddaeus

Wonderful work Thaddaeus! And Happy New Year!

-Jack

> When you replace a for loop with a vectorized numpy function and see the speed improvement
>
> 

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Dec 31, 2020 at 2:38 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> It's been quite a month everywhere! Hope you had a nice holiday season and that you and your family have been doing well. Wishing you a happy and better New Year ahead.
>
> -Jack

On Wed, Dec 9, 2020 at 2:57 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> > Hey Thaddaeus,
> >
> > Those parameter plots look great! Can you add in the diskbb and compTT normalizations as panels as well?

It looks to me like things are probably going wrong because you are probably using bin-number rather than channel number. Sometimes these two get conflated, but here by bin I mean groups of one or more channels, where a channel is the thing that is hard-coded in the spectral file (and PI-bins correspond to 10 eV increments). So you'd want to use channels 50-299 and 300-999 for your two bands.

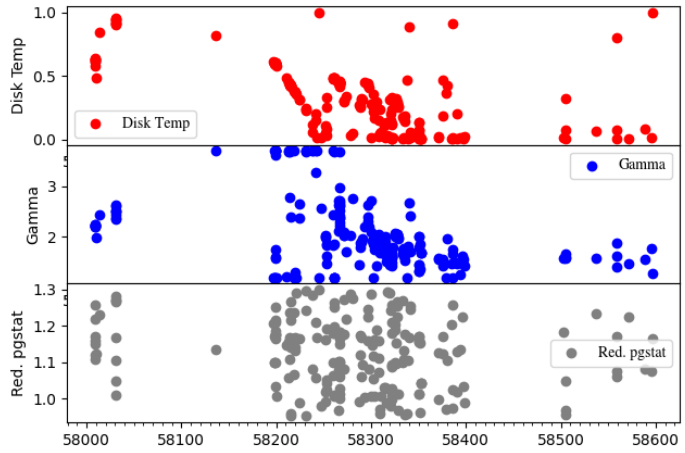
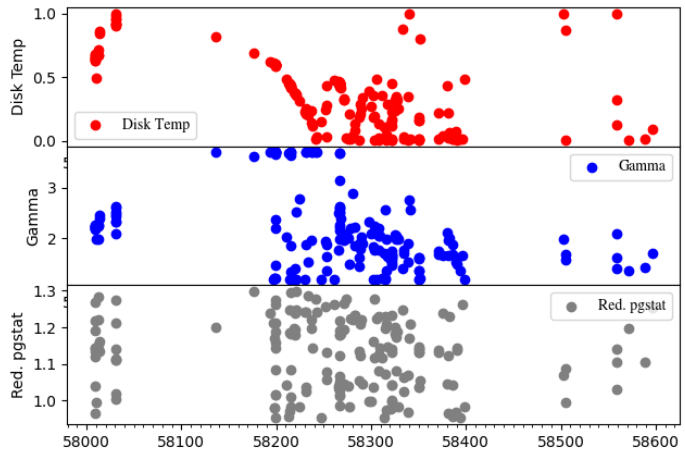
The other thing to check is that the background is scaled properly. So you want to make sure that to background you subtract is:
BG_Spectrum_MatchedToData = BG_Counts_FromBGFile / BGFile_ExpTime * DataFile_ExpTime

Hopefully my shorthand there is clear enough.

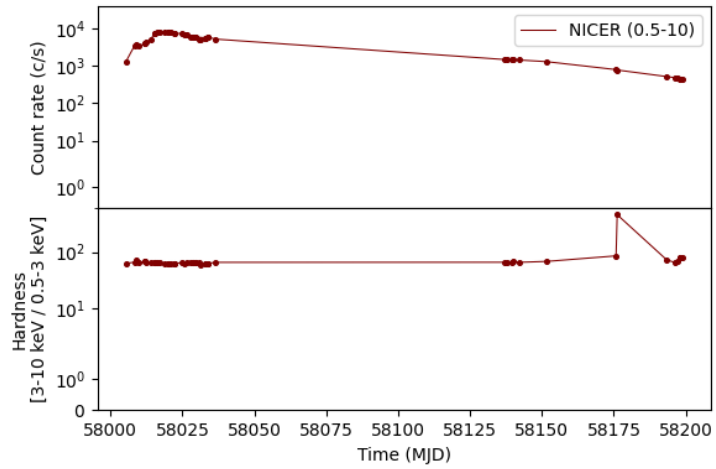
Best,

-Jack

On Wed, Dec 9, 2020 at 2:39 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi Dr. Steiner!
>
> Phew! The number of potential files for fitting has shot up to 720 from 185 now that I'm searching for and including all GTIs files.
>
> I ran a couple fitting routines with a simple tbabs(diskbb+nthcomp) model on all the files just to get a feel for how including a lot more files would possibly change things, and the initial results from these fitting routines were quite unusual (though they were blisteringly fast compared to relxill fits regardless —with nH frozen on these routines, I was fitting all the files with 700 fit iterations each in under an hour on my mom's computer) :
>
> 
>
>
> ^ That routine had diskkbb Tin upper limit set to 1, gamma upper limit set to 3.7, nH frozen to 3.2, and the entire energy range from 0.5-10.0 keV noticed.
>
> 
>
>
> ^That routine was the same except the energy interval 1.5-2.3 keV was ignored.
>
> I think I see a little pattern of correlated variation between disk temp and gamma masked under the erratic points in both plots...I'm wondering, should I set some cutoff exposure time for the files? I recall seeing some ~22 sec exp. time data files when I was looking through other GTIs, and I'm thinking since these files would have less data to constrain the model, crazy parameters can be permissibly returned by XSPEC when these files are fit.

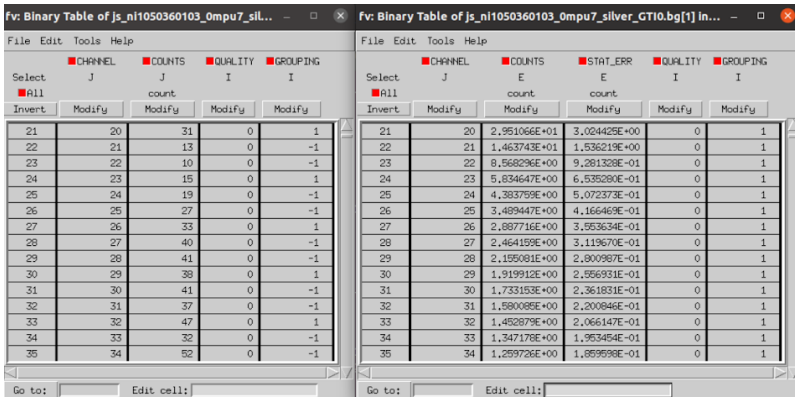
Also, could I get your advice on calculating hardness ratios? I modified my count rate function to correct for background counts and it's working pretty well (see the top subplot below):



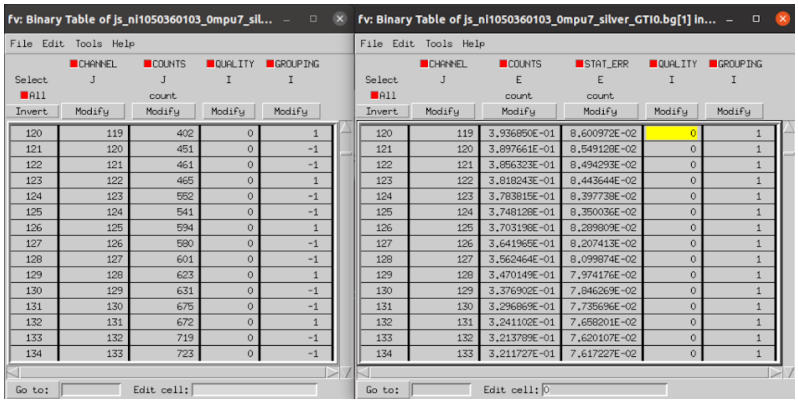
Almost identical to Cuneo's count rate plot, eh?

As you can see in the bottom subplot, the hardness ratio values are crazy, and I can't figure out how to rein them in. The way my function for hardness ratio works is I sum the number of counts in the parameter defined hard band and subtract the number of background counts in the predefined hard band from that value, giving me a net hard band counts sum. I then divide that value by (sum of soft counts-sum soft background counts). Code is attached.

I was checking with fv, and it's interesting that in this case (and what I think is true in general) the bg counts are very low in the hard band but fairly high in the thermal:



Channels 22-99 = 0.5-3 keV



Channels 101-254 = 3.0-10.0 keV

Cheers!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 3:36 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

> Thanksgiving was very nice, thanks!

Yay that's great to hear!

> Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.

Sounds good.

> YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.

!!! I will do this then!

> Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

Will do.

Thanks!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 12:28 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Thanksgiving was very nice, thanks! Glad yours with the grandparents was too.

> I reprocessed the data last weekend (11/22) but the results were a little weird so I've been tweaking some things and prepping for reprocessing again this week.
>
> I was wondering if I should take a different approach to the instrumental residuals by running a preliminary fit on all files and logging the fit statistic landscape, so I can then loop through and identify which files actually require the 1.5-2.2 keV range to be ignored and which can have it noticed (I'm wondering about this because from a preliminary check a lot of the disk temperatures were behaving weirdly, which I think could be related to having less data in the thermal band to constrain them). What do you think?

Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.

> Also, I've only been fitting the silver, zero good time interval files; would you recommend me to fit some of the GTI1s, GT2s, etc. in hopes to get more good fits?

YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.

> P.S. I wrote two new standardized functions (one calculates and returns count rates, the other calculates and returns hardness ratios) and nice documentation for all my standard functions so far.

Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

-Jack

On Mon, Nov 23, 2020 at 8:34 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
Will do!

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 23, 2020 at 4:30 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
Hi Thaddaeus,

Sounds good! Glad your mom's computer has some CPU cycles to share. Keep me posted when you get the results in hand. Meantime, happy Thanksgiving.

-Jack

On Sun, Nov 22, 2020 at 8:01 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
Hi!

> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

Yes I've been fitting with the inner radius frozen to -1. That's good to know.

> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind. So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

Sounds good.

> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Yes I agree it was nice that they showed the underlying component.

I've been running the first iterations of the reprocessing last night and today and I've had a little breakthrough—last night I quickly updated the version of XSPEC I had on the vm that I installed on my mom's computer way back in June (I also installed relxill on it), and I've been dividing the work between her computer and mine—the routines are concluding much quicker (her computer has four cores xD).

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Fri, Nov 20, 2020 at 9:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
Hi Thaddaeus,

Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind.

So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

And in response to your email yesterday:

> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

Yes, spot on!

As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Best,

-Jack

On Fri, Nov 20, 2020 at 8:13 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

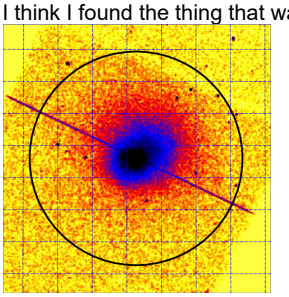
> Shoot! I forgot this bring this up. Would you recommend me to freeze the spin and inclination parameters to published values (that are in agreement with some of my previous best fits) to simplify the parameter space, or should I keep them free? The only benefit I can see from keeping them free would be sometimes error locks up with no variable parameters to use when free parameters get pegged due to their insignificance to the fit, and I think having them free could help prevent this...though spin and inclination seem like parameters that would be the first to get pegged during an error routine anyways.
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Thu, Nov 19, 2020 at 8:07 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hey!
>
> > Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.
>
> Yeah, safety is (rightly so) the first priority.
>
> > A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!
>
> Oo sounds cool, I hope you all have fun!
>
> > Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.
> Perfect.
>
> > The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon.
> Interesting.
>
> > then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

Very interesting.

I think I found the thing that was on the edge of my memory: a Chandra image of the quasar H1821+643.



So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

> Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

I should probably rephrase what I said earlier. When I ignored bad, no channels were ignored so I guess that meant no poor quality flags were raised.

> I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Agreed. I find it so enjoyable: the initial encounter with a seemingly insurmountable challenge, the brainstorming, the troubleshooting, and *especially* the successful execution of a solution. It's extremely satisfying—and to use an analogy from my youth* it feels kinda similar to when I would build things in lego.

*I say an analogy from my youth but that's somewhat misleading. I still occasionally play legos, and these days the only difference is that I have a partner: my younger brother ;).

> After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Definitely. That is exactly what I'm thinking.

> Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

:)).

Best!
Thaddaeus

Oh I almost forgot. I don't remember how/why I came across this paper (Arumugasamy et al. 2018), but after looking at the plots I've been meaning to ask you about them:

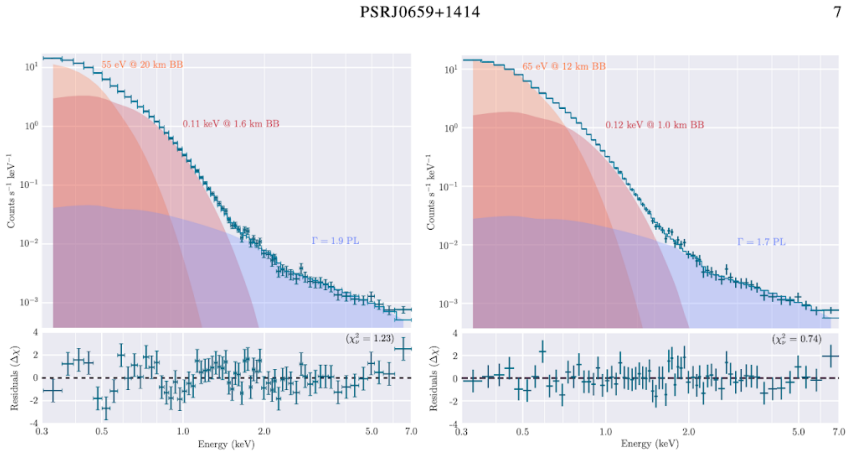


Figure 4. 2BBPL model, without (left) and with (right) `Gabs` absorption component, fit to the phase-integrated spectrum of J0659+1414. The model parameters correspond to maximum posterior probability, which in the case of G2BBPL are different from the distribution medians listed in Table 2.

I've been using the darkgrid style (like the plots above) with the rc font parameter set to Nimbus Roman in most of my plots ... but after seeing these plots I'm thinking I was to switch back to the mpl defaults—these plots are kinda distracting to me in their aesthetic.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."

-Enrico Fermi

On Wed, Nov 18, 2020 at 10:05 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> It was a wild week indeed—and things seem to keep getting crazier. We were in a hybrid model since October 19th, but as of Tuesday we went indefinitely back to all online which is a two sided coin. On one hand the online schedule gives me a lot more free time, but on the other I'm going to miss the extra class time in my harder classes like mechanics and calc. Thanksgiving is around the corner though! Up to any interesting plans yourself? I think we're just going to have my mom's parents over because we already interact with them frequently, but no big gathering this year (obviously).

No kidding! Feels like we're living in a bad TV show.

Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.

A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> Would it be okay if I emailed Dr. Cuneo about this? This is very important for how I structure the confidence intervals code (and whether I make it more sensitive and able to detect more than the four generalized abnormal scenarios described later in this email).

Sure thing, email away.

> Haha! I just read that NuStar's is ~2.5 *milliseconds.*

Yeah and that's actually not too bad compared to most things out there. NICER is just an absolute timing monster.

> I do have my pipeline correct for it automatically, so all your spectra already factor that in.
>
> Cool! Does that mean I can just go ahead and calculate count rates as number of counts / exp. time and not worry about adjusting the result because you've already accounted for deadtime?

Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

> deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.
>
> Yes. This makes sense. I feel like I've seen something related where the center of an object was too bright in the X-Ray band (which oversaturated the detector?) so the color gradient of brightness was black in the center because the author's couldn't determine the brightness at the center. It kinda looked like an artificial coronagraph. Would that be related to a situation where the dead time was more than the event time?

(Deep dive warning:)
Yes, you're speaking about "pileup" which isn't exactly related to deadtime except that both can be important when the source is bright. Deadtime, again, is from the detector shutting down for some portion of the time. Pileup concerns are most often for an imagng type of detector - more like the camera on your phone - that collects an exposure for some amount of time and then read it off.

Chandra, XMM-Newton, Swift, Suzaku, (somewhat confusingly NuStar is in both camps), let X-rays come in from the sky, get focused onto the CCD camera, and then in some time interval, often ~1s, reads off the image and clears the CCD. The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon. So when an individual pixel gets << 1 X-ray per frame on average, you are in good shape. *But* when there is significant probability of getting >~1 X-ray per frame on a given pixel, then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

This registering of multiple X-rays incorrectly as one more energetic single event is pileup, and it is the bane of most imaging detectors for those of us working with bright sources. (Pileup can occur in instruments like NICER too, when two X-rays hit during the event-latch phase, but this is something like a <1% effect for a 100 Crab-bright source, so well in the camp of being negligible for all practical purposes.)

In the most extreme cases of pileup, the pixel digitizer, which usually records values on something like a 16-bit value (e.g., from -32768:32767), can roll over and then appears negative when too much charge hits at once.

> I'm pretty sure the RXTE data I was working with earlier this year didn't have this identifier because if I remember correctly I couldn't execute *$ignore bad* on the data in XSPEC.

Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

> Here's a working version of the confidence interval / error results script:
>
> I put my XSPECtools.py file which has all of these functions into a custom module in my local site-packages folder for python and it seems like it's working perfectly. I import the file like this: *from personalastropy.xspectools import XSPECtools as xt*, and I execute a particular function like this: *xt.returnFitResults()*.
>
> Happy early Thanksgiving!
> Thaddaeus

That's really great Thaddaeus! I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Very well done!

After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

Best,

-Jack

> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Wed, Nov 11, 2020 at 12:07 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hey Thaddaeus,
>
> What a week it's been eh? (But way to go PA!)
>
> > I've held off from running another major fitting routine since the weekend because after I ran that my last one I decided that it was time to write some standardized scripts for wrangling the results from all the log files.
>
> Excellent!
>
> > Speaking of which, when Cuneo uses green triangles for "*parameters consistent with 0 within errors*", does this refer to situations when either the upper value of a parameter is equal to zero (because it got pegged at the hard upper limit) or when the lower limit is invalid and equal to zero?
>
> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> I *love* the outline of your routines, and "wow" is what I have to say about that. Way to go Thaddaeus, it's great you're tackling this and in a very useful manner.

> Out of curiosity, where did Cuneo get the count rate values for his data? I don't think it's the same as the Net Count Rate that's displayed whenever you execute *$show data* in XSPEC....can Cuneo's count rate (c/s) be found by dividing the sum of the counts in the included channels by the total observation time?

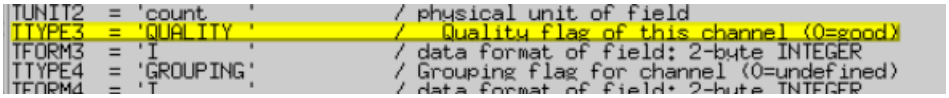
About 99% yes. "Usual" practice for NICER has become to normalize to an active set of 52 detectors. Your spectrum is a collection of all good events. So a tally of all the (good) events between 0.5-10 keV divided by the exposure time is indeed the count rate detected. However, if only using 49 or 50 detectors (also common), one would scale up by 52/49 to show the rate on the full instrument.

The one very tiny wrinkle to all this is a property called "deadtime". Each time the electronics on a detector register an X-ray event, they "latch" meaning that the detector stops listening for new signals. Effectively, the detector is turned off for a short amount of time it uses to calculate properties of event (what time it arrived, and how much charge was deposited). For NICER, this is typically ~20 microseconds.

As a concrete example of what this means in practice: when the count rate per detector reaches ~10^3/s (or in other words the full instrument light curve was at ~50,000/s), there would be ~20e-6 * 1e3 = 2% deadtime. This would mean that the *actual* count rate is ~2% higher than was detected (because the true exposure time was actually 98% the clock time). Here, most likely Cuneo didn't worry about folding in deadtime effects (I do have my pipeline correct for it automatically, so all your spectra already factor that in).

This is obviously a very small here for NICER, but I wanted to dive into it because for many missions that aren't timing beasts like NICER, deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.

> Also sorta random, I came across a value called "quality" when I was looking at a file in fv; are the quality values the values XSPEC refers to when I say *$ignore bad*?



Yes exactly this. This won't tend to come up for NICER, but e.g., for instruments that have some bad pixels and a spatially dispersed spectrum (think the rainbow from a prism), or uncalibrated energy ranges, this can be used.

> * This is probably one of my favorite one-liner functions in Python:
>
> for line in f:
> linelist = (re.sub(' +', ',', line)).split(',')
>
> Where f is a file opened with: *with open(...) as f*.

Python is a wonder for its very efficient one-lining loops!

As usual, you're knocking it out of the park Thaddaeus.

In other news, a new grad student (Santi) just joined me and he's going to start looking at some QPO data.

Best,

-Jack

> On Thu, Oct 29, 2020 at 12:49 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> > Hi Thaddaeus,
> >
> > Whoops - Santiago is my grad student and I notice my advisor brain accidentally called you Santi in my last message. Very sorry about that!
> >
> > Name confusions aside, very glad the patch worked ok. Yeah, setting query to yes can cause problems sometimes as you found. I don't have good rules for avoiding or escaping those runaways, but I've had luck before tweaking the extra convergence terms in the fit command when it's stuck (the delta & beta) and the tolerance term in error.
> >
> > -Jack

On Thu, Oct 29, 2020 at 11:34 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi Dr. Steiner!

Alright I think that patching did the trick; I'm currently running XSPEC v 12.11.0m. Surprisingly I didn't have to rebuild relxill which was nice. Now that the 12.11.0k patch has been installed, I think XSPEC just "snags" and keeps fitting if I have query set to yes instead of crashing—when I was looking into this, a particular observation ran an error routine for three days nonstop in the background until I aborted it!

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 22, 2020 at 12:46 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Santi,

OK, that's encouraging! Let me know if the patching does the trick. Often for small patches, you won't need to recompile local models, but you should quickly see if it has trouble loading and that would be your sign.

It's hard to predict how parallel processing will impact a given task. The outcome depends on where the bottlenecks occur (if processing, then expect significant improvement, if memory swapping, then probably not). For running on multiple threads, a **rough** estimate is sqrt(N) gain, but that's just ballpark.

-Jack

On Thu, Oct 22, 2020 at 11:56 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

I just got some really great news on the crashes!

Last weekend I was dismayed because even though I was playing with the limits extensively (especially for gamma), the crashing was still occurring unabated. This made me wonder if it was a software issue, so I reached out to the HEASARC Help Desk. I found out that a very similar issue was fixed in XSPEC patch 12.11.0k: *For some complex fitting and error command cases, XSPEC may terminate with a GSL message such as 'SVD decomposition failed to converge'.* This wasn't encouraging at first because XSPEC patch 12.11.0k came out in June (I built the current version of xspec on my vm in ~July), but I checked my XSPEC version with *$version,* and it turns out that my XSPEC version is just 12.11.0! I'm working on installing the patch, I bet this will clear a lot of things up. I think I'm going to have to rebuild relxill after I execute the partial system rebuild.

The only thing that confuses me about this is that I didn't really have issues with core dumping when I was fitting relxill a lot back in August (I think it did come up for error fits back then though), so I'm confused why it really started acting up recently.

Also I don't think I've asked this previously, but out of curiosity, should I expect significant reductions in fit times with two core parallel processing?

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 22, 2020 at 10:52 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Just checking in to see how fits are progressing and if playing with those limits has allowed any headway against the crashes?

(Yes, whenever I "rm -rf " I try to take beat and check that I am not about to bilk the whole machine! With great commands-line tools comes great ...)

-Jack

On Fri, Oct 16, 2020 at 3:24 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> Now that I think about it, I do recall noticing that many (if not most) of the fit failures had gammas in the 3.4 range. Good to know about the 1.6-2.2 range for the residuals, I'll play with that now.
>
> Oh about the attachments: the screen shot from visual studio is just some descriptions I was writing about the different functions I was using, and the microsoft paint one just illustrates the positive contribution that is almost always eliminated by a ~1.75 KeV gaussian of varying width.
>
> Over this weekend should I continue to focus on maximizing the number of observations that I can get good fits and confidence intervals for?
>
> I'm intrigued by this disk truncation controversy, I'm going to read about it this weekend.
>
> Many thanks!
> Thaddaeus
>
> P.S. I was reading about some dangerous linux commands the other day; there are some unnerving commands out there. Imagine if someone ran rm -rf in your terminal. Shudder.
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Thu, Oct 15, 2020 at 10:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> > Regarding the application of systematic errors, that's good to know. I've often seen something like the "NICER team recommends the application of a systematic error of 1%" in papers that use NICER data.
>
> Yes, that's a "rough" benchmark, but a reasonable one.
>
> > I love the common.tcl file, it's so handy! I think I'm just going to use it from now on because it's simpler on the execution end (just two commands), and it shows the error strings for each parameter.
>
> Yes, it's quite nice work by Jeremy :).
>
> > A little random: I came across an interesting statement in a paper by Plant et al. 2015 pertaining to observed disk truncation in the hard state of GX 339−4: "This result implies that the current sample of spin estimates in the hard state are inaccurate. Therefore, any distinct conclusions drawn from these estimates, such as the spin-powering of relativistic jets, may well be biased." Since that paper was published has this issue been fully exploited and resolved, or are there still black holes with possibly incorrect spin measurements out there? Also, is physical disk truncation or changing coronal height the more widely accepted cause for profiles similar to disk truncation?
>
> This has been a topic with a lot of controversy that is still ongoing (Garcia et al 2015 and followup papers by Wang et al. 2019 talk about this). I'd say the Plant picture is strongly disfavored at present, but not entirely out. The robust view is that allowing for the possibility of truncation, a (bright) hard-state spin measurement should be regarded as an estimated

lower limit. Presently, the argument is focused around how much truncation could there be in bright hard states, with most reasonable numbers being a few times the ISCO (a factor <10). (Coronal geometry is always an important consideration, but usually they tend to fit very compact.)

> An update on the rsp/arf issue: I modified my approach and fully integrated astropy.io.fits, so when I'm producing my xspec script commands rmf and arf file names are pulled directly from the header of each observation's fits file. Because of the two errors described below, I haven't checked how chi-square improves when the correct rmf and arf files are used though.

Very good, that should make some impact when working.

> I've encountered two weird errors I've never encountered before in XSPEC this week: the first is frequent core dumping [SVD decomposition failed to converge...Default GSL error handler invoked...Segmentation fault (core dumped)], and the second is this odd "XSPEC Error: No variable parameters for fit" after executing "fit" for a model with *multiple* free parameters (including the usual Gamma, FracSctr, Tin, diskbb norm, etc.). My first thought was "*is this related to relxill having problems due to a low gamma"*—but these issues both happen even when Gamma is > 2.

The table can be sparse in a few places, and I think very large Gammas are also sparse (>~3.4, if memory serves). I'd say try freezing parameters and retrying the fit to identify the culprit parameter.

> Also a little random: this week I decided it was time to start commenting on my functions in my python files—at one point I was looking through some older files for a particular technique I used and it took a little while to find the correct version of it, so I decided that from now on I'm describing my functions for future reference.

Yes, documenting code is super important and a great habit to get used to. (I wish I was better about it!)

> This is preliminary, but I attached an example of a chi-scape scarred by a common morph of the residual I last sent you. The positive contribution that begins shortly after 1.6 KeV is almost always eliminated by a ~1.75 KeV gaussian of varying width for most observations. The current issue is modeling the various forms the following negative residuals take. Currently working on those. Earlier you mentioned that we could try ignoring that narrow range where these residuals occur, would ignoring channels between 1.6-2.2 KeV be ignoring too much of the data? If we decide to ignore that range, I would think it best to get >*plt wdata* outputs to exactly nail down the dimensions of this range, but for now should I keep trying to model the residuals?
>
> I was reading through some older emails, going to look into your recommendation regarding logxi.

No, I think 1.6-2.2 isn't crazy and you can certainly roll with that if you are having too much trouble taming the residuals with a model. Maybe do a couple test cases to see how the model parameters are affected on both fronts.

Hey, and I didn't see a description of your attachments in case you want to say anything about those.

Glad you're back in the thick of things.

Best,

-Jack

On Thu, Oct 8, 2020 at 1:35 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi Dr. Steiner!
>
> Quick note: I've been getting back into it this week and should have some interesting things to report this weekend!
>
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Tue, Sep 29, 2020 at 2:19 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Thanks for the update.
>
> > Quick note on the residuals: since we last communicated I investigated the application of a systematic error (3-5%) in the < 2 KeV energy band, but this didn't improve the residuals so I'm assuming that you already accounted for this, like the binning, in your reprocessing?
>
> No, actually. But systematic errors are only incorporated with some fit statistics - chisq for instance, but I don't believe are incorporated in cstat or pgstat. The way I like to think about it is the systematic error acts as a reweighting of the data's constraint and serves to de-emphasize regions with the highest signal. I offer for information sake that that 3-5% is too large a systematic error to include as a rule. Something like 1-2% is more reasonable for a region with poorer calibration quality.
>
> > I also discovered that I've been unintentionally forcing an incorrect response file on multiple MAXI J1535-571 observations—the rsp I was using for all observations was `nicer_d49_55575341.rmf`, but after running a quick check (the fitsio package has been so helpful, thanks for recommending it to me!), I found that multiple observations request `nicer_d49_u100_150_55575341.rmf` in their headers.
>
> Ah! That's very good to know, and may be the culprit for some of the residuals you've been battling. Check that the arf files (keyword ANCRFILE) is also set correctly.
>
> > I'm likely almost done with the SAT (took it Saturday, going to take it again this coming Saturday)*, and once I'm done I aim to return to a progress pace similar to the pace we had in the summer.
>
> Congratulations - that's a nice milestone!
>
> > G'mar chatima tova (if you observe Yom Kippur)!
>
> Same to you.
>
> > *I might take it again in November in the event that I bungle those tests.
>
> I'd put dollars to donuts you did great!
>
> My best,
>
> -Jack

On Tue, Sep 22, 2020 at 1:21 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Interesting I'll look into that!
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi
>
> On Tue, Sep 22, 2020 at 1:15 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
>
> > Similar to that. I used another language (IDL) to automatically produce a series of tcl/tk scripts and then executed those distributed over a number of cores on a workstation.
> >
> > -Jack
> >
> > On Tue, Sep 22, 2020 at 1:13 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> >
> > > Sounds good!
> > >
> > > In that case did you operate solely in TCL?
> > >
> > > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > > -Enrico Fermi
> > >
> > > On Tue, Sep 22, 2020 at 1:10 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> > >
> > > > Hi Thaddaeus,
> > > >
> > > > > For >75% I meant observations where source flux was more than 75% bg flux, and for 4% I meant observations where source flux was ~4 % bg flux. That makes sense—interestingly the observations with source flux ~4% bg flux were, by best fit, in the SPL state, with the highest flux and gamma for all the observations that had reasonable values for these parameters.
> > > >
> > > > Ah, now I see. Yes, when the bg flux is very high, the noise level (N=sqrt(B+S)) where B and S are background and signal counts in a channel, then the constraining power of the data (S/N) is lowest. If the background is reasonably estimated, the fits will likely be good but poorly constrained; if the background is off, then all bets are off.
> > > >
> > > > > xD. I think it's a very impressive feat! Did you use pyxspec to loop through all the observations?
> > > >
> > > > Thanks. I used a different language actually, but python/pyxspec would be an equally good way to do it.
> > > >
> > > > Best,
> > > >
> > > > -Jack

On Tue, Sep 22, 2020 at 12:39 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> That's good to know. I guess what's surprising is some relative of this large residual in the 1.6-2.0 KeV is present in most of the source flux > 75% observations (I wonder if the peak feature falsely inflates the source flux for some of them?). Going back to a couple emails ago, I was checking some of my all time best fits which had source fluxes around ~4%—what allows such faint observations to fit well?

Just to clarify, do you mean 75% and 4% of the Eddington limit, peak luminosity, or something else? Fainter observations, given some typical observation time, obviously have fewer counts which means larger fractional errors and less constraining power on the model. So if there is a deficiency in the calibration or model being used, it will look more prominent in brighter + long exposure time data.

> Also, I'm very intrigued by your methods in one of your 2016 papers , "STRONGER REFLECTION FROM BLACK HOLE ACCRETION DISKS IN SOFT X-RAY STATES". How did you automate fitting the 15,000 observations reliably and within a reasonable time frame?

A reasonable time frame is relative :), but I used about two dozen cores to run the fits which helped. I also stuck with computationally simplistic models, and for my runs with simpl, used lower resolution (via energies) in the run, since RXTE/PCA - the instrument used there - also has rather poor energy resolution compared to NICER.

> Could I possibly have the ~86 observations between obsids 1200220101 – 1200220186?

Spectral files are attached. Response files are too big for me to attach, so I'll keep them up here for a few days: https://nicer-web.mit.edu/nicerview/thadd_resp_m1727.tar.gz

> Ah that makes sense. In that case did Xu et al. just freeze Rin at the ISCO to have less free parameters?

That's the usual tactic, yes, when fitting for spin.

Best,

-Jack

> On Thu, Sep 17, 2020 at 11:00 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
>
> Hi Thaddaeus,
>
> Great to hear you've been digging in and exploring! I would suggest though that you avoid being too concerned about the residual features. If something is localized and only ~percent level deviation, then that's likely sufficient. *The goal should be to fit those sufficiently that they don't alter the continuum, but at some level it becomes diminishing returns to try fitting an artefact.* That said, if you wish, I can get you the data on MAXI J1727-203. Just let me know if you have a particular ObsID from Kevin's paper you want to try.
>
> Your question about degeneracy is great. The condition of parameters being strongly correlated with one another is degeneracy. Basically, this is a very common situation where

two parameters are dependent upon one another to some extent, so a probable value of one will condition a probable range of values with the other. For instance, if you want to fit x and y with the constraint x^2+y^2 = 4+/-1, you would get a ring of radius 2 with x,y values that obviously depend on one another; they are *not independent*. Speaking generally for more realistic cases, the interaction between parameters is often pretty convoluted, but commonly things like Gamma and Normalization are correlated with one another, so a contour plot of the confidence interval would look banana shaped rather than like a circle.

