By maths.

To include axisymmetry, define poloidal magnetic flux as:

$$\psi(R,Z) = \int_0^R R' B_z(R',Z)\, dR'$$

And the toroidal current is:

$$-\mu_0 j_\phi = \frac{\partial}{\partial R}\frac{1}{R}\frac{\partial \psi}{\partial R} + \frac{1}{R}\frac{\partial^2 \psi}{\partial Z^2}$$

Going back to terms of Bz:

$$-\mu_0 j_\phi = \frac{\partial B_z}{\partial R} + \frac{1}{R}\frac{\partial^2}{\partial Z^2}\int_0^R R' B_z(R',Z)\, dR'$$

We only see where the MSE emission is, so we can only integrate from some *R = R0:*

$$-\mu_0 j_\phi = \frac{\partial B_z}{\partial R} + \frac{1}{R}\frac{\partial^2 \psi(R_0,Z)}{\partial Z^2} + \frac{1}{R}\frac{\partial^2}{\partial Z^2}\int_{R_0}^R R' B_z(R',Z) dR'$$

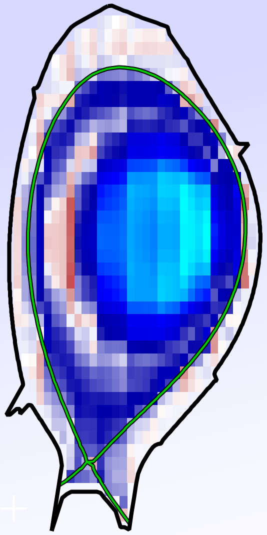
This we have with 1D MSE.

Function of Z that we cannot know.

The new term gives localisation of current in Z (~via curvature of field).

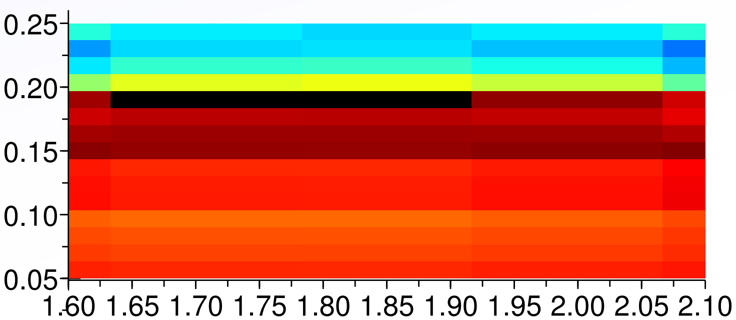
But.... Integral of a second difference of measurement... will be VERY noisy.

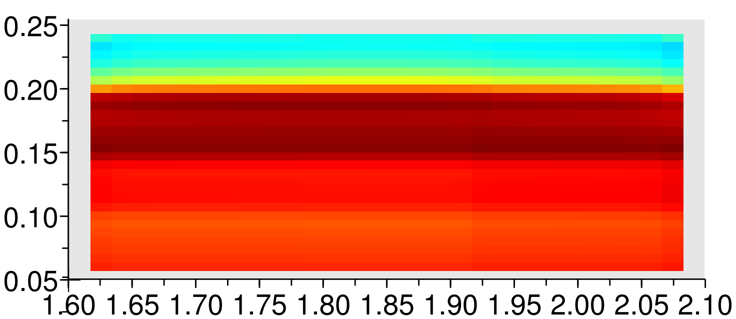
So can we directly calculate *jphi*?

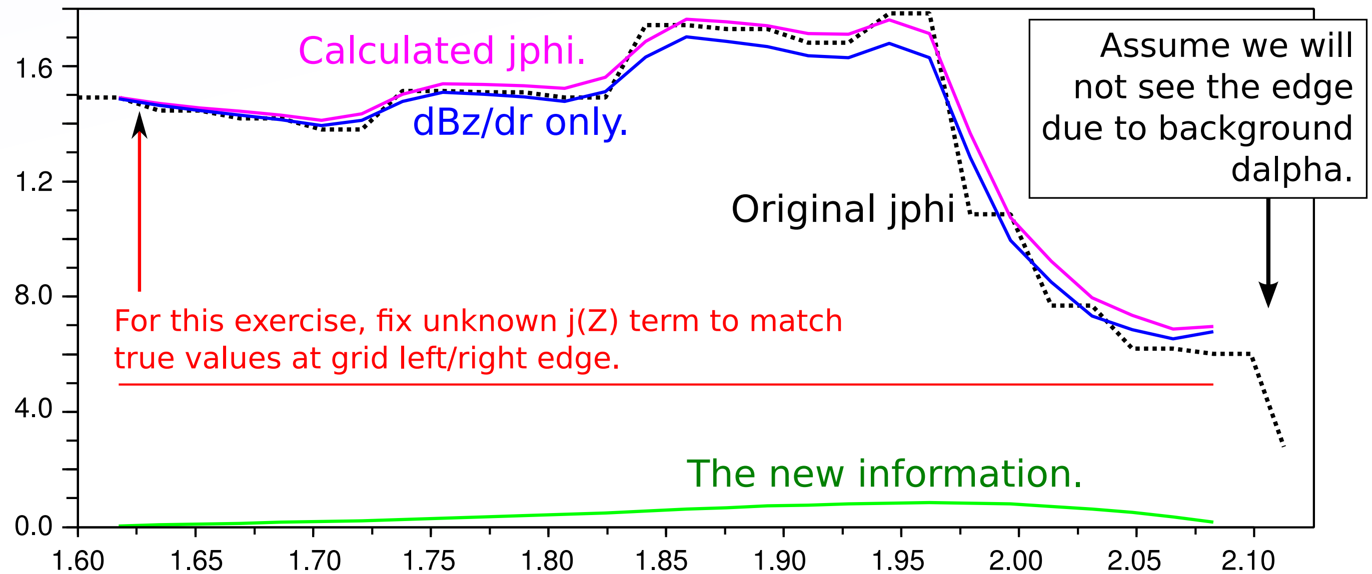


- Take CLISTE current distribution
- Predict 30x30 grid of Bz.
- Try to directly calculate j_phi

$$-\mu_0 j_\phi = \frac{\partial B_z}{\partial R} + \frac{1}{R}\frac{\partial^2 \psi(R_0,Z)}{\partial Z^2} + \frac{1}{R}\frac{\partial^2}{\partial Z^2}\int_{R_0}^{R} R' B_z(R',Z)dR'$$





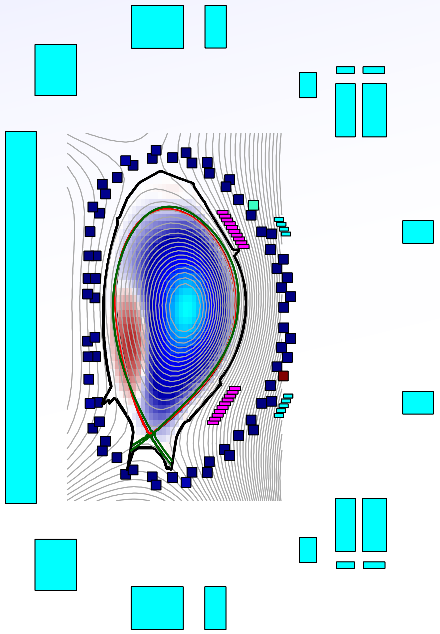


Unknown part above dBz/dR (standard MSE) is < 10% anyway. We do gain it mathmatically but as anticipated, it is entirely lost even with only 1mT noise in Bz (0.02^o pitch angle).

Conclusion: No. You still cannot exactly calculate jphi directly.

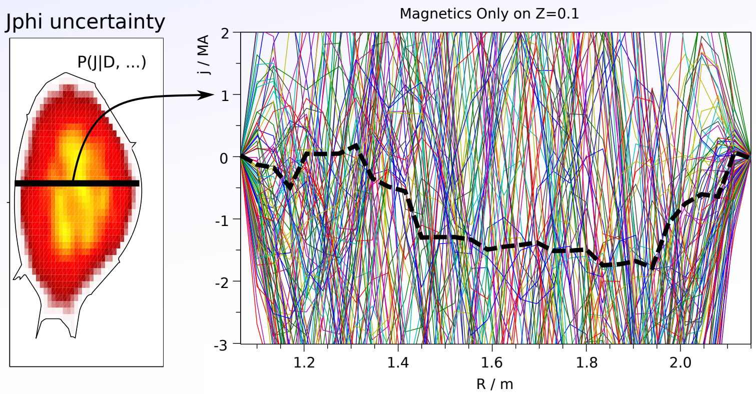
However, we do have measurements of the dBz/dR part at different Zs, and we know that this is most of jphi variation. Together with integral measurements (field pickups and flux loops), it is now part of a complex tomorgraphy problem that we have done before.

By current tomography...



Put description of AUG coils and some pickups into Minerva so we can now do Current Tomorgraphy and Bayesian Equilibrium for AUG.

For magnetics only, we have the usual tomography situation:



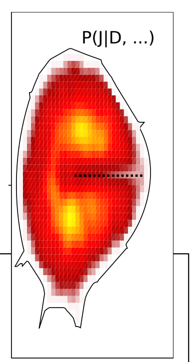
(Almost) no prior/regularisation

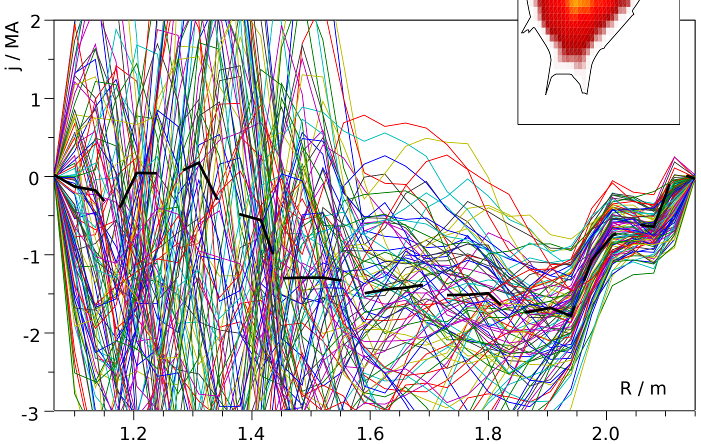
↓

(Almost) infinite uncertainty
(but B/psi still good)

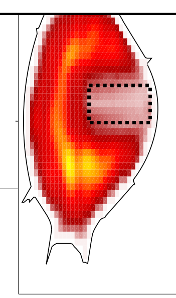
Each case has 900 measurements at sigma = 10mT.
So difference is only in the ***type*** of information.

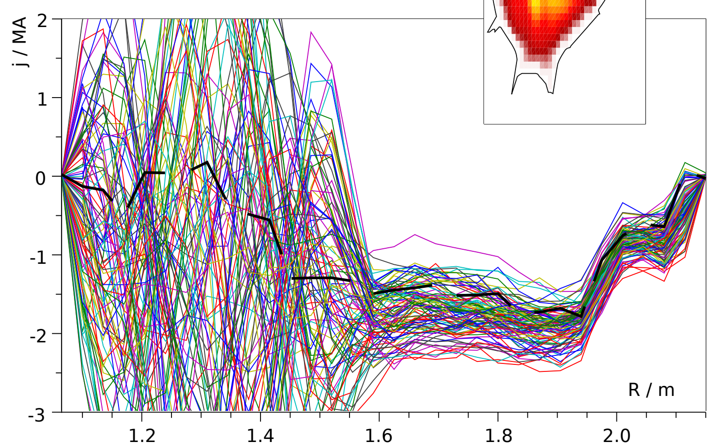
Normal MSE system:
30 x Bz at 30
positions along
NBI centre.



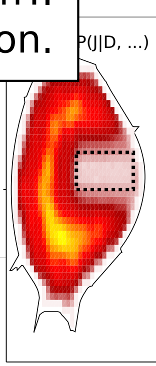


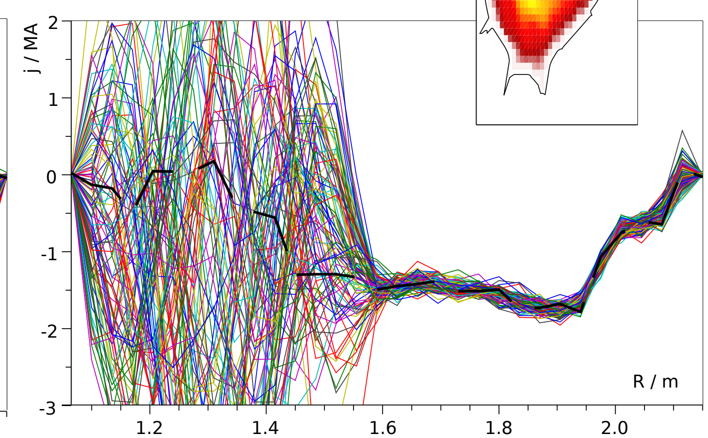
IMSE System:
30x30 grid of Bz
measurements.





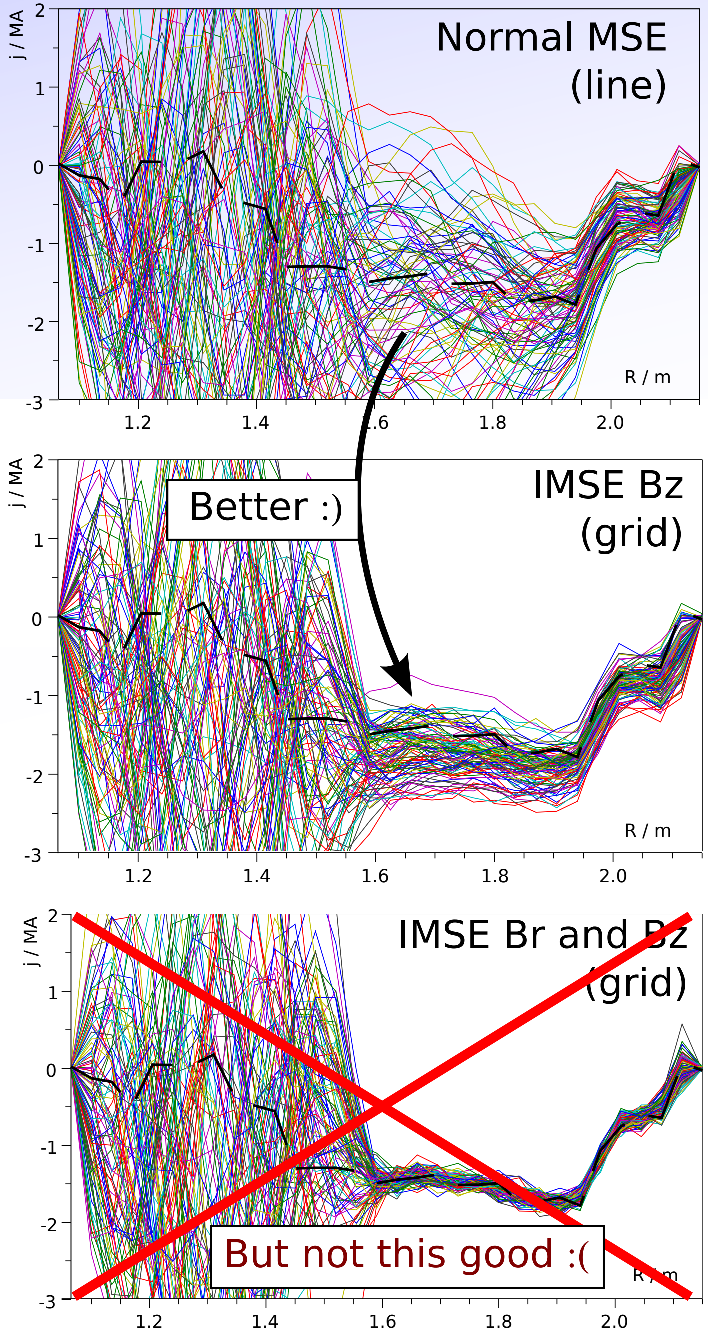
Just for interest:
30x15 grid of Bz
30x16 grid of Br.





All sigmaBr = sigmaBz =10mT

By current tomography II

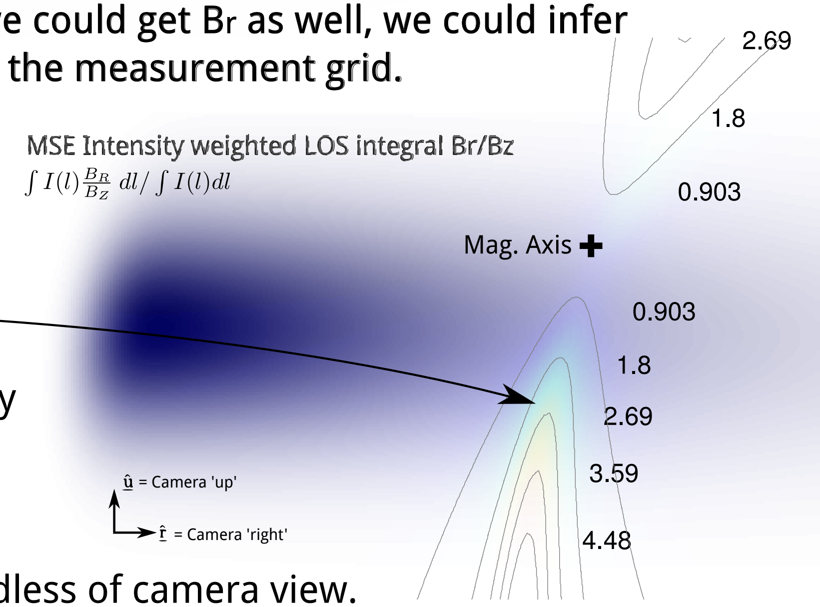


The IMSE still has some a large uncertainty in jphi offset. The unknown term it is not entirely pinned down by the magnetics.

However, the 2D IMSE inference is much better than the equivalent MSE system, for some reason.

Result with Br is much better: If we could get Br as well, we could infer the current almost exactly, within the measurement grid.

Off axis and near the core, the AUG IMSE system will see Br/Bz > 2 with reasonable signal strength:



Unfortunately, the beam geometry means information about Br is always swamped by Bphi. With NBI **v** in the midplane; **v** x **r** and **v** x **phi** are always together, regardless of camera view. There is a slight angle though. Full geomtry:

$$\tan\beta \approx \frac{(\hat{v}\times\hat{\phi})\cdot\hat{r}}{(\hat{v}\times\hat{\phi})\cdot\hat{u}} + \left[\frac{(\hat{v}\times\hat{R})\cdot\hat{r}}{(\hat{v}\times\hat{\phi})\cdot\hat{u}} - \frac{(\hat{v}\times\hat{\phi})\cdot\hat{r}}{(\hat{v}\times\hat{\phi})\cdot\hat{u}}\frac{(\hat{v}\times\hat{R})\cdot\hat{u}}{(\hat{v}\times\hat{\phi})\cdot\hat{u}}\right]\frac{B_R}{B_\phi} + \left[\frac{(\hat{v}\times\hat{Z})\cdot\hat{r}}{(\hat{v}\times\hat{\phi})\cdot\hat{u}} - \frac{(\hat{v}\times\hat{\phi})\cdot\hat{r}}{(\hat{v}\times\hat{\phi})\cdot\hat{u}}\frac{(\hat{v}\times\hat{Z})\cdot\hat{u}}{(\hat{v}\times\hat{\phi})\cdot\hat{u}}\right]\frac{B_Z}{B_\phi}$$

LOS Intensity averages of coefficients gives:

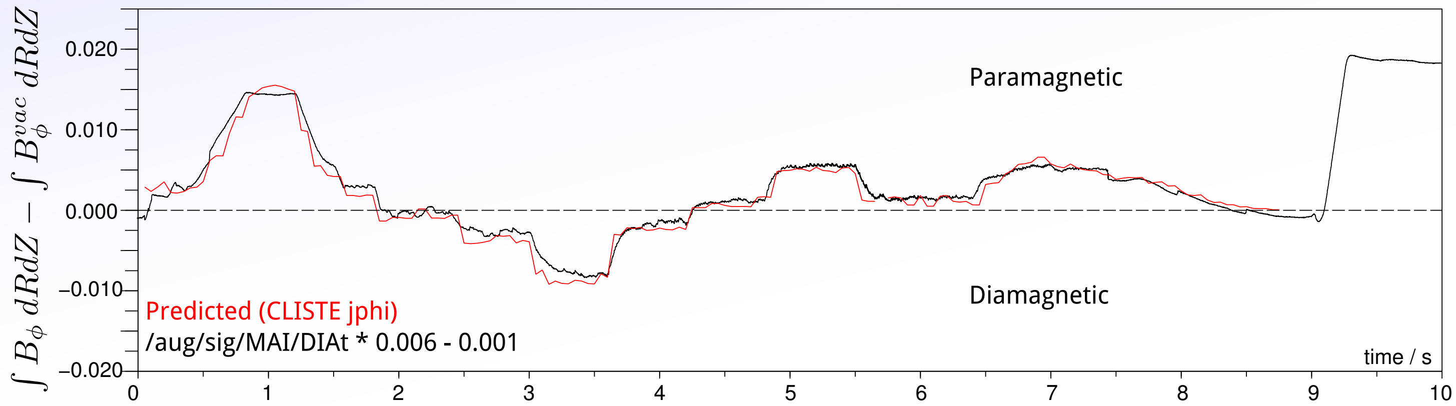
$$\tan\beta \approx 0.17 + 0.54\frac{B_Z}{B_\phi} + 0.05\frac{B_R}{B_\phi}$$

At 5 - 10%, it will have an effect, but we do not expect to see the full current recovery from 2D tomography.

