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# Angle Wheel from 2013.

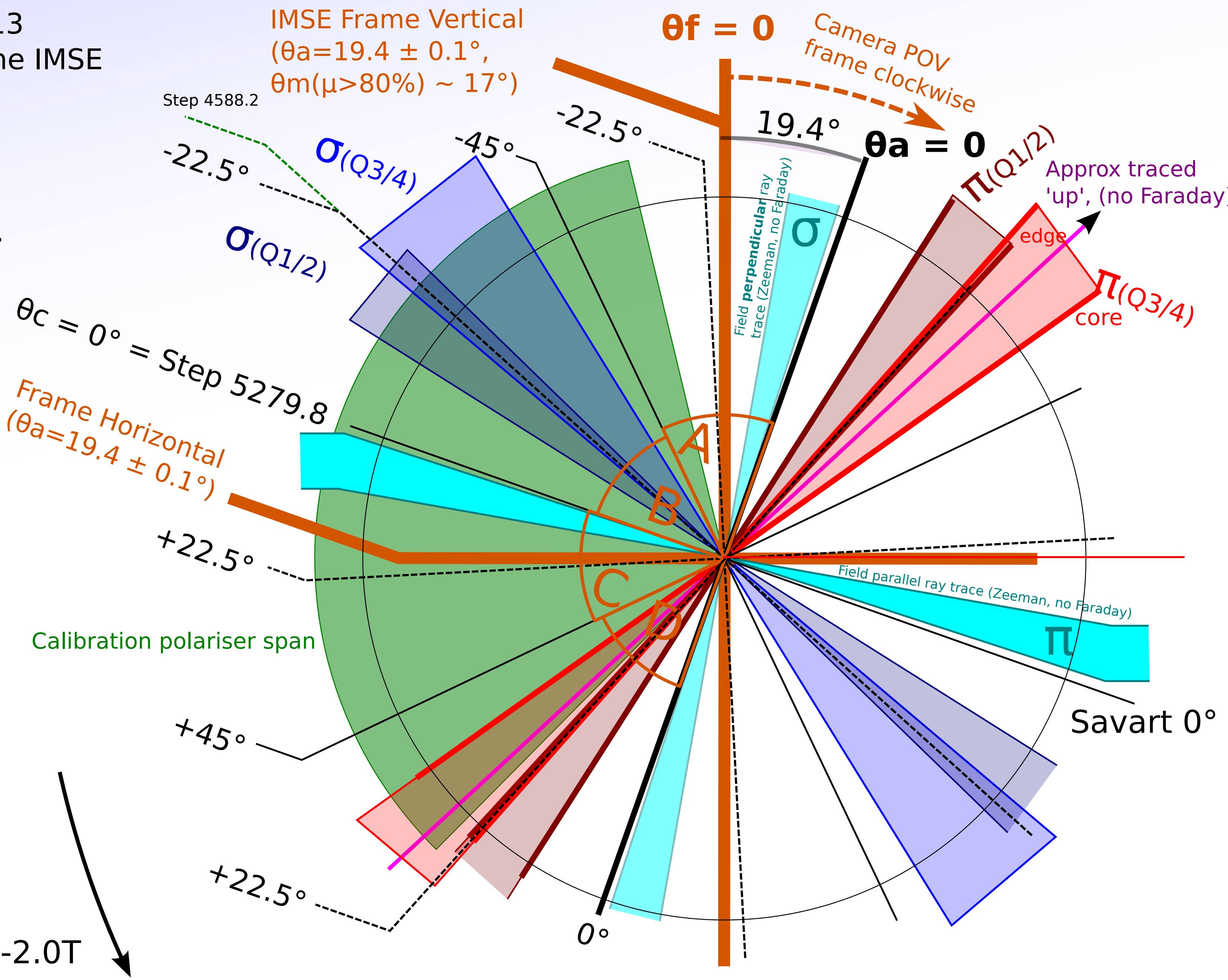
All of the angles from 2013 experiments relative to the IMSE frame.

θf = Clockwise, full 360° from 0° as up to IMSE frame.

θm = Measured angle from demodulation of image, relative to Savart ±45° periodic.

θa = 'Actual' angle relative to Savart, ±45° periodic. θm corrected for intrinsic contrast.

θc = Angle of calibration polariser. Same as θa without the 45° periodicity,



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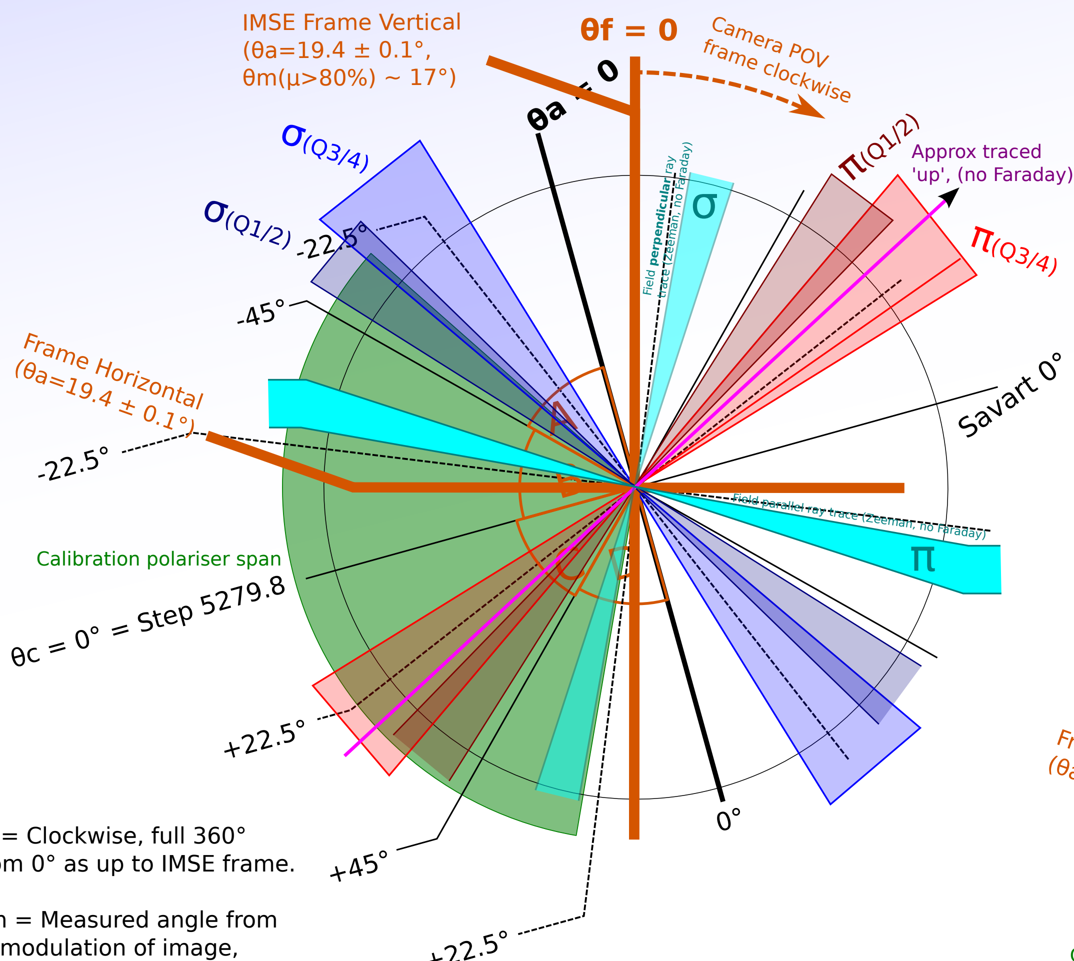
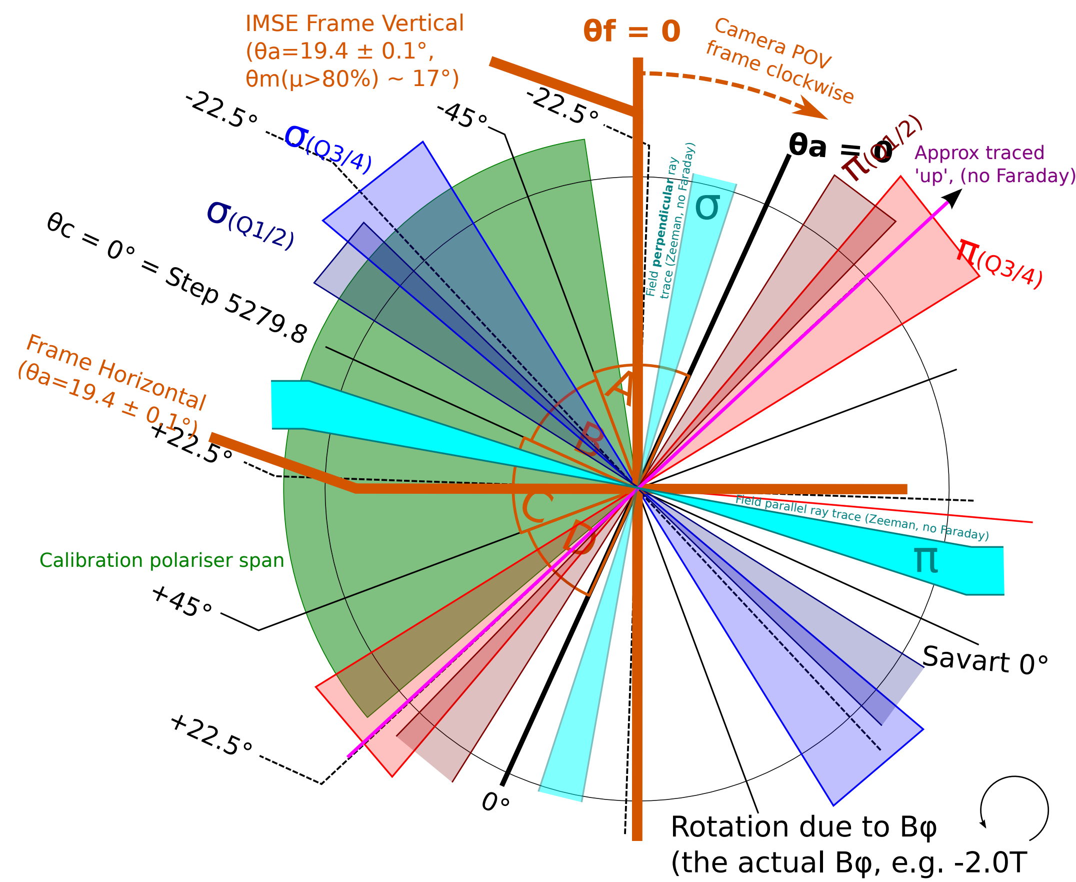
# Angle Wheel candidates for 2014

We want to rotate Savart plate to move Zeeman polarisation away from 0°.
All the MSE must stay well away from 0° and at least a little away from 45°.




Option 1:

Rotate Savart CCW by 35°.
Zeeman now at ~30°
Q1/2 good at > 30°
Q3/4 in 15° < $\theta_a$ < 35°.
Opposite sign to last time. (in next sector)




θf = Clockwise, full 360° from 0° as up to IMSE frame.

θm = Measured angle from demodulation of image, relative to Savart ±45° periodic.

θa = 'Actual' angle relative to Savart, ±45° periodic. θm corrected for intrinsic contrast.

θc = Angle of calibration polariser. Same as θa without the 45° periodicity,

Option 2:

Rotate Savart CW by 5°.
Zeeman now at ~10°
Q1/2 not good at > 7°
Q3/4 in 15° < $\theta_a$ < 35°.
Almost same as last time

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# Angle Wheel for 15/05/2014.

(Just to see what happens)

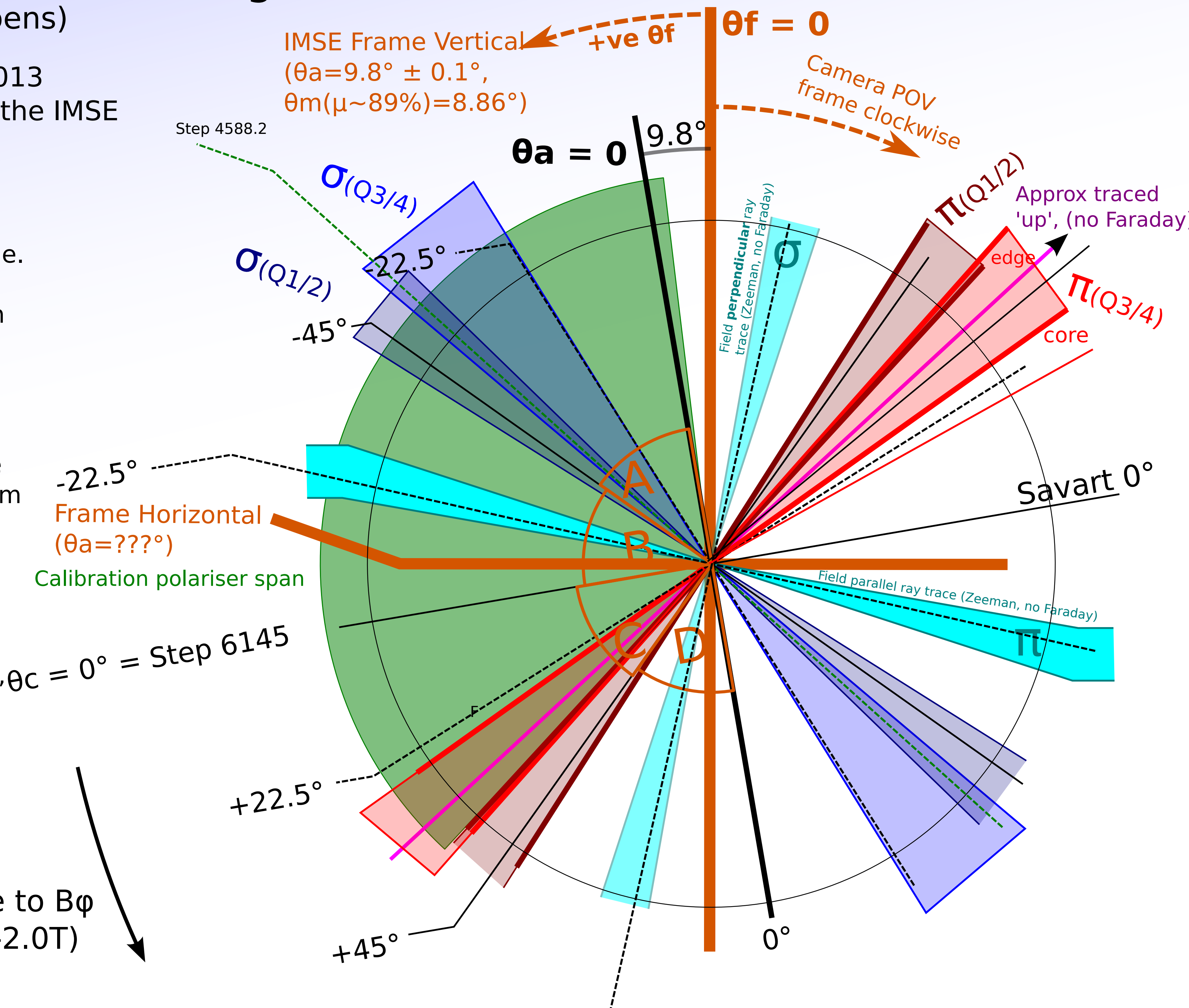
All of the angles from 2013 experiments relative to the IMSE frame.