Best,

-Jack

On Thu, Sep 17, 2020 at 9:18 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> Thanks for checking in! Since we last spoke I've been taking a deeper look into a couple articles (Alabarta et al. 2020, Xu et al. 2018, and Parikh et al. 2019) and playing around a bit with the residual fix you suggested. As tested on multiple observations with varying fit statistic contributions in the 1.6-1.8 KeV range (chi-squared contribs. ranging between 500-3000), the gaussian centered around 1.75 KeV combined with the "edge" works great!
>
> Most importantly, I found a method to more accurately investigate the residual features: *plt>wdata*. Now that I can actually see what the chi-sq contrib. is for every point, I've been having a lot more success in approaching residuals in the 1.6-2.2 KeV range, especially when the residual patterns after 1.8 KeV differ greatly from what can quickly be ameliorated with the edge feature (it seems that for the majority of observations the <1.8 KeV excess is easily subdued by the gaussian fix, but after 1.8 KeV things often get complicated, especially when a third feature shows up between 1.86 to 2.0 KeV. This third feature resembles a sort of gaussian-like crevice about 0.13 KeV in width and often reaches a chi^2 contribution of -5331 (+) at its minima, pushing the "edge" feature out to a threshold energy of ~2+ KeV). I haven't found a definitive solution yet, but a gaussian absorption with an energy of ~1.86 KeV and a width of ~0.14 KeV is looking promising.
>
> While I've been working with these MAXI J1535-571 residuals I've been wondering if the residuals Alabarta et al. discuss in Nicer data for MAXI J1727−203 are of similar shape—is there any chance I could procure some pipeline data from you on MAXI J1727-203 to look into this (if it's not an inconvenience for you and or you don't think it's a distraction on my part of course).
>
> Out of curiosity, what does "parameter degeneracy" mean? My first guess would be this example: if I only told you what the slope of a line was (e.g. '2'), and then asked you for the individual values of Δy and Δx—would Δy and Δx be degenerate? Example of where I came across this: (from Xu et al. 2018) "The inner disk radius, R_{in}, and the black hole spin parameter, a*, are degenerate, as they both control the effective inner accretion disk radius. Therefore, for simplicity, during the spectral fitting we assume that the inner disk extends down to the ISCO by fixing R_{in} at the radius of the ISCO, and fit for the black hole spin as a free parameter."
>
> Best!
> Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

Kiker, Thaddaeus <s024622@students.lmsd.org> Fri, Jan 8, 2021 at 5:38 AM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi Dr. Steiner!

I was a little out of it this week after getting my wisdom teeth pulled, just wanted to let you know I've seen your email and I'm going to work on it this weekend.

Cheers!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Tue, Jan 5, 2021 at 5:23 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hey Thaddaeus,

Wow, that's an impressive and sweet-looking delivery!

> Thank you for your well wishes! I hope the same for you and your family as well!

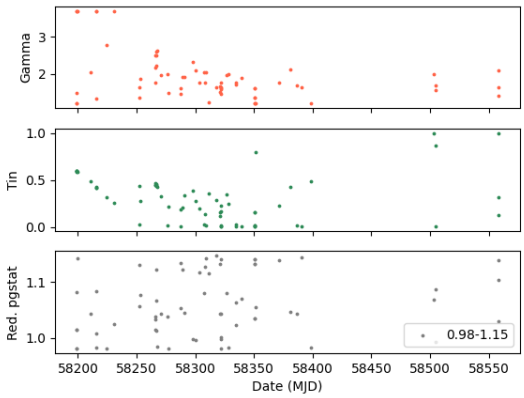
Thanks Thaddaeus, we're doing just fine and couldn't be happier to turn the page on 2020.

> First, a thought back on the relxill models: don't nthcomp and simpl produce different values for gamma and hence they should not be mixed with each other? This thought came to me this morning when I was tidying things up. Since I was tieing relxill's gamma to simpl's, I wonder if that threw anything off. Now that I think about it more, I don't think it would catastrophically derail things, but it would still be inappropriate to tie gammas from different models right?

The default answer is that these should be identical - they both describe the shape of the Comptonized power-law component, and in the same way. For completeness, I should mention there has been some small exploration about decoupling these things with some loose physical arguments, but basically that's a stretch and I would absolutely keep them linked up as you've done.

> The first hint of a breakthrough happened on Tuesday when I realized I forgot to filter for background fraction. I looped through all the data files that contributed to the last plot I sent you and only plotted fits from files where background was < 50% source, giving me this, much neater, plot (where gamma and inner disk temp are actually somewhat correlated:

Ah, yes! Glad you caught that; keeping to the cleanest data is a very good idea, and 50% is reasonable; if things look at all funny, even down to ~20% would be a reasonable place to cut.

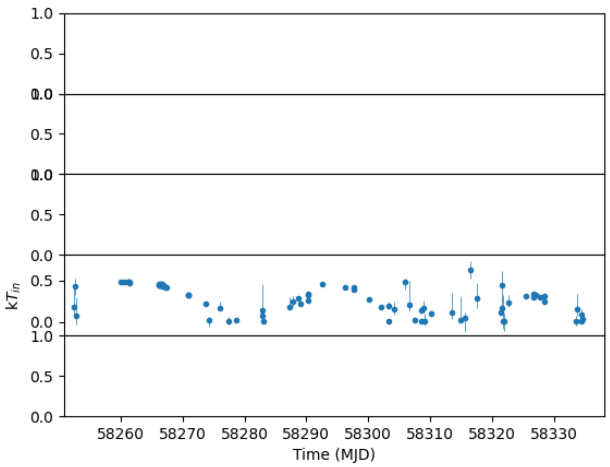


> This cleaned things up pretty well, but I noticed there were only a couple points with gamma > 3 (all of which were right on 3.5), so I ran another routine with the upper limits on gamma set to 3. The points that were right on 3.5 were on 3.0 for gamma in this routine, so I wondered if it wasn't an issue of the upper limit but an issue of insensitivity. When I tried to estimate 1-σ errors on a couple of the fits that had

> 3.5 upper limits, XSPEC returned "***Warning: Parameter pegged at hard limit: 3.5"—which I take as being caused by gamma's insensitivity to the fit in the observations.

It is possible this is because the fit isn't sensitive to Gamma. (You could check this out by comparing fit statistics to the upper-limit 3.5 vs the upper-limit 3 fits, or also looking at the f_sc values - <~ 0.01 being an indication that isn't sensitive.) But alternatively, it's also possible that the data are sensitive to Gamma and either (i) the spectrum is very soft, wanting high Gamma in which case lowering the upper-limit would still cause it to peg but the fit would be markedly worse or (ii) *my guess for most likely for these data*, the background subtraction is off in those cases and oversubtracts, creating an artificial appearance of a very soft spectrum.

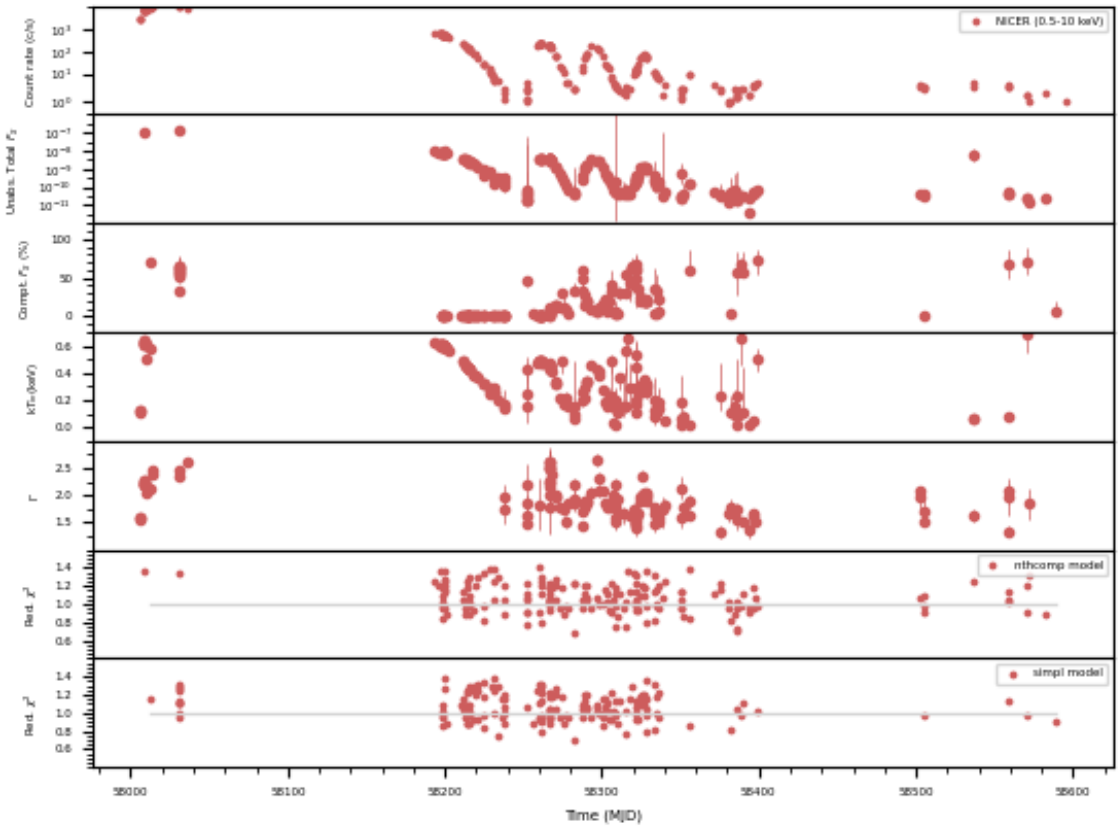
> Then I plotted Tin measurements from that fit routine with gamma's upper limit set to 3.0 with one sigma (all confidence levels I bring up are one sigma unless otherwise noted, as in Cuneo's paper; though I must say, one sigma sounds so much better than 68% confidence even though they're the same thing lol), here's how that turned out:



> Pretty nice, eh?

Yes, this looks *very nice*!

> So fast forward (and a bunch of partial fitting routines later), I'm proud to present these:

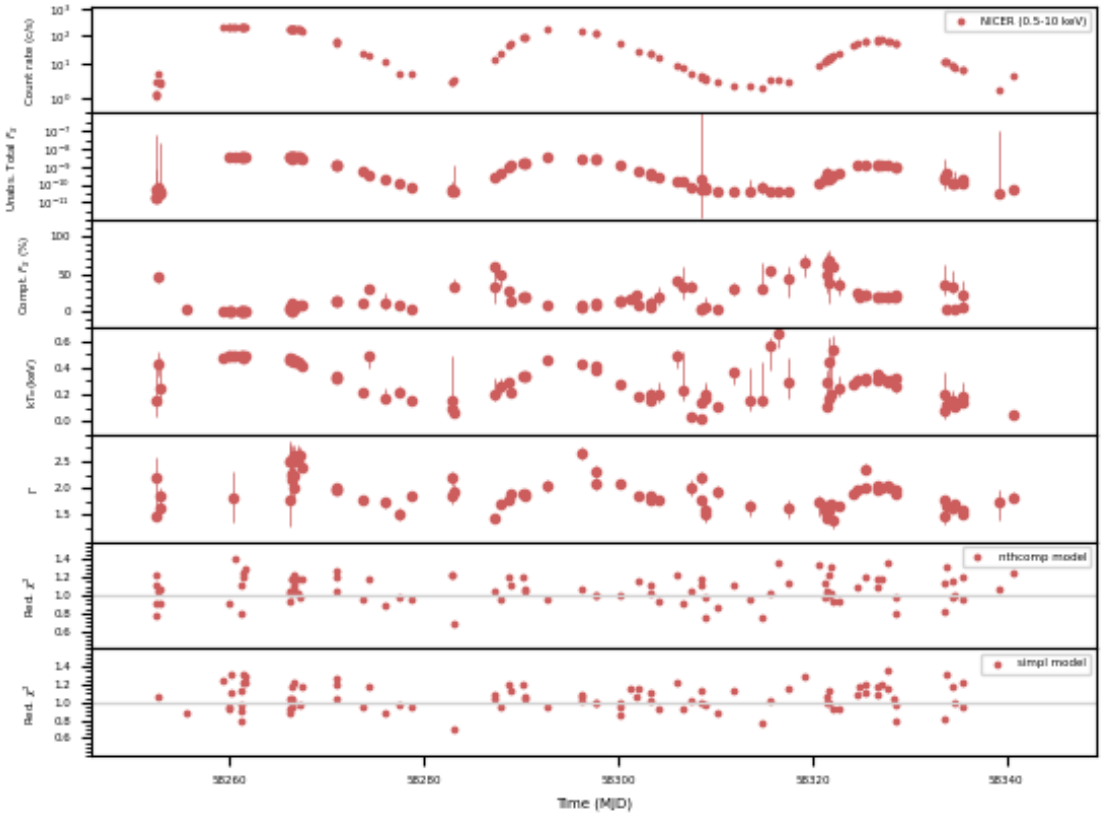


> This is my final plot for the MAXI data. The top panel is background subtracted count rate (a note on this later), the second is unabsorbed total flux on the range 0.5-10.0 keV, the third panel is comptonized flux percentage (calculated with a separate simpl model, a note on this later), the fourth inner disk temp, the fifth gamma, the sixth reduced pgstat for the nthcomp model, and the seventh the reduced pgstat for the simpl model.

Excellent - these look really great. It appears to me that there are some points shown in the simpl panel that aren't shown in the nthcomp panels, and vice-versa. Can you mention what the screening difference is? (e.g., around day 55030)

For a disk-blackbody, the expectation is that L ~ T^4, so you can plot L vs T on a log-log plot and look at that scaling.

> Here's what my plot looks like during the reflaring period investigated by Cuneo:



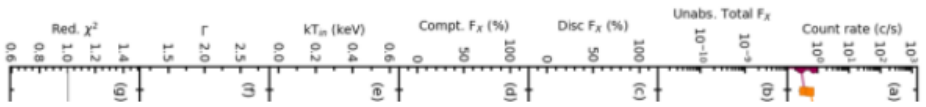
> All parameters were calculated with the nthcomp model except for comptonized fraction. Model descriptions below:
>
> ##Model One##
> Model: tbabs(diskbb+nthcomp)
> Delta fit statistic: 1.
> Noticed Ranges: 0.5-1.5 2.3-10.0 keV
> nH: 3.2 (frozen)
> Tin lower limit: 0.01
> Tin upper limit: 1.0
> Gamma lower limit: 1.2
> Gamma upper limit: 3.0
> High energy rollover: 100 keV
> inp_type: 1
> Redshift: 0
>
> ##Model Two##
> Model: tbabs(simpl(diskbb))
> Delta fit statistic: 1.
> Noticed Ranges: 0.5-1.5 2.3-10.0 keV
> nH: 3.2 (frozen)
> Tin lower limit: 0.01
> Tin upper limit: 1.0
> Gamma lower limit: 1.4 #I adopted this limit from your paper in which you used simpl to fit those 15,000-odd observations.
> Gamma upper limit: 3.4 #I adopted this limit from your paper in which you used simpl to fit those 15,000-odd observations.
> UpScOnly value: 1

Looks good.

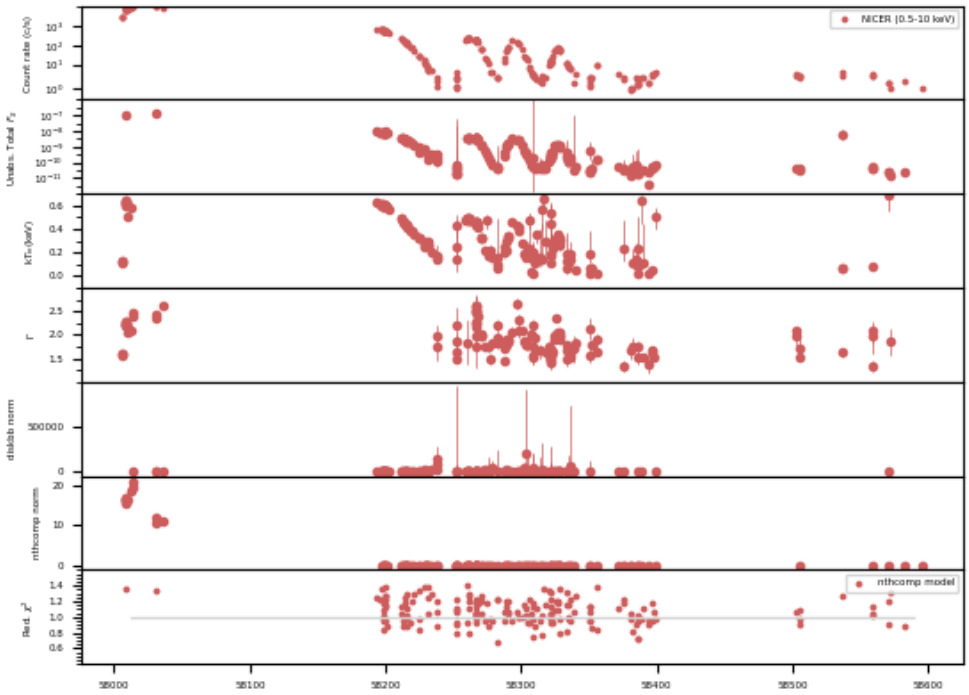
> **********
> Per the background subtracted count rates, I corrected my code to account for your pointers; the important snippet is below:

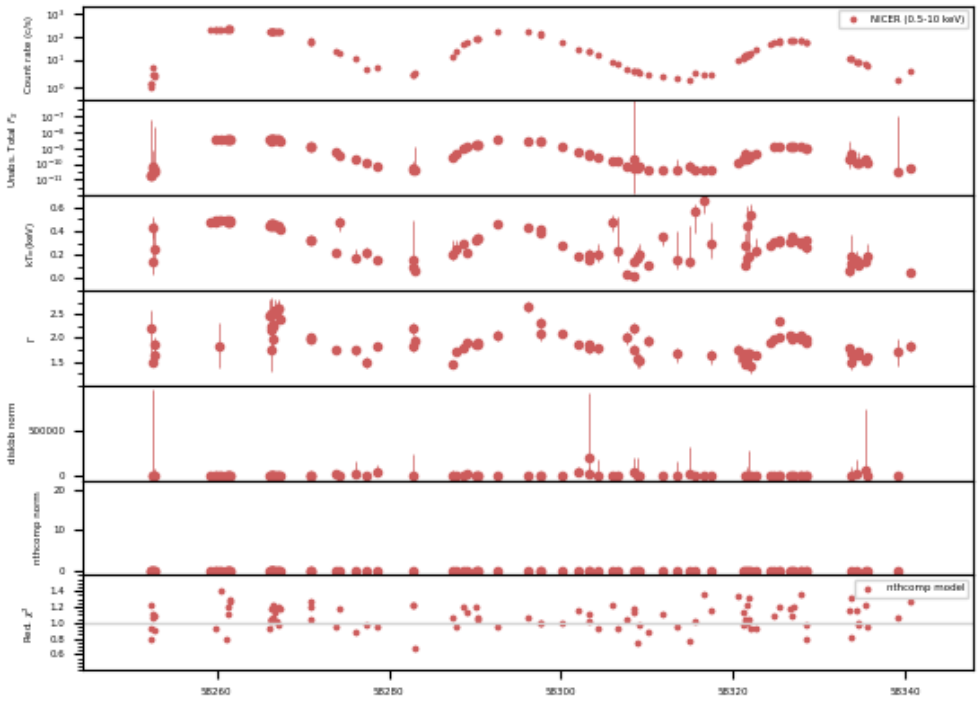
```
scaled_bg_counts = (sum(restricted_bg_counts_list)/bg_exp_time)*exp_time
count_rate = sum(restricted_data_counts_list)-scaled_bg_counts
count_rate = count_rate/exp_time
os.remove(temp_data_file)
os.remove(temp_bg_file)

out_list.append((seg_id+':'+str(count_rate)))
```

To check: I take it restriced_bg_counts_list, and analogous for the data are only using the noticed ranges? If so, all looks good!

> Oh also, a small note on my final plots. I think I found out how Cuneo prevented ytick labels from colliding:
>
> 
>
> I think she set the ylim bounds somewhat lesser and somewhat more than the min(y) and max(y) vals—so I did this and it improves the aesthetics significantly in my opinion.

Yes, this is a nice way to keep the plot clean. (And for x-ranges too.)

> As you requested, here's a plot with the diskbb and nthcomp norm components:
>
> 
>
>
> (Full range)



(cuneo date range)

This is great, but I do have one request: it would be helpful to have the normalizations plotted in log-scale as well (to better see their dynamic change).

> I didn't include values consistent with zero within errors in any of these plots, but I can if you'd like.

Sounds reasonable to me. Can also try plotting them with a different (less prominent) color or symbol as an alternative.

> Right now the raw outputs from my returnConfidenceIntervals() function look like this:
>
> PROBLEMS OUTPUT DEBUG CONSOLE TERMINAL
>
> ```
> 1130360284_1:PHLZ:0.591738
> 1130360284_2:0.249281:0.334173
> 2130360201_3:0.107522:0.107773
> 2130360201_4:0.257621:0.200038
> 2130360201_5:0.36142:0.204905
> 2130360202_0:PHLZ:0.116443
> 2130360202_1:PHLZ:0.718751
> 2130360203_0:0.295584:0.279007
> 2130360204_1:PHLZ:0.727953
> ```
>
> (where PHLZ stands for Parameter Hit Lower Zero and PHUZ stands for Parameter Hit Upper Zero)
>
> Early in the week I thought about changing the output of this function to some kind of multidimensional numpy array like this:
> `[['seg_id_1','seg_id_2'],[lower_1,lower_2],[upper_1,upper_2]]`
>
> But I didn't really feel like it because the current function works fast enough for me (I don't ~~see the need to vectorize~~ (more accurate statement: feel like vectorizing) all my functions yet; unless you would advise me to in which case I'll get right to it lol). *

Ha - while it can absolutely make a big difference in some cases, it sounds like from below you've found those cases where it really matters. Usually when memory juggling big arrays you want to do that once and avoid repeated calls in that vein, and otherwise small transformations of scalars there will be plenty of situations where it looks much nicer but won't make much practical impact.

> I did write a standard function for returning float values of value date, value and a two dimensional array for yerr for parameters based on an input x=the parameters output from returnConfidenceIntervals(), y=theoutput from a returnDates() function I wrote a bit ago, and z = an

> output from the returnFitResults() function I wrote a bit ago. Lol reading that last sentence, it looks a little confusing so I'll add this function to my package and document it in markdown and show it to you in a later email.
>
> I still have to iron out my hardness ratios function, will get to that soon.
>
> A random, last thought: in compsci club at school we've recently been discussing algorithmic efficiency in the context of things like sorting algorithms, which inspired me to think about this in the context of my code. So far I'd say a noticeable improvement that has resulted from this is that I'm limiting the number of computationally expensive tasks I put under for and conditional loops as long as I can execute them outside the loop. An example of this would be declaring a scipy.interpolate.interp1d object outside of a for loop rather than declaring it for every data value in a set (this came up in something else I was working on, but I brought it up because I think it's a good example).

Absolutely, this is just the kind of case you want to call once only.

> All data referenced in the plots, the python file I wrote for creating these plots, and the python file I used to write the fit commands are all attached in the email_attachments .xz tarball.
>
> Cheers (and Happy New Year)!
> Thaddaeus

Wonderful work Thaddaeus! And Happy New Year!

-Jack

> When you replace a for loop with a vectorized numpy function and see the speed improvement
>
> 

> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Thu, Dec 31, 2020 at 2:38 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> It's been quite a month everywhere! Hope you had a nice holiday season and that you and your family have been doing well. Wishing you a happy and better New Year ahead.
>
> -Jack
>
> On Wed, Dec 9, 2020 at 2:57 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
>
> > Hey Thaddaeus,
> >
> > Those parameter plots look great! Can you add in the diskbb and compTT normalizations as panels as well?

It looks to me like things are probably going wrong because you are probably using bin-number rather than channel number. Sometimes these two get conflated, but here by bin I mean groups of one or more channels, where a channel is the thing that is hard-coded in the spectral file (and PI-bins correspond to 10 eV increments). So you'd want to use channels 50-299 and 300-999 for your two bands.

The other thing to check is that the background is scaled properly. So you want to make sure that to background you subtract is:
BG_Spectrum_MatchedToData = BG_Counts_FromBGFile / BGFile_ExpTime * DataFile_ExpTime

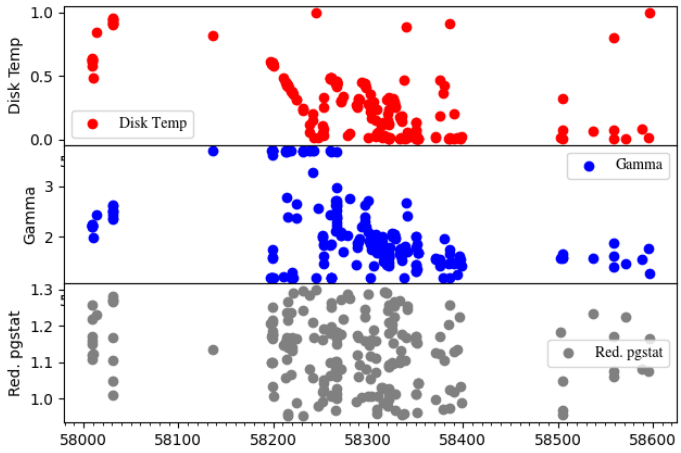
Hopefully my shorthand there is clear enough.

Best,

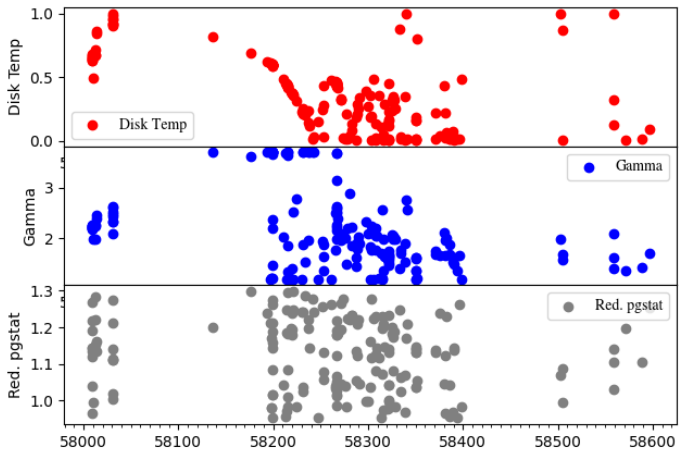
-Jack

On Wed, Dec 9, 2020 at 2:39 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi Dr. Steiner!
>
> Phew! The number of potential files for fitting has shot up to 720 from 185 now that I'm searching for and including all GTIs files.
>
> I ran a couple fitting routines with a simple tbabs(diskbb+nthcomp) model on all the files just to get a feel for how including a lot more files would possibly change things, and the initial results from these fitting routines were quite unusual (though they were blisteringly fast compared to relxill fits regardless —with nH frozen on these routines, I was fitting all the files with 700 fit iterations each in under an hour on my mom's computer) :

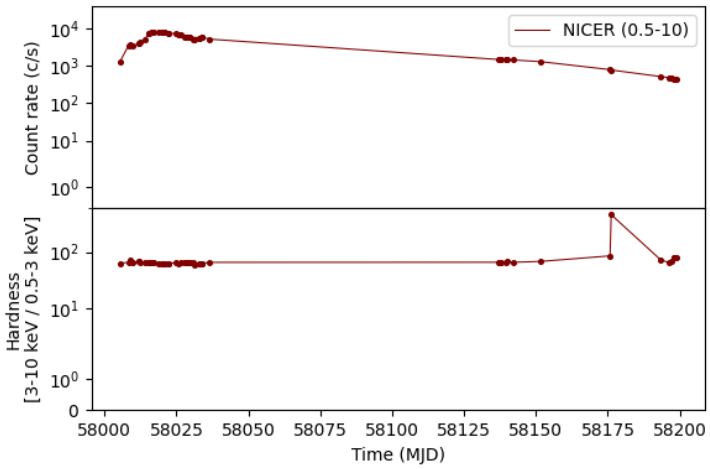


> ^ That routine had diskkbb Tin upper limit set to 1, gamma upper limit set to 3.7, nH frozen to 3.2, and the entire energy range from 0.5-10.0 keV noticed.



> ^That routine was the same except the energy interval 1.5-2.3 keV was ignored.
>
> I think I see a little pattern of correlated variation between disk temp and gamma masked under the erratic points in both plots...I'm wondering, should I set some cutoff exposure time for the files? I recall seeing some ~22 sec exp. time data files when I was looking through other GTIs, and I'm thinking since these files would have less data to constrain the model, crazy parameters can be permissibly returned by XSPEC when these files are fit.

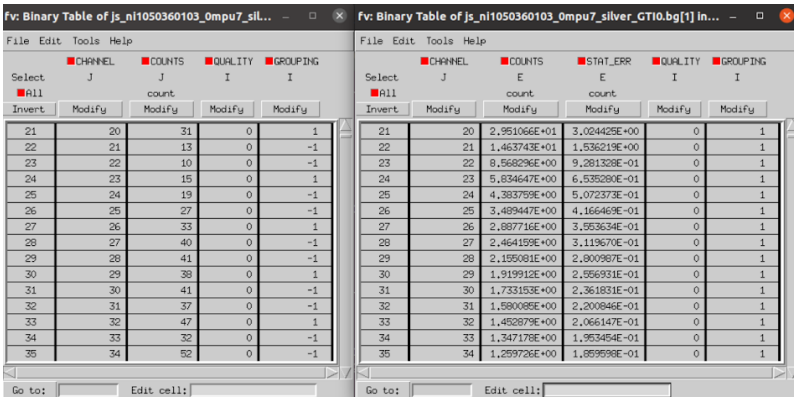
Also, could I get your advice on calculating hardness ratios? I modified my count rate function to correct for background counts and it's working pretty well (see the top subplot below):



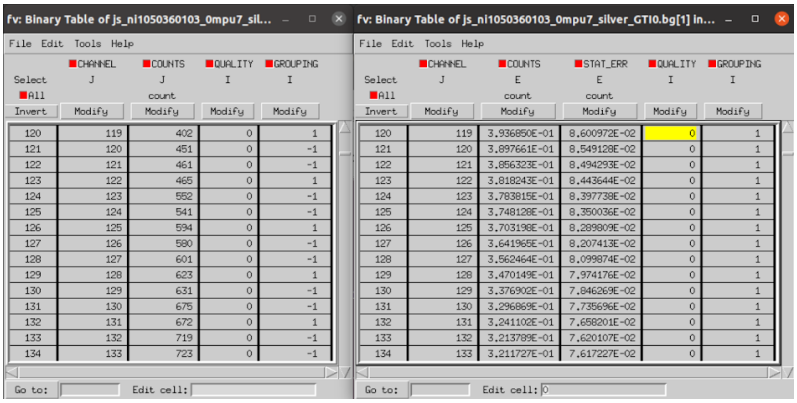
Almost identical to Cuneo's count rate plot, eh?

As you can see in the bottom subplot, the hardness ratio values are crazy, and I can't figure out how to rein them in. The way my function for hardness ratio works is I sum the number of counts in the parameter defined hard band and subtract the number of background counts in the predefined hard band from that value, giving me a net hard band counts sum. I then divide that value by (sum of soft counts-sum soft background counts). Code is attached.

I was checking with fv, and it's interesting that in this case (and what I think is true in general) the bg counts are very low in the hard band but fairly high in the thermal:



Channels 22-99 = 0.5-3 keV



Channels 101-254 = 3.0-10.0 keV

Cheers!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 3:36 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

> Thanksgiving was very nice, thanks!

Yay that's great to hear!

> Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.

Sounds good.

> YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.

!!! I will do this then!

> Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

Will do.

Thanks!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 12:28 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Thanksgiving was very nice, thanks! Glad yours with the grandparents was too.

> I reprocessed the data last weekend (11/22) but the results were a little weird so I've been tweaking some things and prepping for reprocessing again this week.
>
> I was wondering if I should take a different approach to the instrumental residuals by running a preliminary fit on all files and logging the fit statistic landscape, so I can then loop through and identify which files actually require the 1.5-2.2 keV range to be ignored and which can have it noticed (I'm wondering about this because from a preliminary check a lot of the disk temperatures were behaving weirdly, which I think could be related to having less data in the thermal band to constrain them). What do you think?

Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.

> Also, I've only been fitting the silver, zero good time interval files; would you recommend me to fit some of the GTI1s, GT2s, etc. in hopes to get more good fits?

YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.

> P.S. I wrote two new standardized functions (one calculates and returns count rates, the other calculates and returns hardness ratios) and nice documentation for all my standard functions so far.

Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

-Jack

On Mon, Nov 23, 2020 at 8:34 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
Will do!

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 23, 2020 at 4:30 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
Hi Thaddaeus,

Sounds good! Glad your mom's computer has some CPU cycles to share. Keep me posted when you get the results in hand. Meantime, happy Thanksgiving.

-Jack

On Sun, Nov 22, 2020 at 8:01 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
Hi!

> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

Yes I've been fitting with the inner radius frozen to -1. That's good to know.

> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind. So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

Sounds good.

> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Yes I agree it was nice that they showed the underlying component.

I've been running the first iterations of the reprocessing last night and today and I've had a little breakthrough—last night I quickly updated the version of XSPEC I had on the vm that I installed on my mom's computer way back in June (I also installed relxill on it), and I've been dividing the work between her computer and mine—the routines are concluding much quicker (her computer has four cores xD).

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Fri, Nov 20, 2020 at 9:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
Hi Thaddaeus,

Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind.

So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

And in response to your email yesterday:

> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

Yes, spot on!

As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Best,

-Jack

On Fri, Nov 20, 2020 at 8:13 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Shoot! I forgot this bring this up. Would you recommend me to freeze the spin and inclination parameters to published values (that are in agreement with some of my previous best fits) to simplify the parameter space, or should I keep them free? The only benefit I can see from keeping them free would be sometimes error locks up with no variable parameters to use when free parameters get pegged due to their insignificance to the fit, and I think having them free could help prevent this...though spin and inclination seem like parameters that would be the first to get pegged during an error routine anyways.
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Thu, Nov 19, 2020 at 8:07 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hey!
>
> > Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.
>
> Yeah, safety is (rightly so) the first priority.
>
> > A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!
>
> Oo sounds cool, I hope you all have fun!
>
> > Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.
> Perfect.
>
> > The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon.
> Interesting.

> then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

Very interesting.

I think I found the thing that was on the edge of my memory: a Chandra image of the quasar H1821+643.

So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

> Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

I should probably rephrase what I said earlier. When I ignored bad, no channels were ignored so I guess that meant no poor quality flags were raised.

