1 Dec 2009

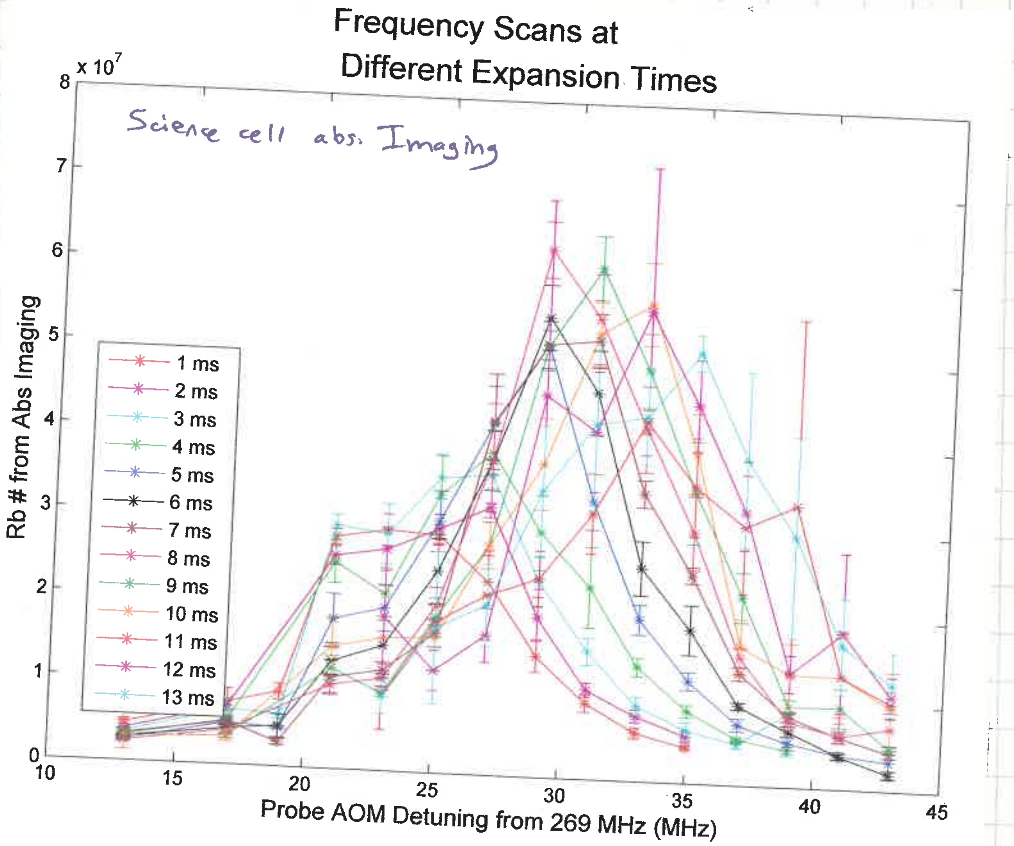
Rb Probe Analysis. We are taking expansion time traces of the Rb Trap at a variety of probe frequencies. It is taking a long time for eddy currents to die down. As the eddy currents die down, the resonant line for the 2->3 imaging transition shifts in frequency.

load 0.1V time in trap 0 5 Averages

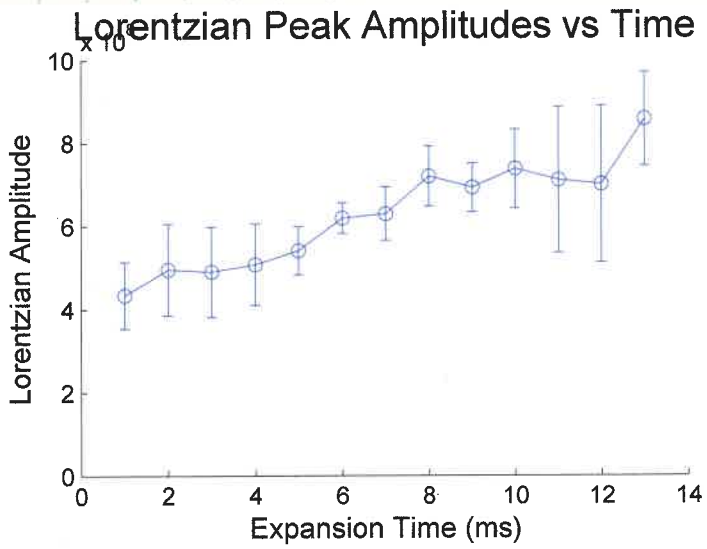
Image Coil ~~voltage~~ control voltage 8V -> 16 Amps

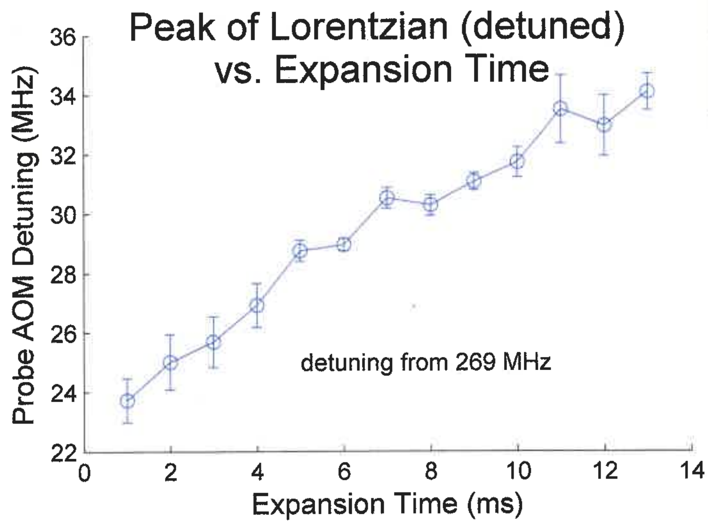
3 Dec 2009

This is a continuation from 1 Dec 2009. On the 1st I took time steps 1:1:10 ms (expansion time). Today I'm going to look further out in time (up to 15 ms). The cloud is extremely broad at 15 ms expansion.

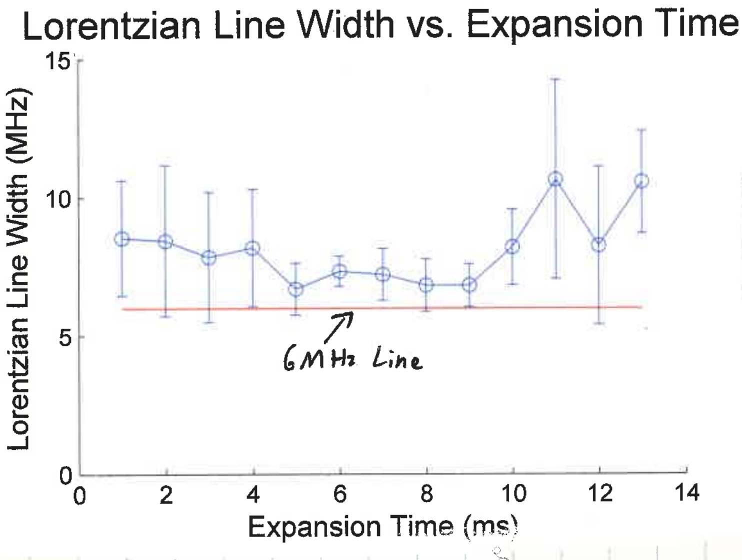


4



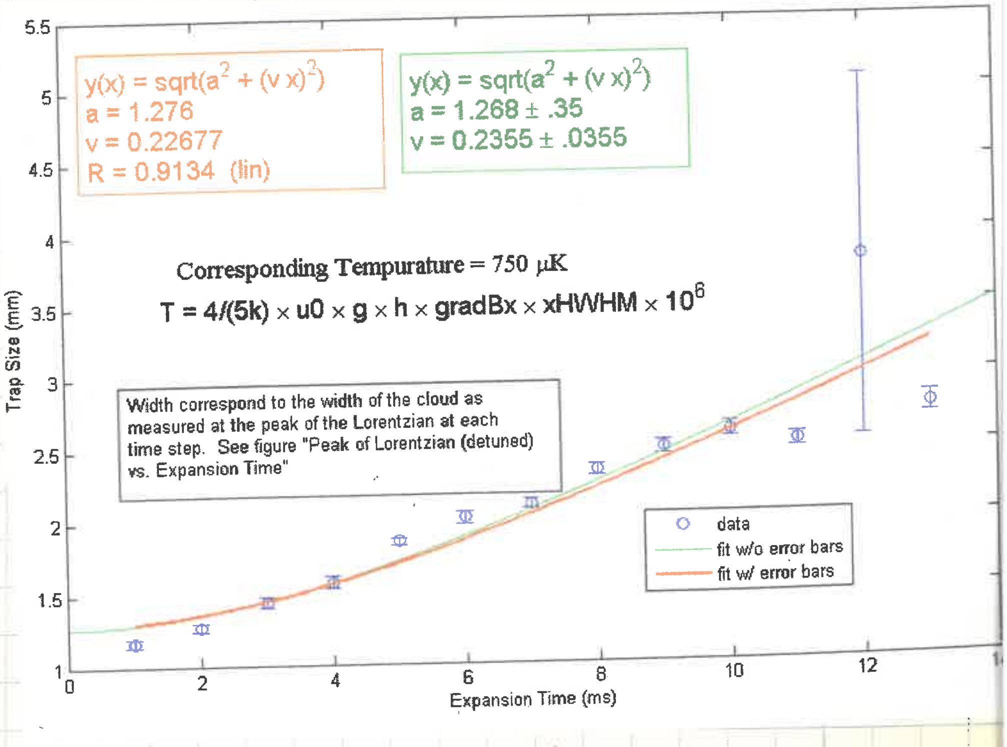


Bias field from Helmholtz coils = 24 G
= 33.6 MHz



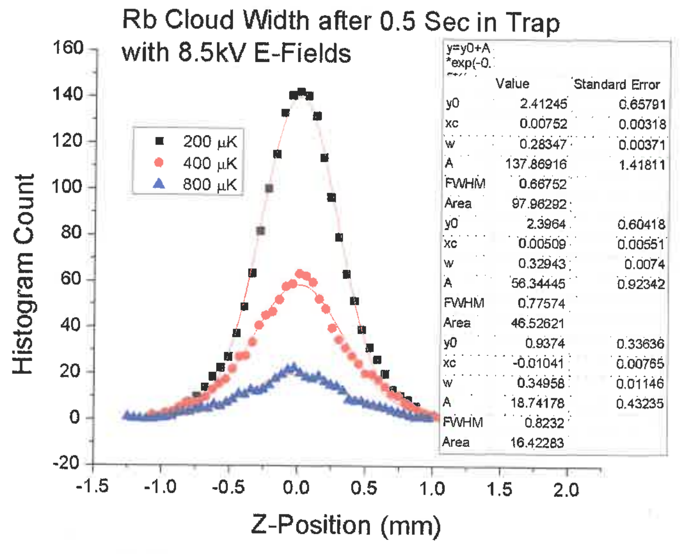
Error bars are 95% confidence bounds

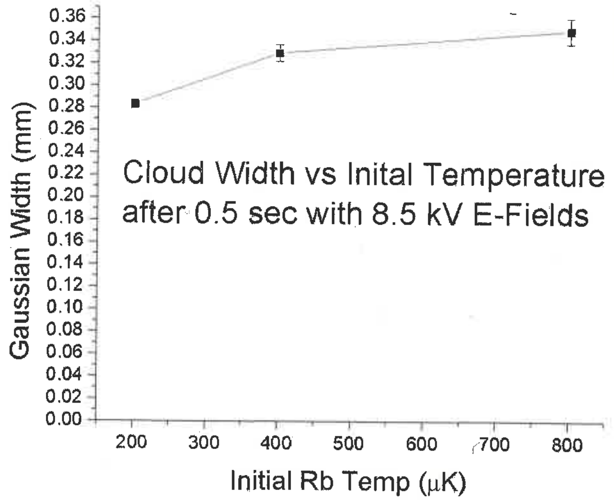
SCIENCE CELL
ABSORBTION IMAGING



Simulation results showing how final Rb trap size should scale w/ initial temperature. Initial Rb distribution is Maxwell-Boltzman distribution

Simulations





\MATLAB\Trap\trap_rbLifetime\outfiles\width vs time

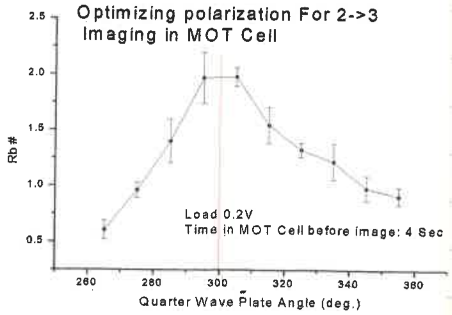
$\boxed{\text{7 Dec 2009}}$

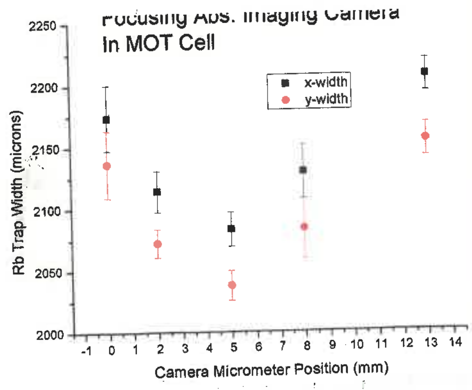
NEW: Abs Imaging in MOT Cell

An absorption camera was mounted ~~to~~ to take ~~pics~~ pictures in the MOT cell region. There is a 2:1 telescope to ~~expand~~ magnify the image of the ~~Rb~~ Rb (25cm : 12.5cm lenses). ~~and~~ There is a 15:6 telescope to expand the beam out of the fiber (initial size ~8 mm)

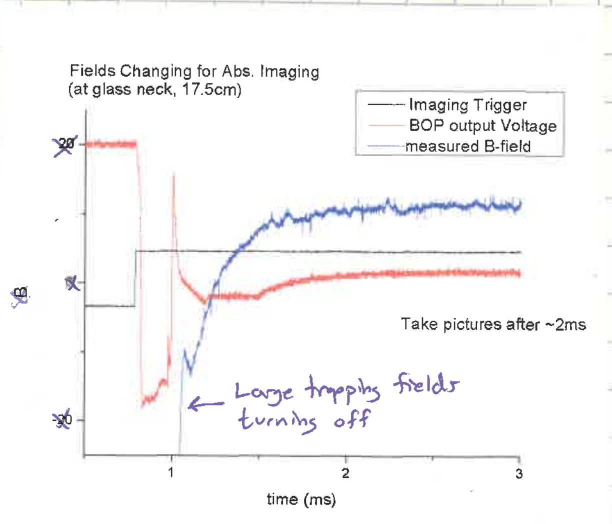
Next step is to focus the camera. The Camera is on a translation stage w/ 13 mm of travel

Image coil ~~bias~~ control 8V
3ms expansion





For new absorbtion imaging, we want to know when we can start taking pictures



In trying to get the magnification of the camera, we ~~move~~ take pictures at different track positions.

for MOT cell Mag = 5.79 um/pixel
science cell Mag = 10.8 um/pixel
10.1

} these are remeasured on next day !!

there is a 2:1 magnification in MOT cell
10:7.5 magnification in science cell
so the measured magnifications do not jive w/ eachother.

science cell imaging is not focused so now we focus science cell imaging.
↳ x-axis is number of turns on thorlabs adjustable lens holder
~~1 turn~~ 1 turn = 0.7mm

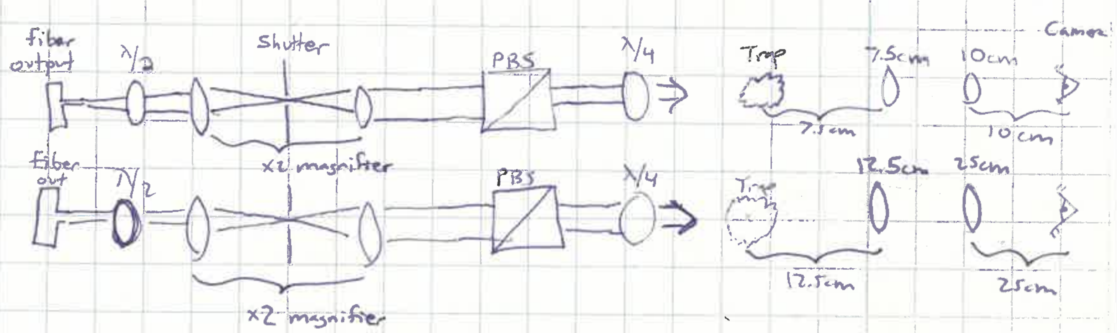
sciCell_focus1.txt

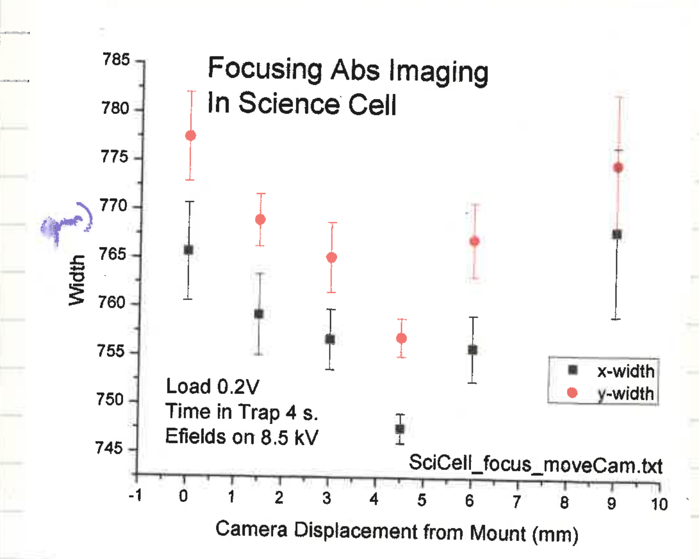
sciCell_focus_9.2mm.txt pushed camera forward 9.2mm

Checked imaging achromat lenses in old absorbtion imaging system [science cell], they were ~~not~~ 7.5cm and 10cm focal lengths as expected. Now, they are.

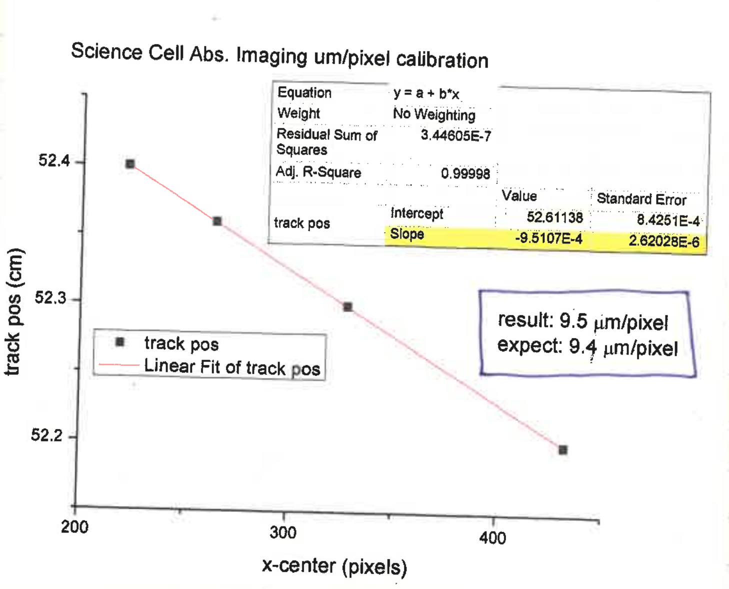
Science Cell Abs Imaging: [52.36cm]

MOT Cell Abs. Imaging: [11.1cm]





Leaving camera at 4.5 mm position



MOT abs: $M = 2$, $\mu m/pixel = 5.8\ \mu m/pixel$

Science abs: $M = 1.33$, $\mu m/pixel = \boxed{9.5\ \mu m/pixel}$ here, close.

Calibrated via track move variation.

expect $5.8\left(\frac{2}{1.33}\right) = 8.7\ \mu m/pixel$ (based on MOT cell imaging)

expect based on 13.3 x 13.3 mm Image area and 1064 pixels

→ 9.4 um/pixel in science cell

6.26 um/pixel in MOT cell

expectations based on pixel size and our magnification.

Abs imaging in the MOT cell should be eddy-current-free and ∴ should be free of the drifting frequency profile at different expansion times like imaging in the science cell [see pg 4, top right corner]. The following data was taken as follows:

↑see next pg.