θf = Clockwise, full 360° from 0° as up to IMSE frame.

θm = Measured angle from demodulation of image, relative to Savart ±45° periodic.

θa = 'Actual' angle relative to Savart, ±45° periodic. θm corrected for intrinsic contrast.

θc = Angle of calibration polariser. Same as θa without the 45° periodicity,




Faraday rotation due to Bφ
(the actual Bφ, e.g. -2.0T)

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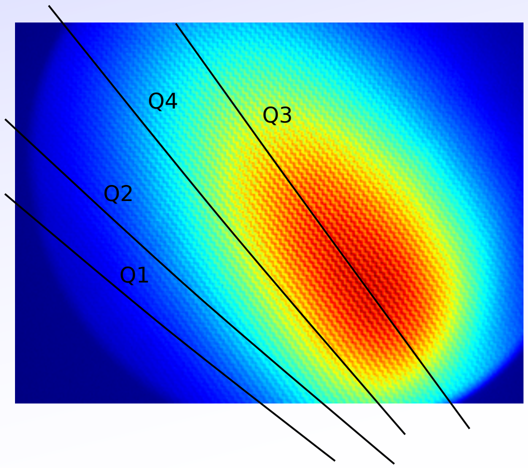
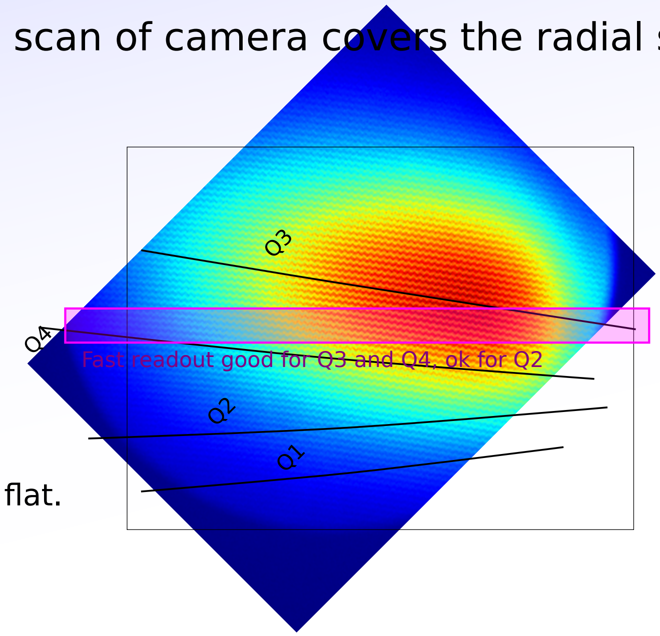
# Setup for May 2014

Also need to rotate camera so that central scan of camera covers the radial scan, for high-speed runs:
The April2013 data looked like:

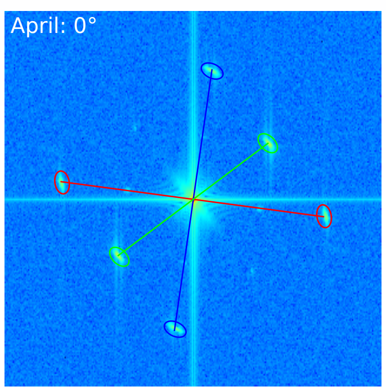



Rotate camera 45° CW gives:

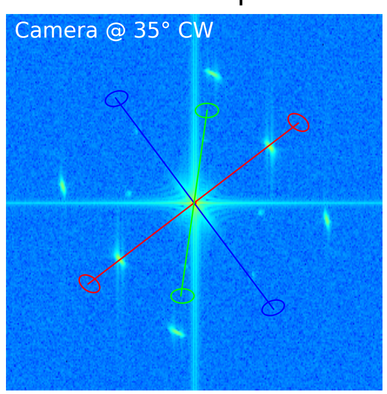
April was 10° CW so we need to go to +35° CW from flat.



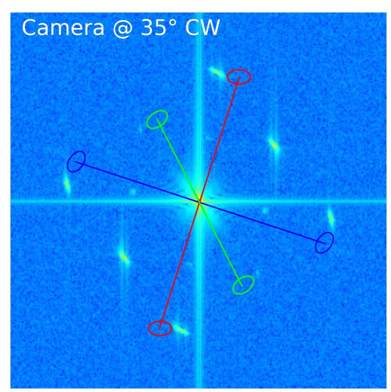

April's FFT looked like this:




Rotating the camera +45° CW rotates the components 45° CCW

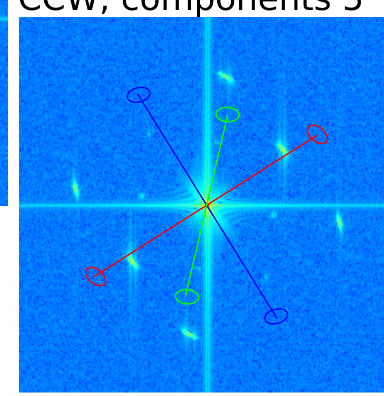



But, we plan to rotate the Savart 35° CCW, which is the same as rotating the camera a furhter 35° CW, or the components further 35° CCW:




So we should avoid the components hitting the spectral leakage.

The other option (Savart 5° CW), effective camera 5° CCW, components 5° CW.



This isn't great.

but.... The new camera will be a lot larger image area (if we don't change the lenses), so might not get the leakage anyway as the pattern will not hit the edges

# Improvements

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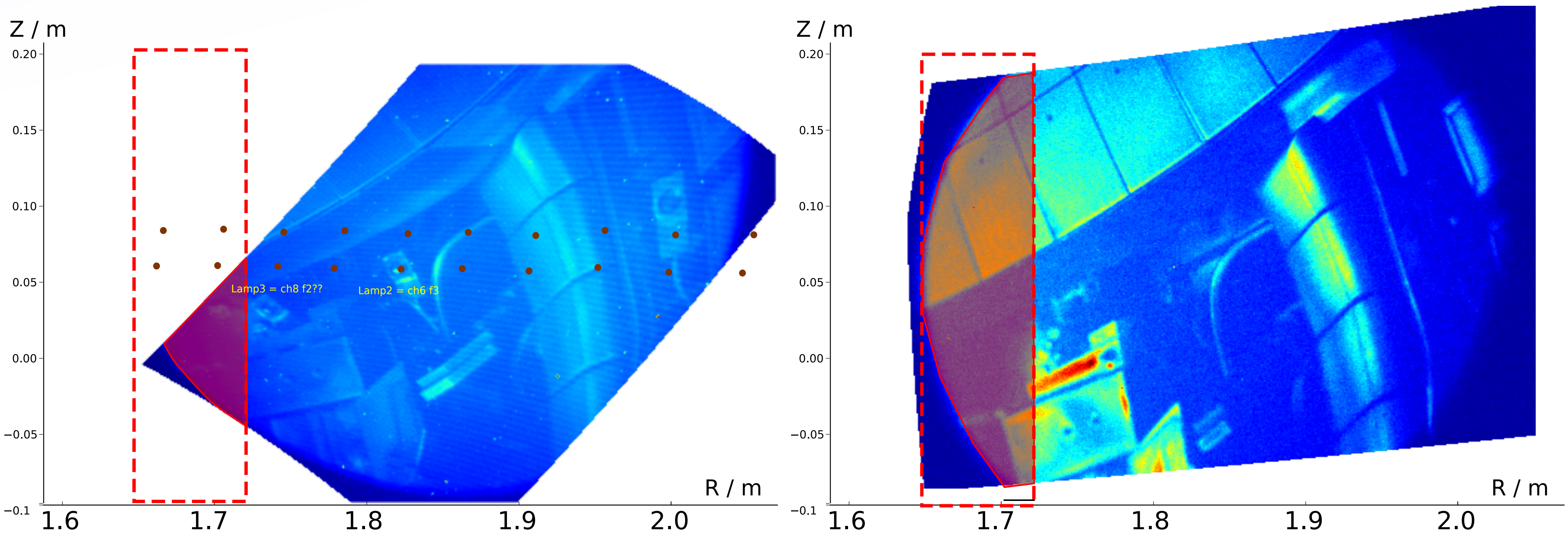
gmds/AUG/29317

- Further improvement to filters --> More signal + less background.

- New camera.
  - Faster - 5ms normal operation, down to 1ms possible with reduced viewing area.
  - Higher sensitivity and lower noise.
  - More flexible configuration.
  - but... much more sensitive to radiation.

- Changed the MSE mirror to view more of the plasma core.

With the new camera, available data on core is much more:



# Sensitivity Improvement

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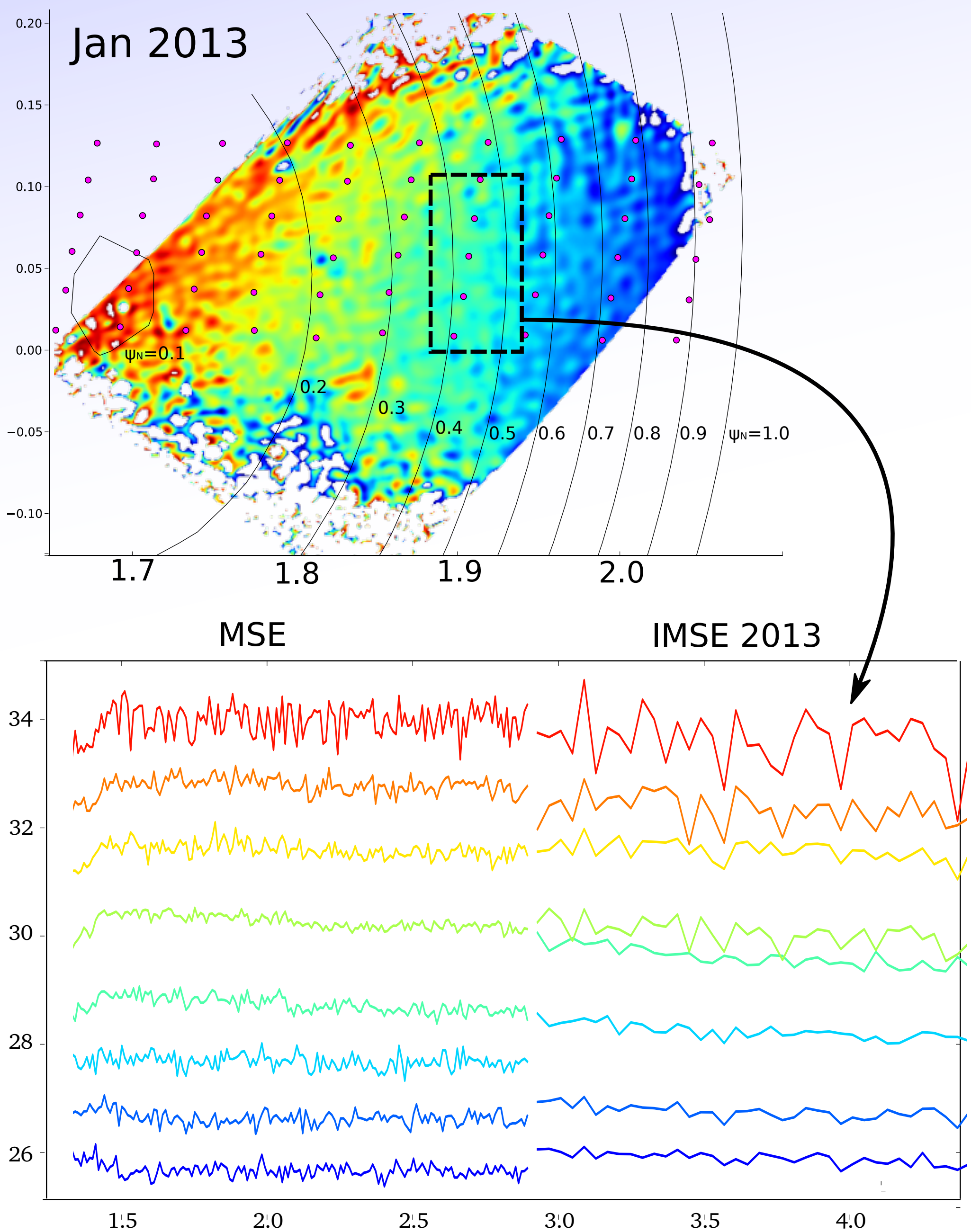
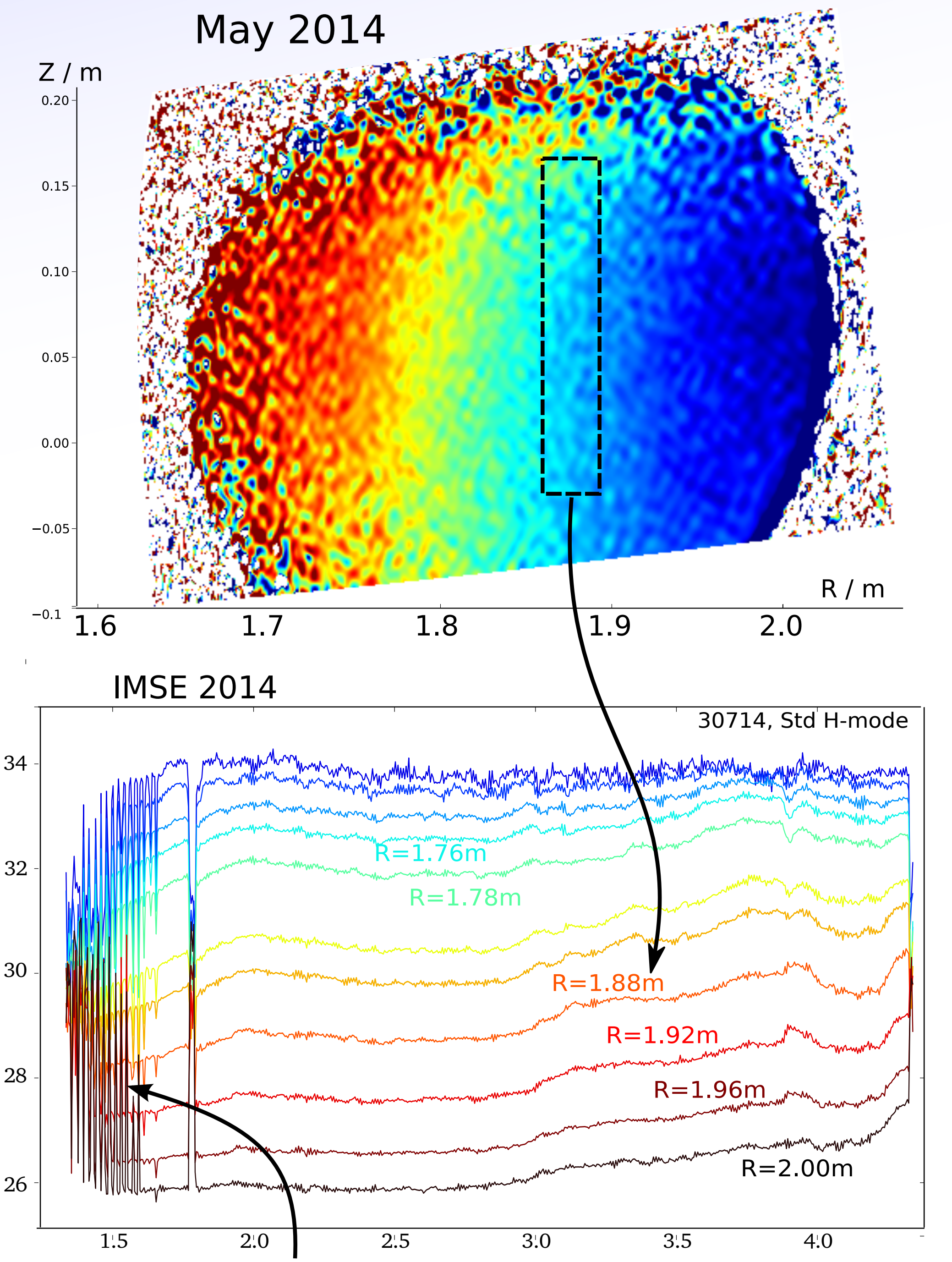
Improved both signal to noise and time resolution:




IMSE not effected by beam start-up voltage changes. Blips + modulation work.

