Reply to Daniel & Glenn

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July 3, 2015

1 CLARIFICATIONS

> ***1) How do you scale (if you scale) the residuals you subtract?***

The residuals are not scaled. I simply took the median of the residuals that have the same dither position and position angle, and subtracted the median combination from the original images. Since The chisq distribution of the fitting looks very reasonable for me, I did not attempt to make further adjustment of the residual models.

> ***2) How do the residuals images look like? What is the relative fraction of the light that is not subtracted well?***

> ***3) When you calculate the brightness of the sources, how do you use the information of the residuals and the TinyTim-based PSF?***

These are very important points. However I did not deal with them very carefully in the fit. As I showed in our previous discussion, Tinytim does not provide a precise PSF model so that the residual of tinytim PSF subtraction has a 2% - 4% average relative fraction of original image(mean(residual/image) ~ 2%-%4). For the fitting routine I used this time, the fluxes of A and B purely calculated from the amplitudes of the two components of the PSF. I did not count any flux from the residual model into the fluxes of A or B.

An average relative fraction of 2-4% of the residual model could be a significant source of uncertainty. Especially for the case that I ignored the flux from the residual totally. However, I have not come up with any ideas on including the residual flux to the final result. I think we need further discussion about this point.

> ***a) When you say you carried out a "two component tinytim PSF fit", it is not clear to me what you mean. I assume the linear superposition of two TinyTim PSFs. But do you mean one for A and one for B that are otherwise identical (or perhaps different in some manner other than brightness?). Or do you mean for B (and perhaps also A) you have used a combination of TinyTim PSFs that, in combination (perhaps with different defocus of color terms?) better represent the observed PSF?***

I used different PSFs for A and B. My fitting routine firstly generates a list of PSFs using the position of A and the NIR spectrum of A (Bonnefoy et. al. 2014) with different telescope jittering. I fit this PSF to the image of A with an area where the image of B is mostly excluded (using a 3-pixel radius mask centered on B) to find the best matched position and Jittering. Next, I used this jittering to generate a PSF of B with B's position on detector and B's spectrum (Patience et al. 2010). In the third step, I fit the two PSFs together with the position of A fixed , and to find the best matched position of B and amplitudes of 2 PSFs.

> ***b) To me it is interesting (but not really unexpected) that binning by "same position angle" (i.e., spacecraft roll angle) improves. Off-rolling does thermally destablise the PSF to some extent - by changing the angle to the sub-solar point on the Earth as the telescope orbits and can drive quasi-breathing modes. I am a bit surprised that these stabilize out as well as they seem to do (as exhibited by the factor of 10 improvement in chisq). It would be interesting to know how much of that improvement is do to "same dither position" and how much to "same roll" (as differently binned). I am sure they both play together to reduce the residuals, but knowing this could have influence on how future observations might be constructed.***

The position dependency of the residual pattern comes from at least three parts.

1. Instrumental as Glenn described

2. The imperfection of flat field is not modeled with TinyTim.

3. The PSFs are sampled differently at different position with a low sample rate.

It is actually difficult to differentiate the improvement in terms of different instrumental effect, since the there are significant uncertainty and artifect generated by last two points.

> ***Overall, these look good; the question is, of course, how much of the apparent trend is real. One note here: It is curious how both A and B have an increase in the first 3 orbits and then a different behavior in orbits 4-6.***

> ***Could you explain how exactly do you determine the normalization factors for the different angles/dithering positions?***

I median combined the fluxes calculated with images taking with same filter, dithering position and roll angles, and devided the median combination from every photometric result of these exposures.

2 Discussion and further analysis

> ***Daniel asks: "how much of the apparent trend is real". This might be difficult to ascertain. Clearly the F125W and F160W for A seem well correlated (1st 3 vs last 3 orbits) , but that correlation possibly could still be instrumental. If you took the new light curves as binned, for B:***

> ***if you average all the points together in each orbit (loosing the finer temporal information) is the change in normalized flux for B over the six orbits also "consistent" from F125W to F160W **AND** is it correlated with A? It's a little harder to tell (quantitatively) as plotted for B simply because it is noisier than A. In both A and B (both bands) the flux (by eye) also seems lower in the last three orbits and MAYBE the is a small secular rise in orbits 1-3. I would expect that behavior in A and B to be independent, whereas if correalted that may be an instrumental signature.***

I binned the light curve of A and B by orbits using average. According to the result I obtained, there are no clear correlation/anti-correlation betwee the fluxes of A and B. Figure 2.1 presented the orbit-binned light curves.