> I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Agreed. I find it so enjoyable: the initial encounter with a seemingly insurmountable challenge, the brainstorming, the troubleshooting, and *especially* the successful execution of a solution. It's extremely satisfying—and to use an analogy from my youth* it feels kinda similar to when I would build things in lego.

*I say an analogy from my youth but that's somewhat misleading. I still occasionally play legos, and these days the only difference is that I have a partner: my younger brother ;).

> After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Definitely. That is exactly what I'm thinking.

> Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

:)).

Best!
Thaddaeus

Oh I almost forgot. I don't remember how/why I came across this paper (Arumugasamy et al. 2018), but after looking at the plots I've been meaning to ask you about them:

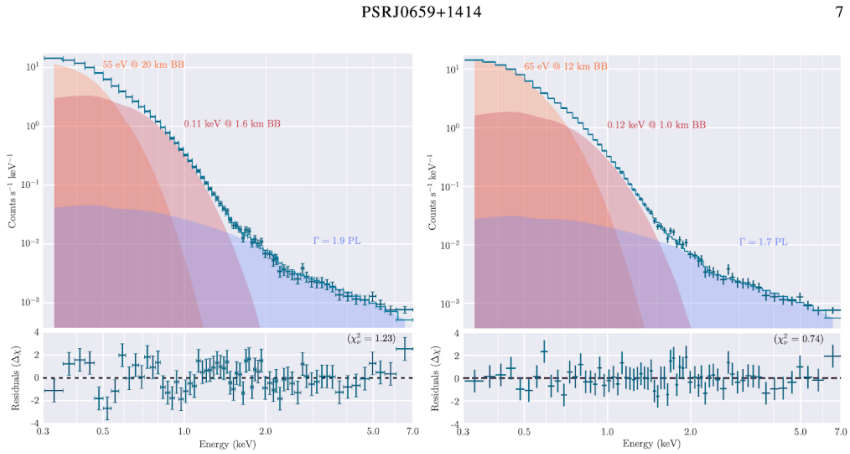


Figure 4. 2BBPL model, without (left) and with (right) `Gabs` absorption component, fit to the phase-integrated spectrum of J0659+1414. The model parameters correspond to maximum posterior probability, which in the case of G2BBPL are different from the distribution medians listed in Table 2.

I've been using the darkgrid style (like the plots above) with the rc font parameter set to Nimbus Roman in most of my plots ... but after seeing these plots I'm thinking I was to switch back to the mpl defaults—these plots are kinda distracting to me in their aesthetic.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Wed, Nov 18, 2020 at 10:05 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
Hi Thaddaeus,

> It was a wild week indeed—and things seem to keep getting crazier. We were in a hybrid model since October 19th, but as of Tuesday we went indefinitely back to all online which is a two sided coin. On one hand the online schedule gives me a lot more free time, but on the other I'm going to miss the extra class time in my harder classes like mechanics and calc. Thanksgiving is around the corner though! Up to any interesting plans yourself? I think we're just going to have my mom's parents over because we already interact with them frequently, but no big gathering this year (obviously).

No kidding! Feels like we're living in a bad TV show.

Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.

A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> Would it be okay if I emailed Dr. Cuneo about this? This is very important for how I structure the confidence intervals code (and whether I make it more sensitive and able to detect more than the four generalized abnormal scenarios described later in this email).

Sure thing, email away.

> Haha! I just read that NuStar's is ~2.5 *milliseconds*.

Yeah and that's actually not too bad compared to most things out there. NICER is just an absolute timing monster.

> I do have my pipeline correct for it automatically, so all your spectra already factor that in.
>
> Cool! Does that mean I can just go ahead and calculate count rates as number of counts / exp. time and not worry about adjusting the result because you've already accounted for deadtime?

Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

> deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.
>
> Yes. This makes sense. I feel like I've seen something related where the center of an object was too bright in the X-Ray band (which oversaturated the detector?) so the color gradient of brightness was black in the center because the author's couldn't determine the brightness at the center. It kinda looked like an artificial coronagraph. Would that be related to a situation where the dead time was more than the event time?

(Deep dive warning:)
Yes, you're speaking about "pileup" which isn't exactly related to deadtime except that both can be important when the source is bright. Deadtime, again, is from the detector shutting down for some portion of the time. Pileup concerns are most often for an imagng type of detector - more like the camera on your phone - that collects an exposure for some amount of time and then read it off.

Chandra, XMM-Newton, Swift, Suzaku, (somewhat confusingly NuStar is in both camps), let X-rays come in from the sky, get focused onto the CCD camera, and then in some time interval, often ~1s, reads off the image and clears the CCD. The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon. So when an individual pixel gets << 1 X-ray per frame on average, you are in good shape. *But* when there is significant probability of getting

>~1 X-ray per frame on a given pixel, then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

This registering of multiple X-rays incorrectly as one more energetic single event is pileup, and it is the bane of most imaging detectors for those of us working with bright sources. (Pileup can occur in instruments like NICER too, when two X-rays hit during the event-latch phase, but this is something like a <1% effect for a 100 Crab-bright source, so well in the camp of being negligible for all practical purposes.)

In the most extreme cases of pileup, the pixel digitizer, which usually records values on something like a 16-bit value (e.g., from -32768:32767), can roll over and then appears negative when too much charge hits at once.

> I'm pretty sure the RXTE data I was working with earlier this year didn't have this identifier because if I remember correctly I couldn't execute *$ignore bad* on the data in XSPEC.

Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

> Here's a working version of the confidence interval / error results script:
>
> I put my XSPECtools.py file which has all of these functions into a custom module in my local site-packages folder for python and it seems like it's working perfectly. I import the file like this: *from personalastropy.xspectools import XSPECtools as xt*, and I execute a particular function like this: *xt.returnFitResults()*.
>
> Happy early Thanksgiving!
> Thaddaeus

That's really great Thaddaeus! I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Very well done!

After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

Best,

-Jack

> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Wed, Nov 11, 2020 at 12:07 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hey Thaddaeus,
>
> What a week it's been eh? (But way to go PA!)
>
> > I've held off from running another major fitting routine since the weekend because after I ran that my last one I decided that it was time to write some standardized scripts for wrangling the results from all the log files.
>
> Excellent!
>
> > Speaking of which, when Cuneo uses green triangles for "*parameters consistent with 0 within errors*", does this refer to situations when either the upper value of a parameter is equal to zero (because it got pegged at the hard upper limit) or when the lower limit is invalid and equal to zero?
>
> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).

I *love* the outline of your routines, and "wow" is what I have to say about that. Way to go Thaddaeus, it's great you're tackling this and in a very useful manner.

> Out of curiosity, where did Cuneo get the count rate values for his data? I don't think it's the same as the Net Count Rate that's displayed whenever you execute *$show data* in XSPEC....can Cuneo's count rate (c/s) be found by dividing the sum of the counts in the included channels by the total observation time?

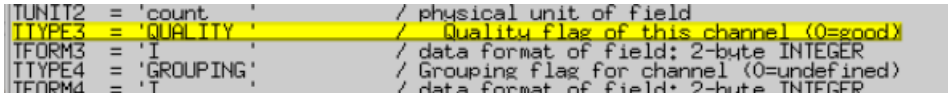
About 99% yes. "Usual" practice for NICER has become to normalize to an active set of 52 detectors. Your spectrum is a collection of all good events. So a tally of all the (good) events between 0.5-10 keV divided by the exposure time is indeed the count rate detected. However, if only using 49 or 50 detectors (also common), one would scale up by 52/49 to show the rate on the full instrument.

The one very tiny wrinkle to all this is a property called "deadtime". Each time the electronics on a detector register an X-ray event, they "latch" meaning that the detector stops listening for new signals. Effectively, the detector is turned off for a short amount of time it uses to calculate properties of event (what time it arrived, and how much charge was deposited). For NICER, this is typically ~20 microseconds.

As a concrete example of what this means in practice: when the count rate per detector reaches ~10^3/s (or in other words the full instrument light curve was at ~50,000/s), there would be ~20e-6 * 1e3 = 2% deadtime. This would mean that the *actual* count rate is ~2% higher than was detected (because the true exposure time was actually 98% the clock time). Here, most likely Cuneo didn't worry about folding in deadtime effects (I do have my pipeline correct for it automatically, so all your spectra already factor that in).

This is obviously a very small here for NICER, but I wanted to dive into it because for many missions that aren't timing beasts like NICER, deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.

> Also sorta random, I came across a value called "quality" when I was looking at a file in fv; are the quality values the values XSPEC refers to when I say *$ignore bad*?



Yes exactly this. This won't tend to come up for NICER, but e.g., for instruments that have some bad pixels and a spatially dispersed spectrum (think the rainbow from a prism), or uncalibrated energy ranges, this can be used.

> * This is probably one of my favorite one-liner functions in Python:
>
> for line in f:
> linelist = (re.sub(' +', ',', line)).split(',')
>
> Where f is a file opened with: *with open(...) as f*.

Python is a wonder for its very efficient one-lining loops!

As usual, you're knocking it out of the park Thaddaeus.

In other news, a new grad student (Santi) just joined me and he's going to start looking at some QPO data.

Best,

-Jack

> On Thu, Oct 29, 2020 at 12:49 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> > Hi Thaddaeus,
> >
> > Whoops - Santiago is my grad student and I notice my advisor brain accidentally called you Santi in my last message. Very sorry about that!
> >
> > Name confusions aside, very glad the patch worked ok. Yeah, setting query to yes can cause problems sometimes as you found. I don't have good rules for avoiding or escaping those runaways, but I've had luck before tweaking the extra convergence terms in the fit command when it's stuck (the delta & beta) and the tolerance term in error.

-Jack

On Thu, Oct 29, 2020 at 11:34 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi Dr. Steiner!
>
> Alright I think that patching did the trick; I'm currently running XSPEC v 12.11.0m. Surprisingly I didn't have to rebuild relxill which was nice. Now that the 12.11.0k patch has been installed, I think XSPEC just "snags" and keeps fitting if I have query set to yes instead of crashing—when I was looking into this, a particular observation ran an error routine for three days nonstop in the background until I aborted it!
>
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Thu, Oct 22, 2020 at 12:46 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Santi,
>
> OK, that's encouraging! Let me know if the patching does the trick. Often for small patches, you won't need to recompile local models, but you should quickly see if it has trouble loading and that would be your sign.
>
> It's hard to predict how parallel processing will impact a given task. The outcome depends on where the bottlenecks occur (if processing, then expect significant improvement, if memory swapping, then probably not). For running on multiple threads, a **rough** estimate is sqrt(N) gain, but that's just ballpark.
>
> -Jack
>
> On Thu, Oct 22, 2020 at 11:56 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
>
> > Hi!
> >
> > I just got some really great news on the crashes!
> >
> > Last weekend I was dismayed because even though I was playing with the limits extensively (especially for gamma), the crashing was still occurring unabated. This made me wonder if it was a software issue, so I reached out to the HEASARC Help Desk. I found out that a very similar issue was fixed in XSPEC patch 12.11.0k: *For some complex fitting and error command cases, XSPEC may terminate with a GSL message such as 'SVD decomposition failed to converge'.* This wasn't encouraging at first because XSPEC patch 12.11.0k came out in June (I built the current version of xspec on my vm in ~July), but I checked my XSPEC version with *$version,* and it turns out that my XSPEC version is just 12.11.0! I'm working on installing the patch, I bet this will clear a lot of things up. I think I'm going to have to rebuild relxill after I execute the partial system rebuild.
> >
> > The only thing that confuses me about this is that I didn't really have issues with core dumping when I was fitting relxill a lot back in August (I think it did come up for error fits back then though), so I'm confused why it really started acting up recently.
> >
> > Also I don't think I've asked this previously, but out of curiosity, should I expect significant reductions in fit times with two core parallel processing?
> >
> > Thaddaeus
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
> >
> > On Thu, Oct 22, 2020 at 10:52 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Just checking in to see how fits are progressing and if playing with those limits has allowed any headway against the crashes?

(Yes, whenever I "rm -rf " I try to take beat and check that I am not about to bilk the whole machine! With great commands-line tools comes great ...)

-Jack

On Fri, Oct 16, 2020 at 3:24 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> Now that I think about it, I do recall noticing that many (if not most) of the fit failures had gammas in the 3.4 range. Good to know about the 1.6-2.2 range for the residuals, I'll play with that now.
>
> Oh about the attachments: the screen shot from visual studio is just some descriptions I was writing about the different functions I was using, and the microsoft paint one just illustrates the positive contribution that is almost always eliminated by a ~1.75 KeV gaussian of varying width.
>
> Over this weekend should I continue to focus on maximizing the number of observations that I can get good fits and confidence intervals for?
>
> I'm intrigued by this disk truncation controversy, I'm going to read about it this weekend.
>
> Many thanks!
> Thaddaeus
>
> P.S. I was reading about some dangerous linux commands the other day; there are some unnerving commands out there. Imagine if someone ran rm -rf in your terminal. Shudder.
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Thu, Oct 15, 2020 at 10:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> > Regarding the application of systematic errors, that's good to know. I've often seen something like the "NICER team recommends the application of a systematic error of 1%" in papers that use NICER data.
>
> Yes, that's a "rough" benchmark, but a reasonable one.
>
> > I love the common.tcl file, it's so handy! I think I'm just going to use it from now on because it's simpler on the execution end (just two commands), and it shows the error strings for each parameter.
>
> Yes, it's quite nice work by Jeremy :).
>
> > A little random: I came across an interesting statement in a paper by Plant et al. 2015 pertaining to observed disk truncation in the hard state of GX 339−4: "This result implies that the current sample of spin estimates in the hard state are inaccurate. Therefore, any distinct conclusions drawn from these estimates, such as the spin-powering of relativistic jets, may well be biased." Since that paper was published has this issue been fully exploited and resolved, or are there still black holes with possibly incorrect spin measurements out there? Also, is physical disk truncation or changing coronal height the more widely accepted cause for profiles similar to disk truncation?
>
> This has been a topic with a lot of controversy that is still ongoing (Garcia et al 2015 and followup papers by Wang et al. 2019 talk about this). I'd say the Plant picture is strongly

disfavored at present, but not entirely out. The robust view is that allowing for the possibility of truncation, a (bright) hard-state spin measurement should be regarded as an estimated lower limit. Presently, the argument is focused around how much truncation could there be in bright hard states, with most reasonable numbers being a few times the ISCO (a factor <10). (Coronal geometry is always an important consideration, but usually they tend to fit very compact.)

> An update on the rsp/arf issue: I modified my approach and fully integrated astropy.io.fits, so when I'm producing my xspec script commands rmf and arf file names are pulled directly from the header of each observation's fits file. Because of the two errors described below, I haven't checked how chi-square improves when the correct rmf and arf files are used though.

Very good, that should make some impact when working.

> I've encountered two weird errors I've never encountered before in XSPEC this week: the first is frequent core dumping [SVD decomposition failed to converge...Default GSL error handler invoked...Segmentation fault (core dumped)], and the second is this odd "XSPEC Error: No variable parameters for fit" after executing "fit" for a model with *multiple* free parameters (including the usual Gamma, FracSctr, Tin, diskbb norm, etc.). My first thought was "*is this related to relxill having problems due to a low gamma"*—but these issues both happen even when Gamma is > 2.

The table can be sparse in a few places, and I think very large Gammas are also sparse (>~3.4, if memory serves). I'd say try freezing parameters and retrying the fit to identify the culprit parameter.

> Also a little random: this week I decided it was time to start commenting on my functions in my python files—at one point I was looking through some older files for a particular technique I used and it took a little while to find the correct version of it, so I decided that from now on I'm describing my functions for future reference.

Yes, documenting code is super important and a great habit to get used to. (I wish I was better about it!)

> This is preliminary, but I attached an example of a chi-scape scarred by a common morph of the residual I last sent you. The positive contribution that begins shortly after 1.6 KeV is almost always eliminated by a ~1.75 KeV gaussian of varying width for most observations. The current issue is modeling the various forms the following negative residuals take. Currently working on those. Earlier you mentioned that we could try ignoring that narrow range where these residuals occur, would ignoring channels between 1.6-2.2 KeV be ignoring too much of the data? If we decide to ignore that range, I would think it best to get >*plt wdata* outputs to exactly nail down the dimensions of this range, but for now should I keep trying to model the residuals?
>
> I was reading through some older emails, going to look into your recommendation regarding logxi.

No, I think 1.6-2.2 isn't crazy and you can certainly roll with that if you are having too much trouble taming the residuals with a model. Maybe do a couple test cases to see how the model parameters are affected on both fronts.

Hey, and I didn't see a description of your attachments in case you want to say anything about those.

Glad you're back in the thick of things.

Best,

-Jack

On Thu, Oct 8, 2020 at 1:35 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi Dr. Steiner!
>
> Quick note: I've been getting back into it this week and should have some interesting things to report this weekend!
>
> Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Tue, Sep 29, 2020 at 2:19 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Thanks for the update.
>
> > Quick note on the residuals: since we last communicated I investigated the application of a systematic error (3-5%) in the < 2 KeV energy band, but this didn't improve the residuals so I'm assuming that you already accounted for this, like the binning, in your reprocessing?
>
> No, actually. But systematic errors are only incorporated with some fit statistics - chisq for instance, but I don't believe are incorporated in cstat or pgstat. The way I like to think about it is the systematic error acts as a reweighting of the data's constraint and serves to de-emphasize regions with the highest signal. I offer for information sake that that 3-5% is too large a systematic error to include as a rule. Something like 1-2% is more reasonable for a region with poorer calibration quality.
>
> > I also discovered that I've been unintentionally forcing an incorrect response file on multiple MAXI J1535-571 observations—the rsp I was using for all observations was `nicer_d49_55575341.rmf`, but after running a quick check (the fitsio package has been so helpful, thanks for recommending it to me!), I found that multiple observations request `nicer_d49_u100_150_55575341.rmf` in their headers.
>
> Ah! That's very good to know, and may be the culprit for some of the residuals you've been battling. Check that the arf files (keyword ANCRFILE) is also set correctly.
>
> > I'm likely almost done with the SAT (took it Saturday, going to take it again this coming Saturday)*, and once I'm done I aim to return to a progress pace similar to the pace we had in the summer.
>
> Congratulations - that's a nice milestone!
>
> > G'mar chatima tova (if you observe Yom Kippur)!
>
> Same to you.
>
> > *I might take it again in November in the event that I bungle those tests.
>
> I'd put dollars to donuts you did great!
>
> My best,

-Jack

> On Tue, Sep 22, 2020 at 1:21 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Interesting I'll look into that!
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi
>
> On Tue, Sep 22, 2020 at 1:15 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Similar to that. I used another language (IDL) to automatically produce a series of tcl/tk scripts and then executed those distributed over a number of cores on a workstation.
>
> -Jack
>
> On Tue, Sep 22, 2020 at 1:13 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Sounds good!
>
> In that case did you operate solely in TCL?
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi
>
> On Tue, Sep 22, 2020 at 1:10 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Thaddaeus,
>
> > For >75% I meant observations where source flux was more than 75% bg flux, and for 4% I meant observations where source flux was ~4 % bg flux. That makes sense—interestingly the observations with source flux ~4% bg flux were, by best fit, in the SPL state, with the highest flux and gamma for all the observations that had reasonable values for these parameters.
>
> Ah, now I see. Yes, when the bg flux is very high, the noise level (N=sqrt(B+S)) where B and S are background and signal counts in a channel, then the constraining power of the data (S/N) is lowest. If the background is reasonably estimated, the fits will likely be good but poorly constrained; if the background is off, then all bets are off.
>
> > xD. I think it's a very impressive feat! Did you use pyxspec to loop through all the observations?
>
> Thanks. I used a different language actually, but python/pyxspec would be an equally good way to do it.

Best,

-Jack

On Tue, Sep 22, 2020 at 12:39 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> That's good to know. I guess what's surprising is some relative of this large residual in the 1.6-2.0 KeV is present in most of the source flux > 75% observations (I wonder if the peak feature falsely inflates the source flux for some of them?). Going back to a couple emails ago, I was checking some of my all time best fits which had source fluxes around ~4%—what allows such faint observations to fit well?

Just to clarify, do you mean 75% and 4% of the Eddington limit, peak luminosity, or something else? Fainter observations, given some typical observation time, obviously have fewer counts which means larger fractional errors and less constraining power on the model. So if there is a deficiency in the calibration or model being used, it will look more prominent in brighter + long exposure time data.

> Also, I'm very intrigued by your methods in one of your 2016 papers , "STRONGER REFLECTION FROM BLACK HOLE ACCRETION DISKS IN SOFT X-RAY STATES". How did you automate fitting the 15,000 observations reliably and within a reasonable time frame?

A reasonable time frame is relative :), but I used about two dozen cores to run the fits which helped. I also stuck with computationally simplistic models, and for my runs with simpl, used lower resolution (via energies) in the run, since RXTE/PCA - the instrument used there - also has rather poor energy resolution compared to NICER.

> Could I possibly have the ~86 observations between obsids 1200220101 – 1200220186?

Spectral files are attached. Response files are too big for me to attach, so I'll keep them up here for a few days: https://nicer-web.mit.edu/nicerview/thadd_resp_m1727.tar.gz

> Ah that makes sense. In that case did Xu et al. just freeze Rin at the ISCO to have less free parameters?

That's the usual tactic, yes, when fitting for spin.

Best,

-Jack

On Thu, Sep 17, 2020 at 11:00 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Great to hear you've been digging in and exploring! I would suggest though that you avoid being too concerned about the residual features. If something is localized and only ~percent level deviation, then that's likely sufficient. *The goal should be to fit those sufficiently that they don't alter the continuum, but at some level it becomes diminishing returns to try fitting an artefact.* That said, if you wish, I can get you the data on

MAXI J1727-203. Just let me know if you have a particular ObsID from Kevin's paper you want to try.

Your question about degeneracy is great. The condition of parameters being strongly correlated with one another is degeneracy. Basically, this is a very common situation where two parameters are dependent upon one another to some extent, so a probable value of one will condition a probable range of values with the other. For instance, if you want to fit x and y with the constraint x^2+y^2 = 4+/-1, you would get a ring of radius 2 with x,y values that obviously depend on one another; they are *not independent*. Speaking generally for more realistic cases, the interaction between parameters is often pretty convoluted, but commonly things like Gamma and Normalization are correlated with one another, so a contour plot of the confidence interval would look banana shaped rather than like a circle.

Best,

-Jack

On Thu, Sep 17, 2020 at 9:18 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> Thanks for checking in! Since we last spoke I've been taking a deeper look into a couple articles (Alabarta et al. 2020, Xu et al. 2018, and Parikh et al. 2019) and playing around a bit with the residual fix you suggested. As tested on multiple observations with varying fit statistic contributions in the 1.6-1.8 KeV range (chi-squared contribs. ranging between 500-3000), the gaussian centered around 1.75 KeV combined with the "edge" works great!
>
> Most importantly, I found a method to more accurately investigate the residual features: *plt>wdata*. Now that I can actually see what the chi-sq contrib. is for every point, I've been having a lot more success in approaching residuals in the 1.6-2.2 KeV range, especially when the residual patterns after 1.8 KeV differ greatly from what can quickly be ameliorated with the edge feature (it seems that for the majority of observations the <1.8 KeV excess is easily subdued by the gaussian fix, but after 1.8 KeV things often get complicated, especially when a third feature shows up between 1.86 to 2.0 KeV. This third feature resembles a sort of gaussian-like crevice about 0.13 KeV in width and often reaches a chi^2 contribution of -5331 (+) at its minima, pushing the "edge" feature out to a threshold energy of ~2+ KeV). I haven't found a definitive solution yet, but a gaussian absorption with an energy of ~1.86 KeV and a width of ~0.14 KeV is looking promising.
>
> While I've been working with these MAXI J1535-571 residuals I've been wondering if the residuals Alabarta et al. discuss in Nicer data for MAXI J1727−203 are of similar shape—is there any chance I could procure some pipeline data from you on MAXI J1727-203 to look into this (if it's not an inconvenience for you and or you don't think it's a distraction on my part of course).
>
> Out of curiosity, what does "parameter degeneracy" mean? My first guess would be this example: if I only told you what the slope of a line was (e.g. '2'), and then asked you for the individual values of Δy and Δx—would Δy and Δx be degenerate? Example of where I came across this: (from Xu et al. 2018) "The inner disk radius, R_{in}, and the black hole spin parameter, a*, are degenerate, as they both control the effective inner accretion disk radius. Therefore, for simplicity, during the spectral fitting we assume that the inner disk extends down to the ISCO by

fixing R_{in} at the radius of the ISCO, and fit for the black hole spin as a free parameter."

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

Steiner, James <james.steiner@cfa.harvard.edu> Fri, Jan 8, 2021 at 7:00 AM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

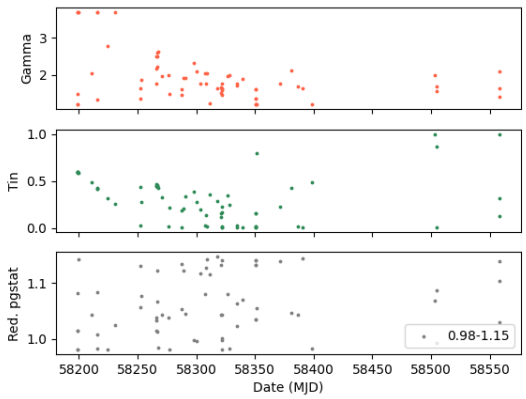
Hi Thaddaeus,

Ah, I remember an ice-cream fueled haze of recovery in the aftermath. Hope you are getting some of the same!

-Jack

On Fri, Jan 8, 2021 at 8:38 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

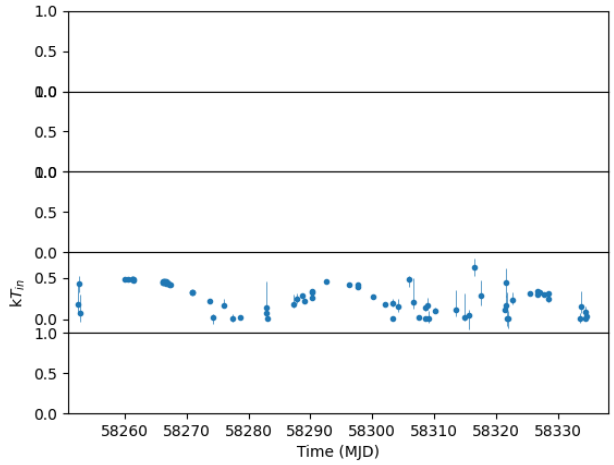
> Hi Dr. Steiner!
>
> I was a little out of it this week after getting my wisdom teeth pulled, just wanted to let you know I've seen your email and I'm going to work on it this weekend.
>
> Cheers!
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi
>
> On Tue, Jan 5, 2021 at 5:23 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
>
> > Hey Thaddaeus,
> >
> > Wow, that's an impressive and sweet-looking delivery!
> >
> > > Thank you for your well wishes! I hope the same for you and your family as well!
> >
> > Thanks Thaddaeus, we're doing just fine and couldn't be happier to turn the page on 2020.
> >
> > > First, a thought back on the relxill models: don't nthcomp and simpl produce different values for gamma and hence they should not be mixed with each other? This thought came to me this morning when I was tidying things up. Since I was tieing relxill's gamma to simpl's, I wonder if that threw anything off. Now that I think about it more, I don't think it would catastrophically derail things, but it would still be inappropriate to tie gammas from different models right?
> >
> > The default answer is that these should be identical - they both describe the shape of the Comptonized power-law component, and in the same way. For completeness, I should mention there has been some small exploration about decoupling these things with some loose physical arguments, but basically that's a stretch and I would absolutely keep them linked up as you've done.
> >
> > > The first hint of a breakthrough happened on Tuesday when I realized I forgot to filter for background fraction. I looped through all the data files that contributed to the last plot I sent you and only plotted fits from files where background was < 50% source, giving me this, much neater, plot (where gamma and inner disk temp are actually somewhat correlated:
> >
> > Ah, yes! Glad you caught that; keeping to the cleanest data is a very good idea, and 50% is reasonable; if things look at all funny, even down to ~20% would be a reasonable place to cut.



This cleaned things up pretty well, but I noticed there were only a couple points with gamma > 3 (all of which were right on 3.5), so I ran another routine with the upper limits on gamma set to 3. The points that were right on 3.5 were on 3.0 for gamma in this routine, so I wondered if it wasn't an issue of the upper limit but an issue of insensitivity. When I tried to estimate 1-σ errors on a couple of the fits that had 3.5 upper limits, XSPEC returned "***Warning: Parameter pegged at hard limit: 3.5"—which I take as being caused by gamma's insensitivity to the fit in the observations.

It is possible this is because the fit isn't sensitive to Gamma. (You could check this out by comparing fit statistics to the upper-limit 3.5 vs the upper-limit 3 fits, or also looking at the f_sc values - <~ 0.01 being an indication that isn't sensitive.) But alternatively, it's also possible that the data are sensitive to Gamma and either (i) the spectrum is very soft, wanting high Gamma in which case lowering the upper-limit would still cause it to peg but the fit would be markedly worse or (ii) *my guess for most likely for these data*, the background subtraction is off in those cases and oversubtracts, creating an artificial appearance of a very soft spectrum.

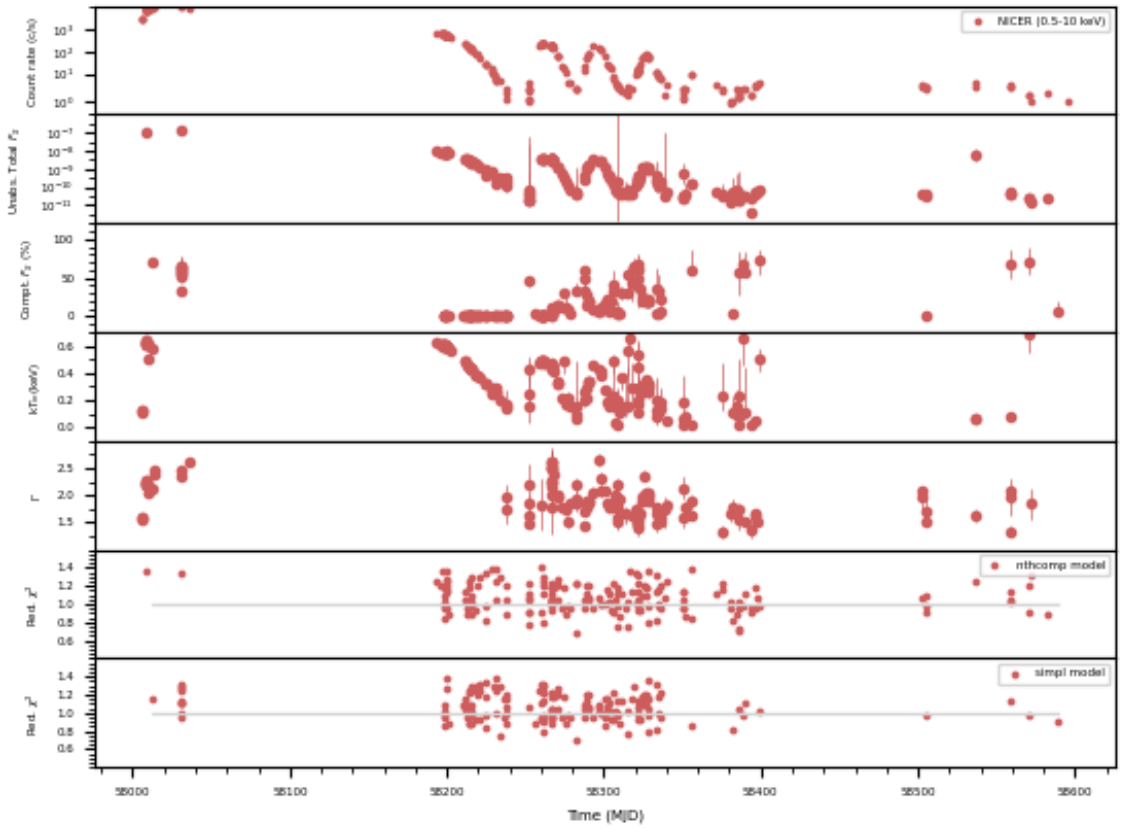
Then I plotted Tin measurements from that fit routine with gamma's upper limit set to 3.0 with one sigma (all confidence levels I bring up are one sigma unless otherwise noted, as in Cuneo's paper; though I must say, one sigma sounds so much better than 68% confidence even though they're the same thing lol), here's how that turned out:



Pretty nice, eh?

Yes, this looks *very nice*!

So fast forward (and a bunch of partial fitting routines later), I'm proud to present these:

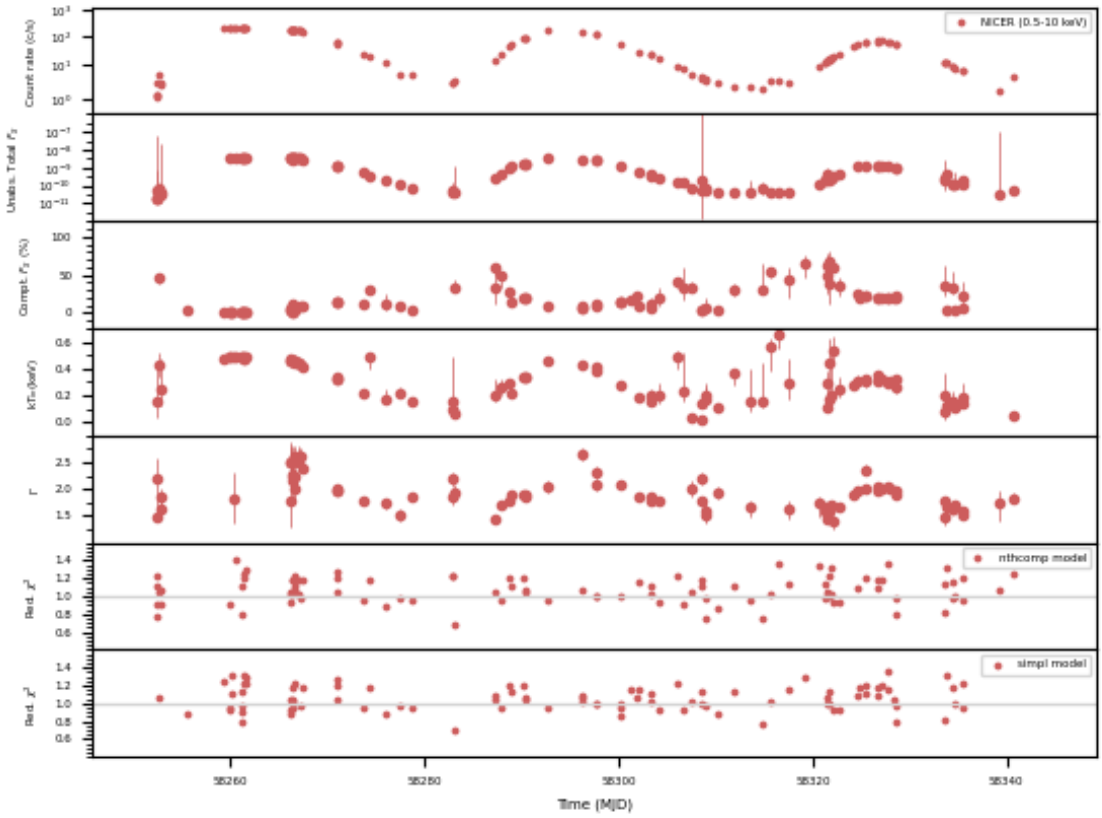


This is my final plot for the MAXI data. The top panel is background subtracted count rate (a note on this later), the second is unabsorbed total flux on the range 0.5-10.0 keV, the third panel is comptonized flux percentage (calculated with a separate simpl model, a note on this later), the fourth inner disk temp, the fifth gamma, the sixth reduced pgstat for the nthcomp model, and the seventh the reduced pgstat for the simpl model.

