Beam shape at DØ

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- Method of analysis
- General results
- Beam width after the March mini-shutdown

Beam width measurement at DØ

The model being used is very simple:

Two beams with no X-Y coupling, same “optic” for p and pbar.

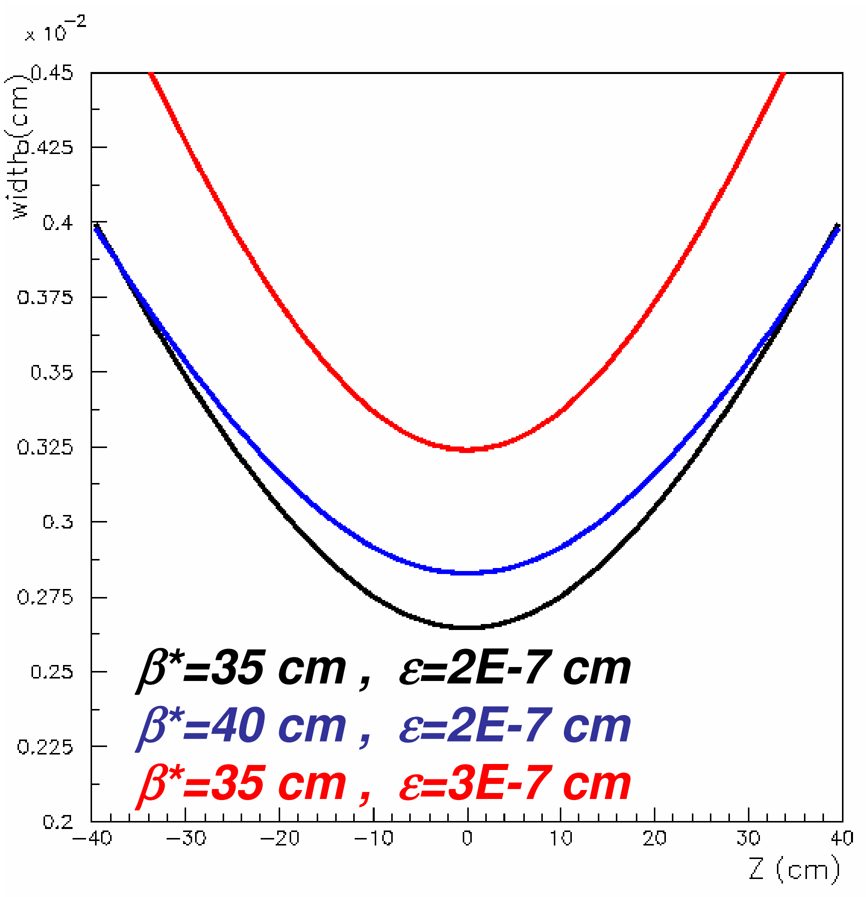
The interaction region is a drift at the Tevatron, so one expects (neglecting solenoid effects):

$$\sigma^2 = \varepsilon_{eff}\left[\beta^* + \frac{(z-z_0)^2}{\beta^*}\right]$$

$$\varepsilon_{eff} = \frac{\varepsilon_p \varepsilon_{pbar}}{\varepsilon_p + \varepsilon_{pbar}}$$

The beams division people expect

β*=35 cm.



measuring the shape of the luminous region at D0

vertex method

$$\sigma^2_{obs} = \sigma^2_{beam} + k \times \sigma^2_{vertex}$$

Uses:

•coordinates of the reconstructed vertexes

•estimated errors on this vertexes

Assumes:

•unbiased reconstructed vertex position

•error estimation proportional to the real error

pair of tracks method

$$d_i = y\cos(\varphi_i) - x\sin(\varphi_i)$$

$$\langle d_1 d_2 \rangle = \sigma_F^2 \cos(\varphi_1 - \varphi_2)$$

Uses:

•track parameters

Assumes:

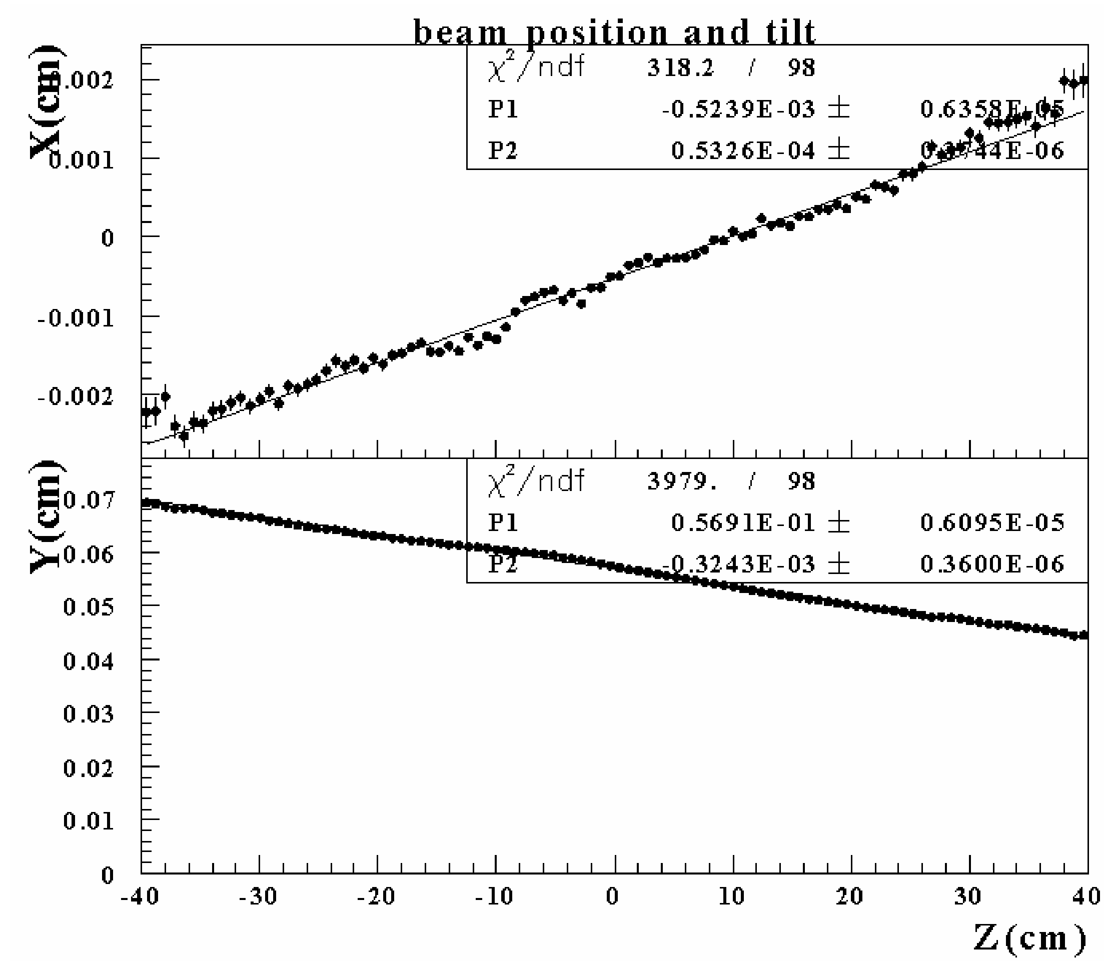
•unbiased track parameters

•uncorrelated errors in the track parameters

Here we assume circular beams, but in our calculation we do not make this assumption (formula a bit more complicated).

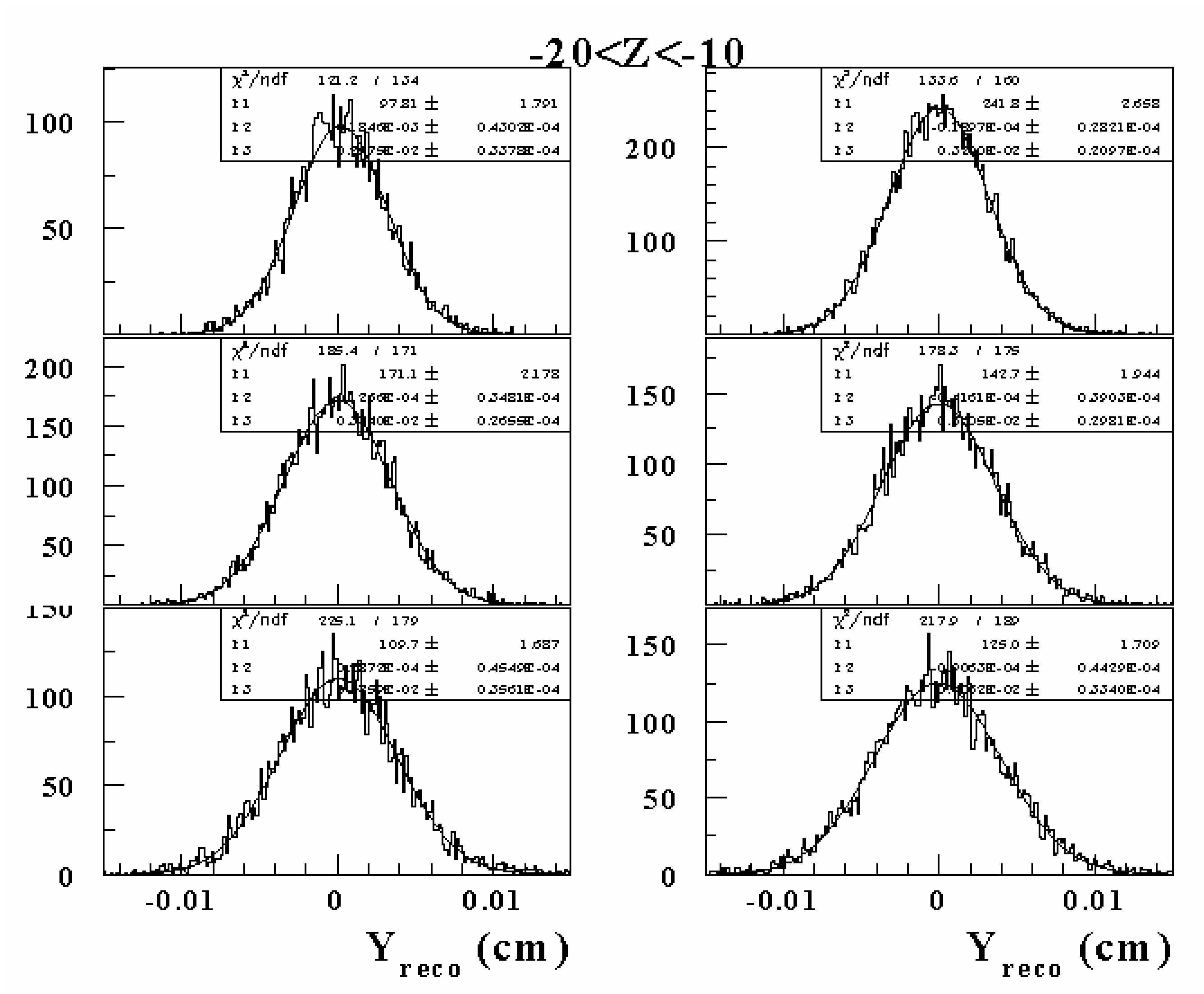
Vertex method. Step 1

Take one full run, and determine the beam tilt and position for X and Y independently.