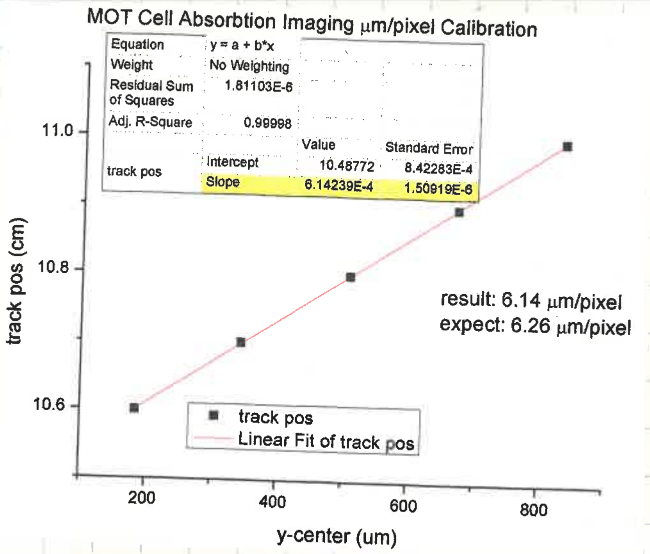
1) Load MOT + B-trap [1.0 V load]
2) move to science cell ~~fo~~
3) E-fields on for 2 seconds
4) move to MOT cell absorbtion imaging
5) take pictures.

The science cell E-fields were used to avoid saturation of the camera via their reduction of signal.

[note: Since fixing the current supplies last week, the electric fields seem to be having much less effect on Rb, it may be colder than before (with the oscillating currents around). We will re-investigate this after finishing tweaking up the MOT cell absorbtion imaging.]

	Measured	Expected
MOT cell	6.15 μm/pix	6.26 μm/pix
Science Cell	9.5 μm/pix	9.4 μm/pix

Scales for absorption Imaging



Also, measured probe beam intensity entering MOT cell is

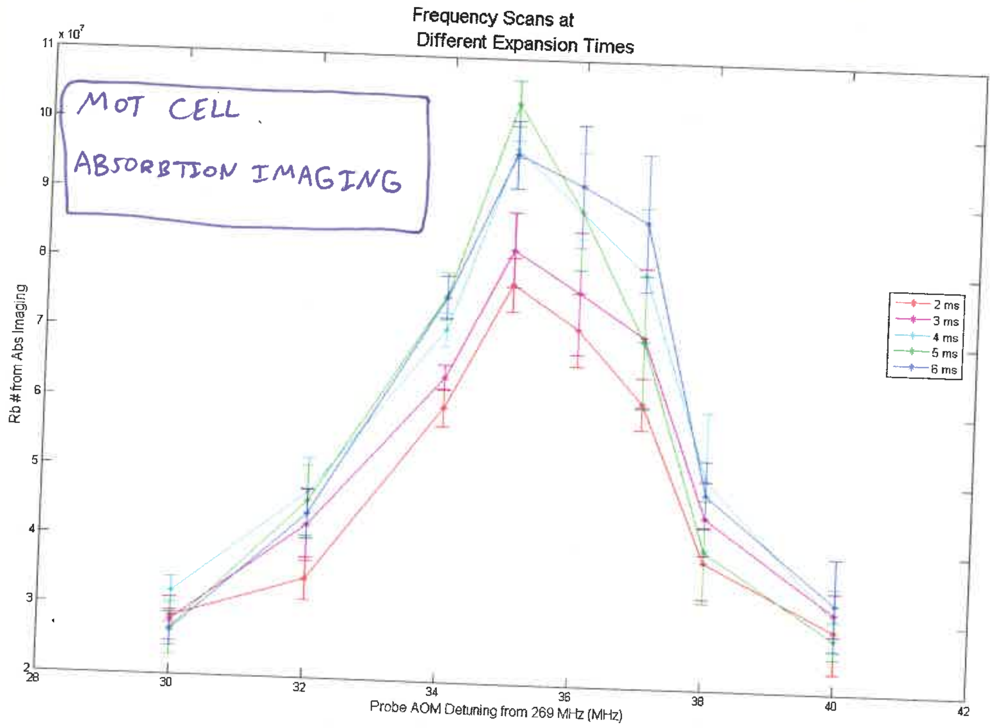
$$I = 150\mu W / 1\ cm^2 \ *$$

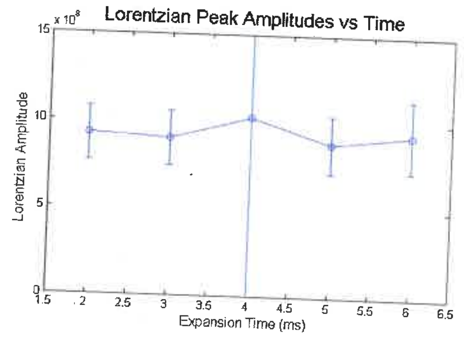
such that

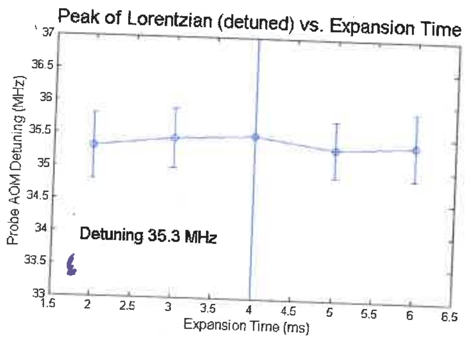
$$\frac{I}{I_{sat}} = 0.094$$

where $I_{sat} = 1.6\ mW/cm^2$

*note: 150 μW at entry to science cell corresponds to 310 μW at shutter entrance.

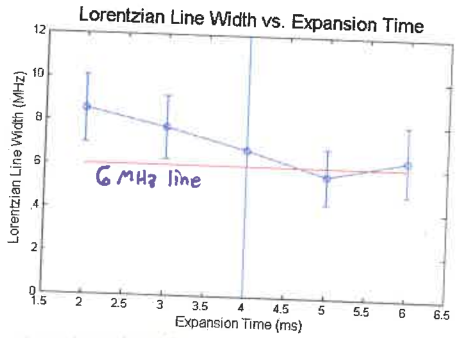




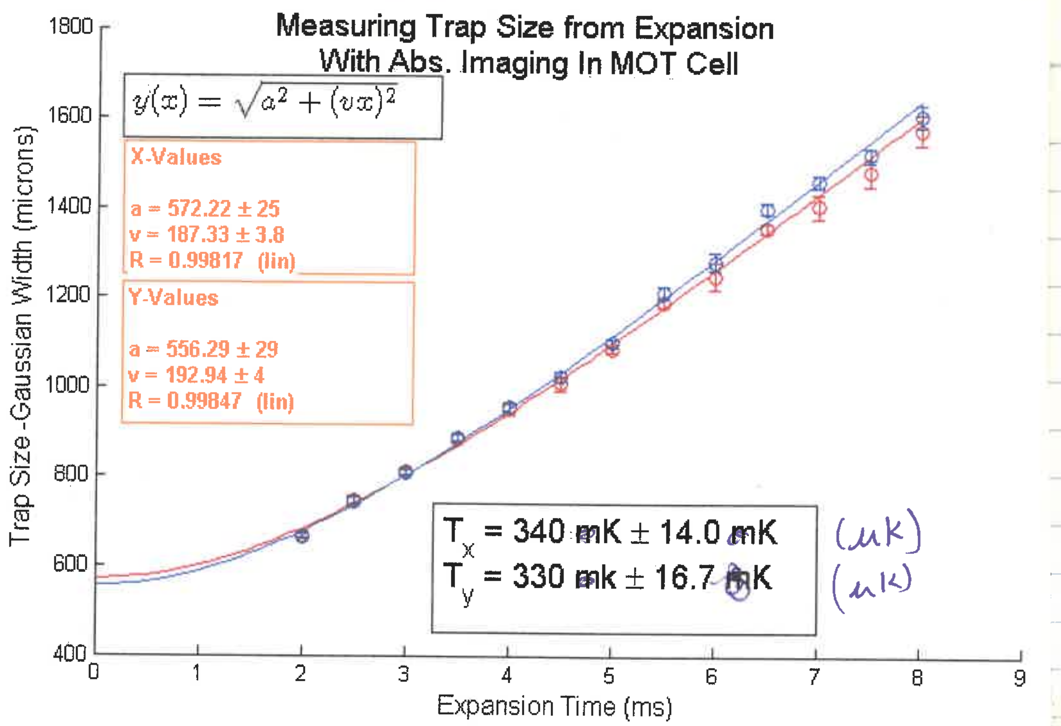


← no expansion-time dependent shift ☺

Error bars are 95% confidence



→ 233.7 MHz probe shutter AOM
269.0 MHz probe AOM
} 35.3 MHz detuning.

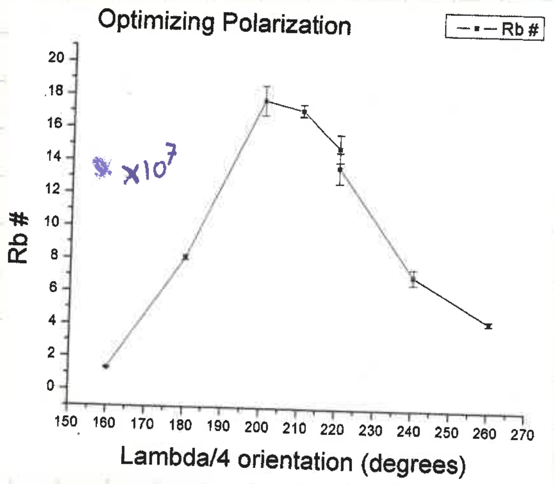


load = 0.2V
time in trap = 3.5 s
w/ E-fields on.

12/9/2009

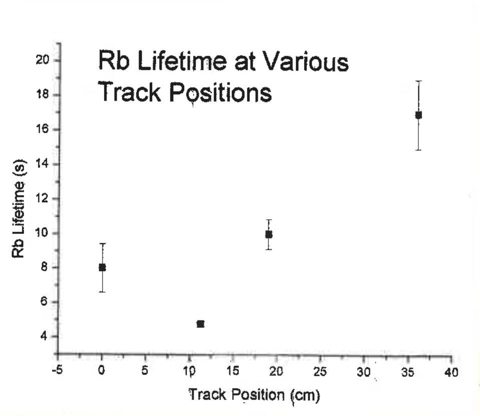
The mirror below the MOT cell used for absorbtion imaging is unstable and we have low OD's. We replaced it with a known good mirror and re-aligned MOT cell abs. imaging. OD's are much better now [were ~ 1.0, now ~ 3.0]. Re-optimized λ/4 polarization to 205° (see below)

MOT mirror was bumped, re-aligned, TA still at 450 mW
MOT load: load 3.1 Volts, τ = 13 sec



~~Now that absorbtion imaging in the MOT cell is working, we want to nail down the relationship between the absorbtion imaging in the two locations. We'll start by getting a handle on transit loss by looking at~~ lifetimes at ~~various positions~~

as Fitch was saying before he crossed it out, we are going to take lifetimes in various positions of the Rb corridor to see if ~~there the~~ are areas w/ ~~partic~~ particularly high loss rates.



load 0.1V Strong Field 4.8V
imaging in MOT cell after 7ms Expansion

run	RbLifetime_0000cm.txt
5ms	_11.35cm.txt
	_19.00cm.txt
	_36.00cm.txt
6ms	_11.35cmb.txt - added cloth to block probe shutter.
6ms →	_11.35ccm.txt - added covering around MOT shutters
9ms →	_11.35dcm.txt - taped cell around neck and 11.35cm area
7ms →	_11.35ecm.txt - removed tape and probe shutter coverings ↳ put probe shutter outside the box. made picture taking noisy
7ms →	_11.35fcm.txt put probe shutter back inside box added tape around box.
10$\frac{5}{}$ms →	_11.35gcm.txt built foam-core box around probe shutter inside box.

→ going w/ this. lifetime = 10.3 s ± 0.1 s

Now that we have our final configuration, we'll do lifetimes at various positions.
0.1V load, changing expansion times to not saturate ~~via~~ via

time in trap (s)	expansion (ms)
0	11
5	6 to 8
10	5 to 7
15	3
↓	↓

[MOT cell absorbtion imaging]

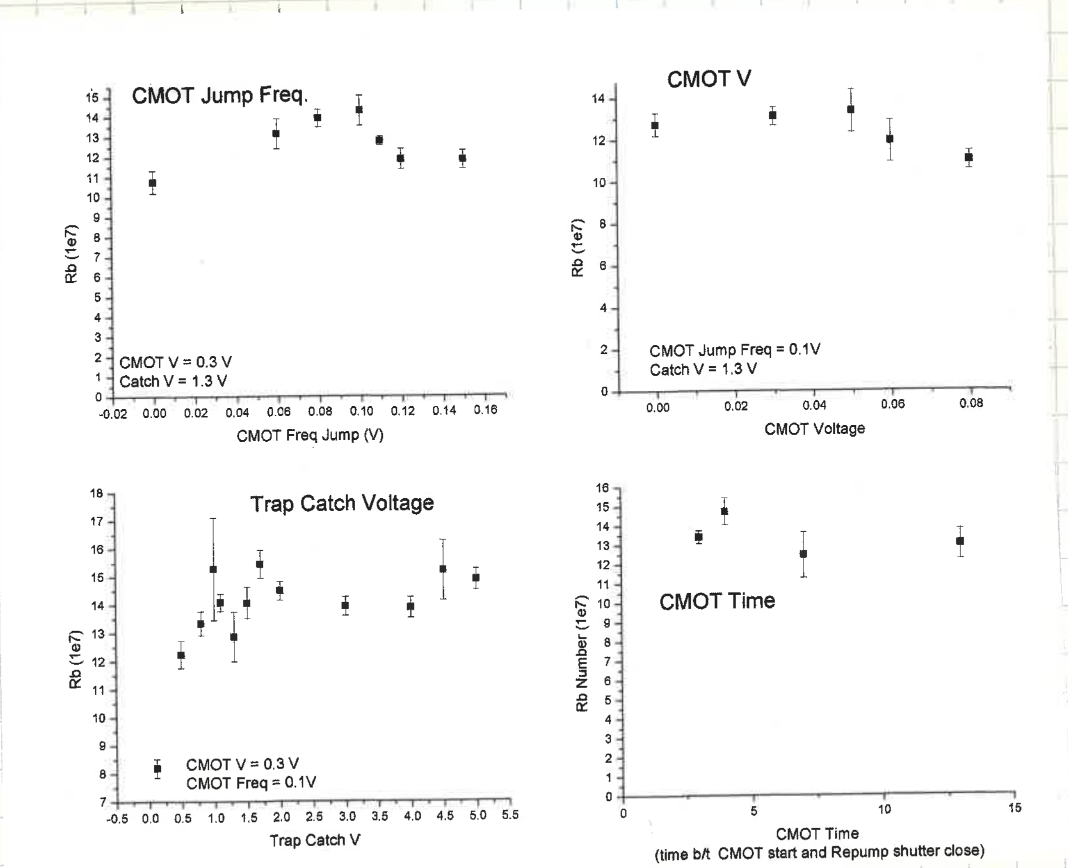
run Rblifetimes_0cm.txt
Rblifetimes_20cm.txt
Rblifetimes_30cm.txt

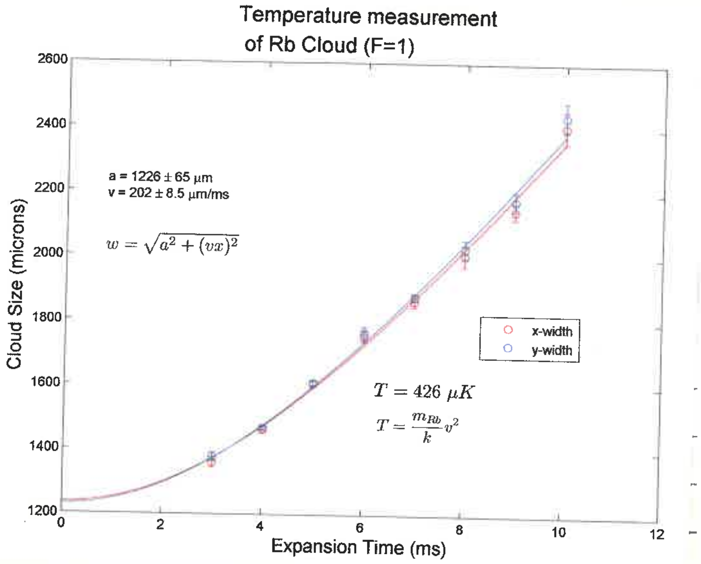
CATASTROPHIC TURBO PUMP CONTROLLER FAILURE
(1500 L/sec pump)

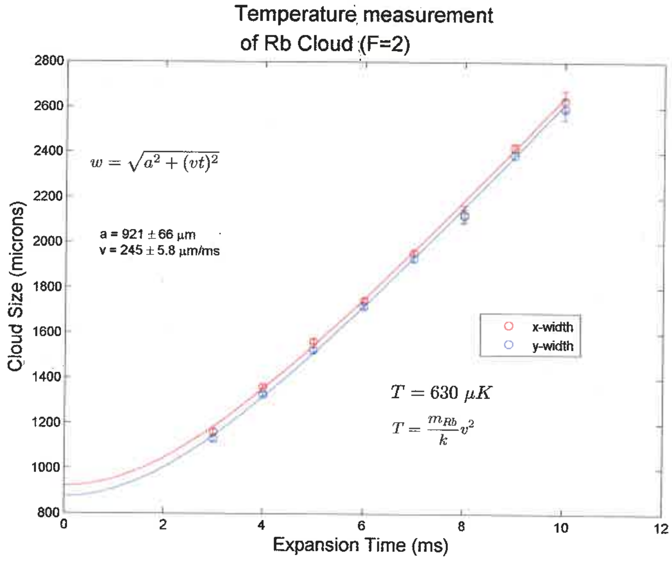
lifetime at 19cm is 11.5 ± .8 seconds, so it looks like no damage to the ~~o~~ vacuum in the Rb cell.

RbLifetime_19cm.txt (14 Dec 2009)

14 Dec 2009







load : 0.3V
time in trap 7 seconds

load : 0.2V
time in trap : 7sec.

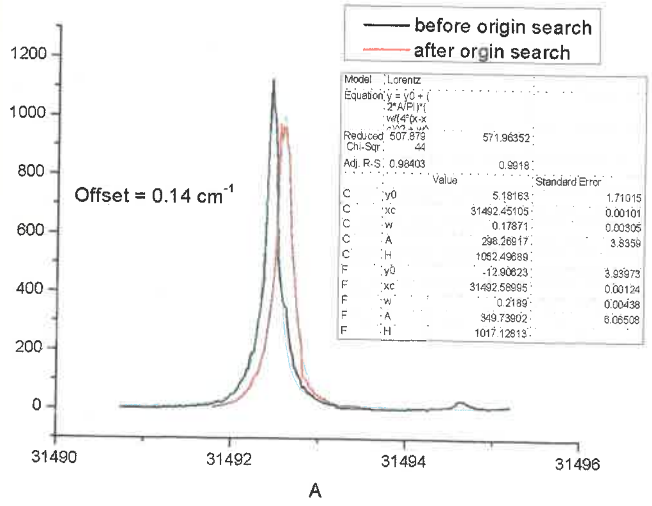
$\boxed{\text{16 Dec 2009}}$

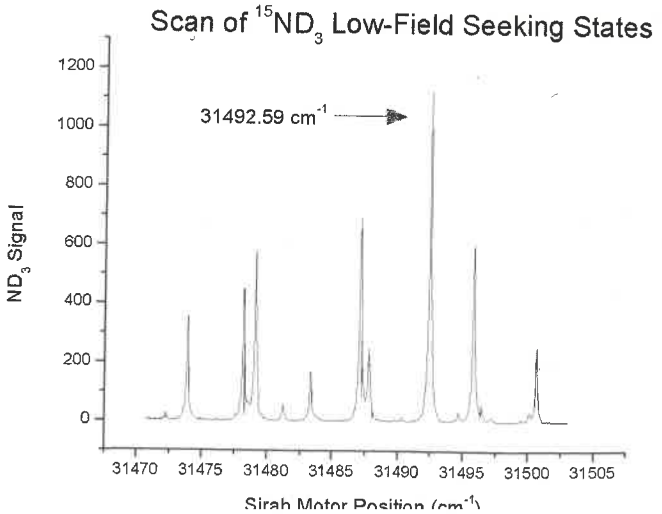
Laser scan of $^{15}ND_3$ This scan is being taken in little bay as kelvinator is out of commission (DCU 600 failure - shorted to case).

gas 3.4% $^{15}ND_3$ $\frac{.1\ \text{bar}}{42\ \text{psi}}$ (absolute)
laser ~18 mJ
MCP 3000V
valve 2

scanning backwards in wavelength because the auto-tracker tracks better

run 15ND3_Scan01.txt
run recalibrated → did origin scan and rescanned one peak to determine offset





Peaks

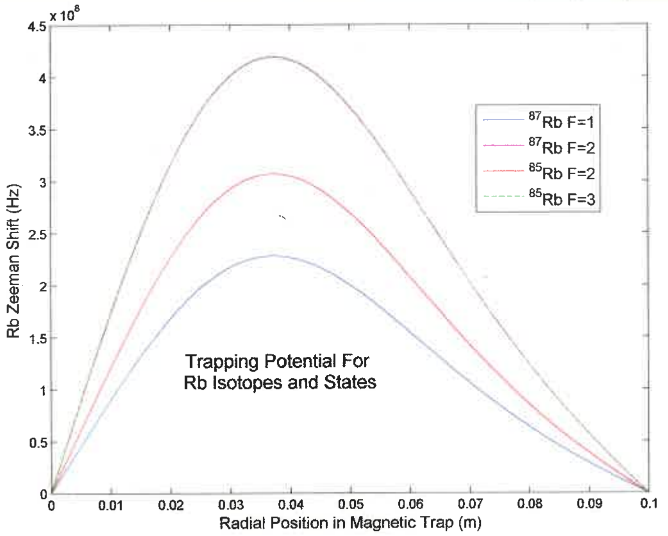
	Area	Center	Width	Height
1	93.24021	31474.02989	0.16134	367.91245
2	104.69195	31478.3075	0.1515	439.93212
3	167.17384	31479.21846	0.19363	549.64788
4	12.99032	31481.37095	0.15136	54.63642
5	45.92553	31483.44573	0.17437	167.67178
6	191.76192	31487.25904	0.17677	690.61876
7	78.87853	31487.92946	0.23405	214.55061
8	300.63055	31492.59107	0.18016	1062.30573
9	183.25274	31495.94711	0.19697	592.29372
10	64.9451	31500.77004	0.17044	242.58405

Statistics

DF	1584
COD (R^2)	0.97598
ReducedChiSq	269.99178

Listing of peak properties from $^{15}ND_3$ laser Scan

peaks are numbered left → Right



We replaced the the first beam cube after the fiber with one that is polarizing. This seems to fix the problem of the MOT moving when the fiber polarization changes.