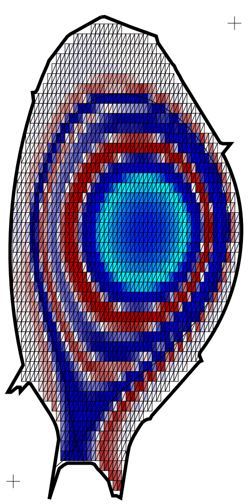
Para/Diamagnetics

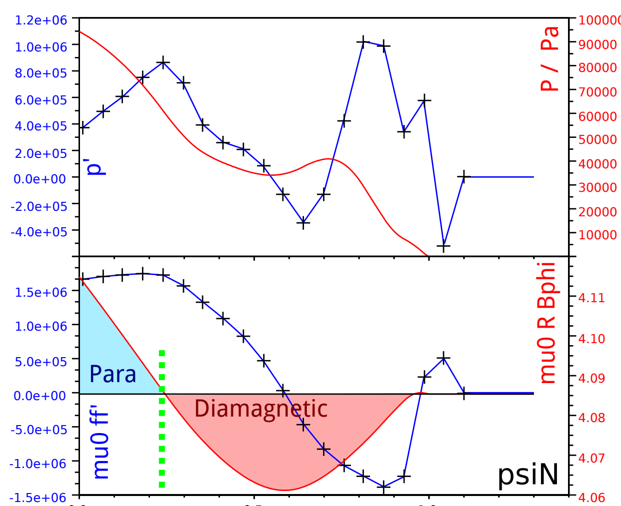
Some notes about Renee's results from the equilibrium point of view:

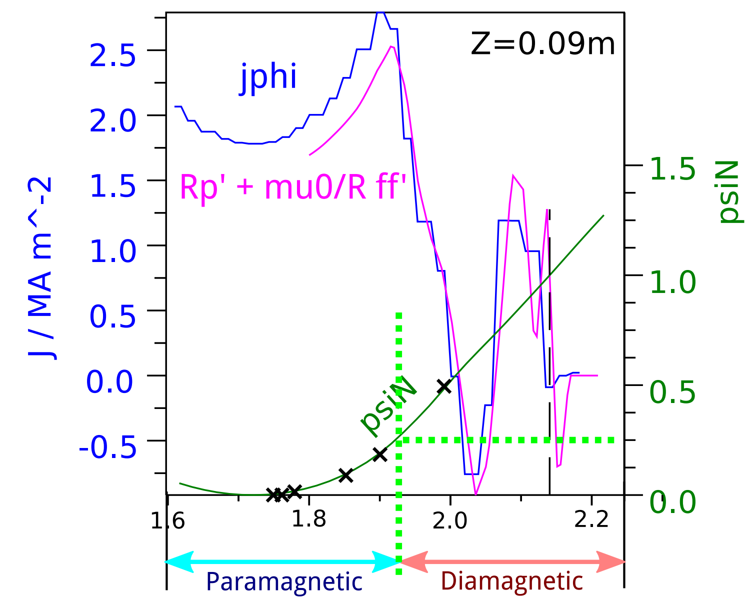
Just to see, we can load CLISTE's jphi into Minerva and integrate the toroidal flux over the whole vessel (calc. grid). There is a diagmagnetic signal outside the vessel which appears to be uncalibrated. With an offset and scale it mostly agrees with what CLISTE says:



Also, I can now run the code from my PhD work on JET which tries to extract the pedestal pressure from magnetics, wuth the AUG magnetic model. (P. McCarthy has already shown this works at AUG, as I did at JET). With sufficient relaxation of the ff' and p' smoothing priors, it actually finds an equilibrium which is paramagnetic in the very core and diamagnetic at the edge (albeit with a slightly silly pressure profile):

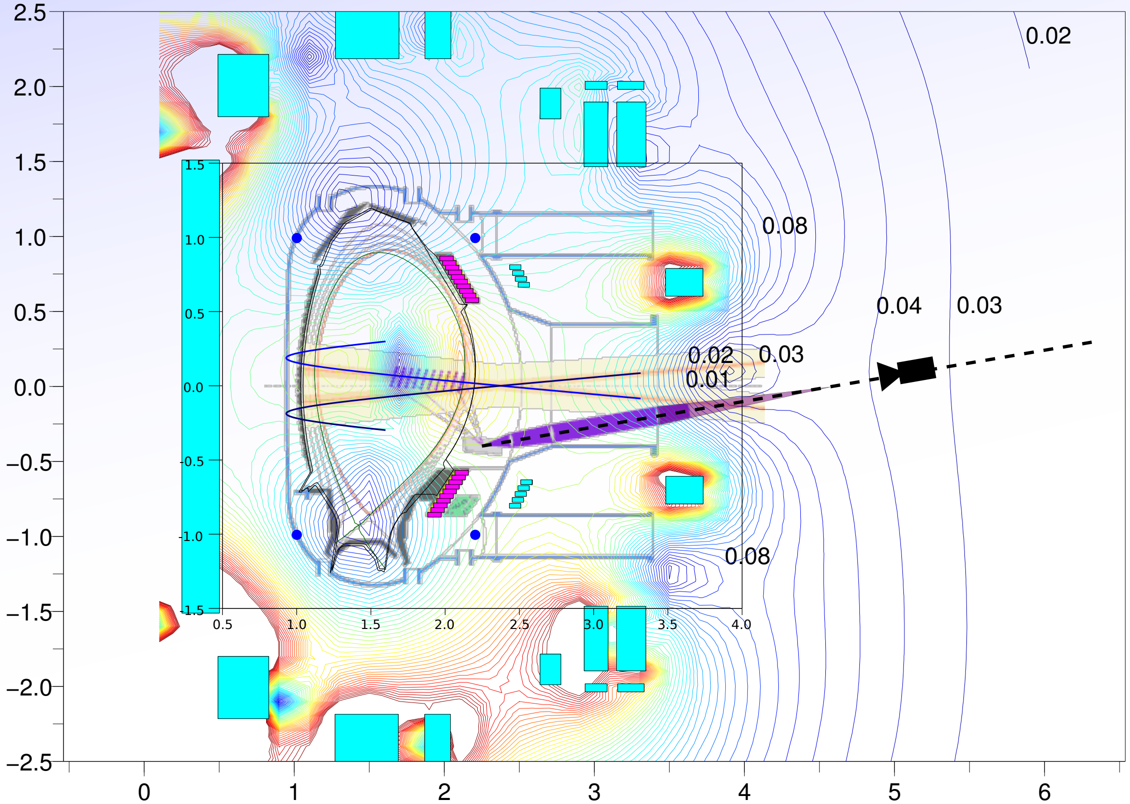






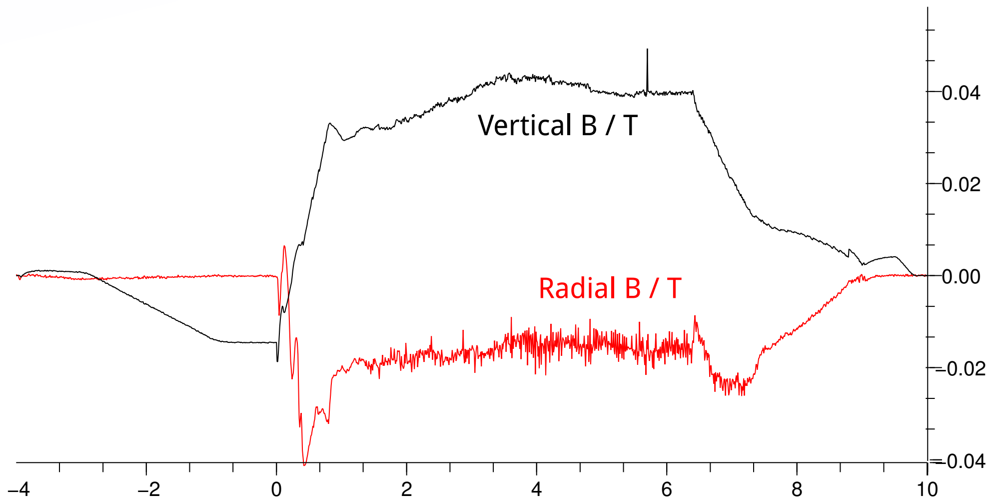
I'm not saying that this is happening, just that with a strong pedestal pressure gradient, it could be.

Other progress (Hardware)



Ideally, we want to fix the camera and optic plates directly to the viewing optics (no fibre etc).

Camera will be subject to magetic field, which Minerva can predict from the PF coils.
For the highest plasma current (Ip=1.2MA), |B| < 50mT:



- The camera we have (12bit 1376x1040 Imager QE) was used, next to the coils in Pilot (PSI) so may survive this. Apart from a very slow frame rate (10Hz), it is otherwise perfectly suited so could be used for a first attempt.

- Faraday rotation due the field in the Savart plates will not be a problem, but the main delay plate might be. (I'm assuming Lithium Niobate, but I can't find a Verdet constant for it in the Literature. Any suggestions?)

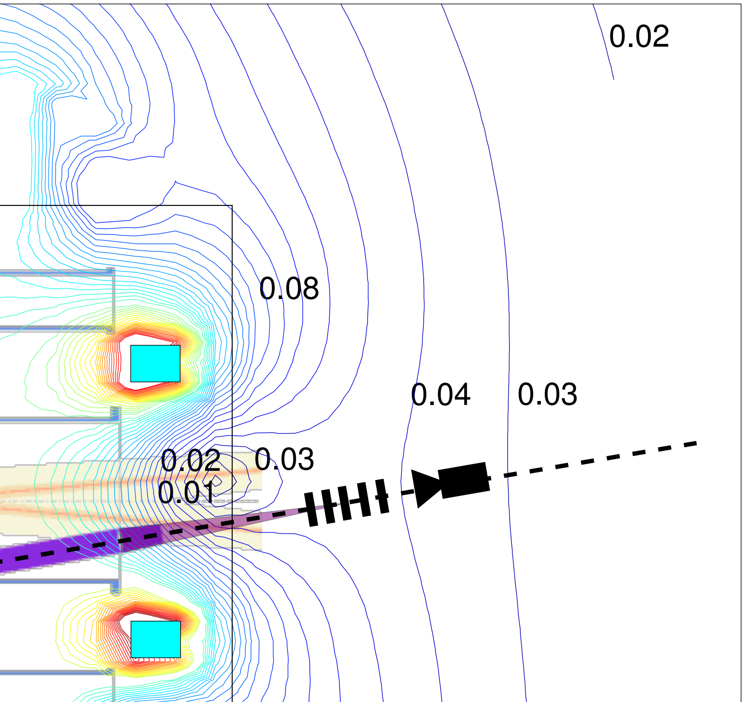
Poloidal Field at camera

50mT on the camera may be OK, and we should check direction sensitivity with whatever camera we use.
 - Could start with the imager QE that we have.

Field on optics:

Verdet constant for Quartz (Savart plates) is 16640 T-1 m-1 at 589.3nm which gives Faraday rotation of almost 0.01 deg mm^-1 in Savart plates with 50mT field perp to plate. (In reality it will be almost // to plate surface.)

Plates in sim currently 4/8/16mm. For 16mm, absolute worst case gives 0.16deg. So we are probably OK, but probably should measure the field.



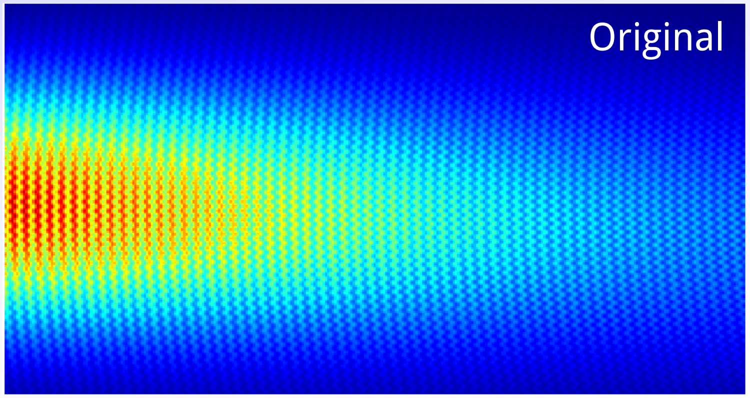
Delay Plates:
Lithium Niobate LiNbO3 (dielectric crystal)??
Can't find the verdet constant so calculated from 'becquerel' formula.
That gives 0.3 degrees per mm at 100mT, which
at e.g. t=6mm (max net constrast at 764 wave at 654nm)
--> 1.8deg
- Need to check this and ask JH.

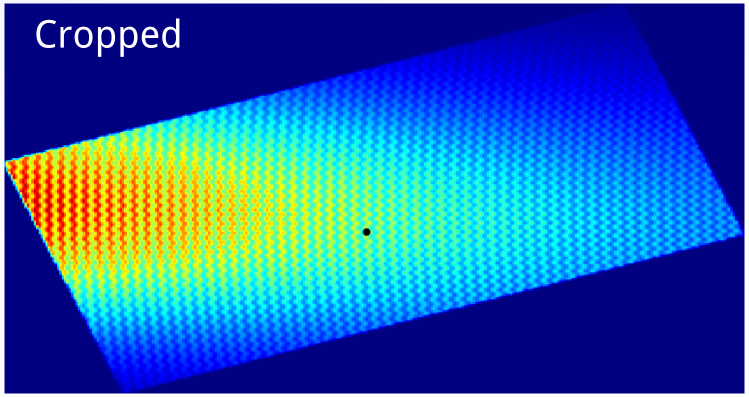
- What can the imagerQE take?
- Measure the field at AUG.