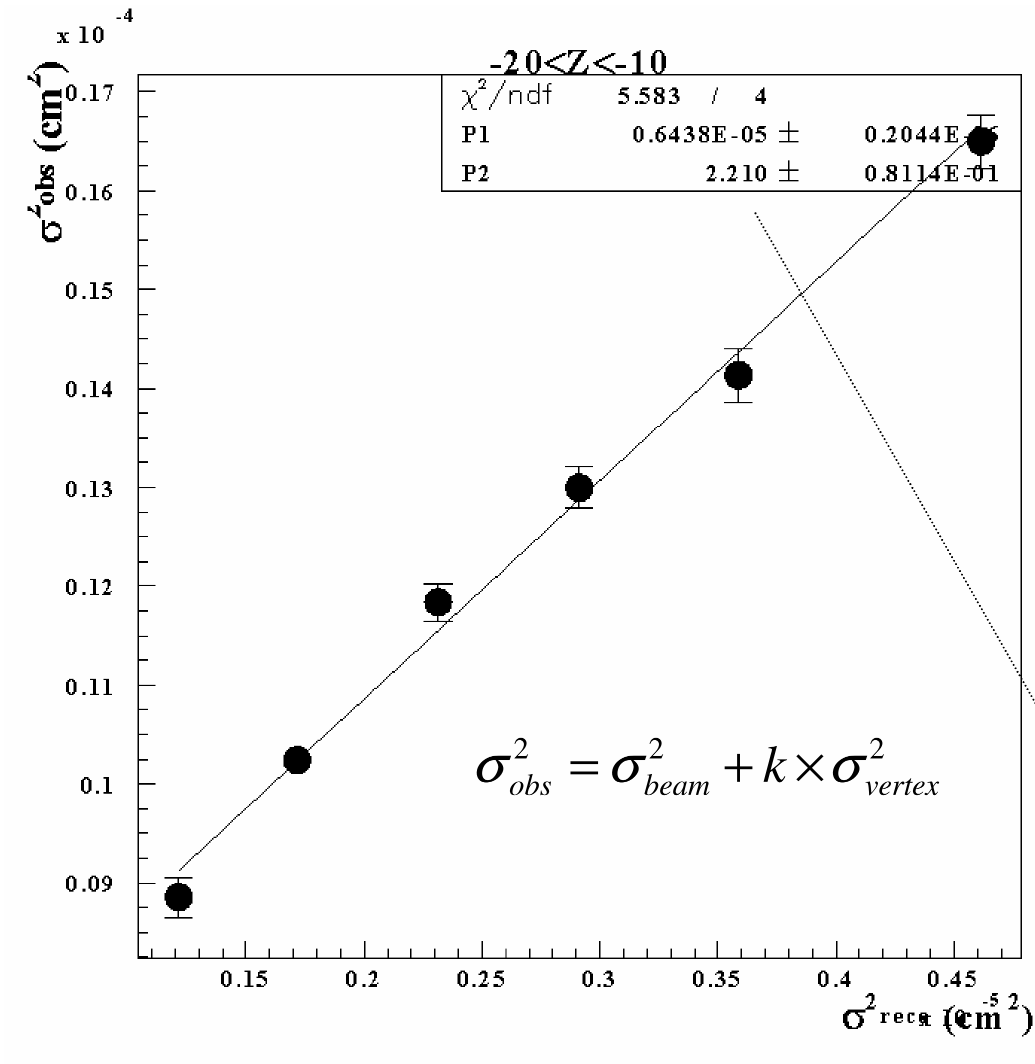


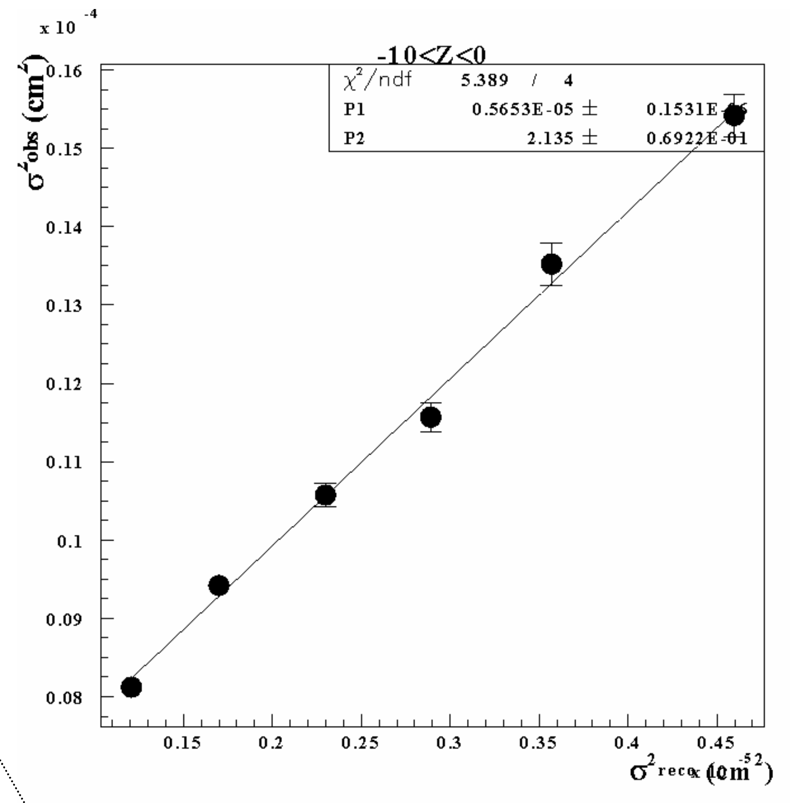
Vertex method. Step 2

For each Z beam (10 cm), separate the data in σ_{reco} bins and fit the width of the observed distribution.



Vertex method. Step 3

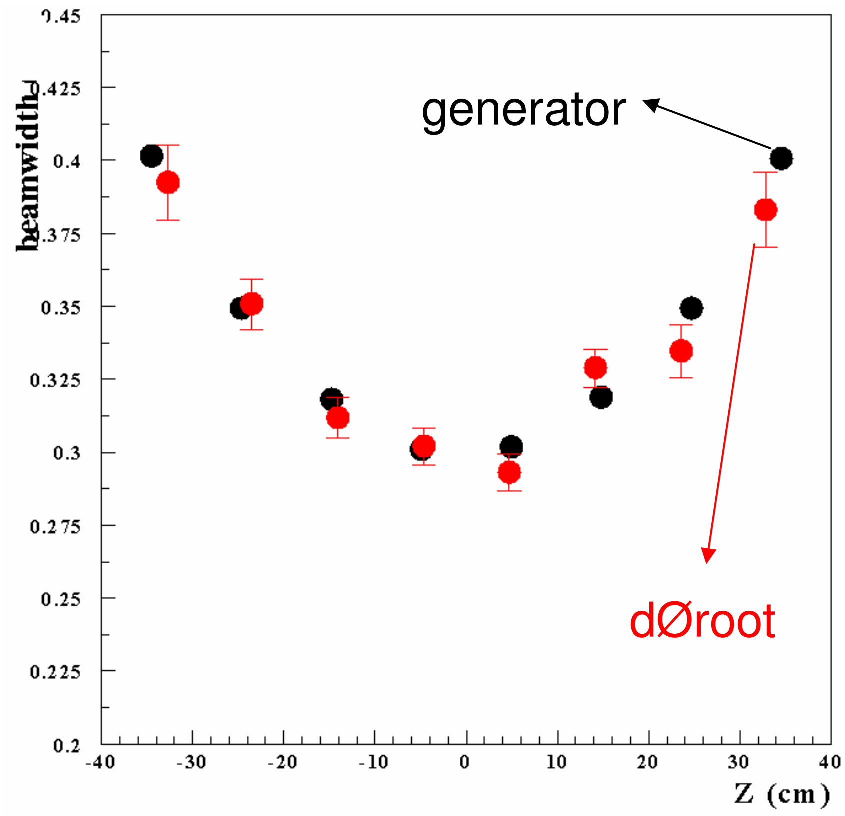




k=1 if you have a good estimator for the error in the vertex position.

fit the linear equations and determine k and σ_{beam}.

MC calibration

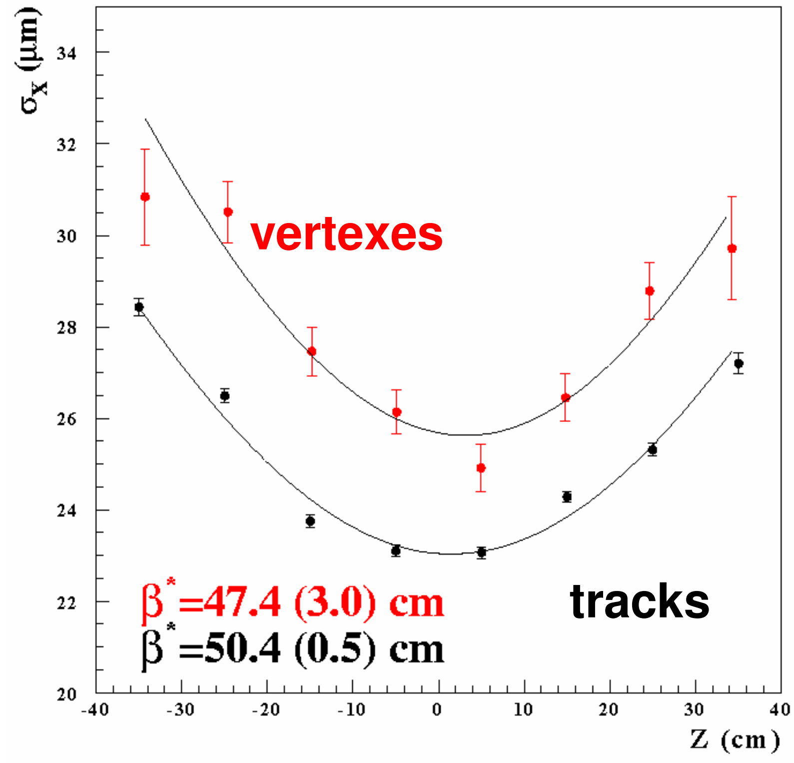


MC test of the beam width measurement.

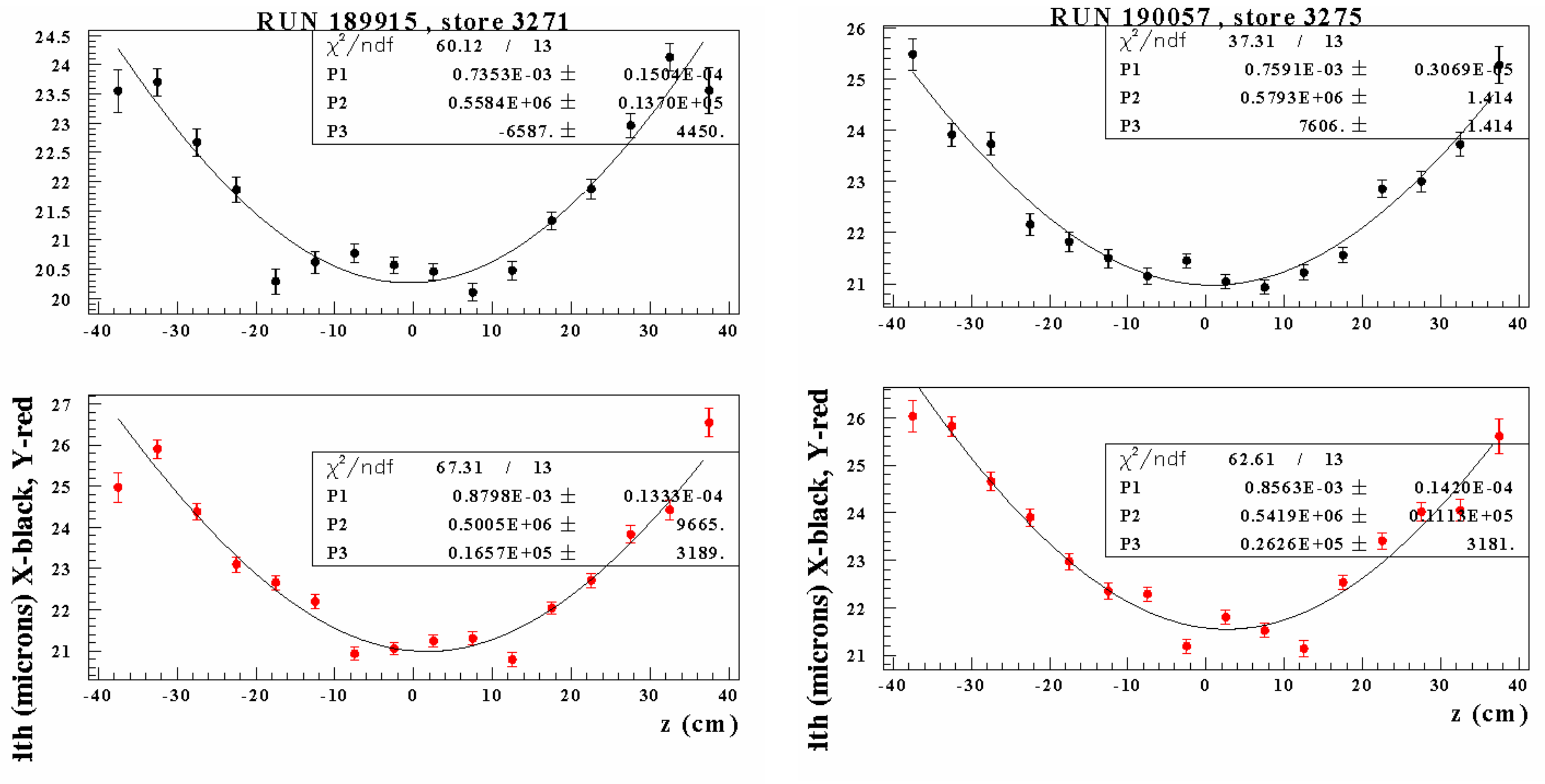
β^0 measurement: systematics

Evaluation of the systematic errors comparing our two measurements.