Excellent - these look really great. It appears to me that there are some points shown in the simpl panel that aren't shown in the nthcomp panels, and vice-versa. Can you mention what the screening difference is? (e.g., around day 55030)

For a disk-blackbody, the expectation is that L ~ T^4, so you can plot L vs T on a log-log plot and look at that scaling.

Here's what my plot looks like during the reflaring period investigated by Cuneo:



All parameters were calculated with the nthcomp model except for comptonized fraction. Model descriptions below:

##Model One##
Model: tbabs(diskbb+nthcomp)
Delta fit statistic: 1.
Noticed Ranges: 0.5-1.5 2.3-10.0 keV
nH: 3.2 (frozen)
Tin lower limit: 0.01
Tin upper limit: 1.0
Gamma lower limit: 1.2
Gamma upper limit: 3.0
High energy rollover: 100 keV
inp_type: 1
Redshift: 0

##Model Two##
Model: tbabs(simpl(diskbb))
Delta fit statistic: 1.
Noticed Ranges: 0.5-1.5 2.3-10.0 keV
nH: 3.2 (frozen)
Tin lower limit: 0.01
Tin upper limit: 1.0
Gamma lower limit: 1.4 #I adopted this limit from your paper in which you used simpl to fit those 15,000-odd observations.
Gamma upper limit: 3.4 #I adopted this limit from your paper in which you used simpl to fit those 15,000-odd observations.
UpScOnly value: 1

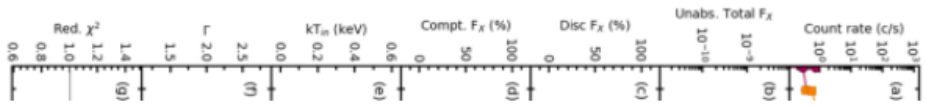
Looks good.

****
Per the background subtracted count rates, I corrected my code to account for your pointers; the important snippet is below:

```
scaled_bg_counts = (sum(restricted_bg_counts_list)/bg_exp_time)*exp_time
count_rate = sum(restricted_data_counts_list)-scaled_bg_counts
count_rate = count_rate/exp_time
os.remove(temp_data_file)
os.remove(temp_bg_file)

out_list.append((seg_id+':'+str(count_rate)))
```

To check: I take it restriced_bg_counts_list, and analogous for the data are only using the noticed ranges? If so, all looks good!

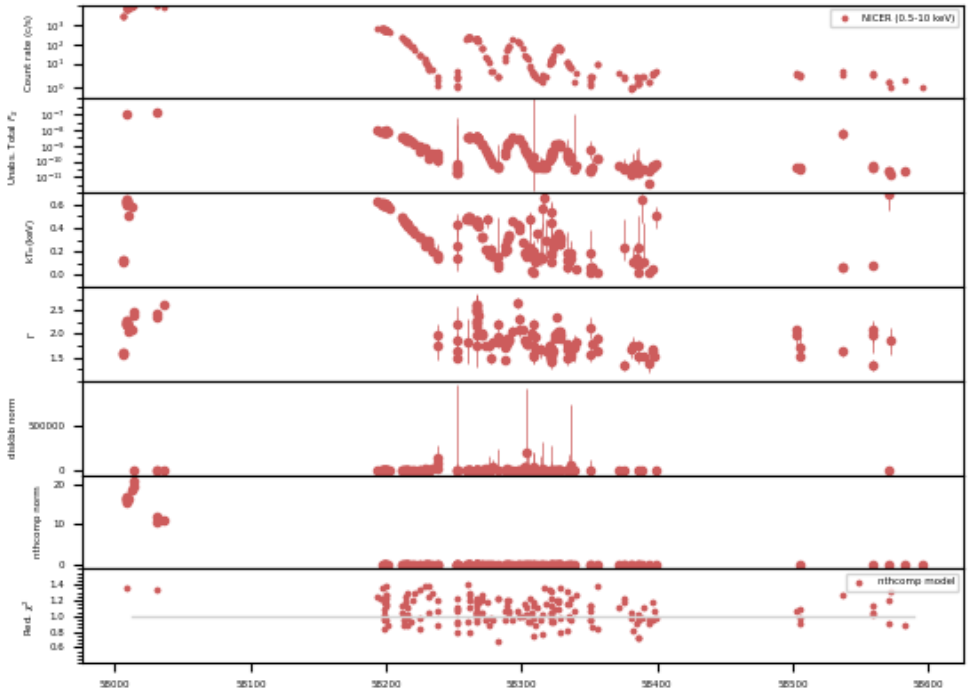
Oh also, a small note on my final plots. I think I found out how Cuneo prevented ytick labels from colliding:



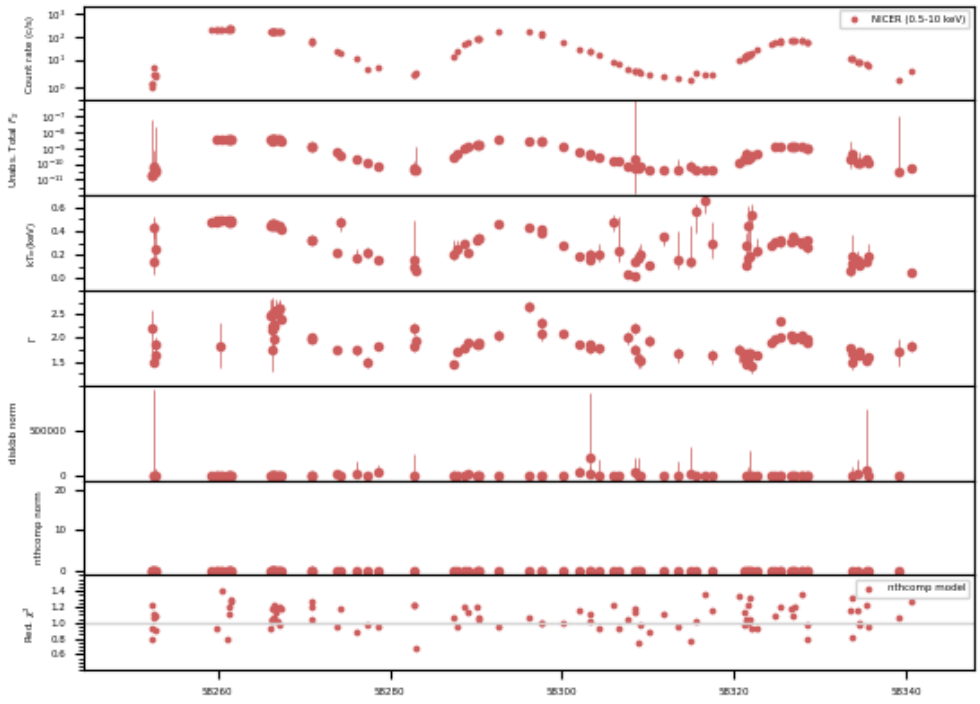
I think she set the ylim bounds somewhat lesser and somewhat more than the min(y) and max(y) vals—so I did this and it improves the aesthetics significantly in my opinion.

Yes, this is a nice way to keep the plot clean. (And for x-ranges too.)

As you requested, here's a plot with the diskbb and nthcomp norm components:



(Full range)



(cuneo date range)

This is great, but I do have one request: it would be helpful to have the normalizations plotted in log-scale as well (to better see their dynamic change).

> I didn't include values consistent with zero within errors in any of these plots, but I can if you'd like.

Sounds reasonable to me. Can also try plotting them with a different (less prominent) color or symbol as an alternative.

> Right now the raw outputs from my returnConfidenceIntervals() function look like this:
>
> PROBLEMS OUTPUT DEBUG CONSOLE TERMINAL
>
> ```
> 1130360284_1:PHLZ:0.591738
> 1130360284_2:0.249281:0.334173
> 2130360201_3:0.107522:0.107773
> 2130360201_4:0.257621:0.200038
> 2130360201_5:0.36142:0.204905
> 2130360202_0:PHLZ:0.116443
> 2130360202_1:PHLZ:0.718751
> 2130360203_0:0.295584:0.279007
> 2130360204_1:PHLZ:0.727953
> ```
>
> (where PHLZ stands for Parameter Hit Lower Zero and PHUZ stands for Parameter Hit Upper Zero)
>
> Early in the week I thought about changing the output of this function to some kind of multidimensional numpy array like this:
> `[['seg_id_1','seg_id_2'],[lower_1,lower_2],[upper_1,upper_2]]`
>
> But I didn't really feel like it because the current function works fast enough for me (I don't ~~see the need to vectorize~~ (more accurate statement: feel like vectorizing) all my functions yet; unless you would advise me to in which case I'll get right to it lol). *

Ha - while it can absolutely make a big difference in some cases, it sounds like from below you've found those cases where it really matters. Usually when memory juggling big arrays you want to do that once and avoid repeated calls in that vein, and otherwise small transformations of scalars there will be plenty of situations where it looks much nicer but won't make much practical impact.

> I did write a standard function for returning float values of value date, value and a two dimensional array for yerr for parameters based on an input x=the parameters output from returnConfidenceIntervals(), y=theoutput from a returnDates() function I wrote a bit ago, and z =

> an output from the returnFitResults() function I wrote a bit ago. Lol reading that last sentence, it looks a little confusing so I'll add this function to my package and document it in markdown and show it to you in a later email.
>
> I still have to iron out my hardness ratios function, will get to that soon.
>
> A random, last thought: in compsci club at school we've recently been discussing algorithmic efficiency in the context of things like sorting algorithms, which inspired me to think about this in the context of my code. So far I'd say a noticeable improvement that has resulted from this is that I'm limiting the number of computationally expensive tasks I put under for and conditional loops as long as I can execute them outside the loop. An example of this would be declaring a scipy.interpolate.interp1d object outside of a for loop rather than declaring it for every data value in a set (this came up in something else I was working on, but I brought it up because I think it's a good example).

Absolutely, this is just the kind of case you want to call once only.

> All data referenced in the plots, the python file I wrote for creating these plots, and the python file I used to write the fit commands are all attached in the email_attachments .xz tarball.
>
> Cheers (and Happy New Year)!
> Thaddaeus

Wonderful work Thaddaeus! And Happy New Year!

-Jack

> When you replace a for loop with a vectorized numpy function and see the speed improvement
>
> 

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Dec 31, 2020 at 2:38 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> It's been quite a month everywhere! Hope you had a nice holiday season and that you and your family have been doing well. Wishing you a happy and better New Year ahead.
>
> -Jack
>
> On Wed, Dec 9, 2020 at 2:57 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
>
> > Hey Thaddaeus,
> >
> > Those parameter plots look great! Can you add in the diskbb and compTT normalizations as panels as well?

It looks to me like things are probably going wrong because you are probably using bin-number rather than channel number. Sometimes these two get conflated, but here by bin I mean groups of one or more channels, where a channel is the thing that is hard-coded in the spectral file (and PI-bins correspond to 10 eV increments). So you'd want to use channels 50-299 and 300-999 for your two bands.

The other thing to check is that the background is scaled properly. So you want to make sure that to background you subtract is:
BG_Spectrum_MatchedToData = BG_Counts_FromBGFile / BGFile_ExpTime * DataFile_ExpTime

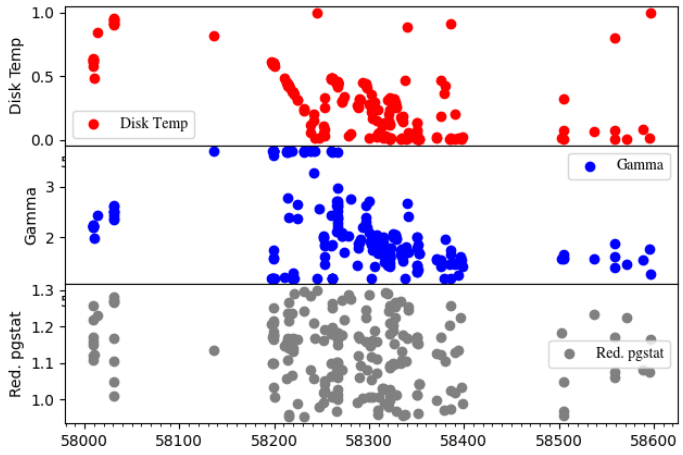
Hopefully my shorthand there is clear enough.

Best,

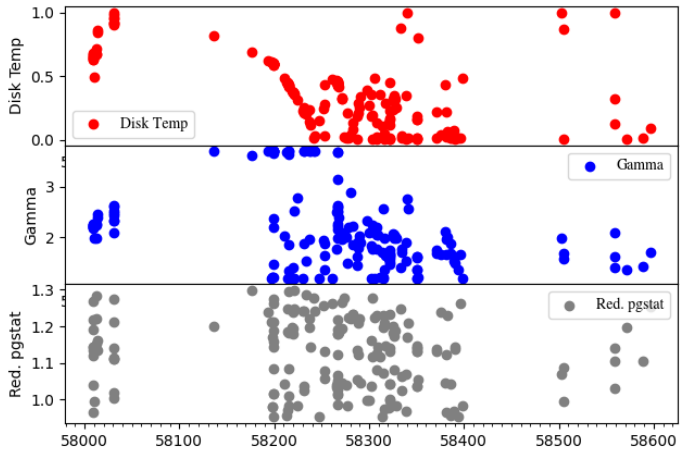
-Jack

On Wed, Dec 9, 2020 at 2:39 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi Dr. Steiner!
>
> Phew! The number of potential files for fitting has shot up to 720 from 185 now that I'm searching for and including all GTIs files.
>
> I ran a couple fitting routines with a simple tbabs(diskbb+nthcomp) model on all the files just to get a feel for how including a lot more files would possibly change things, and the initial results from these fitting routines were quite unusual (though they were blisteringly fast compared to relxill fits regardless —with nH frozen on these routines, I was fitting all the files with 700 fit iterations each in under an hour on my mom's computer) :



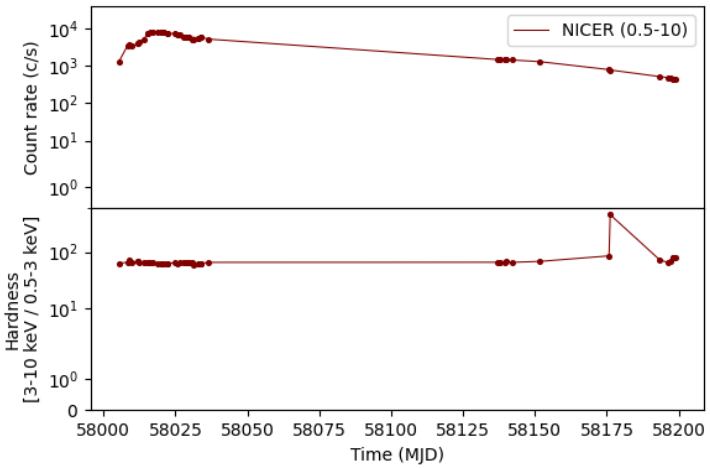
> ^ That routine had diskkbb Tin upper limit set to 1, gamma upper limit set to 3.7, nH frozen to 3.2, and the entire energy range from 0.5-10.0 keV noticed.



> ^That routine was the same except the energy interval 1.5-2.3 keV was ignored.
>
> I think I see a little pattern of correlated variation between disk temp and gamma masked under the erratic points in both plots...I'm wondering, should I set some cutoff exposure time for the files? I recall seeing some ~22 sec exp. time data files when I was looking

through other GTIs, and I'm thinking since these files would have less data to constrain the model, crazy parameters can be permissibly returned by XSPEC when these files are fit.

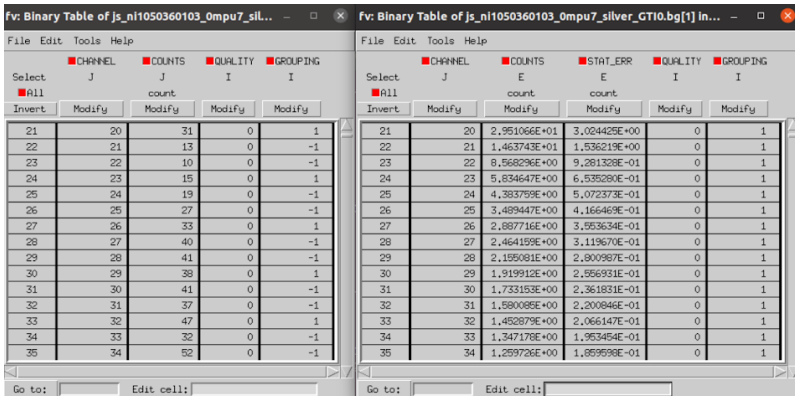
Also, could I get your advice on calculating hardness ratios? I modified my count rate function to correct for background counts and it's working pretty well (see the top subplot below):



Almost identical to Cuneo's count rate plot, eh?

As you can see in the bottom subplot, the hardness ratio values are crazy, and I can't figure out how to rein them in. The way my function for hardness ratio works is I sum the number of counts in the parameter defined hard band and subtract the number of background counts in the predefined hard band from that value, giving me a net hard band counts sum. I then divide that value by (sum of soft counts-sum soft background counts). Code is attached.

I was checking with fv, and it's interesting that in this case (and what I think is true in general) the bg counts are very low in the hard band but fairly high in the thermal:



Channels 22-99 = 0.5-3 keV

Channels 101-254 = 3.0-10.0 keV

Cheers!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 3:36 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

> Thanksgiving was very nice, thanks!

Yay that's great to hear!

> Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.

Sounds good.

> YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.

!!! I will do this then!

> Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

Will do.

Thanks!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 12:28 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Thanksgiving was very nice, thanks! Glad yours with the grandparents was too.

> I reprocessed the data last weekend (11/22) but the results were a little weird so I've been tweaking some things and prepping for reprocessing again this week.
>
> I was wondering if I should take a different approach to the instrumental residuals by running a preliminary fit on all files and logging the fit statistic landscape, so I can then loop through and identify which files actually require the 1.5-2.2 keV range to be ignored and which can have it noticed (I'm wondering about this because from a preliminary check a lot of the disk temperatures were behaving weirdly, which I think could be related to having less data in the thermal band to constrain them). What do you think?

Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.

> Also, I've only been fitting the silver, zero good time interval files; would you recommend me to fit some of the GTI1s, GT2s, etc. in hopes to get more good fits?

YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.

> P.S. I wrote two new standardized functions (one calculates and returns count rates, the other calculates and returns hardness ratios) and nice documentation for all my standard functions so far.

Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

-Jack

On Mon, Nov 23, 2020 at 8:34 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Will do!

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 23, 2020 at 4:30 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Sounds good! Glad your mom's computer has some CPU cycles to share. Keep me posted when you get the results in hand. Meantime, happy Thanksgiving.

-Jack

On Sun, Nov 22, 2020 at 8:01 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

Yes I've been fitting with the inner radius frozen to -1. That's good to know.

> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind. So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

Sounds good.

> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Yes I agree it was nice that they showed the underlying component.

I've been running the first iterations of the reprocessing last night and today and I've had a little breakthrough—last night I quickly updated the version of XSPEC I had on the vm that I installed on my mom's computer way back in June (I also installed relxill on it), and I've been dividing the work between her computer and mine—the routines are concluding much quicker (her computer has four cores xD).

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Fri, Nov 20, 2020 at 9:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind.

So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

And in response to your email yesterday:

> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

Yes, spot on!

As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Best,

-Jack

On Fri, Nov 20, 2020 at 8:13 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Shoot! I forgot this bring this up. Would you recommend me to freeze the spin and inclination parameters to published values (that are in agreement with some of my previous best fits) to simplify the parameter space, or should I keep them free? The only benefit I can see from keeping them free would be sometimes error locks up with no variable parameters to use when free parameters get pegged due to their insignificance to the fit, and I think having them free could help prevent this...though spin and inclination seem like parameters that would be the first to get pegged during an error routine anyways.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Nov 19, 2020 at 8:07 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hey!

> Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.

Yeah, safety is (rightly so) the first priority.

> A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

Oo sounds cool, I hope you all have fun!

> Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

Perfect.

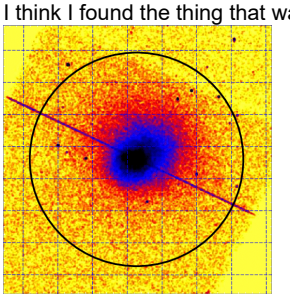
> The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon.

Interesting.

> then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

Very interesting.

I think I found the thing that was on the edge of my memory: a Chandra image of the quasar H1821+643.



So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

> Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

I should probably rephrase what I said earlier. When I ignored bad, no channels were ignored so I guess that meant no poor quality flags were raised.

> I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Agreed. I find it so enjoyable: the initial encounter with a seemingly insurmountable challenge, the brainstorming, the troubleshooting, and *especially* the successful execution of a solution. It's extremely satisfying—and to use an analogy from my youth* it feels kinda similar to when I would build things in lego.

*I say an analogy from my youth but that's somewhat misleading. I still occasionally play legos, and these days the only difference is that I have a partner: my younger brother ;).

> After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Definitely. That is exactly what I'm thinking.

> Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

:)).

Best!
Thaddaeus

Oh I almost forgot. I don't remember how/why I came across this paper (Arumugasamy et al. 2018), but after looking at the plots I've been meaning to ask you about them:

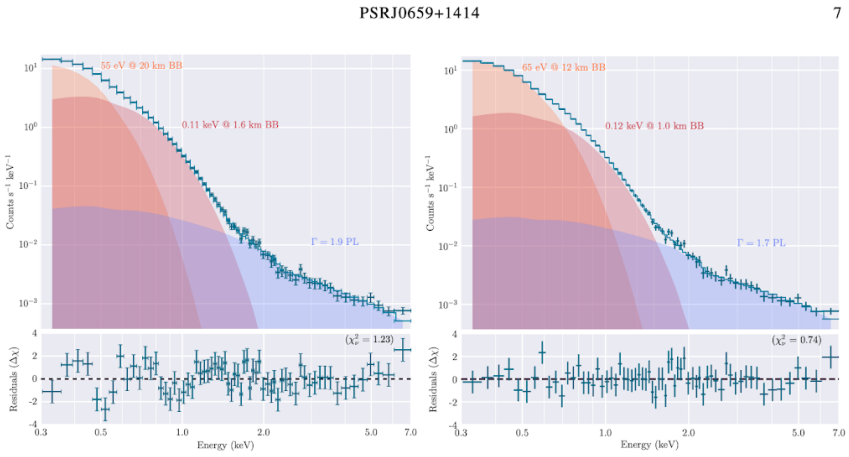


Figure 4. 2BBPL model, without (left) and with (right) `Gabs` absorption component, fit to the phase-integrated spectrum of J0659+1414. The model parameters correspond to maximum posterior probability, which in the case of G2BBPL are different from the distribution medians listed in Table 2.

I've been using the darkgrid style (like the plots above) with the rc font parameter set to Nimbus Roman in most of my plots ... but after seeing these plots I'm thinking I was to switch back to the mpl defaults—these plots are kinda distracting to me in their aesthetic.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Wed, Nov 18, 2020 at 10:05 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> It was a wild week indeed—and things seem to keep getting crazier. We were in a hybrid model since October 19th, but as of Tuesday we went indefinitely back to all online which is a two sided coin. On one hand the online schedule gives me a lot more free time, but on the other I'm going to miss the extra class time in my harder classes like mechanics and calc. Thanksgiving is around the corner though! Up to any interesting plans yourself? I think we're just going to have my mom's parents over because we already interact with them frequently, but no big gathering this year (obviously).

No kidding! Feels like we're living in a bad TV show.

Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.

A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> Would it be okay if I emailed Dr. Cuneo about this? This is very important for how I structure the confidence intervals code (and whether I make it more sensitive and able to detect more than the four generalized abnormal scenarios described later in this email).

Sure thing, email away.

> Haha! I just read that NuStar's is ~2.5 *milliseconds*.

Yeah and that's actually not too bad compared to most things out there. NICER is just an absolute timing monster.

> I do have my pipeline correct for it automatically, so all your spectra already factor that in.
>
> Cool! Does that mean I can just go ahead and calculate count rates as number of counts / exp. time and not worry about adjusting the result because you've already accounted for deadtime?

Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

> deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.
>
> Yes. This makes sense. I feel like I've seen something related where the center of an object was too bright in the X-Ray band (which oversaturated the detector?) so the color gradient of brightness was black in the center because the author's couldn't determine the brightness at the center. It kinda looked like an artificial coronagraph. Would that be related to a situation where the dead time was more than the event time?

(Deep dive warning:)
Yes, you're speaking about "pileup" which isn't exactly related to deadtime except that both can be important when the source is bright. Deadtime, again, is from the detector shutting down for some portion of the time. Pileup concerns are most often for an imagng type of detector - more like the camera on your phone - that collects an exposure for some amount of time and then read it off.

Chandra, XMM-Newton, Swift, Suzaku, (somewhat confusingly NuStar is in both camps), let X-rays come in from the sky, get focused onto the CCD camera, and then in some time interval, often ~1s, reads off the image and clears the CCD. The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon. So when an individual pixel gets << 1 X-ray per frame on average, you are in good shape. *But* when there is significant probability of getting >~1 X-ray per frame on a given pixel, then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

This registering of multiple X-rays incorrectly as one more energetic single event is pileup, and it is the bane of most imaging detectors for those of us working with bright sources. (Pileup can occur in instruments like NICER too, when two X-rays hit during the event-latch phase, but this is something like a <1% effect for a 100 Crab-bright source, so well in the camp of being negligible for all practical purposes.)

In the most extreme cases of pileup, the pixel digitizer, which usually records values on something like a 16-bit value (e.g., from -32768:32767), can roll over and then appears negative when too much charge hits at once.

> I'm pretty sure the RXTE data I was working with earlier this year didn't have this identifier because if I remember correctly I couldn't execute *$ignore bad* on the data in XSPEC.

Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

> Here's a working version of the confidence interval / error results script:
>
> I put my XSPECtools.py file which has all of these functions into a custom module in my local site-packages folder for python and it seems like it's working perfectly. I import the file like this: *from personalastropy.xspectools import XSPECtools as xt*, and I execute a particular function like this: *xt.returnFitResults()*.
>
> Happy early Thanksgiving!
> Thaddaeus

That's really great Thaddaeus! I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Very well done!

After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

Best,

-Jack

> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Wed, Nov 11, 2020 at 12:07 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hey Thaddaeus,
>
> What a week it's been eh? (But way to go PA!)
>
> > I've held off from running another major fitting routine since the weekend because after I ran that my last one I decided that it was time to write some standardized scripts for wrangling the results from all the log files.
>
> Excellent!

> Speaking of which, when Cuneo uses green triangles for "*parameters consistent with 0 within errors*", does this refer to situations when either the upper value of a parameter is equal to zero (because it got pegged at the hard upper limit) or when the lower limit is invalid and equal to zero?

Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).

I *love* the outline of your routines, and "wow" is what I have to say about that. Way to go Thaddaeus, it's great you're tackling this and in a very useful manner.

> Out of curiosity, where did Cuneo get the count rate values for his data? I don't think it's the same as the Net Count Rate that's displayed whenever you execute *$show data* in XSPEC....can Cuneo's count rate (c/s) be found by dividing the sum of the counts in the included channels by the total observation time?

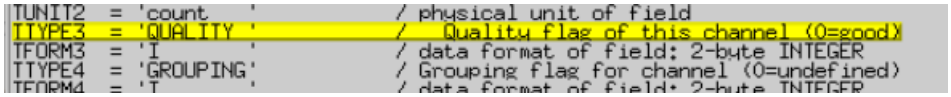
About 99% yes. "Usual" practice for NICER has become to normalize to an active set of 52 detectors. Your spectrum is a collection of all good events. So a tally of all the (good) events between 0.5-10 keV divided by the exposure time is indeed the count rate detected. However, if only using 49 or 50 detectors (also common), one would scale up by 52/49 to show the rate on the full instrument.

The one very tiny wrinkle to all this is a property called "deadtime". Each time the electronics on a detector register an X-ray event, they "latch" meaning that the detector stops listening for new signals. Effectively, the detector is turned off for a short amount of time it uses to calculate properties of event (what time it arrived, and how much charge was deposited). For NICER, this is typically ~20 microseconds.

As a concrete example of what this means in practice: when the count rate per detector reaches ~10^3/s (or in other words the full instrument light curve was at ~50,000/s), there would be ~20e-6 * 1e3 = 2% deadtime. This would mean that the *actual* count rate is ~2% higher than was detected (because the true exposure time was actually 98% the clock time). Here, most likely Cuneo didn't worry about folding in deadtime effects (I do have my pipeline correct for it automatically, so all your spectra already factor that in).

This is obviously a very small here for NICER, but I wanted to dive into it because for many missions that aren't timing beasts like NICER, deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.

> Also sorta random, I came across a value called "quality" when I was looking at a file in fv; are the quality values the values XSPEC refers to when I say *$ignore bad*?



Yes exactly this. This won't tend to come up for NICER, but e.g., for instruments that have some bad pixels and a spatially dispersed spectrum (think the rainbow from a prism), or uncalibrated energy ranges, this can be used.

> * This is probably one of my favorite one-liner functions in Python:
>
> for line in f:
> linelist = (re.sub(' +', ',', line)).split(',')
>
> Where f is a file opened with: *with open(...) as f.*

Python is a wonder for its very efficient one-lining loops!

As usual, you're knocking it out of the park Thaddaeus.

In other news, a new grad student (Santi) just joined me and he's going to start looking at some QPO data.

Best,

-Jack

> On Thu, Oct 29, 2020 at 12:49 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Whoops - Santiago is my grad student and I notice my advisor brain accidentally called you Santi in my last message. Very sorry about that!

Name confusions aside, very glad the patch worked ok. Yeah, setting query to yes can cause problems sometimes as you found. I don't have good rules for avoiding or escaping those runaways, but I've had luck before tweaking the extra convergence terms in the fit command when it's stuck (the delta & beta) and the tolerance term in error.

-Jack

On Thu, Oct 29, 2020 at 11:34 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi Dr. Steiner!
>
> Alright I think that patching did the trick; I'm currently running XSPEC v 12.11.0m. Surprisingly I didn't have to rebuild relxill which was nice. Now that the 12.11.0k patch has been installed, I think XSPEC just "snags" and keeps fitting if I have query set to yes instead of crashing—when I was looking into this, a particular observation ran an error routine for three days nonstop in the background until I aborted it!
>
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Thu, Oct 22, 2020 at 12:46 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Santi,
>
> OK, that's encouraging! Let me know if the patching does the trick. Often for small patches, you won't need to recompile local models, but you should quickly see if it has trouble loading and that would be your sign.
>
> It's hard to predict how parallel processing will impact a given task. The outcome depends on where the bottlenecks occur (if processing, then expect significant improvement, if memory swapping, then probably not). For running on multiple threads, a **rough** estimate is sqrt(N) gain, but that's just ballpark.
>
> -Jack
>
> On Thu, Oct 22, 2020 at 11:56 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
>
> > Hi!
> >
> > I just got some really great news on the crashes!
> >
> > Last weekend I was dismayed because even though I was playing with the limits extensively (especially for gamma), the crashing was still occurring unabated. This made me wonder if it was a software issue, so I reached out to the HEASARC Help Desk. I found out that a very similar issue was fixed in XSPEC patch 12.11.0k: *For some complex fitting and error command cases, XSPEC may terminate with a GSL message such as 'SVD decomposition failed to converge'.* This wasn't encouraging at first because XSPEC patch 12.11.0k came out in June (I built the current version of xspec on my vm in ~July), but I checked my XSPEC version with *$version,* and it turns out that my XSPEC version is just 12.11.0! I'm working on installing the patch, I bet this will clear a lot of things up. I think I'm going to have to rebuild relxill after I execute the partial system rebuild.
> >
> > The only thing that confuses me about this is that I didn't really have issues with core dumping when I was fitting relxill a lot back in August (I think it did come up for error fits back then though), so I'm confused why it really started acting up recently.
> >
> > Also I don't think I've asked this previously, but out of curiosity, should I expect significant reductions in fit times with two core parallel processing?
> >
> > Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 22, 2020 at 10:52 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Just checking in to see how fits are progressing and if playing with those limits has allowed any headway against the crashes?

(Yes, whenever I "rm -rf " I try to take beat and check that I am not about to bilk the whole machine! With great commands-line tools comes great ...)

-Jack

On Fri, Oct 16, 2020 at 3:24 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

Now that I think about it, I do recall noticing that many (if not most) of the fit failures had gammas in the 3.4 range. Good to know about the 1.6-2.2 range for the residuals, I'll play with that now.

Oh about the attachments: the screen shot from visual studio is just some descriptions I was writing about the different functions I was using, and the microsoft paint one just illustrates the positive contribution that is almost always eliminated by a ~1.75 KeV gaussian of varying width.

Over this weekend should I continue to focus on maximizing the number of observations that I can get good fits and confidence intervals for?

I'm intrigued by this disk truncation controversy, I'm going to read about it this weekend.