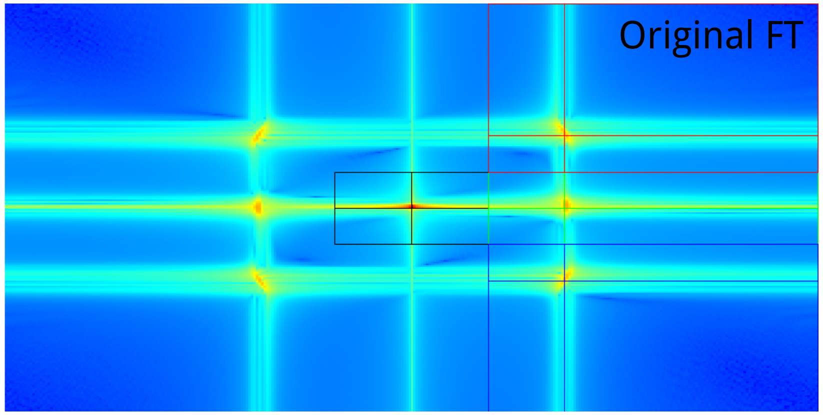
Non-statistical distribution

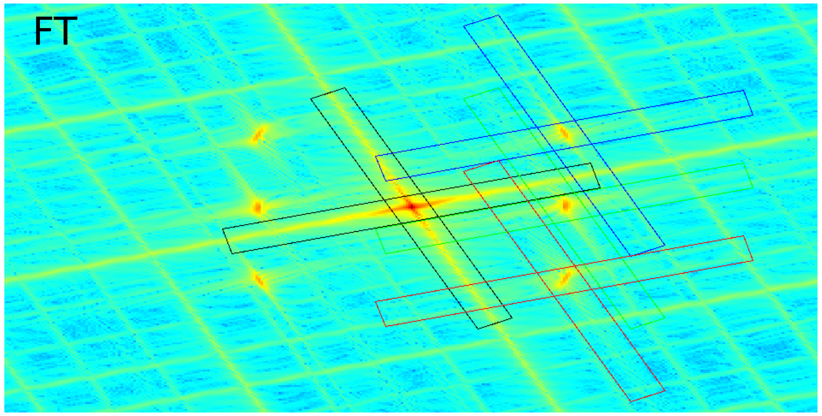
Looked at it, not important :p

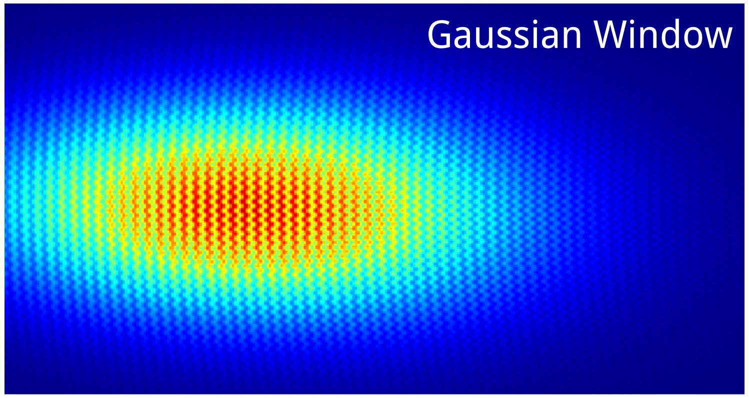
Demodulation Tweaks

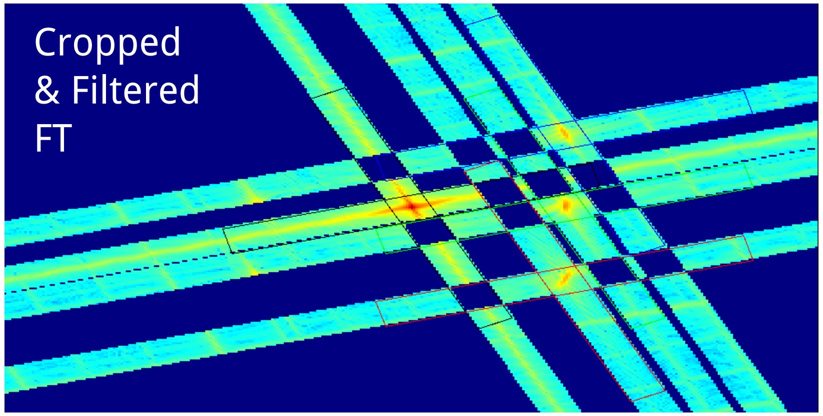


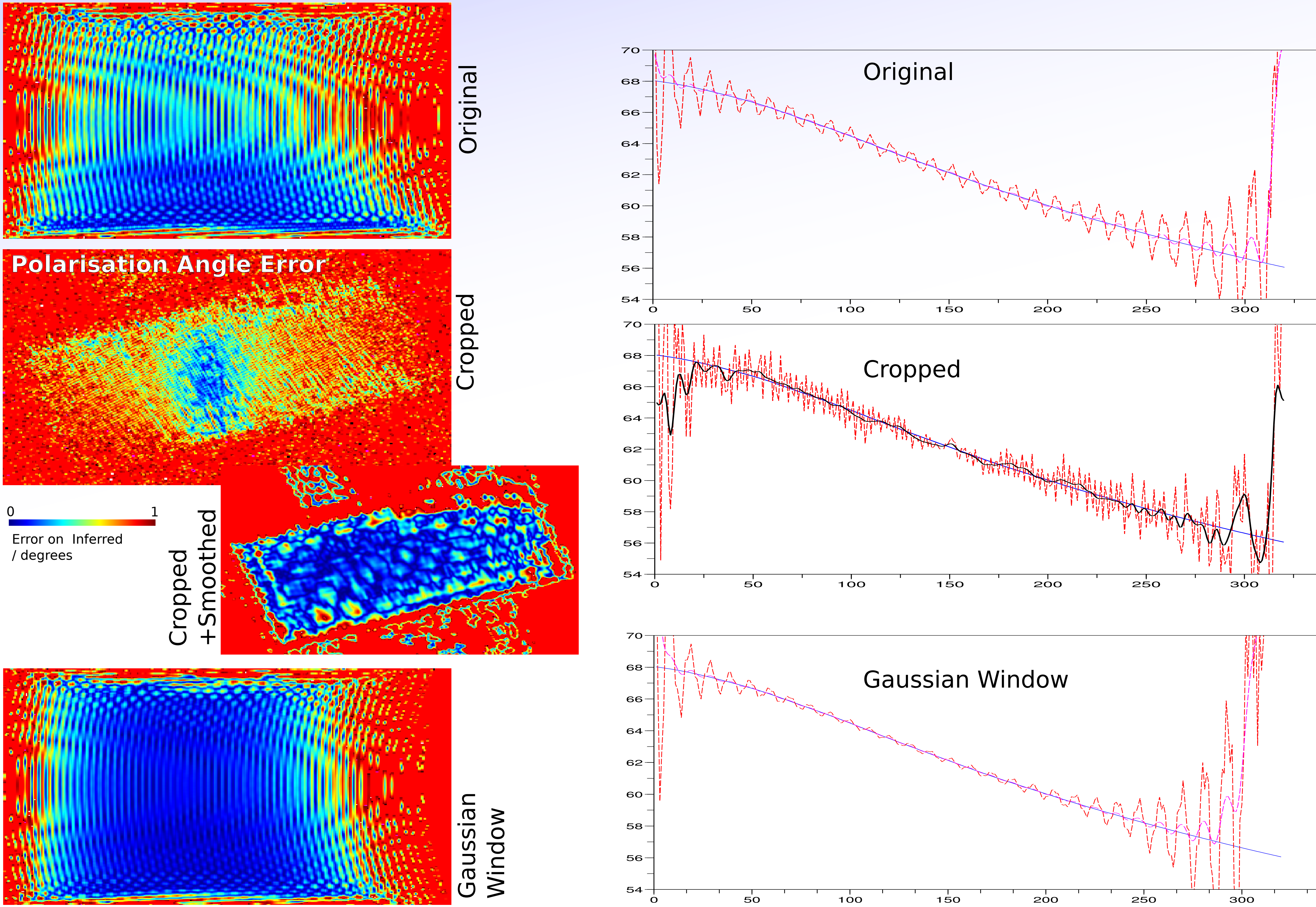




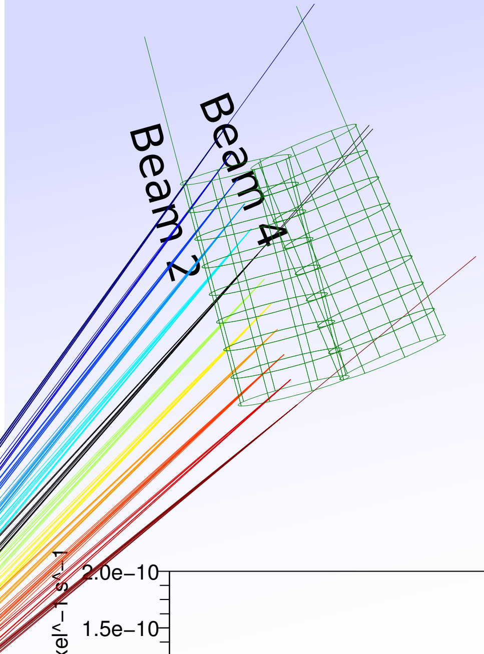


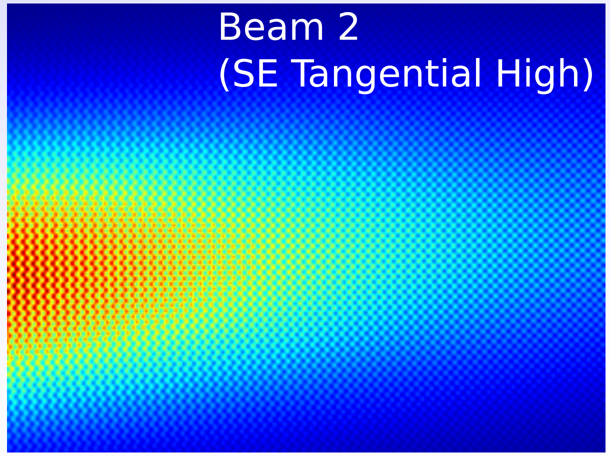


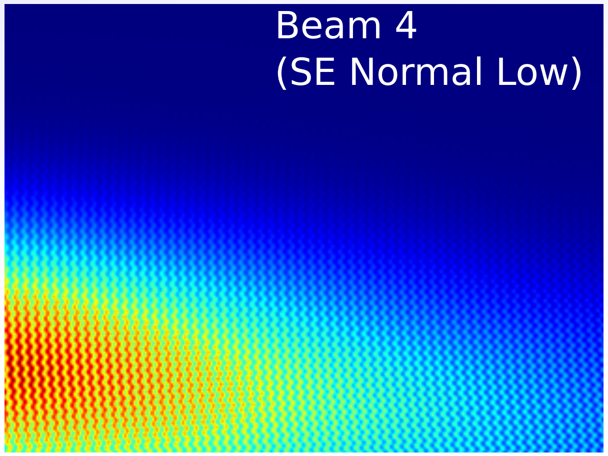


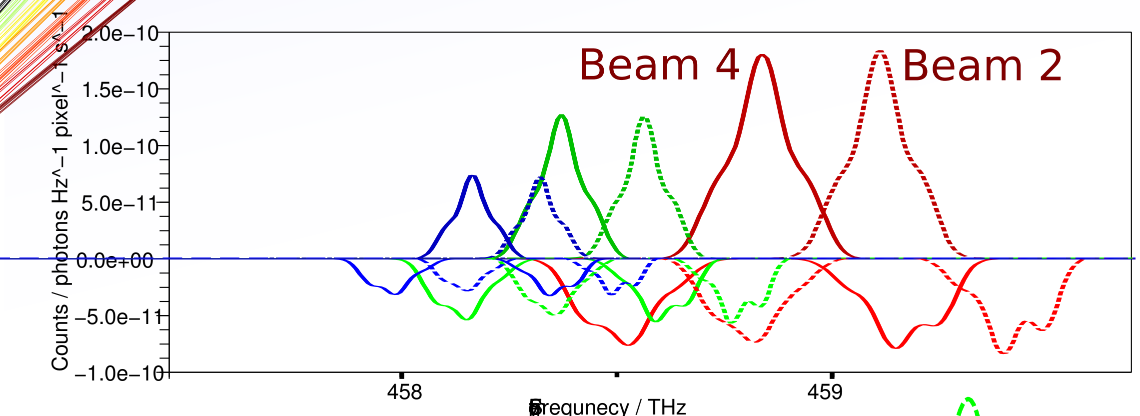

Original
Polarisation Angle Error
Cropped
0
1
Error on Inferred
/ degrees
Cropped
+Smoothed
Gaussian
Window
Original
Cropped
Gaussian Window
70
68
66
64
62
60
58
56
54
0
50
100
150
200
250
300

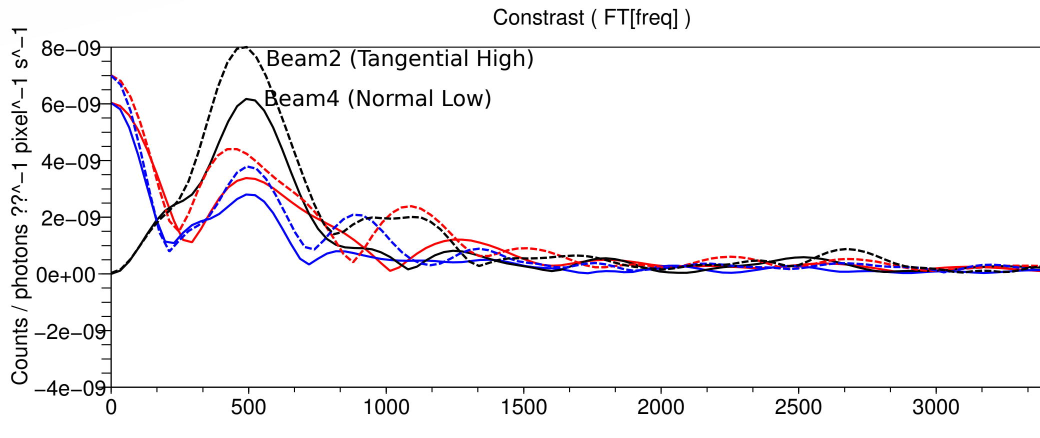
Spectral Contrast Weighted Averages

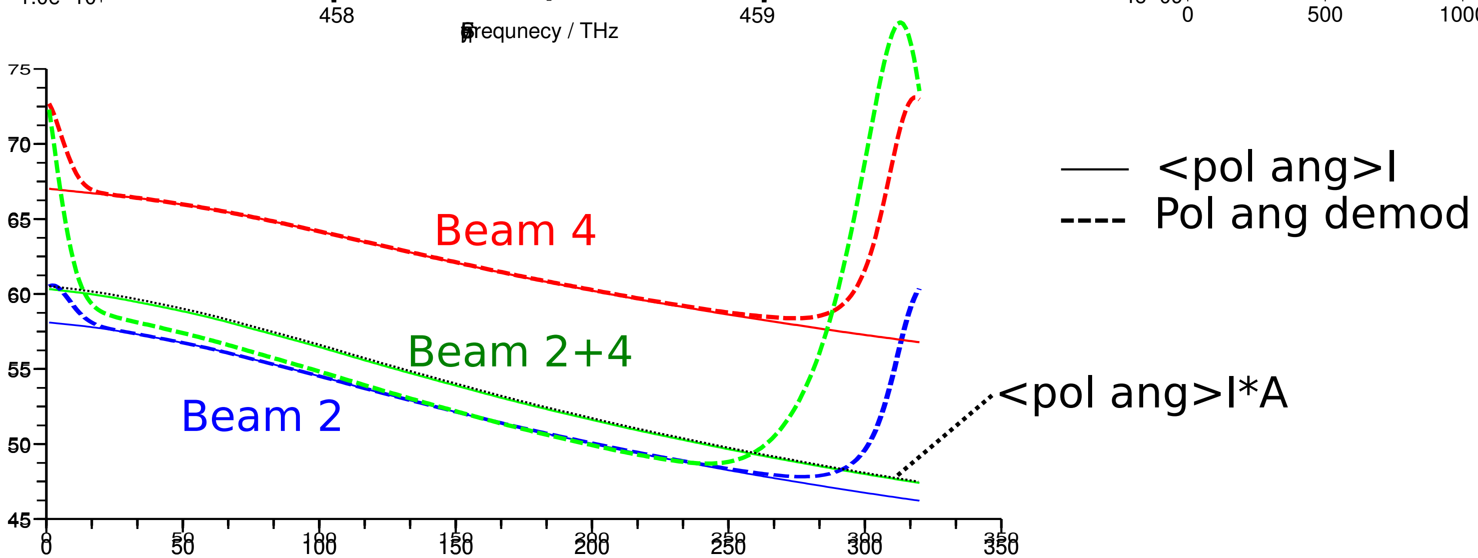




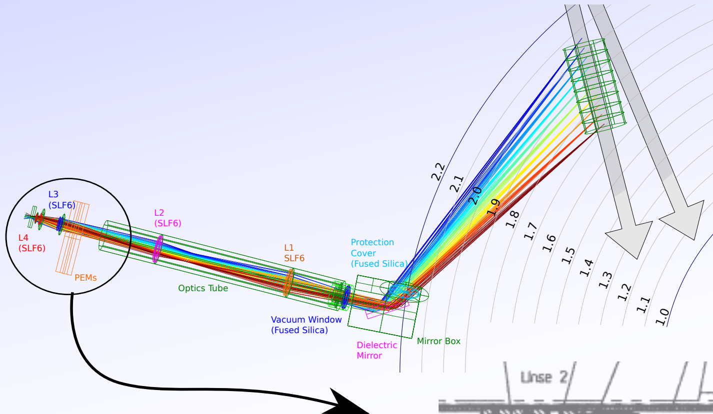


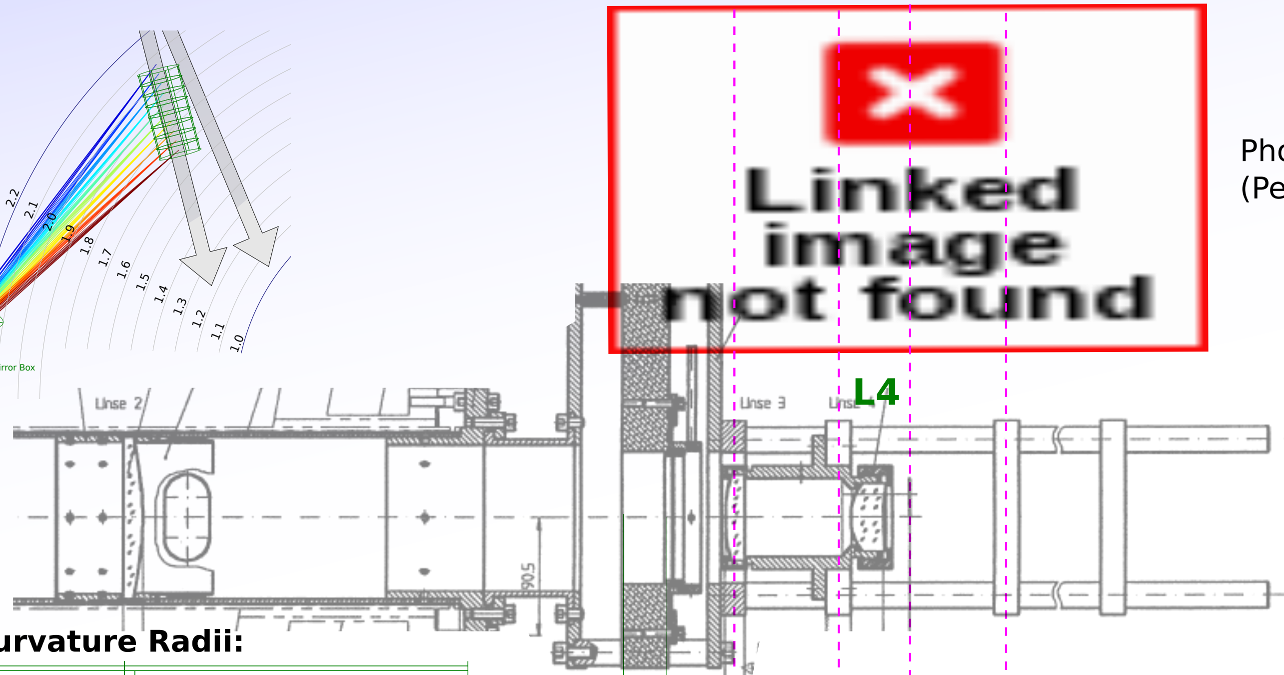






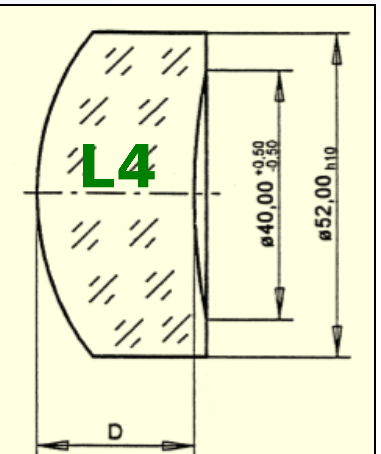
Ray Tracing - Inconsistency of Lens 4.





Photograph (28/03/12) (Perspective corrected).

Technical Drawing



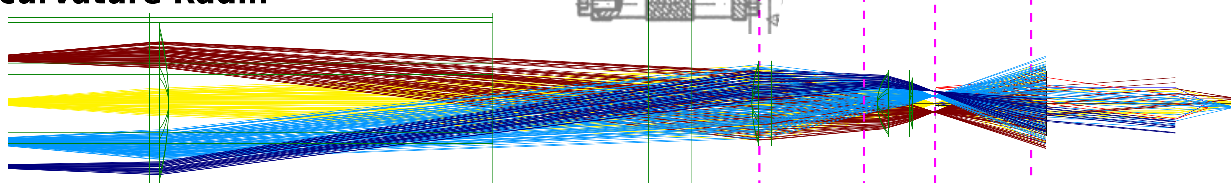
4. w (...)

3. Fasenwinkel zur optischen Achse

2. ((f_d = 75,56 mm))

1. ((Rechenmaße))

1) Trust curvature Radii:



2) Trust focal length, move image plane:

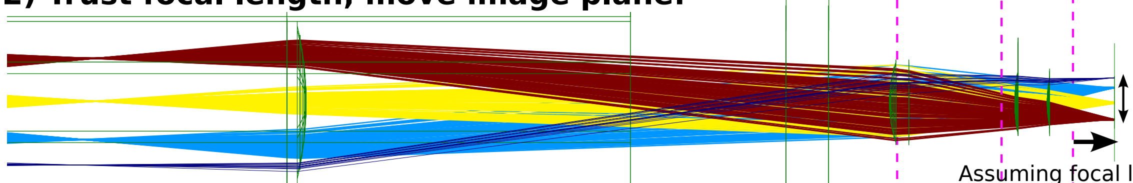


Image width now ~ 50mm which looks too big for fibres

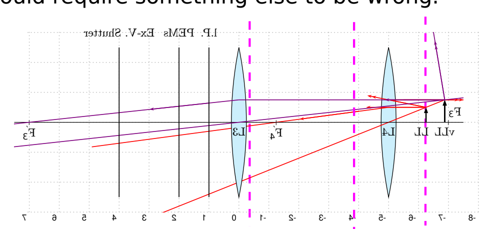
Assuming focal length moves image plane position,

Curvature radii and thickness don't agree [T. Löbhard]

R = 42,322 (4360)	n_d = 1,80518 +- 0,0010	R = -102,408 (7030)
freier Ø 47,0	v_d = 25,39 +- 0,8 %	freier Ø 35,0
3 / 5 (2) -	1 / 1 x 0,40	3 / 5 (2) -
4 / 0'	2 / 02	4 / 4'
5 / 3 x 0,25 ; R0,5	6 / 10	5 / 3 x 0,25
⊗ $T_{656\,nm}$		⊗ $T_{656\,nm}$
Fase 0,0	D = 25,0 +- 0,3	Fase 0,5 +- 0,1 / 45°

2) Trrust focal length, change something else.

T. Löbhard assumed focal length but kept image position.
Implies rays parallel through PEMs (which is compelling)
but would require something else to be wrong.

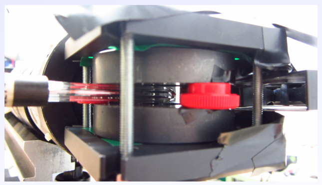


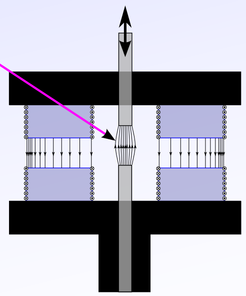
Where is the image plane??

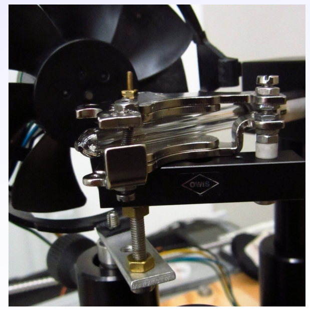
Magneton/HDD Magnets Zeeman Lamp

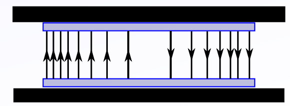
Small calibration lamp and Neodymium magnets from an old HDD and/or magnets from a microwave magneton.
Using all the delay plates I've got, and one of them tilted to produce fringes.

1) Magneton magnets
~150mT w/o top pole
~200mT with top pole



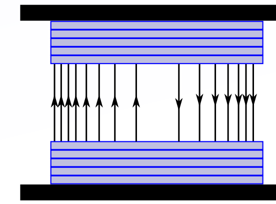






2) HDD magnets ~350mT

TODO:
Take a photo
after Easter



3) Lots of HDD magnets ~300-600mT

1) Spectral Single Spatial:

Neon/Mercury lamp (needs the fan to keep neon lines).
Zeeman splitting is:

$$\Delta E = \mu_B g B$$

g is O(1)

$$\Delta\lambda = \frac{hc}{\lambda_0^2}\Delta E$$

mu$_B$ ~ 5.8x10^{-5} eV / T
dL = 5.8e-5 eV/T * 100e-3 T * (650 nm)^2 / h / c in nm

per 100mT:
at 491nm = 0.0011 g nm / 100mT
at 650nm = 0.0020 g nm / 100mT
at 720nm = 0.0024 g nm / 100mT

longer l is better:
dL ~ l^2

For 720nm, g ~ 1.9

LiNbO3 (491nm): no=2.347, ne=2.254, (ne-no) = -0.093
LiNbO3 (650nm): no=2.282, ne=2.199, (ne-no) = -0.083
LiNbO3 (720nm): no=2.267, ne=2.186, (ne-no) = -0.081

[DeSerio 'The Zeeman Effect'
www.phys.utk.edu/labs/modphys/Zeeman%20Effect.pdf]
"Most levels in neon are not well described by LS coupling.
Because of this, the g-factor is not given by [normal equ]"
491nm: (Not Neon, it's mercury)
650nm: g=1.137 <-- This could be useful for calibration of final system
724.5nm: g=1.984 <-- looks to be the highest in neon, also at 703.2 743.9, 808.2

Fringe contrast for Gaussian of width sL at I0.

$$A = \exp\left(-\frac{\sigma_\lambda^2}{2\lambda_0^2}\Delta\phi^2\right)$$

$$\ln A = -2\pi^2\left[(n_o - n_e)\frac{\sigma_\lambda}{\lambda_0}\frac{L}{\lambda_0}\right]^2$$

NB: This is the wrong calculation.

For lithium niobate: at 650nm, no = 2.282, ne=2.199,
half contrast point (A = 0.5) is:

$$\sigma_\lambda^2 L^2 = 9\times10^{-25}$$

S0 at 725nm, with g=1.9, B=300mT, L=69cm
Should get about A = 60% for dM=+/-1 (perp to field),
compared to the still ~100% for Dm=0 (para to field)
because the vert ones shouldn't be split and should be narrow.