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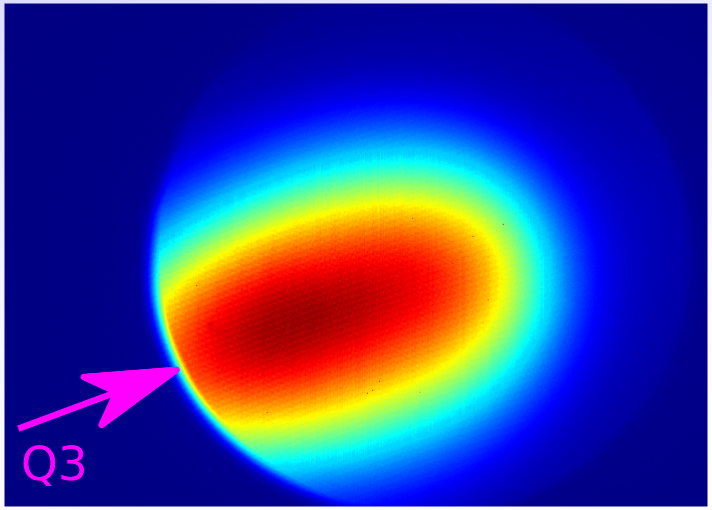
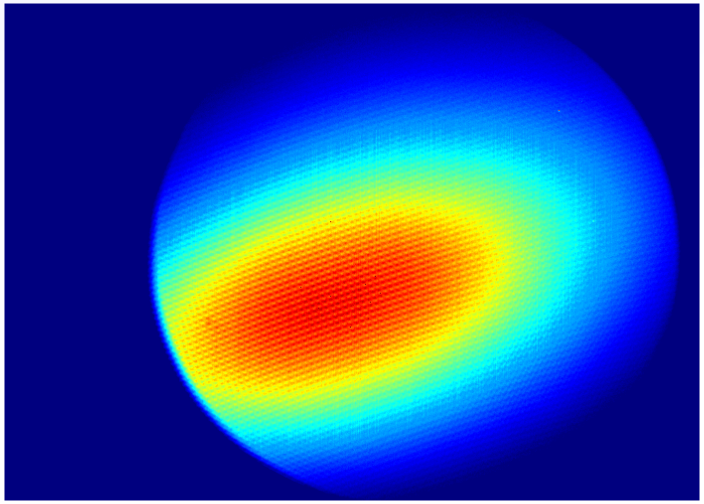
# Beam into gas.

Recorded data for all the beam into gas shots on wednesday.

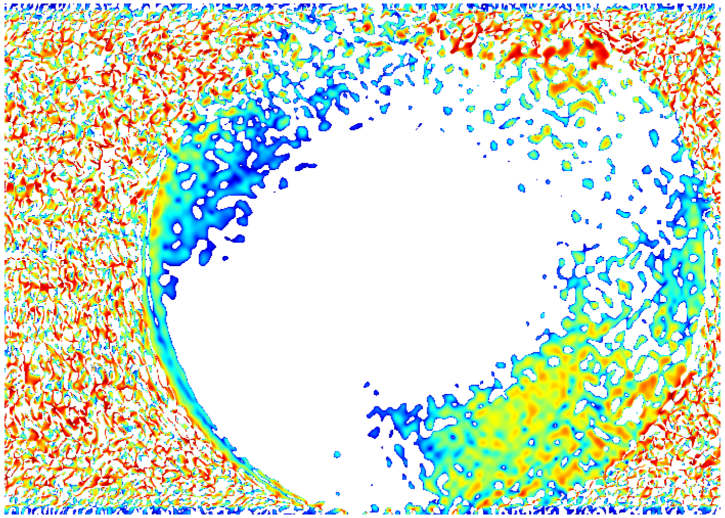



No Bt --> No polarisation --> No fringes



Bt -->Polarised --> Weak but OK fringes.

Attemped demodulation:



This did not work for the IMSE at all, even with the calibration polariser in front of the diagnostic.

Possibilities:
a) Strong polarised background signal ("Secondary Neutrals")

b) IMSE is optimised for the plasma spectrum, something could be very different for the beam into gas spectrum.

# Camera Faults

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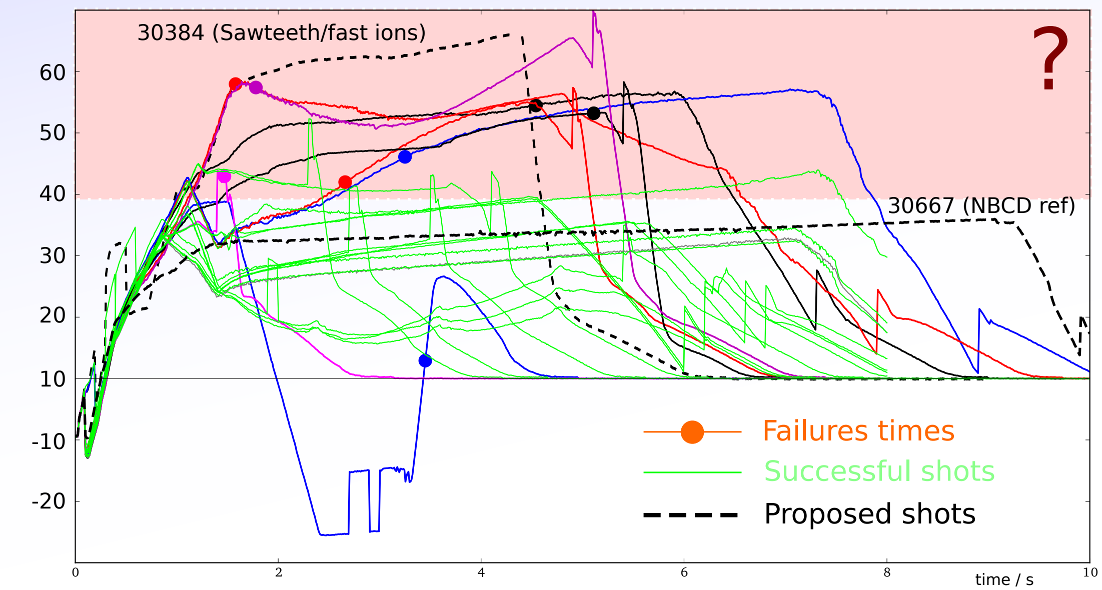
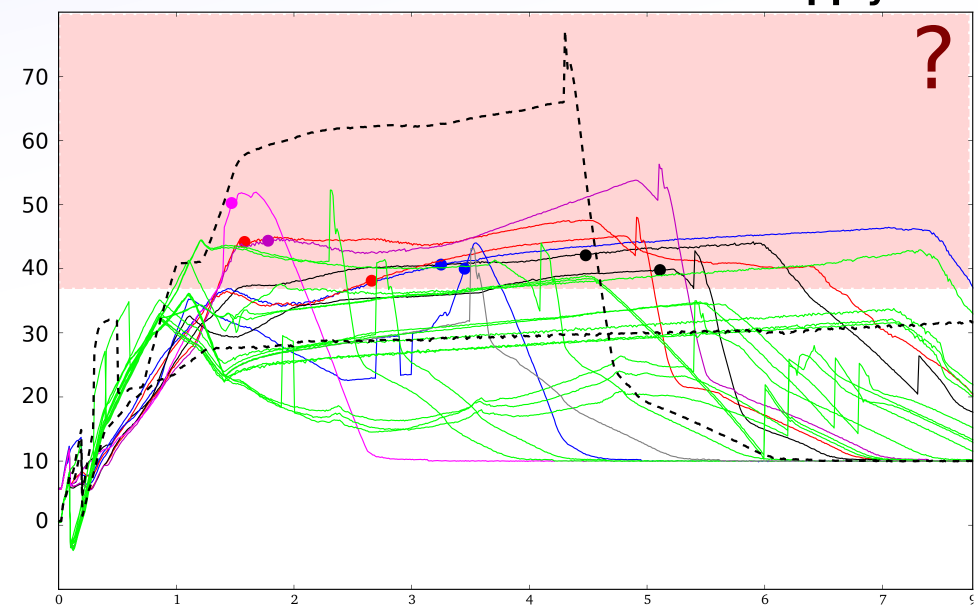
During the second to last week, the camera stopped during many shots.
Eventually found some correlation with the magnetic field at power supply and/or camera.

Calculated vertical field at IMSE **camera**.




Calculated vertical field at **Power supply.**




Replaced power supply with a full variable power supply in rack, wired the 12V (up to 7A) forward to the camera. Raised supply to > 13.5V so that voltage at camera is ~12.5V with cooler on. That didn't fix it the drop-outs, but is probably a good idea anyway.

Added 5mm Iron around camera - this solved the problem - All shots on Tuesday afternoon worked without a problem.

Camera will be used in W7X and was tested up to ~100s mT - need to examine the exact test conditions and reconsider results!

# IMSE vs MSE - augMay2014 initial

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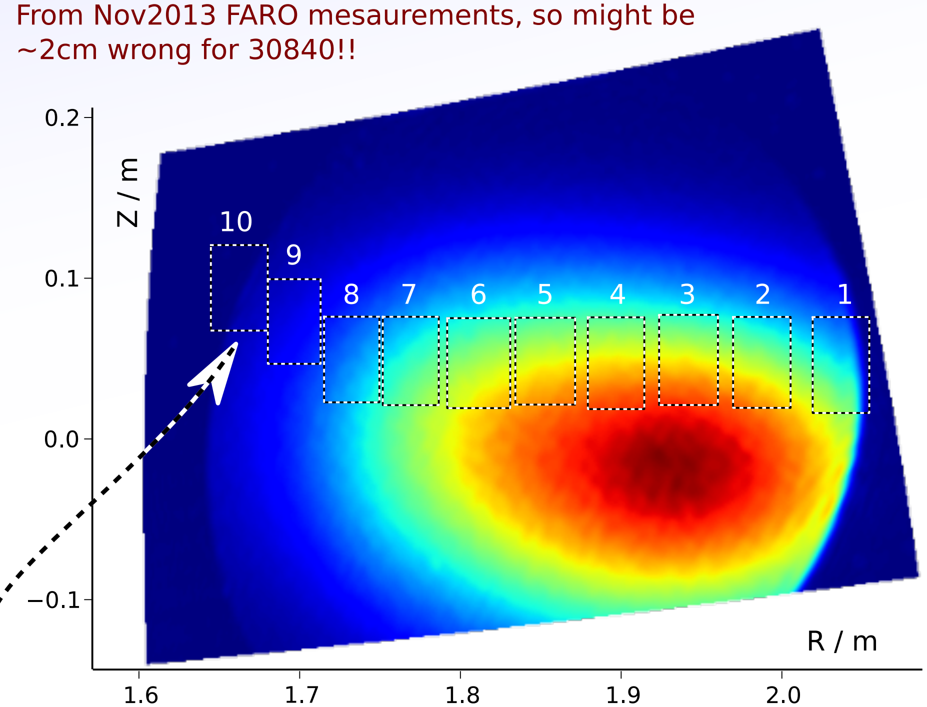
First, we can check the IMSE directly against the MSE polarisation angles, to make sure they see the same thing - since they are connected to the same optics.

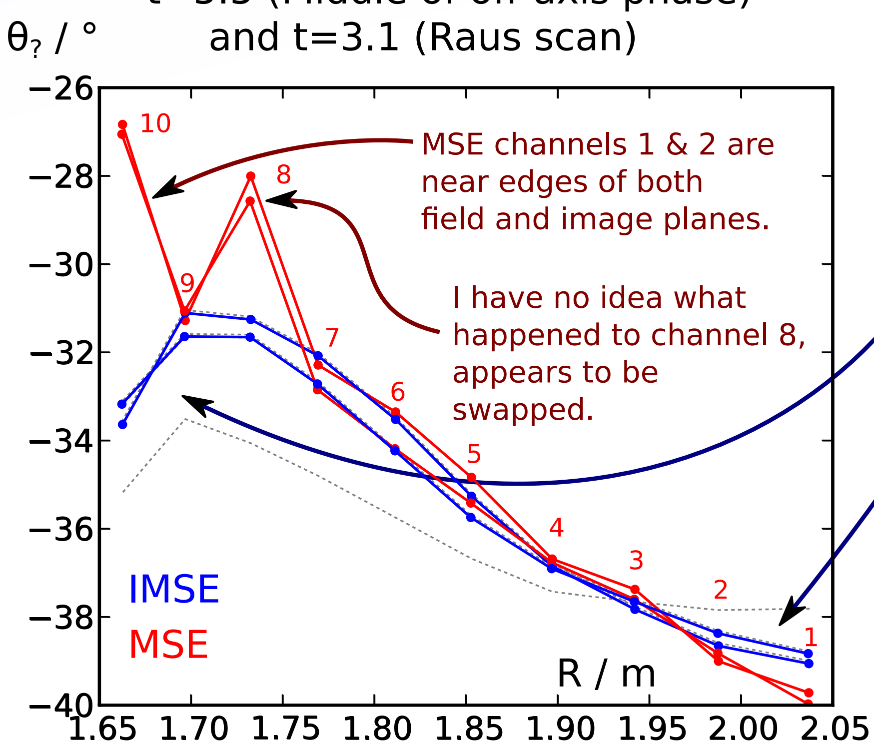
Using two 800kA NBCD pulses:
- IMSE:30823 , MSE:30840.
- Same shot program (~repeat).
- No significantly different mode activity.
- Same equilibrium results.
- Very similar polarimeter signals.

Instrinsic contrast calibration is not good at IMSE image edges, expect some offset!

MSE equivalent regions on IMSE transformed image.
From Nov2013 FARO mesaurements, so might be ~2cm wrong for 30840!!