Many thanks!
Thaddaeus

P.S. I was reading about some dangerous linux commands the other day; there are some unnerving commands out there. Imagine if someone ran rm -rf in your terminal. Shudder.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 15, 2020 at 10:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> Regarding the application of systematic errors, that's good to know. I've often seen something like the "NICER team recommends the application of a systematic error of 1%" in papers that use NICER data.

Yes, that's a "rough" benchmark, but a reasonable one.

> I love the common.tcl file, it's so handy! I think I'm just going to use it from now on because it's simpler on the execution end (just two commands), and it shows the error strings for each parameter.

Yes, it's quite nice work by Jeremy :).

A little random: I came across an interesting statement in a paper by Plant et al. 2015 pertaining to observed disk truncation in the hard state of GX 339−4: “This result implies that the current sample of spin estimates in the hard state are inaccurate. Therefore, any distinct conclusions drawn from these estimates, such as the spin-powering of relativistic jets, may well be biased.” Since that paper was published has this issue been fully exploited and resolved, or are there still black holes with possibly incorrect spin measurements out there? Also, is physical disk truncation or changing coronal height the more widely accepted cause for profiles similar to disk truncation?

This has been a topic with a lot of controversy that is still ongoing (Garcia et al 2015 and followup papers by Wang et al. 2019 talk about this). I'd say the Plant picture is strongly disfavored at present, but not entirely out. The robust view is that allowing for the possibility of truncation, a (bright) hard-state spin measurement should be regarded as an estimated lower limit. Presently, the argument is focused around how much truncation could there be in bright hard states, with most reasonable numbers being a few times the ISCO (a factor <10). (Coronal geometry is always an important consideration, but usually they tend to fit very compact.)

An update on the rsp/arf issue: I modified my approach and fully integrated astropy.io.fits, so when I’m producing my xspec script commands rmf and arf file names are pulled directly from the header of each observation’s fits file. Because of the two errors described below, I haven't checked how chi-square improves when the correct rmf and arf files are used though.

Very good, that should make some impact when working.

I’ve encountered two weird errors I’ve never encountered before in XSPEC this week: the first is frequent core dumping [SVD decomposition failed to converge…Default GSL error handler invoked…Segmentation fault (core dumped)], and the second is this odd “XSPEC Error: No variable parameters for fit” after executing “fit” for a model with *multiple* free parameters (including the usual Gamma, FracSctr, Tin, diskbb norm, etc.). My first thought was “*is this related to relxill having problems due to a low gamma”*—but these issues both happen even when Gamma is > 2.

The table can be sparse in a few places, and I think very large Gammas are also sparse (>~3.4, if memory serves). I'd say try freezing parameters and retrying the fit to identify the culprit parameter.

Also a little random: this week I decided it was time to start commenting on my functions in my python files—at one point I was looking through some older files for a particular technique I used and it took a little while to find the correct version of it, so I decided that from now on I’m describing my functions for future reference.

Yes, documenting code is super important and a great habit to get used to. (I wish I was better about it!)

This is preliminary, but I attached an example of a chi-scape scarred by a common morph of the residual I last sent you. The positive contribution that begins shortly after 1.6 KeV is almost always eliminated by a ~1.75 KeV gaussian of varying width for most observations. The current issue is modeling the various forms the following negative residuals take. Currently working on those. Earlier you mentioned that we could try ignoring that narrow range where these residuals occur, would ignoring channels between 1.6-2.2 KeV be ignoring too much of the data? If we decide to ignore that range, I would think it best to get >*plt wdata* outputs to exactly nail down the dimensions of this range, but for now should I keep trying to model the residuals?

I was reading through some older emails, going to look into your recommendation regarding logxi.

No, I think 1.6-2.2 isn't crazy and you can certainly roll with that if you are having too much trouble taming the residuals with a model. Maybe do a couple test cases to see how the model parameters are affected on both fronts.

Hey, and I didn't see a description of your attachments in case you want to say anything about those.

Glad you're back in the thick of things.

Best,

-Jack

On Thu, Oct 8, 2020 at 1:35 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi Dr. Steiner!
>
> Quick note: I've been getting back into it this week and should have some interesting things to report this weekend!
>
> Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Tue, Sep 29, 2020 at 2:19 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Thanks for the update.
>
> > Quick note on the residuals: since we last communicated I investigated the application of a systematic error (3-5%) in the < 2 KeV energy band, but this didn't improve the residuals so I'm assuming that you already accounted for this, like the binning, in your reprocessing?
>
> No, actually. But systematic errors are only incorporated with some fit statistics - chisq for instance, but I don't believe are incorporated in cstat or pgstat. The way I like to think about it is the systematic error acts as a reweighting of the data's constraint and serves to de-emphasize regions with the highest signal. I offer for information sake that that 3-5% is too large a systematic error to include as a rule. Something like 1-2% is more reasonable for a region with poorer calibration quality.
>
> > I also discovered that I've been unintentionally forcing an incorrect response file on multiple MAXI J1535-571 observations—the rsp I was using for all observations was `nicer_d49_55575341.rmf`, but after running a quick check (the fitsio package has been so helpful, thanks for recommending it to me!), I found that multiple observations request `nicer_d49_u100_150_55575341.rmf` in their headers.
>
> Ah! That's very good to know, and may be the culprit for some of the residuals you've been battling. Check that the arf files (keyword ANCRFILE) is also set correctly.
>
> > I'm likely almost done with the SAT (took it Saturday, going to take it again this coming Saturday)*, and once I'm done I aim to return to a progress pace similar to the pace we had in the summer.

Congratulations - that's a nice milestone!

> G'mar chatima tova (if you observe Yom Kippur)!

Same to you.

> *I might take it again in November in the event that I bungle those tests.

I'd put dollars to donuts you did great!

My best,

-Jack

> On Tue, Sep 22, 2020 at 1:21 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> > Interesting I'll look into that!
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
> >
> > On Tue, Sep 22, 2020 at 1:15 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> > > Similar to that. I used another language (IDL) to automatically produce a series of tcl/tk scripts and then executed those distributed over a number of cores on a workstation.
> > >
> > > -Jack
> > >
> > > On Tue, Sep 22, 2020 at 1:13 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> > > > Sounds good!
> > > >
> > > > In that case did you operate solely in TCL?
> > > >
> > > > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > > > -Enrico Fermi
> > > >
> > > > On Tue, Sep 22, 2020 at 1:10 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> > > > > Hi Thaddaeus,
> > > > >
> > > > > For >75% I meant observations where source flux was more than 75% bg flux, and for 4% I meant observations where source flux was ~4 % bg flux. That makes sense—interestingly the observations with source flux ~4% bg flux were, by best fit, in the SPL state, with the highest flux and gamma for all the observations that had reasonable values for these parameters.

Ah, now I see. Yes, when the bg flux is very high, the noise level (N=sqrt(B+S)) where B and S are background and signal counts in a channel, then the constraining power of the data (S/N) is lowest. If the background is reasonably estimated, the fits will likely be good but poorly constrained; if the background is off, then all bets are off.

> xD. I think it's a very impressive feat! Did you use pyxspec to loop through all the observations?

Thanks. I used a different language actually, but python/pyxspec would be an equally good way to do it.

Best,

-Jack

On Tue, Sep 22, 2020 at 12:39 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> That's good to know. I guess what's surprising is some relative of this large residual in the 1.6-2.0 KeV is present in most of the source flux > 75% observations (I wonder if the peak feature falsely inflates the source flux for some of them?). Going back to a couple emails ago, I was checking some of my all time best fits which had source fluxes around ~4%—what allows such faint observations to fit well?

Just to clarify, do you mean 75% and 4% of the Eddington limit, peak luminosity, or something else? Fainter observations, given some typical observation time, obviously have fewer counts which means larger fractional errors and less constraining power on the model. So if there is a deficiency in the calibration or model being used, it will look more prominent in brighter + long exposure time data.

> Also, I'm very intrigued by your methods in one of your 2016 papers , "STRONGER REFLECTION FROM BLACK HOLE ACCRETION DISKS IN SOFT X-RAY STATES". How did you automate fitting the 15,000 observations reliably and within a reasonable time frame?

A reasonable time frame is relative :), but I used about two dozen cores to run the fits which helped. I also stuck with computationally simplistic models, and for my runs with simpl, used lower resolution (via energies) in the run, since RXTE/PCA - the instrument used there - also has rather poor energy resolution compared to NICER.

> Could I possibly have the ~86 observations between obsids 1200220101 – 1200220186?

Spectral files are attached. Response files are too big for me to attach, so I'll keep them up here for a few days: https://nicer-web.mit.edu/nicerview/thadd_resp_m1727.tar.gz

> Ah that makes sense. In that case did Xu et al. just freeze Rin at the ISCO to have less free parameters?

That's the usual tactic, yes, when fitting for spin.

Best,

-Jack

On Thu, Sep 17, 2020 at 11:00 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Great to hear you've been digging in and exploring! I would suggest though that you avoid being too concerned about the residual features. If something is localized and only ~percent level deviation, then that's likely sufficient. *The goal should be to fit those sufficiently that they don't alter the continuum, but at some level it becomes diminishing returns to try fitting an artefact.* That said, if you wish, I can get you the data on MAXI J1727-203. Just let me know if you have a particular ObsID from Kevin's paper you want to try.
>
> Your question about degeneracy is great. The condition of parameters being strongly correlated with one another is degeneracy. Basically, this is a very common situation where two parameters are dependent upon one another to some extent, so a probable value of one will condition a probable range of values with the other. For instance, if you want to fit x and y with the constraint x^2+y^2 = 4+/-1, you would get a ring of radius 2 with x,y values that obviously depend on one another; they are *not independent*. Speaking generally for more realistic cases, the interaction between parameters is often pretty convoluted, but commonly things like Gamma and Normalization are correlated with one another, so a contour plot of the confidence interval would look banana shaped rather than like a circle.
>
> Best,
>
> -Jack

On Thu, Sep 17, 2020 at 9:18 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> Thanks for checking in! Since we last spoke I've been taking a deeper look into a couple articles (Alabarta et al. 2020, Xu et al. 2018, and Parikh et al. 2019) and playing around a bit with the residual fix you suggested. As tested on multiple observations with varying fit statistic contributions in the 1.6-1.8 KeV range (chi-squared contribs. ranging between 500-3000), the gaussian centered around 1.75 KeV combined with the "edge" works great!
>
> Most importantly, I found a method to more accurately investigate the residual features: *plt>wdata*. Now that I can actually see what the chi-sq contrib. is for every point, I've been having a lot more success in approaching residuals in the 1.6-2.2 KeV range, especially when the residual patterns after 1.8 KeV differ greatly from what can quickly be ameliorated with the edge feature (it seems that for the majority of observations the <1.8 KeV excess is easily subdued by the gaussian fix, but after 1.8 KeV things often get complicated, especially when a third feature shows up between 1.86 to 2.0 KeV. This third feature resembles a sort of gaussian-like crevice about 0.13 KeV in width and often reaches a chi^2 contribution of -5331 (+) at its minima, pushing the "edge" feature out to a threshold energy of ~2+ KeV). I haven't found a definitive solution yet, but a gaussian absorption with an energy of ~1.86 KeV and a width of ~0.14 KeV is looking promising.
>
> While I've been working with these MAXI J1535-571 residuals I've been wondering if the residuals Alabarta et

al. discuss in Nicer data for MAXI J1727−203 are of similar shape—is there any chance I could procure some pipeline data from you on MAXI J1727-203 to look into this (if it's not an inconvenience for you and or you don't think it's a distraction on my part of course).

Out of curiosity, what does "parameter degeneracy" mean? My first guess would be this example: if I only told you what the slope of a line was (e.g. '2'), and then asked you for the individual values of Δy and Δx—would Δy and Δx be degenerate? Example of where I came across this: (from Xu et al. 2018) "The inner disk radius, R_{in}, and the black hole spin parameter, a*, are degenerate, as they both control the effective inner accretion disk radius. Therefore, for simplicity, during the spectral fitting we assume that the inner disk extends down to the ISCO by fixing R_{in} at the radius of the ISCO, and fit for the black hole spin as a free parameter."

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

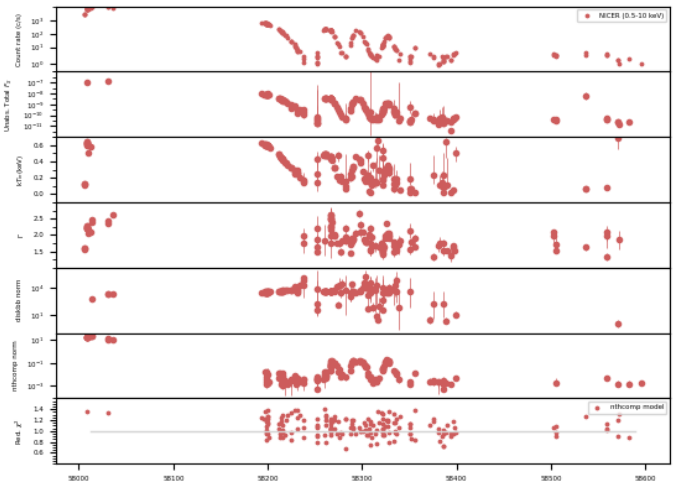
Kiker, Thaddaeus <s024622@students.lmsd.org> Tue, Jan 19, 2021 at 6:34 PM
To: "Steiner, James" <james.steiner@cfa.harvard.edu>

Hi Dr. Steiner!

Phew sorry it took me a bit to get back to you on this!

> This is great, but I do have one request: it would be helpful to have the normalizations plotted in log-scale as well (to better see their dynamic change).

Done (plot below). nthcomp norm looks interestingly correlated—but what is nthcomp norm exactly?



> Excellent - these look really great. It appears to me that there are some points shown in the simpl panel that aren't shown in the nthcomp panels, and vice-versa. Can you mention what the screening difference is? (e.g., around day 55030)

I'm fairly confident I used the same reduced. pgstat cutoff for both the nthcomp and simpl models, so I'm wondering if those particular gti's that fitted well for simpl didn't fit well for nthcomp around that point.

> To check: I take it restriced_bg_counts_list, and analogous for the data are only using the noticed ranges? If so, all looks good!

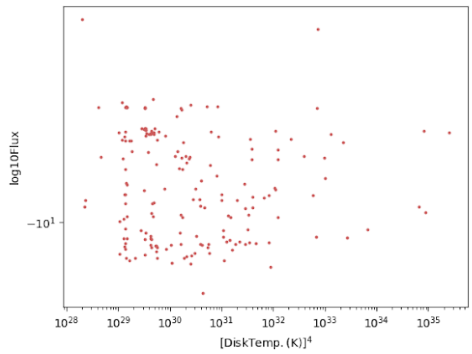
Correct (yay!).

> For a disk-blackbody, the expectation is that L ~ T^4, so you can plot L vs T on a log-log plot and look at that scaling.

So I've gotten started on this. The below plot is obviously not nice (I haven't modified the cflux log10Flux values to luminosities yet), but I think I have my temperatures in K correct. This was the method I used:

```
b_const = (1380649/16021766340) #Boltzman const in $eV \cdot K^{-1}$
tin_mod = 1000*(1/(b_const*1))
print(tin_mod)
#Checked this with David Cohen's website which states $1 keV \approx 11.6 \cdot 10^{6} K$
```

Plot:



I used the lone "1" in $b_const \cdot 1$ to line my transformation up with David Cohen's website's statement that 1 keV is approximately equal to $11.6 \cdot 10^{6} K$. In the final code I just substituted an array of diskbb Tins for that "1". I think this will scale nicely when I transform the log10fluxes because I remember this bhb's luminosity was somewhere in the $10^{28}-10^{39}$ range (I just looked back at an old email—calculated a luminosity of 1.2067*10^38 ergs/cm^2/s for one observation so sounds right).

Still working on plotting the values that were consistent with zero within errors (I know how I want to do this, I just have to make the changes to my code) and standardizing / documenting the handy functions I wrote up for this effort.

Cheers!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Fri, Jan 8, 2021 at 10:00 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Ah, I remember an ice-cream fueled haze of recovery in the aftermath. Hope you are getting some of the same!
>
> -Jack
>
> On Fri, Jan 8, 2021 at 8:38 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
>
> > Hi Dr. Steiner!
> >
> > I was a little out of it this week after getting my wisdom teeth pulled, just wanted to let you know I've seen your email and I'm going to work on it this weekend.
> >
> > Cheers!
> > Thaddaeus
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Tue, Jan 5, 2021 at 5:23 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hey Thaddaeus,

Wow, that's an impressive and sweet-looking delivery!

> Thank you for your well wishes! I hope the same for you and your family as well!

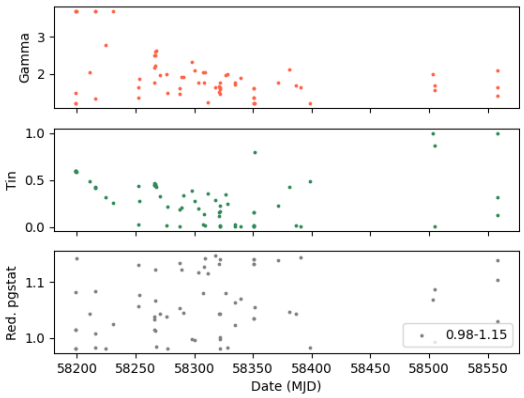
Thanks Thaddaeus, we're doing just fine and couldn't be happier to turn the page on 2020.

> First, a thought back on the relxill models: don't nthcomp and simpl produce different values for gamma and hence they should not be mixed with each other? This thought came to me this morning when I was tidying things up. Since I was tieing relxill's gamma to simpl's, I wonder if that threw anything off. Now that I think about it more, I don't think it would catastrophically derail things, but it would still be inappropriate to tie gammas from different models right?

The default answer is that these should be identical - they both describe the shape of the Comptonized power-law component, and in the same way. For completeness, I should mention there has been some small exploration about decoupling these things with some loose physical arguments, but basically that's a stretch and I would absolutely keep them linked up as you've done.

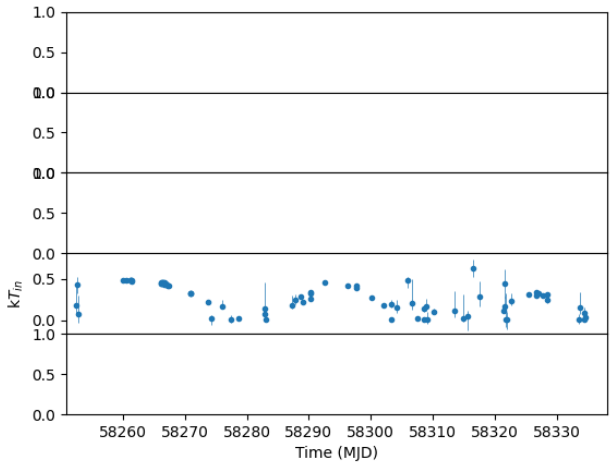
> The first hint of a breakthrough happened on Tuesday when I realized I forgot to filter for background fraction. I looped through all the data files that contributed to the last plot I sent you and only plotted fits from files where background was < 50% source, giving me this, much neater, plot (where gamma and inner disk temp are actually somewhat correlated:

Ah, yes! Glad you caught that; keeping to the cleanest data is a very good idea, and 50% is reasonable; if things look at all funny, even down to ~20% would be a reasonable place to cut.

> 
>
>
> This cleaned things up pretty well, but I noticed there were only a couple points with gamma > 3 (all of which were right on 3.5), so I ran another routine with the upper limits on gamma set to 3. The points that were right on 3.5 were on 3.0 for gamma in this routine, so I wondered if it wasn't an issue of the upper limit but an issue of insensitivity. When I tried to estimate 1-σ errors on a couple of the fits that had 3.5 upper limits, XSPEC returned "***Warning: Parameter pegged at hard limit: 3.5"—which I take as being caused by gamma's insensitivity to the fit in the observations.

It is possible this is because the fit isn't sensitive to Gamma. (You could check this out by comparing fit statistics to the upper-limit 3.5 vs the upper-limit 3 fits, or also looking at the f_sc values - <~ 0.01 being an indication that isn't sensitive.) But alternatively, it's also possible that the data are sensitive to Gamma and either (i) the spectrum is very soft, wanting high Gamma in which case lowering the upper-limit would still cause it to peg but the fit would be markedly worse or (ii) *my guess for most likely for these data*, the background subtraction is off in those cases and oversubtracts, creating an artificial appearance of a very soft spectrum.

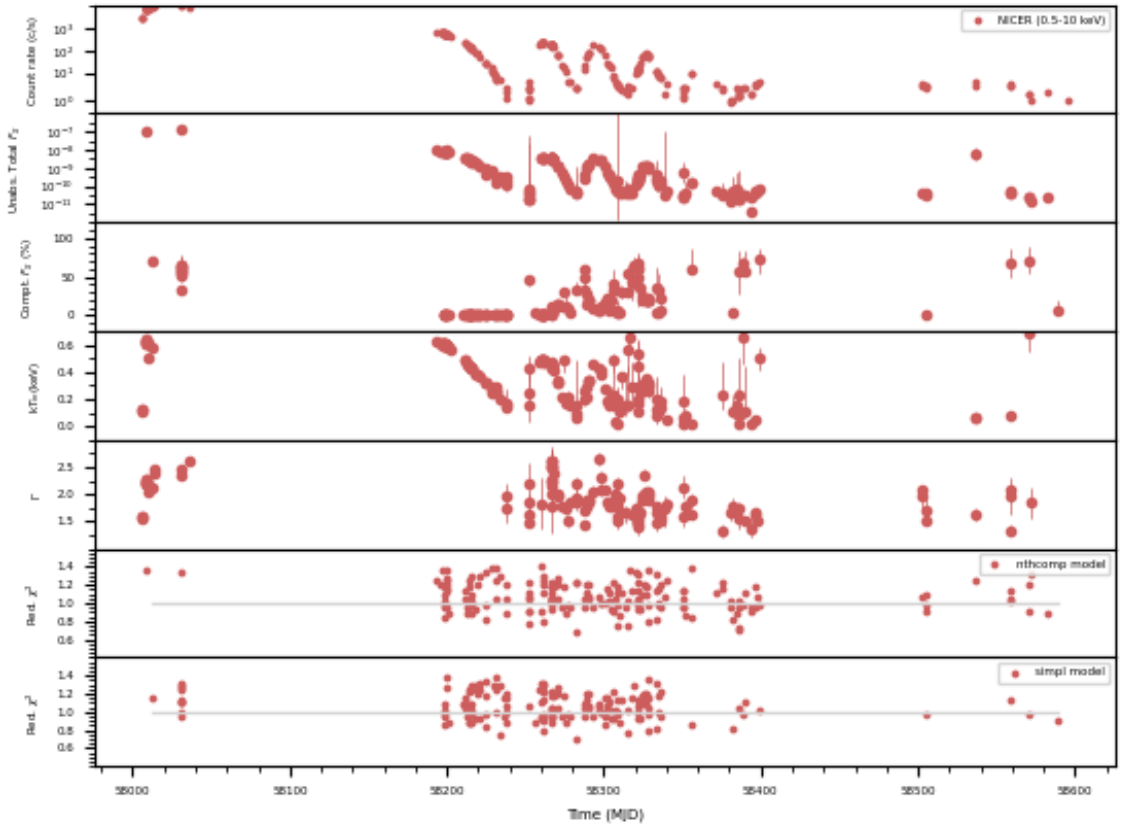
> Then I plotted Tin measurements from that fit routine with gamma's upper limit set to 3.0 with one sigma (all confidence levels I bring up are one sigma unless otherwise noted, as in Cuneo's paper; though I must say, one sigma sounds so much better than 68% confidence even though they're the same thing lol), here's how that turned out:



> Pretty nice, eh?

Yes, this looks *very nice*!

> So fast forward (and a bunch of partial fitting routines later), I'm proud to present these:

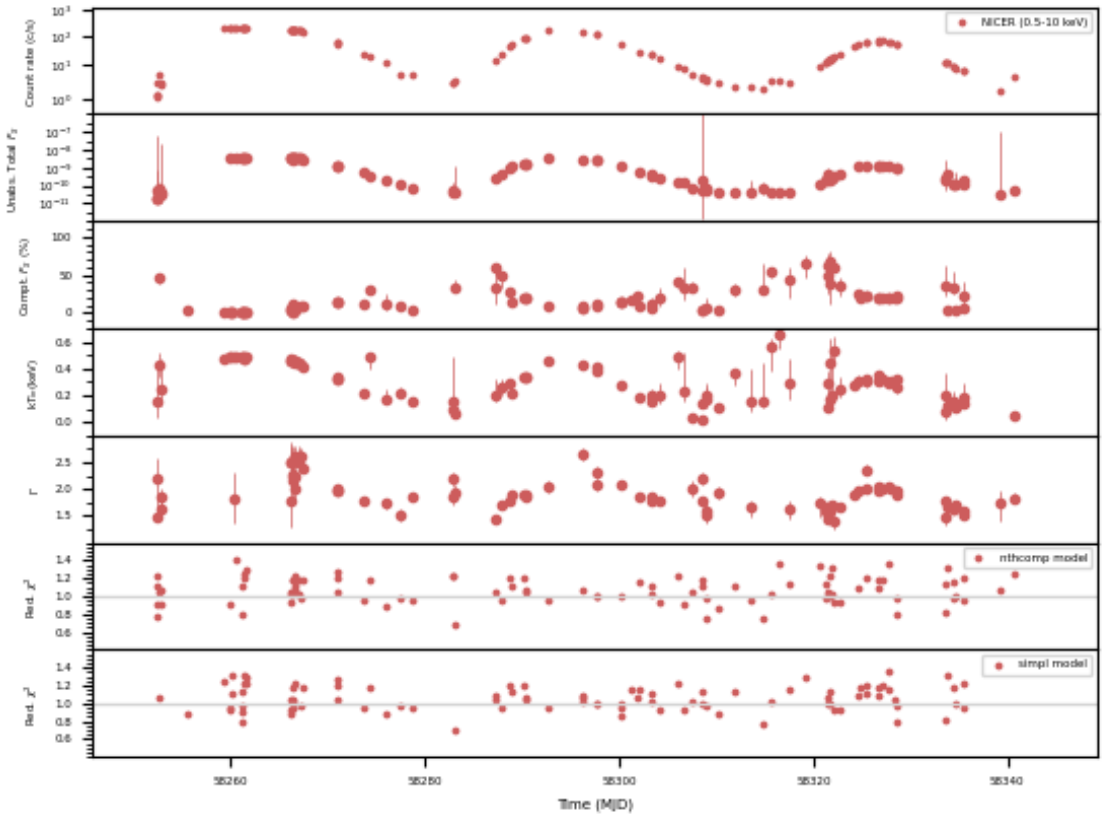


> This is my final plot for the MAXI data. The top panel is background subtracted count rate (a note on this later), the second is unabsorbed total flux on the range 0.5-10.0 keV, the third panel is comptonized flux percentage (calculated with a separate simpl model, a note on this later), the fourth inner disk temp, the fifth gamma, the sixth reduced pgstat for the nthcomp model, and the seventh the reduced pgstat for the simpl model.

Excellent - these look really great. It appears to me that there are some points shown in the simpl panel that aren't shown in the nthcomp panels, and vice-versa. Can you mention what the screening difference is? (e.g., around day 55030)

For a disk-blackbody, the expectation is that L ~ T^4, so you can plot L vs T on a log-log plot and look at that scaling.

> Here's what my plot looks like during the reflaring period investigated by Cuneo:



All parameters were calculated with the nthcomp model except for comptonized fraction. Model descriptions below:

##Model One##
Model: tbabs(diskbb+nthcomp)
Delta fit statistic: 1.
Noticed Ranges: 0.5-1.5 2.3-10.0 keV
nH: 3.2 (frozen)
Tin lower limit: 0.01
Tin upper limit: 1.0
Gamma lower limit: 1.2
Gamma upper limit: 3.0
High energy rollover: 100 keV
inp_type: 1
Redshift: 0

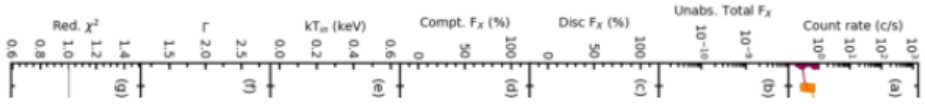
##Model Two##
Model: tbabs(simpl(diskbb))
Delta fit statistic: 1.
Noticed Ranges: 0.5-1.5 2.3-10.0 keV
nH: 3.2 (frozen)
Tin lower limit: 0.01
Tin upper limit: 1.0
Gamma lower limit: 1.4 #I adopted this limit from your paper in which you used simpl to fit those 15,000-odd observations.
Gamma upper limit: 3.4 #I adopted this limit from your paper in which you used simpl to fit those 15,000-odd observations.
UpScOnly value: 1

Looks good.

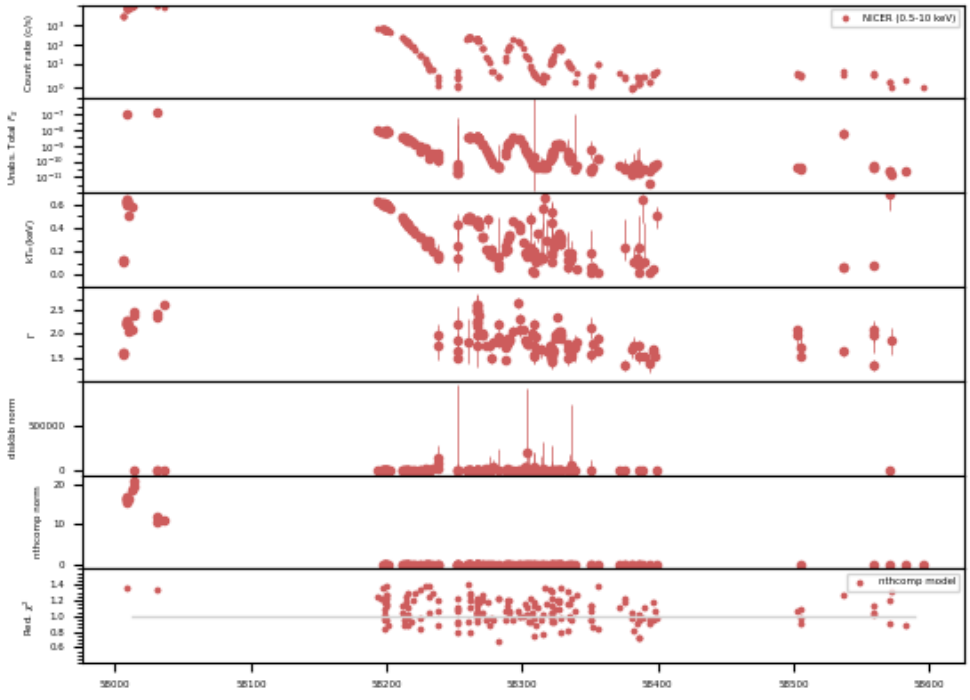
****
Per the background subtracted count rates, I corrected my code to account for your pointers; the important snippet is below:

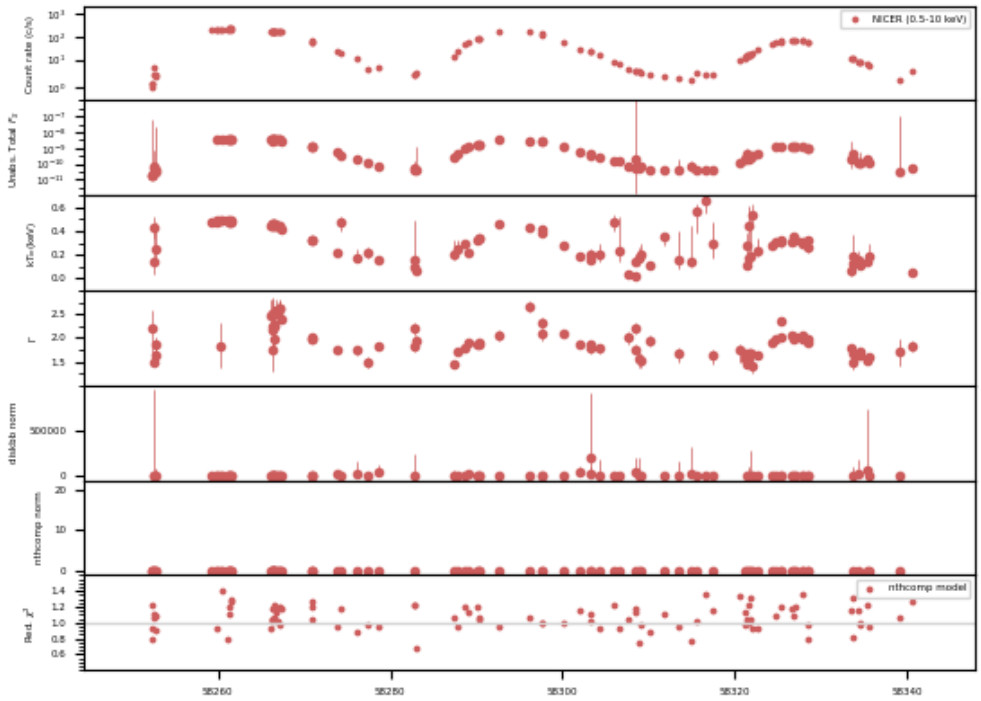
```
scaled_bg_counts = (sum(restricted_bg_counts_list)/bg_exp_time)*exp_time
count_rate = sum(restricted_data_counts_list)-scaled_bg_counts
count_rate = count_rate/exp_time
os.remove(temp_data_file)
os.remove(temp_bg_file)

out_list.append((seg_id+':'+str(count_rate)))
```

To check: I take it restriced_bg_counts_list, and analogous for the data are only using the noticed ranges? If so, all looks good!

> Oh also, a small note on my final plots. I think I found out how Cuneo prevented ytick labels from colliding:
>
> 
>
>
> I think she set the ylim bounds somewhat lesser and somewhat more than the min(y) and max(y) vals—so I did this and it improves the aesthetics significantly in my opinion.

Yes, this is a nice way to keep the plot clean. (And for x-ranges too.)

> As you requested, here's a plot with the diskbb and nthcomp norm components:
>
> 
>
>
> (Full range)



(cuneo date range)

This is great, but I do have one request: it would be helpful to have the normalizations plotted in log-scale as well (to better see their dynamic change).

> I didn't include values consistent with zero within errors in any of these plots, but I can if you'd like.

Sounds reasonable to me. Can also try plotting them with a different (less prominent) color or symbol as an alternative.