For proper Zeeman polarimetry CIS, they should add up to
A = 20% I0 at L=69mm

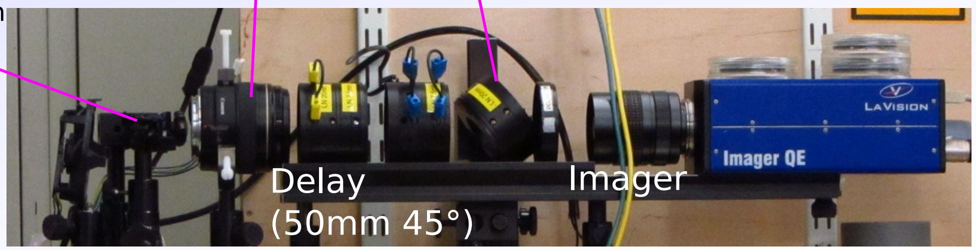
Magneton/HDD Magnets Zeeman Lamp

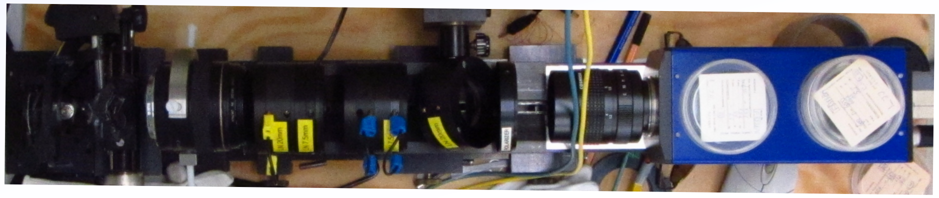
Experiment 1: With 1 tilted 20mm LiNb plate (+50mm untilted)
fo = 100mm, fi = 75mm

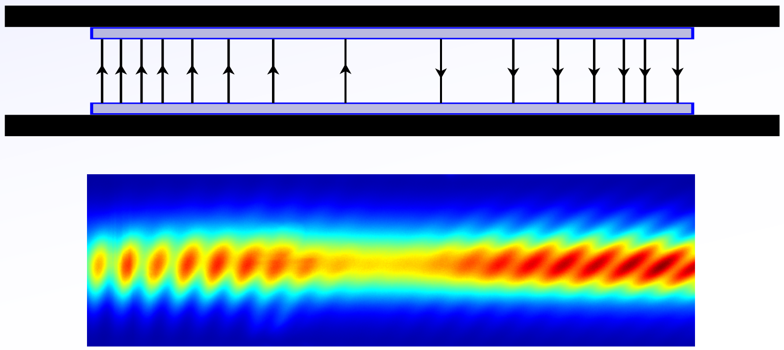
Magnet +Lamp on translation stage

Objective

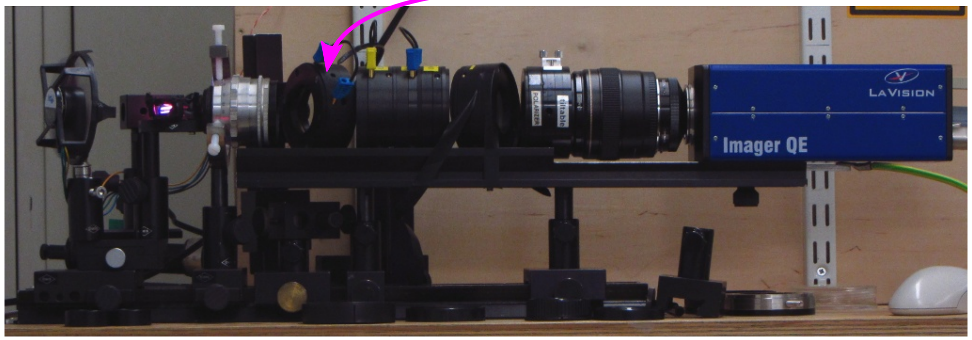
Tilted (20mm 45° at 45°)

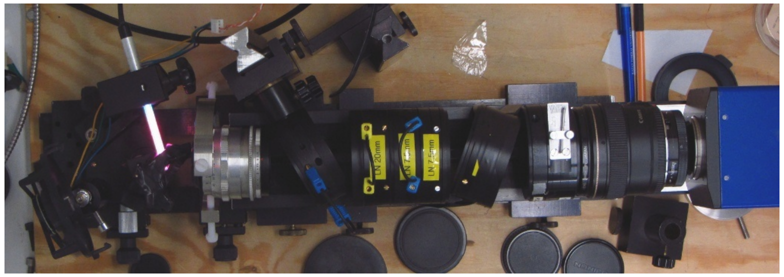






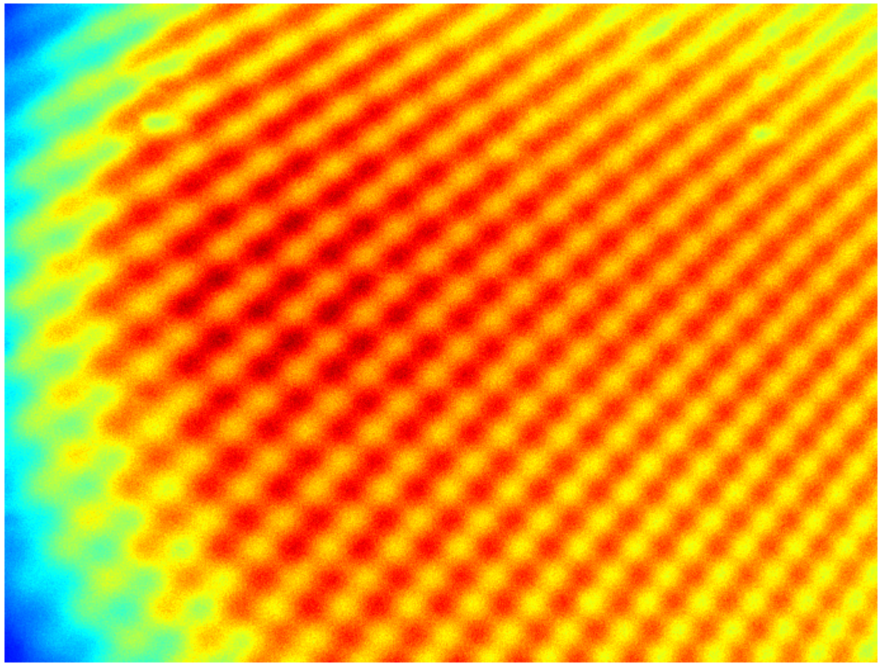
Experiment 2: ADSH - Add 15mm tilted plate with axis at 45° to field.
Zoom in on high field area (fi=25mm, fo=135mm)





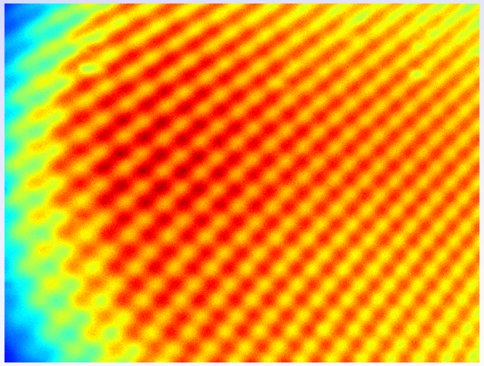
About 30% contrast.
Which is surprisingly good.

Objective a bit out of focus to get the whole image covered:

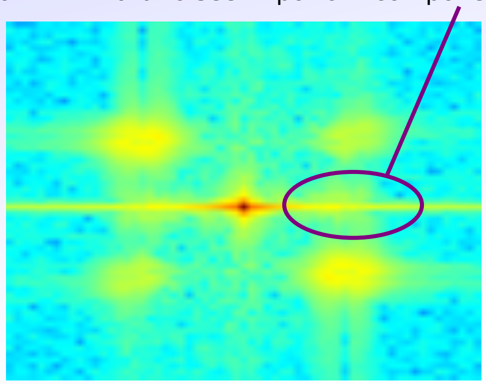


Magneton/HDD Magnets Zeeman Lamp

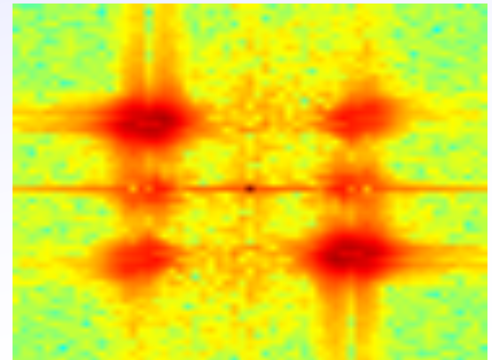
Image



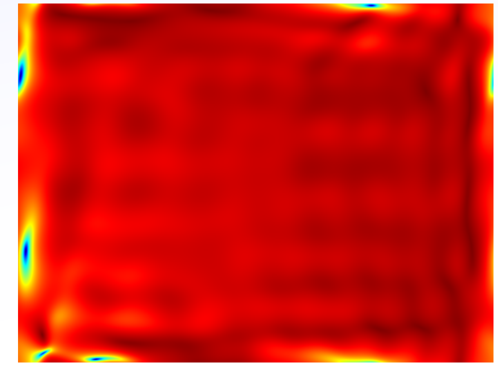
Raw FFT: Hard to see important component



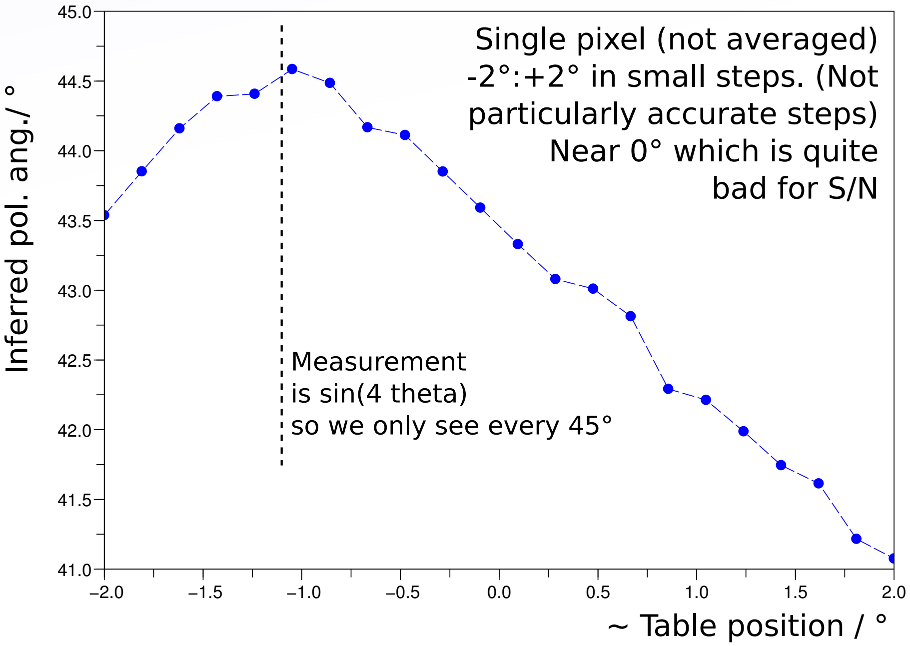
FFT with I0 + edge effects removal:

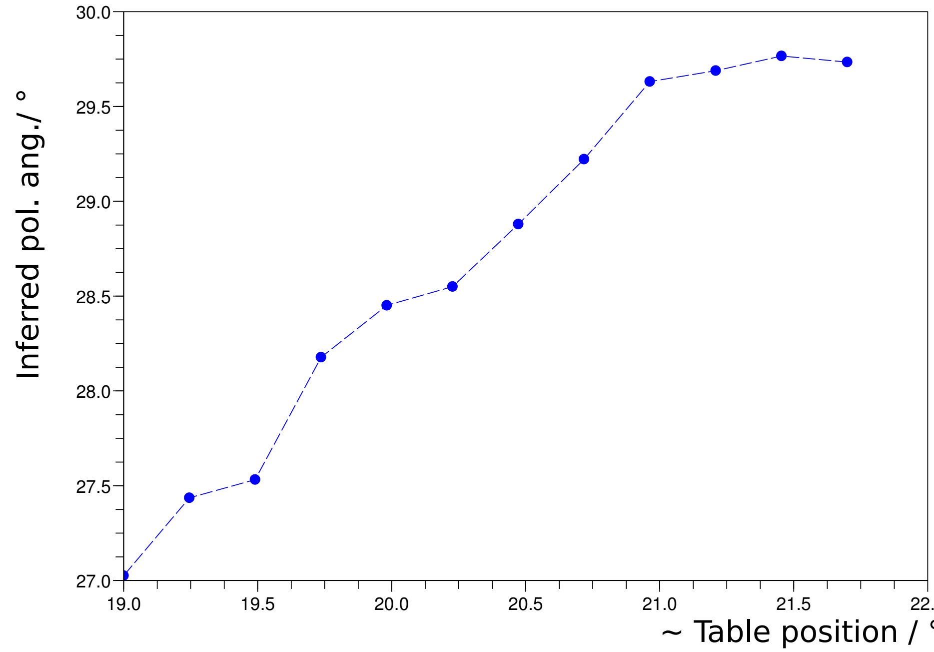


Polarisation Angle



Still some edge effects causing systematic spatial noise ~ +/- 0.5°.
However... Rotating bulb and magnets on rotation table:

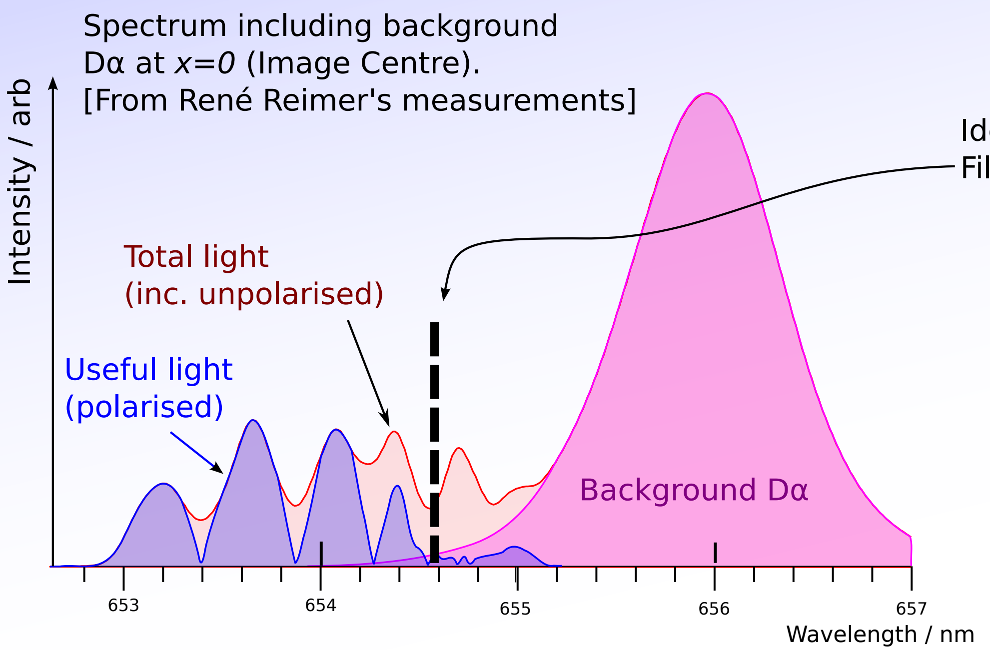


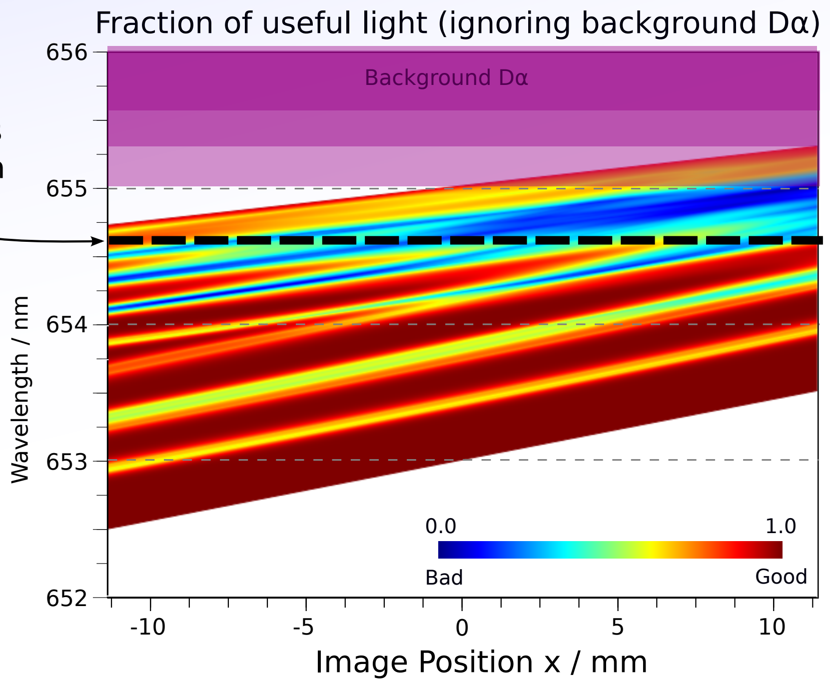


Summary:
Throwing together some old hardware, PC fan and HDD magnets, we can infer polarisation angle images down to at least ~0.5°, at best 0.1° having improved demodulation methods.
0.1° is what we want for the final system with all the proper hardware, so things are looking promising.

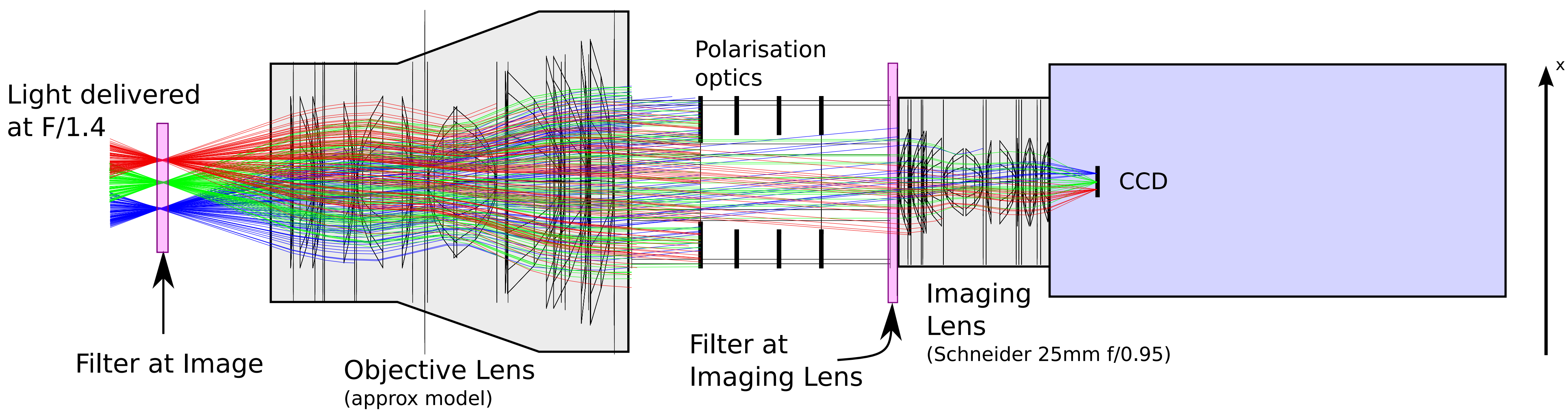
IMSE Design - Spectrum and Filter

Oliver Ford, IPP Greifswald





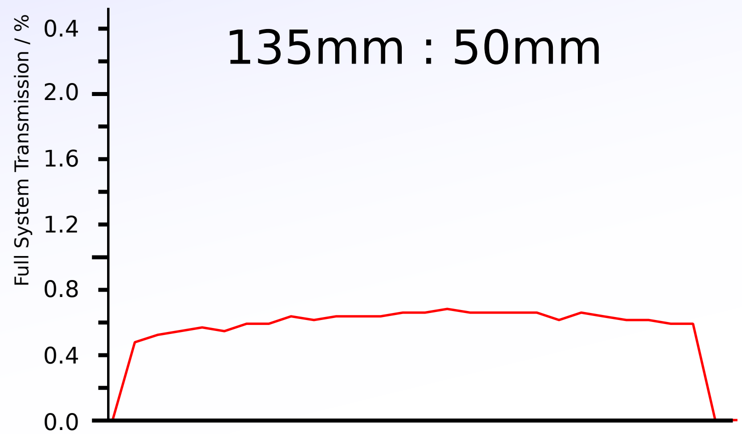
Filter can be placed at intermediate image plane, or on the front of the imaging lens (in the parallel rays):



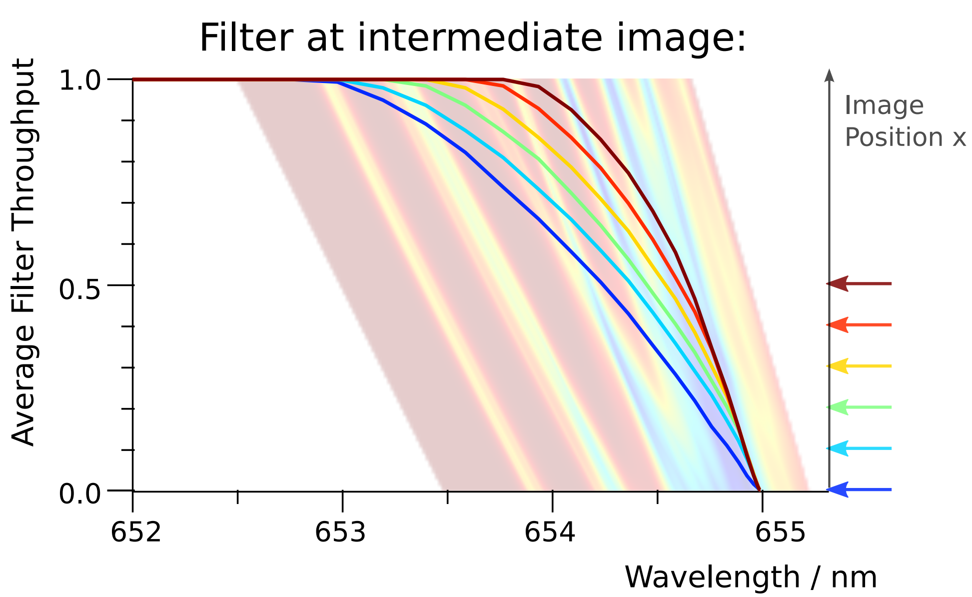
Oliver Ford, IPP Greifswald

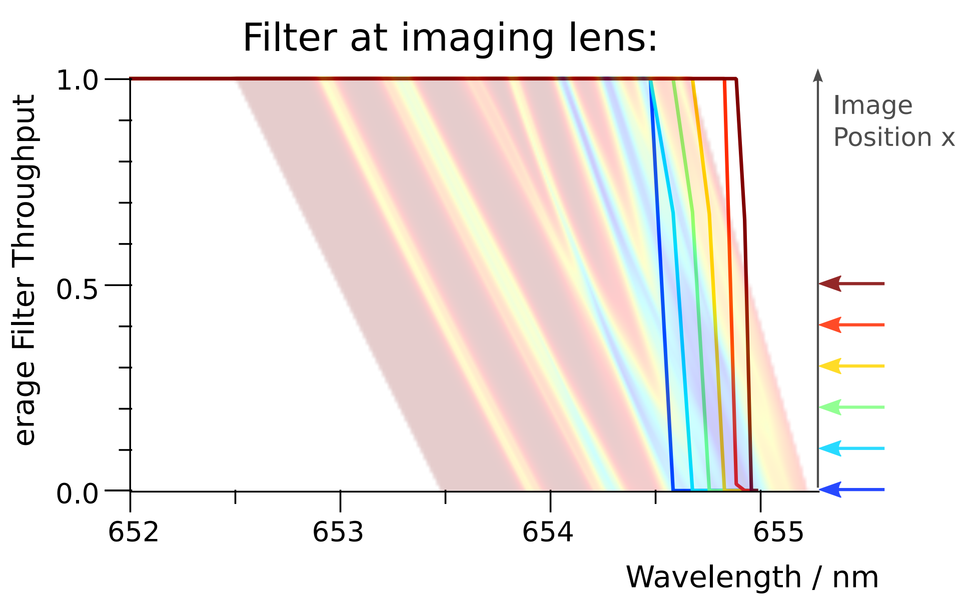
IMSE Design - Throughput and filter shift.