The different method give slightly different results, but this uncertainty can not explain the difference between 35 and 50 cm in β^*.

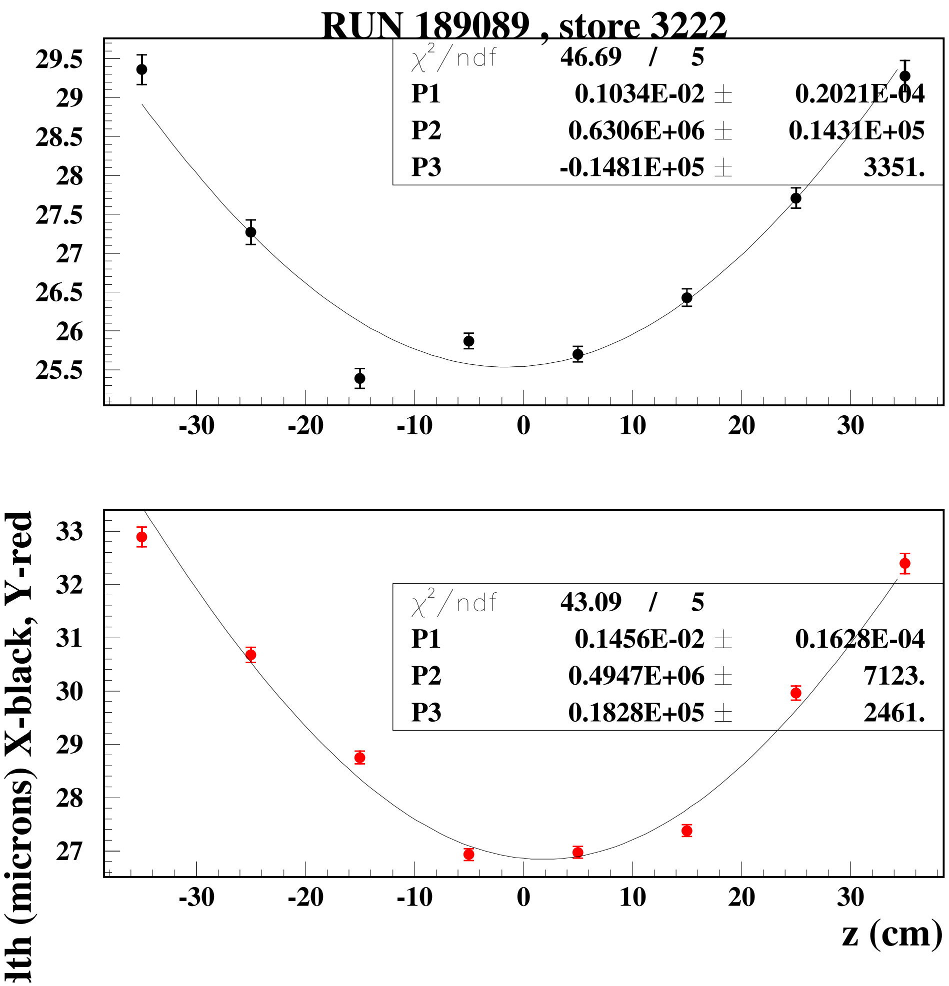


Some results

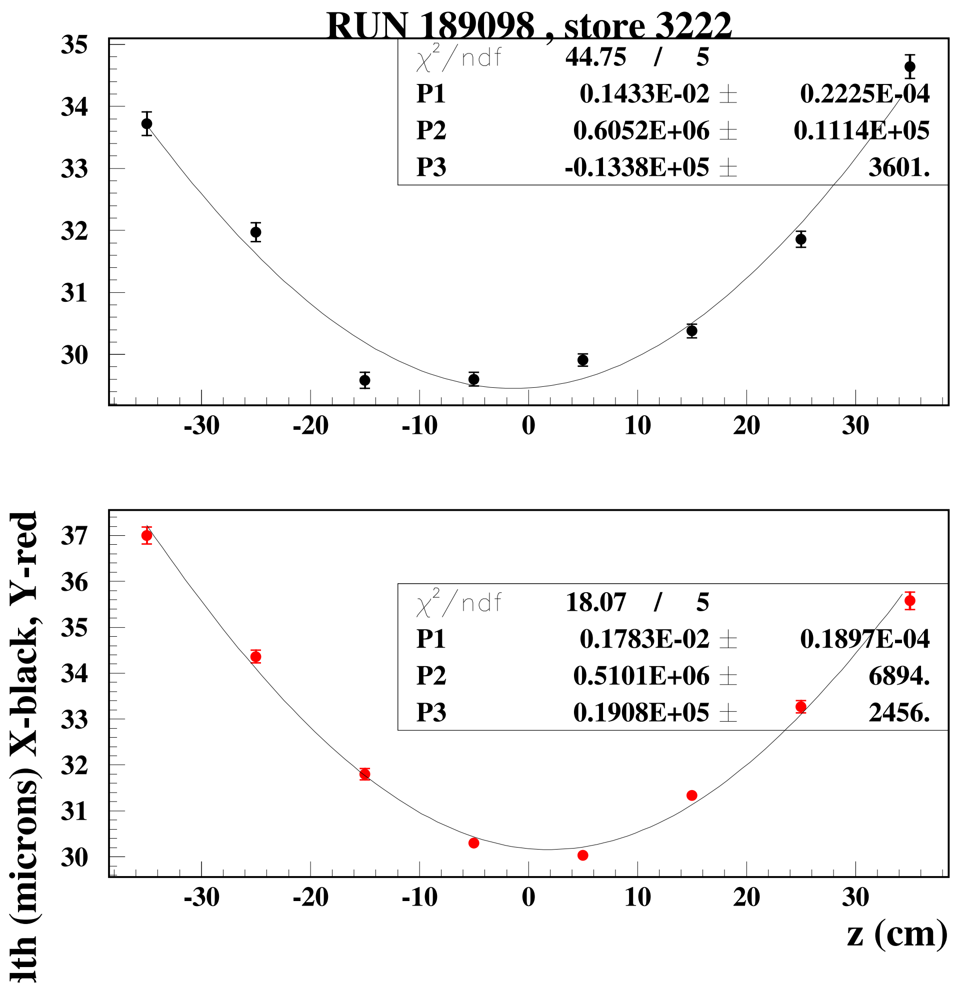


Beam change during the store (1)