> Right now the raw outputs from my returnConfidenceIntervals() function look like this:
>
> PROBLEMS OUTPUT DEBUG CONSOLE TERMINAL
>
> ```
> 1130360284_1:PHLZ:0.591738
> 1130360284_2:0.249281:0.334173
> 2130360201_3:0.107522:0.107773
> 2130360201_4:0.257621:0.200038
> 2130360201_5:0.36142:0.204905
> 2130360202_0:PHLZ:0.116443
> 2130360202_1:PHLZ:0.718751
> 2130360203_0:0.295584:0.279007
> 2130360204_1:PHLZ:0.727953
> ```
>
> (where PHLZ stands for Parameter Hit Lower Zero and PHUZ stands for Parameter Hit Upper Zero)
>
> Early in the week I thought about changing the output of this function to some kind of multidimensional numpy array like this:
> `[['seg_id_1','seg_id_2'],[lower_1,lower_2],[upper_1,upper_2]]`
>
> But I didn't really feel like it because the current function works fast enough for me (I don't ~~see the need to vectorize~~ (more accurate statement: feel like vectorizing) all my functions yet; unless you would advise me to in which case I'll get right to it lol). *

Ha - while it can absolutely make a big difference in some cases, it sounds like from below you've found those cases where it really matters. Usually when memory juggling big arrays you want to do that once and avoid repeated calls in that vein, and otherwise small transformations of scalars there will be plenty of situations where it looks much nicer but won't make much practical impact.

> I did write a standard function for returning float values of value date, value and a two dimensional array for yerr for parameters based on an input x=the parameters output from returnConfidenceIntervals(), y=theoutput from a returnDates() function I wrote a bit ago, and z =

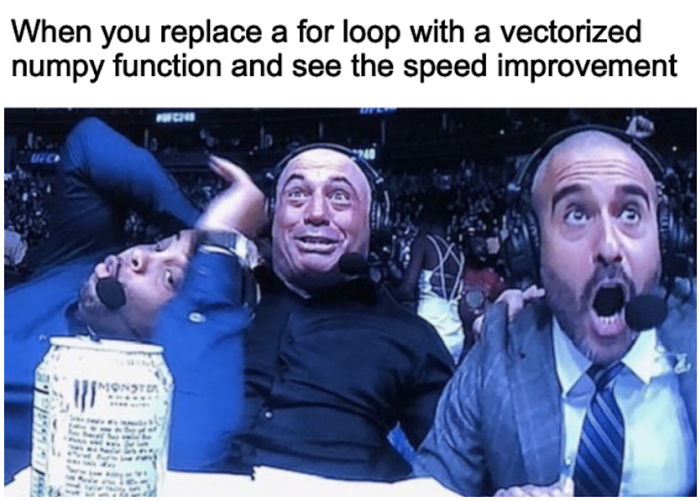
> an output from the returnFitResults() function I wrote a bit ago. Lol reading that last sentence, it looks a little confusing so I'll add this function to my package and document it in markdown and show it to you in a later email.
>
> I still have to iron out my hardness ratios function, will get to that soon.
>
> A random, last thought: in compsci club at school we've recently been discussing algorithmic efficiency in the context of things like sorting algorithms, which inspired me to think about this in the context of my code. So far I'd say a noticeable improvement that has resulted from this is that I'm limiting the number of computationally expensive tasks I put under for and conditional loops as long as I can execute them outside the loop. An example of this would be declaring a scipy.interpolate.interp1d object outside of a for loop rather than declaring it for every data value in a set (this came up in something else I was working on, but I brought it up because I think it's a good example).

Absolutely, this is just the kind of case you want to call once only.

> All data referenced in the plots, the python file I wrote for creating these plots, and the python file I used to write the fit commands are all attached in the email_attachments .xz tarball.
>
> Cheers (and Happy New Year)!
> Thaddaeus

Wonderful work Thaddaeus! And Happy New Year!

-Jack



"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Dec 31, 2020 at 2:38 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> It's been quite a month everywhere! Hope you had a nice holiday season and that you and your family have been doing well. Wishing you a happy and better New Year ahead.
>
> -Jack
>
> On Wed, Dec 9, 2020 at 2:57 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
>
> > Hey Thaddaeus,
> >
> > Those parameter plots look great! Can you add in the diskbb and compTT normalizations as panels as well?

It looks to me like things are probably going wrong because you are probably using bin-number rather than channel number. Sometimes these two get conflated, but here by bin I mean groups of one or more channels, where a channel is the thing that is hard-coded in the spectral file (and PI-bins correspond to 10 eV increments). So you'd want to use channels 50-299 and 300-999 for your two bands.

The other thing to check is that the background is scaled properly. So you want to make sure that to background you subtract is:
BG_Spectrum_MatchedToData = BG_Counts_FromBGFile / BGFile_ExpTime * DataFile_ExpTime

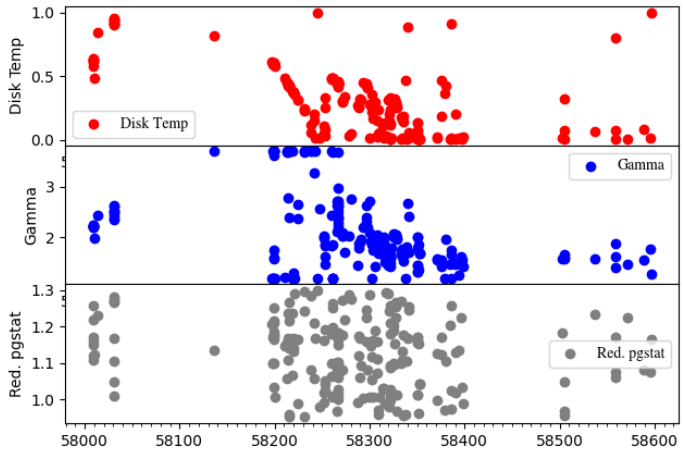
Hopefully my shorthand there is clear enough.

Best,

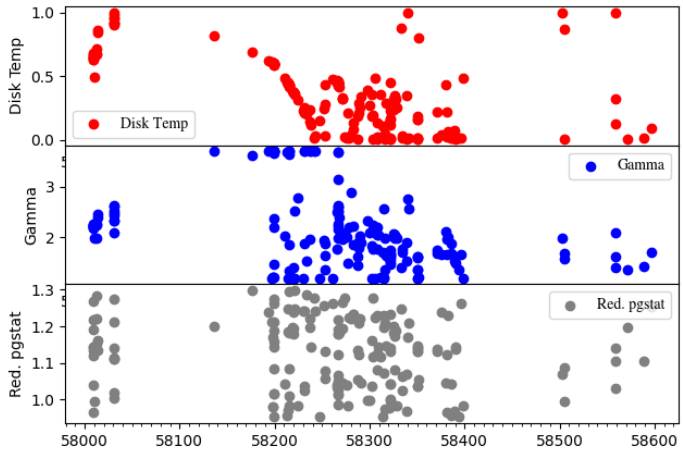
-Jack

On Wed, Dec 9, 2020 at 2:39 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi Dr. Steiner!
>
> Phew! The number of potential files for fitting has shot up to 720 from 185 now that I'm searching for and including all GTIs files.
>
> I ran a couple fitting routines with a simple tbabs(diskbb+nthcomp) model on all the files just to get a feel for how including a lot more files would possibly change things, and the initial results from these fitting routines were quite unusual (though they were blisteringly fast compared to relxill fits regardless —with nH frozen on these routines, I was fitting all the files with 700 fit iterations each in under an hour on my mom's computer) :



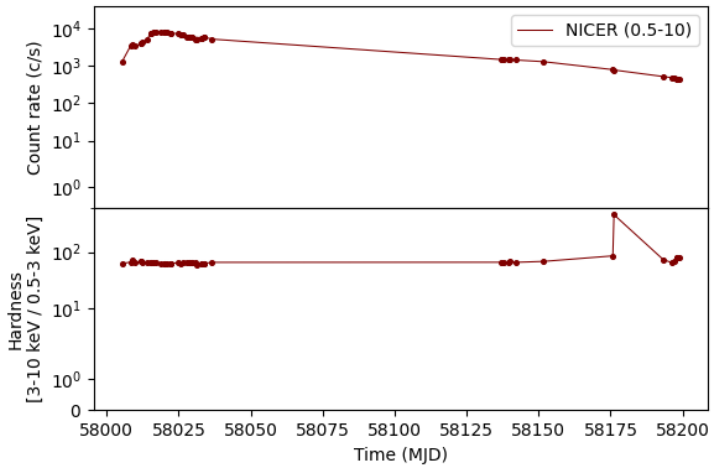
> ^ That routine had diskkbb Tin upper limit set to 1, gamma upper limit set to 3.7, nH frozen to 3.2, and the entire energy range from 0.5-10.0 keV noticed.



> ^That routine was the same except the energy interval 1.5-2.3 keV was ignored.
>
> I think I see a little pattern of correlated variation between disk temp and gamma masked under the erratic points in both plots...I'm wondering, should I set some cutoff exposure time for the files? I recall seeing some ~22 sec exp. time data files when I

was looking through other GTIs, and I'm thinking since these files would have less data to constrain the model, crazy parameters can be permissibly returned by XSPEC when these files are fit.

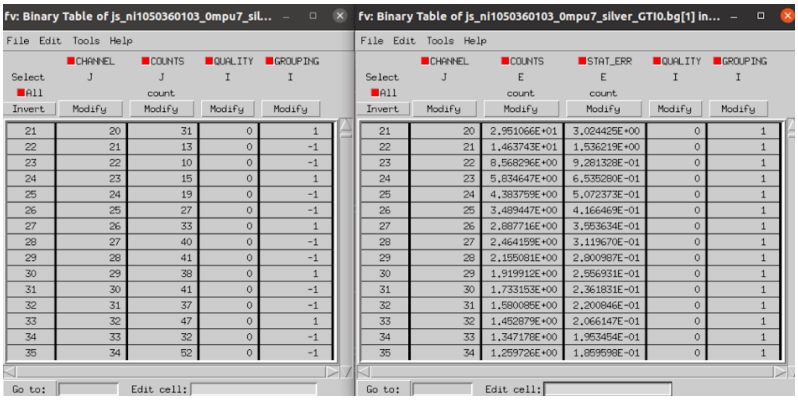
Also, could I get your advice on calculating hardness ratios? I modified my count rate function to correct for background counts and it's working pretty well (see the top subplot below):



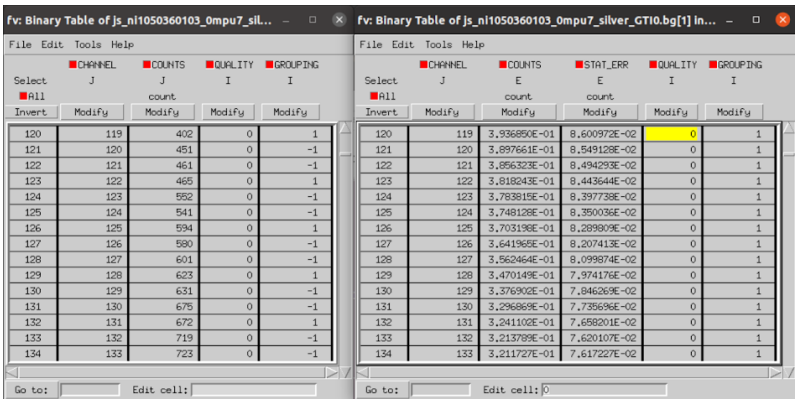
Almost identical to Cuneo's count rate plot, eh?

As you can see in the bottom subplot, the hardness ratio values are crazy, and I can't figure out how to rein them in. The way my function for hardness ratio works is I sum the number of counts in the parameter defined hard band and subtract the number of background counts in the predefined hard band from that value, giving me a net hard band counts sum. I then divide that value by (sum of soft counts-sum soft background counts). Code is attached.

I was checking with fv, and it's interesting that in this case (and what I think is true in general) the bg counts are very low in the hard band but fairly high in the thermal:



Channels 22-99 = 0.5-3 keV



Channels 101-254 = 3.0-10.0 keV

Cheers!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 3:36 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> > Thanksgiving was very nice, thanks!
> Yay that's great to hear!
>
> > Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.
> Sounds good.
>
> > YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.
> !!! I will do this then!
>
> > Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)
> Will do.
>
> Thanks!
> Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 12:28 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Thanksgiving was very nice, thanks! Glad yours with the grandparents was too.
>
> > I reprocessed the data last weekend (11/22) but the results were a little weird so I've been tweaking some things and prepping for reprocessing again this week.
> >
> > I was wondering if I should take a different approach to the instrumental residuals by running a preliminary fit on all files and logging the fit statistic landscape, so I can then loop through and identify which files actually require the 1.5-2.2 keV range to be ignored and which can have it noticed (I'm wondering about this because from a preliminary check a lot of the disk temperatures were behaving weirdly, which I think could be related to having less data in the thermal band to constrain them). What do you think?
>
> Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.
>
> > Also, I've only been fitting the silver, zero good time interval files; would you recommend me to fit some of the GTI1s, GT2s, etc. in hopes to get more good fits?
>
> YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.
>
> > P.S. I wrote two new standardized functions (one calculates and returns count rates, the other calculates and returns hardness ratios) and nice documentation for all my standard functions so far.

Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

-Jack

On Mon, Nov 23, 2020 at 8:34 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Will do!

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 23, 2020 at 4:30 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Sounds good! Glad your mom's computer has some CPU cycles to share. Keep me posted when you get the results in hand. Meantime, happy Thanksgiving.

-Jack

On Sun, Nov 22, 2020 at 8:01 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

Yes I've been fitting with the inner radius frozen to -1. That's good to know.

> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind. So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

Sounds good.

> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Yes I agree it was nice that they showed the underlying component.

I've been running the first iterations of the reprocessing last night and today and I've had a little breakthrough—last night I quickly updated the version of XSPEC I had on the vm that I installed on my mom's computer way back in June (I also installed relxill on it), and I've been dividing the work between her computer and mine—the routines are concluding much quicker (her computer has four cores xD).

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Fri, Nov 20, 2020 at 9:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind.

So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

And in response to your email yesterday:

> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

Yes, spot on!

As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Best,

-Jack

On Fri, Nov 20, 2020 at 8:13 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Shoot! I forgot this bring this up. Would you recommend me to freeze the spin and inclination parameters to published values (that are in agreement with some of my previous best fits) to simplify the parameter space, or should I keep them free? The only benefit I can see from keeping them free would be sometimes error locks up with no variable parameters to use when free parameters get pegged due to their insignificance to the fit, and I think having them free could help prevent this...though spin and inclination seem like parameters that would be the first to get pegged during an error routine anyways.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Nov 19, 2020 at 8:07 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hey!

> Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.

Yeah, safety is (rightly so) the first priority.

> A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

Oo sounds cool, I hope you all have fun!

> Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

Perfect.

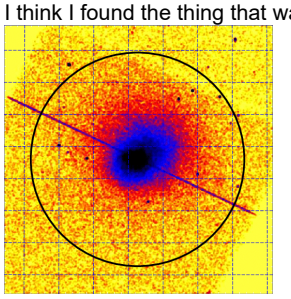
> The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon.

Interesting.

> then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

Very interesting.

I think I found the thing that was on the edge of my memory: a Chandra image of the quasar H1821+643.



So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

> Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

I should probably rephrase what I said earlier. When I ignored bad, no channels were ignored so I guess that meant no poor quality flags were raised.

> I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Agreed. I find it so enjoyable: the initial encounter with a seemingly insurmountable challenge, the brainstorming, the troubleshooting, and *especially* the successful execution of a solution. It's extremely satisfying—and to use an analogy from my youth* it feels kinda similar to when I would build things in lego.

*I say an analogy from my youth but that's somewhat misleading. I still occasionally play legos, and these days the only difference is that I have a partner: my younger brother ;).

> After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Definitely. That is exactly what I'm thinking.

> Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

:)).

Best!
Thaddaeus

Oh I almost forgot. I don't remember how/why I came across this paper (Arumugasamy et al. 2018), but after looking at the plots I've been meaning to ask you about them:

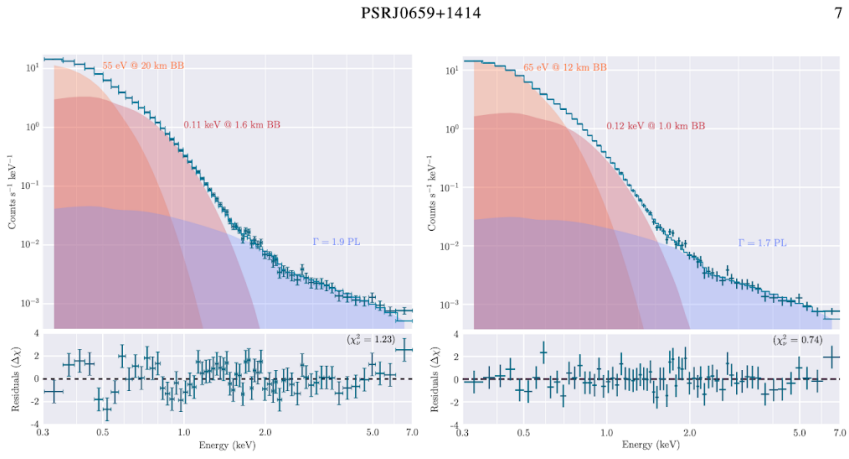


Figure 4. 2BBPL model, without (left) and with (right) `Gabs` absorption component, fit to the phase-integrated spectrum of J0659+1414. The model parameters correspond to maximum posterior probability, which in the case of G2BBPL are different from the distribution medians listed in Table 2.

I've been using the darkgrid style (like the plots above) with the rc font parameter set to Nimbus Roman in most of my plots ... but after seeing these plots I'm thinking I was to switch back to the mpl defaults—these plots are kinda distracting to me in their aesthetic.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Wed, Nov 18, 2020 at 10:05 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> It was a wild week indeed—and things seem to keep getting crazier. We were in a hybrid model since October 19th, but as of Tuesday we went indefinitely back to all online which is a two sided coin. On one hand the online schedule gives me a lot more free time, but on the other I'm going to miss the extra class time in my harder classes like mechanics and calc. Thanksgiving is around the corner though! Up to any interesting plans yourself? I think we're just going to have my mom's parents over because we already interact with them frequently, but no big gathering this year (obviously).

No kidding! Feels like we're living in a bad TV show.

Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.

A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> Would it be okay if I emailed Dr. Cuneo about this? This is very important for how I structure the confidence intervals code (and whether I make it more sensitive and able to detect more than the four generalized abnormal scenarios described later in this email).

Sure thing, email away.

> Haha! I just read that NuStar's is ~2.5 *milliseconds.*

Yeah and that's actually not too bad compared to most things out there. NICER is just an absolute timing monster.

> I do have my pipeline correct for it automatically, so all your spectra already factor that in.
>
> Cool! Does that mean I can just go ahead and calculate count rates as number of counts / exp. time and not worry about adjusting the result because you've already accounted for deadtime?

Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

> deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.
>
> Yes. This makes sense. I feel like I've seen something related where the center of an object was too bright in the X-Ray band (which oversaturated the detector?) so the color gradient of brightness was black in the center because the author's couldn't determine the brightness at the center. It kinda looked like an artificial coronagraph. Would that be related to a situation where the dead time was more than the event time?

(Deep dive warning:)
Yes, you're speaking about "pileup" which isn't exactly related to deadtime except that both can be important when the source is bright. Deadtime, again, is from the detector shutting down for some portion

of the time. Pileup concerns are most often for an imagng type of detector - more like the camera on your phone - that collects an exposure for some amount of time and then read it off.

Chandra, XMM-Newton, Swift, Suzaku, (somewhat confusingly NuStar is in both camps), let X-rays come in from the sky, get focused onto the CCD camera, and then in some time interval, often ~1s, reads off the image and clears the CCD. The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon. So when an individual pixel gets << 1 X-ray per frame on average, you are in good shape. *But* when there is significant probability of getting >~1 X-ray per frame on a given pixel, then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

This registering of multiple X-rays incorrectly as one more energetic single event is pileup, and it is the bane of most imaging detectors for those of us working with bright sources. (Pileup can occur in instruments like NICER too, when two X-rays hit during the event-latch phase, but this is something like a <1% effect for a 100 Crab-bright source, so well in the camp of being negligible for all practical purposes.)

In the most extreme cases of pileup, the pixel digitizer, which usually records values on something like a 16-bit value (e.g., from -32768:32767), can roll over and then appears negative when too much charge hits at once.

> I'm pretty sure the RXTE data I was working with earlier this year didn't have this identifier because if I remember correctly I couldn't execute *$ignore bad* on the data in XSPEC.

Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

> Here's a working version of the confidence interval / error results script:
>
> I put my XSPECtools.py file which has all of these functions into a custom module in my local site-packages folder for python and it seems like it's working perfectly. I import the file like this: *from personalastropy.xspectools import XSPECtools as xt*, and I execute a particular function like this: *xt.returnFitResults()*.
>
> Happy early Thanksgiving!
> Thaddaeus

That's really great Thaddaeus! I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Very well done!

After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

Best,

-Jack

> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Wed, Nov 11, 2020 at 12:07 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hey Thaddaeus,
>
> What a week it's been eh? (But way to go PA!)
>
> > I've held off from running another major fitting routine since the weekend because after I ran that my last one I decided that it was time to write some standardized scripts for wrangling the results from all the log files.

Excellent!

> Speaking of which, when Cuneo uses green triangles for "*parameters consistent with 0 within errors*", does this refer to situations when either the upper value of a parameter is equal to zero (because it got pegged at the hard upper limit) or when the lower limit is invalid and equal to zero?

Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).

I *love* the outline of your routines, and "wow" is what I have to say about that. Way to go Thaddaeus, it's great you're tackling this and in a very useful manner.

> Out of curiosity, where did Cuneo get the count rate values for his data? I don't think it's the same as the Net Count Rate that's displayed whenever you execute *$show data* in XSPEC....can Cuneo's count rate (c/s) be found by dividing the sum of the counts in the included channels by the total observation time?

About 99% yes. "Usual" practice for NICER has become to normalize to an active set of 52 detectors. Your spectrum is a collection of all good events. So a tally of all the (good) events between 0.5-10 keV divided by the exposure time is indeed the count rate detected. However, if only using 49 or 50 detectors (also common), one would scale up by 52/49 to show the rate on the full instrument.

The one very tiny wrinkle to all this is a property called "deadtime". Each time the electronics on a detector register an X-ray event, they "latch" meaning that the detector stops listening for new signals. Effectively, the detector is turned off for a short amount of time it uses to calculate properties of event (what time it arrived, and how much charge was deposited). For NICER, this is typically ~20 microseconds.

As a concrete example of what this means in practice: when the count rate per detector reaches ~10^3/s (or in other words the full instrument light curve was at ~50,000/s), there would be ~20e-6 * 1e3 = 2% deadtime. This would mean that the *actual* count rate is ~2% higher than was detected (because the true exposure time was actually 98% the clock time). Here, most likely Cuneo didn't worry about folding in deadtime effects (I do have my pipeline correct for it automatically, so all your spectra already factor that in).

This is obviously a very small here for NICER, but I wanted to dive into it because for many missions that aren't timing beasts like NICER, deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.

> Also sorta random, I came across a value called "quality" when I was looking at a file in fv; are the quality values the values XSPEC refers to when I say *$ignore bad*?

```
TUNIT2  = 'count    '           / physical unit of field
TTYPE3  = 'QUALITY  '           /   Quality flag of this channel (0=good)
TFORM3  = 'I        '           / data format of field: 2-byte INTEGER
TTYPE4  = 'GROUPING'            / Grouping flag for channel (0=undefined)
TFORM4  = 'I        '           / data format of field: 2-byte INTEGER
```

Yes exactly this. This won't tend to come up for NICER, but e.g., for instruments that have some bad pixels and a spatially dispersed spectrum (think the rainbow from a prism), or uncalibrated energy ranges, this can be used.

> * This is probably one of my favorite one-liner functions in Python:
>
> for line in f:
> linelist = (re.sub(' +', ',', line)).split(',')
>
> Where f is a file opened with: *with open(...) as f.*

Python is a wonder for its very efficient one-lining loops!

As usual, you're knocking it out of the park Thaddaeus.

In other news, a new grad student (Santi) just joined me and he's going to start looking at some QPO data.

Best,

-Jack

On Thu, Oct 29, 2020 at 12:49 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Whoops - Santiago is my grad student and I notice my advisor brain accidentally called you Santi in my last message. Very sorry about that!

Name confusions aside, very glad the patch worked ok. Yeah, setting query to yes can cause problems sometimes as you found. I don't have good rules for avoiding or escaping those runaways, but I've had luck before tweaking the extra convergence terms in the fit command when it's stuck (the delta & beta) and the tolerance term in error.

-Jack

On Thu, Oct 29, 2020 at 11:34 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi Dr. Steiner!

Alright I think that patching did the trick; I'm currently running XSPEC v 12.11.0m. Surprisingly I didn't have to rebuild relxill which was nice. Now that the 12.11.0k patch has been installed, I think XSPEC just "snags" and keeps fitting if I have query set to yes instead of crashing—when I was looking into this, a particular observation ran an error routine for three days nonstop in the background until I aborted it!

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 22, 2020 at 12:46 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Santi,

OK, that's encouraging! Let me know if the patching does the trick. Often for small patches, you won't need to recompile local models, but you should quickly see if it has trouble loading and that would be your sign.

It's hard to predict how parallel processing will impact a given task. The outcome depends on where the bottlenecks occur (if processing, then expect significant improvement, if memory swapping, then probably not). For running on multiple threads, a **rough** estimate is sqrt(N) gain, but that's just ballpark.

-Jack

On Thu, Oct 22, 2020 at 11:56 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

I just got some really great news on the crashes!

Last weekend I was dismayed because even though I was playing with the limits extensively (especially for gamma), the crashing was still occurring unabated. This made me wonder if it was a software issue, so I reached out to the HEASARC Help Desk. I found out that a very similar issue was fixed in XSPEC patch 12.11.0k: *For some complex fitting and error command cases, XSPEC may terminate with a GSL message such as 'SVD decomposition failed to converge'.* This wasn't encouraging at first because XSPEC patch 12.11.0k came out in June (I built the current version of xspec on my vm in ~July), but I checked my XSPEC version with *$version,* and it turns out that my XSPEC version is just 12.11.0! I'm working on installing the patch, I bet this will clear a lot of things up. I think I'm going to have to rebuild relxill after I execute the partial system rebuild.

The only thing that confuses me about this is that I didn't really have issues with core dumping when I was fitting relxill a lot back in August (I think it did come up for error fits back then though), so I'm confused why it really started acting up recently.

Also I don't think I've asked this previously, but out of curiosity, should I expect significant reductions in fit times with two core parallel processing?

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 22, 2020 at 10:52 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Just checking in to see how fits are progressing and if playing with those limits has allowed any headway against the crashes?

(Yes, whenever I "rm -rf " I try to take beat and check that I am not about to bilk the whole machine! With great commands-line tools comes great ...)

-Jack

On Fri, Oct 16, 2020 at 3:24 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

Now that I think about it, I do recall noticing that many (if not most) of the fit failures had gammas in the 3.4 range. Good to know about the 1.6-2.2 range for the residuals, I'll play with that now.

Oh about the attachments: the screen shot from visual studio is just some descriptions I was writing about the different functions I was using, and the microsoft paint one just illustrates the positive contribution that is almost always eliminated by a ~1.75 KeV gaussian of varying width.

Over this weekend should I continue to focus on maximizing the number of observations that I can get good fits and confidence intervals for?

I'm intrigued by this disk truncation controversy, I'm going to read about it this weekend.

Many thanks!
Thaddaeus

P.S. I was reading about some dangerous linux commands the other day; there are some unnerving commands out there. Imagine if someone ran rm -rf in your terminal. Shudder.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 15, 2020 at 10:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> Regarding the application of systematic errors, that's good to know. I've often seen something like the "NICER team recommends the application of a systematic error of 1%" in papers that use NICER data.

Yes, that's a "rough" benchmark, but a reasonable one.

> I love the common.tcl file, it's so handy! I think I'm just going to use it from now on because it's simpler on the execution end (just two commands), and it shows the error strings for each parameter.

Yes, it's quite nice work by Jeremy :).

> A little random: I came across an interesting statement in a paper by Plant et al. 2015 pertaining to observed disk truncation in the hard state of GX 339−4: “This result implies that the current sample of spin estimates in the hard state are inaccurate. Therefore, any distinct conclusions drawn from these estimates, such as the spin-powering of relativistic jets, may well be biased.” Since that paper was published has this issue been fully exploited and resolved, or are there still black holes with possibly incorrect spin measurements out there? Also, is physical disk truncation or changing coronal height the more widely accepted cause for profiles similar to disk truncation?

This has been a topic with a lot of controversy that is still ongoing (Garcia et al 2015 and followup papers by Wang et al. 2019 talk about this). I'd say the Plant picture is strongly disfavored at present, but not entirely out. The robust view is that allowing for the possibility of truncation, a (bright) hard-state spin measurement should be regarded as an estimated lower limit. Presently, the argument is focused around how much truncation could there be in bright hard states, with most reasonable numbers being a few times the ISCO (a factor <10). (Coronal geometry is always an important consideration, but usually they tend to fit very compact.)

> An update on the rsp/arf issue: I modified my approach and fully integrated astropy.io.fits, so when I’m producing my xspec script commands rmf and arf file names are pulled directly from the header of each observation’s fits file. Because of the two errors described below, I haven't checked how chi-square improves when the correct rmf and arf files are used though.

Very good, that should make some impact when working.

> I’ve encountered two weird errors I’ve never encountered before in XSPEC this week: the first is frequent core dumping [SVD decomposition failed to converge…Default GSL error handler invoked…Segmentation fault (core dumped)], and the second is this odd “XSPEC Error: No variable parameters for fit” after executing “fit” for a model with *multiple* free parameters (including the usual Gamma, FracSctr, Tin, diskbb norm, etc.). My first thought was “*is this related to relxill having problems due to a low gamma”*—but these issues both happen even when Gamma is > 2.

The table can be sparse in a few places, and I think very large Gammas are also sparse (>~3.4, if memory serves). I'd say try freezing parameters and retrying the fit to identify the culprit parameter.

> Also a little random: this week I decided it was time to start commenting on my functions in my python files—at one point I was looking through some older files for a particular technique I used and it took a little while to find the correct version of it, so I decided that from now on I’m describing my functions for future reference.

Yes, documenting code is super important and a great habit to get used to. (I wish I was better about it!)

> This is preliminary, but I attached an example of a chi-scape scarred by a common morph of the residual I last sent you. The positive contribution that begins shortly after 1.6 KeV is almost always eliminated by a ~1.75 KeV gaussian of varying width for most observations. The current issue is modeling the various forms the following negative residuals take. Currently working on those. Earlier you mentioned that we could try ignoring that narrow range where these residuals occur, would ignoring channels between 1.6-2.2 KeV be ignoring too much of the data? If we decide to ignore that range, I would think it best to get >*plt wdata* outputs to exactly nail down the dimensions of this range, but for now should I keep trying to model the residuals?

I was reading through some older emails, going to look into your recommendation regarding logxi.

No, I think 1.6-2.2 isn't crazy and you can certainly roll with that if you are having too much trouble taming the residuals with a model. Maybe do a couple test cases to see how the model parameters are affected on both fronts.

Hey, and I didn't see a description of your attachments in case you want to say anything about those.

Glad you're back in the thick of things.

Best,

-Jack

On Thu, Oct 8, 2020 at 1:35 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi Dr. Steiner!

Quick note: I've been getting back into it this week and should have some interesting things to report this weekend!

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Tue, Sep 29, 2020 at 2:19 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Thanks for the update.

> Quick note on the residuals: since we last communicated I investigated the application of a systematic error (3-5%) in the < 2 KeV energy band, but this didn't improve the residuals so I'm assuming that you already accounted for this, like the binning, in your reprocessing?

No, actually. But systematic errors are only incorporated with some fit statistics - chisq for instance, but I don't believe are incorporated in cstat or pgstat. The way I like to think about it is the systematic error acts as a reweighting of the data's constraint and serves to de-emphasize regions with the highest signal. I offer for information sake that that 3-5% is too large a systematic error to include as a rule. Something like 1-2% is more reasonable for a region with poorer calibration quality.

> I also discovered that I've been unintentionally forcing an incorrect response file on multiple MAXI J1535-571 observations—the rsp I was using for all observations was `nicer_d49_55575341.rmf`, but after running a quick check (the fitsio package has been so helpful, thanks for recommending it to me!), I found that multiple observations request `nicer_d49_u100_150_55575341.rmf` in their headers.

Ah! That's very good to know, and may be the culprit for some of the residuals you've been battling. Check that the arf files (keyword ANCRFILE) is also set correctly.

> I'm likely almost done with the SAT (took it Saturday, going to take it again this coming Saturday)*, and once I'm done I aim to return to a progress pace similar to the pace we had in the summer.

Congratulations - that's a nice milestone!

> G'mar chatima tova (if you observe Yom Kippur)!

Same to you.

> *I might take it again in November in the event that I bungle those tests.

I'd put dollars to donuts you did great!

My best,

-Jack

> On Tue, Sep 22, 2020 at 1:21 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Interesting I'll look into that!
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi
>
> On Tue, Sep 22, 2020 at 1:15 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Similar to that. I used another language (IDL) to automatically produce a series of tcl/tk scripts and then executed those distributed over a number of cores on a workstation.
>
> -Jack
>
> On Tue, Sep 22, 2020 at 1:13 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> Sounds good!
>
> In that case did you operate solely in TCL?
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi
>
> On Tue, Sep 22, 2020 at 1:10 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Thaddaeus,

> For >75% I meant observations where source flux was more than 75% bg flux, and for 4% I meant observations where source flux was ~4 % bg flux. That makes sense—interestingly the observations with source flux ~4% bg flux were, by best fit, in the SPL state, with the highest flux and gamma for all the observations that had reasonable values for these parameters.

Ah, now I see. Yes, when the bg flux is very high, the noise level (N=sqrt(B+S)) where B and S are background and signal counts in a channel, then the constraining power of the data (S/N) is lowest. If the background is reasonably estimated, the fits will likely be good but poorly constrained; if the background is off, then all bets are off.

> xD. I think it's a very impressive feat! Did you use pyxspec to loop through all the observations?

Thanks. I used a different language actually, but python/pyxspec would be an equally good way to do it.