For the 135mm:50mm standard case, light throughput is only ~0.4% of MSE emission to mirror.
(~6% of light delivered to intermediate image).



Some proportion of the light goes through the filter at a very steep angle and shifts the filter short-pass into the useful spectrum. The filter functions for different image positions calculated by the ray tracer are shown below. These assume a filter effective index of $n = 2.0$ and an ideal sharp 655nm short-pass filter at normal incidence:

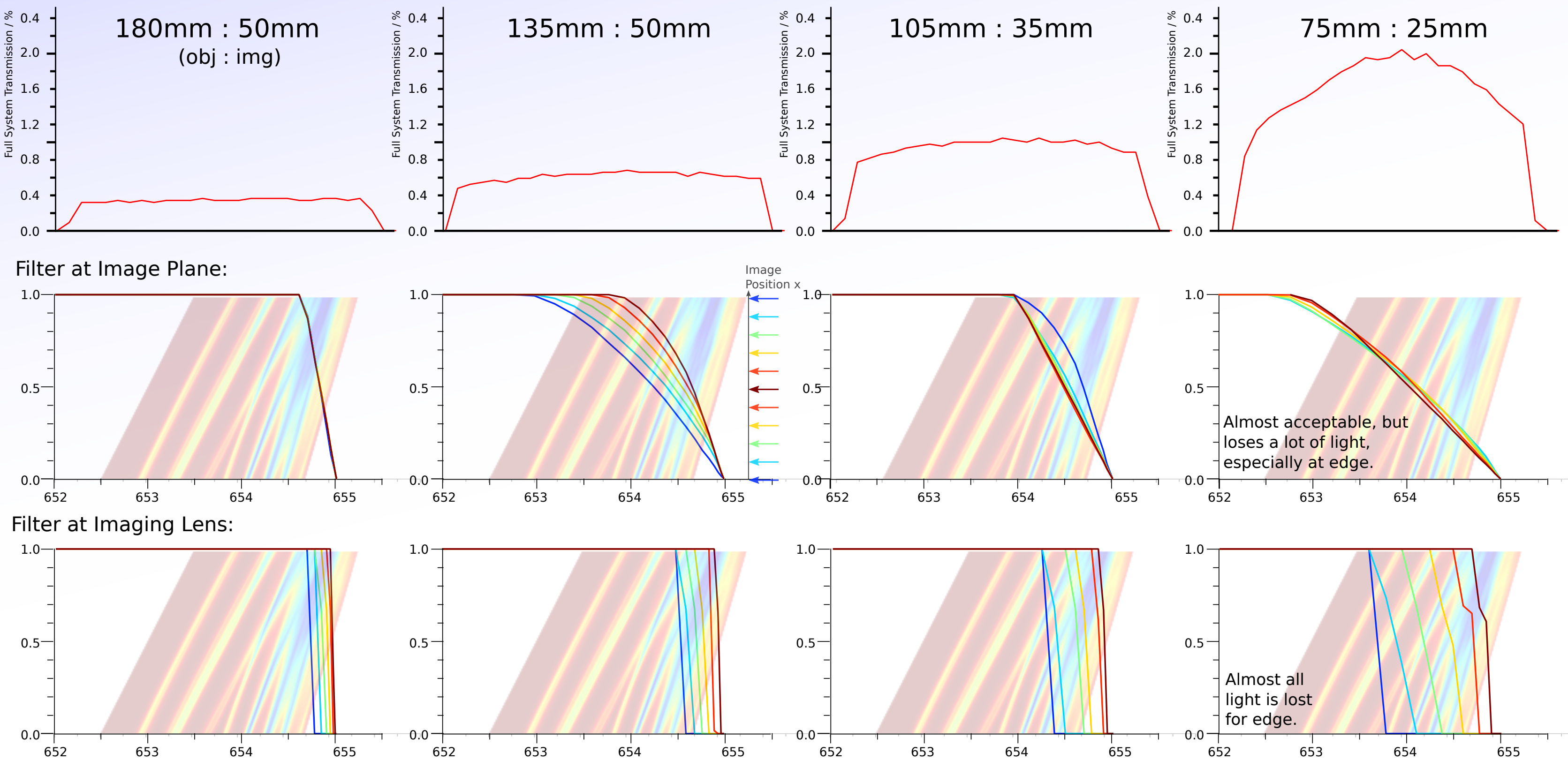


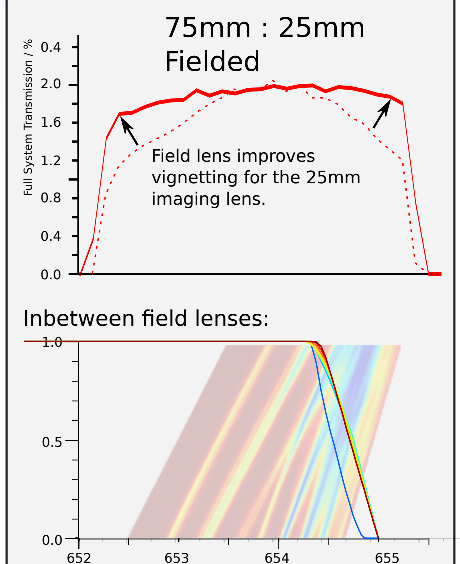


Oliver Ford
IPP Greifswald

IMSE Design - Throughput and filter shift (ray-tracer)

Throughput of light, and angle of light through the filter depends on the pair of lenses.
(It depends on the exact model of the lens, not just the focal length and F/#)

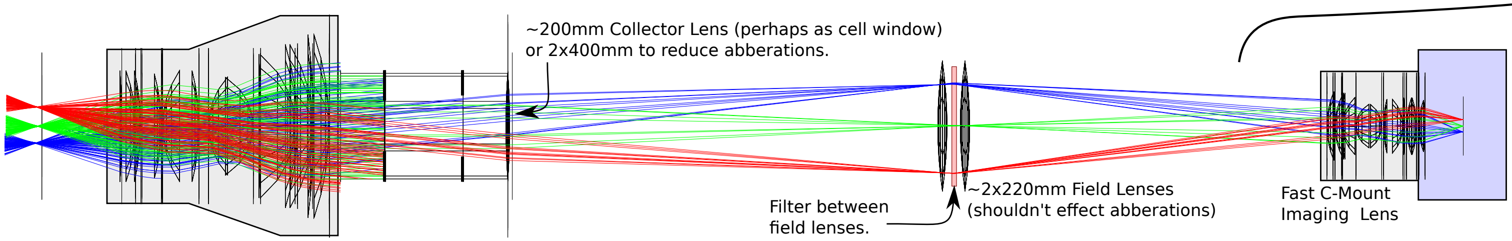


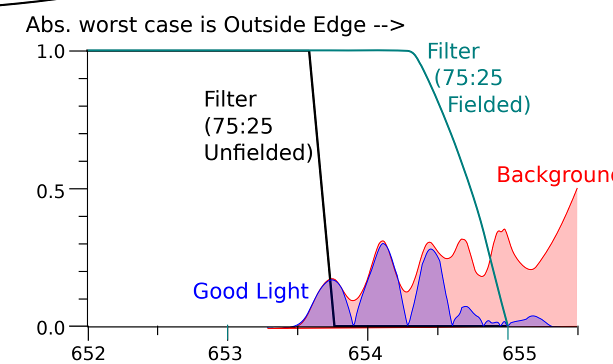


Significant improvement for the filter, and now with the possiblity of tuning it by tilting it.

However, it requires a bigger filter.

75:25 gives ~3x more light than 135:50 but angles are too big for filter, and most/all light is lost at edge channel.
In reality vignetting was also higher and edge of image is entirely lost (can only see ~19mm of fibre plane)
Fielding the light after the cell into the imaging lens (should) solve the vignetting and It also helps with the filter a lot:





For the fielded case at the very edge, it integrates up to about 66% of the good light under the filter which is 66% * 1.6% = 1% of collected light, this is already > 2x the safe 135:50 case, and we're still at ~3x for the rest image.

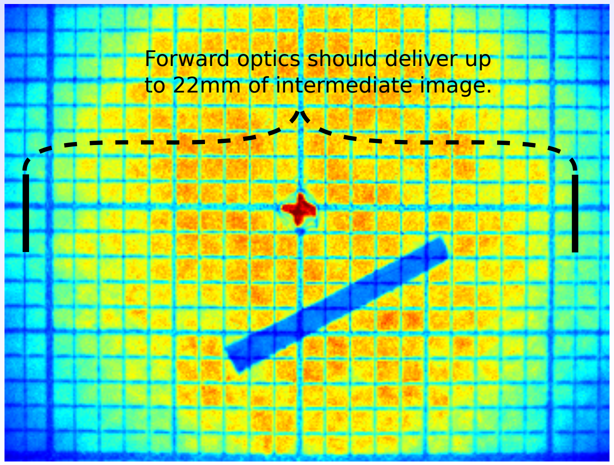
But... abberation after plates hurts our fringe contrast so the collector lens needs to be good (without being a camera objective lens)

IMSE Design - Throughput and vignetting (lab)

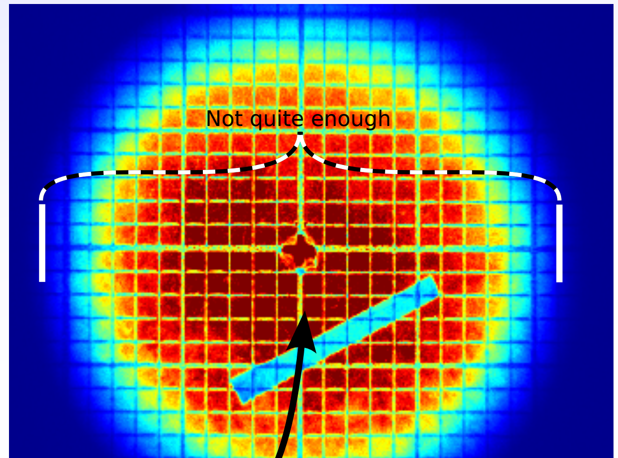
Oliver Ford
IPP Greifswald

In the lab, the situation is similar, but a bit worse:

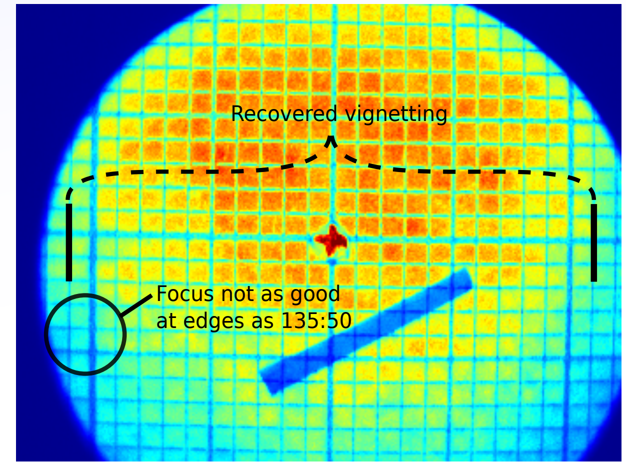
135:50



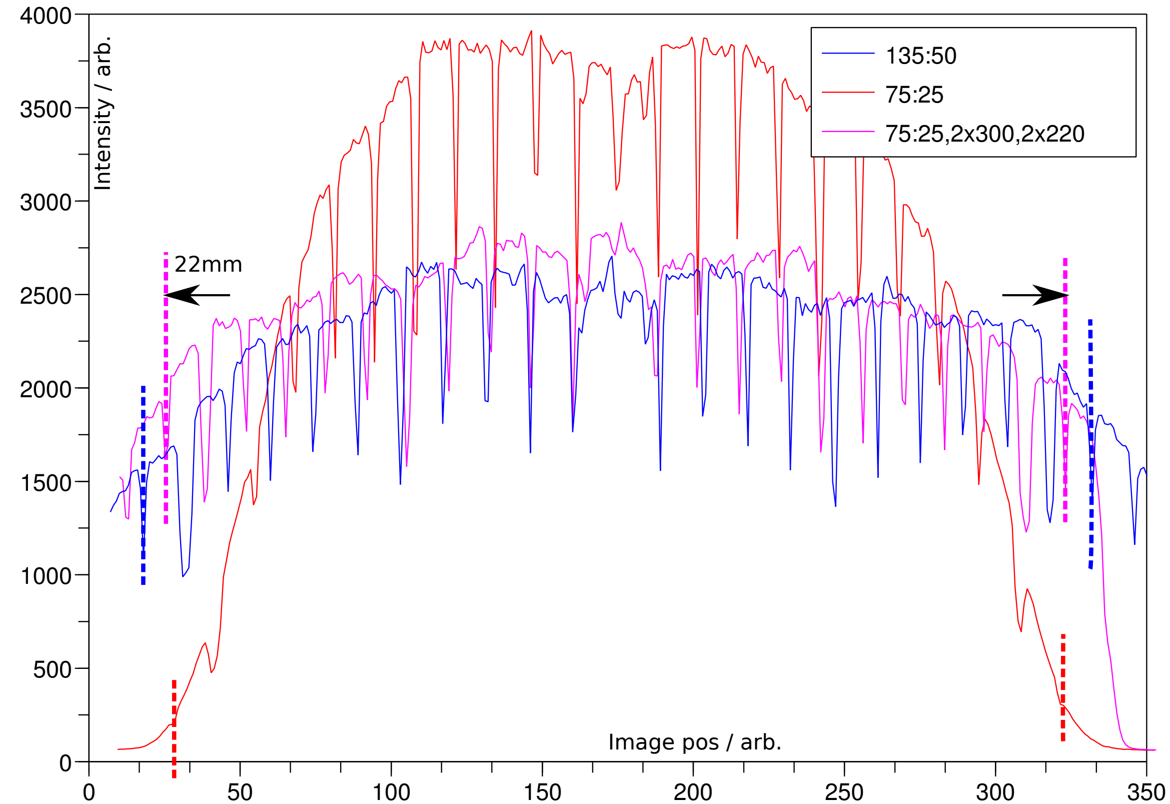
75:25



75:25 +2x300mm collectors,
+2x220mm fields



75:25 gives only a 50% increase in light in centre (1.5x as much as the 135:50) and the vignetting loses too much of the edge. The graph paper is at first image plane and we probably need to see 22mm of it.



Fielding fixes vignetting for 75:25 but uses 4 lenses. They are uncoated old lenses that were sitting in a cupbaord since 1960. All 4 lenses together only transmit ~60% of original intensity (measured) and leaves light level almost exactly back where we started.

However, with coated optimised lenses coupled with the improvement in the filter angles, it will improve the S/N by at least 50%.

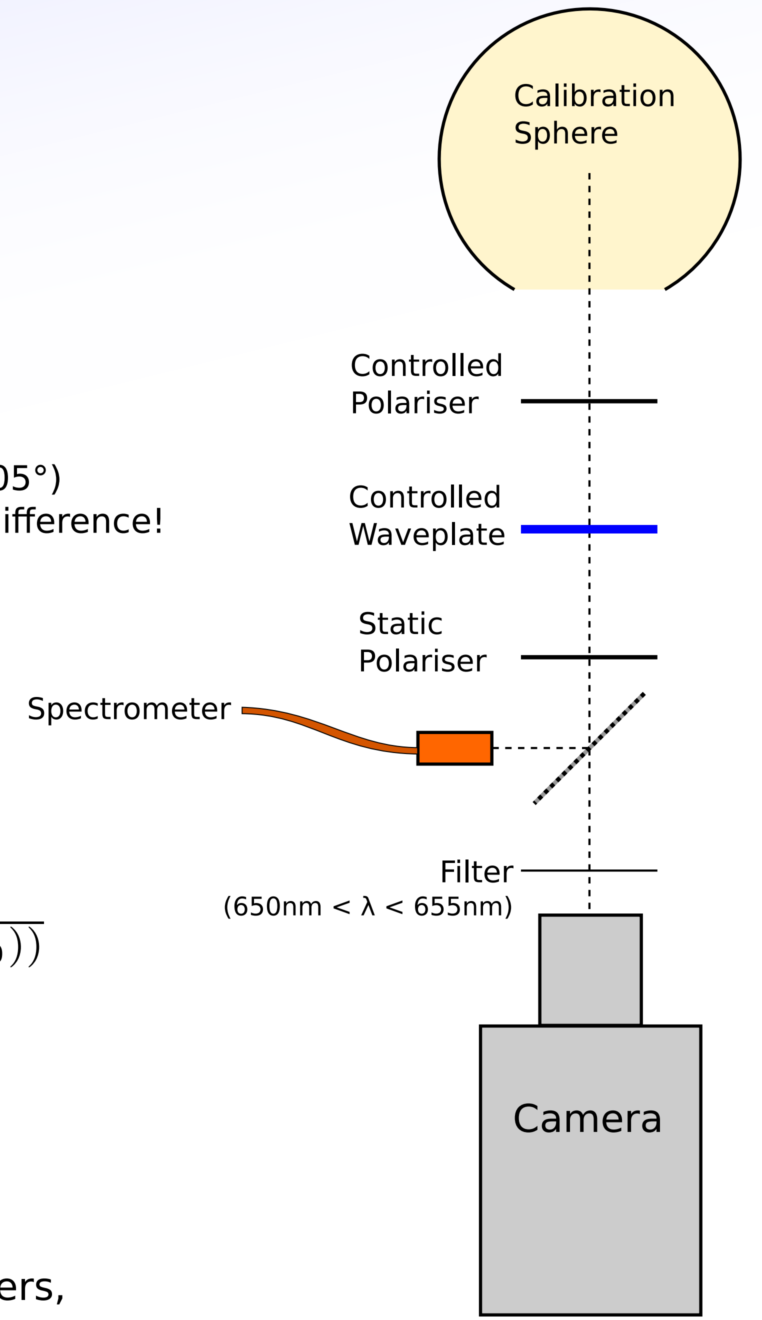
Waveplate Tests

Oliver Ford
IPP Greifswald

The AUG IMSE system has 3 auxiliary waveplates, specified as:

- λ/2 at 653.5nm
- λ/4 at 653.5nm
- λ/4 Ferro-electric Liquid Crystal at 653nm -
  (Always λ/4 and should switch principle axis orientation by 45°)

Are these exact? Can the inaccuracies or non-ideal effects cause the non-zero ellipticity seen by the IMSE?



1) Laser align polarisers, camera, and spectrometer.
2) Full scan first polariser with no waveplate to find 0° and 45° positions (±~0.05°)
3) Insert and laser align waveplate. Non-normal incidence makes a significant difference!
4) Set polarisers crossed and scan waveplate rotation
   - complete extinction for all wavelengths at 0°.
5) Set waveplate at 0°, measure spectrum normalisation.
5) Set waveplate at 45° (now ±~0.2°), measure spectrum.