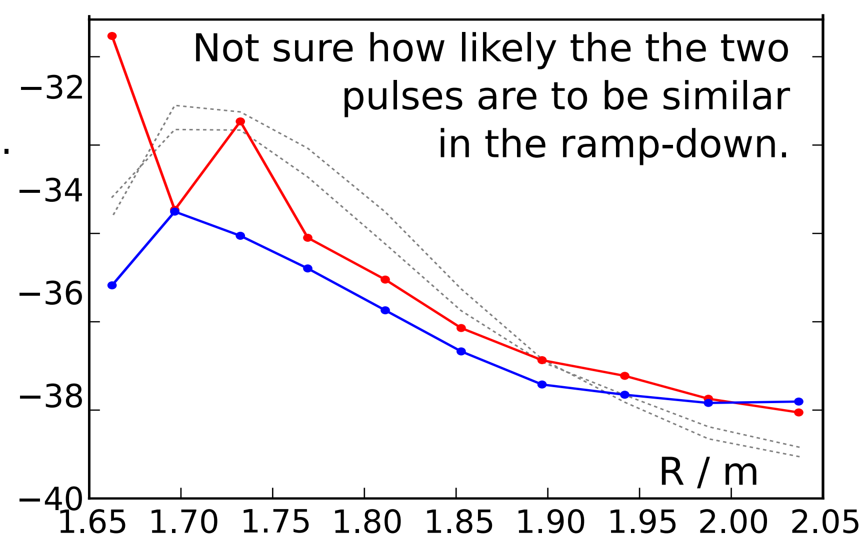



t=5.5 (Middle of off-axis phase)
and t=3.1 (Raus scan)




This was a bad idea.

t=9.6 (Middle of ramp-down)



# IMSE vs MSE

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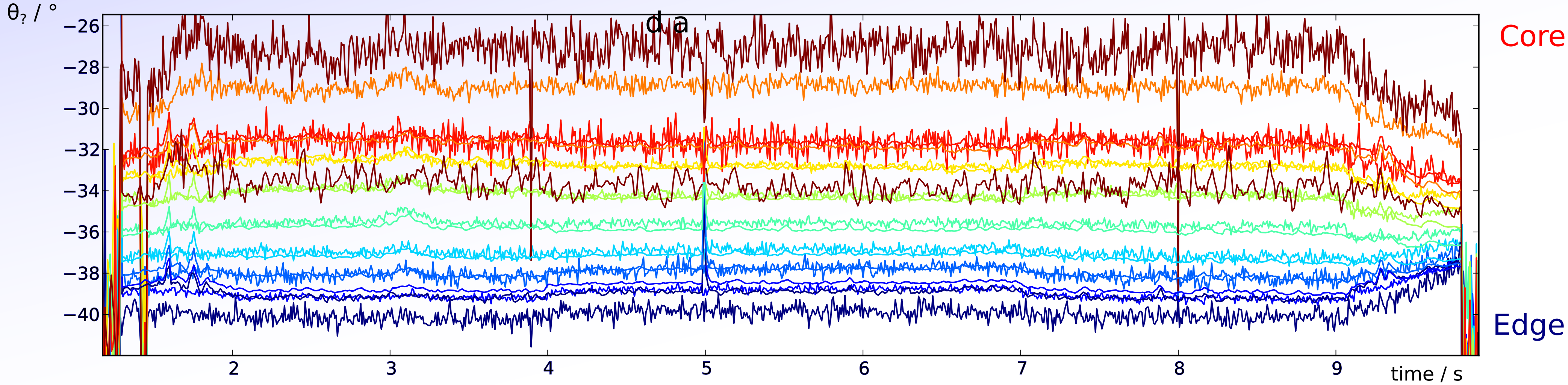
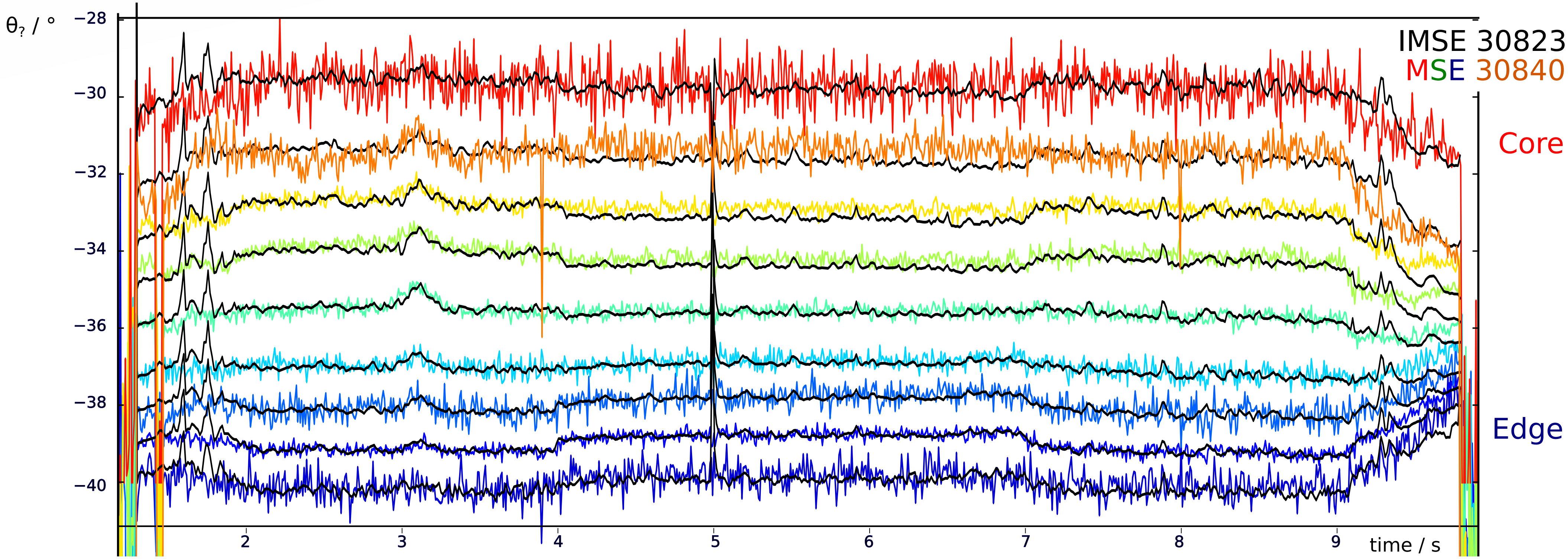
The raw time traces show a similar stroy - some offset and lots of noise on MSE at core/edge.




Adjusting the offsets independently to see the temporal behaviour shows very good agreement:



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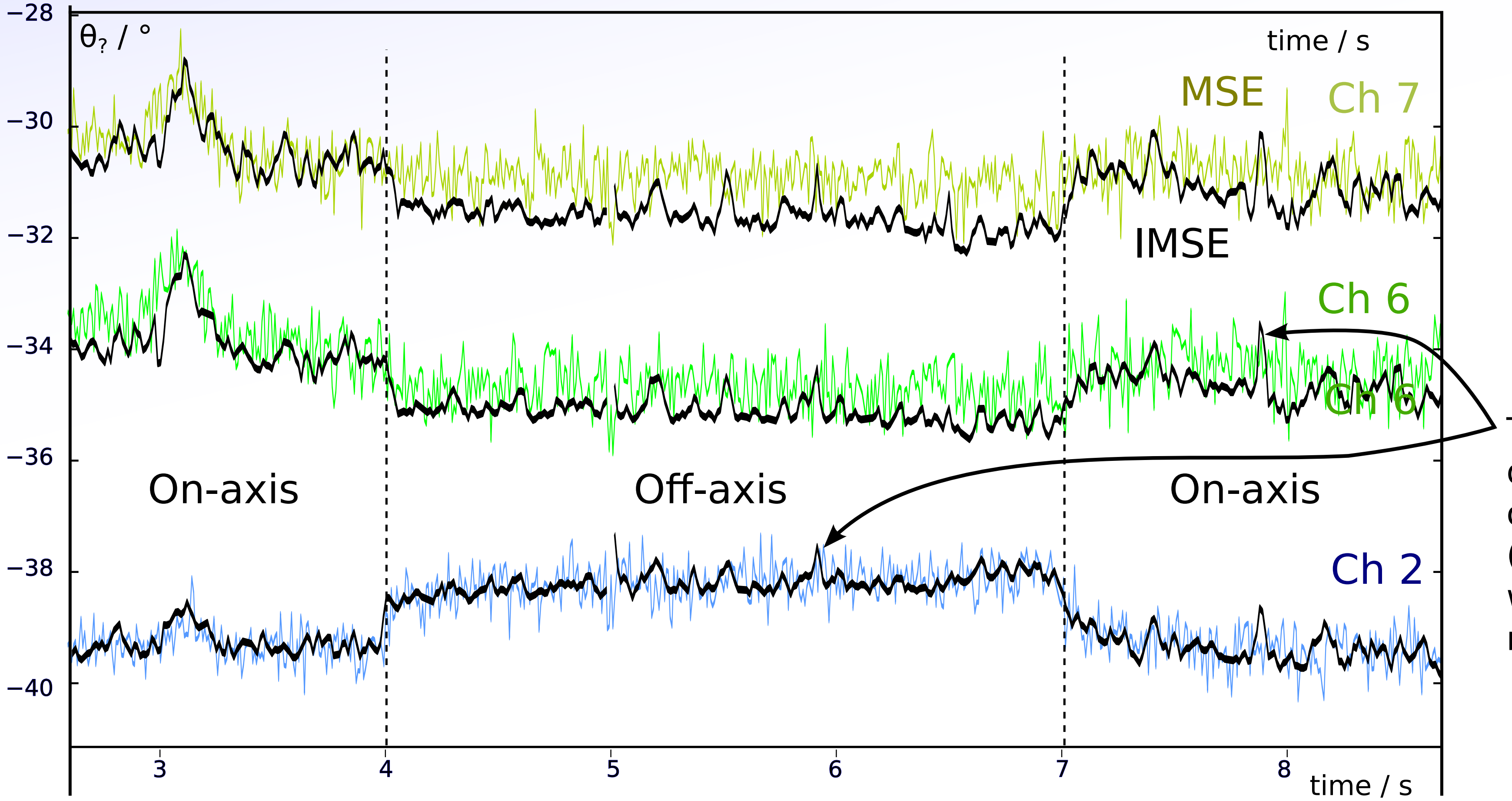
# Neutral Beam Current Drive

These are the off-axis NBCD shots, where we are looking to see if the IMSE can detect the current profile changing on the current diffusion timescale after the switch to off-axis NBI.

Firstly, the IMSE shows slightly more of a jump in the core as the switch is made:




These are probably due to vibrations of the IMSE camera (see later) which is solvable next time.

This gave me some concern that the IMSE is susceptible to background contamination.
Here, the background drops by ~20% during the off-axis period (probably changing charge exchange Hα 'Halo' or FIDA emission).

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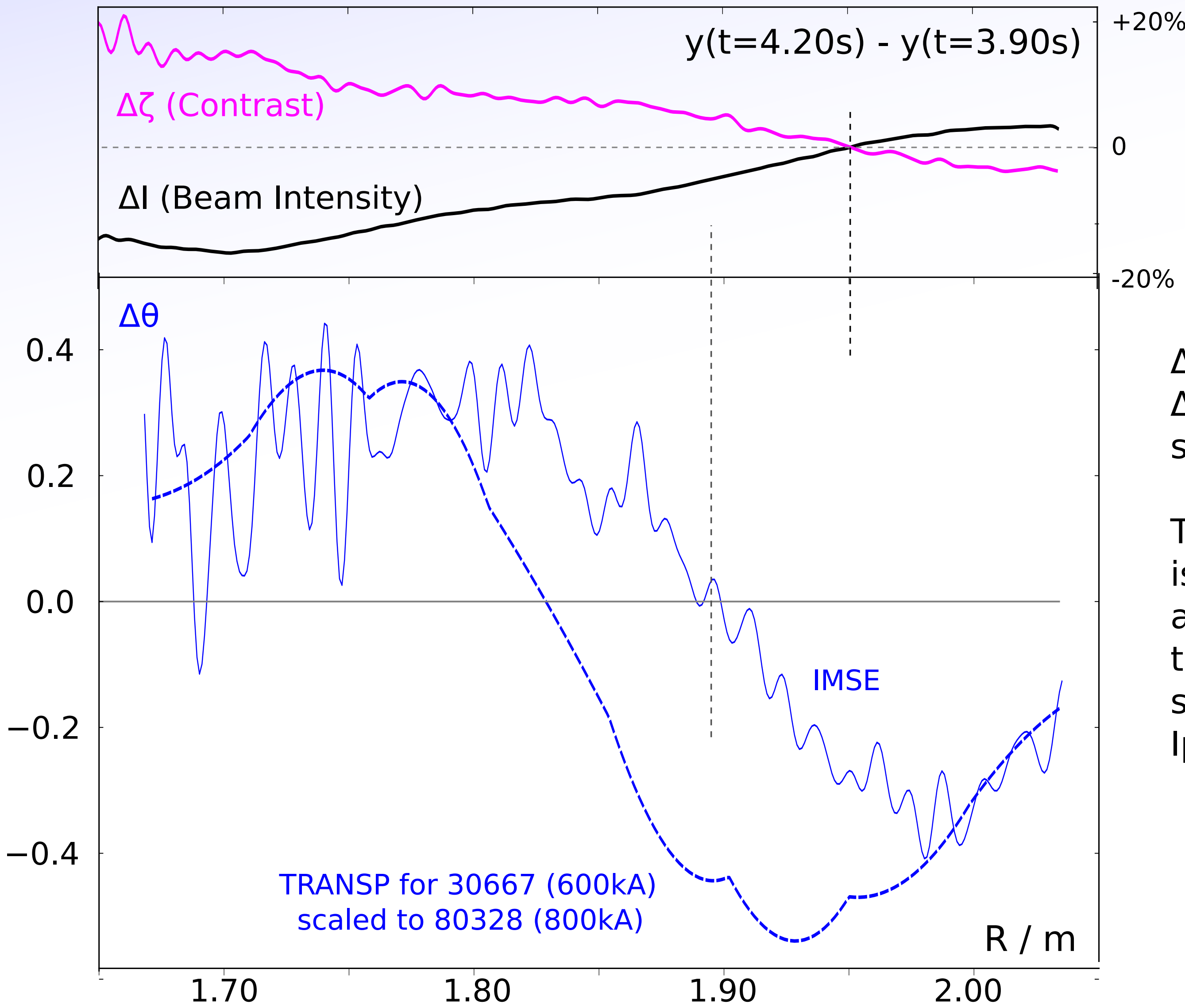
# Neutral Beam Current Drive

The change of the intensity I, contrast ζ and angle θ all have opposite sign at the core and edge.
We can check to see if the position of the inversion of the change is in the same.




ΔI and Δζ inversions at 1.95m
Δθ inversion at 1.90m
so, Δθ is *probably* real.

The prediction from TRANSP is the same magnitude but in a different position, although this TRANSP run was for a 600kA shot and has been scaled to the 800kA Ip here.