7-FEB-04, ~0 hrs into the store

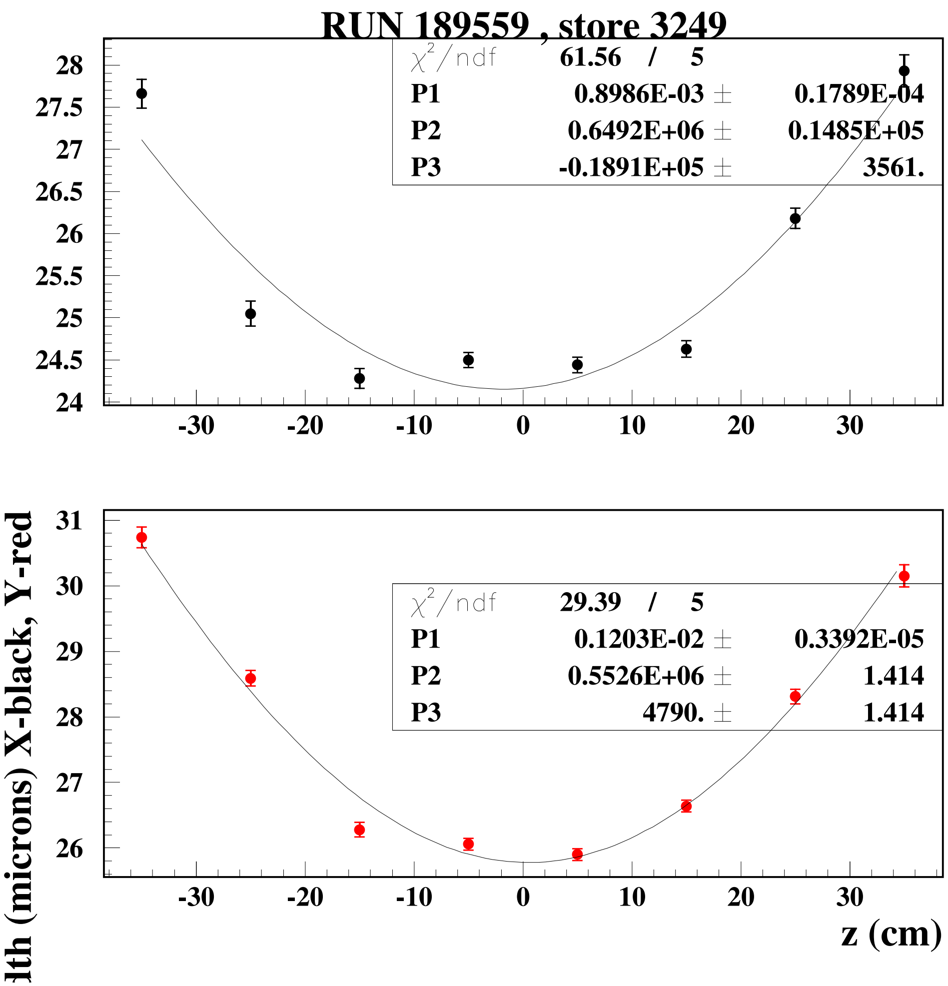


8-FEB-04, ~27 hrs into the store

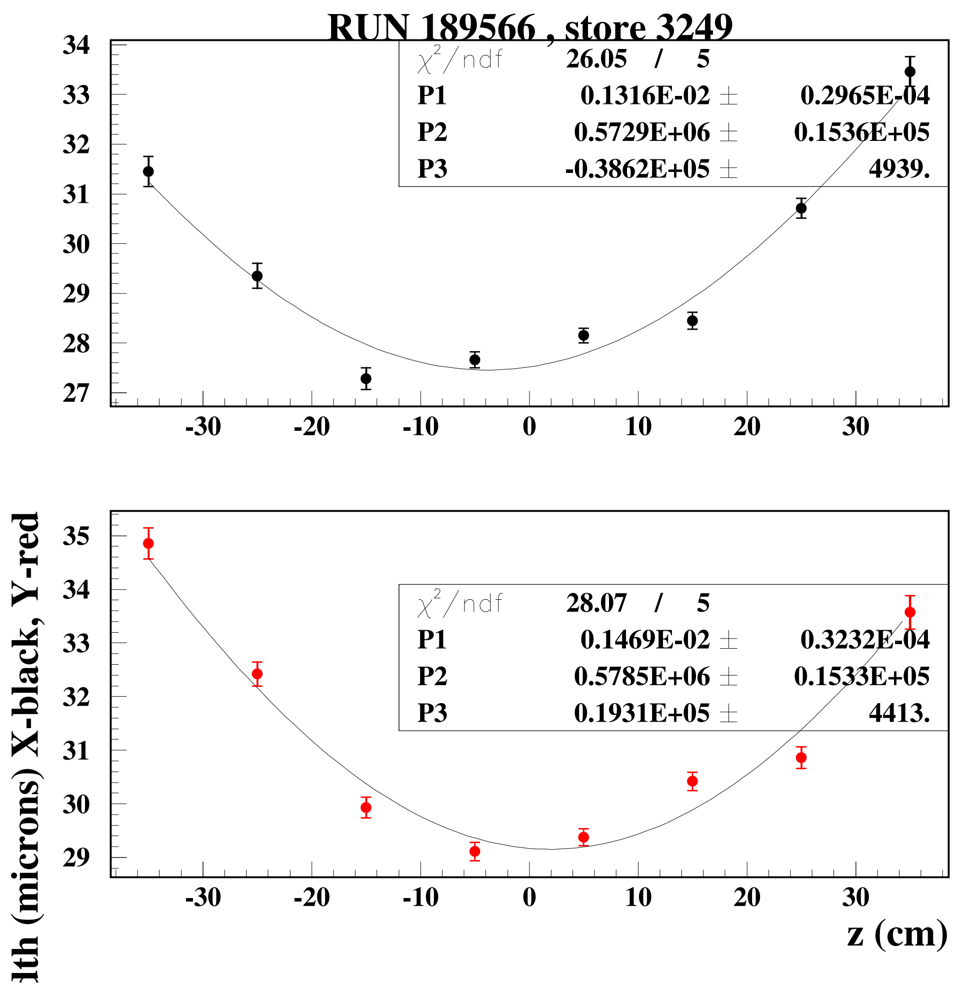


Beam change during the store (2)

21-FEB-04, ~3 hrs into the store

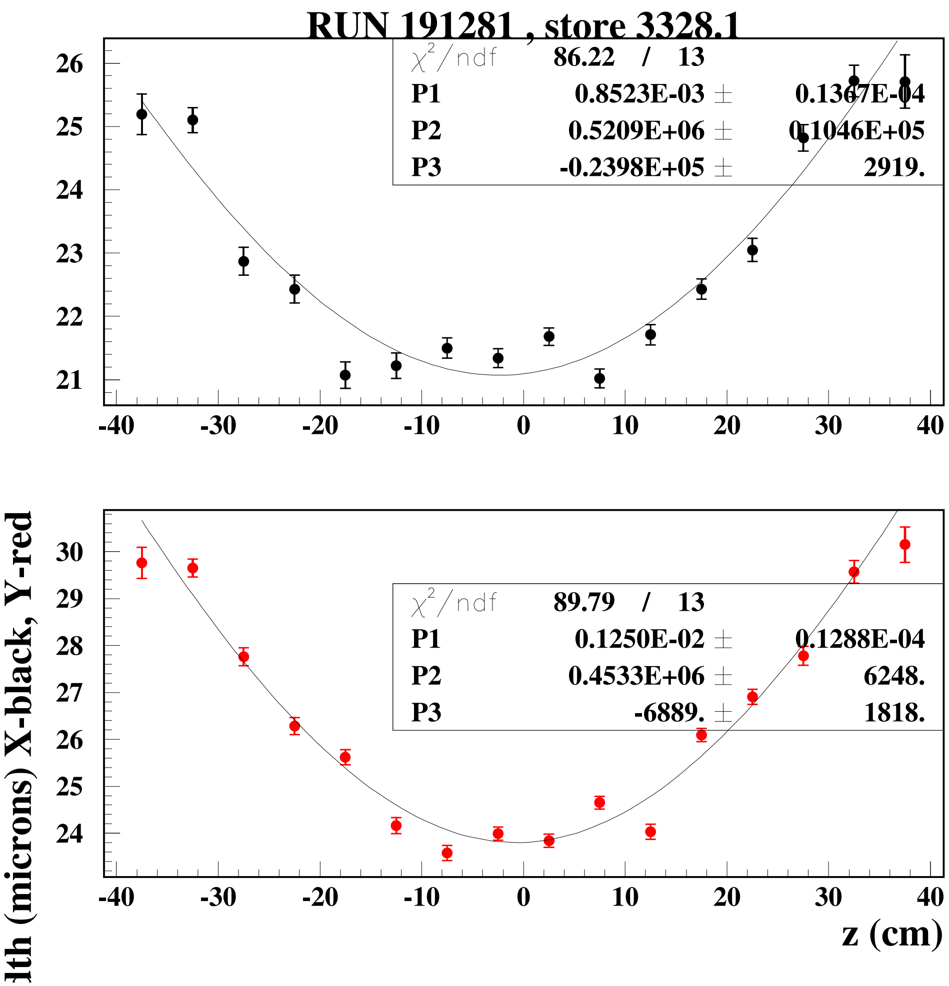


21-FEB-04, ~24 hrs into the store

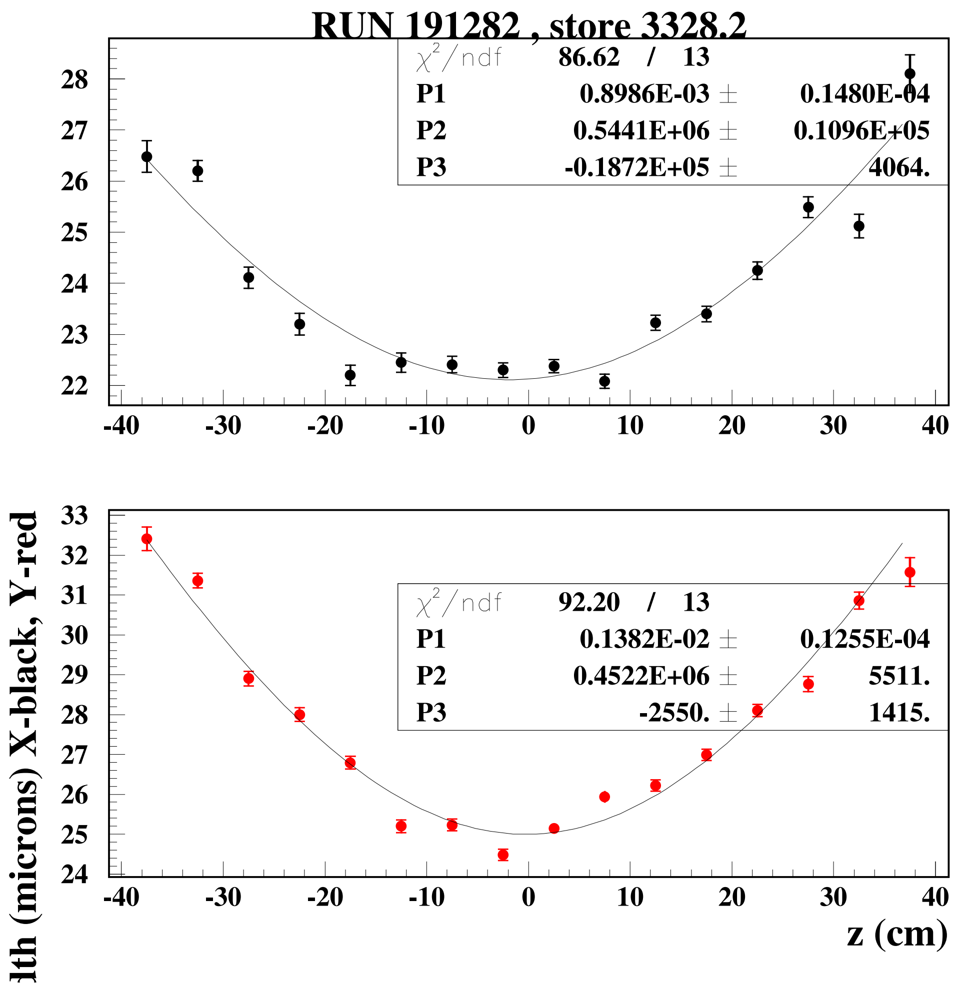


Beam change during the store (3)