Phase diff: $\phi(\lambda) = \frac{2\pi L}{\lambda}(n_o(\lambda) - n_e(\lambda))$

Thickness L set from design wavelength λ0: $L = \frac{\phi(\lambda_0)\lambda_0}{2\pi(n_o(\lambda_0) - n_e(\lambda_0))}$

For other wavelengths: $\phi(\lambda) = \frac{\lambda_0}{\lambda}\frac{(n_o(\lambda) - n_e(\lambda))}{(n_o(\lambda_0) - n_e(\lambda_0))}\phi(\lambda_0)$

For crossed polarisers, expect intensity: $I \propto \frac{1}{2} \pm \frac{1}{2}cos(\phi)$

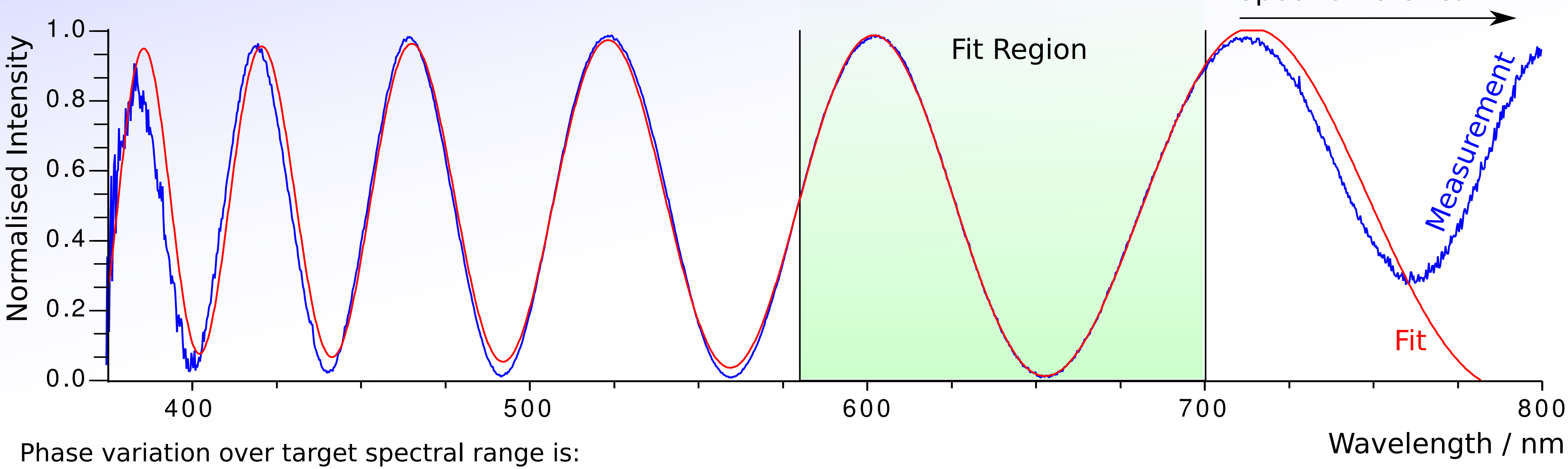
+for aligned polarisers,
- for crossed

Waveplate Tests - Half Wave

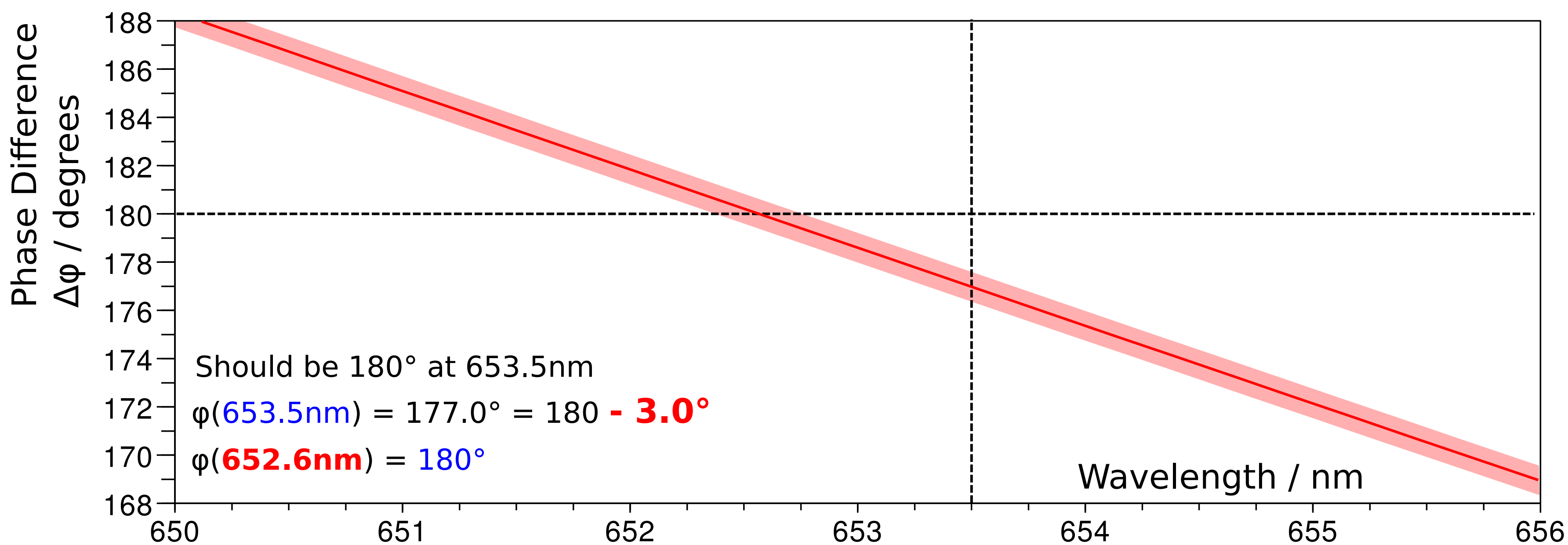
Oliver Ford
IPP Greifswald
gmds/SPECLAB/397

Plot Δφ through Sellmeier equation, get plate order (N = 5) correct for full range.
Then fit Δφ(λ0) to nearby part of visible spectrum.

Sellmeier Equs?
Spectrometer cal?



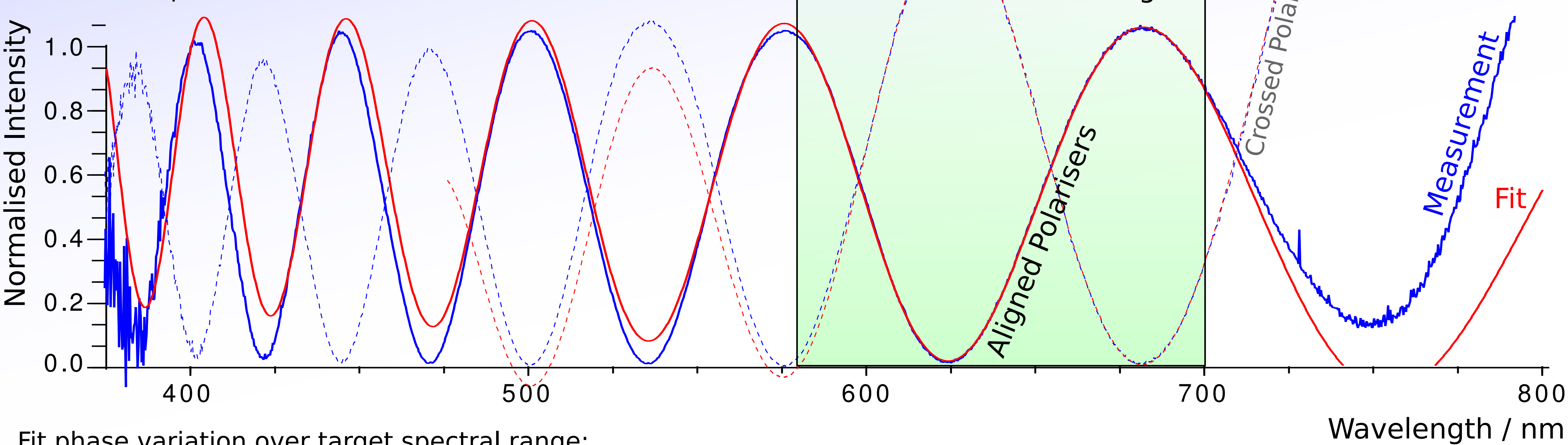
Phase variation over target spectral range is:



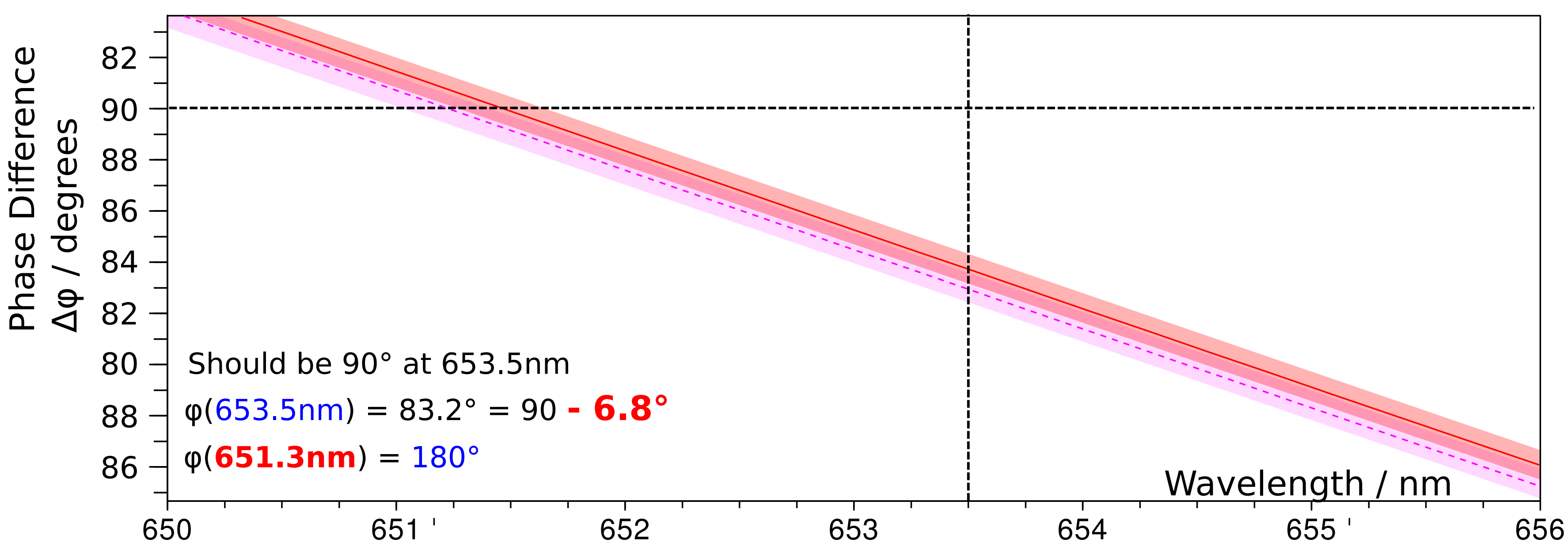
Waveplate Tests - Quarter Wave

Oliver Ford
IPP Greifswald
gmds/SPECLAB/400

For the λ/4 I took the spectrum with polarisers aligned (solid) and with them crossed (dashed):
As with λ/2, plate order is N=5.



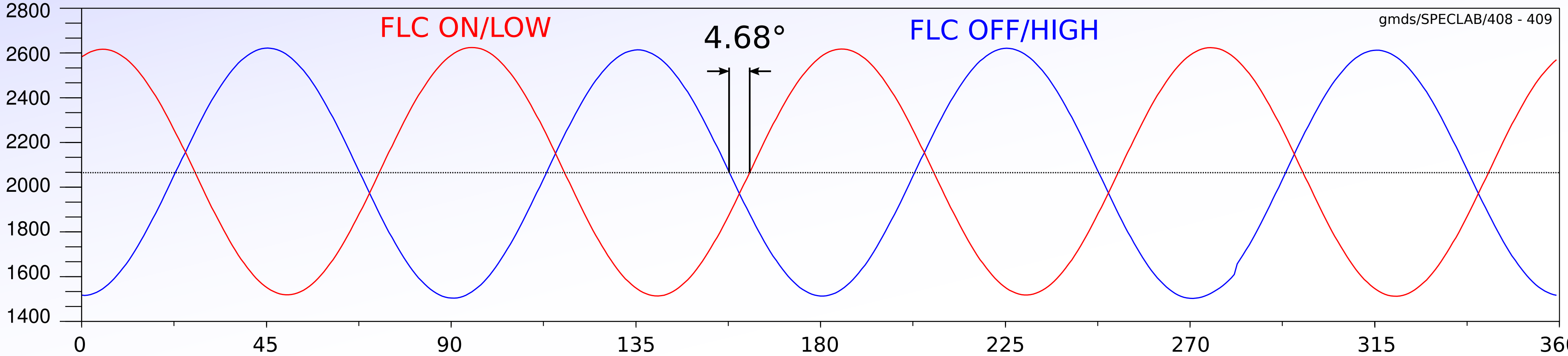
Fit phase variation over target spectral range:



Waveplate Tests - FLC

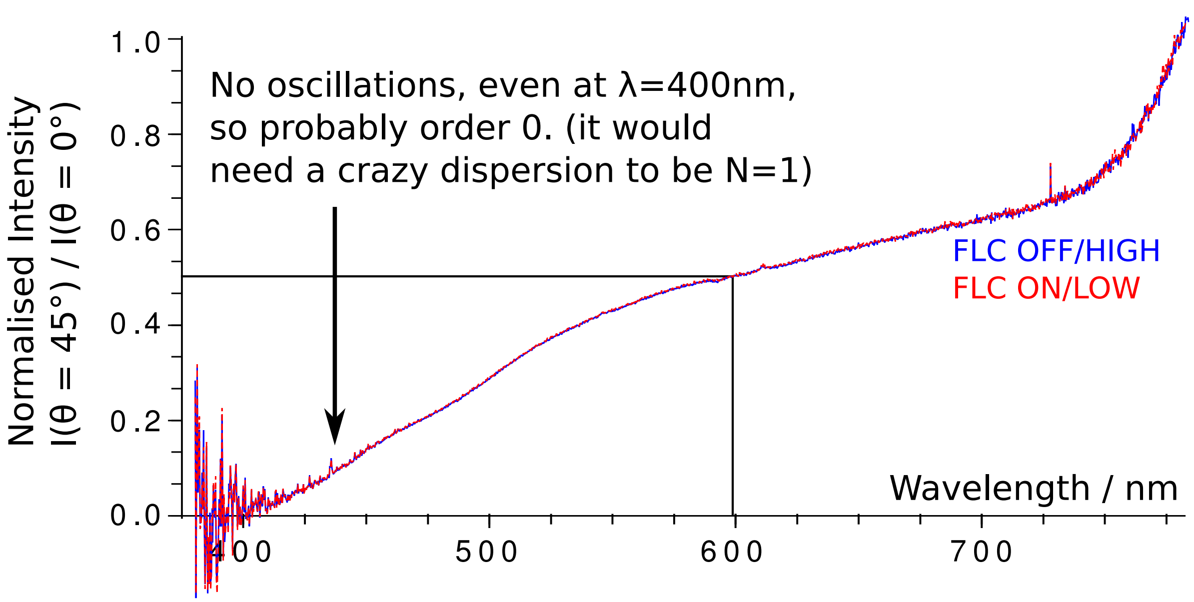
Oliver Ford
IPP Greifswald
gmds/SPECLAB/408,409

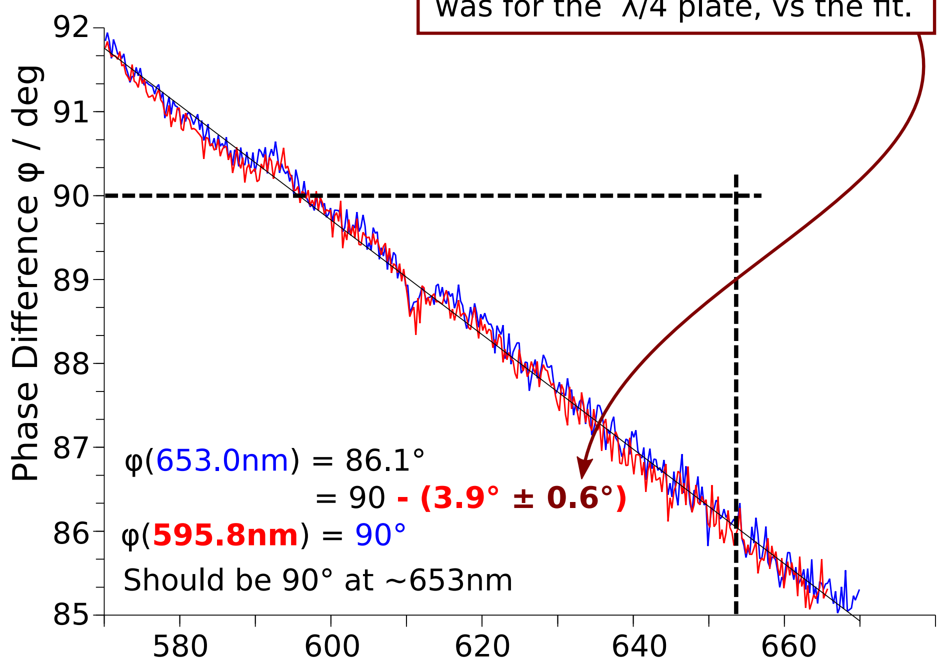
First, scan FLC between aligned polarisers to find axis in both ON/LOW and OFF/HIGH modes.



ON axis should be 45° from OFF, but is ***4.68°*** less (±0.05° from *sin θ* fits of avg image centre).
This is apparently fairly temperature sensitive.
Next, use fitted sine to average spectrum at all max/min (θ = 0° and θ = 45° respectively).
Plot spectrum, but can't fit it as I don't have the dispersion (don't know the material),
so have to trust the normalised reading = $\frac{1}{2} + \frac{1}{2}cos(\phi)$

Because I don't really trust the I(45°)/I(0°) method. This is how far out the same method was for the λ/4 plate, vs the fit.

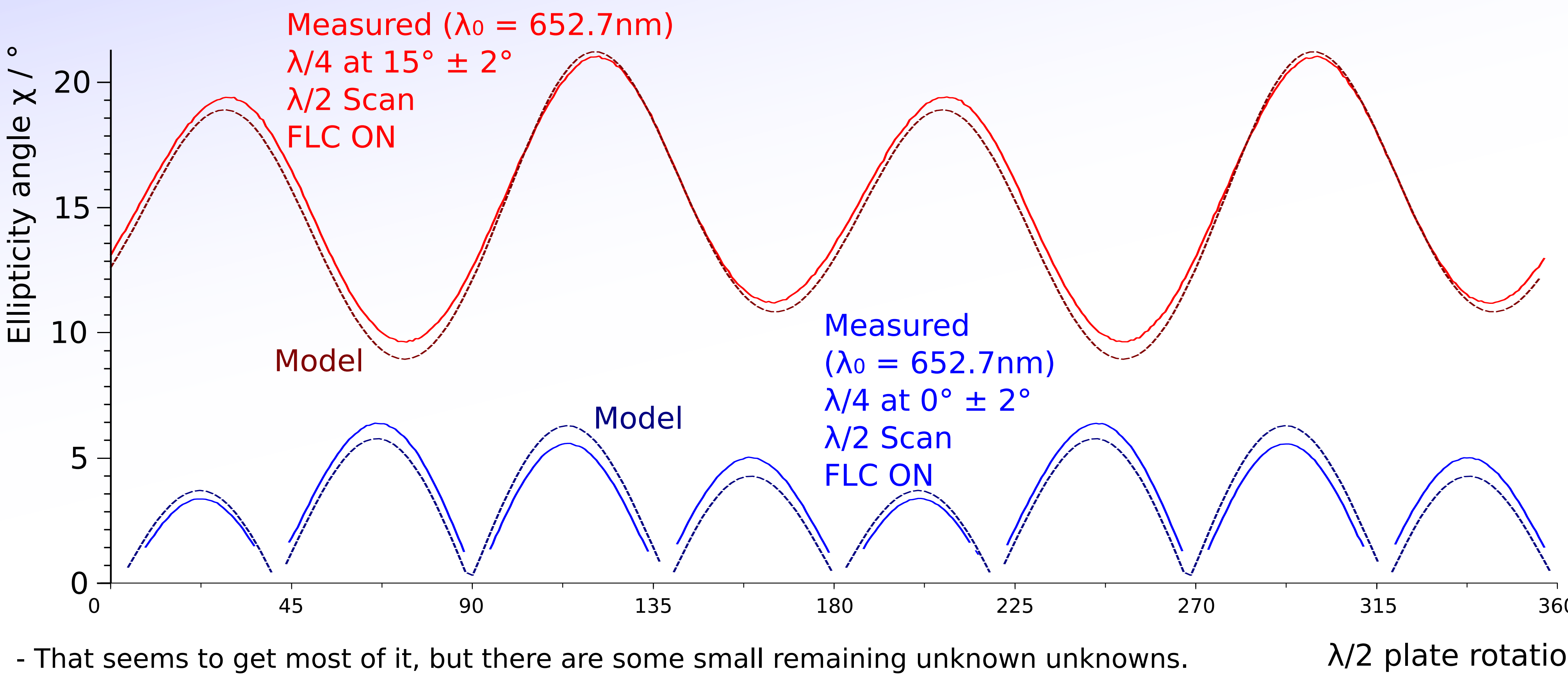




Effect on test setup

Oliver Ford
IPP Greifswald
IMSEProc/PlateGenSource vs gmds/SPECLAB/377,385

The full spectrum test setup had (π-, σ, π+) at (652.3, 652.7, 653.1nm) and the λ/2 plate before the FLC.
Simulating the λ/2, λ/4 and FLC measured phase shifts and offset angles:



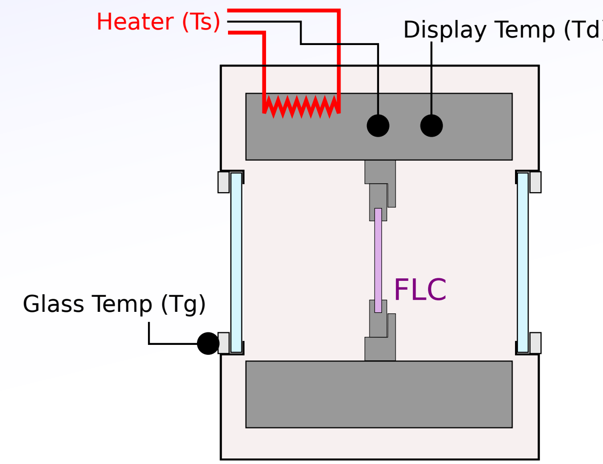
- That seems to get most of it, but there are some small remaining unknown unknowns.
- Phase offsets in all three of λ/2, λ/4 and FLC are a signficant concern.
- λ/2 and λ/4 do not need to be used in plasma measurement:
  - Should adjust the temp cell orientation rather than using the λ/2 - *change mech design!!*
  - Will need some true zero-order precise plates to get performance test down to 0.1° (and a pol. cube, to be sure).
- φ <> 90° effect can be eliminated from switched system, not sure about φ < 90° and Δθ <> 45° together, but that relies on temperature stability of FLC inaccuracy (will test this week).