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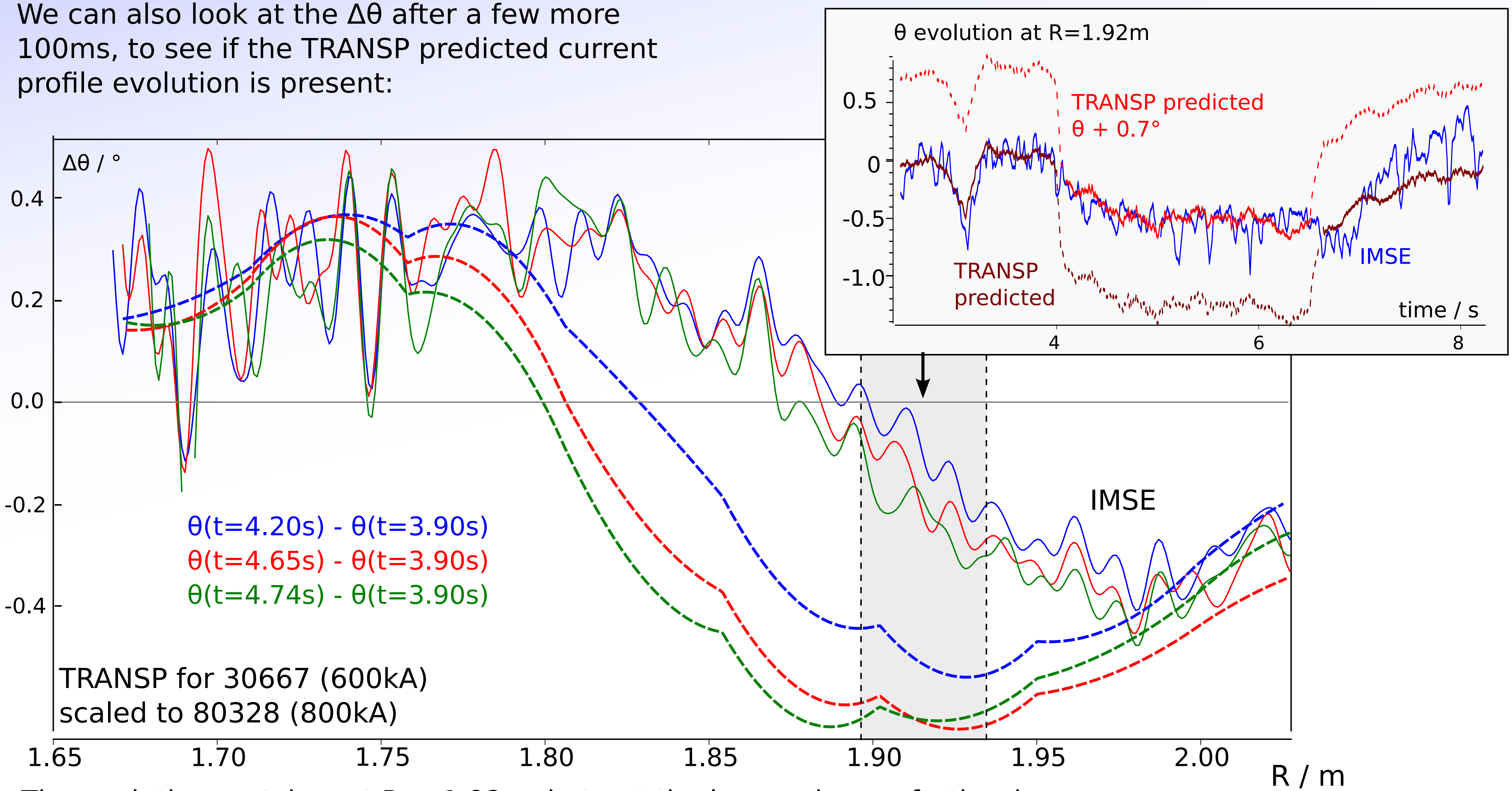
# Neutral Beam Current Drive

We can also look at the Δθ after a few more 100ms, to see if the TRANSP predicted current profile evolution is present:




The evolution matches at R ~ 1.92m, but not the larger change further in.
If we ignore the shift at that position, the evolution magnitude looks correct
--> Need a proper TRANSP run for this pulse.
--> Need to independently separate plasma movement and current evolution **in the measurements.**
**Please note:** Only time evolution of θ is diagnosable at this level!
0.1° is the best systematic error/calibration level you can really hope to acheive for any type of MSE.

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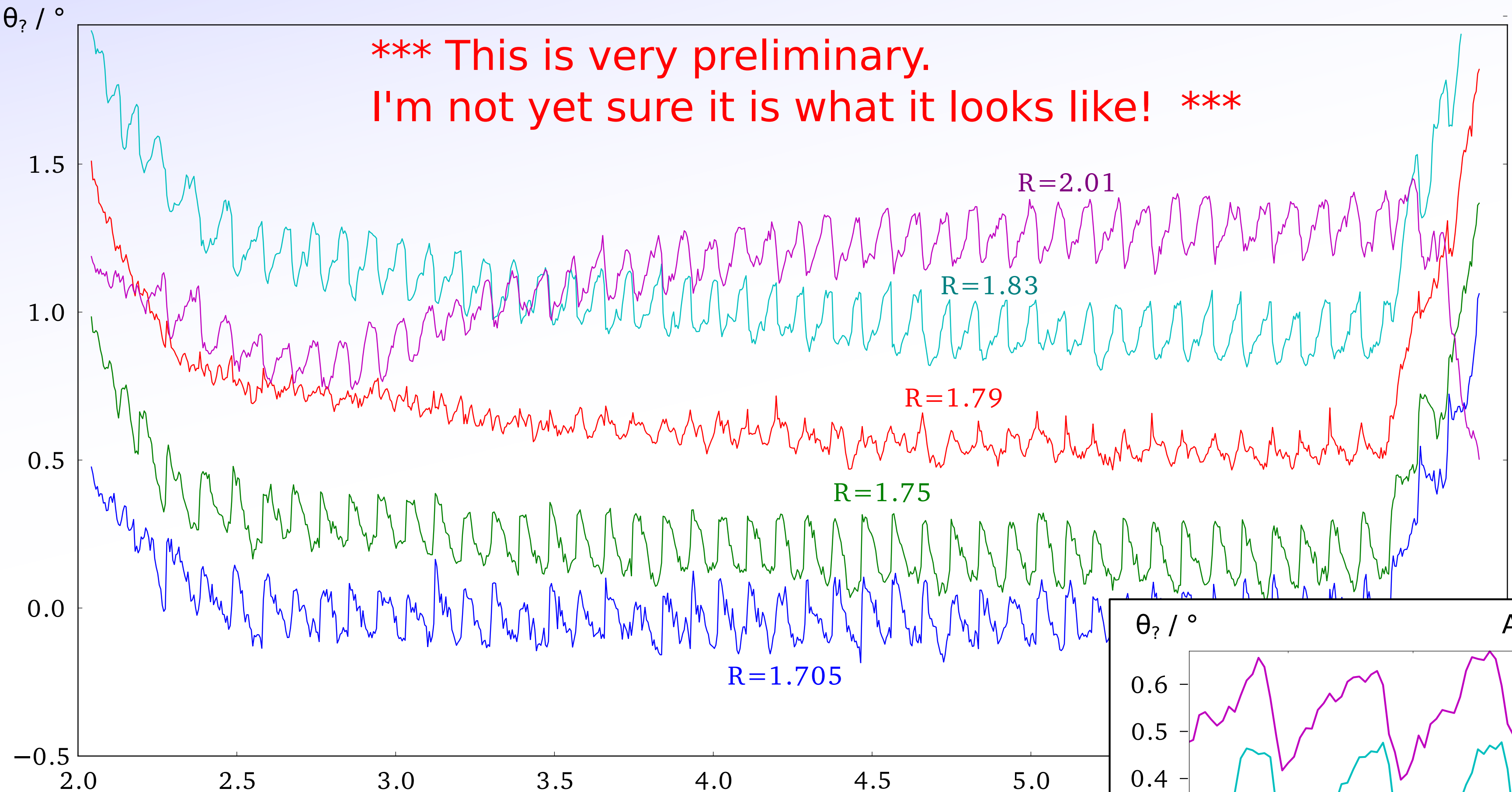
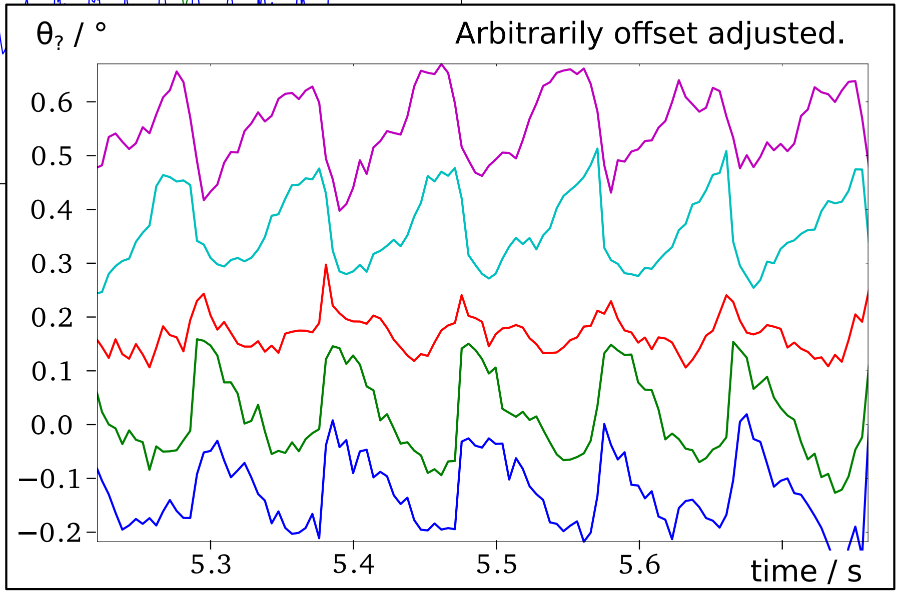
# Sawteeth

Tuesday also had some very nice discharges with large/slow sawteeth.
Some were missed, but the camera shielding came just in time for the last few.




Data looks very good and shows the sawtooth pattern very clearly throughout the shot.

- Evolution direction inverts at R~1.79m.
- Pattern is unexpectedly large near the edge - R ~2.01m ??

Still need to check for contamination by other variables, plasma position and Shafranov shift.

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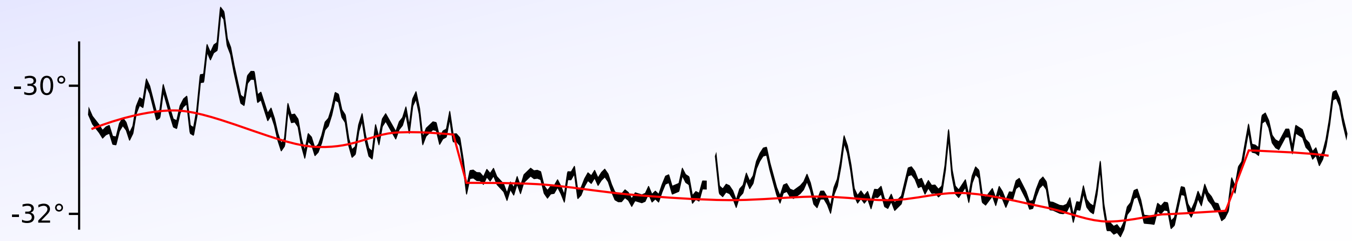
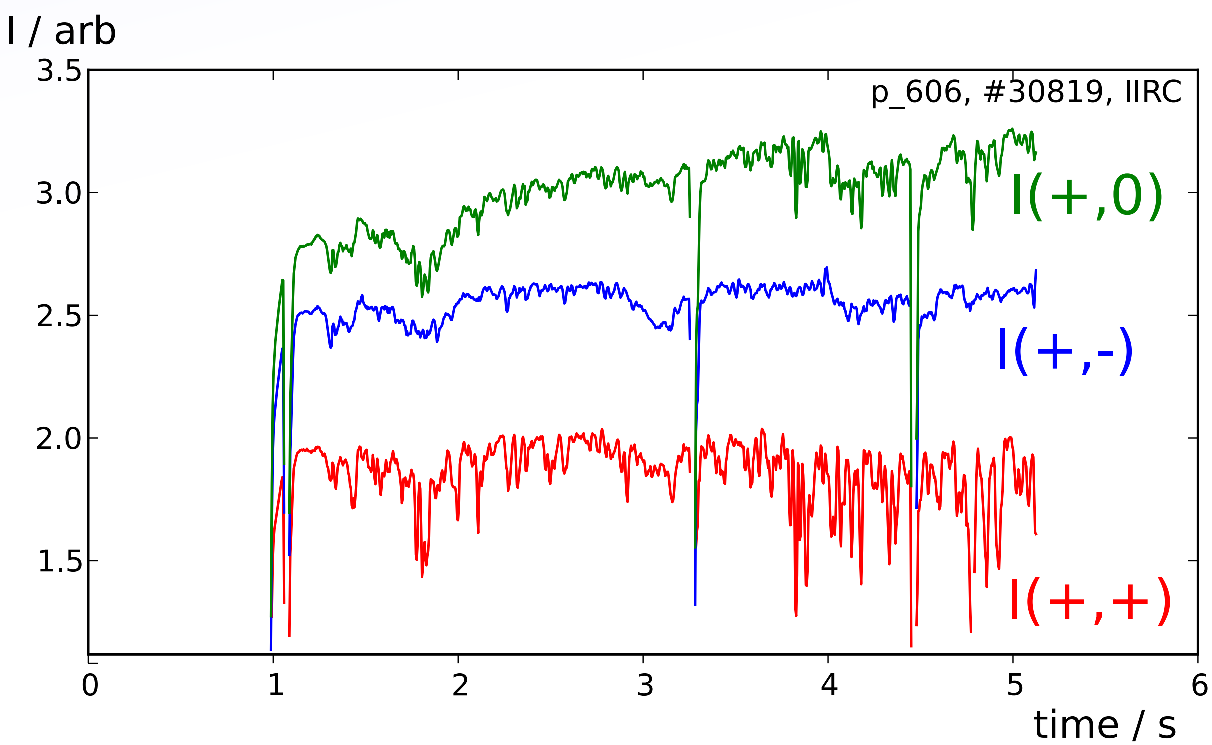
# Vibrations?

The NBCD dischrages shows perticularly bad large deviations, always in one direction:




The Zyla camera was mounted with a system I built myself to easily allow full rotational and positional adjustment in all 3 directions. It's perhaps not quite stable enough, especially with the iron magnetic field shield on.




Spikes are always -ve in the component amplitudes - i.e. negative spikes in component contrasts.

Movement generally reduces overall contrast due to bluring of the fringes, but can affect different components differently if blur is in one direction (likely, due to mount asymmetry).