After a power balance and slight MOT alignment correction,

MOT load = 2.4V, τ = 15 seconds

A strange aspect of picture taking, the magnetic trap has two hills in the x-cut, like

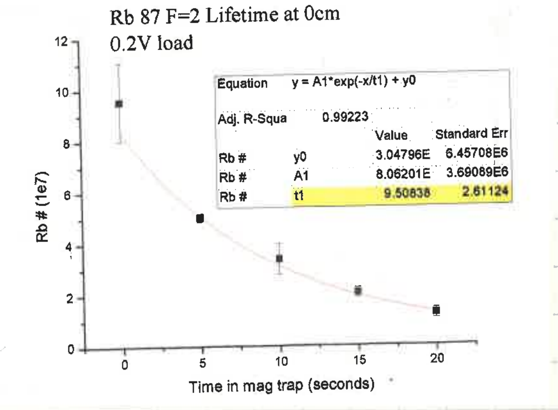
"the butt"

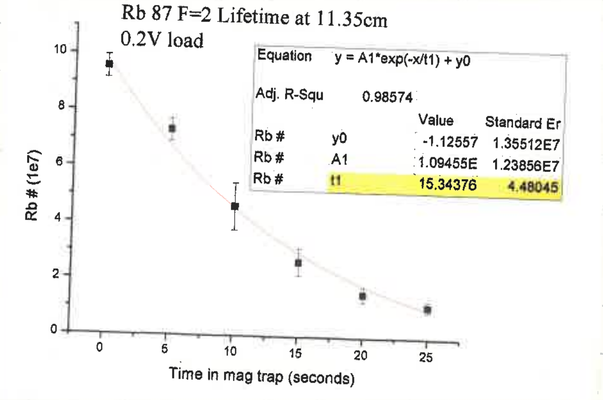
This structure goes away with ≥ 1 sec of wait in magnetic trap, [^{87}Rb] but doesn't seem to be affected by loading parameters.

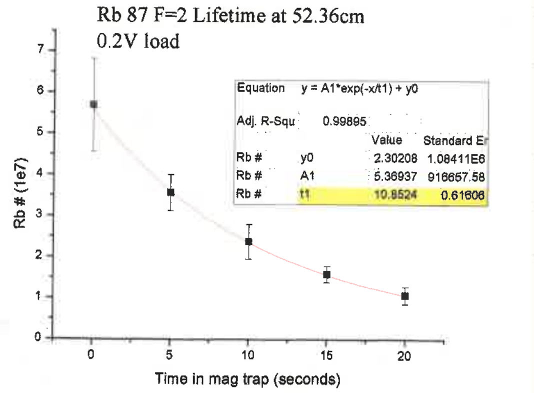
I'm going to look at $^{87}Rb_{F=2}$ lifetimes at various positions...

run
0cm 87RbF2Lifetime.txt
11.35cm 87RbF2Lifetime.txt
52.36cm 87RbF2Lifetime.txt
~~40cm 87RbF2Lifetime.txt~~
~~30cm 87RbF2Lifetime.txt~~

4 averages, MOT cell abs imaging
0.2V load

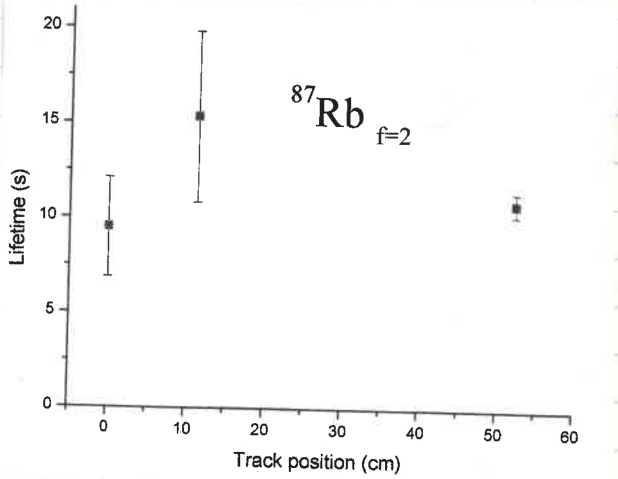






31 Dec 09 MOT load 2.93V, 12.2s ^{87}Rb

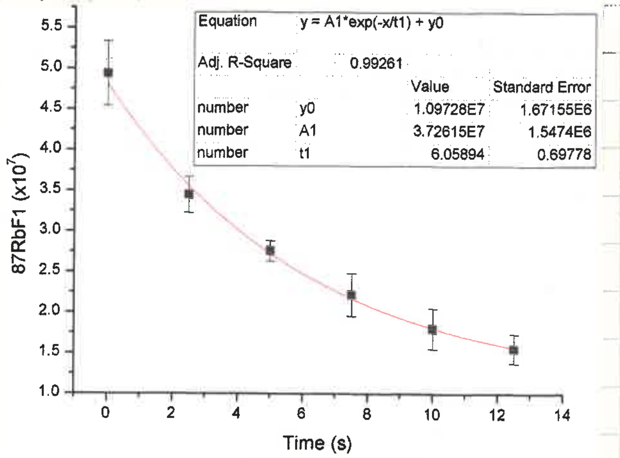
The same two lobed structure appears with the F=1 atoms



Paul optomized F=1 previously. However the MOT shutters have moved location. The optomized timing turns off the MOT shutters 1.5ms earlier. This is consistant with F=2 ^{87}Rb.

Lifetime in Science cell @ 52.3cm MOT load = 0.5V

run 5236cm87RbF1Lifetime.txt



^{87}Rb (F=1)
Science Cell
$\tau = 6.1(7)$

* 2→2 AOM was "on" all the time for these data

MOT LOAD	^{87}Rb	2.6V	13.3s
	^{85}Rb	1.7V	6.1s
	^{87}Rb	3.0V	13.6s
	^{87}Rb	2.8V	13.8s

Relock MOT laser

We replaced the the first beam cube after the fiber with one that is polarizing. This seems to fix the problem of the MOT moving when the fiber polarization changes.

After a power balance and slight MOT alignment correction,

MOT load = 2.4V, τ = 15 seconds

A strange aspect of picture taking, the magnetic trap has two hills in the x-cut, like

"the butt"

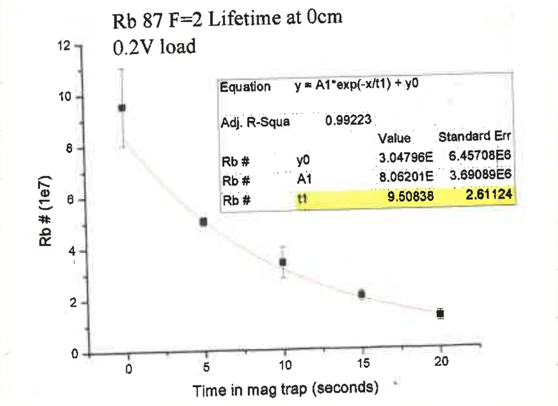
This structure goes away with ≥ 1 sec of wait in magnetic trap, [^{87}Rb] ~~on~~ but doesn't seem to be affected by loading parameters.

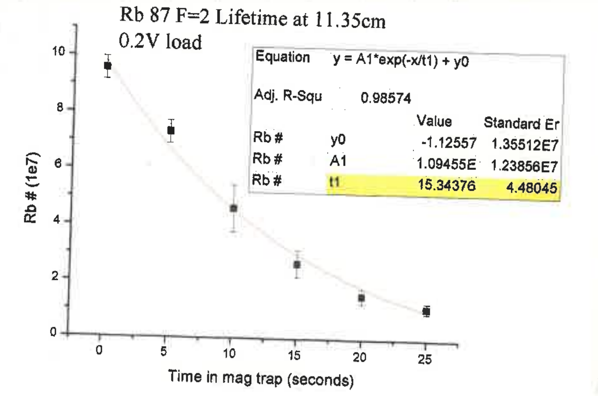
I'm going to look at $^{87}Rb_{F=2}$ lifetimes at various positions...

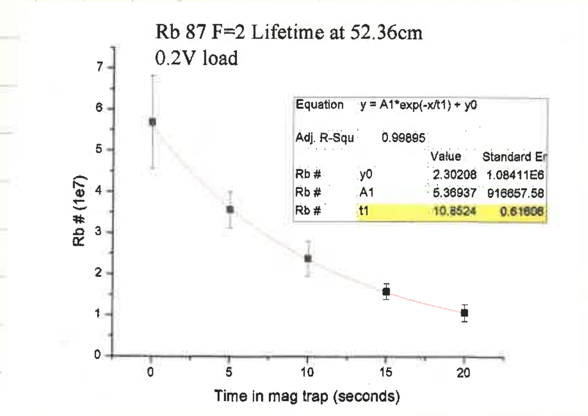
run

0cm87RbF2Lifetime.txt
11.35cm87RbF2Lifetime.txt
52.36cm87RbF2Lifetime.txt
~~40cm87RbF2Lifetime.txt~~
~~30cm87RbF2Lifetime.txt~~

4 averages, MOT cell abs imaging
0.2V load



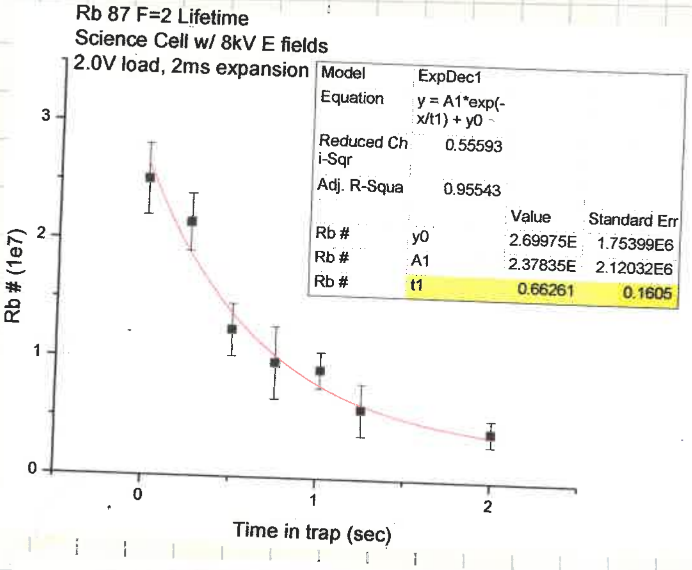




[New batch of gas]

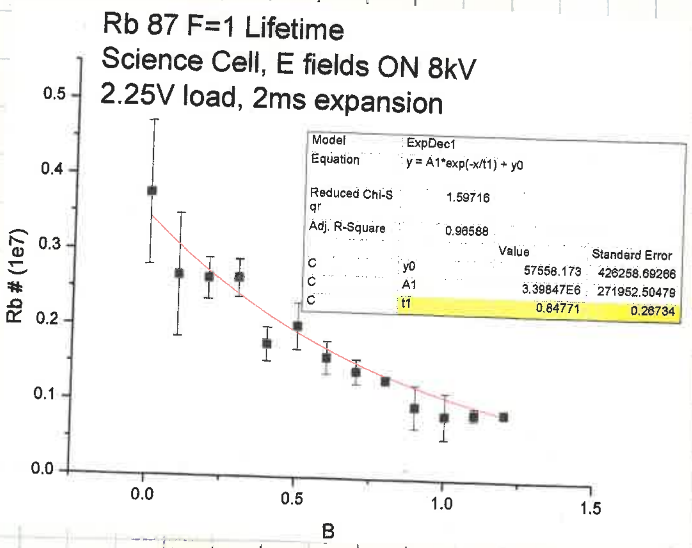
Jan 05, 2010

Now that ^{87}Rb (F=1,2) is tweaked up we want to nail down actual collision parameters (Rb lifetime, initial number, etc.). I'll start with Rb lifetime in the science cell, the rest of the experiment is running as normal: valve and laser on etc. [sirah laser shutter always in the way], E-fields are ON [±8 kV], imaging via MOT cell absorbtion.



Having some imaging [camera] problems, half the image is being cut off intermitantly.
→ avoid trap times of exactly 1 second.

} 3 averages

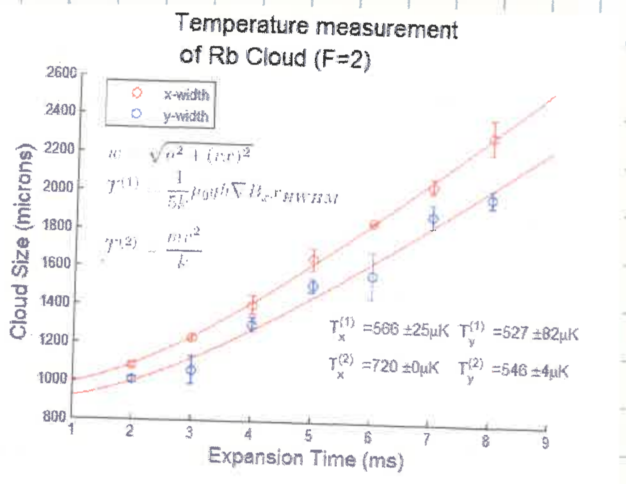


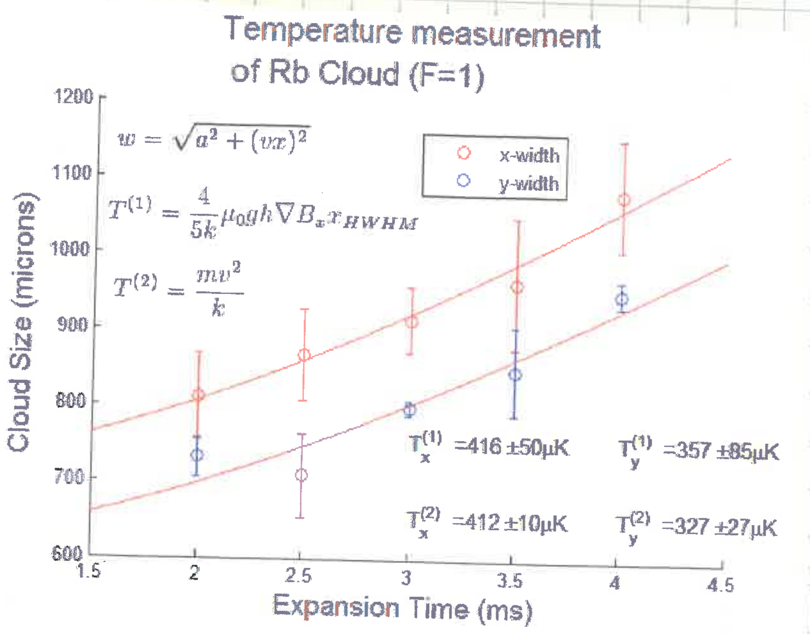
← Recall that F=1 atoms take a larger initial hit from the E-fields

Now to measure trap temperature for the F=1 and F=2 ^{87}Rb states, ~~still~~ in the presence of the E-fields. Still using MOT absorbtion. Measuring after 500ms in trap w/ E-fields

run	Rb Expansion_F=2.txt	^{87}Rb	← after 500ms in E-trap, 2.0V load, 3 avgs
	Rb Expansion_F=1.txt		← after 250ms in E-trap, 2.5V load, 3 avgs

see plots on next page.





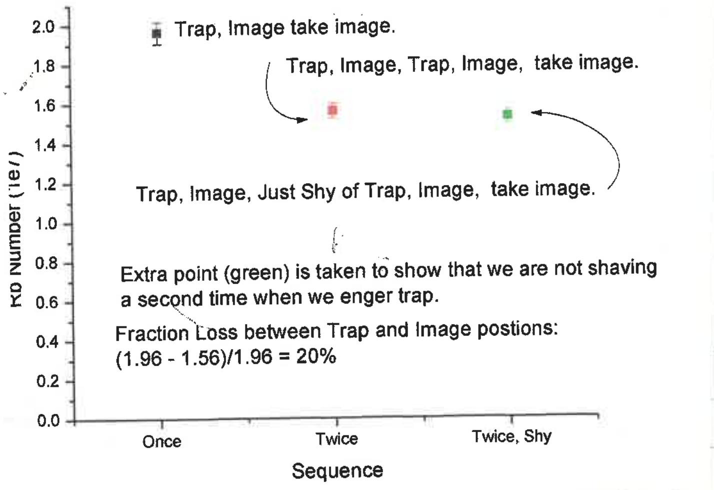
Temperature measurements of ^{87}Rb after exposure to electric fields.

We want to see how much Rb we lose during transport from E-trap to imaging regions.

to do this we will compare numbers in the following 2 sequences:

1: move to TOFMS, move to image, take picture

2) move to TOFMS, move to image, move to TOFMS, move to image, take picture



shy point is 50.36 cm

load = 2V
time in E-trap = 0.5 sec
E-field = 8 kV
Expansion = 4 ms
^{87}Rb F=2

6 Jan 10

^{87}Rb MOT load $\tau = 14.3$s $A = \cancel{2.6}$ 2.8 V
^{85}Rb $\tau = 7$s $A = 1.73$ V

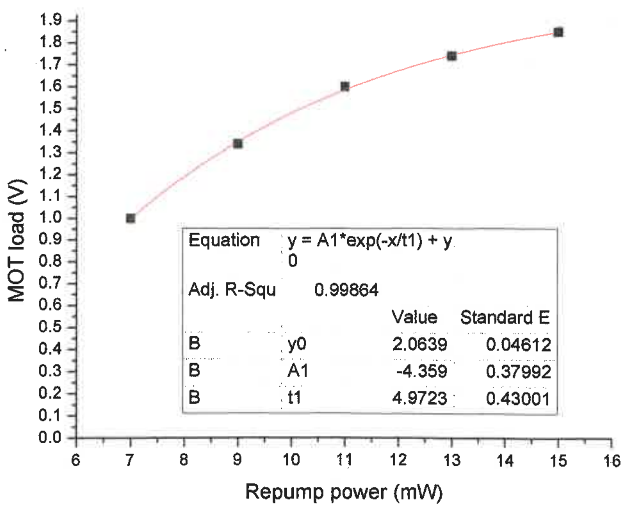
We changed a few things with the Repump laser

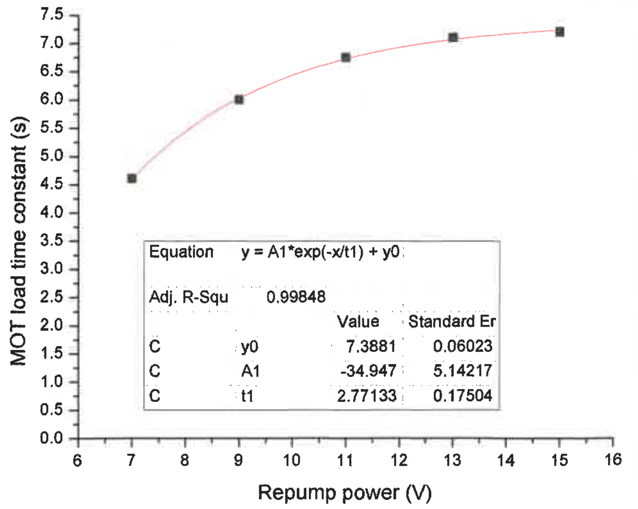
1) Replaced 2nd opto isolator with a nicer one - reduced feedback from fiber into laser

2) Moved Rb cell & Opto-isolator to reduce B-field broadening on Sat-spec.