- With small ellipticity (χ < 5°) and set at a strategic operating angle, the ADSH system works to 0.1°, but none of the PDSHs, even with interlace calibration work better than 1° so cross checks, single fringe measurements, and most importantly ellipticity measurements can not be performed.

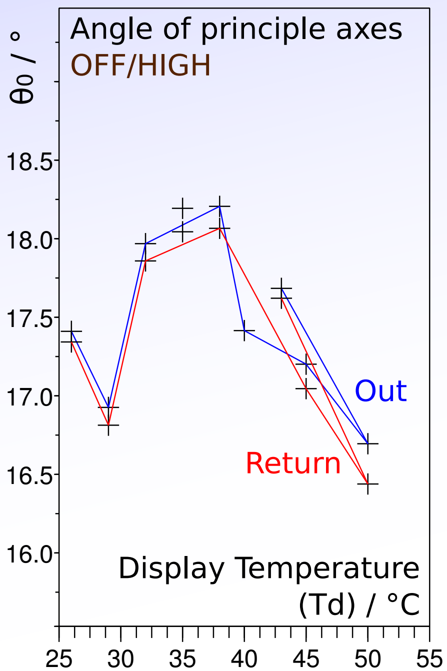
Waveplate Tests - Temperature Effect on FLC

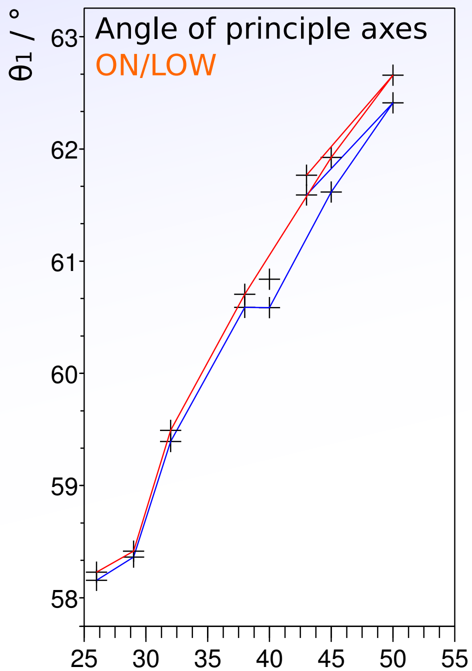
Oliver Ford
IPP Greifswald
gmds/SPECLAB/408,409

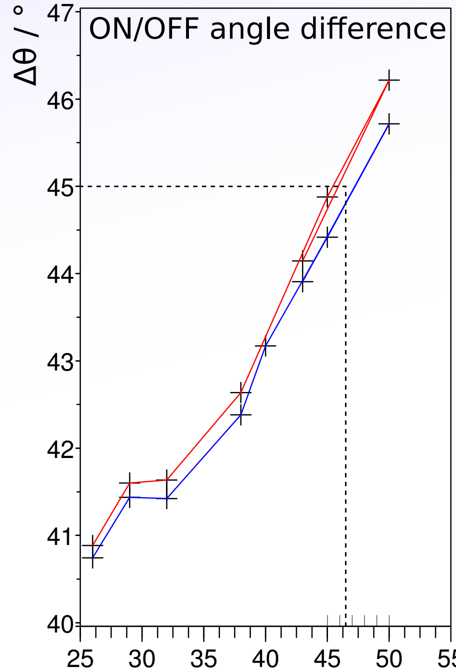
Loaded FLC into centre of temperature cell with windows.
Set a temperature, measured in block and on glass retrainer rings.



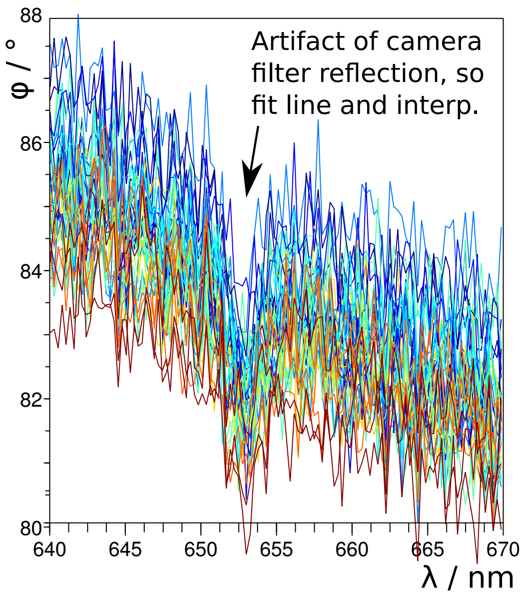
Tg ~ Td-3 and air temp inside ~(Tg+Td)/2

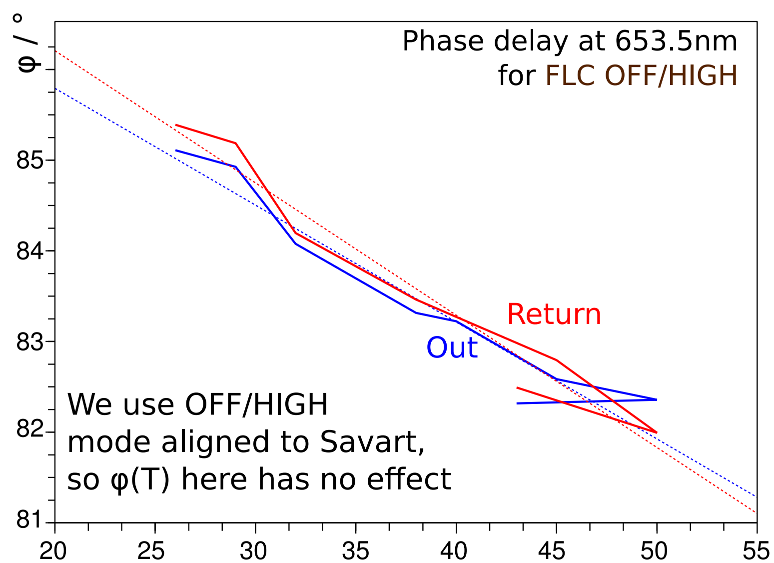


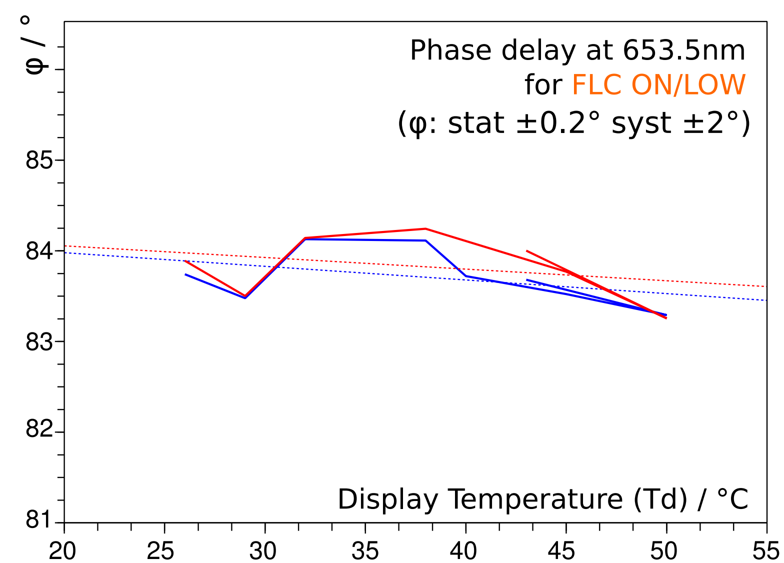




Temperature seems to effect only θ when on, and only φ when off. Running at 47°C gives 45°±0.5° switch and Δφ = 84°



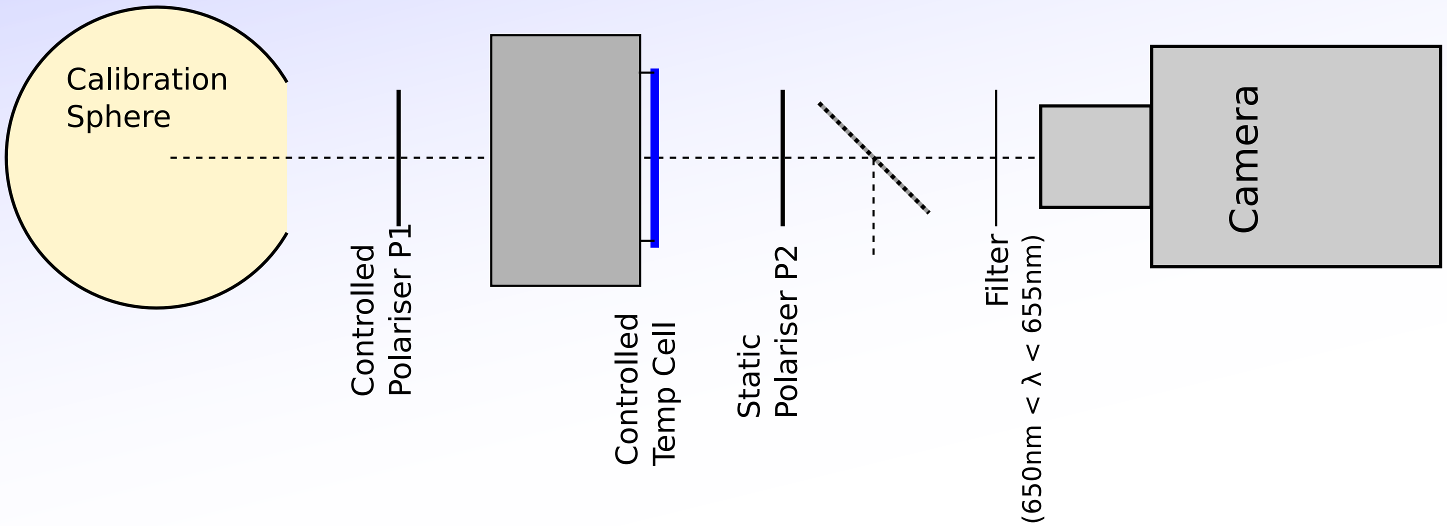




Alignment Check

Oliver Ford
IPP Greifswald
gmds/SPECLAB/408,409

Check alignment of plates relative to their casettes in proper setting in oven.



SPECLAB/481: Put P2 in cell with cell set to 0, spin P1 --> max at -12875
SPECLAB/482: Put P2 in static mount, spin P1 --> max at +2460
So oven needs to be moved by +15335 to get components aligned with polarisers.
SPECLAB/483: Check effect of windows on polarisation --> Hardly any.
SPECLAB/484-494: Spin oven, look for difference of max from +15335:

Reference done with pol face to source, so eveything is ref to that.

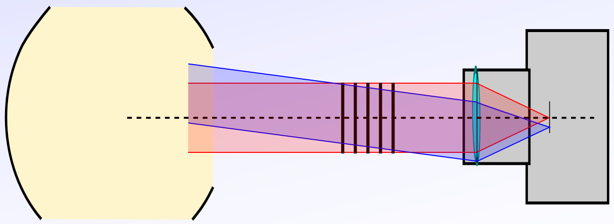
Component	Text facing source	Text facing camera	Set Direction	Error from polariser
Polariser	-0.09°	-0.37°	F-S	N/A
Savart	+0.43°	-0.47°	F-S	+0.45
Displacer	-1.41°	+1.24°	F-C	+1.33
Delay	+0.56°	-0.58°	F-S	+0.57
FLC	-0.11°	+0.02°	F-C	+0.07

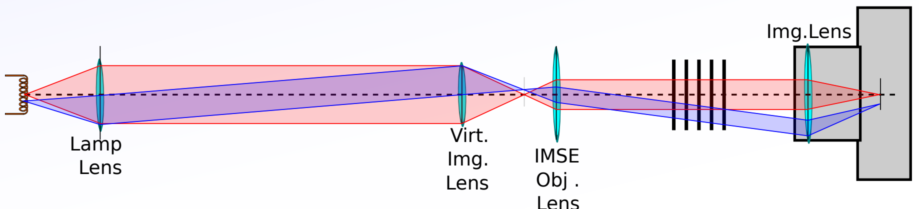
Have re-aligned these since

Intrinsic Contrast (a.k.a 'the magic number')

Oliver Ford
IPP Greifswald
gmds/SPECLAB/408,409

This was originally from trying to figure out what caused a change in the single scalar value correction factor required when lighting the ADSH system with different sources. The *cos(±y)* components of the FFT seemed to be reduced by a factor positively related to the amount of surface area of the Savart plate used:





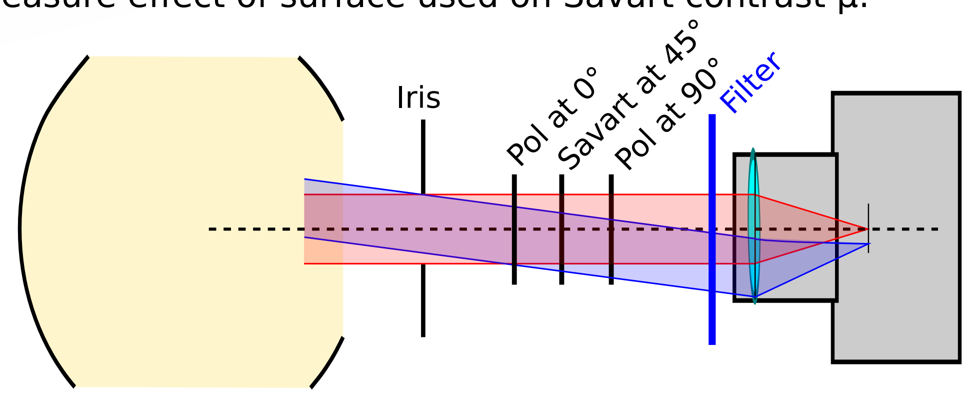
Diagrams are not to scale and are completely made up.

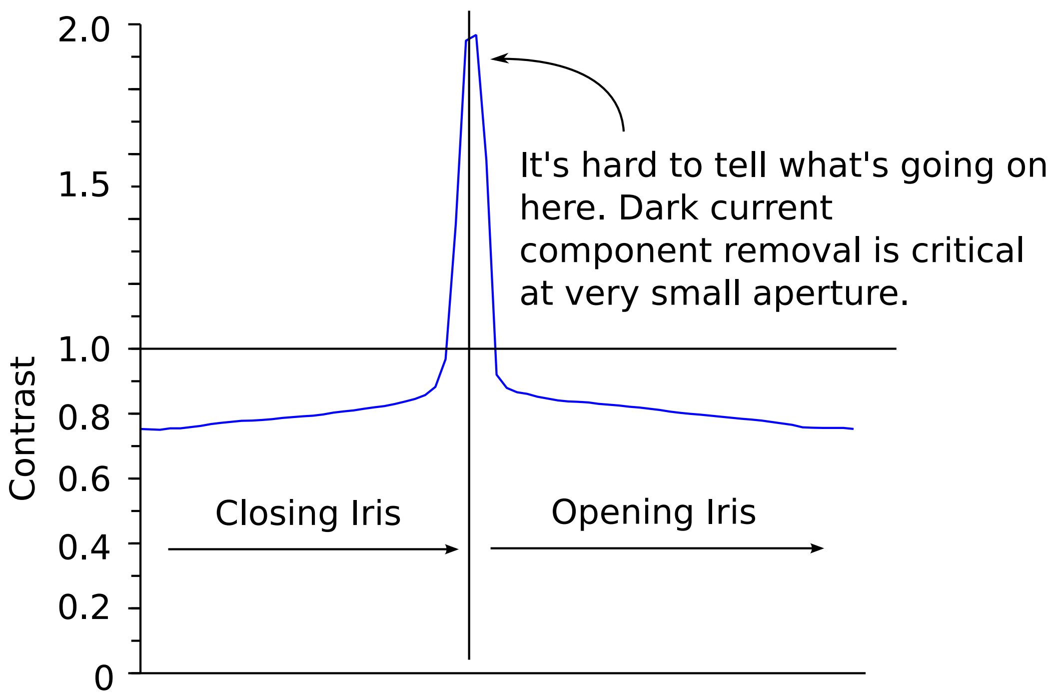
The ADSH system is:

$$I \propto 1 + \zeta\, cos2\theta\, cos(x) + \zeta\, sin2\theta\, sin(x) sin(y)$$

If the Savart plate has some 'intrinsic' reduction in contrast - my magic number μ:
sin(y) becomes *μ.sin(y)* and *μ* only appears in the (+,+) and (+,-) components, instead of *tan2θ*, we now measure *μ.tan2θ*

Measure effect of surface used on Savart contrast μ:





So what causes the 'intrinsic contrast'??

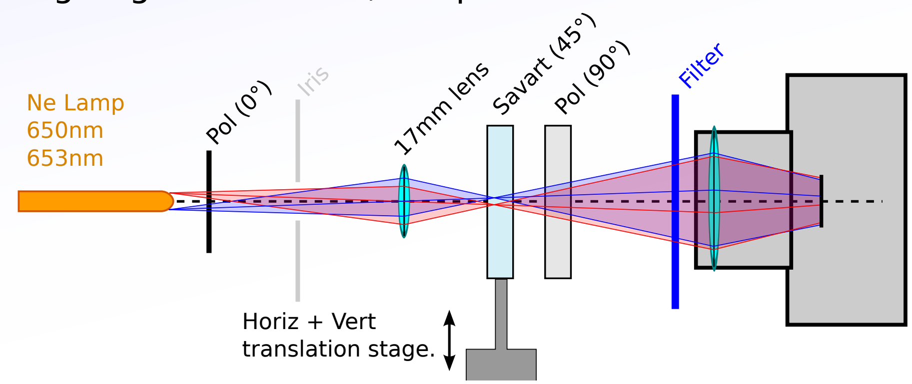
BTW: Only the Savart matters. Intrinsic contrast in the displacer and delay plates appear in all 3 FT components so are lost in the calculation.

Intrinsic Contrast - Surface Quality

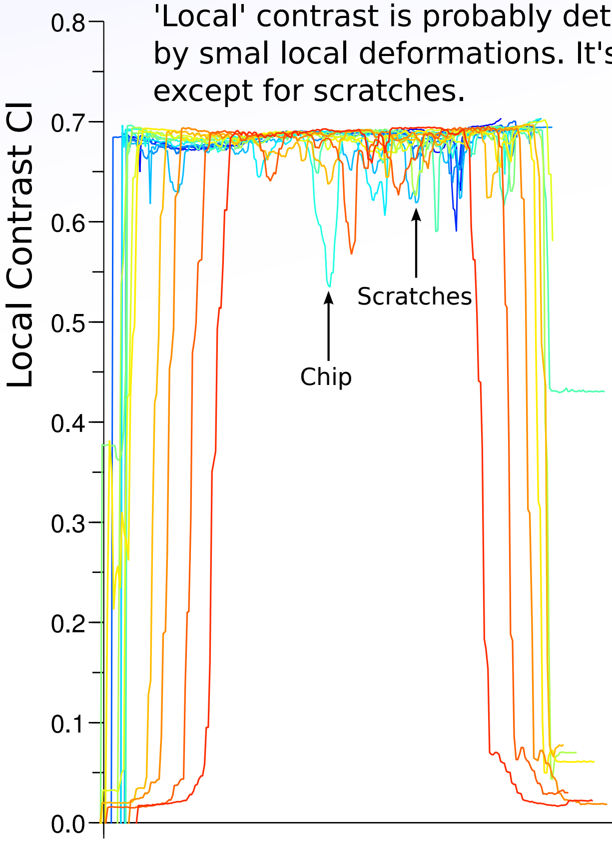
Oliver Ford
IPP Greifswald
gmds/SPECLAB/408,409

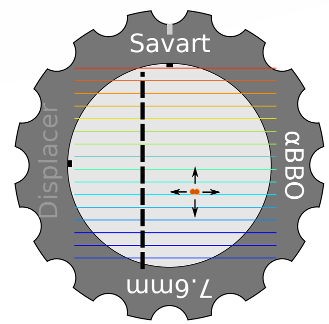
Hypothesise that contrast comes from variations in the plate thickness.
This would need to be < 180° slow variations in phase, for the same angle, across surface of the plate.
Fast variations would average quickly and stay averages. For anything larger than 360°, the plate wouldn't work at all.