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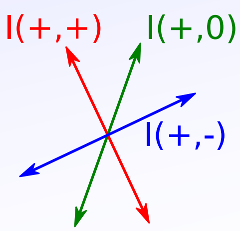
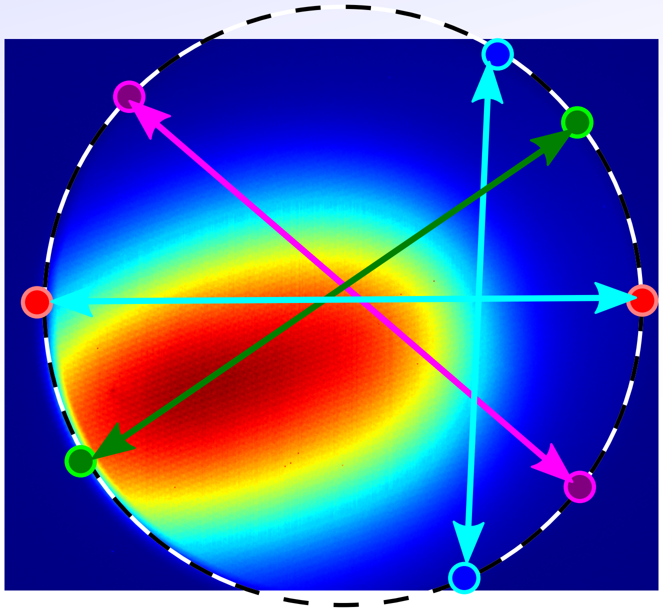
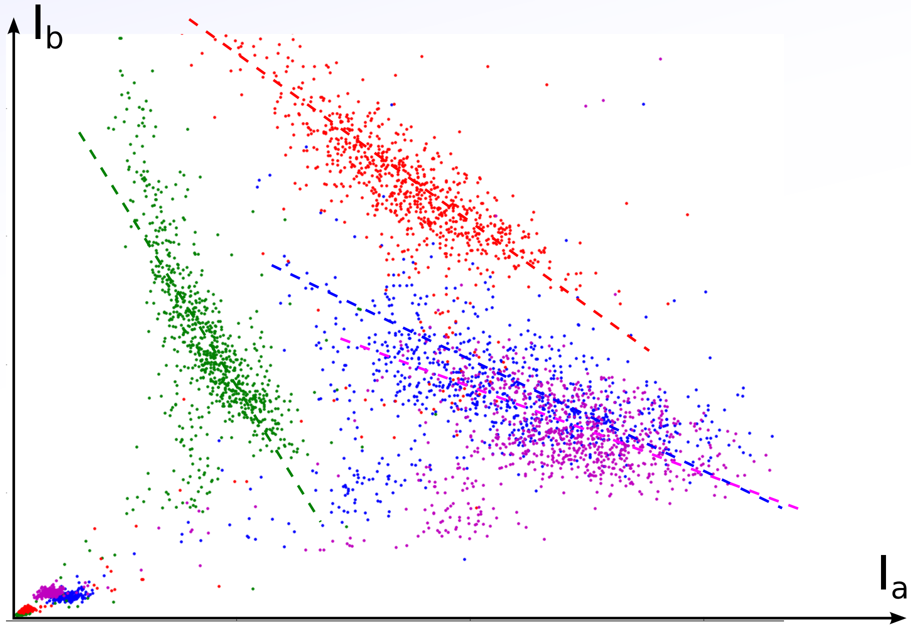
# Vibrations?

We can look at amplitude on the edge of the 'fixed' circular image limit (due to optics in tube) as a generally measurement of vibrations:





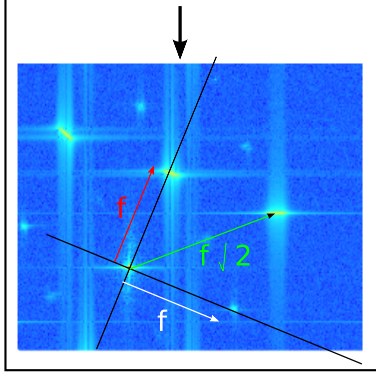

This seems to indicate left/right motion, but I(+,+) is worst and goes perp to that.
...Need to think about this a bit more.

NB: This shows the oscillation of camera and frame vs tube, not necessarily camera vs plates, which affects the contrasts.

The Rolling Shutter shots were much worse. This could just be integration time, but rolling could blur vertically too.
At 4.7ms exp, with ~512 lines, each row takes 4.7ms / 256 = 18μs to read out.

... Not exactly sure how that would work, need to go to the lab and hit it with things.

It also may effect (+,+) and (+,-) more as they are √2 higher frequency:



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# Calibration for Physics day augMay2016

The Savart was rotated on Wednesday, p_560 - 561, so everything in 562 - 617 has one calib.
The calib/info shots for this block are:

| | |
|---|---|
| 562 | "Cold cal" - Slow careful scan of calibration polariser with white light source. |
| 563-565 | "Filter info" - Ne lamp through the 3 filters to show filter AOI information. |
| 575 | Warm cal SCAN, rolling shutter, messy beams |
| 576 | Warm cal FIXED, Motor didn't run so only get 1 point |
| 577 | Warm cal, beam blips only |
| 582 | Good |
| ----- | --- v --- Start of the actual physics day. Boronisation inbetween but diagnostic untouched --- v --- |
| 611 | Motor skipped. Very messy, vibrations? rolling shutter? |

So the primary calibration is 582, with 575 and maybe 611 as checks.

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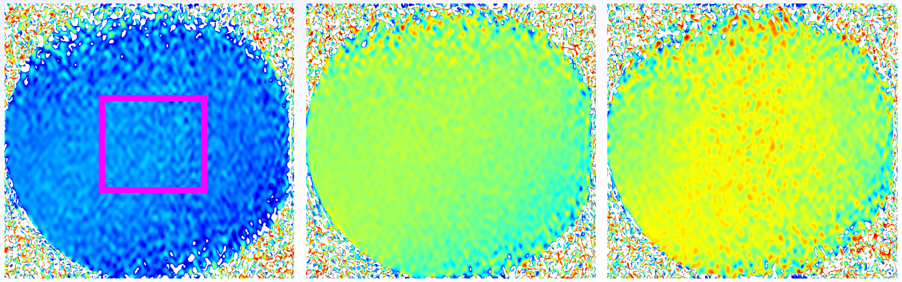
# Calibration for Physics day - p_575

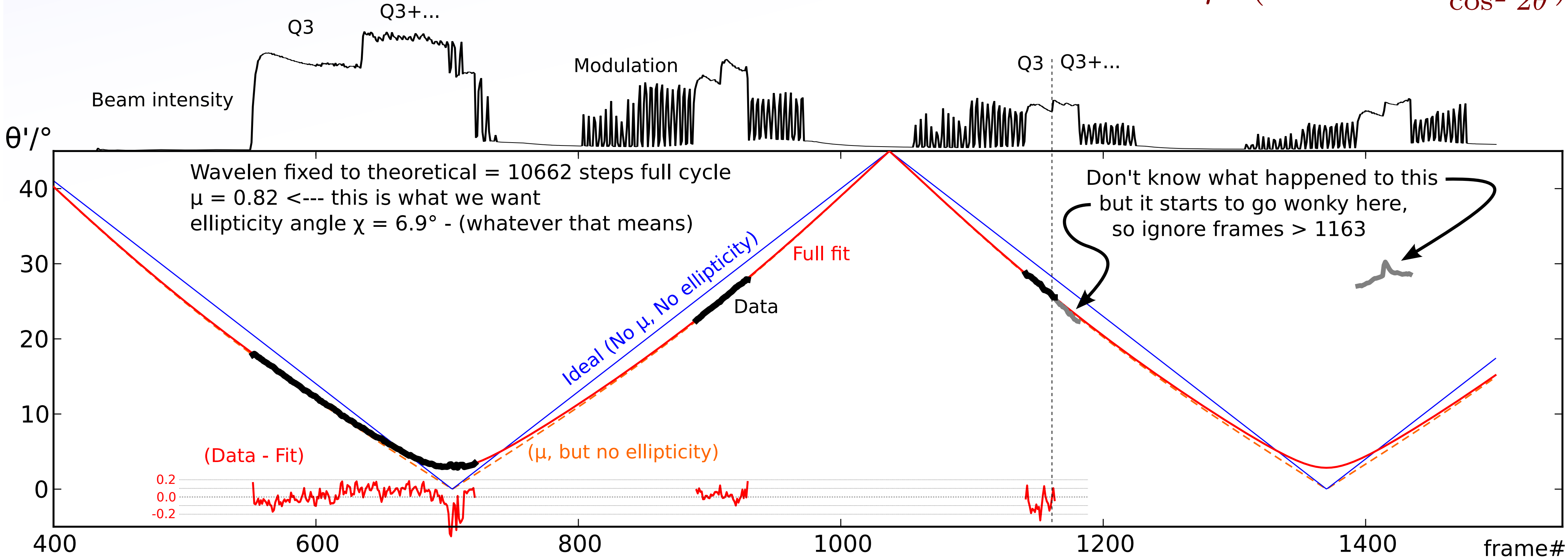
575 Warm cal SCAN, rolling shutter, messy beams
** 50kV beams **

Beams are going on and off, modulated, plasma is changing and some are multi-beam. Otherwise, looks good.



For any point/region on the image, we can fit the calibration scan. The wavelength is fixed to the 'known' 10662 steps for full cycle from the cold calibrations. The start step, μ and some arbitary ellipticity are free parameters. For the central region:

$$\tan^2 2\theta' = -\mu^2(\tan^2 2\theta + \frac{\tan^2\chi}{\cos^2 2\theta})$$




The appearance of the other beams does seem to induce changes in μ that give locally up to δθ ~0.1°, but it doesn't seem to effect the fit here at all.

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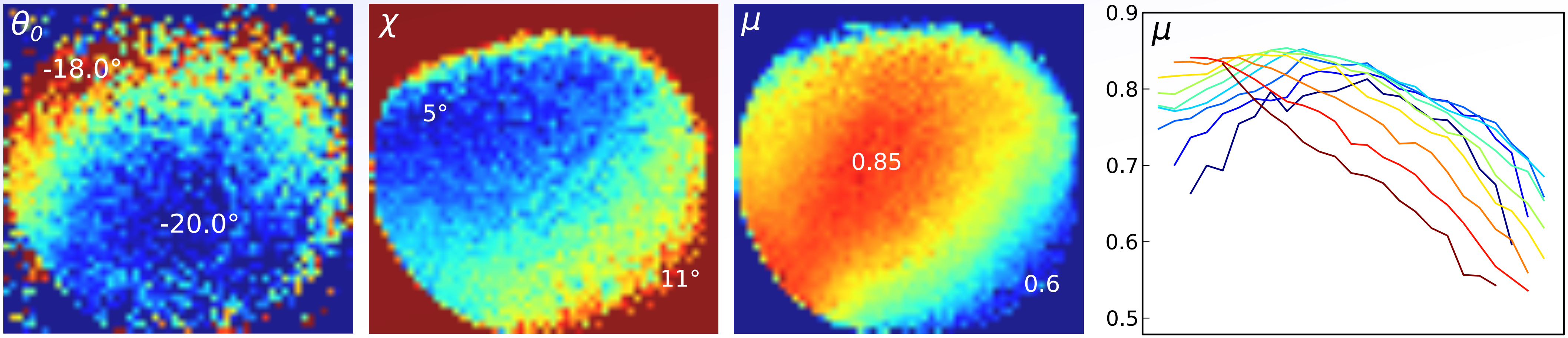
# Calibration for Physics day - p_575

575 Warm cal SCAN, rolling shutter, messy beams
** 50kV beams **

$$\tan^2 2\theta' = -\mu^2 (\tan^2 2\theta + \frac{\tan^2 \chi}{\cos^2 2\theta})$$

Fitting every 10x10 blocks, gives:




μ is the calibration we want - it should fix the linearity.
It's effect on the measured θ is big and is worst at 22.5°:
μ = 0.8 --> (θ'-θ) = 3.0°, μ = 0.6 --> (θ'-θ) = 7.0°.
Once corrected, the effect of getting it wrong is quite bad:
δμ = 0.01 --> δθ(22.5°) = **0.2°** but most of will go in with the offset calibration.
The actual nonlinearity is:
δμ = 0.01 --> θ'(35) - θ'(22.5) - (35 - 22.5) < **0.05°**

$\theta_0$ is arbitary but should be flat. It could be due to the bendy film polariser, but if it is in the measurement, it will go with all the other offsets.

I've no idea where χ comes from. It's smaller in the cold calibration so isn't the polariser. If it's present in the measurement at this level, it gives a worst non-linearity at the bottom of the working range (θ~15°):
χ~8° --> δθ = 0.4°, and worst non linearity: θ'(35) - θ'(15) - (35 - 15) = **0.25°**. which is quite bad :(
Fortunately, it looks ok in the beam centre. Generally, the bottom right of the picture is to be avoided.

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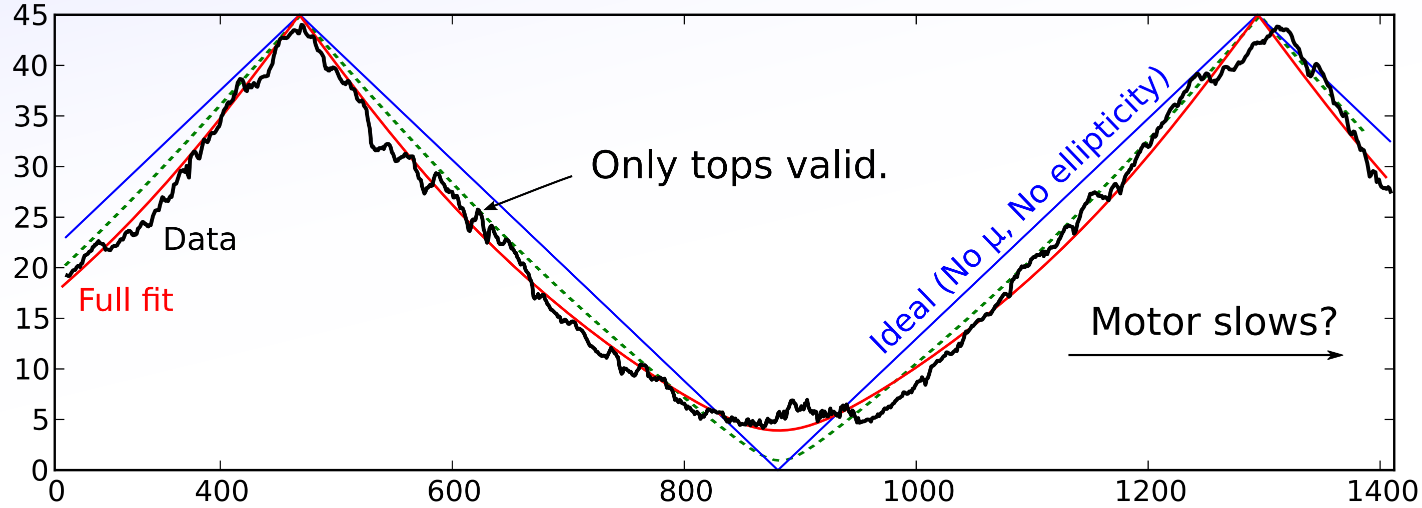
# Calibration for Physics day - p_611

611 Motor skipped. Very messy, vibrations? rolling shutter?

$$\tan^2 2\theta' = -\mu^2(\tan^2 2\theta + \frac{\tan^2 \chi}{\cos^2 2\theta})$$

This calib is very messy. Vibrations are dropping the constrasts and the motor didn't run smoothly. However, it's the only one on the day, so it's processed here only as a check that it looks roughly the same as the others.

Unfortunately, no, μ here is a lot lower.