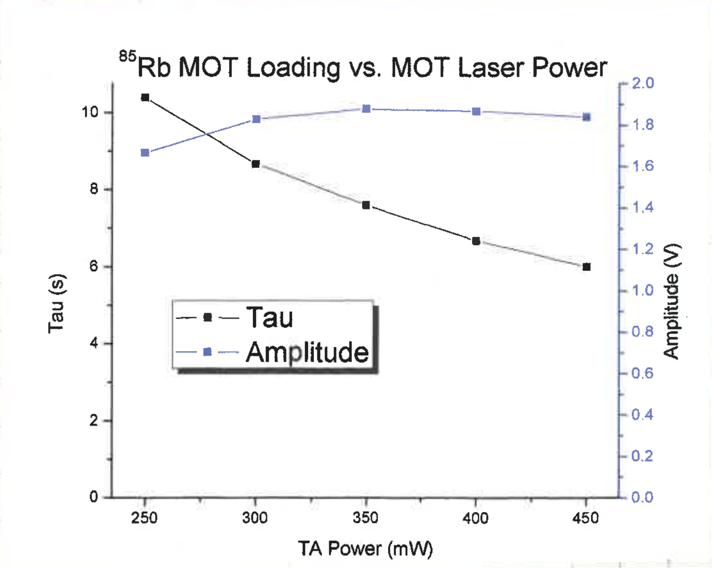
3) Put in ND filter to reduce power broaden in sat-spec.

^{85}Rb





We could also ...



It appears that we may have some light assisted collision in the MOT for 85

9 Jan 2009

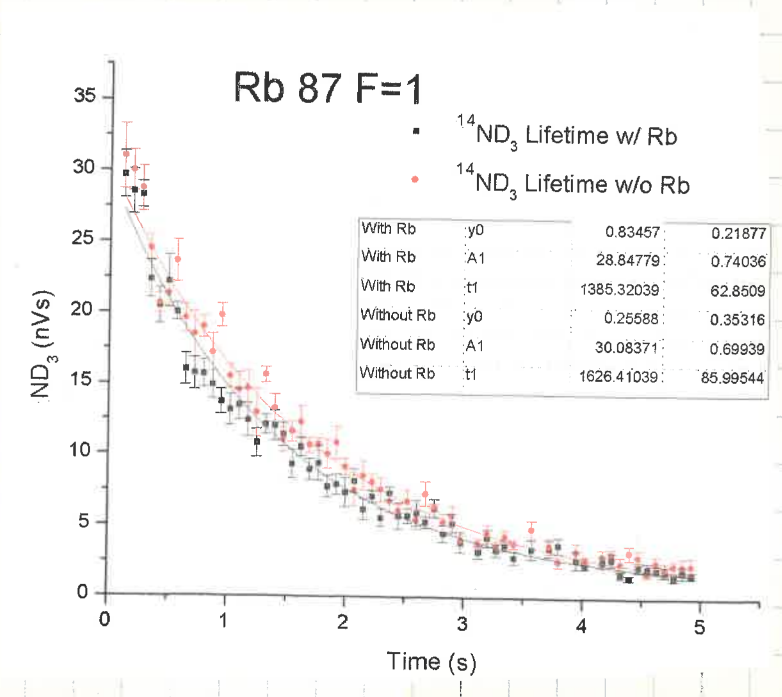
Laser 31511 cm⁻¹ 16mJ
trap 8kV
TOFMS 1200V / 800V

MOT Load = 1.7V
Mag Trap = 500Amps

^{87}Rb (F=1) N_0 = 3.9E~~7~~6 ±0.5 (by absorption imaging)
^{87}Rb (F=2 N_0 = 4.2E7 ±.6 0 sec 8kV

MCP = 5000V
Anode 290V

"trap On" = 8 sec (e-static)



Water cooled cable sheered at the bott ~~(top)~~ (bottom coil)

12 Jan 2009

85 MOT Load = 2.2V
τ = 7.4 sec

found a bug in the programming on saturday (11 ms wait after cmot was not enforced. We are still fighting w/ 85 picture taking consistency. Perhaps fixing this bug helped. We will now find out.

Absorption Imaging

for reference 87 give N = 1.38 ± .102 5 Avgs 7%
N = 1.31 ± .083 10 Avgs 6%

85 give N = .96 ± .138 10 Avgs 14%
↑ stretch states ↑ $\times 10^8$

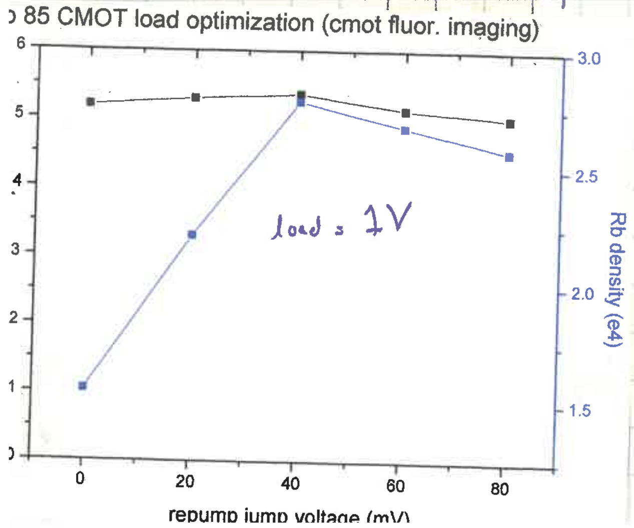
for 87 Load = 0.35
for 85 Load = 0.5

13 Jan 2009

Fixed a couple of labview timing errors in the CMOT fluorescence imaging vi. Many parameters did not save correctly: CMOT location ≠ mag trap location, triggering problems etc.

Jumping repump optimization... implemented Noah's "jump voltage output on high input, ground on low input" DAC box to jump the MOT laser.

MOT Jump = + 100mV for Rb 87 (655 = 1010001111 in box)
+ 40mV for Rb 85 (262 = 100000110 in box)

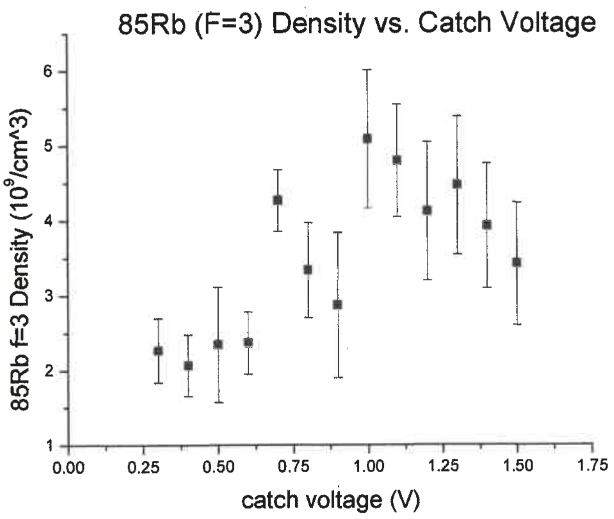


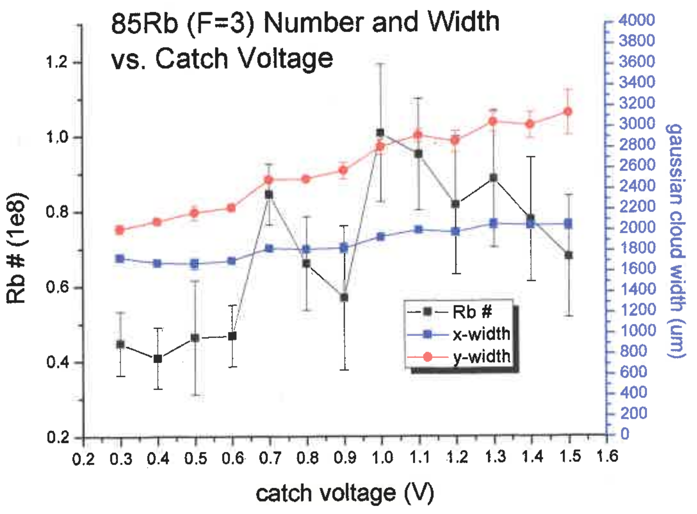
Repump jump set to 45 mV.
Subsequent absorption imaging shows identical signal : noise as yesterday (14% after 10 shots).

Next I'll optimize the catch voltage, still ^{85}Rb, f=3 : MOT cell Abs imaging,

0.35 V load, 7ms expansion time [max OD ~ 1]

taking 4 averages





4 averages, no time waiting in mag. trap.

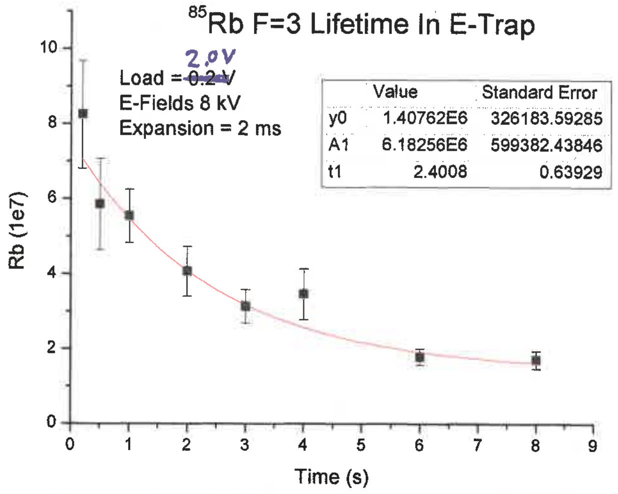
Optimal catch voltage ~ 1 Volt

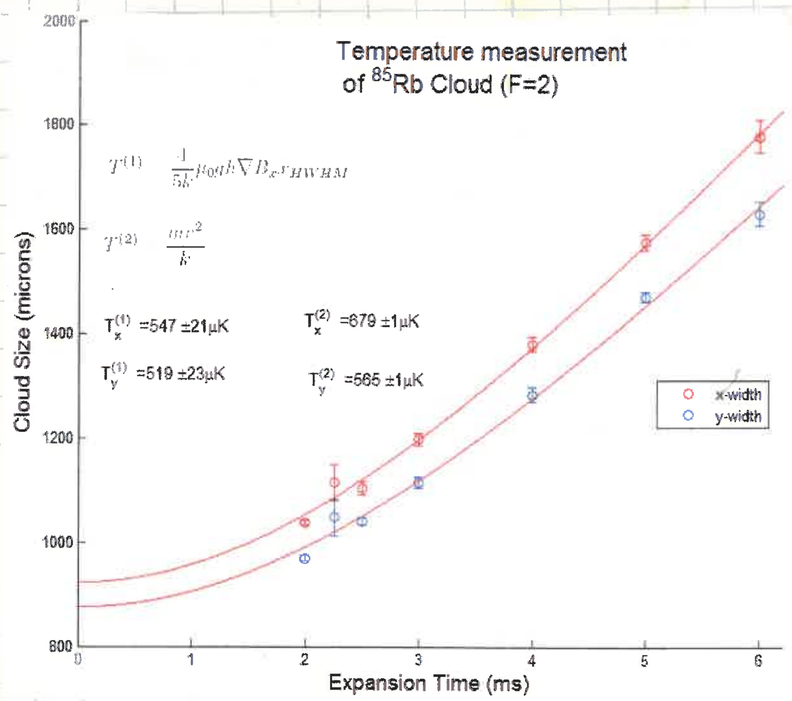
14 Jan 2010

measuring lifetime and temperature of ~~85~~ 85Rb F=3 in the science cell w/ Efields on

Efields 8 kV   Expansion 2ms   Load 2V

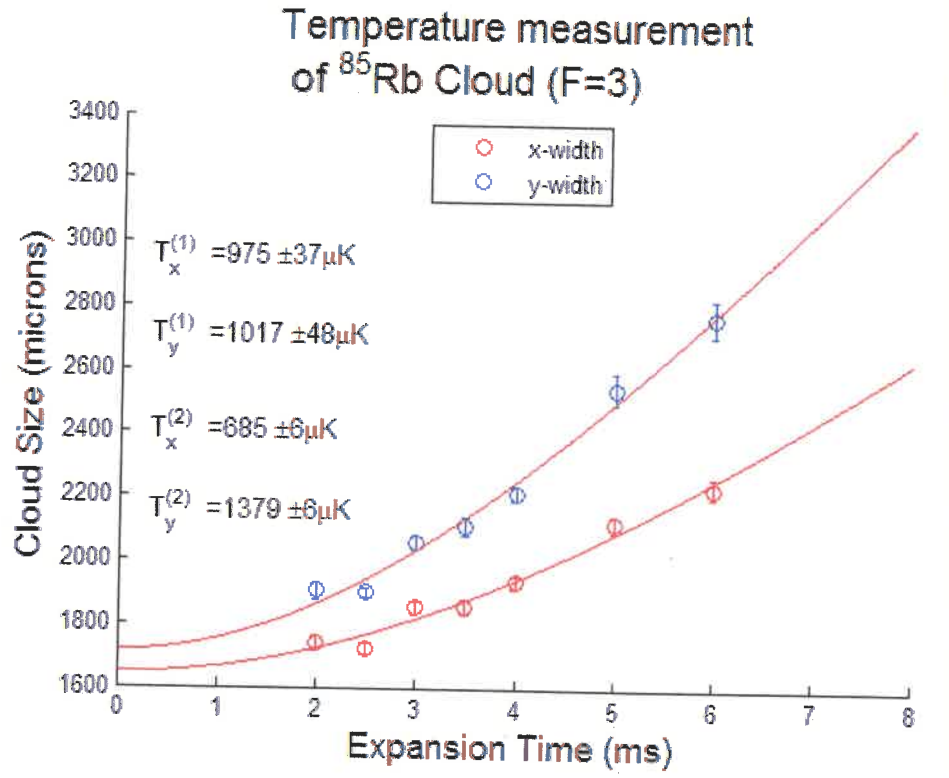
Absorption imaging in MOT cell.





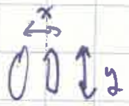
2.0V load, 750 ms wait in E-trap w/ ±8 kV fields
MOT cell absorbtion imaging
10 averages/point

← After encountering high electric fields

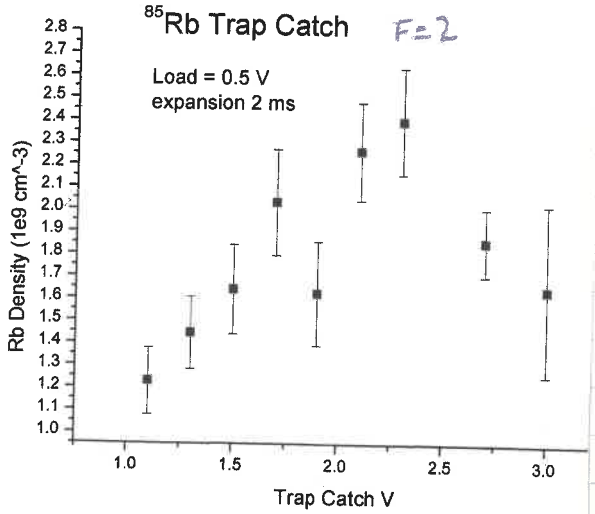


Temperature before entering E-trap

load 0.5V
wait 5sec in trap

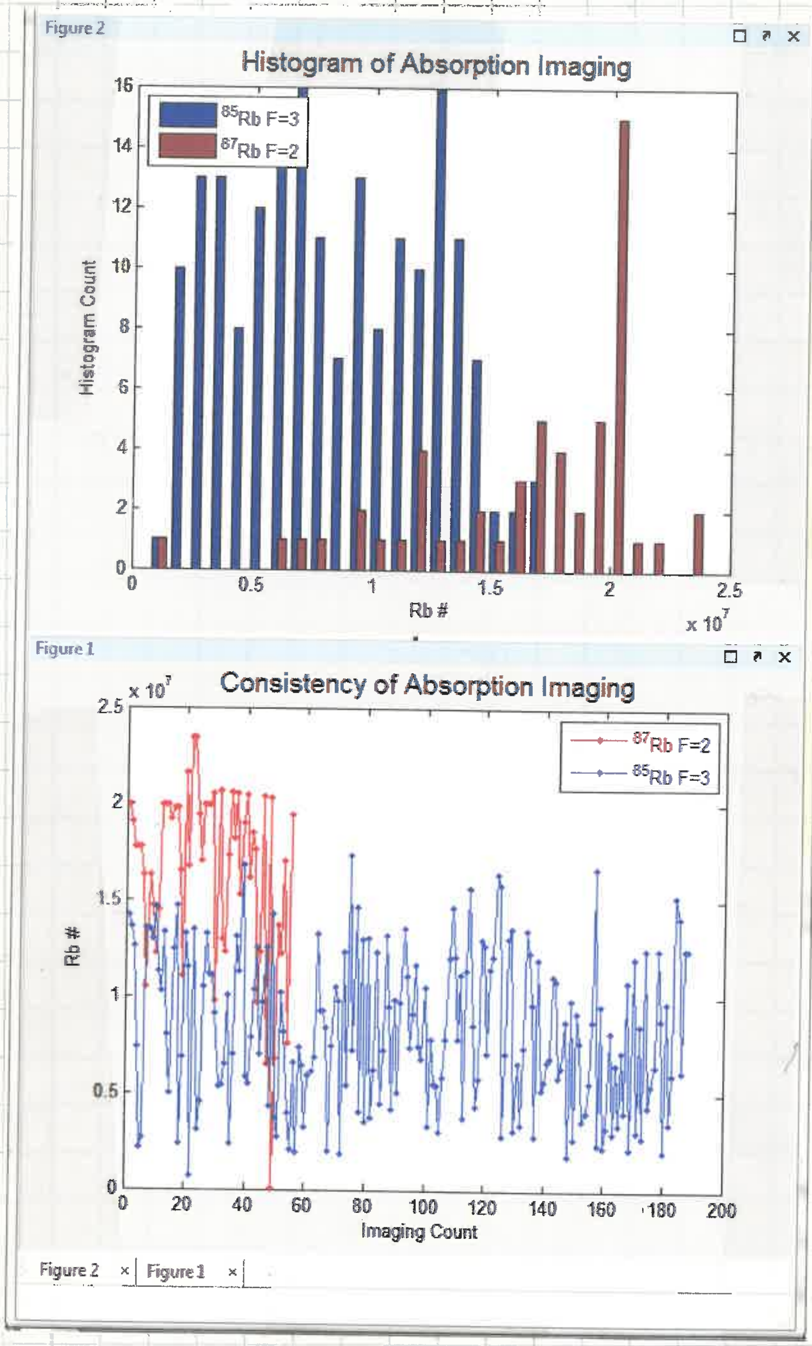


there is still a "butt" in the trap. The butt is in the x-dim



15 Jan 2009

looking at the instabilities of ~~85~~ ^{85}Rb absorption imaging.