Best,

-Jack

> On Tue, Sep 22, 2020 at 12:39 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> Hi Thaddaeus,
>
> > That's good to know. I guess what's surprising is some relative of this large residual in the 1.6-2.0 KeV is present in most of the source flux > 75% observations (I wonder if the peak feature falsely inflates the source flux for some of them?). Going back to a couple emails ago, I was checking some of my all time best fits which had source fluxes around ~4%—what allows such faint observations to fit well?
>
> Just to clarify, do you mean 75% and 4% of the Eddington limit, peak luminosity, or something else? Fainter observations, given some typical observation time, obviously have fewer counts which means larger fractional errors and less constraining power on the model. So if there is a deficiency in the calibration or model being used, it will look more prominent in brighter + long exposure time data.
>
> > Also, I'm very intrigued by your methods in one of your 2016 papers , "STRONGER REFLECTION FROM BLACK HOLE ACCRETION DISKS IN SOFT X-RAY STATES". How did you automate fitting the 15,000 observations reliably and within a reasonable time frame?
>
> A reasonable time frame is relative :), but I used about two dozen cores to run the fits which helped. I also stuck with computationally simplistic models, and for my runs with simpl, used lower resolution (via energies) in the run, since RXTE/PCA - the instrument used there - also has rather poor energy resolution compared to NICER.
>
> > Could I possibly have the ~86 observations between obsids 1200220101 – 1200220186?
>
> Spectral files are attached. Response files are too big for me to attach, so I'll keep them up here for a few days: https://nicer-web.mit.edu/nicerview/thadd_resp_m1727.tar.gz
>
> > Ah that makes sense. In that case did Xu et al. just freeze Rin at the ISCO to have less free parameters?

That's the usual tactic, yes, when fitting for spin.

Best,

-Jack

On Thu, Sep 17, 2020 at 11:00 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Great to hear you've been digging in and exploring! I would suggest though that you avoid being too concerned about the residual features. If something is localized and only ~percent level deviation, then that's likely sufficient. *The goal should be to fit those sufficiently that they don't alter the continuum, but at some level it becomes diminishing returns to try fitting an artefact.* That said, if you wish, I can get you the data on MAXI J1727-203. Just let me know if you have a particular ObsID from Kevin's paper you want to try.
>
> Your question about degeneracy is great. The condition of parameters being strongly correlated with one another is degeneracy. Basically, this is a very common situation where two parameters are dependent upon one another to some extent, so a probable value of one will condition a probable range of values with the other. For instance, if you want to fit x and y with the constraint x^2+y^2 = 4+/-1, you would get a ring of radius 2 with x,y values that obviously depend on one another; they are *not independent*. Speaking generally for more realistic cases, the interaction between parameters is often pretty convoluted, but commonly things like Gamma and Normalization are correlated with one another, so a contour plot of the confidence interval would look banana shaped rather than like a circle.
>
> Best,
>
> -Jack
>
> On Thu, Sep 17, 2020 at 9:18 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
>
> > Hi!
> >
> > Thanks for checking in! Since we last spoke I've been taking a deeper look into a couple articles (Alabarta et al. 2020, Xu et al. 2018, and Parikh et al. 2019) and playing around a bit with the residual fix you suggested. As tested on multiple observations with varying fit statistic contributions in the 1.6-1.8 KeV range (chi-squared contribs. ranging between 500-3000), the gaussian centered around 1.75 KeV combined with the "edge" works great!
> >
> > Most importantly, I found a method to more accurately investigate the residual features: *plt>wdata*. Now that I can actually see what the chi-sq contrib. is for every point, I've been having a lot more success in approaching residuals in the 1.6-2.2 KeV range, especially when the residual patterns after 1.8 KeV differ greatly from what can quickly be ameliorated with the edge feature (it seems that for the majority of observations the <1.8 KeV excess is easily subdued by the gaussian fix, but after 1.8 KeV things often get complicated, especially when a third feature

shows up between 1.86 to 2.0 KeV. This third feature resembles a sort of gaussian-like crevice about 0.13 KeV in width and often reaches a chi^2 contribution of -5331 (+) at its minima, pushing the "edge" feature out to a threshold energy of ~2+ KeV). I haven't found a definitive solution yet, but a gaussian absorption with an energy of ~1.86 KeV and a width of ~0.14 KeV is looking promising.

While I've been working with these MAXI J1535-571 residuals I've been wondering if the residuals Alabarta et al. discuss in Nicer data for MAXI J1727−203 are of similar shape—is there any chance I could procure some pipeline data from you on MAXI J1727-203 to look into this (if it's not an inconvenience for you and or you don't think it's a distraction on my part of course).

Out of curiosity, what does "parameter degeneracy" mean? My first guess would be this example: if I only told you what the slope of a line was (e.g. '2'), and then asked you for the individual values of Δy and Δx—would Δy and Δx be degenerate? Example of where I came across this: (from Xu et al. 2018) "The inner disk radius, R_{in}, and the black hole spin parameter, a*, are degenerate, as they both control the effective inner accretion disk radius. Therefore, for simplicity, during the spectral fitting we assume that the inner disk extends down to the ISCO by fixing R_{in} at the radius of the ISCO, and fit for the black hole spin as a free parameter."

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

Steiner, James <james.steiner@cfa.harvard.edu> Tue, Jan 26, 2021 at 10:47 PM
To: "Kiker, Thaddaeus" <s024622@students.lmsd.org>

Hi Thaddaeus,

My turn to apologize for a long delay. It's proposal week for NuSTAR, so it's been busy on that front. Thanks for updating the plots -- looks great!

The nthcomp normalization I don't believe has a trivial definition but it will be broadly indicative of the flux of the nonthermal component.
(Although the precise scaling between norm and flux changes with Gamma, kTe, etc.).
Using the simpl variant, the meaning is more clearcut.

> Excellent - these look really great. It appears to me that there are some points shown in the simpl panel that aren't shown in the nthcomp panels, and vice-versa. Can you mention what the screening difference is? (e.g., around day 55030)

I'm fairly confident I used the same reduced. pgstat cutoff for both the nthcomp and simpl models, so I'm wondering if those particular gti's that fitted well for simpl didn't fit well for nthcomp around that point.

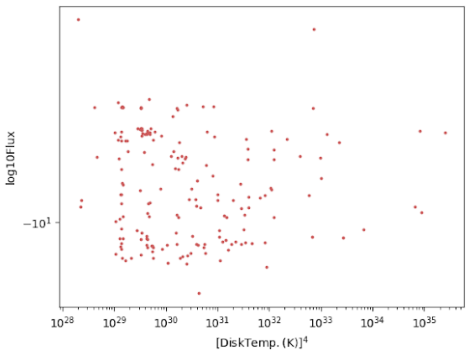
Can you pick a couple to check into on that front to check out and plot the fits, compare the goodness? I'm surprised that the fits would be significantly different in quality since the models are so similar.

> For a disk-blackbody, the expectation is that L ~ T^4, so you can plot L vs T on a log-log plot and look at that scaling.

So I've gotten started on this. The below plot is obviously not nice (I haven't modified the cflux log10Flux values to luminosities yet), but I think I have my temperatures in K correct. This was the method I used:

```
b_const = (1380649/16021766340) #Boltzman const in $eV \cdot K^{-1}$
tin_mod = 1000*(1/(b_const*1))
print(tin_mod)
#Checked this with David Cohen's website which states $1 keV \approx 11.6 \cdot 10^{6} K$
```

Plot:



I used the lone “1” in $b_const \cdot 1$ to line my transformation up with David Cohen’s website’s statement that 1 keV is approximately equal to $11.6 \cdot 10^{6} K$. In the final code I just substituted an array of diskbb Tins for that “1”. I think this will scale nicely when I transform the log10fluxes because I remember this bhb’s luminosity was somewhere in the $10^{28}-10^{39}$ range (I just looked back at an old email—calculated a luminosity of 1.2067*10^38 ergs/cm^2/s for one observation so sounds right).

So this plot looks suspicious to me, so I expect something is probably going wrong. The reason I'm suspicious is I expect to see something close to a linear correlation. I'll explain why below. But what I note here is I don't see an obvious correlation that looks near-to-linear. So I expect somehow points are getting shuffled somewhere and so potentially the arrays aren't in the same order.

Stepping back for a moment to revisit how best to plot this, let me offer that what you are plotting is entirely reasonable but also contains extra legwork that you don't need to tackle. Since you are plotting quantities in log-scales (i.e., a given ratio has equal spacing), this means if you just show temperature (kT) versus luminosity, you will be showing equal spacing of log(L) and log(T).

Let's consider how a unit conversion would manifest, where you might plot kT in keV rather than temperature in deg. C. Multiplicative factors like this just introduce an a offset shift (let's make a constant "c" here as the conversion factor: log(cT) = log(T) + log(c).)

Most importantly, for a power-law relationship like L ~ T^4, this will look linear on a log-log plot, because log(T^4) = 4*log(T).

Hopefully that's clear. So I would advise you plot the fitted temperature (kT) in the keV units versus your flux, and on a log-log plot let's see if it looks linear.

(By the way, how are you using cflux, and over what range? It could be that this is the culprit - ideally this would be computed in broadband and before the absorption is in scope.)

Observing proposals are due in on Friday, so I will be more responsive after the deadline.

Hope you are doing well!

-Jack

On Fri, Jan 8, 2021 at 10:00 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Ah, I remember an ice-cream fueled haze of recovery in the aftermath. Hope you are getting some of the same!

-Jack

On Fri, Jan 8, 2021 at 8:38 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

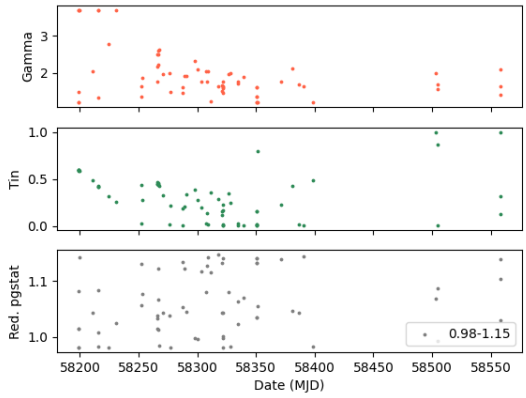
Hi Dr. Steiner!

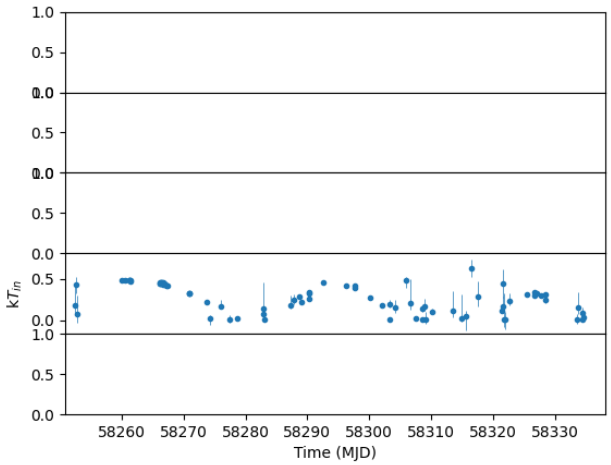
I was a little out of it this week after getting my wisdom teeth pulled, just wanted to let you know I've seen your email and I'm going to work on it this weekend.

Cheers!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Tue, Jan 5, 2021 at 5:23 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

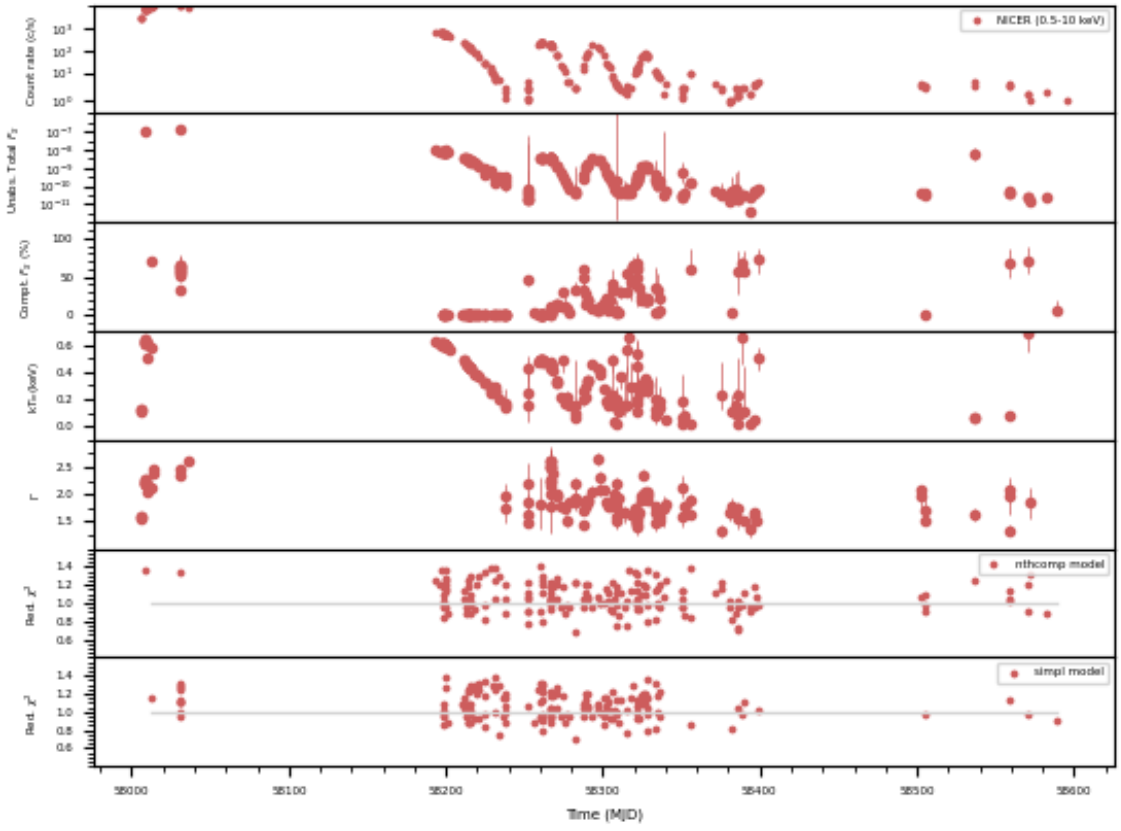
> Hey Thaddaeus,
>
> Wow, that's an impressive and sweet-looking delivery!
>
> > Thank you for your well wishes! I hope the same for you and your family as well!
>
> Thanks Thaddaeus, we're doing just fine and couldn't be happier to turn the page on 2020.
>
> > First, a thought back on the relxill models: don't nthcomp and simpl produce different values for gamma and hence they should not be mixed with each other? This thought came to me this morning when I was tidying things up. Since I was tieing relxill's gamma to simpl's, I wonder if that threw anything off. Now that I think about it more, I don't think it would catastrophically derail things, but it would still be inappropriate to tie gammas from different models right?
>
> The default answer is that these should be identical - they both describe the shape of the Comptonized power-law component, and in the same way. For completeness, I should mention there has been some small exploration about decoupling these things with some loose physical arguments, but basically that's a stretch and I would absolutely keep them linked up as you've done.
>
> > The first hint of a breakthrough happened on Tuesday when I realized I forgot to filter for background fraction. I looped through all the data files that contributed to the last plot I sent you and only plotted fits from files where background was < 50% source, giving me this, much neater, plot (where gamma and inner disk temp are actually somewhat correlated:
>
> Ah, yes! Glad you caught that; keeping to the cleanest data is a very good idea, and 50% is reasonable; if things look at all funny, even down to ~20% would be a reasonable place to cut.
>
> > 
> >
> >
> > This cleaned things up pretty well, but I noticed there were only a couple points with gamma > 3 (all of which were right on 3.5), so I ran another routine with the upper limits on gamma set to 3. The points that were right on 3.5 were on 3.0 for gamma in this routine, so I wondered if it wasn't an issue of the upper limit but an issue of insensitivity. When I tried to estimate 1-σ errors on a couple of the fits that had 3.5 upper limits, XSPEC returned "***Warning: Parameter pegged at hard limit: 3.5"—which I take as being caused by gamma's insensitivity to the fit in the observations.
>
> It is possible this is because the fit isn't sensitive to Gamma. (You could check this out by comparing fit statistics to the upper-limit 3.5 vs the upper-limit 3 fits, or also looking at the f_sc values - <~ 0.01 being an indication that isn't sensitive.) But alternatively, it's also possible that the data are sensitive to Gamma and either (i) the spectrum is very soft, wanting high Gamma in which case lowering the upper-limit would still cause it to peg but the fit would be markedly worse or (ii) *my guess for most likely for these data*, the background subtraction is off in those cases and oversubtracts, creating an artificial appearance of a very soft spectrum.
>
> > Then I plotted Tin measurements from that fit routine with gamma's upper limit set to 3.0 with one sigma (all confidence levels I bring up are one sigma unless otherwise noted, as in Cuneo's paper; though I must say, one sigma sounds so much better than 68% confidence even though they're the same thing lol), here's how that turned out:



Pretty nice, eh?

Yes, this looks *very nice*!

So fast forward (and a bunch of partial fitting routines later), I'm proud to present these:

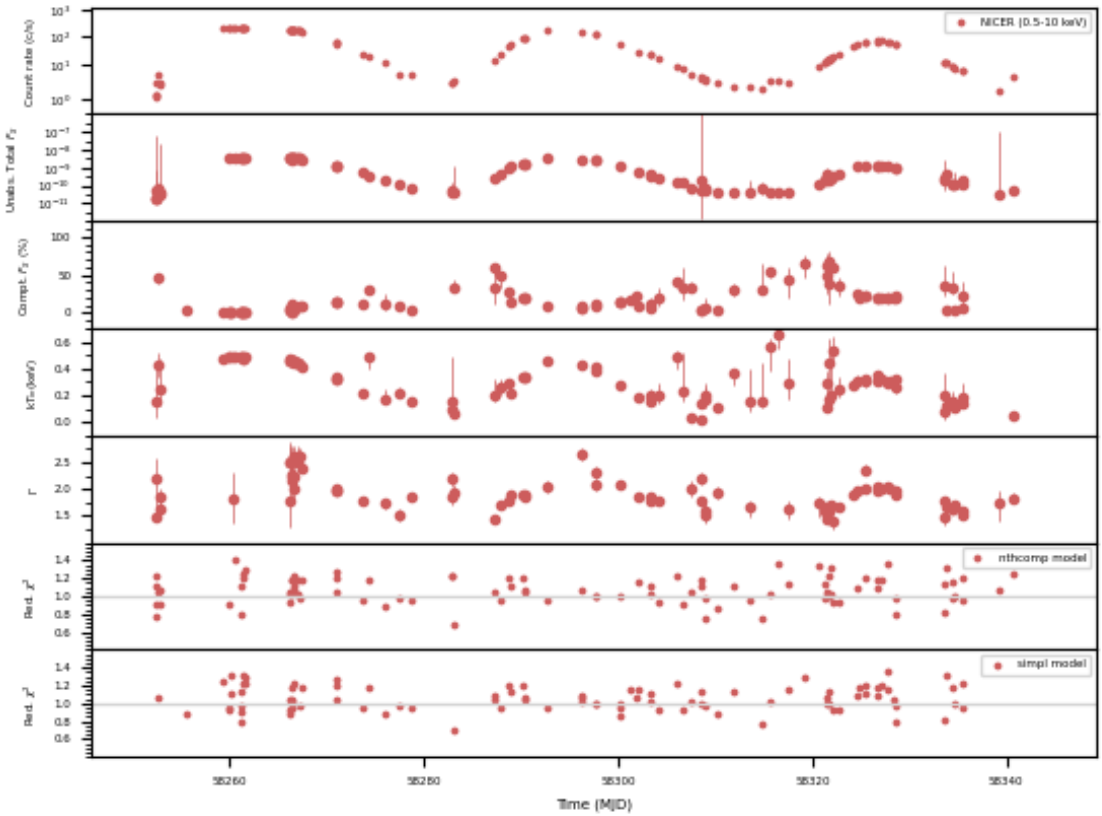


This is my final plot for the MAXI data. The top panel is background subtracted count rate (a note on this later), the second is unabsorbed total flux on the range 0.5-10.0 keV, the third panel is comptonized flux percentage (calculated with a separate simpl model, a note on this later), the fourth inner disk temp, the fifth gamma, the sixth reduced pgstat for the nthcomp model, and the seventh the reduced pgstat for the simpl model.

Excellent - these look really great. It appears to me that there are some points shown in the simpl panel that aren't shown in the nthcomp panels, and vice-versa. Can you mention what the screening difference is? (e.g., around day 55030)

For a disk-blackbody, the expectation is that L ~ T^4, so you can plot L vs T on a log-log plot and look at that scaling.

Here's what my plot looks like during the reflaring period investigated by Cuneo:



All parameters were calculated with the nthcomp model except for comptonized fraction. Model descriptions below:

##Model One##
Model: tbabs(diskbb+nthcomp)
Delta fit statistic: 1.
Noticed Ranges: 0.5-1.5 2.3-10.0 keV
nH: 3.2 (frozen)
Tin lower limit: 0.01
Tin upper limit: 1.0
Gamma lower limit: 1.2
Gamma upper limit: 3.0
High energy rollover: 100 keV
inp_type: 1
Redshift: 0

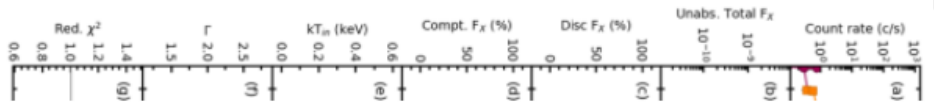
##Model Two##
Model: tbabs(simpl(diskbb))
Delta fit statistic: 1.
Noticed Ranges: 0.5-1.5 2.3-10.0 keV
nH: 3.2 (frozen)
Tin lower limit: 0.01
Tin upper limit: 1.0
Gamma lower limit: 1.4 #I adopted this limit from your paper in which you used simpl to fit those 15,000-odd observations.
Gamma upper limit: 3.4 #I adopted this limit from your paper in which you used simpl to fit those 15,000-odd observations.
UpScOnly value: 1

Looks good.

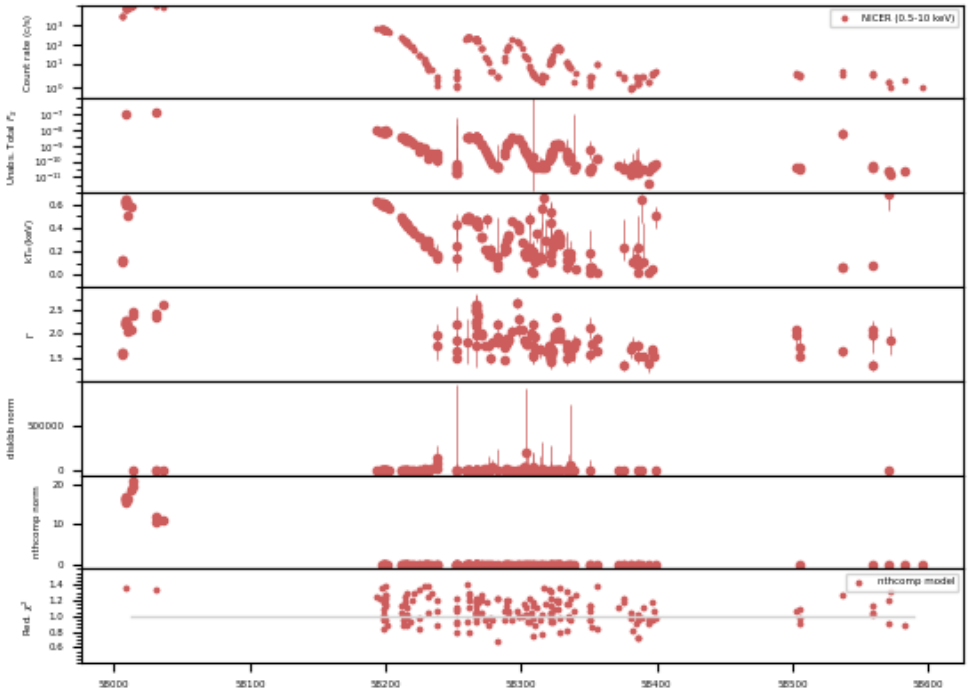
****
Per the background subtracted count rates, I corrected my code to account for your pointers; the important snippet is below:

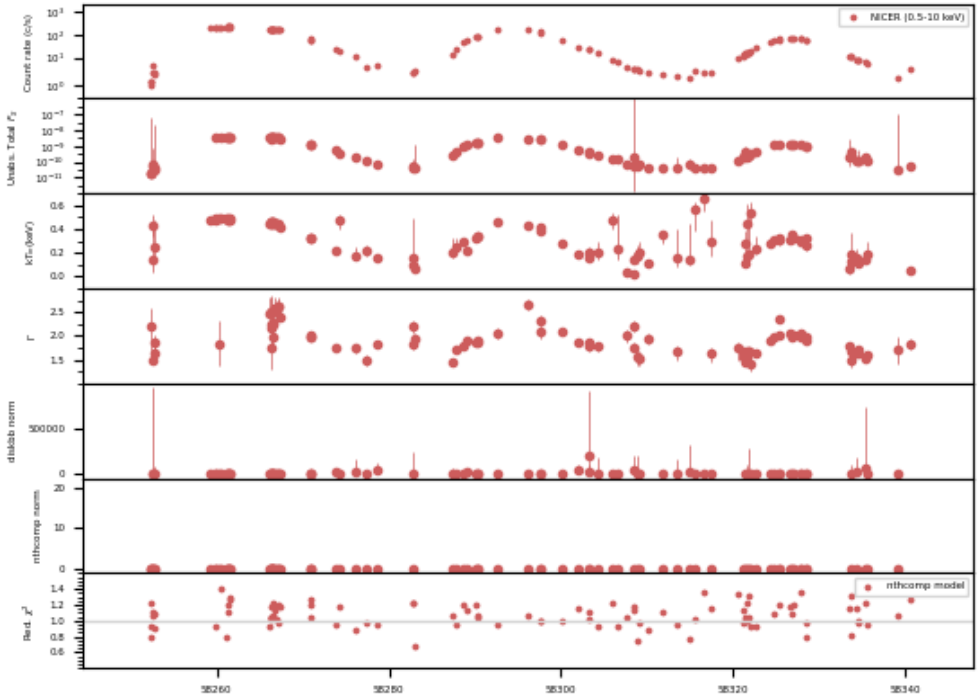
```
scaled_bg_counts = (sum(restricted_bg_counts_list)/bg_exp_time)*exp_time
count_rate = sum(restricted_data_counts_list)-scaled_bg_counts
count_rate = count_rate/exp_time
os.remove(temp_data_file)
os.remove(temp_bg_file)

out_list.append((seg_id+':'+str(count_rate)))
```

To check: I take it restriced_bg_counts_list, and analogous for the data are only using the noticed ranges? If so, all looks good!

> Oh also, a small note on my final plots. I think I found out how Cuneo prevented ytick labels from colliding:
>
> 
>
>
> I think she set the ylim bounds somewhat lesser and somewhat more than the min(y) and max(y) vals—so I did this and it improves the aesthetics significantly in my opinion.

Yes, this is a nice way to keep the plot clean. (And for x-ranges too.)

> As you requested, here's a plot with the diskbb and nthcomp norm components:
>
> 
>
>
> (Full range)



(cuneo date range)

This is great, but I do have one request: it would be helpful to have the normalizations plotted in log-scale as well (to better see their dynamic change).

> I didn't include values consistent with zero within errors in any of these plots, but I can if you'd like.

Sounds reasonable to me. Can also try plotting them with a different (less prominent) color or symbol as an alternative.

> Right now the raw outputs from my returnConfidenceIntervals() function look like this:
>
> PROBLEMS OUTPUT DEBUG CONSOLE TERMINAL
>
> ```
> 1130360284_1:PHLZ:0.591738
> 1130360284_2:0.249281:0.334173
> 2130360201_3:0.107522:0.107773
> 2130360201_4:0.257621:0.200038
> 2130360201_5:0.36142:0.204905
> 2130360202_0:PHLZ:0.116443
> 2130360202_1:PHLZ:0.718751
> 2130360203_0:0.295584:0.279007
> 2130360204_1:PHLZ:0.727953
> ```
>
> (where PHLZ stands for Parameter Hit Lower Zero and PHUZ stands for Parameter Hit Upper Zero)
>
> Early in the week I thought about changing the output of this function to some kind of multidimensional numpy array like this:
> `[['seg_id_1','seg_id_2'],[lower_1,lower_2],[upper_1,upper_2]]`
>
> But I didn't really feel like it because the current function works fast enough for me (I don't ~~see the need to vectorize~~ (more accurate statement: feel like vectorizing) all my functions yet; unless you would advise me to in which case I'll get right to it lol). *

Ha - while it can absolutely make a big difference in some cases, it sounds like from below you've found those cases where it really matters. Usually when memory juggling big arrays you want to do that once and avoid repeated calls in that vein, and otherwise small transformations of scalars there will be plenty of situations where it looks much nicer but won't make much practical impact.

> I did write a standard function for returning float values of value date, value and a two dimensional array for yerr for parameters based on an input x=the parameters output from returnConfidenceIntervals(), y=theoutput from a returnDates() function I wrote a bit ago, and

> z = an output from the returnFitResults() function I wrote a bit ago. Lol reading that last sentence, it looks a little confusing so I'll add this function to my package and document it in markdown and show it to you in a later email.
>
> I still have to iron out my hardness ratios function, will get to that soon.
>
> A random, last thought: in compsci club at school we've recently been discussing algorithmic efficiency in the context of things like sorting algorithms, which inspired me to think about this in the context of my code. So far I'd say a noticeable improvement that has resulted from this is that I'm limiting the number of computationally expensive tasks I put under for and conditional loops as long as I can execute them outside the loop. An example of this would be declaring a scipy.interpolate.interp1d object outside of a for loop rather than declaring it for every data value in a set (this came up in something else I was working on, but I brought it up because I think it's a good example).

Absolutely, this is just the kind of case you want to call once only.

> All data referenced in the plots, the python file I wrote for creating these plots, and the python file I used to write the fit commands are all attached in the email_attachments .xz tarball.
>
> Cheers (and Happy New Year)!
> Thaddaeus

Wonderful work Thaddaeus! And Happy New Year!

-Jack

> When you replace a for loop with a vectorized numpy function and see the speed improvement
>
> 

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Dec 31, 2020 at 2:38 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> It's been quite a month everywhere! Hope you had a nice holiday season and that you and your family have been doing well. Wishing you a happy and better New Year ahead.
>
> -Jack
>
> On Wed, Dec 9, 2020 at 2:57 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
>
> > Hey Thaddaeus,
> >
> > Those parameter plots look great! Can you add in the diskbb and compTT normalizations as panels as well?

It looks to me like things are probably going wrong because you are probably using bin-number rather than channel number. Sometimes these two get conflated, but here by bin I mean groups of one or more channels, where a channel is the thing that is hard-coded in the spectral file (and PI-bins correspond to 10 eV increments). So you'd want to use channels 50-299 and 300-999 for your two bands.

The other thing to check is that the background is scaled properly. So you want to make sure that to background you subtract is:
BG_Spectrum_MatchedToData = BG_Counts_FromBGFile / BGFile_ExpTime * DataFile_ExpTime

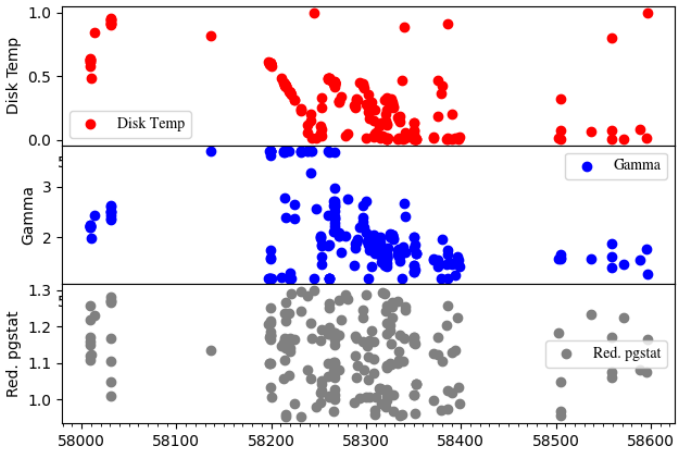
Hopefully my shorthand there is clear enough.

Best,

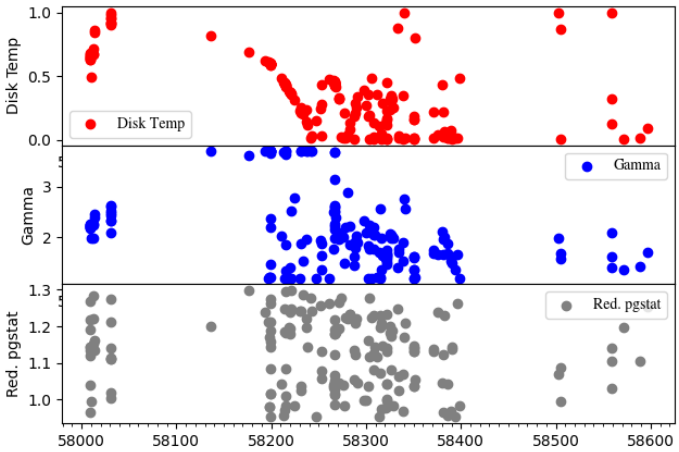
-Jack

On Wed, Dec 9, 2020 at 2:39 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi Dr. Steiner!
>
> Phew! The number of potential files for fitting has shot up to 720 from 185 now that I'm searching for and including all GTIs files.
>
> I ran a couple fitting routines with a simple tbabs(diskbb+nthcomp) model on all the files just to get a feel for how including a lot more files would possibly change things, and the initial results from these fitting routines were quite unusual (though they were blisteringly fast compared to relxill fits regardless —with nH frozen on these routines, I was fitting all the files with 700 fit iterations each in under an hour on my mom's computer) :



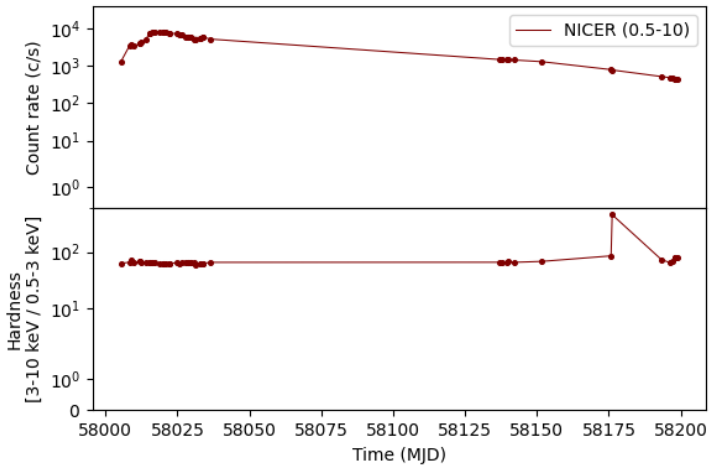
> ^ That routine had diskkbb Tin upper limit set to 1, gamma upper limit set to 3.7, nH frozen to 3.2, and the entire energy range from 0.5-10.0 keV noticed.