But it looks as if that's explanable purely becamse the deformations will always be downwards, e.g. this is the μ=0.85 (dotted green).
Tried only fitting tops, but it's too ambiguous.

Can't confirm μ magnitude with this, but the fit image maps look the same shape.

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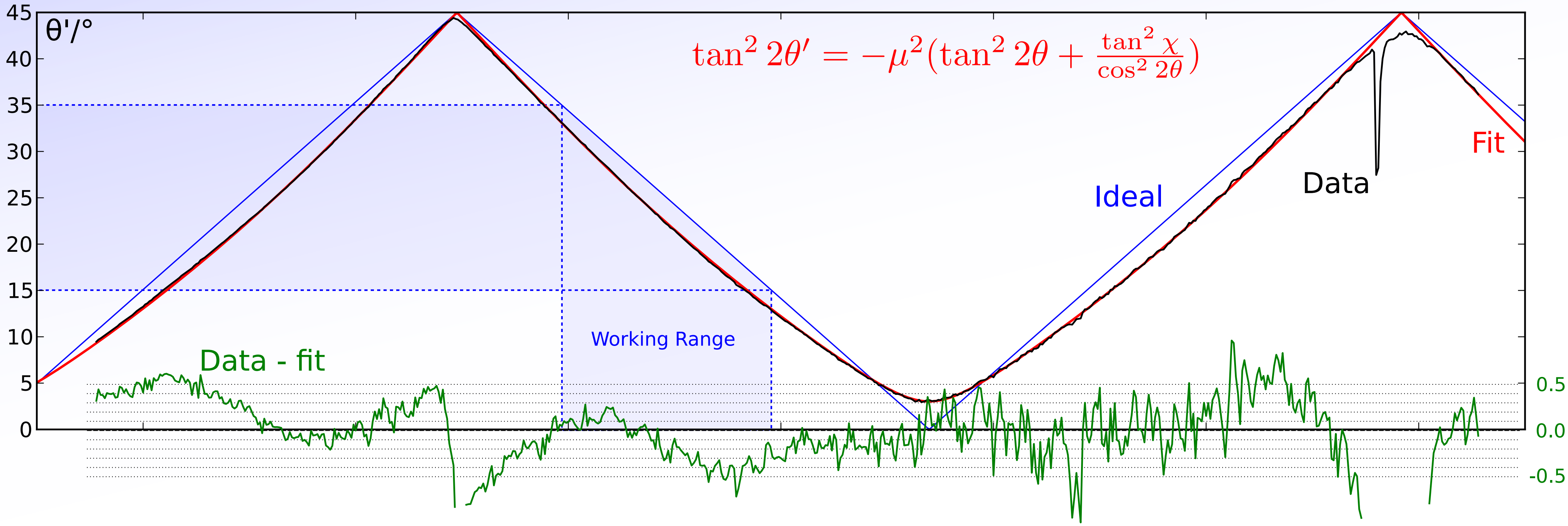
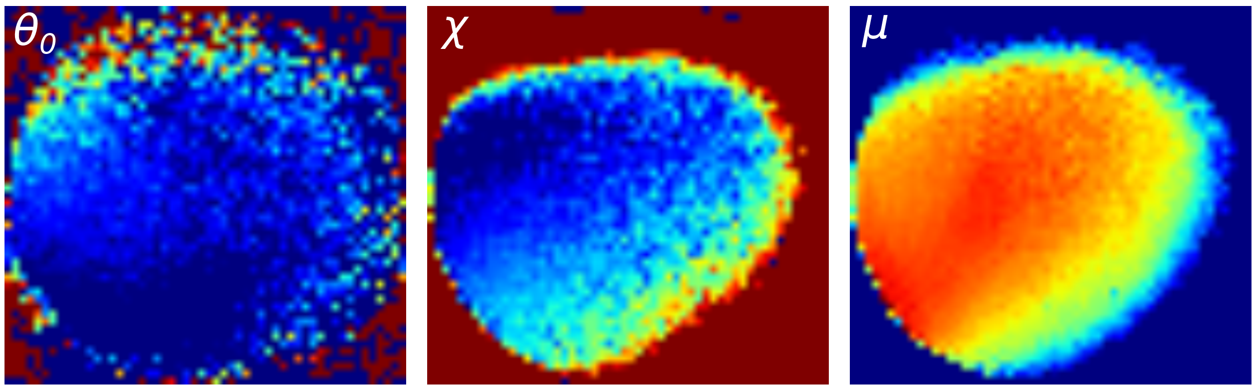
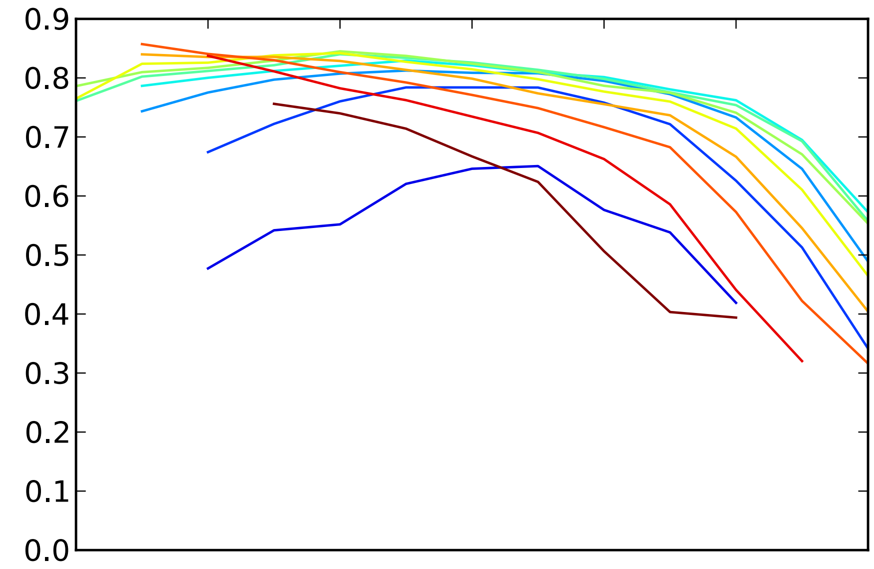
# Calibration for Physics day - p_5821

This one is much better:




From fit, absolute worst is 0.4° of non-linearity over 12° of $\theta_a$. This is a very wide fit though, so it could easily just be caused by the film polariser. Maps look like:



Mostly the same as p_575, with 0.2 difference in μ at the edge. This isn't great, so we definitely need a better palte, but it'll do for now.

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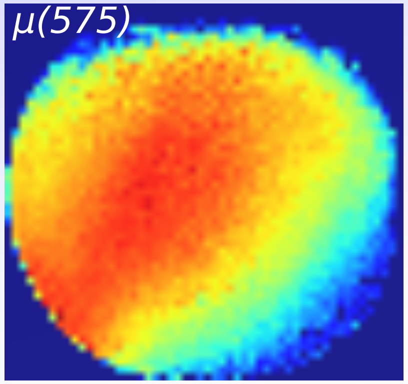
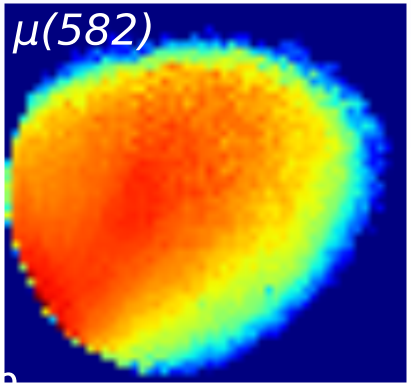
# Calibration for Physics day

The two calibrations were done on pulses with very different programs, yet return roughly the same μ.




p_575 = #30796 "L-H transitions with impurities"
Bt = -2.5 T
Ip = 800 kA
ne = 6.4
Pnbi = 3.6 MW
V = 50kV

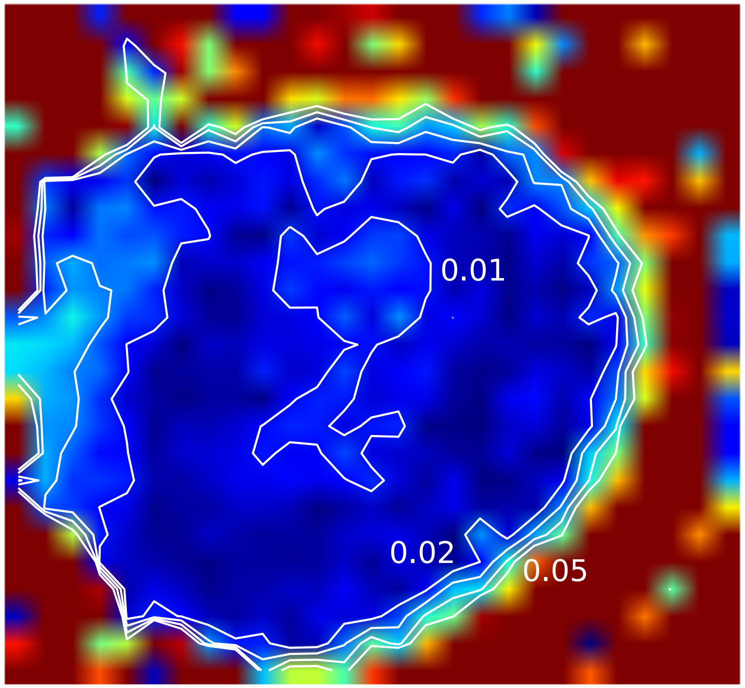



p_582 = #30796 "N seeding at low q95"
Bt = -1.8 T
Ip = 1 MA
ne = 9.6
Pnbi = 7.5 MW
V = 58kV

Beam voltage and |B| are different so spectrum and total contrast is very different.
That this doesn't seem to affect μ much is very reassuring!

We do need to stay away from the bottom right corner though.

|μ(582) - μ(575)|




Remember $\delta\mu = 0.01 \rightarrow \delta\theta_{nl} = 0.05$

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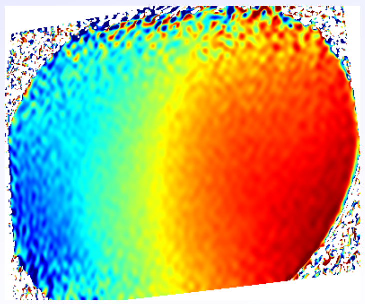
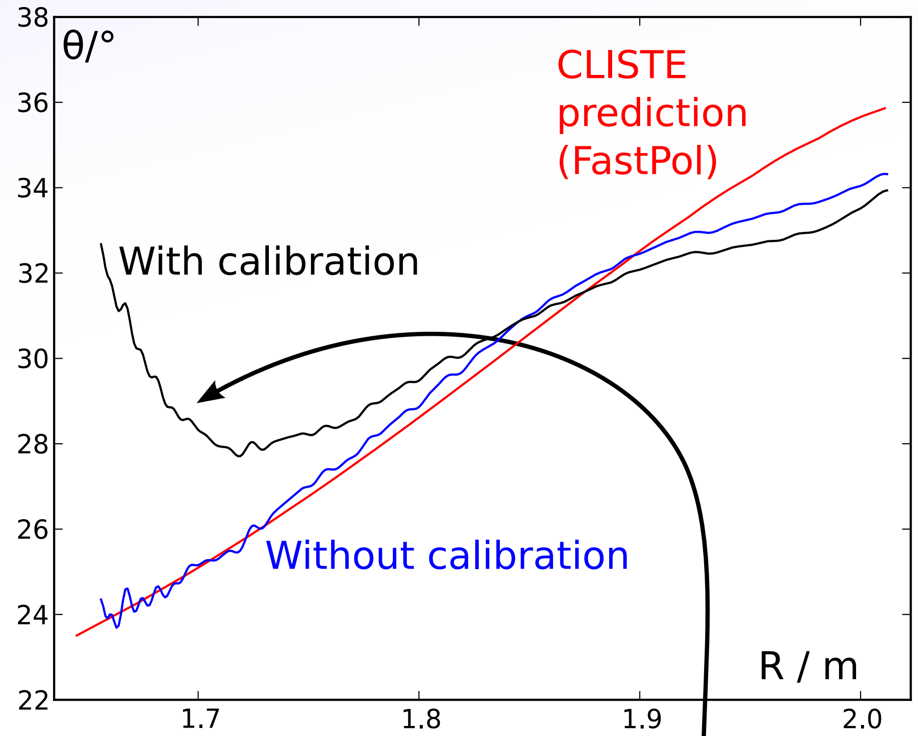
# μ Calibration, wait.... what?

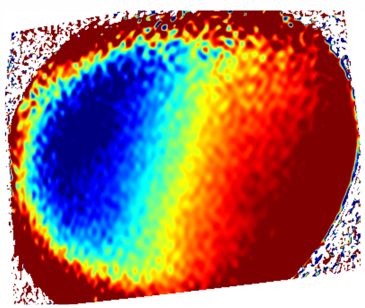
That's all very nice, but doesn't seem to work at all.

The uncalibrated angle for a nice shot with long stable current profile (p_572), looks like this:

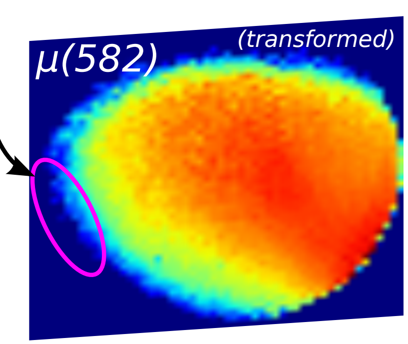





Including the μ from p_582,
makes it do this:



The data from the real plasma measurements looks really good, and doesn't seem to have a large μ variation. The slow linear sweep could be countered by something else (probably geometry), so could be correct, but the swing up at the edge is definietly wrong.




What affects the calibration but not the measurement???

This whole μ calibration exercuse has worked in the past.
The direct linearity effect is too small to directly see vs the inaccurate CLISTE prediction in the core, even when μ<0.5 from the calibration. :(

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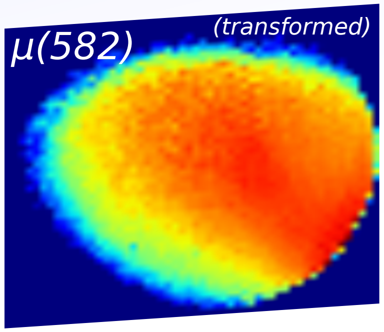
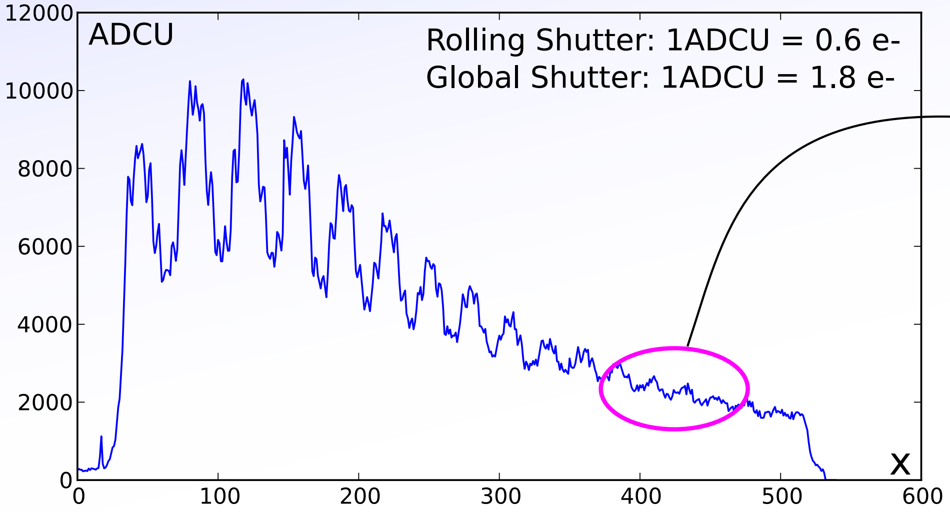
# μ Calibration, noise Effect.