2V load, into E-trap for 200ms then take MOT cell image for 85

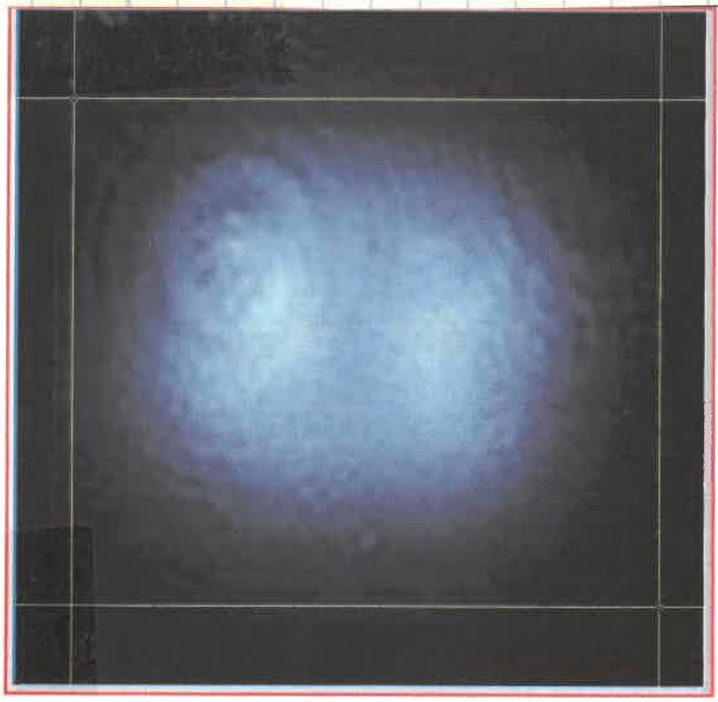
0.4V load into E trap for 2s then mot cell image

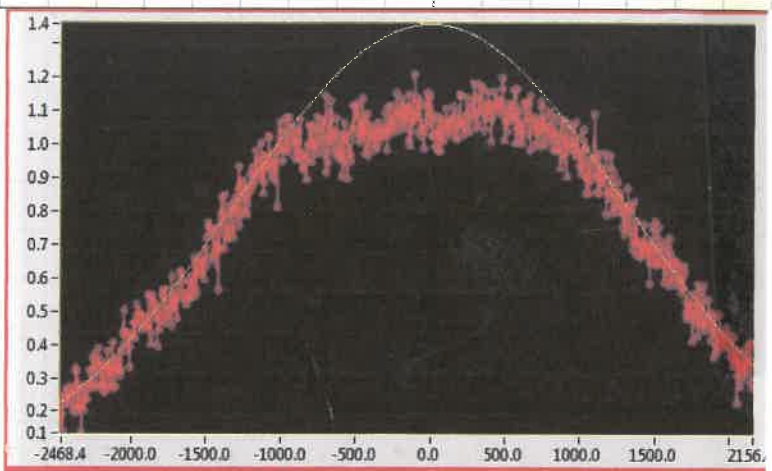
^{87}Rb F=2 ~~std~~ mean : 1.7E7
std : .47E7
28%

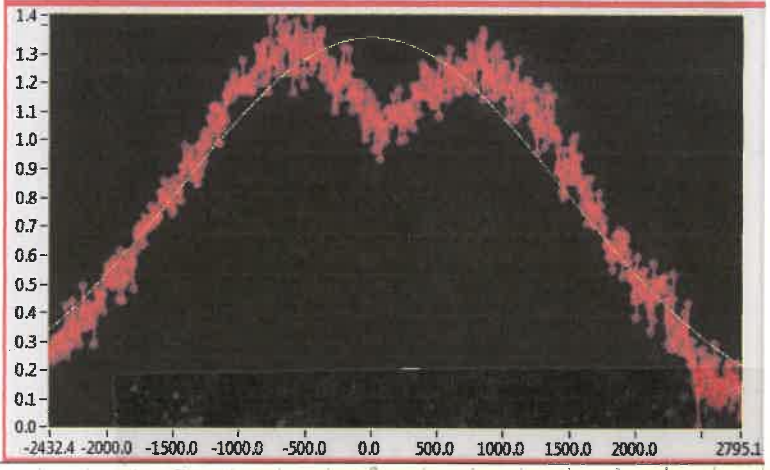
^{85}Rb F=2 mean : 8.3E6
std : 4.5E6
48%

18 Jan 2000

we are trying to figure out the ^{85}Rb noise and "Butt"







Removed OP coils, no effect.

Blocked bot ~~2~~ 3->3 and OP light: -> 1.45E8 ± .1 = 7% after 10 averages

Blocked 3->3 light: 1.21E8 ± .06 = 5%

Blocked OP light: 1.05E8 ± .094 = 9%

Neither Blocked: 1.05E8 ± .099 = 9%

$\boxed{\text{load = 0.4V, wait 4 sec in trap}}$

3->3 is contributing to a lot of the noise

~~try~~ tried not opening 3→3 shutter : 7.7%

not opening OP shutter : ~~8.0~~ 10.6%

did better job blocking 0-mode out of MOT AOM :
~~a~~ no light blocked ~~10.~~ 11%

TA Power 20% (102 mW) 2.29 ± .18 (E^{+8})
90% (454 mW) .589 ± .05 (E8) } load = 0.2V.

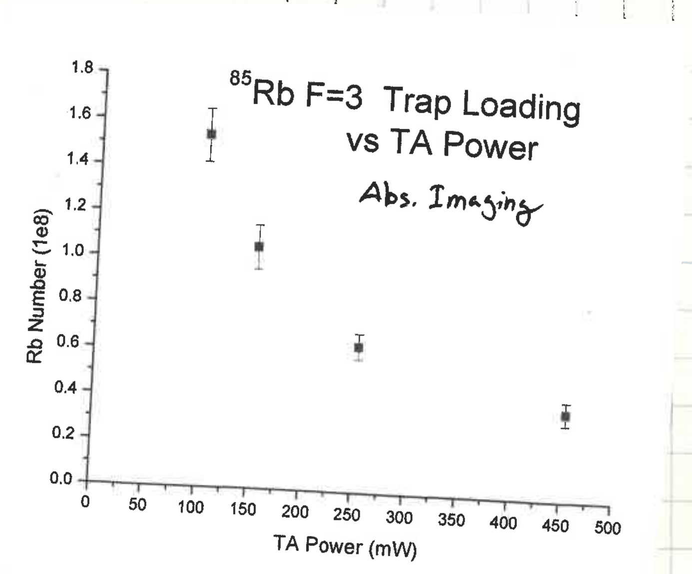
TOO MUCH TA POWER !!!!

Possible "MOT butt" progress: (^{85}Rb, F=3)
Fiddling with the magnetic trap ramping [less steps: 1000→250, and longer time: 50ms → 200ms] as well as adjusting the MOT absorbtion imaging position [there are 2 interference rings that seem rather consistent and amplify the butt curvature feature] seems to have banished the butt.
I'll now re-introduce optical pumping and 3→3 light.
Also re-installing the optical pumping coils.

slight butt-ness still present, but stability seems slightly improved. ~7% everything on.
[^{85}Rb, F=3, 0.5v load]

19 Jan 2010

This morning, the butt still exists and the noise is still on the order of 12-13%, using same parameters as above.



we are not doing a good job w/ the CMOT?

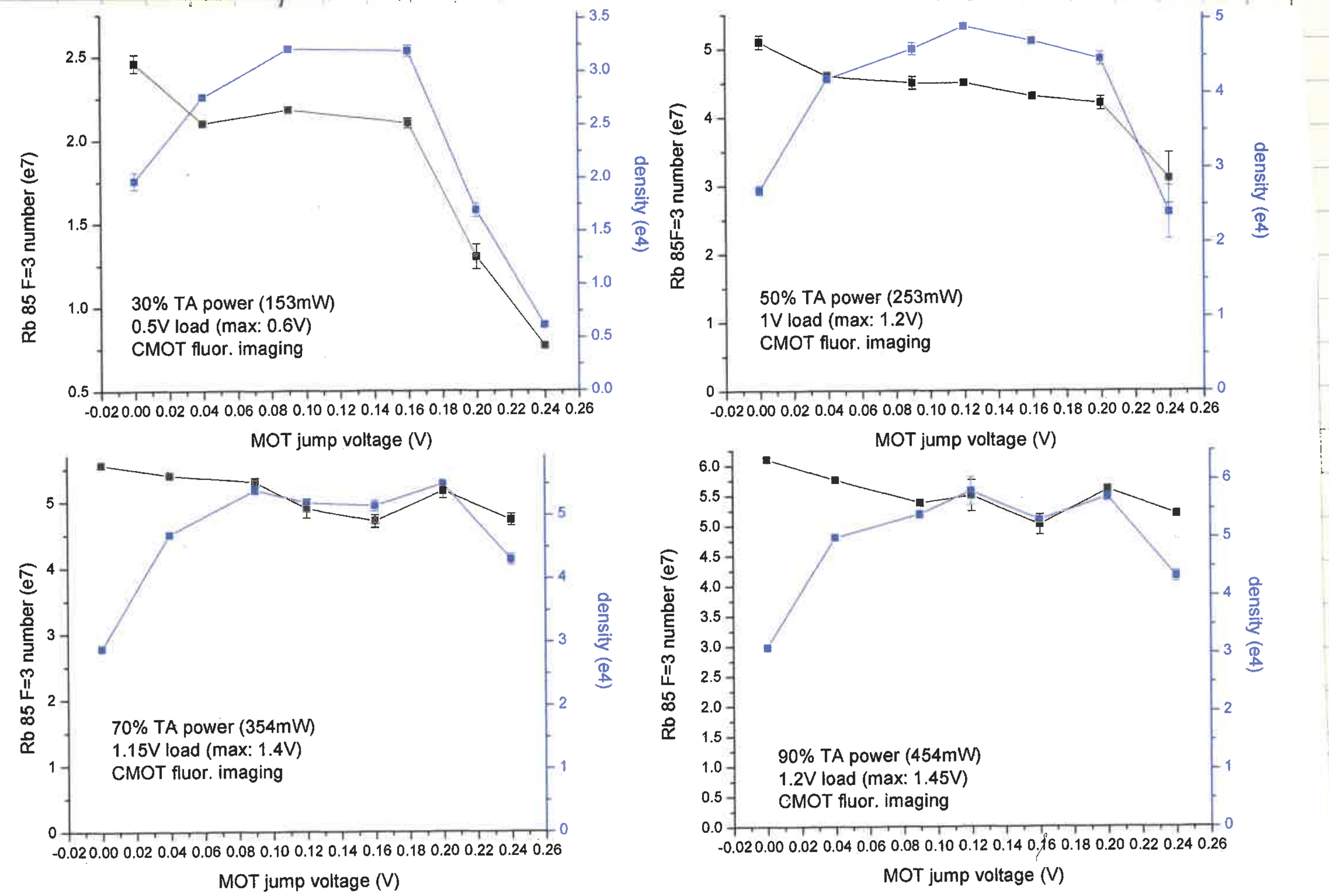
Note: due to a labview programming error, MOT was not being jumped.

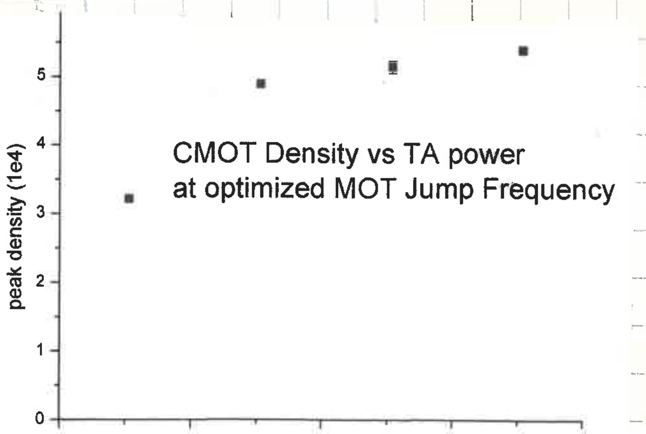
Looking again at CMOT images optimization of MOT jump.

At full MOT laser power (454mW), there is no effect from jumping the ~~CMOT~~ MOT.

At lower laser power (150mW) there is an effect. This suggests that we may be power broadened?

We need to optimize mot jump at different ~~b.~~ MOT laser powers. To try to be consistent, we will load to 83% of the max MOT load for each laser power.



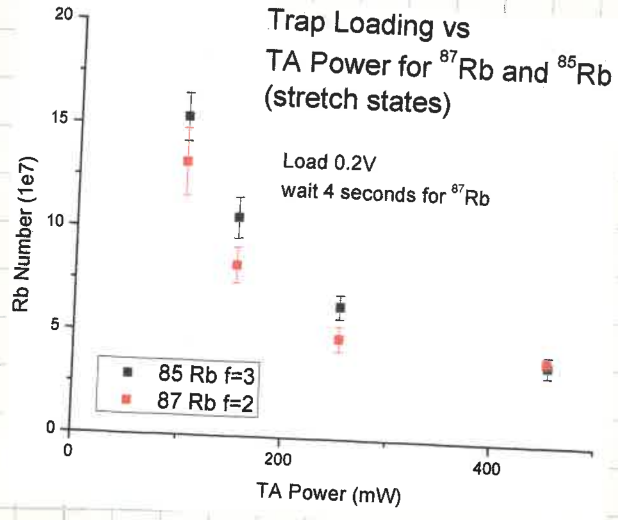


MOT Jump = 0.1

The different MOT power plots may be misleading because fluorescence imaging uses MOT light to detect atoms ⇒ less MOT light = less detected.

the CMOT Flourescence imaging data shows that the optimum jump ≅ 0.1 V, for all TA powers. However even at optimum jump we still get more ~~trapped~~ trapped atoms (via abs. imaging) at lower laser power.

Just out of curiosity, do we also see this w/ ^{87}Rb?



I tried turning off the TA during the 4 sec wait for ^{87}Rb to see if we had light-dependent lifetime from the TA. This had no effect (at 454 mW)

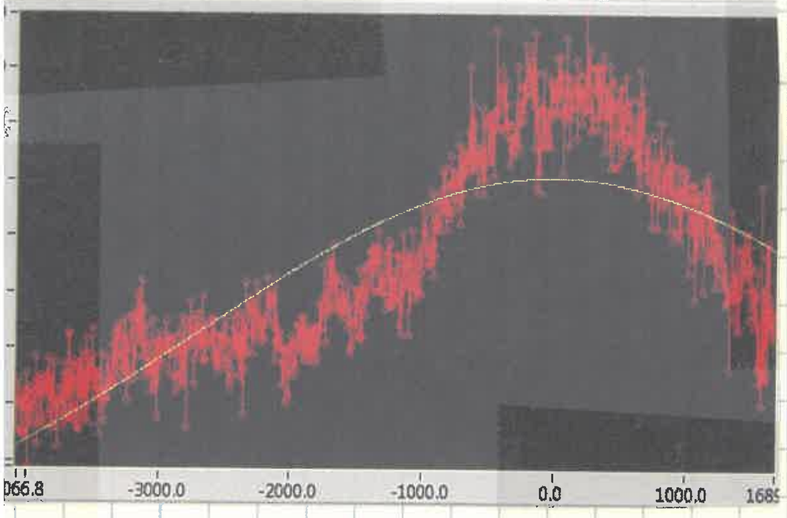
$\boxed{\text{20 Jan 2010}}$

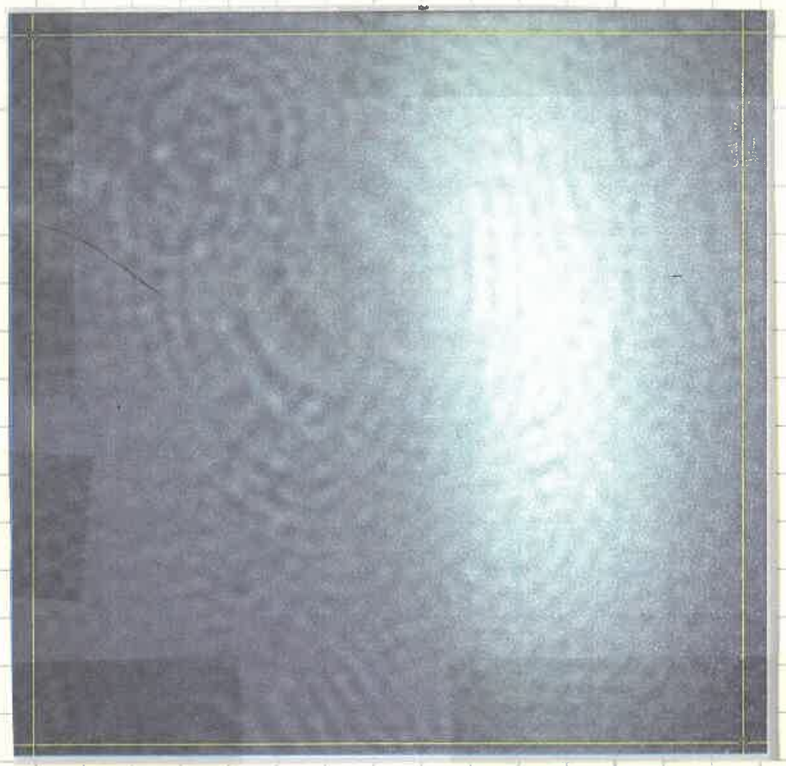
~~What's this~~

Trying to figure out the butt. Is it a coil turning off issue? We will ramp down coils and then take pictures.

We ramp the coils down just before Abs imaging. There is ~10ms delay between finishing the ramp and taking the picture (labview timing)

We have ramped from 1 ms to 50 ms





5ms ramp

Above ~30 ms ramp time, ~~the~~ image looks ~~is~~ as like a "nice Gaussian". Below ~30 ms the we start to see ~~pic~~ lopsided images like the one above. It is bimodal between good pictures and lopsided pictures

Replaced Gate resistors for bottom coils from 10 to 20 ohms
left 10 Ω resistors in top coil gates.

This resistance increase seems to have mostly gotten rid of the problem. The absorbtion images still show a bit of a flat top, with (maybe) an occasional small butt. Changed top coil gate resistors to 20Ω as well, no noticeable improvement. No sign of oscillations with this slightly higher resistance [recall ramp oscillations occurred with 100Ω resistors, pg 142 in "Rb book"].

Another change to coil control feedback circuits, changed proportional gain from $1K/2K = 0.5$ to $4K/2K = 2$. This seemed to slightly stabilize the coil turn on behavior.

result:

Absorbtion images still seem a little bit flat-topped, but no butt and the X + Y widths now agree to within a few %.

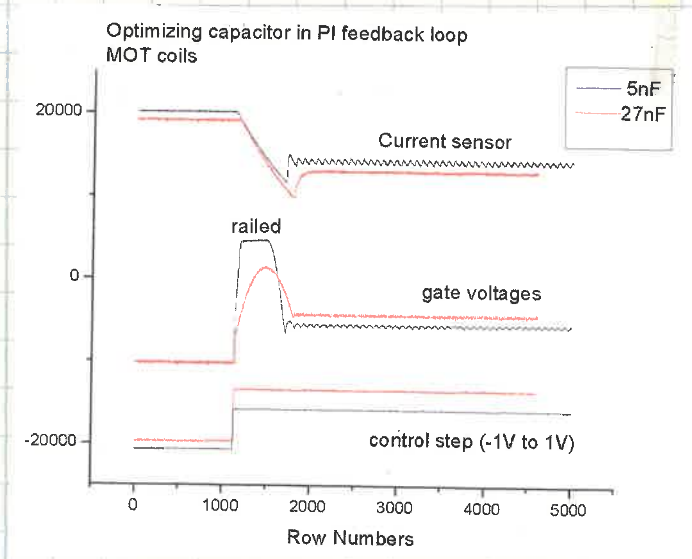
Consistency still poor: 10 shots ⇒ 12% std dev of the mean.