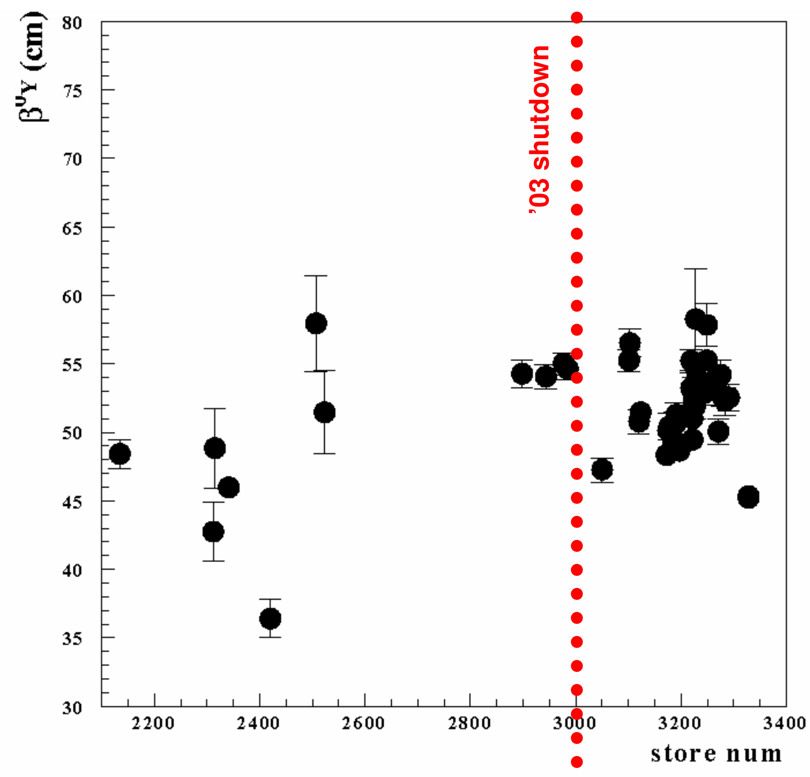
29-MAR-04, ~2 hrs into the store

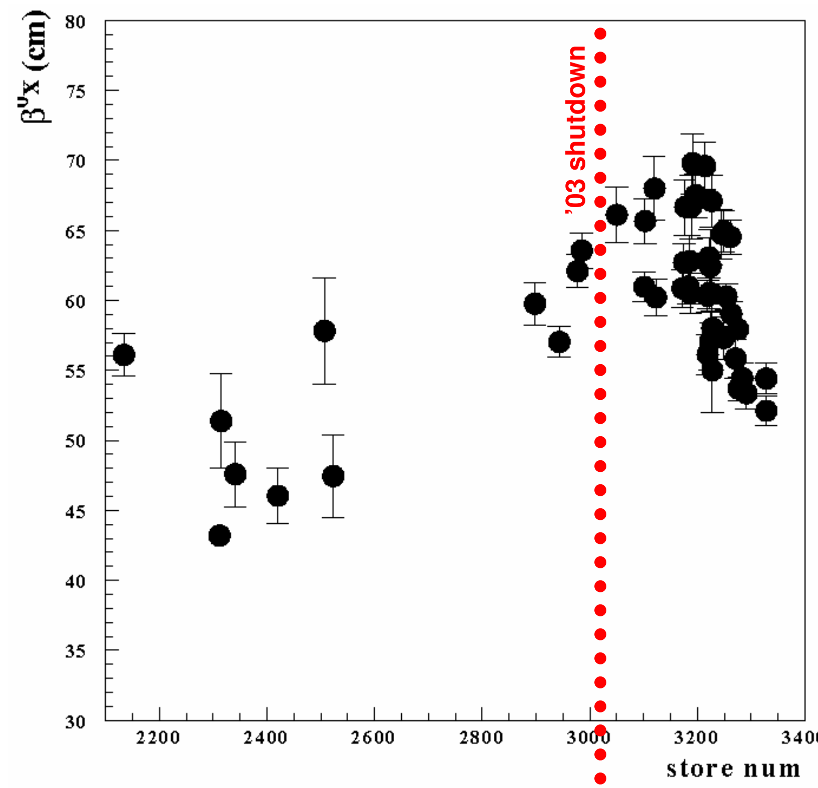


29-MAR-04, ~6 hrs into the store



What we learned



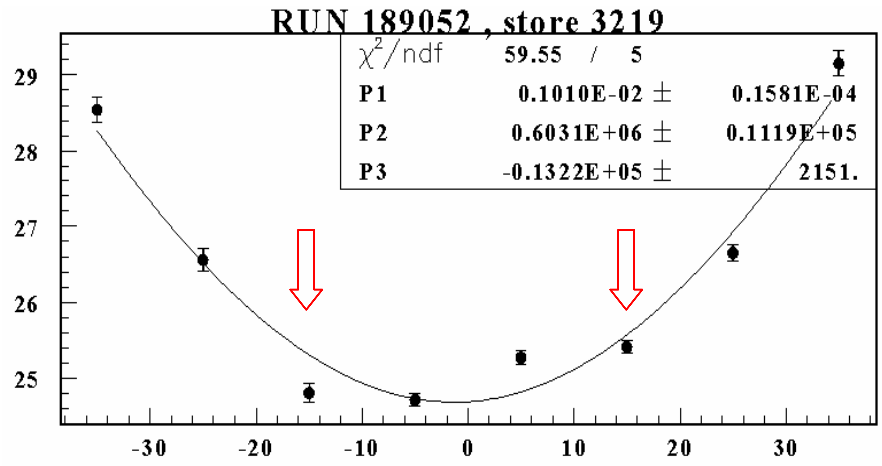


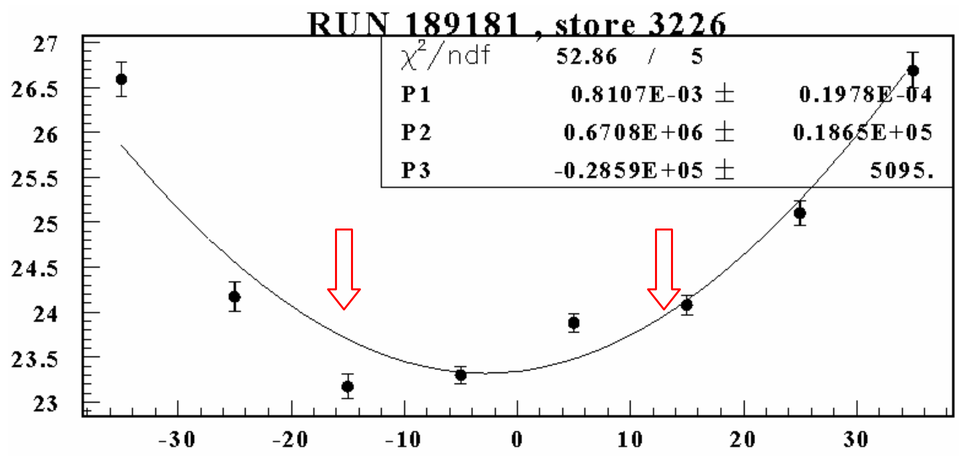
1) If we assume the a very simple model for the shape of the luminous region, we get β* much larger than the expected value of 35cm.

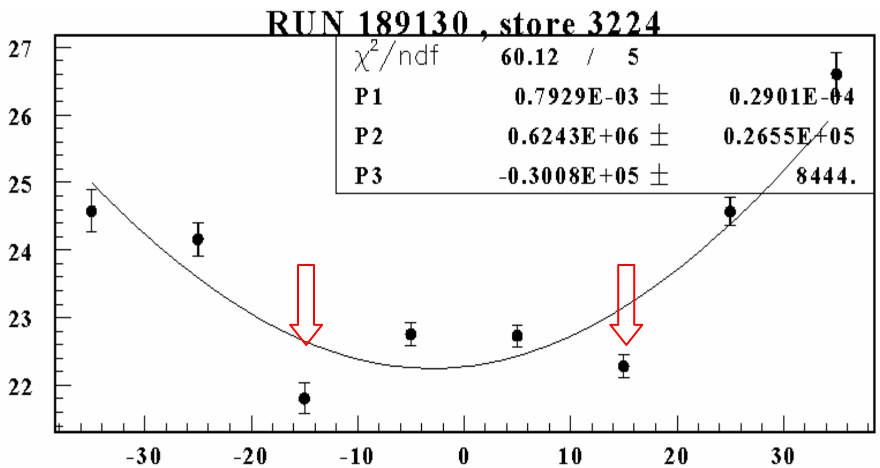
2) Could this be related to the different in luminosity between DØ and CDF? CDF sees β* closer to 35cm.

3) We also know that the simple model does not fit our data correctly. See $\chi 2$ in the two previous slides.

One idea: 2 waists (1)

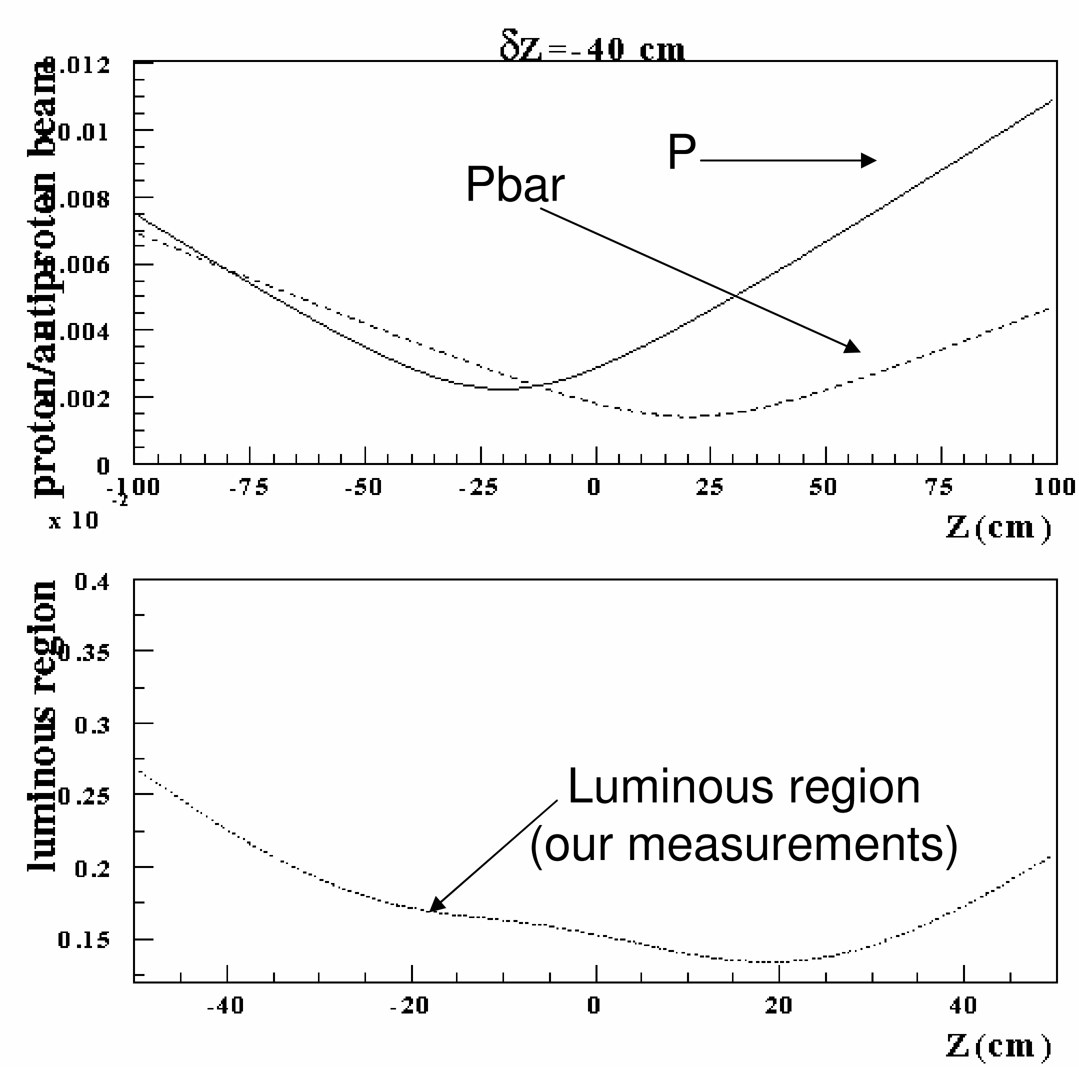






Seems to me like we have 2 waists. Depending on the emittances of each beam, which one dominates.

One idea: 2 waists (2)