Part of the story might be the noise...

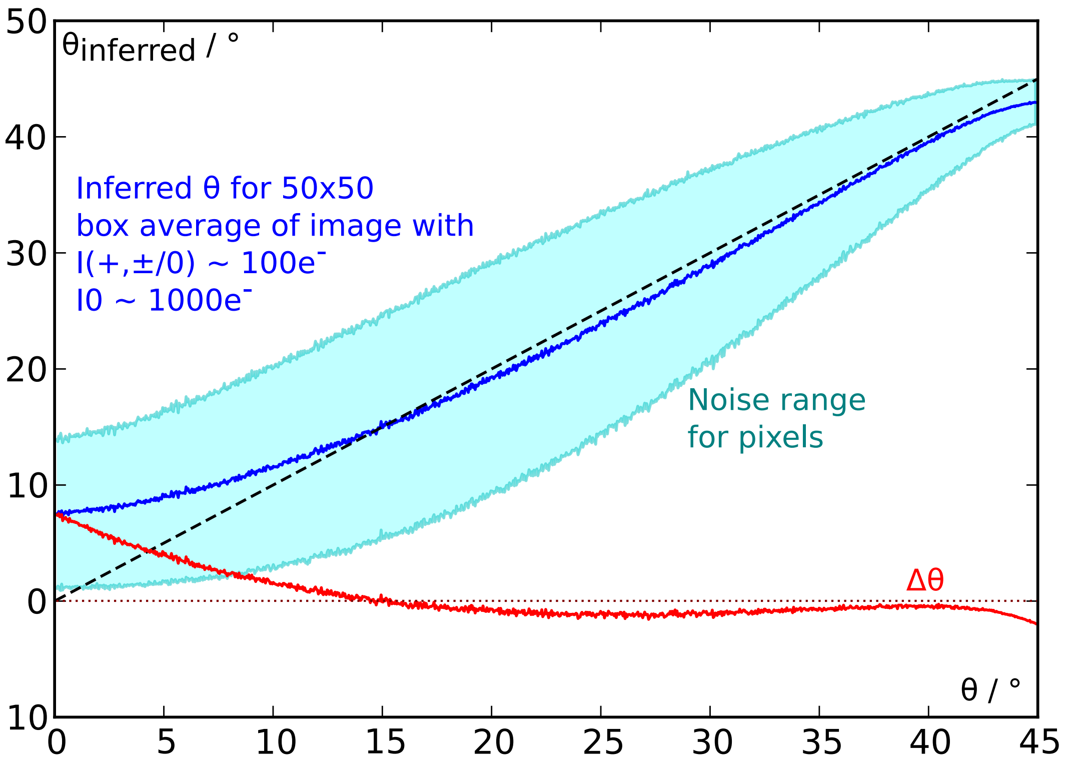
Due to the anti-optimistion of spectrum with the polariser, the contrast (Σ I(+,±/0)) is very weak, especially in that corner.




Noise is ~ √N / N for photoelectrons

Levels are
Osc = 200 counts = 100 e-
Background = 2000adu = 1000 e-




That explains it fairly nicely. It will also effect the measurement for low signal levels (which is common in the core)

So....

1) IMSEProc needs to handle absolute signal levels, count/ADCU conversion and propagated errors all the way through.

2) Need to do a proper full Bayesian fit for the calibration.

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# μ Calibration, spectrum Effect.

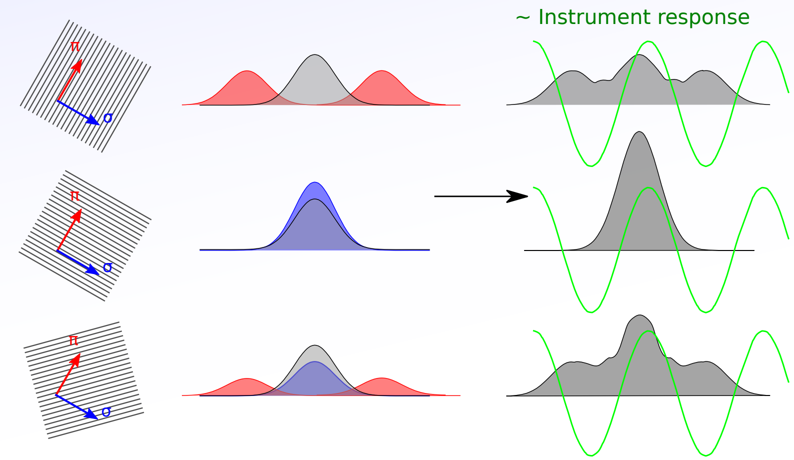
The anti-optimistion has another effect.

The whole problem of the misbalance was that the component constrasts are the FT of the spectrum onto their spatial variable (x ± y):

$$\zeta_a = \Gamma(\beta x + \gamma)$$

$$\zeta_{b/c} = \Gamma(\beta x + \gamma \pm \alpha y)$$

$$\Gamma(x') = \int I(\omega') \exp\left[-i\omega' x'\right] d\omega'$$




'I' includes sign changes due to the delay plate and also due to the flipping polarisation.

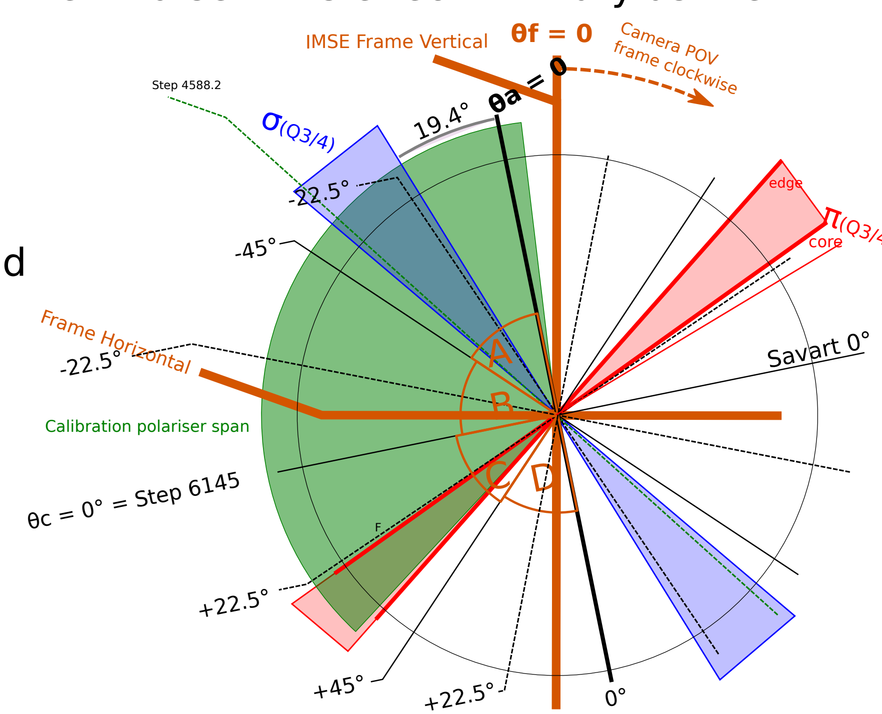
For the normal operation, it is probably quite smooth but with the anti-optimised calibration it could have a strong variation with x' - which is different for (+,+), (+,-). The balanced forumale doesn't entirely remove the problem - only to first order, which might now not be enough. Even worse - this effect will vary as the polariser rotates.

Hmm... however, near sigma aligned (~22.5° on the way up), the anti-optimisation goes away, so everything should be OK.

Well, maybe not, because the other beam enery components could come into play.

grumble :(

Options:
1) Hope the problem goes away with a better Savart plate.
2) Try to make a cold-cal with similar light delivery to the actual.
3) Find some way to get rid of the anti-optimsiation problem



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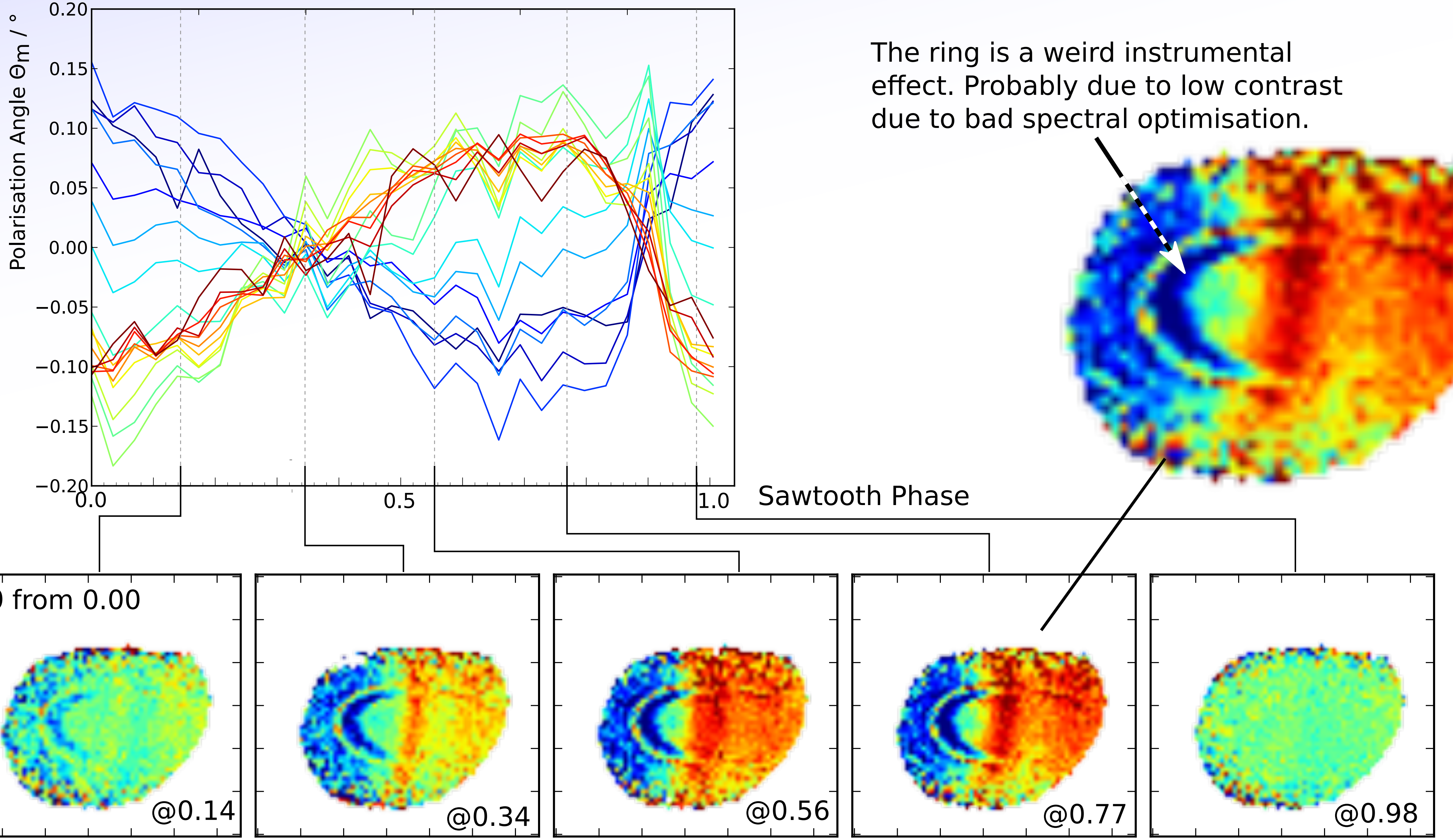
# Sawtooth Average Delta Images

Establish approx sawtooth phase from polarisation angle at edge, where the signal is clear.
Average other images in ~30 blocks of phase with respect to that.



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# Effect of plate imperfections on Δφ

Phase is:

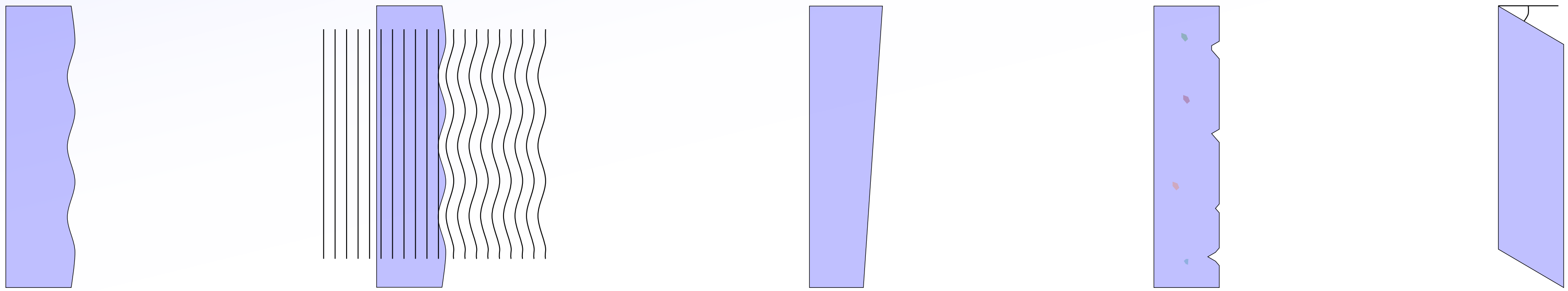
$$\Delta\phi = \frac{2\pi L}{\lambda_0}\left[\frac{(n_o-n_e)}{2} + \frac{(n_o^2-n_e^2)}{(n_o^2+n_e^2)}\cos\delta\sin\alpha\right]$$

$$\frac{\delta\phi}{\Delta\phi} = \frac{\delta L}{L}\,\frac{2\pi\Delta N}{2\lambda_0}$$

Without the 2 for delay plates.

Plate quality is given as:

Surface Flatness | Wavefront Distortion | Wedge/Parallelism | Scratch/Dig | Perpendicularity?

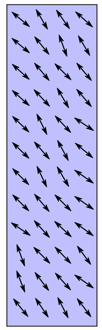


Most of these are given as a Δλ/λ e.g. "λ/4 at xxx nm."

CLaser: 20 arc secs
20/3600 = 0.006°
over 35mm, δL = 3.4μm
δφ ~ 220°!!! @(αBBO, λ=653nm)

For Δφ < 30°, require δL < 0.5μm.
Same is true for surface flatness.

Angular variation



Generally: Large slow changes over plate surface give changes with light cone, so are a big problem.
Fast changes, even if large, reduce contrast but in a which which is easily calibrated.