Unfortunately, our integrating capacitor is 56 nF ⇒ $f_{integrator} \simeq 700$ Hz which is very slow. We want to keep this value below the coil resonance [$\underbrace{\text{coil inductance } L}_{\sim 1-10\,mH}$ + mosfet source-drain $\underbrace{\text{capacitance}}_{\sim 5\,nF}$]

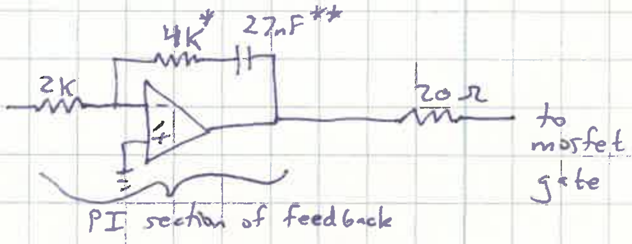
21 Jan 2009

After seeing a funky * expansion time - temperature measurement set, we changed the capacitance in the PI loop from 56nF to 27nF. ~~which there~~

[arb scale and offset]



original value = 56nF = slow
5nF → oscillations + railing

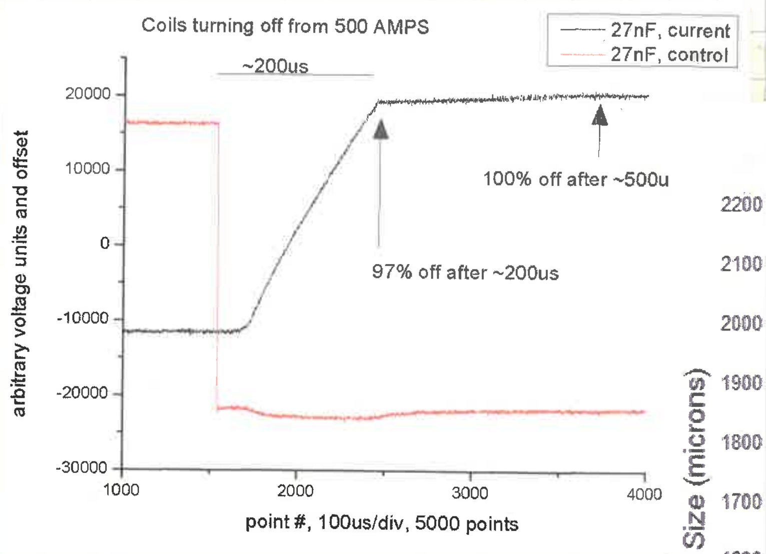


* old value was 1K
** old value was 56nF
- both circuits use these values -

5000 points taken (1ms/division)

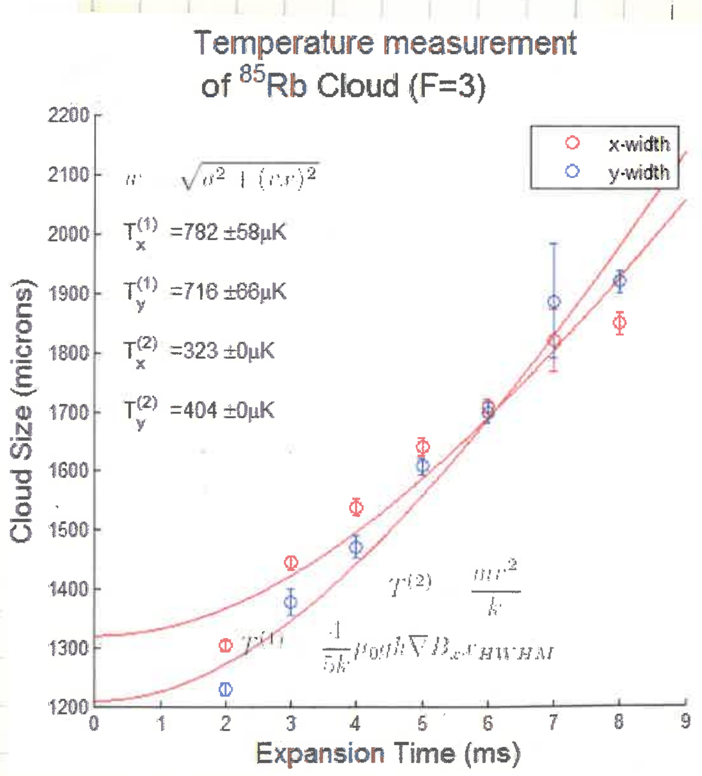
field jump to ~ 100 amps before ramping up to 500 Amps.

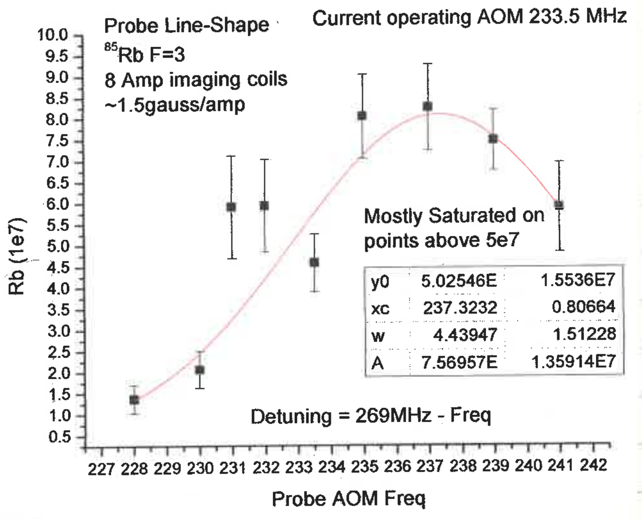
With the new circuit values, current turn off [over home position] looks like:



expansion after coil servo modifications →

0.2V Load





$\boxed{\text{22 Jan 2010}}$

Yesterday there was a lot of playing w/ magnetic fields. We still have a problem x- and y- widths not matching for ^{85}Rb. I want to see if this is also a problem for ^{87}Rb.

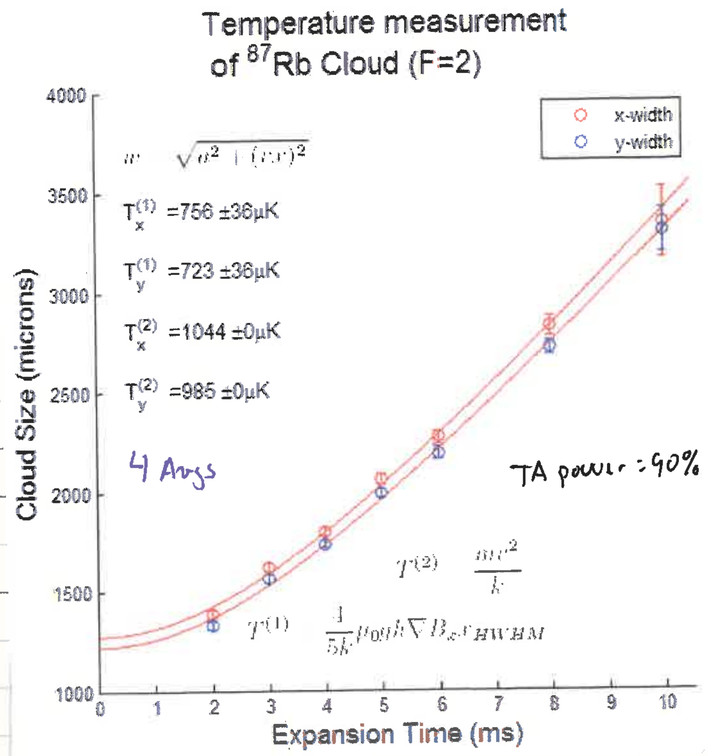
the widths for ^{87}Rb match much better.

I re-aligned the probe laser to center it on the imaging lenses - it may have been ~~bumped~~ bumped during the last major MOT realignment.

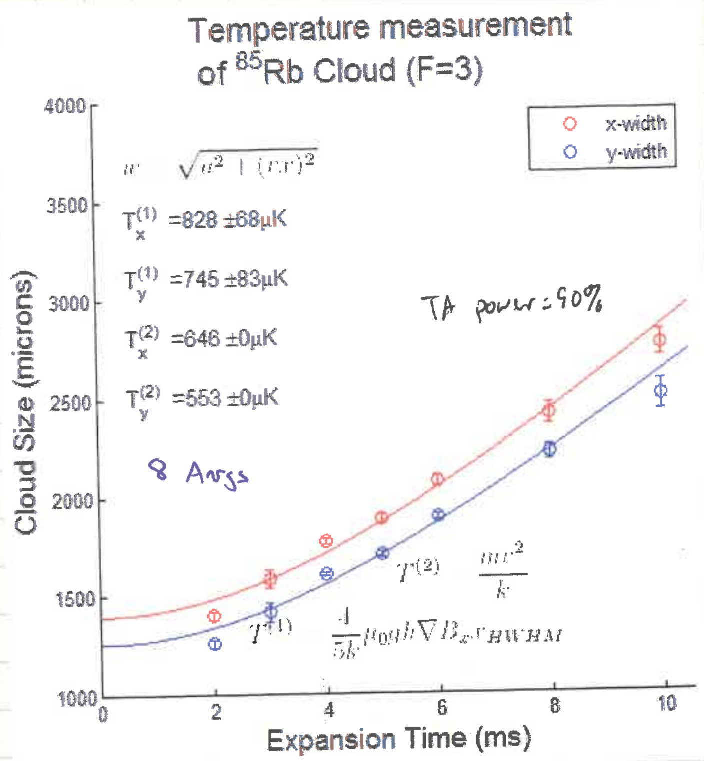
this took care of the very annoying diffraction spots we were seeing!

Now taking temperature measurement of ^{87}Rb F=2.

Rb87F2_expansion.txt

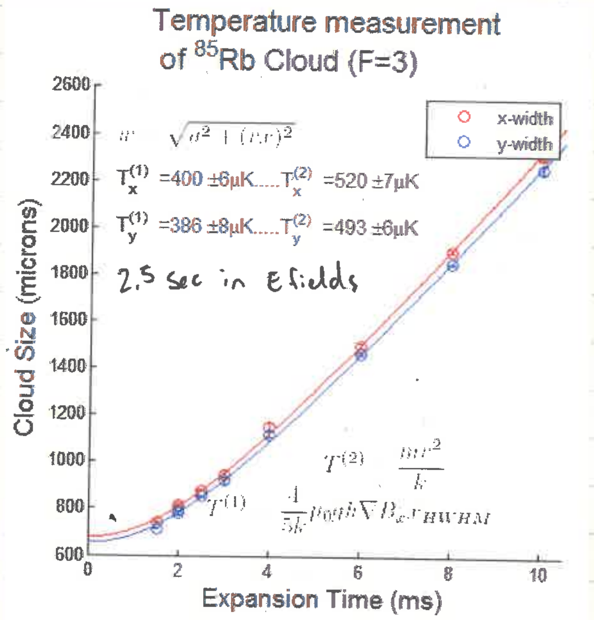


load = 0.4V
no E-field
time in trap = 10sec



tA power = 30%
Load 0.35V

~~Load~~ =



Changed capacitors (** on pg 33) to 39nF. Gives better turn-on response. Turn-off is still a little slower for bottom coil (couple μs difference) looking at magnetic trap size vs TA power, after exposing atoms to 8kV E-field for 2 sec. Loading to 70% of max.

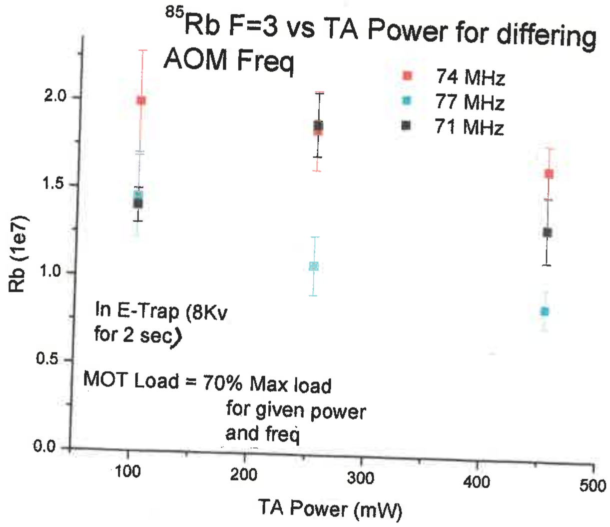
TA power	Max load	load level
153mW	.9	.712
454mW	1.3	.91
254mW	1.2	.84

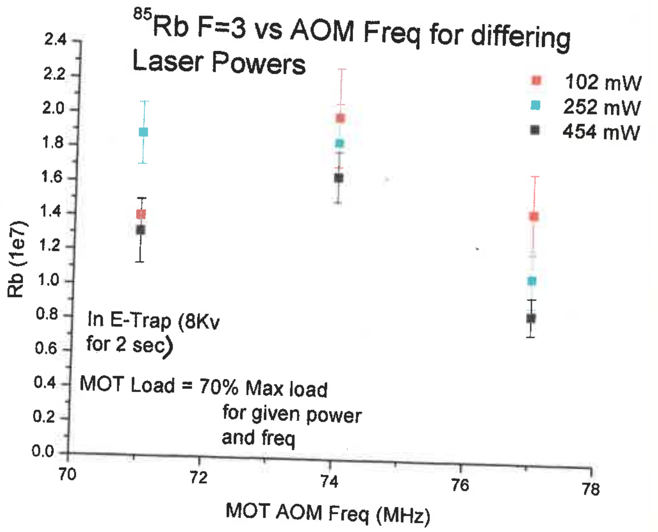
Optimizing TA power, detuning, mot V

[strong field = 490 amps]

mW	MHz	V	max	load
(90%) 454	74	.07	1	0.7
(50%) 254	74	.07	.9	0.64 ←
(20%) 102	74	.1	.33	0.23
102	77	.07	.58	0.4
254	77	.06	1.18	0.83
454	77	.05	1.25	1
454	71	.08	0.507	0.40
254	71	0.1	0.417	0.29
102	71	0.13	0.159	0.11

10 Avgs





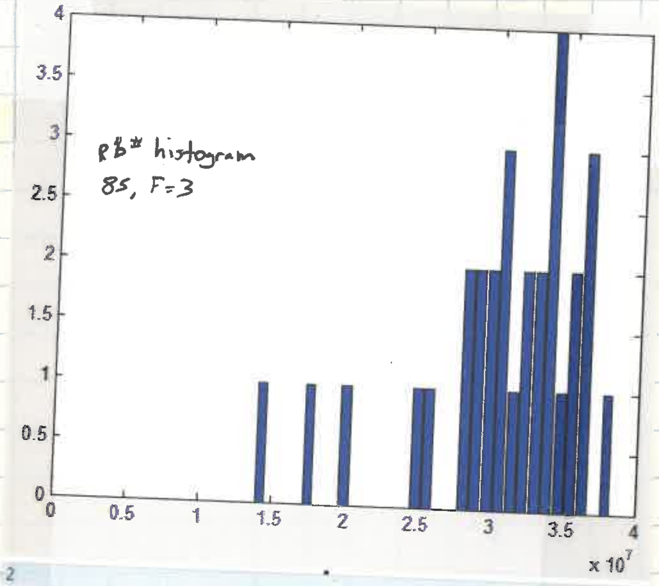
Choosing 254 mW [50% power on TA] and 74 MHz MOT AOM frequency

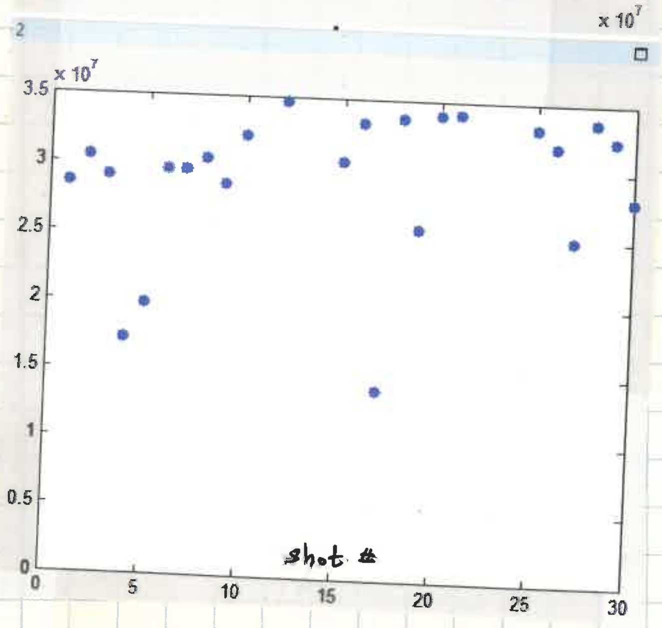
Working on stability of $^{85}Rb_{F=3}$ absorbtion imaging.

With the Rb 87 timings, optical pumping of F=3 ^{85}Rb seems to do nothing. Increasing optical pumping time from 100μs to 400μs shows some improvement. Rb # is now very bimodal, the "good" shots are ~ 2×10^7 after 6 seconds in e-trap with 0.35v load. "Bad" shots are ~ 6×10^6. 3→3' lock seems to be drifting slightly, may be correlated with the bad shots.

Re-aligning 3→3 beam and reducing pumping time back to 100μs... then went back to 400μs length

$\boxed{\text{Stability greatly improved}}$ $^{85}Rb_{F=3}$





The ~~~~ stability and magnetic trap "butt" problems of ^{85}Rb seem to now be fixed.

Problem	Solution(s)
Butt	• improve coil feedback settings. • re-align abr. imaging probe laser to camera
stability	• re-align 3→3 beam, • optically pump for 400μs. (was 100μs)

maybe not, instability returned next morning.

26 Jan 2010

MOT load ^{85}Rb: 1V load, 6.71 seconds = T [at 0.07 V]

Plan for today is to verify $^{85}Rb_{F=3}$ absorbtion picture stability, then tweak up optical pumping parameters, trap catch voltages, and check $^{85}Rb_{F=2}$ consistency.

Inconsistent and low $^{85}Rb_{F=3}$ pictures have returned...
optical pumping is achieving positive results but also introduces a lot of noise

Switching to absorbtion imaging of $^{85}Rb_{F=2}$ shows a bipolar behavior, but "drop-outs" are zero. About 1/2 of the points are dropouts. Fluorescence imaging does not show any dropouts after 12 points, so instability seems to be due to the absorbtion imaging sequence.

After restarting the camera...

$^{85}Rb_{F=2}$ w/ no E-fields down to science cell and back = no dropouts [passive box mode]
$^{85}Rb_{F=2}$ w/ no E-fields down to science, wait 500ms, back = no drop-outs [active mode]
" " with E-fields on = no drop-outs
last but 0 time in trap = very rare (~1/15) dropouts, sometimes more
back to 500ms in trap = dropouts becoming more common.

may have been an unhappy repump

Back to fluorescence, ~2 dropouts in 100 shots.

$\boxed{\text{27 Jan 2010}}$

^{85}Rb F=3 Fields on 8kV, but grounded master pulse; moving into trap
Standard dev. ~~0.00000~~ 1.47 E7 } 42%
mean 3.5 E7
not sure of 3→3 lock stability, actually I'm sure it sucked.

1- 0.4
50%

^{85}Rb F=2 no fields, moving into trap 31 pts 85RbF2_hist_0kV
mean = 7.4E7 std = .99E7 13.4%

85RbF2_hist_8kV 0.5 sec in Efield 34 pts
mean = 7.95E6 std = 1.4E6 18%
~~that~~ it looked like there was a drift in the data.
I'm going to try taking it again

85RbF2_hist_8kV_2 — Same as before, looking for the drift — ~~33~~ 33 pts
mn = 6.6E6 std 2.27E6 34%

85RbF2_imageF3 — just want to make sure that we have no F=3 atoms in our trap.

85RbF2_flourImage_8kV — Flouresence imaging of F=2, we can be sure there are no F=3 from the previous run — .5sec in 8kV

there are 84 "drop" count

stats including drops: mn = 1.94 std = .628 32%
stats dropping drops: mn = 2.13 std = .145 6.8%

~~85RbF2_flo~~
85RbF2_fluorImage_NoMov — to see if we still get the drop out points if we don't move the track
This also does not trigger E-fields
No Dropouts in 38 shots
mn = 24.2 std = 1.09 4.5%

By watching the "flash of atoms" during the Fluor imaging, we can see that we get "dropout" shots ~~even~~ even when atoms are present. Therefore it must be a triggering issue w/ the camera and not an issue of lasing or atoms.

Thus far we have shown that we can load exclusively ~~F=3~~ 85Rb F=2 from absorption imaging. And from fluoresence imaging we have a stable molecule count (of ^{85}Rb F=2). This leaves us with absorption imaging being the cause of our instability.

85RbF2_probe_lineShape_2.txt

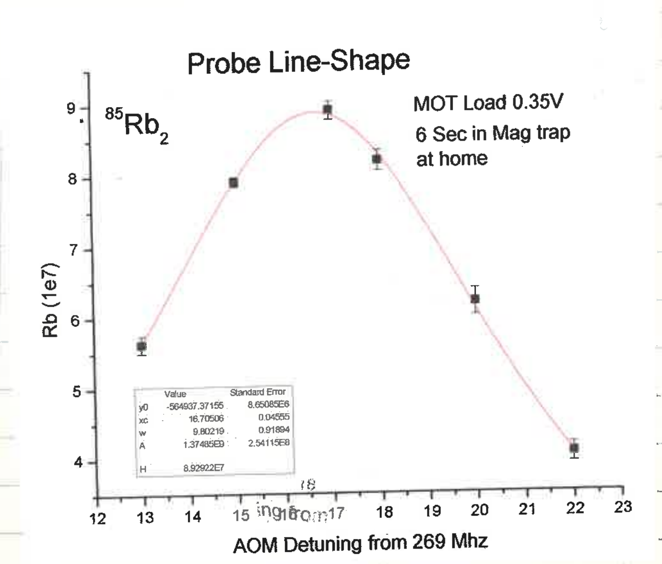
started to take this data, very small errorbars ~2% after 20 pts, probe unlocked and ~~error~~ we relocked but the error bars got big. We played around w/ the lock and got them small again.

Absorption imaging is stable for 85 Rb F=2, except when we turn on E-fields.