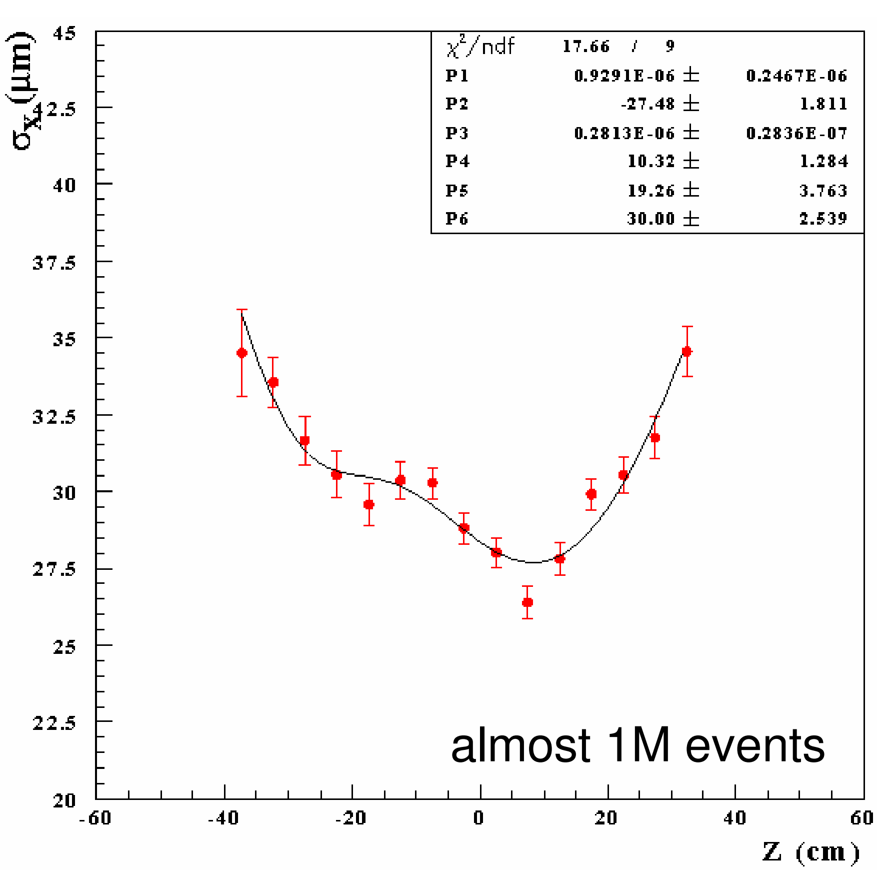


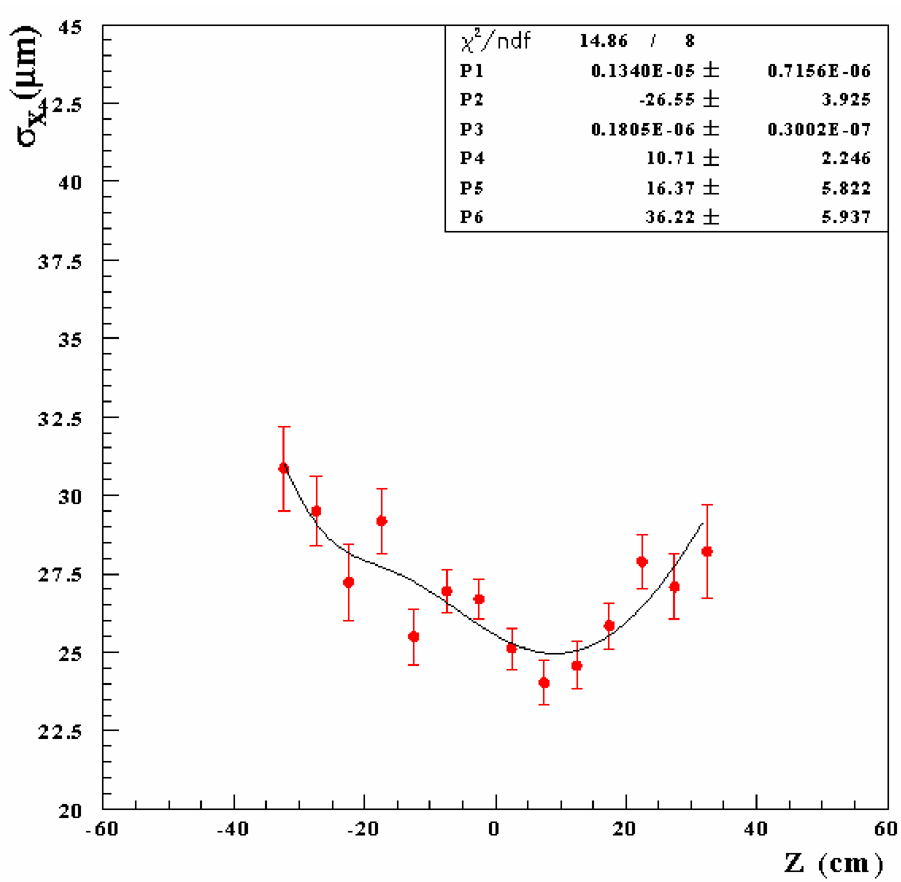
Our data is consistent with something like this?

We don't know yet. N. Gelgand (from the Tevatron department) is calculating what needs to go wrong to get this kind of problem at the IP.

β* for the luminous region looks larger than for each beam.

One idea: 2 waists (3)





Our data can be fitted with this model. But assumes that beams remember how they were injected.

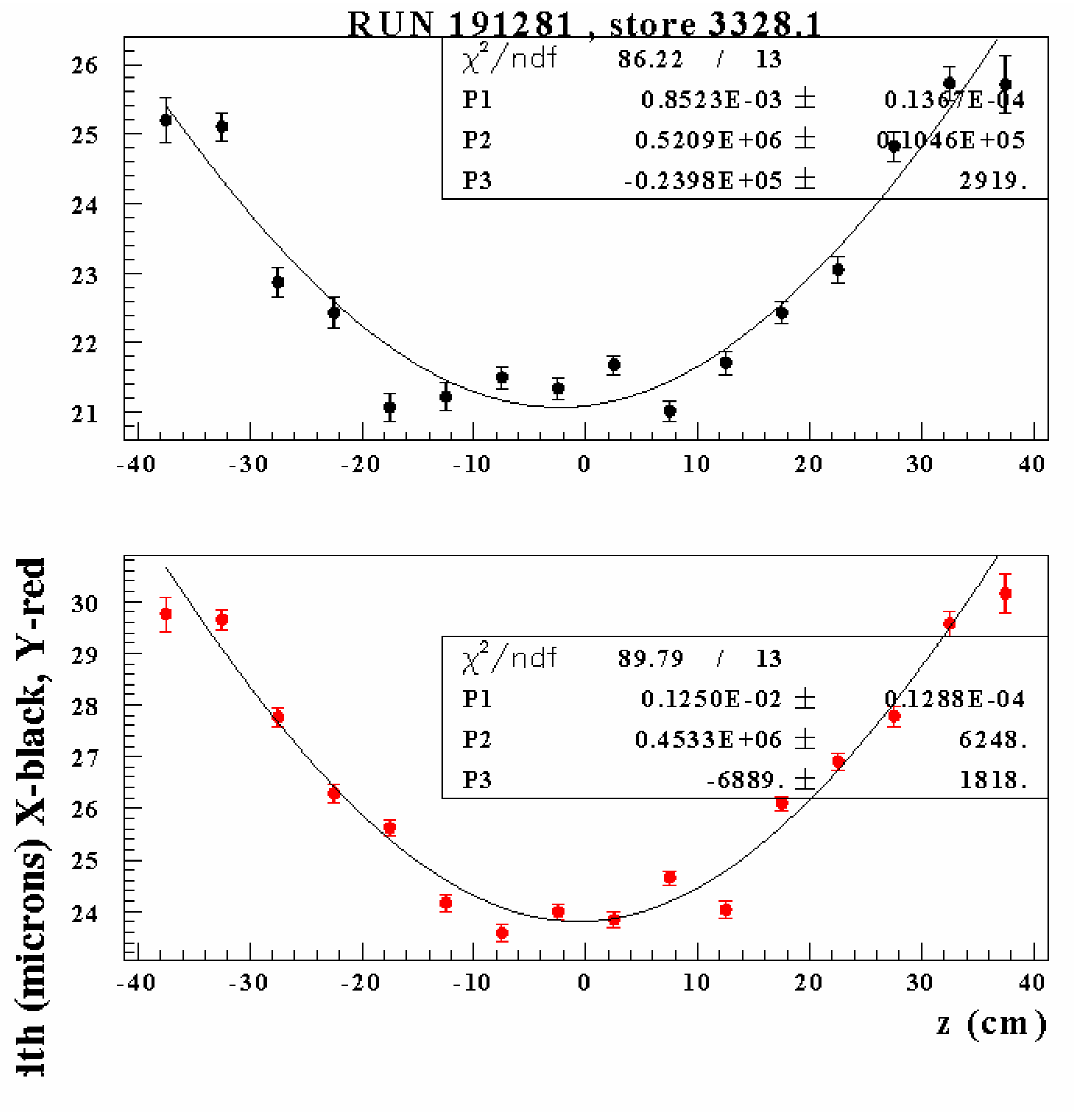
Results after the March mini-shutdown

After short shutdown in March2004, the factor LDØ/LCDF (usually around 0.93) is now ~0.97. We repeated the measurements for the new data.

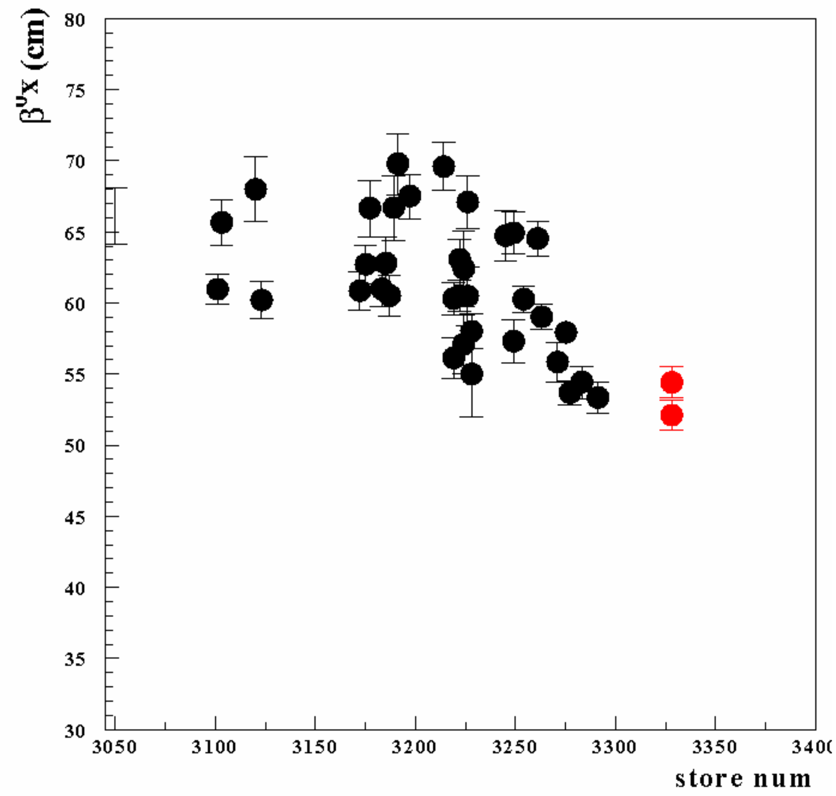
The results suggest:

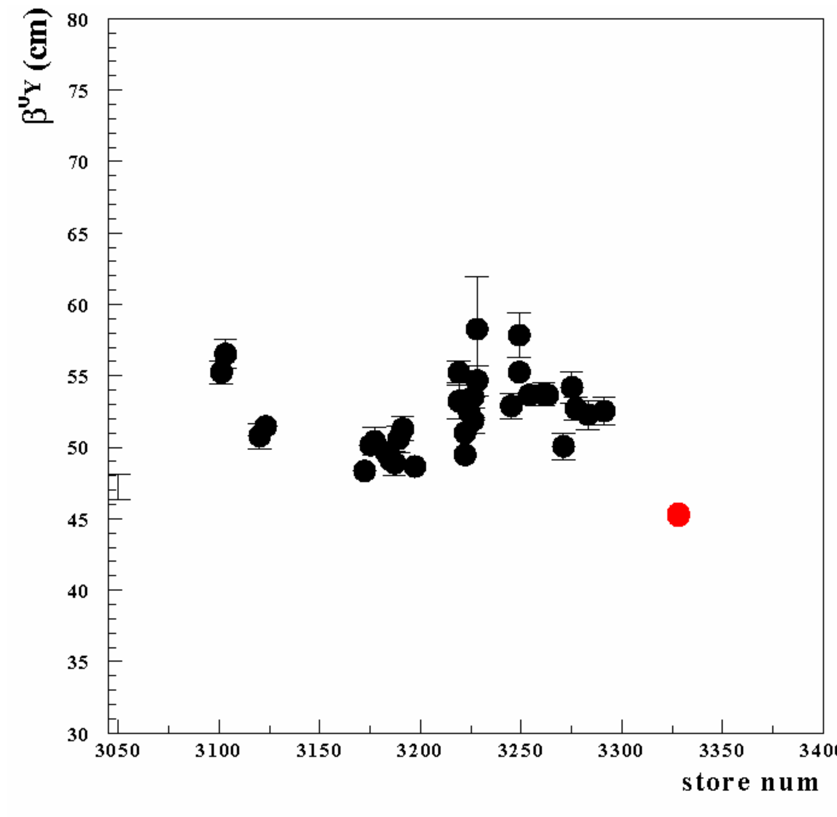
1) We still see the strange shape that can not be explained by a single β*.

2) β* from the fit to the simple model is now smaller than before.



Results after the March mini-shutdown (continue)



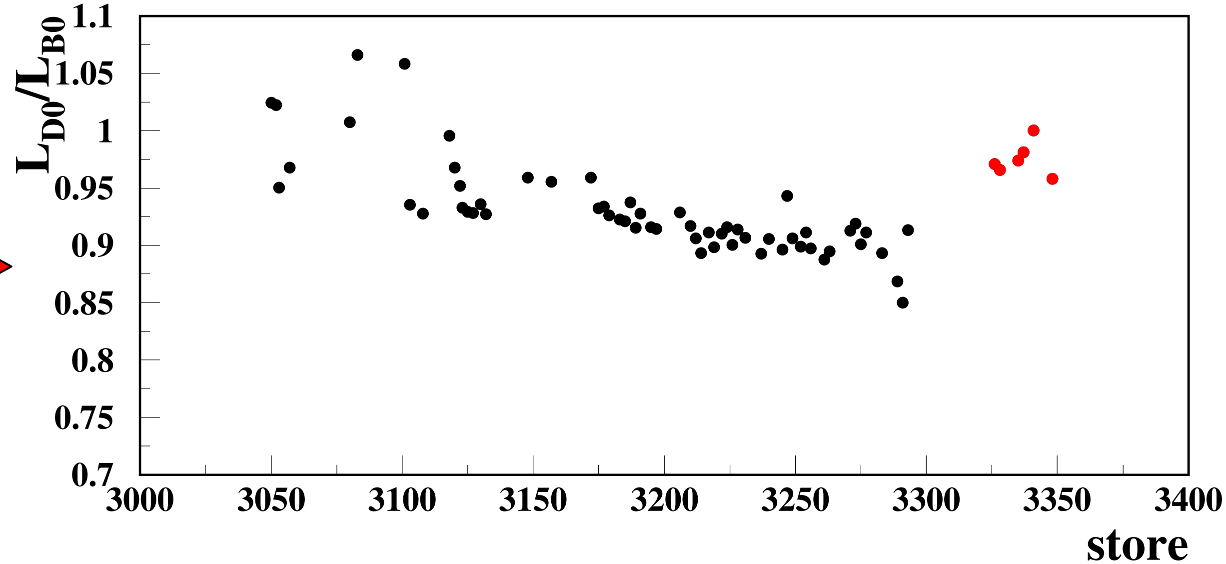


After the 03/04 shutdown, the β* fitted using the simple model, are smaller. This is evident in β^*_Y, not so much in β^*_X.