Focus the image of the Ne calibration lamp end onto a vary small (< 1mm) area of Savart front surface with enough angular variation to light most of CCD image (CCD image is focused at infinity, not on lamp). Then translate plate and measure both contrast and phase of image centre, as a function of surface.



'Local' contrast is probably determined by smal local deformations. It's very constant except for scratches.





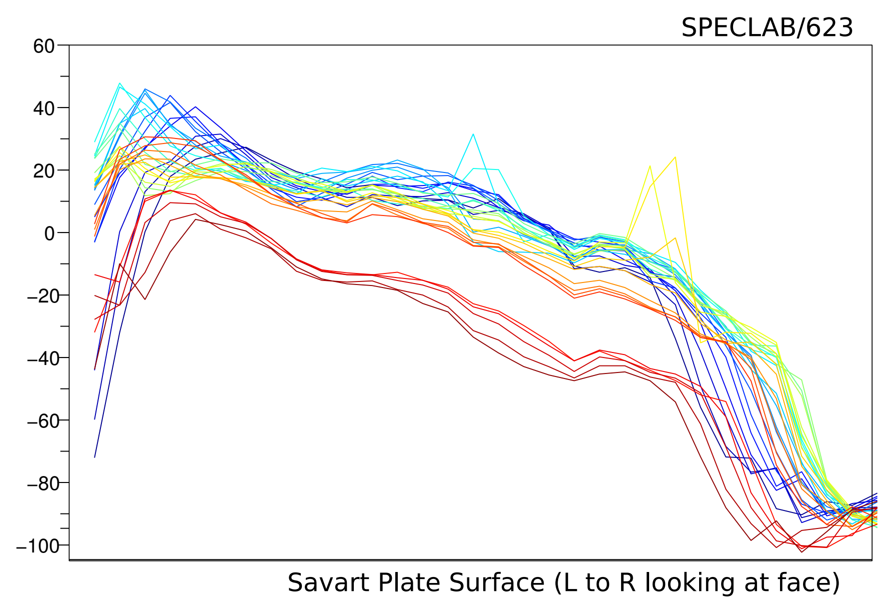
Scratches are >> 5μm deep so are completely randomising and don't affect local phase average much. They do affect overall contrast but very little since fraction of surface area is small.

Phase has a continuous slope and also falls off rapidly at top edge of surface. Max variation is ~50° and averaging over 50° of phase variations does not reduce contrast by more than ~10%, so this *can* explain the μ~90% of the better cases, but not the 56% of the calib sphere.

Coupled with 70% local constrast, this is 63% - closer but not enough.

The 70% local contrast is odd - as it's lower than the 90% that's the best I've seen - there must be another dependence.

Conclusion: This isn't the major problem.



Intrinsic Contrast - Focus

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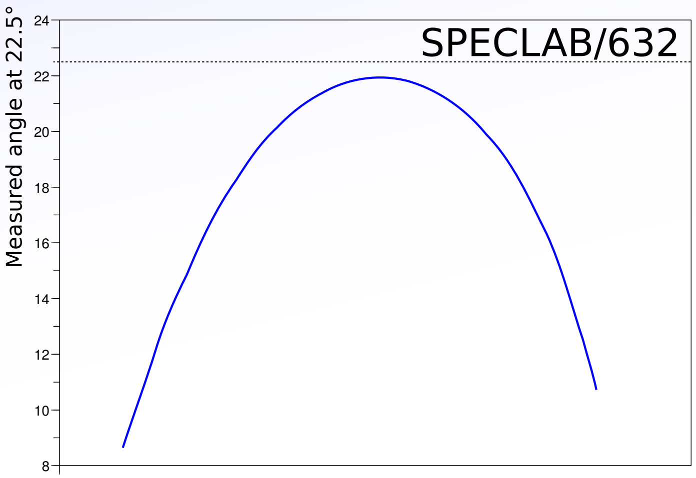
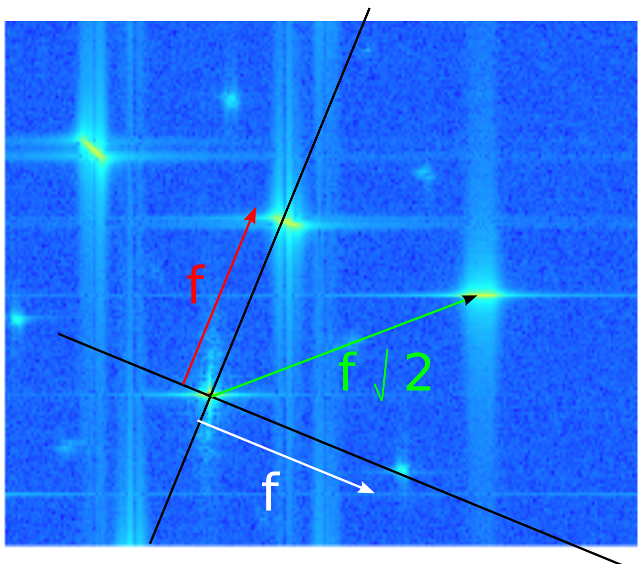
For some reason, μ is a *very* strong function of the camera imaging lens focus.

The experiments up to now have only been roughly (by eye on fringes) focused. Some (inc savart surface) were focused by maximising the fringe contrast. Unfortunately, the contrast vs focus ring curve is multimodal, so some experimetns were very far out (the μ = 56% ones). So, we can't trust the local contrast etc results up to this point.

SPECLAB/632: With wide open apertures and polariser set to 22.5°, scan focus ring (stepper motor linear stage wedged against focus ring with rubber):





So this explains the very low μ=56% etc and the variation with input light cone, since the focus ability of the lens changes with input F number (i.e. depth of field varies with aperture).

But why does focus effect result?
Focus will decrease contrast but shouldn't it be the same for both sets of fringes, since they're the same frequency?

Well, no, the (+,+) and (+,-) components are √2 higher in frequency than the (+,0) component, so are accordingly reduced in contrast. This is one reason to avoid very high frequencies, as the effect is unexpectedly severe.

Solutions / mitigation:

1) Stabilised imaging lens mount, adapter and added screw to lock focus ring - this must be *highly* stable against viabration.
2) Optimise imaging lens focus against (+,+) components and lock.

Focus varies directly with image position and may also via input light variations over the image plane (vignetting). So, unavoidably now:

4) The system requires a caibration image for the target light input (beam) for a known polarisation, preferably 22.5°.

Objecttive Lens

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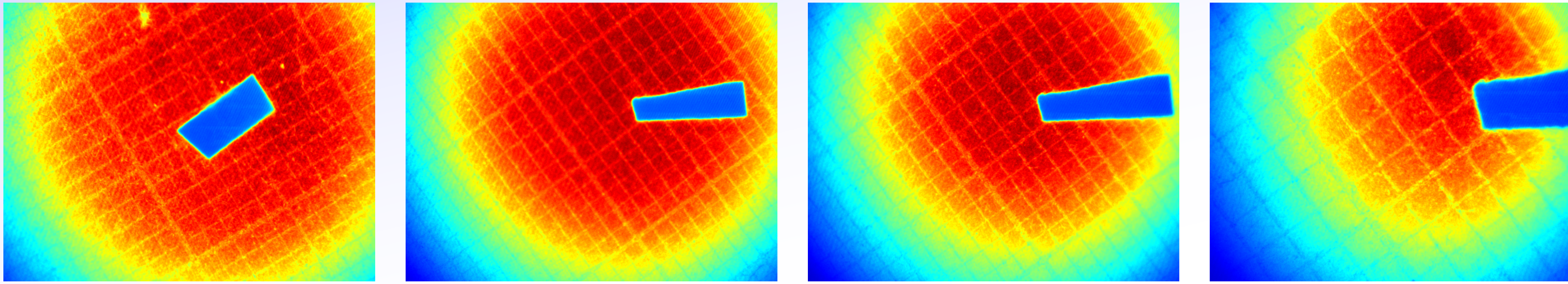
The original plan was the 135/2.8 but there's also a variable 70-160/3.5.

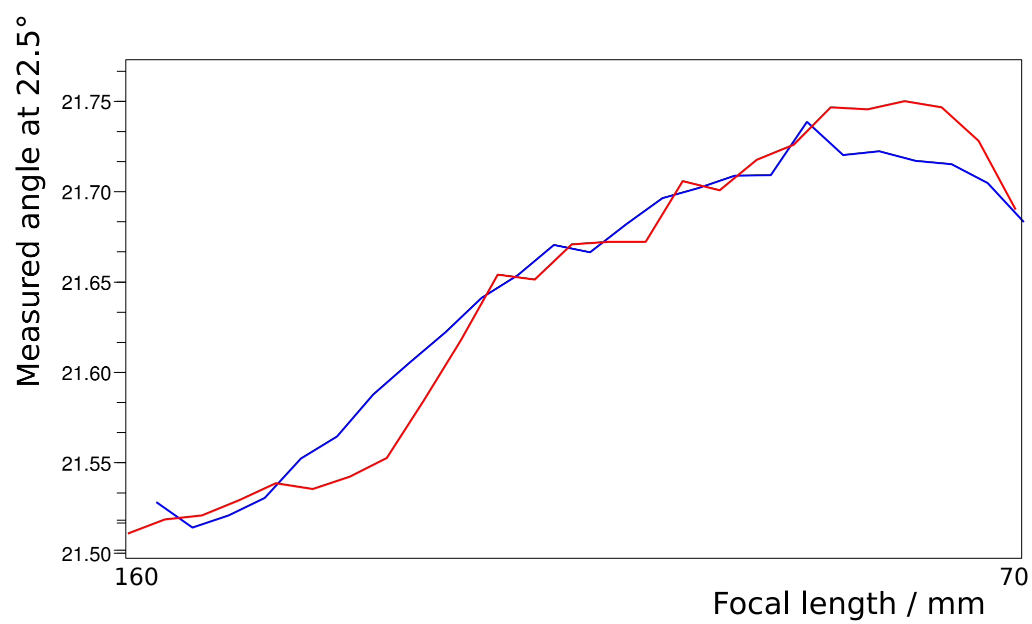


Lens	Full clear View	Relative Peak Intensity
135/2.8	21mm	100%
70-160/3.5 @ 70	11mm	50%
70-160/3.5 @ 135	20mm	70%
70-160/3.5 @ 160	24mm	75%

It loses up to 30% for the full image area but gives some flexibility. The fixed 135/2.8 doesn't quite cover the expected virt. image area of 23mm whereas the variable 70-160 covers it at ~150.

It also allows us to zoom in on e.g. the core or pedestal etc. However, since the lens is now the most restrictive component to the input light, it's focus, aperture and focal length might change the calibration.

It does, but only by ~0.2° - a lot less than I'd expect. Moving the image doesn't have a noticable effect either.



Temperature Effect (full system)

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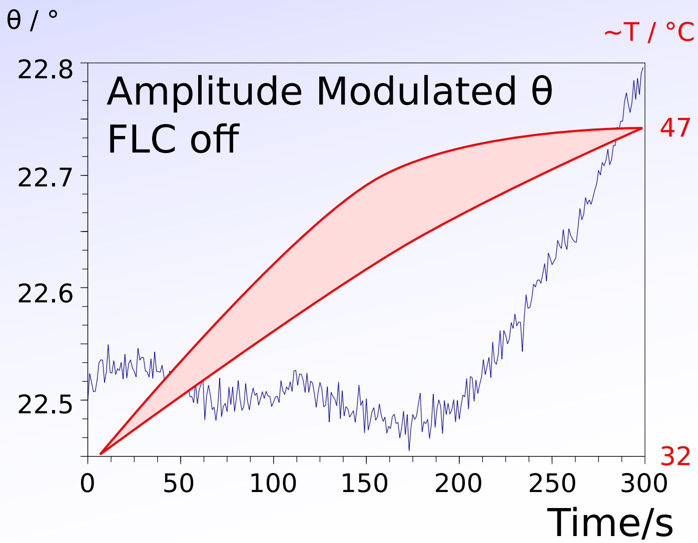
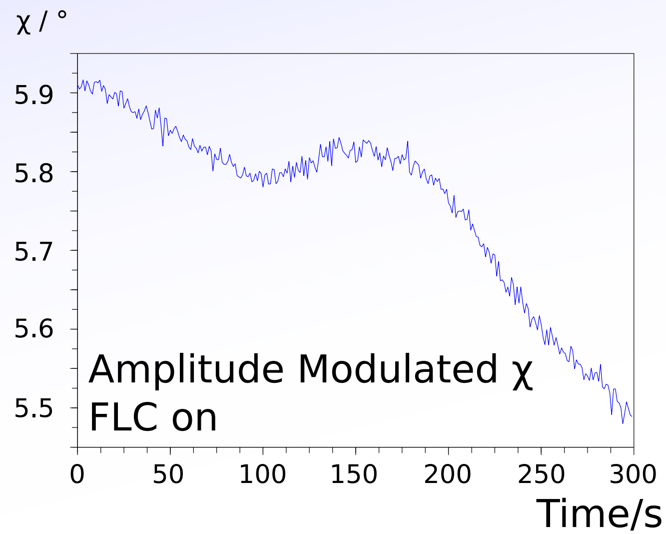
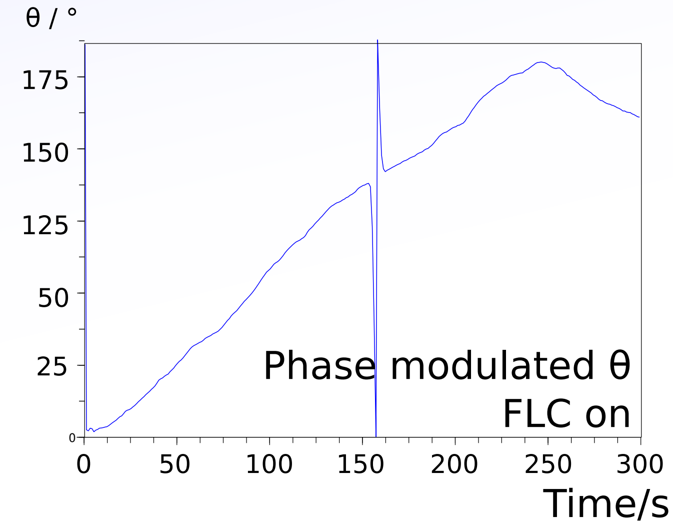
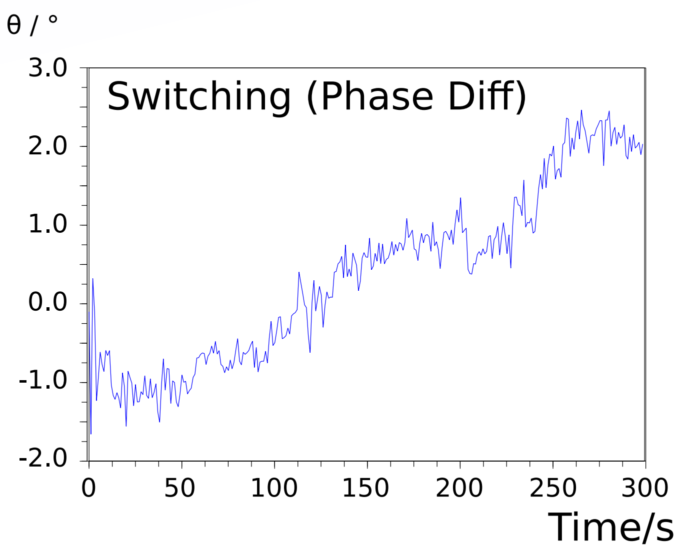
Temperature variation of various measurement types:









ADSH varies by ~0.8° in response to ΔT of 15K, so ~ 0.05°/K. Stabilisation in ±1°C is easily achievable and will give required < ±0.1°. Ampltiude derived χ response is similar.

Phase measurement is far worse: ~10°/K. This could be due to temperature dependence of FLC axis, or direct dependence of Savart, displacer and delay plate phases. It would require stability to 0.01°C for required accuracy.

Phase difference (switching) system is ~0.2°/K which is on the limit of acceptable.

Spectrum Dependence

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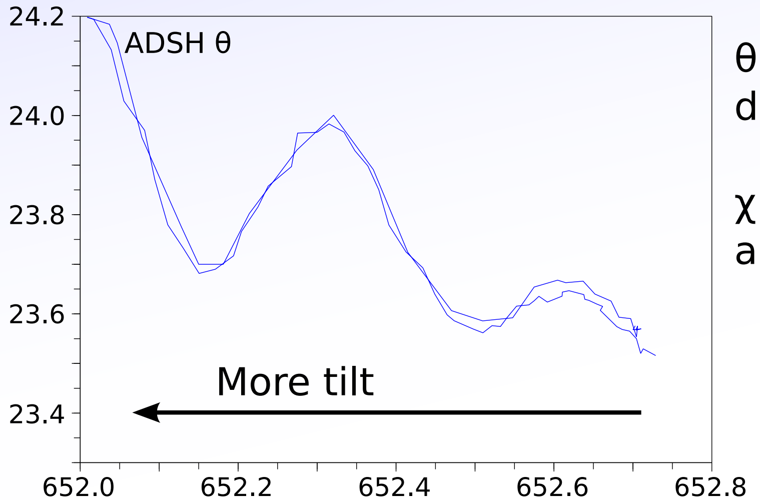
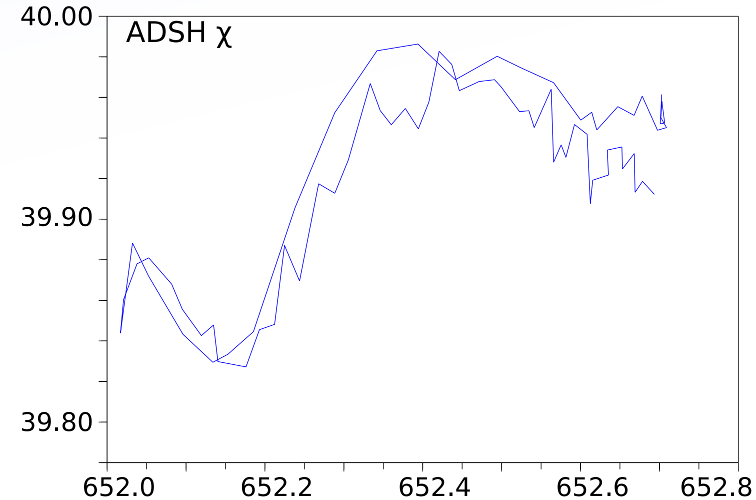
This is the most important for the system, since it is expected to change within the shot, and is difficult to calibrate offline with the correct spectrum - the details of which are unknown.

Varying the tilt of the MSE filters used to simulate the peaks (and back again):
X-axis is recorded from spectrometer which averages to a single (but unknown) angle.



θ response is stronger than desired at large tilt.

χ response is almost 0, but χ is anyway 5.15° and should be 0°.



This however could be due to some of the component shifting outside the range of the imaging filter on the camera. This shouldn't happen because the second filter angle is tied almost exactly to the image position, and the angle through the first filter should be the same - although the experimental setup doesn't completely gaurentee the infinity focus of the simulation system.

If it is this - then it shouldn't effect the plasma measurement, since changes in the spectrum here shouldn't relate to changes in the emission cone, beam volume etc.
In fact, I can't think of a reason that the emission cone of the plasma should change.

However - it might be a result of the frequency variation over image.

At this point, the best plan I can think of for January is this:
- Do at least one shot withouth calib to get the approximate angle of the plasma σ/π

Ray Tracing

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It's now vital to know the properties of the input light field for different scenarios. Of particular interest is the difference between light from the beam and from the back wall (calibration lamps) since the calibration lamps could be used as a calibration/check of μ for 3 image positions (offline, no faraday rotation etc).

Picked calib lamp position using photos and 3D model, I think it's roughly -2.395m, -0.254m, 0.586m

Autofocus says:

Beam centre: Spot size at best focus ~ 140μm = 14pixels = 2.3 finges (this won't be this bad)
Calib lamp: Best focus is 1.7mm nearer to L4. Spot size at beam's best focus ~500μm = 50 pixels = 8 fringes.

Assuming the 140μm will actually be better, this could be as low as ~4 fringes, so isn't enough.

Will need to defocus the obj lens to use calib lamps.
This will no doubt change the input light cone, and hence the calib image.

Focusing the obj lens on the beam can be achieved by focusing on the calib lamp and then pulling back by ~1.7mm - whatever that is on it's focus ring.

Might want to make up some kind of automatic focus.

Interestingly, the big canon EF USM lens is the required 135mm, so we can possibly use the USM focus and apterture on that - There is code for the Arduino to do it.