We are going to leave E-fields off for now and take a probe lineshape

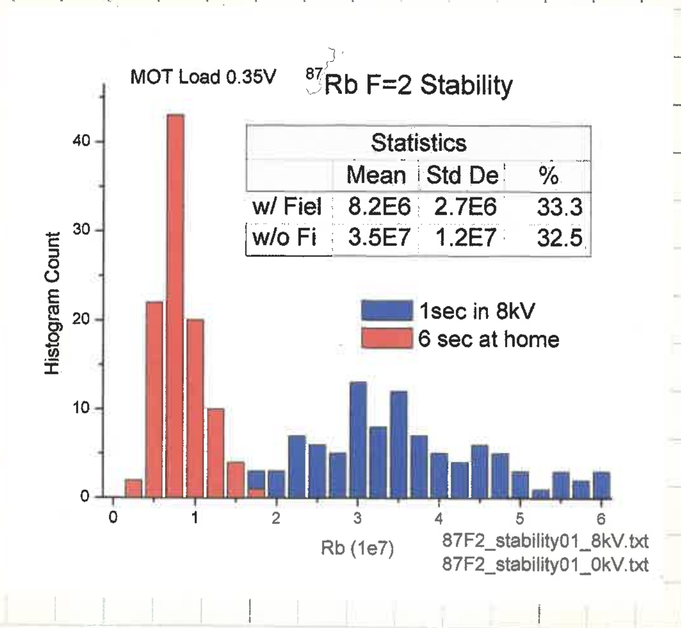
85RbF2_probe_lineshape_2.txt
(probe AOM shutter)

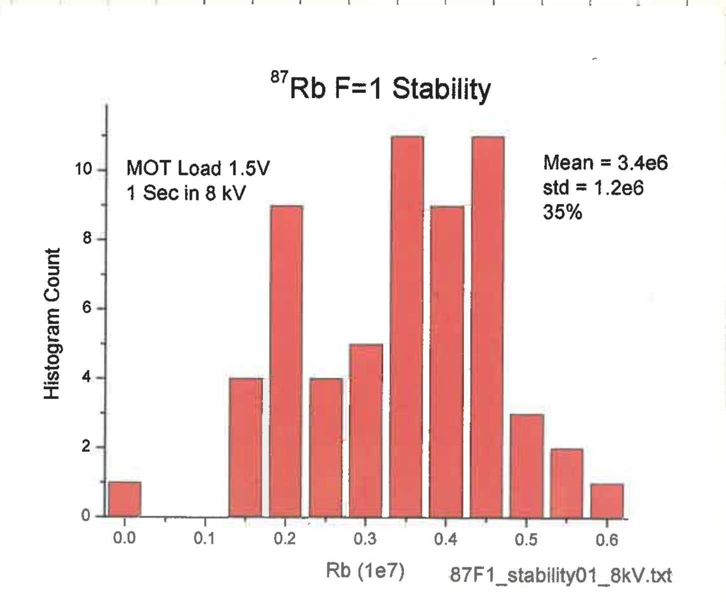
Set AOM to 252.3



28 Jan 2010

Yesterday we had good stability w/ 85 F=2 abs. imaging. No E-fields First today, I'm going to make sure this is still the case.

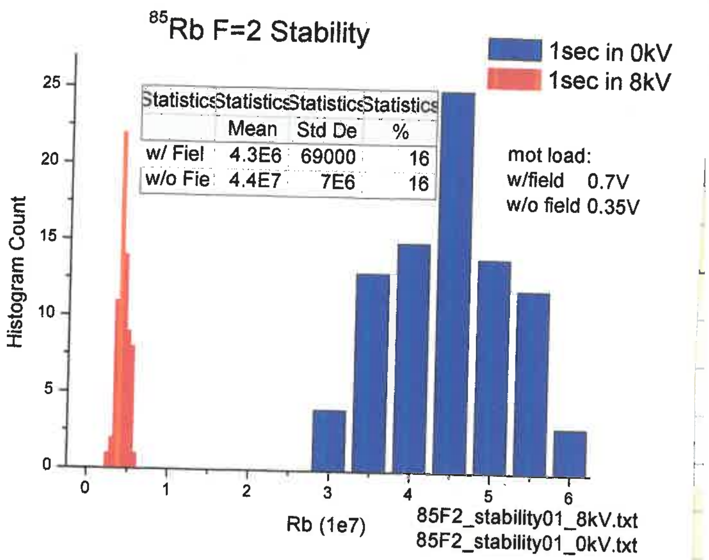


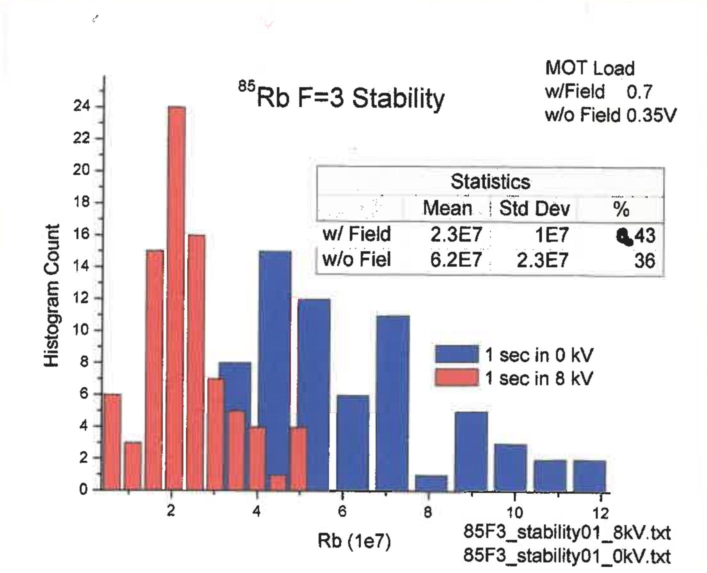


100 pts each

60 pts

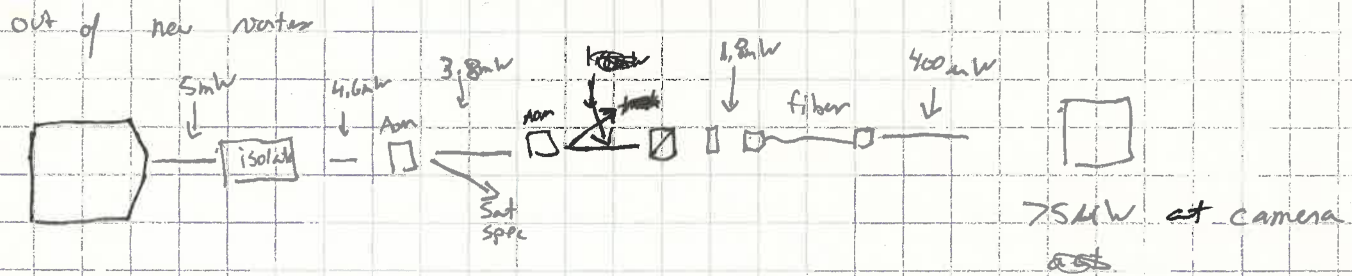
Now we have numbers for the stability of ^{87}Rb, we will do the same thing w/ ^{85}Rb and compare.





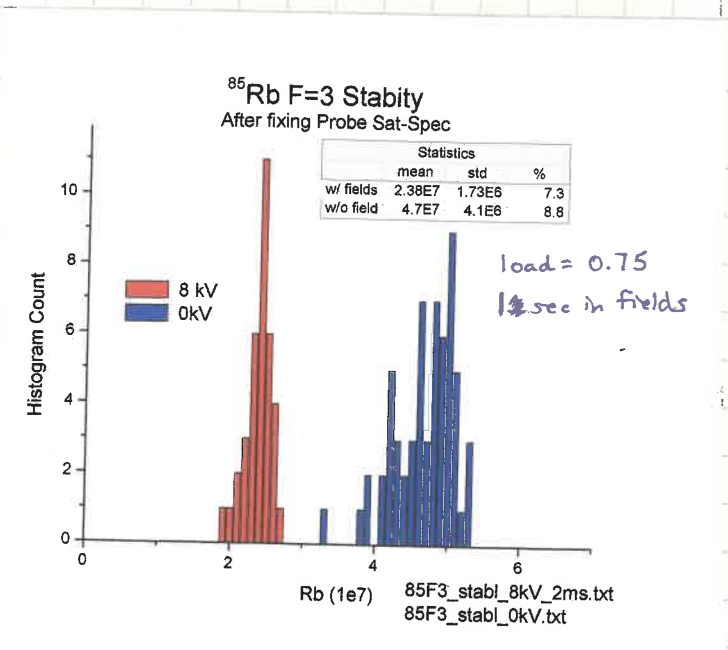
29 Jan 2010

Yesterday we replaced the Probe vortex with a vortex from Javan Pino. In addition we replaced the opto-isolator (from Linus) w/ a New Focus opto-isolator. The New Focus opto-isolator is smaller B-field, so its ~~too pullen~~ does not broaden the sat-spec lines.



~~With~~ With these improvements, $^{85}Rb_{F=3}$ stability [in all cases] has greatly improved, see histograms on next page. We also switched back to dual BOP imaging coil supplies, each running 16 amps @ ~1.5 gauss/amp. Absorbtion pictures at 2ms expansion were unstable, but 5ms was stable with only a single supply. With dual supplies, 2ms is now stable.

lesson: $\boxed{\text{Instability was probe-laser}}$



load = 0.75
1 sec in fields

Now that we have stable absorbtion imaging, we can redo optimizations.
We'll start with $^{85}Rb_{F=3}$ MOT power vs MOT detuning vs Load level (70% max)
a la pg 36, metric will be Rb # after being in the electrostatic ~~trap~~.

$Rb_{F=3}$

MOT P (mW)	MOT AOM (MHz)	MAX MOT load (V)	Used Load level (70%) (V)
254 [50%]	74	0.92 @ 8A	0.65
454 [90%]	74	1.07 @ 6A, τ=9s	0.75
102 [20%]	74	0.461 @ 10A, τ=17s	0.32
102	77	0.584 @ 10A, τ=16s	0.41
254	77	1.517 @ 6A, τ≈9s	1.05
454	77	1.62 @ 4A, τ~6sec	1.13
454	71	0.656 @ 8A, τ~5sec	0.5
ST → 254	71	0.604 @ 10A, τ~6sec	0.45
102	71	0.241 @ 12A, τ~13s	0.18

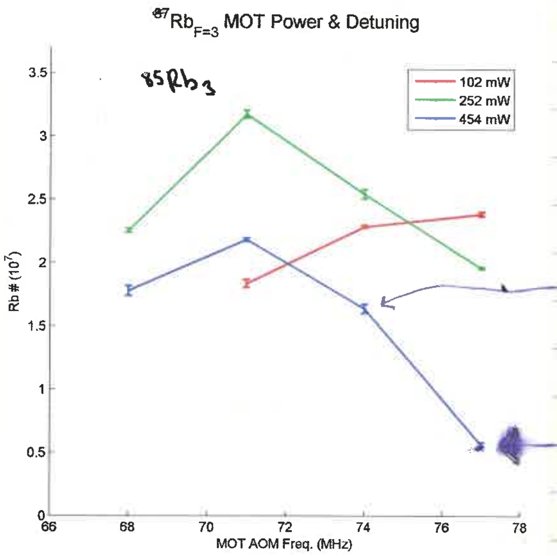
20 shots at each configuration
1 second in E-trap, 3ms exp
• higher TA power has shorter load times
10 shots each

procedure: 1) choose MOT AOM frequency
2) choose one of three MOT laser powers (20%, 50%, or 90%)
3) maximize MOT load by varying trapping current for MOT
4) stay at max MOT-load trapping current and load to 70% of max
5) see how many Rb atoms make it back from the E-trap.

continued

* 20% TA power probably has too long of a load time to be used.

254	68	0.233 @ 12A, τ~4s	0.17
454	68	0.315 @ 10A, τ~3s	0.22



Choosing 254 mW [50%] and 71 MHz.
With those settings, the ^{85}Rb has a max MOT-load of **0.65 V**

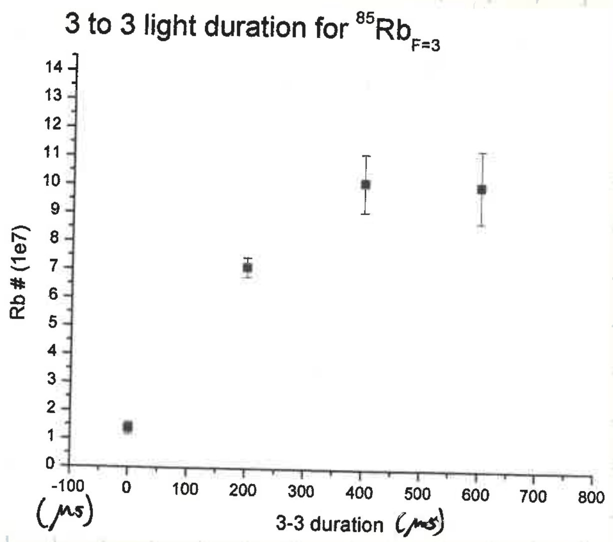
and here more recently

We were running here a while ago

Next thing to do is to tweak up the CMOT parameters for ^{85}Rb.
Then we will do this procedure again for ^{87}Rb.

$\boxed{\text{01 Feb 2010}}$

Our picture-taking of ^{85}Rb is still stable this morning.
We will proceed now w/ optimizing the optical pumping w/ the 3->3 light.

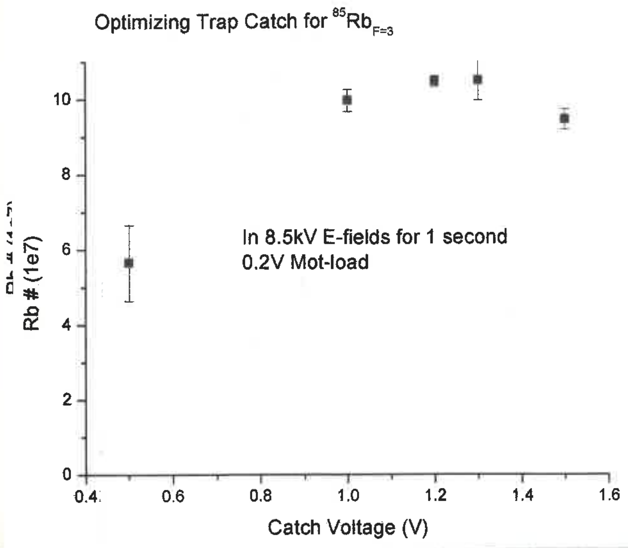


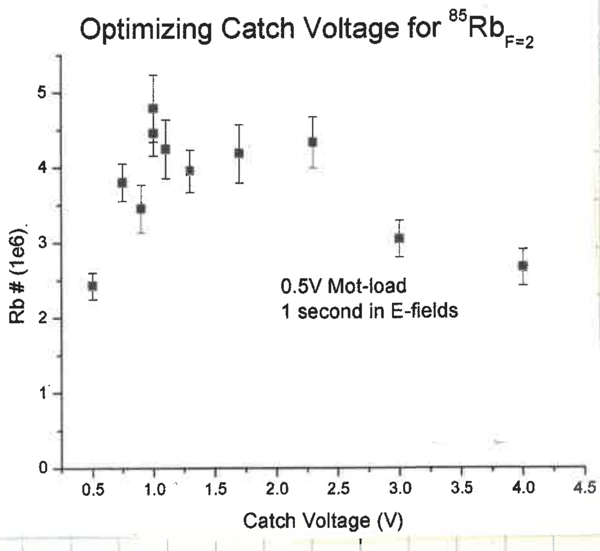
0.2 V load, 1 second in ±8 kV fields.
MOT cell absorbtion imaging.

$\boxed{\text{Choosing 500 μs duration}}$

Also "optimized" Repump shutter off time relative to MOT off time.
Rb # is very insensitive to this.

Last thing to do is optimize the trap catch for ^{85}Rb F=2 and F=3

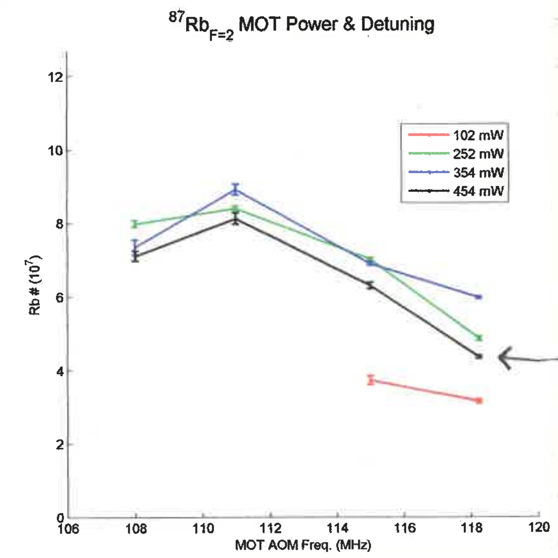




Trap catch	^{85}Rb F=2	1.0V
	^{85}Rb F=3	1.2V

$\boxed{\text{2 Feb 2010}}$

Now that we are satisfied with ^{85}Rb optimization, we will run through the same optimization for ^{87}Rb, using Rb # after exposure to the e-fields as our metric. Firstly, MOT loading parameters



(70% power)

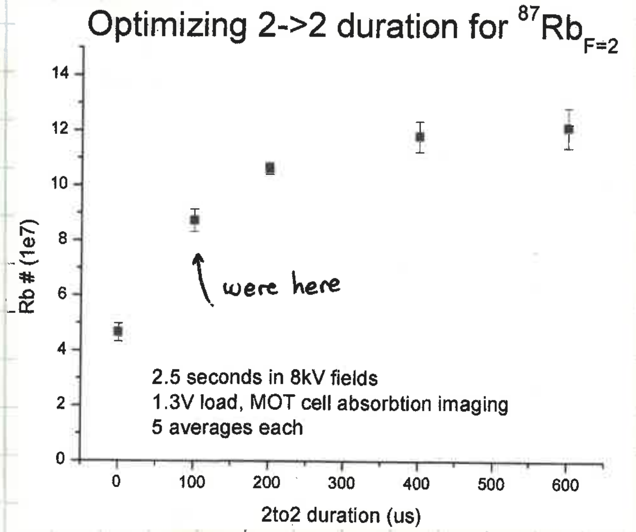
choosing $\boxed{\text{110 MHz, 354 mW}}$

with this choice, max MOT-load is

$\boxed{\text{1.83 V @ 10 amps}}$

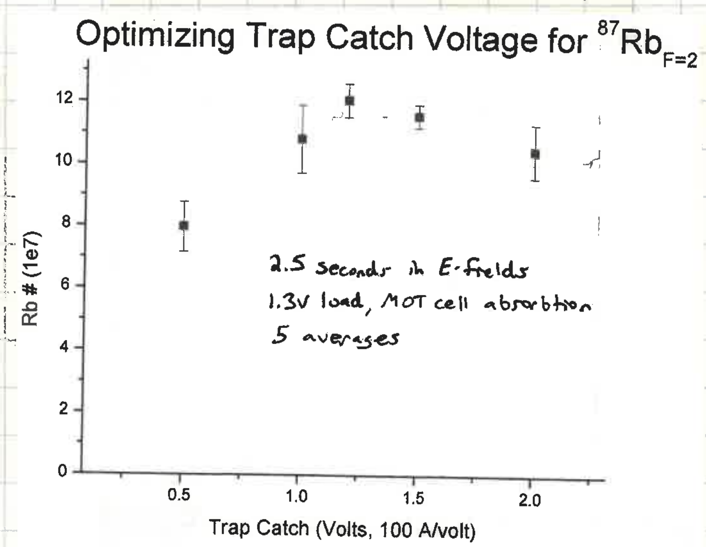
were here.