Did the luminosity go up at D0 ?

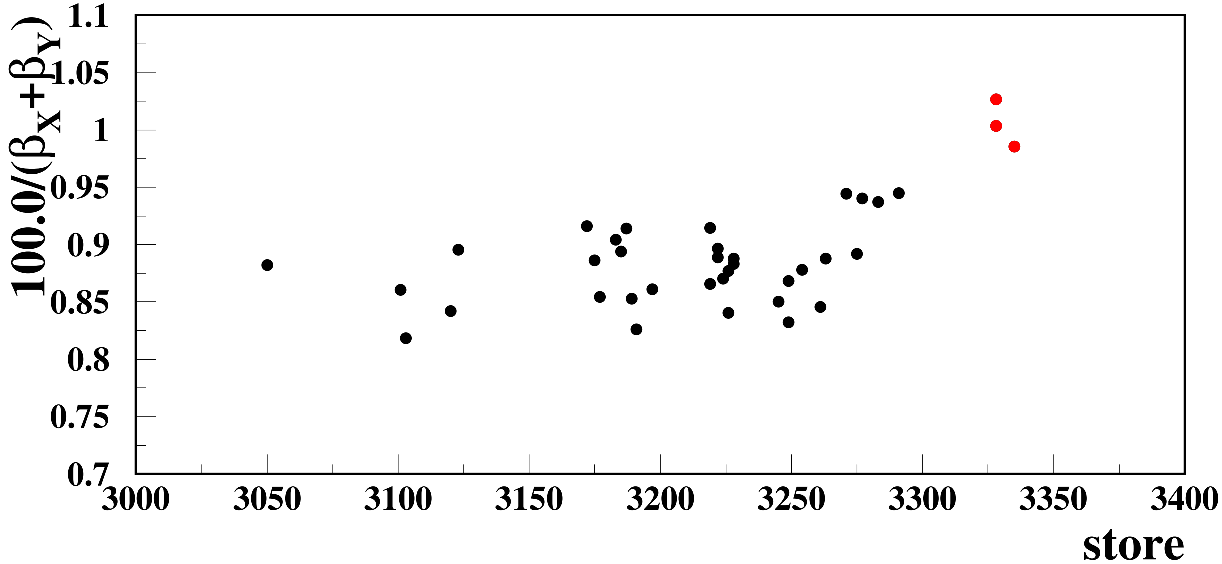
Ratio of luminosities as reported by the experiments to ACNET

The points in red corresponds to data taken after the March mini-shutdown



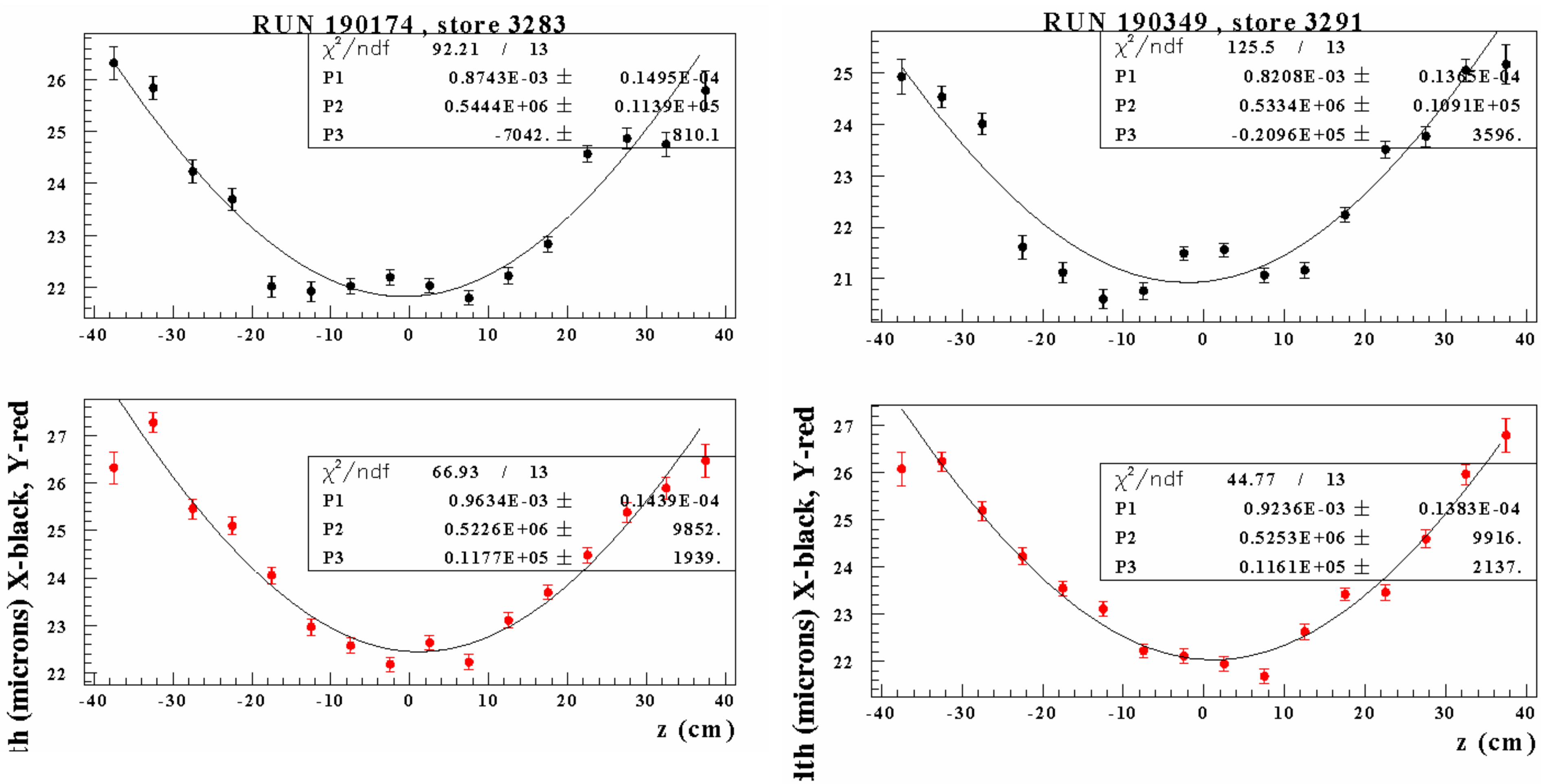
Inverse β* . The factor of 100 is arbitrary.

The points in red corresponds to data taken after the March mini-shutdown



Backup

Some results



Could this be an alignment issue?

Two possible alignment problems

$$X_{meas} = X_{real} + X_{real} \times A(Z) + B(Z)$$

$$X_{meas} - X_{real} = X_{real} \times A(Z) + B(Z)$$

If the ***A(Z)=0*** $\Rightarrow$ we are not affected by alignment because:
(just need to be sure our ***Z*** bins are small enough)

$$\Delta X_{meas} = \Delta X_{real}$$

If the ***A(Z)≠0*** $\Rightarrow$ we have to be careful because ***we will deform our luminous region shape***

$$\Delta X_{meas} = \Delta X_{real}[1 + A(Z)]$$

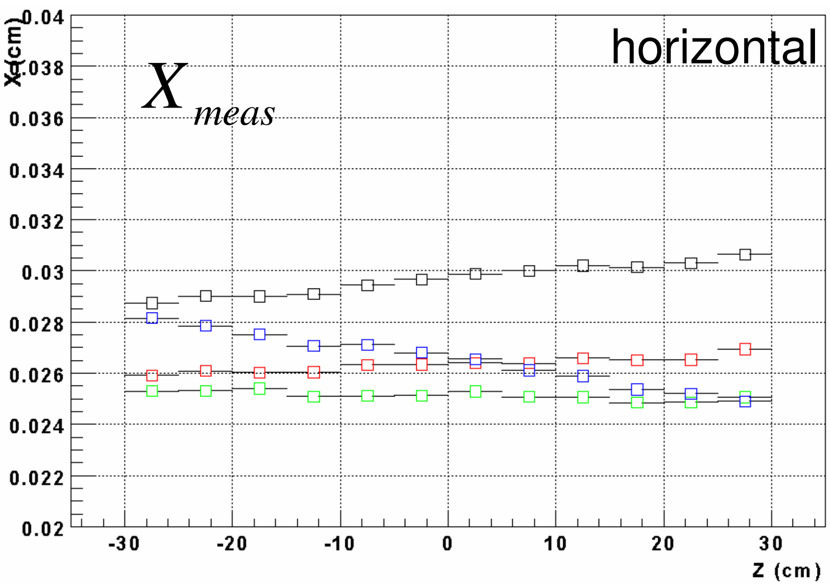
We can tell the difference by comparing ($\boldsymbol{X_{meas}-X_{real}}$) in stores with different $\boldsymbol{X_{real}}$.

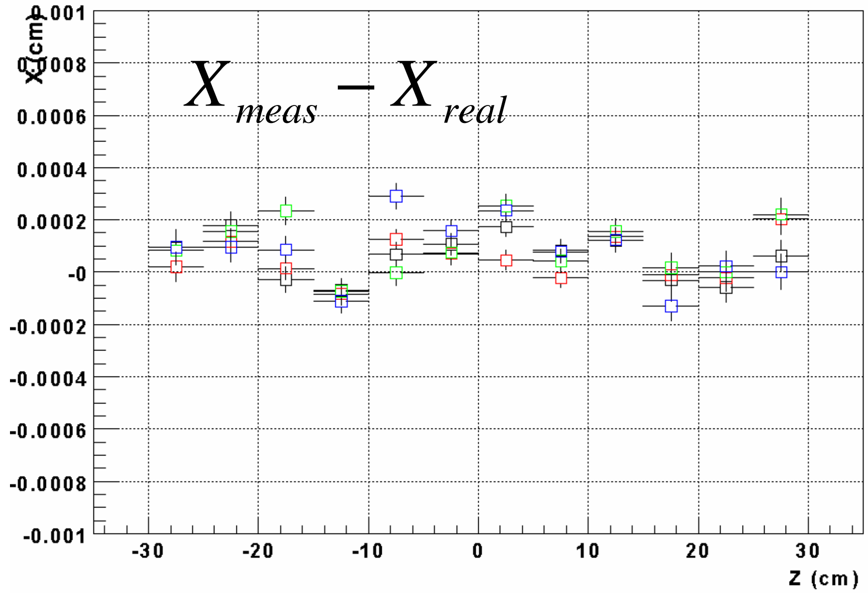
$$X_{meas}^{store1} - X_{real}^{store1} = X_{real}^{store1} \times A(Z) + B(Z)$$