> ***This looks quite nice and indeed, the 10.9 hours is not a period that I would obviously associate with an instrumental artifact. Have you tried the same for the F160W LC? It would be a nice verification if you would get a similar periodicity.***

By eye, unbinned light curve of F160W does not show a sine-wave like shape. However, in the orbit-binned curve, the light curve of B showed a sine wave shaped curve as shown in Figure 2.1. For F160W LC of B, The least square fit results in a sine curve with a period of 9.3 hr. I also tried to fit the F160W light curve with a sine curve that has a fixed period of 10.9 hr. I plotted the two best fit sine curves together with the original observation points as in Figure 2.2. Considering

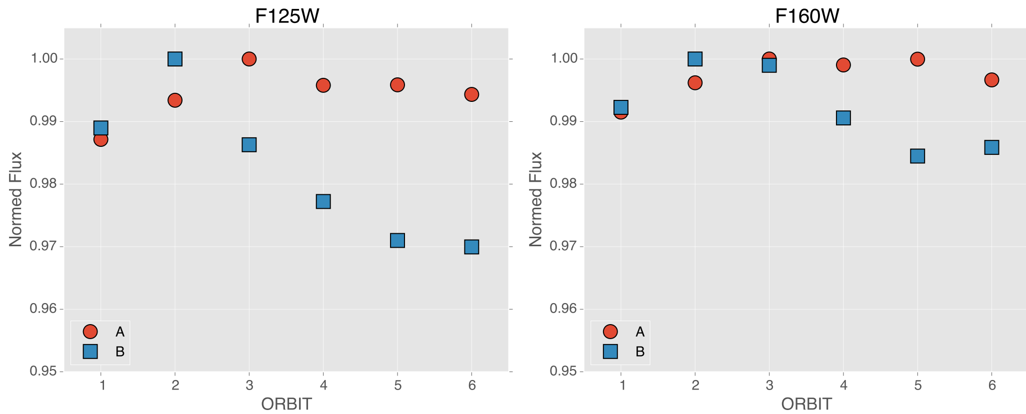

Figure 2.1: Orbit-binned light curves. Binned curve for B showed a sine wave shaped curve

the large scattering of photometric measurement of B, it is actually hard to tell from Figure 2.2 whether the 9.3 hr light curve fits better than the 10.9 hr light curve.