> ^That routine was the same except the energy interval 1.5-2.3 keV was ignored.
>
> I think I see a little pattern of correlated variation between disk temp and gamma masked under the erratic points in both plots...I'm wondering, should I set some cutoff exposure time for the files? I recall seeing some ~22 sec exp. time data files when I

was looking through other GTIs, and I'm thinking since these files would have less data to constrain the model, crazy parameters can be permissibly returned by XSPEC when these files are fit.

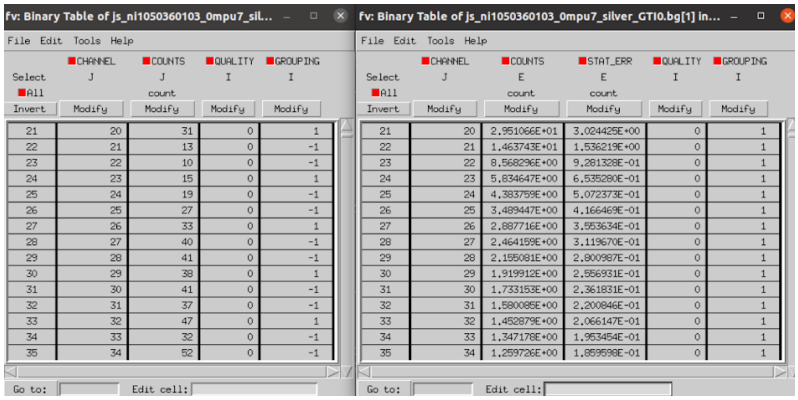
Also, could I get your advice on calculating hardness ratios? I modified my count rate function to correct for background counts and it's working pretty well (see the top subplot below):



Almost identical to Cuneo's count rate plot, eh?

As you can see in the bottom subplot, the hardness ratio values are crazy, and I can't figure out how to rein them in. The way my function for hardness ratio works is I sum the number of counts in the parameter defined hard band and subtract the number of background counts in the predefined hard band from that value, giving me a net hard band counts sum. I then divide that value by (sum of soft counts-sum soft background counts). Code is attached.

I was checking with fv, and it's interesting that in this case (and what I think is true in general) the bg counts are very low in the hard band but fairly high in the thermal:



Channels 22-99 = 0.5-3 keV

Channels 101-254 = 3.0-10.0 keV

Cheers!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 3:36 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

> Thanksgiving was very nice, thanks!

Yay that's great to hear!

> Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.

Sounds good.

> YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.

!!! I will do this then!

> Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

Will do.

Thanks!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 30, 2020 at 12:28 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Thanksgiving was very nice, thanks! Glad yours with the grandparents was too.

> I reprocessed the data last weekend (11/22) but the results were a little weird so I've been tweaking some things and prepping for reprocessing again this week.
>
> I was wondering if I should take a different approach to the instrumental residuals by running a preliminary fit on all files and logging the fit statistic landscape, so I can then loop through and identify which files actually require the 1.5-2.2 keV range to be ignored and which can have it noticed (I'm wondering about this because from a preliminary check a lot of the disk temperatures were behaving weirdly, which I think could be related to having less data in the thermal band to constrain them). What do you think?

Some sorting based on preliminary fits is a very reasonable idea. But I'd be more cautious about blanket application of an ignore based on fit performance. I'd say first keep track of the ones that fit well and don't fit well and then do a quick eye inspection of them all. One thing that can happen is short observations necessarily contain less signal compared to ones with more time (around the same flux of course), so you don't want to segregate physical models based on exposure time but rather based on the source behavior.

> Also, I've only been fitting the silver, zero good time interval files; would you recommend me to fit some of the GTI1s, GT2s, etc. in hopes to get more good fits?

YES! The GTI numberings are in sequential order in time of observation, so most of the best (i.e, longest observations with lowest background) will be at higher GTI number just by random chance. It's worth exploring all of these.

> P.S. I wrote two new standardized functions (one calculates and returns count rates, the other calculates and returns hardness ratios) and nice documentation for all my standard functions so far.

Very nice!! I took a quick peek and suggest you might want to add an option to subtract the in-band background when calculating both too. (That will mostly matter for the fainter data, of course.)

-Jack

On Mon, Nov 23, 2020 at 8:34 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Will do!

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Mon, Nov 23, 2020 at 4:30 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Sounds good! Glad your mom's computer has some CPU cycles to share. Keep me posted when you get the results in hand. Meantime, happy Thanksgiving.

-Jack

On Sun, Nov 22, 2020 at 8:01 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

> Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

Yes I've been fitting with the inner radius frozen to -1. That's good to know.

> As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind. So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

Sounds good.

> As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Yes I agree it was nice that they showed the underlying component.

I've been running the first iterations of the reprocessing last night and today and I've had a little breakthrough—last night I quickly updated the version of XSPEC I had on the vm that I installed on my mom's computer way back in June (I also installed relxill on it), and I've been dividing the work between her computer and mine—the routines are concluding much quicker (her computer has four cores xD).

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Fri, Nov 20, 2020 at 9:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Good question! Do you have the inner radius frozen to -1 (i.e., at the ISCO)? If fitting spin, Rin should be frozen. Conversely, if you have Rin free I'd fix spin *at its maximum value*, 0.998. The reason for this is that really *only* one inner-disk size scale is being determined in the fit procedure, and efforts to jointly fit both will simply cause your fit to be seeking out a strongly degenerate track between Rin and spin which can additionally impair the rest of the fit from converging efficeintly.

As a rule of thumb, I tend to find value in first assessing the consistency between observations before going about freezing things at a source-fixed quantity. (Also worth mentioning, the reflection-determined inclination has a history of being pretty unreliable and problematic.) This serves the dual purpose of checking how reliably the model finds a single value (i.e., a check for systematic modeling issues), and also crosschecks the value you have in mind.

So I'd suggest first trying without and then depending on the kind of consistency and pegs/unconstrained fits potentially revisiting that. (For instance, the low S/N observations will certainly have trouble informing those parameters.)

And in response to your email yesterday:

> So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

Yes, spot on!

As for those plots, I do find them aesthetically a bit busy, but I *do* like that they present the underlying component and not just the aggregate fit.

Best,

-Jack

On Fri, Nov 20, 2020 at 8:13 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Shoot! I forgot this bring this up. Would you recommend me to freeze the spin and inclination parameters to published values (that are in agreement with some of my previous best fits) to simplify the parameter space, or should I keep them free? The only benefit I can see from keeping them free would be sometimes error locks up with no variable parameters to use when free parameters get pegged due to their insignificance to the fit, and I think having them free could help prevent this...though spin and inclination seem like parameters that would be the first to get pegged during an error routine anyways.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Nov 19, 2020 at 8:07 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hey!

> Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.

Yeah, safety is (rightly so) the first priority.

> A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

Oo sounds cool, I hope you all have fun!

> Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

Perfect.

> The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon.

Interesting.

> then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

Very interesting.

I think I found the thing that was on the edge of my memory: a Chandra image of the quasar H1821+643.

So in summary, deadtime is when the detector possibly misses a photon impact (or more) because it's recording a previous one, and pileup is kinda like a detector getting oversaturated/overwhelmed?

> Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

I should probably rephrase what I said earlier. When I ignored bad, no channels were ignored so I guess that meant no poor quality flags were raised.

> I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Agreed. I find it so enjoyable: the initial encounter with a seemingly insurmountable challenge, the brainstorming, the troubleshooting, and *especially* the successful execution of a solution. It's extremely satisfying—and to use an analogy from my youth* it feels kinda similar to when I would build things in lego.

*I say an analogy from my youth but that's somewhat misleading. I still occasionally play legos, and these days the only difference is that I have a partner: my younger brother ;).

> After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Definitely. That is exactly what I'm thinking.

> Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

:)).

Best!
Thaddaeus

Oh I almost forgot. I don't remember how/why I came across this paper (Arumugasamy et al. 2018), but after looking at the plots I've been meaning to ask you about them:

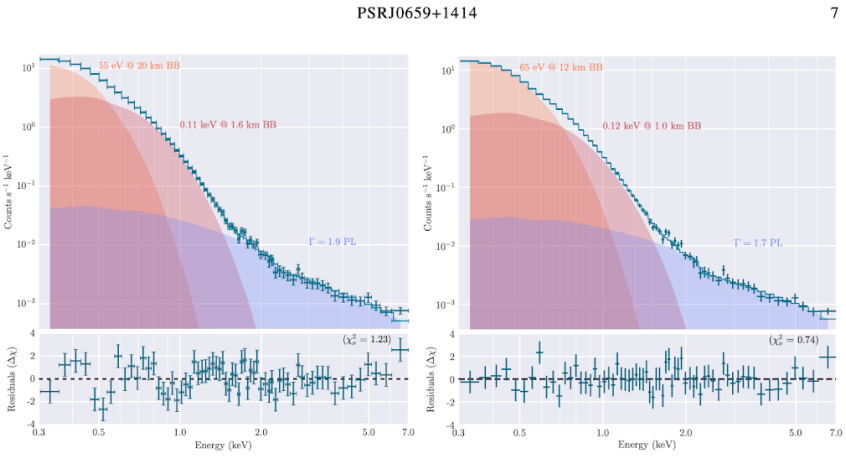


Figure 4. 2BBPL model, without (left) and with (right) `Gabs` absorption component, fit to the phase-integrated spectrum of J0659+1414. The model parameters correspond to maximum posterior probability, which in the case of G2BBPL are different from the distribution medians listed in Table 2.

I've been using the darkgrid style (like the plots above) with the rc font parameter set to Nimbus Roman in most of my plots ... but after seeing these plots I'm thinking I was to switch back to the mpl defaults—these plots are kinda distracting to me in their aesthetic.

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Wed, Nov 18, 2020 at 10:05 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

> It was a wild week indeed—and things seem to keep getting crazier. We were in a hybrid model since October 19th, but as of Tuesday we went indefinitely back to all online which is a two sided coin. On one hand the online schedule gives me a lot more free time, but on the other I'm going to miss the extra class time in my harder classes like mechanics and calc. Thanksgiving is around the corner though! Up to any interesting plans yourself? I think we're just going to have my mom's parents over because we already interact with them frequently, but no big gathering this year (obviously).

No kidding! Feels like we're living in a bad TV show.

Even though it's obviously for good and safe reasons, I'm sorry to hear school is moving to remote-only. I'm sure that's a major disappointment especially for the fun & challenging classes.

A low-key Thanksgiving sounds about right. We're actually going a bit adventurous and meeting up with my sister-in-law out of town (just her). She and we are ahead of time (now) and then getting tested just ahead of that as a precaution... this will be the first family we've visited since Feb!

> Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).
>
> Would it be okay if I emailed Dr. Cuneo about this? This is very important for how I structure the confidence intervals code (and whether I make it more sensitive and able to detect more than the four generalized abnormal scenarios described later in this email).

Sure thing, email away.

> Haha! I just read that NuStar's is ~2.5 *milliseconds*.

Yeah and that's actually not too bad compared to most things out there. NICER is just an absolute timing monster.

> I do have my pipeline correct for it automatically, so all your spectra already factor that in.
>
> Cool! Does that mean I can just go ahead and calculate count rates as number of counts / exp. time and not worry about adjusting the result because you've already accounted for deadtime?

Yes, I should have been clearer that I was giving background purely for edification, and that simply doing the division is all you need to do on your end.

> deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.
>
> Yes. This makes sense. I feel like I've seen something related where the center of an object was too bright in the X-Ray band (which oversaturated the detector?) so the color gradient of brightness was black in the center because the author's couldn't determine the brightness at the center. It kinda looked like an artificial coronagraph. Would that be related to a situation where the dead time was more than the event time?

(Deep dive warning:)
Yes, you're speaking about "pileup" which isn't exactly related to deadtime except that both can be important when the source is bright. Deadtime, again, is from the detector shutting down for some portion

of the time. Pileup concerns are most often for an imagng type of detector - more like the camera on your phone - that collects an exposure for some amount of time and then read it off.

Chandra, XMM-Newton, Swift, Suzaku, (somewhat confusingly NuStar is in both camps), let X-rays come in from the sky, get focused onto the CCD camera, and then in some time interval, often ~1s, reads off the image and clears the CCD. The goal of CCD imagers for X-rays (which I should mention is not the same as the goal for when they are used in optical), is to detect every individual X-ray photon. So when an individual pixel gets << 1 X-ray per frame on average, you are in good shape. *But* when there is significant probability of getting >~1 X-ray per frame on a given pixel, then what happens is the detector can't tell the difference between e.g., 2 hits by 1 keV X-ray photons versus 1 hit by a 2 keV photon.

This registering of multiple X-rays incorrectly as one more energetic single event is pileup, and it is the bane of most imaging detectors for those of us working with bright sources. (Pileup can occur in instruments like NICER too, when two X-rays hit during the event-latch phase, but this is something like a <1% effect for a 100 Crab-bright source, so well in the camp of being negligible for all practical purposes.)

In the most extreme cases of pileup, the pixel digitizer, which usually records values on something like a 16-bit value (e.g., from -32768:32767), can roll over and then appears negative when too much charge hits at once.

> I'm pretty sure the RXTE data I was working with earlier this year didn't have this identifier because if I remember correctly I couldn't execute *$ignore bad* on the data in XSPEC.

Hmm, it shouldn't ever mind you trying that out, even if there are no flagged channels.

> Here's a working version of the confidence interval / error results script:
>
> I put my XSPECtools.py file which has all of these functions into a custom module in my local site-packages folder for python and it seems like it's working perfectly. I import the file like this: *from personalastropy.xspectools import XSPECtools as xt*, and I execute a particular function like this: *xt.returnFitResults()*.
>
> Happy early Thanksgiving!
> Thaddaeus

That's really great Thaddaeus! I was speaking to a friend of mine about some of the most rewarding aspects of doing research, and mentioning that coming up with an idea for how to solve a problem you are faced with, coding it up to do what you want, and then getting that running is a really rewarding enterprise. I'm sure engineers and mechanics building custom hardware feel the same thing.

Very well done!

After hearing back from Virginia (Cuneo) are you ready to reprocess the full set?

Happy early Thanksgiving to you too. Hope you have a great time with your folks and grandparents.

Best,

-Jack

> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Wed, Nov 11, 2020 at 12:07 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hey Thaddaeus,
>
> What a week it's been eh? (But way to go PA!)
>
> > I've held off from running another major fitting routine since the weekend because after I ran that my last one I decided that it was time to write some standardized scripts for wrangling the results from all the log files.

Excellent!

> Speaking of which, when Cuneo uses green triangles for "*parameters consistent with 0 within errors*", does this refer to situations when either the upper value of a parameter is equal to zero (because it got pegged at the hard upper limit) or when the lower limit is invalid and equal to zero?

Without being 100% positive, I'm pretty confident this indicates the lower limit *allows* 0 (i.e., a positive feature is insignificantly detected).

I *love* the outline of your routines, and "wow" is what I have to say about that. Way to go Thaddaeus, it's great you're tackling this and in a very useful manner.

> Out of curiosity, where did Cuneo get the count rate values for his data? I don't think it's the same as the Net Count Rate that's displayed whenever you execute *$show data* in XSPEC....can Cuneo's count rate (c/s) be found by dividing the sum of the counts in the included channels by the total observation time?

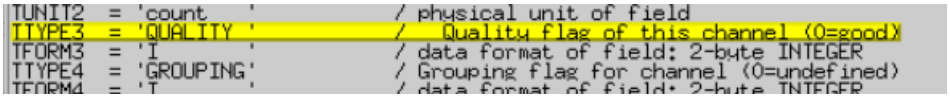
About 99% yes. "Usual" practice for NICER has become to normalize to an active set of 52 detectors. Your spectrum is a collection of all good events. So a tally of all the (good) events between 0.5-10 keV divided by the exposure time is indeed the count rate detected. However, if only using 49 or 50 detectors (also common), one would scale up by 52/49 to show the rate on the full instrument.

The one very tiny wrinkle to all this is a property called "deadtime". Each time the electronics on a detector register an X-ray event, they "latch" meaning that the detector stops listening for new signals. Effectively, the detector is turned off for a short amount of time it uses to calculate properties of event (what time it arrived, and how much charge was deposited). For NICER, this is typically ~20 microseconds.

As a concrete example of what this means in practice: when the count rate per detector reaches ~10^3/s (or in other words the full instrument light curve was at ~50,000/s), there would be ~20e-6 * 1e3 = 2% deadtime. This would mean that the *actual* count rate is ~2% higher than was detected (because the true exposure time was actually 98% the clock time). Here, most likely Cuneo didn't worry about folding in deadtime effects (I do have my pipeline correct for it automatically, so all your spectra already factor that in).

This is obviously a very small here for NICER, but I wanted to dive into it because for many missions that aren't timing beasts like NICER, deadtime can reach very close to 100% for bright sources. This occurs when tau_dead > t_event. where tau_dead is the deadtime per event and t_event is the average time between events. Instruments like NuSTAR deal with this for bright sources.

> Also sorta random, I came across a value called "quality" when I was looking at a file in fv; are the quality values the values XSPEC refers to when I say *$ignore bad*?



Yes exactly this. This won't tend to come up for NICER, but e.g., for instruments that have some bad pixels and a spatially dispersed spectrum (think the rainbow from a prism), or uncalibrated energy ranges, this can be used.

> * This is probably one of my favorite one-liner functions in Python:
>
> for line in f:
> linelist = (re.sub(' +', ',', line)).split(',')
>
> Where f is a file opened with: *with open(...) as f.*

Python is a wonder for its very efficient one-lining loops!

As usual, you're knocking it out of the park Thaddaeus.

In other news, a new grad student (Santi) just joined me and he's going to start looking at some QPO data.

Best,

-Jack

On Thu, Oct 29, 2020 at 12:49 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Thaddaeus,

Whoops - Santiago is my grad student and I notice my advisor brain accidentally called you Santi in my last message. Very sorry about that!

Name confusions aside, very glad the patch worked ok. Yeah, setting query to yes can cause problems sometimes as you found. I don't have good rules for avoiding or escaping those runaways, but I've had luck before tweaking the extra convergence terms in the fit command when it's stuck (the delta & beta) and the tolerance term in error.

-Jack

On Thu, Oct 29, 2020 at 11:34 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi Dr. Steiner!

Alright I think that patching did the trick; I'm currently running XSPEC v 12.11.0m. Surprisingly I didn't have to rebuild relxill which was nice. Now that the 12.11.0k patch has been installed, I think XSPEC just "snags" and keeps fitting if I have query set to yes instead of crashing—when I was looking into this, a particular observation ran an error routine for three days nonstop in the background until I aborted it!

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 22, 2020 at 12:46 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

Hi Santi,

OK, that's encouraging! Let me know if the patching does the trick. Often for small patches, you won't need to recompile local models, but you should quickly see if it has trouble loading and that would be your sign.

It's hard to predict how parallel processing will impact a given task. The outcome depends on where the bottlenecks occur (if processing, then expect significant improvement, if memory swapping, then probably not). For running on multiple threads, a **rough** estimate is sqrt(N) gain, but that's just ballpark.

-Jack

On Thu, Oct 22, 2020 at 11:56 AM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

Hi!

I just got some really great news on the crashes!

Last weekend I was dismayed because even though I was playing with the limits extensively (especially for gamma), the crashing was still occurring unabated. This made me wonder if it was a software issue, so I reached out to the HEASARC Help Desk. I found out that a very similar issue was fixed in XSPEC patch 12.11.0k: *For some complex fitting and error command cases, XSPEC may terminate with a GSL message such as 'SVD decomposition failed to converge'.* This wasn't encouraging at first because XSPEC patch 12.11.0k came out in June (I built the current version of xspec on my vm in ~July), but I checked my XSPEC version with *$version,* and it turns out that my XSPEC version is just 12.11.0! I'm working on installing the patch, I bet this will clear a lot of things up. I think I'm going to have to rebuild relxill after I execute the partial system rebuild.

The only thing that confuses me about this is that I didn't really have issues with core dumping when I was fitting relxill a lot back in August (I think it did come up for error fits back then though), so I'm confused why it really started acting up recently.

Also I don't think I've asked this previously, but out of curiosity, should I expect significant reductions in fit times with two core parallel processing?

Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi

On Thu, Oct 22, 2020 at 10:52 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Just checking in to see how fits are progressing and if playing with those limits has allowed any headway against the crashes?
>
> (Yes, whenever I "rm -rf " I try to take beat and check that I am not about to bilk the whole machine! With great commands-line tools comes great ...)
>
> -Jack
>
> On Fri, Oct 16, 2020 at 3:24 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
>
> > Hi!
> >
> > Now that I think about it, I do recall noticing that many (if not most) of the fit failures had gammas in the 3.4 range. Good to know about the 1.6-2.2 range for the residuals, I'll play with that now.
> >
> > Oh about the attachments: the screen shot from visual studio is just some descriptions I was writing about the different functions I was using, and the microsoft paint one just illustrates the positive contribution that is almost always eliminated by a ~1.75 KeV gaussian of varying width.
> >
> > Over this weekend should I continue to focus on maximizing the number of observations that I can get good fits and confidence intervals for?
> >
> > I'm intrigued by this disk truncation controversy, I'm going to read about it this weekend.
> >
> > Many thanks!
> > Thaddaeus
> >
> > P.S. I was reading about some dangerous linux commands the other day; there are some unnerving commands out there. Imagine if someone ran rm -rf in your terminal. Shudder.
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi
>
> On Thu, Oct 15, 2020 at 10:17 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
>
> > Hi Thaddaeus,
> >
> > > Regarding the application of systematic errors, that's good to know. I've often seen something like the "NICER team recommends the application of a systematic error of 1%" in papers that use NICER data.
> >
> > Yes, that's a "rough" benchmark, but a reasonable one.
> >
> > > I love the common.tcl file, it's so handy! I think I'm just going to use it from now on because it's simpler on the execution end (just two commands), and it shows the error strings for each parameter.

Yes, it's quite nice work by Jeremy :).

> A little random: I came across an interesting statement in a paper by Plant et al. 2015 pertaining to observed disk truncation in the hard state of GX 339−4: “This result implies that the current sample of spin estimates in the hard state are inaccurate. Therefore, any distinct conclusions drawn from these estimates, such as the spin-powering of relativistic jets, may well be biased.” Since that paper was published has this issue been fully exploited and resolved, or are there still black holes with possibly incorrect spin measurements out there? Also, is physical disk truncation or changing coronal height the more widely accepted cause for profiles similar to disk truncation?

This has been a topic with a lot of controversy that is still ongoing (Garcia et al 2015 and followup papers by Wang et al. 2019 talk about this). I'd say the Plant picture is strongly disfavored at present, but not entirely out. The robust view is that allowing for the possibility of truncation, a (bright) hard-state spin measurement should be regarded as an estimated lower limit. Presently, the argument is focused around how much truncation could there be in bright hard states, with most reasonable numbers being a few times the ISCO (a factor <10). (Coronal geometry is always an important consideration, but usually they tend to fit very compact.)

> An update on the rsp/arf issue: I modified my approach and fully integrated astropy.io.fits, so when I’m producing my xspec script commands rmf and arf file names are pulled directly from the header of each observation’s fits file. Because of the two errors described below, I haven't checked how chi-square improves when the correct rmf and arf files are used though.

Very good, that should make some impact when working.

> I’ve encountered two weird errors I’ve never encountered before in XSPEC this week: the first is frequent core dumping [SVD decomposition failed to converge…Default GSL error handler invoked…Segmentation fault (core dumped)], and the second is this odd “XSPEC Error: No variable parameters for fit” after executing “fit” for a model with *multiple* free parameters (including the usual Gamma, FracSctr, Tin, diskbb norm, etc.). My first thought was “*is this related to relxill having problems due to a low gamma”*—but these issues both happen even when Gamma is > 2.

The table can be sparse in a few places, and I think very large Gammas are also sparse (>~3.4, if memory serves). I'd say try freezing parameters and retrying the fit to identify the culprit parameter.

> Also a little random: this week I decided it was time to start commenting on my functions in my python files—at one point I was looking through some older files for a particular technique I used and it took a little while to find the correct version of it, so I decided that from now on I’m describing my functions for future reference.

Yes, documenting code is super important and a great habit to get used to. (I wish I was better about it!)

> This is preliminary, but I attached an example of a chi-scape scarred by a common morph of the residual I last sent you. The positive contribution that begins shortly after 1.6 KeV is almost always eliminated by a ~1.75 KeV gaussian of varying width for most observations. The current issue is modeling the various forms the following negative residuals take. Currently working on those. Earlier you mentioned that we could try ignoring that narrow range where these residuals occur, would ignoring channels between 1.6-2.2 KeV be ignoring too much of the data? If we decide to ignore that range, I would think it best to get >*plt wdata* outputs to exactly nail down the dimensions of this range, but for now should I keep trying to model the residuals?

> I was reading through some older emails, going to look into your recommendation regarding logxi.

No, I think 1.6-2.2 isn't crazy and you can certainly roll with that if you are having too much trouble taming the residuals with a model. Maybe do a couple test cases to see how the model parameters are affected on both fronts.

Hey, and I didn't see a description of your attachments in case you want to say anything about those.

Glad you're back in the thick of things.

Best,

-Jack

> On Thu, Oct 8, 2020 at 1:35 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
> > Hi Dr. Steiner!
> >
> > Quick note: I've been getting back into it this week and should have some interesting things to report this weekend!
> >
> > Thaddaeus
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
>
> On Tue, Sep 29, 2020 at 2:19 AM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> > Hi Thaddaeus,
> >
> > Thanks for the update.
> >
> > > Quick note on the residuals: since we last communicated I investigated the application of a systematic error (3-5%) in the < 2 KeV energy band, but this didn't improve the residuals so I'm assuming that you already accounted for this, like the binning, in your reprocessing?
> >
> > No, actually. But systematic errors are only incorporated with some fit statistics - chisq for instance, but I don't believe are incorporated in cstat or pgstat. The way I like to think about it is the systematic error acts as a reweighting of the data's constraint and serves to de-emphasize regions with the highest signal. I offer for information sake that that 3-5% is too large a systematic error to include as a rule. Something like 1-2% is more reasonable for a region with poorer calibration quality.
> >
> > > I also discovered that I've been unintentionally forcing an incorrect response file on multiple MAXI J1535-571 observations—the rsp I was using for all observations was `nicer_d49_55575341.rmf`, but after running a quick check (the fitsio package has been so helpful, thanks for recommending it to me!), I found that multiple observations request `nicer_d49_u100_150_55575341.rmf` in their headers.

Ah! That's very good to know, and may be the culprit for some of the residuals you've been battling. Check that the arf files (keyword ANCRFILE) is also set correctly.

> I'm likely almost done with the SAT (took it Saturday, going to take it again this coming Saturday)*, and once I'm done I aim to return to a progress pace similar to the pace we had in the summer.

Congratulations - that's a nice milestone!

> G'mar chatima tova (if you observe Yom Kippur)!

Same to you.

> *I might take it again in November in the event that I bungle those tests.

I'd put dollars to donuts you did great!

My best,

-Jack

On Tue, Sep 22, 2020 at 1:21 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Interesting I'll look into that!
>
> *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> -Enrico Fermi

On Tue, Sep 22, 2020 at 1:15 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Similar to that. I used another language (IDL) to automatically produce a series of tcl/tk scripts and then executed those distributed over a number of cores on a workstation.
>
> -Jack
>
> On Tue, Sep 22, 2020 at 1:13 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:
>
> > Sounds good!
> >
> > In that case did you operate solely in TCL?
> >
> > *"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."*
> > -Enrico Fermi
> >
> > On Tue, Sep 22, 2020 at 1:10 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:
> >
> > > Hi Thaddaeus,

> For >75% I meant observations where source flux was more than 75% bg flux, and for 4% I meant observations where source flux was ~4 % bg flux. That makes sense—interestingly the observations with source flux ~4% bg flux were, by best fit, in the SPL state, with the highest flux and gamma for all the observations that had reasonable values for these parameters.

Ah, now I see. Yes, when the bg flux is very high, the noise level (N=sqrt(B+S)) where B and S are background and signal counts in a channel, then the constraining power of the data (S/N) is lowest. If the background is reasonably estimated, the fits will likely be good but poorly constrained; if the background is off, then all bets are off.

> xD. I think it's a very impressive feat! Did you use pyxspec to loop through all the observations?

Thanks. I used a different language actually, but python/pyxspec would be an equally good way to do it.

Best,

-Jack

On Tue, Sep 22, 2020 at 12:39 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> > That's good to know. I guess what's surprising is some relative of this large residual in the 1.6-2.0 KeV is present in most of the source flux > 75% observations (I wonder if the peak feature falsely inflates the source flux for some of them?). Going back to a couple emails ago, I was checking some of my all time best fits which had source fluxes around ~4%—what allows such faint observations to fit well?
>
> Just to clarify, do you mean 75% and 4% of the Eddington limit, peak luminosity, or something else? Fainter observations, given some typical observation time, obviously have fewer counts which means larger fractional errors and less constraining power on the model. So if there is a deficiency in the calibration or model being used, it will look more prominent in brighter + long exposure time data.
>
> > Also, I'm very intrigued by your methods in one of your 2016 papers , "STRONGER REFLECTION FROM BLACK HOLE ACCRETION DISKS IN SOFT X-RAY STATES". How did you automate fitting the 15,000 observations reliably and within a reasonable time frame?
>
> A reasonable time frame is relative :), but I used about two dozen cores to run the fits which helped. I also stuck with computationally simplistic models, and for my runs with simpl, used lower resolution (via energies) in the run, since RXTE/PCA - the instrument used there - also has rather poor energy resolution compared to NICER.
>
> > Could I possibly have the ~86 observations between obsids 1200220101 – 1200220186?
>
> Spectral files are attached. Response files are too big for me to attach, so I'll keep them up here for a few days: https://nicer-web.mit.edu/nicerview/thadd_resp_m1727.tar.gz
>
> > Ah that makes sense. In that case did Xu et al. just freeze Rin at the ISCO to have less free parameters?

That's the usual tactic, yes, when fitting for spin.

Best,

-Jack

On Thu, Sep 17, 2020 at 11:00 PM Steiner, James <james.steiner@cfa.harvard.edu> wrote:

> Hi Thaddaeus,
>
> Great to hear you've been digging in and exploring! I would suggest though that you avoid being too concerned about the residual features. If something is localized and only ~percent level deviation, then that's likely sufficient. *The goal should be to fit those sufficiently that they don't alter the continuum, but at some level it becomes diminishing returns to try fitting an artefact.* That said, if you wish, I can get you the data on MAXI J1727-203. Just let me know if you have a particular ObsID from Kevin's paper you want to try.
>
> Your question about degeneracy is great. The condition of parameters being strongly correlated with one another is degeneracy. Basically, this is a very common situation where two parameters are dependent upon one another to some extent, so a probable value of one will condition a probable range of values with the other. For instance, if you want to fit x and y with the constraint x^2+y^2 = 4+/-1, you would get a ring of radius 2 with x,y values that obviously depend on one another; they are *not independent*. Speaking generally for more realistic cases, the interaction between parameters is often pretty convoluted, but commonly things like Gamma and Normalization are correlated with one another, so a contour plot of the confidence interval would look banana shaped rather than like a circle.
>
> Best,
>
> -Jack

On Thu, Sep 17, 2020 at 9:18 PM Kiker, Thaddaeus <s024622@students.lmsd.org> wrote:

> Hi!
>
> Thanks for checking in! Since we last spoke I've been taking a deeper look into a couple articles (Alabarta et al. 2020, Xu et al. 2018, and Parikh et al. 2019) and playing around a bit with the residual fix you suggested. As tested on multiple observations with varying fit statistic contributions in the 1.6-1.8 KeV range (chi-squared contribs. ranging between 500-3000), the gaussian centered around 1.75 KeV combined with the "edge" works great!
>
> Most importantly, I found a method to more accurately investigate the residual features: *plt>wdata*. Now that I can actually see what the chi-sq contrib. is for every point, I've been having a lot more success in approaching residuals in the 1.6-2.2 KeV range, especially when the residual patterns after 1.8 KeV differ greatly from what can quickly be ameliorated with the edge feature (it seems that for the majority of observations the <1.8 KeV excess is easily subdued by the gaussian fix, but after 1.8 KeV things often get complicated, especially when a third

feature shows up between 1.86 to 2.0 KeV. This third feature resembles a sort of gaussian-like crevice about 0.13 KeV in width and often reaches a chi^2 contribution of -5331 (+) at its minima, pushing the "edge" feature out to a threshold energy of ~2+ KeV). I haven't found a definitive solution yet, but a gaussian absorption with an energy of ~1.86 KeV and a width of ~0.14 KeV is looking promising.

While I've been working with these MAXI J1535-571 residuals I've been wondering if the residuals Alabarta et al. discuss in Nicer data for MAXI J1727–203 are of similar shape—is there any chance I could procure some pipeline data from you on MAXI J1727-203 to look into this (if it's not an inconvenience for you and or you don't think it's a distraction on my part of course).

Out of curiosity, what does "parameter degeneracy" mean? My first guess would be this example: if I only told you what the slope of a line was (e.g. '2'), and then asked you for the individual values of Δy and Δx—would Δy and Δx be degenerate? Example of where I came across this: (from Xu et al. 2018) "The inner disk radius, R_{in}, and the black hole spin parameter, a*, are degenerate, as they both control the effective inner accretion disk radius. Therefore, for simplicity, during the spectral fitting we assume that the inner disk extends down to the ISCO by fixing R_{in} at the radius of the ISCO, and fit for the black hole spin as a free parameter."

Best!
Thaddaeus

"Before I came here I was confused about this subject. Having listened to your lecture I am still confused. But on a higher level."
-Enrico Fermi