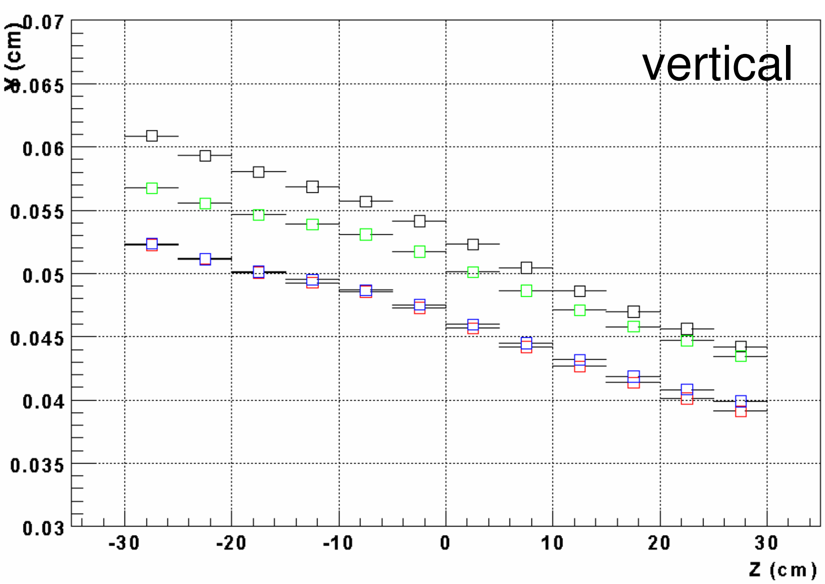
Beam position and deviation from straight line (early 2004)

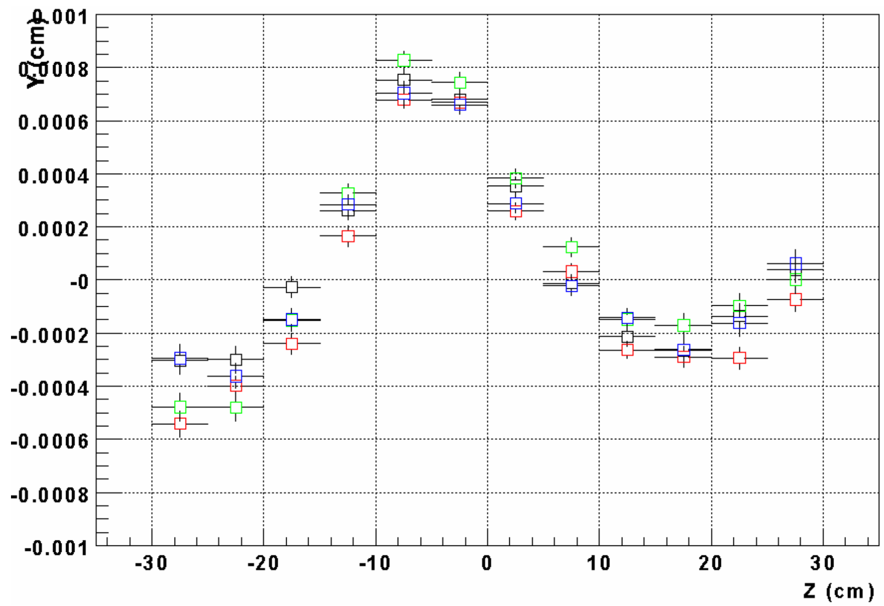
position

position – linear fit

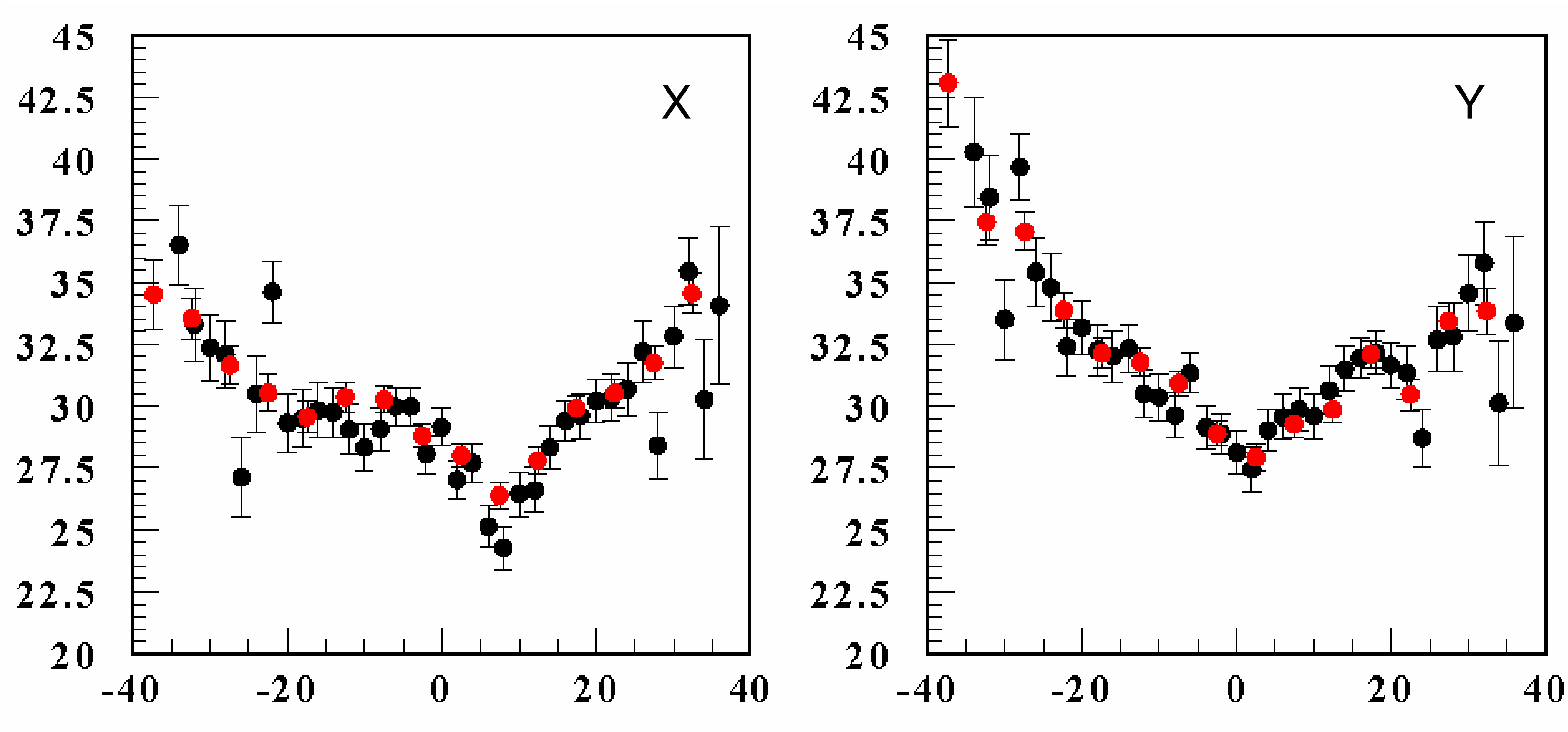








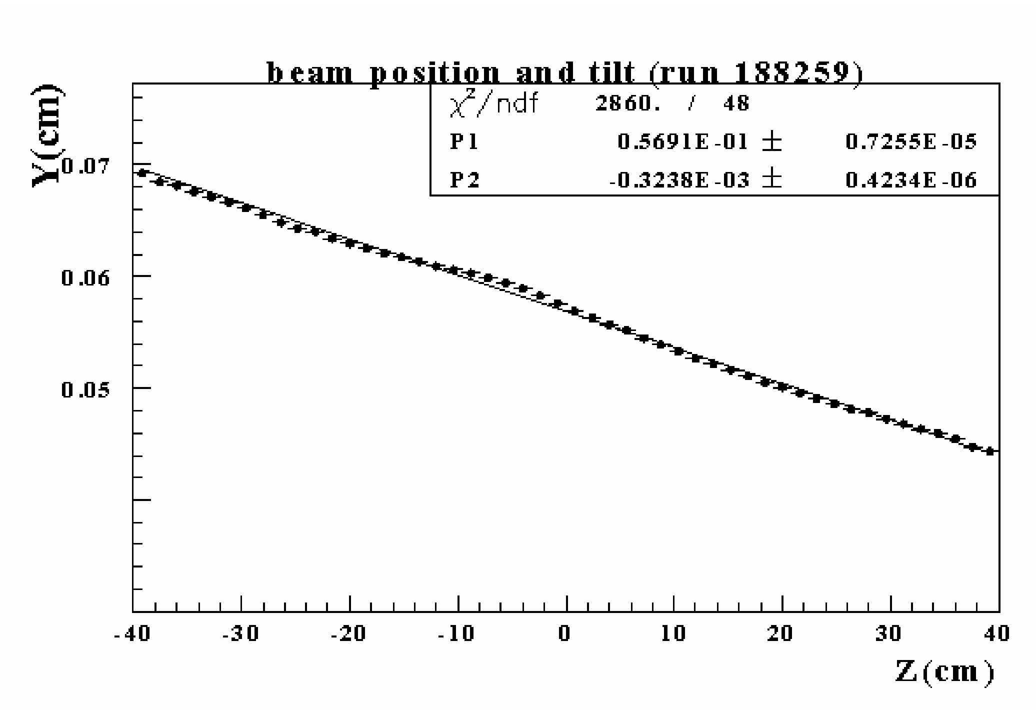
Changing the binning

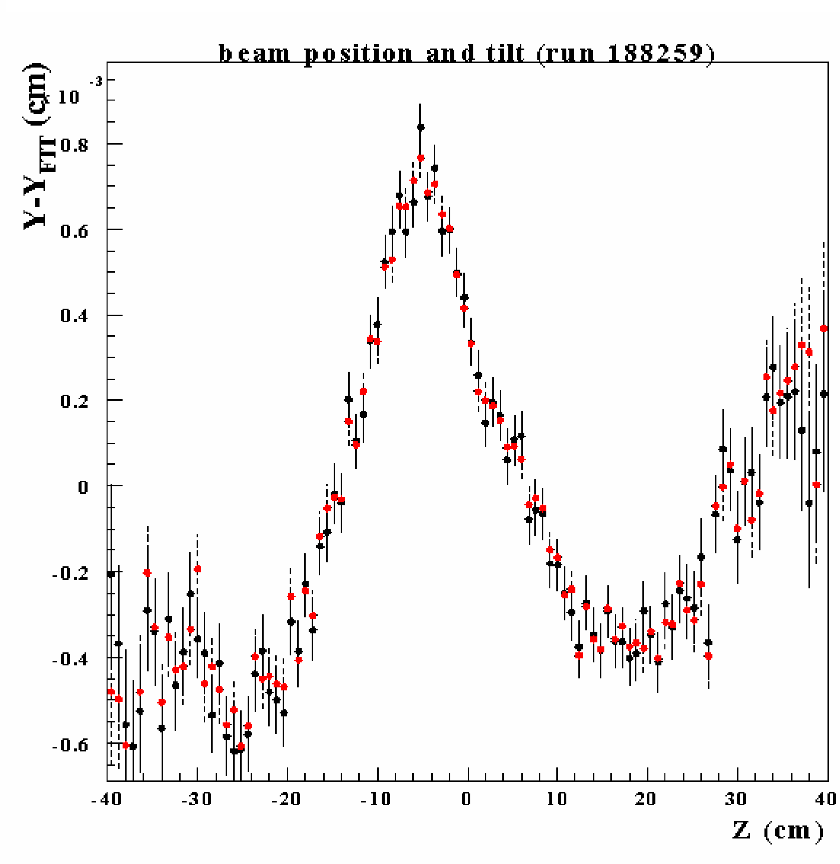


After changing the binning 5 cm to 2 cm, one can see that the observed shape is not determined by the Z binning.

At this point it is unlikely that the shape is due to alignment issues. There is still one more test to do, reconstructing the same run with two different alignment files and check if anything changes.

Beam position





The deviation from linearity does not depend on the algorithm used for the reconstruction of the primary vertex (dØroot vs dØreco)

The situation

We have some measurements of the luminous region that do not give the results we expect. Two options:

(1) The measurements could be wrong.
(2) Our expectations could be wrong.

As shown in the previous slides, we spend a lot of time discarding (1) (MC calibration, check for alignment bias, check the results with two different methods)

What could be wrong in our expectations?