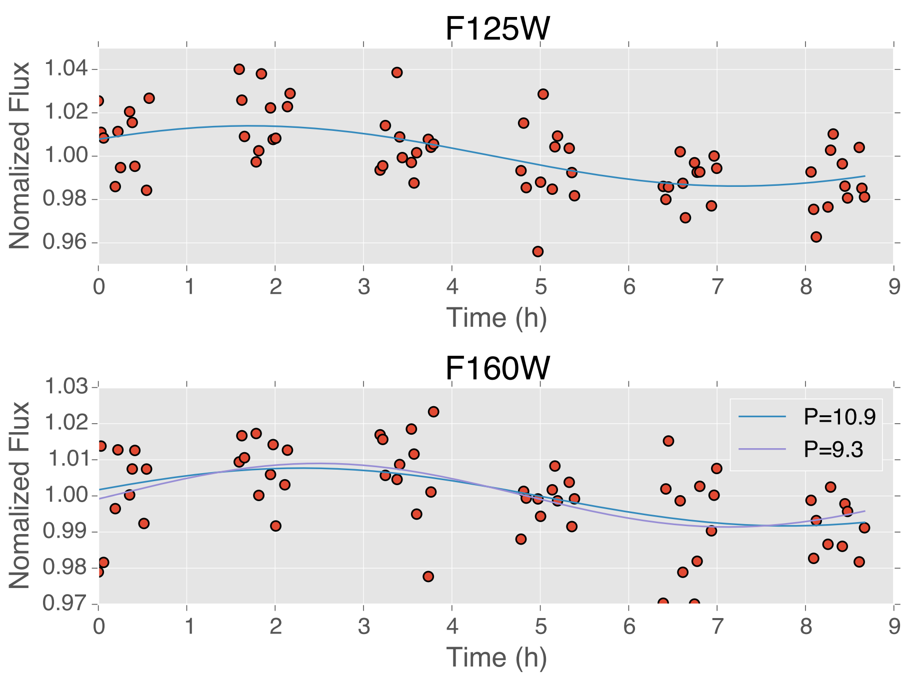

Figure 2.2: sinusoidal wave fit to the light curve of 2M1207B. For the F160W panel, two curves for periods of 9.3 hr and 10.9 hr are plotted.

> ***Also, to be consistent, can you please use the same fitting procedure for A (in both filters) and send similar plots?***

I did the fitting for A in both fittings. The rise in first three orbits is the main feature that defines the sinusoidal wave. However if it were truly part of a sin wave, the sine function would not be well constrained by this small segment. For the light curve of F125W, the fitting routine found a best fitted curve with a period of 6.86 hr. However the points in the 5th orbit are totally off the fitted curve. For F160W, the fitting routine failed to converge when fitting a sine curve to the light curve. Figure 2.3 presented the fitted sinusoidal wave and the observational points for 2M1207A with F125W.

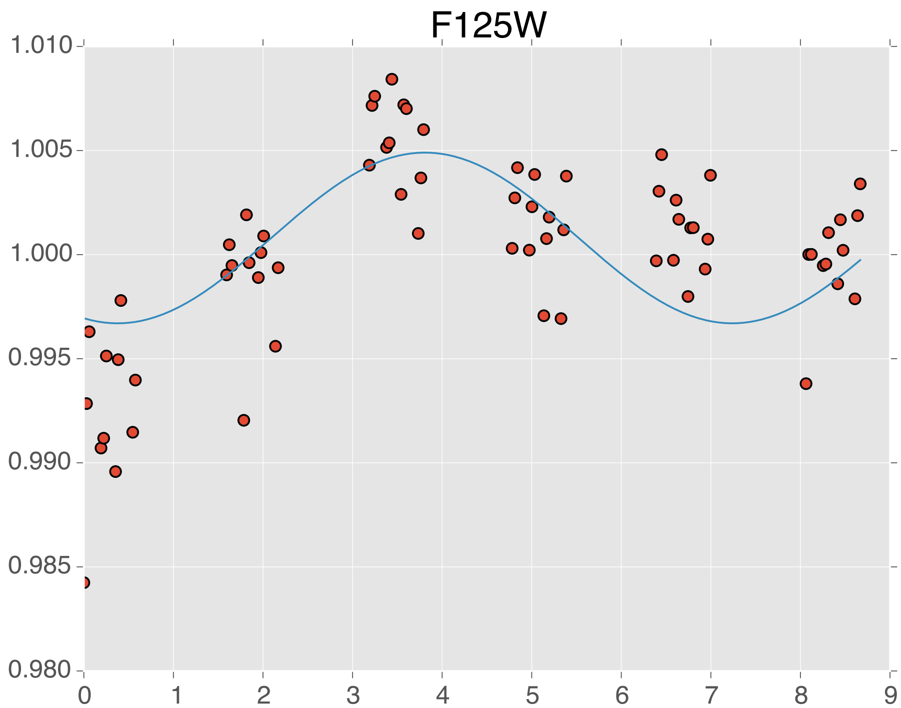

Figure 2.3: sine wave fitting for 2M1207A.

> ***(c) I assume the 10.9 hour fit came from a least-squares analysis – not quite a full observing/sampling period. "By eye" it certainly loooks good, but might be interesting to also do a simple periodogram analysis, just to see if this might be an alias?***

I thought about this idea before. However, 10.9 hour is longer than the observation baseline and we if that were the case, we did not have a periodical signal. I was wondering whether the periodogram would work for data that does not cover a whole period.