Next is to optimize (or verify) the 2→2 timing for ^{87}Rb

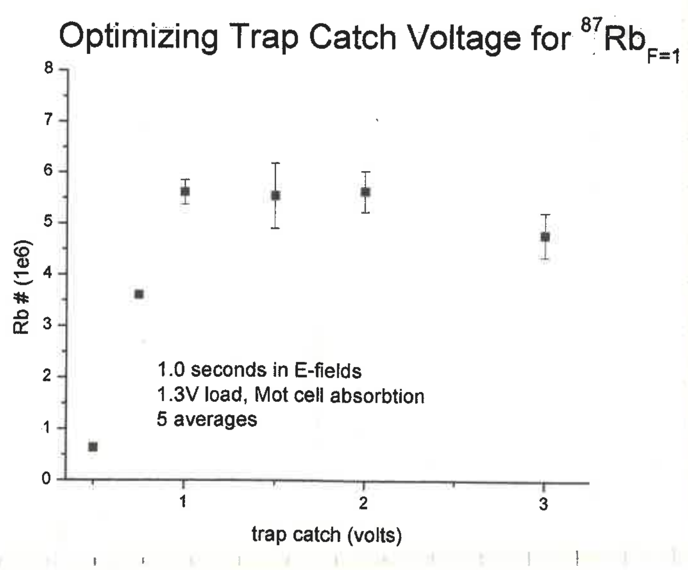


choosing 500 μs 2→2 duration $^{87}Rb_{F=2}$

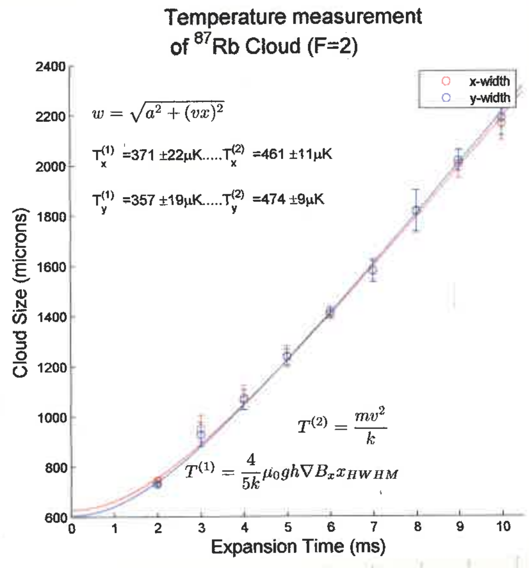
And lastly for the trap catch... still best at 1.2V.



choosing 1.2V trap catch (unchanged) $^{87}Rb_{F=2}$



choosing 1.5V trap catch (unchanged) $^{87}Rb_{F=1}$



mot Load = 0.2V
3 sec in 8kV + 7 sec in 0kV
3 Avgs

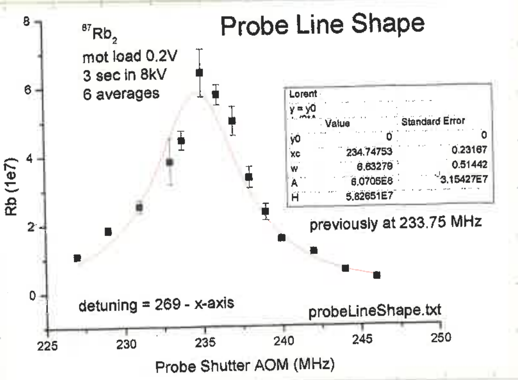
very nice temperature measurement, good agreement b/t x- and y- widths

still not great agreement b/t $T^{(1)}$ and $T^{(2)}$

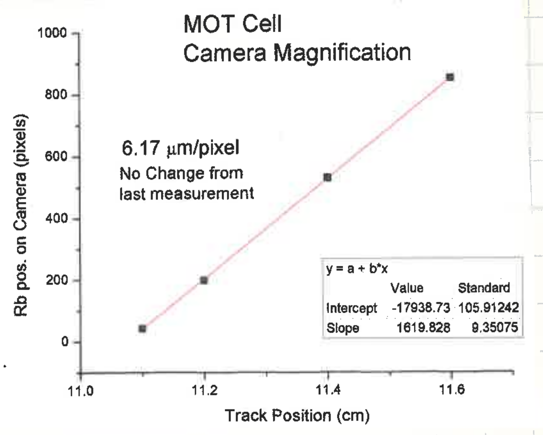
much colder than we had on 22 Jan (~1mK)

$\boxed{\text{3 Feb 2009}}$

Measuring Probe line-shape

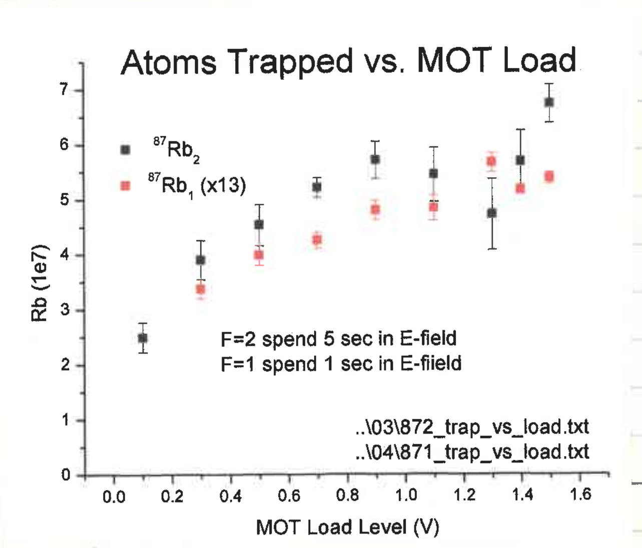


New probe detuning = 34.25 MHz

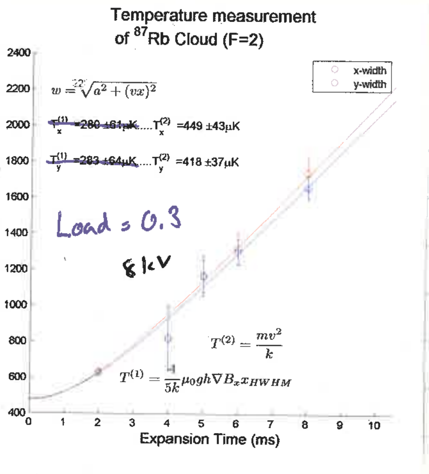


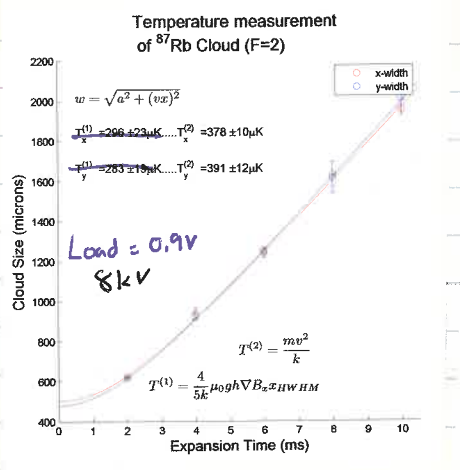
previous magnification is 6.15 μm/pix, so the magnification ~~is still~~ has not changed

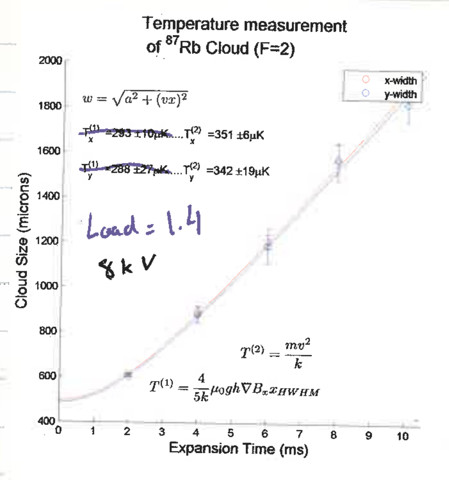
^{87}Rb F=1 is extremely noisy and we are getting many "dropped shots". Turns out the OP shutter ~~is~~ ~~de~~ controller is dead.

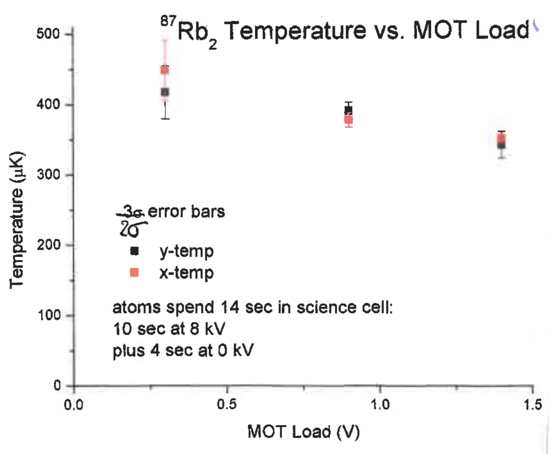


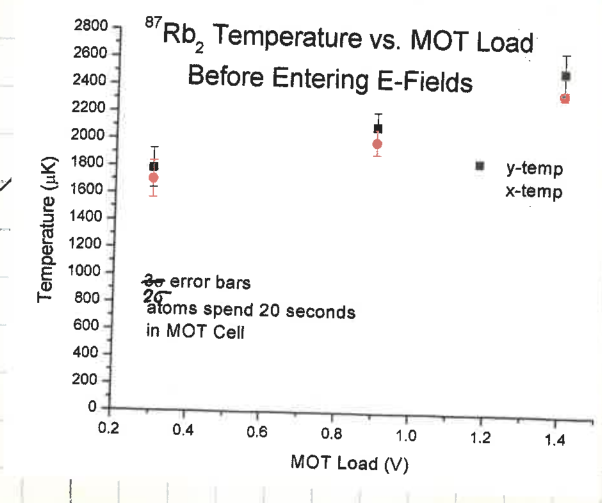
The trapped vs load profile is not what I was expecting. It is not linear at large load levels. This could be a temperature thing.









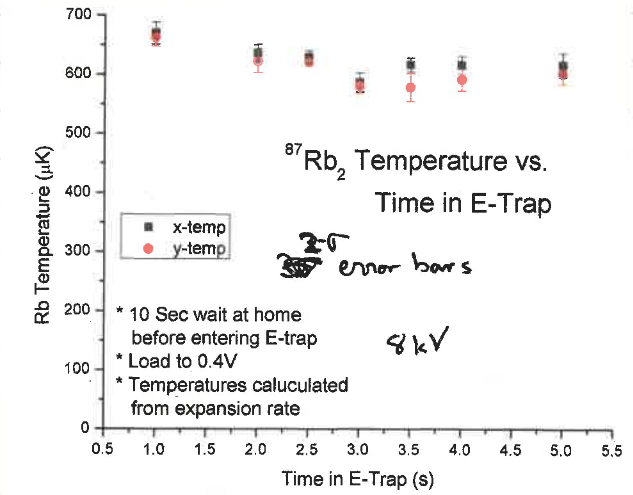


48

5 Feb 2010

I want to measure a heating rate in the E-trap. Unfortunately ^{87}Rb (F=1) ~~is~~ has too low of a count to expand for very much, and F=2 has ~~too~~ much to expand for short times. With a load of 0.1V the minimum expansion time is 5ms, and this does not provide a very good fit.

I have added a wait in magnetic trap at the home position. the atoms will wait there for 10 seconds, then move into the E-trap.



the temperature decreases slightly ~~for the~~ in the first couple seconds.

At later ~~the~~ times in the trap the signal is low enough that the error bars at large expansion time increases, as does the ~~divergence~~ difference b/t x- and y-widths.

files for above data are labeled: 872_temperature_[t]sec.txt where t = time in E-trap.

8 Feb 2010

Back to ND_3

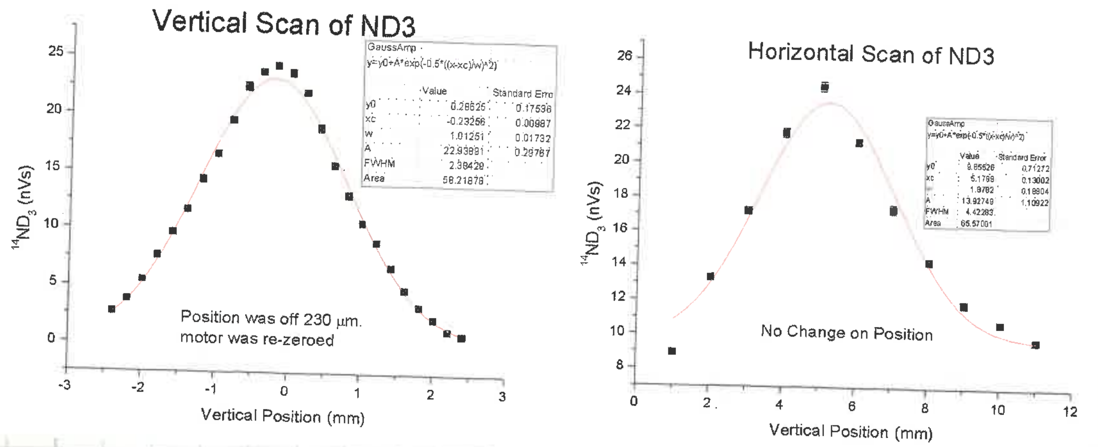
Today I would like to test if we can put 2.5% $^{14}ND_3$ and ~~2.5%~~ 2.5% $^{15}ND_3$ in Kr. Currently we run w/ 5% $^{14}ND_3$ in Kr, but we would like to quickly be able to switch ~~B~~ b/t (14 & 15) ND_3 without changing gas.

- Steps
  - verify $^{14}ND_3$ signal w/ 5% $^{14}ND_3$ mixture
  - check and compare 2.5% mixture

If the 2.5% is ok, then mix 2.5% $^{15}ND_3$ + 2.5% $^{14}ND_3$

If 2.5% is not ok, try mixing ~~5%~~ 10% $^{14}ND_3$

The position of the laser has changed slightly



with 5% mixture	~24 nVs	(waited 10 min for this mixture)
with 2.5% mixture	~14 nVs	(waited 20 min for this mixture)
with 10% mixture	12 nVs	(waited 20 min for this mixture)
with 2.5% each $^{14}ND_3$ + $^{15}ND_3$	15 nVs ($^{14}ND_3$)	
	5 nVs ($^{15}ND_3$)	

trapping for 50 ms

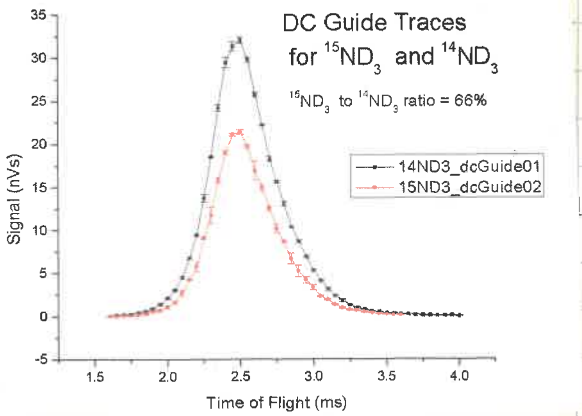
trapping signal for $^{15}ND_3$ is pretty small (5 nVs) - down a factor of 3 for $^{14}ND_3$

I'm going to take some DC guide traces of both (14) and (15) to see how overall signal compares.

we still have a gas mixture of 2.5% $^{14}ND_3$ + 2.5% $^{15}ND_3$ in Kr

Laser 17 mJ at 31511 cm^{-1}
17 mJ at 31492.6 cm^{-1} → change to 31492.8 cm^{-1}

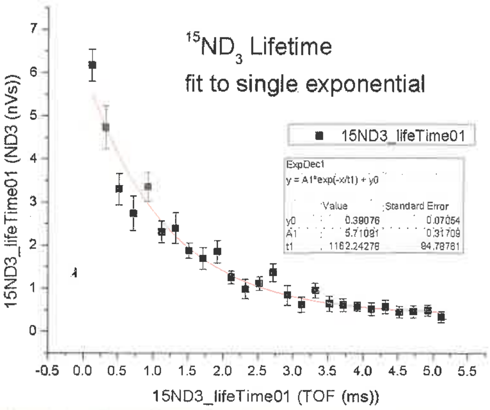
MCP -3900 V 1st Stage gain
Slower 2 kV DC guide
TOFMS 1200 / 800 V
valve 50 μs @ 280 V 6.3E-6 torr at 10 Hz (source chamber)
815 torr backing pressure.
Gas 2.5% $^{14}ND_3$ + 2.5% $^{15}ND_3$ in Kr

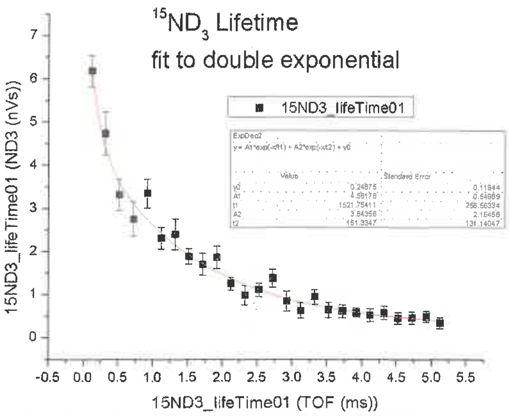


run 15ND3_dcGuide01.txt
run 14ND3_dcGuide01.txt

I did a quick, by hand laser scan and found that the peak is actually at 31492.8 cm^{-1}.

run 15ND3_dcGuide02.txt





Laser shutter was not working, so not implemented.

From Meijer's paper; non-adiabatic lifetime in E-trap: $^{15}ND_3$ 1.2 s
$^{14}ND_3$ 7 s

15 Feb 2010

The gas mixing system was rebuilt last week. Now it ~~contain~~ consists of 2 separate cylinders so we can mix a batch of $^{14}ND_3$ and $^{15}ND_3$ separately and still be able to swap b/t the two.

Now I will again look at the 2 ND_3 isotopes to see ~~if we~~ how much we can trap

Laser 31492.8 cm^{-1} 17 mJ
31511.0 cm^{-1}
MCP -3900 V 1st stage gain (for DC guide)
slower 2 kV dcGuide
TOFMS 1200/800
valve 50 μs @ 280 V 6.6E-6 torr at 10 Hz
830 mtorr backing
Gas 5% $^{14}ND_3$ in Kr and 5% $^{15}ND_3$ in Kr
in separate cylinders

52

run 15ND3_dcGuide01.txt

run 15ND3_bunch01.txt   MCP = -3900 v_i = 415 m/s

delay = 550 μs

Trapped $^{15}ND_3$ = 11 nVs   slow @ 64.605°

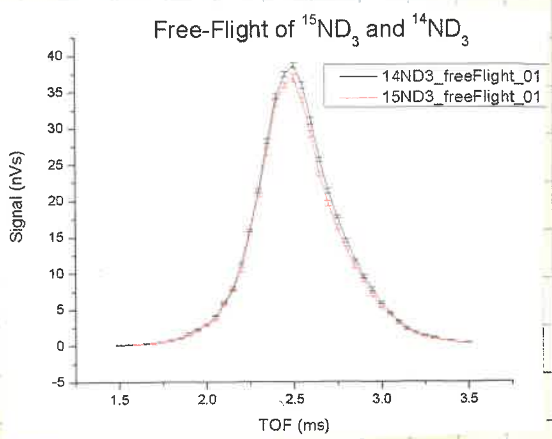
(MCP = -5000)   detect @ 50 ms

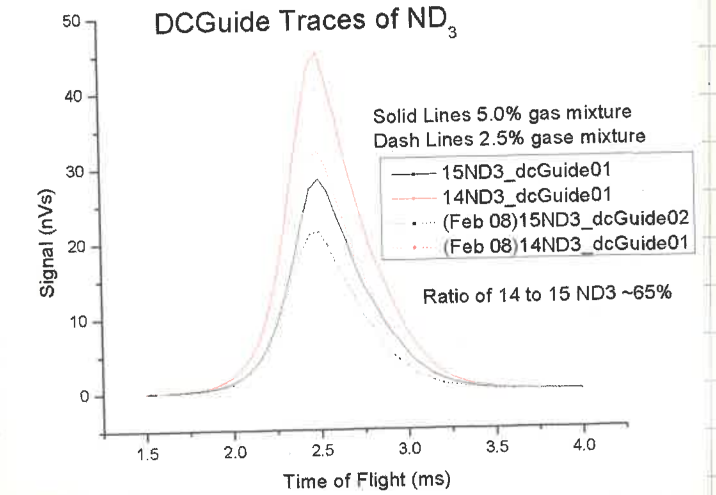
$^{14}ND_3$ = 20 nVs   slow @ 61.4°

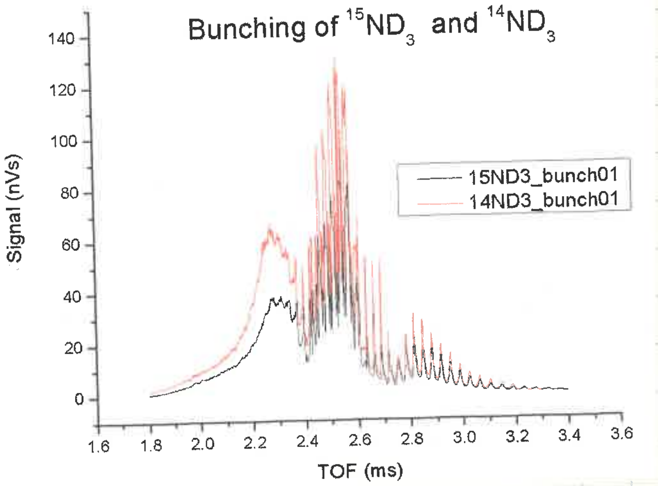
switching back to $^{14}ND_3$

run 14ND3_dcGuide01.txt   MCP = -3900

run ~~15~~ 14ND3_bunch01.txt







⇓ $^{14}ND_3$ is slightly saturated at the peaks of the bunching.

Not sure why $^{15}ND_3$ is so much smaller than $^{14}ND_3$. Could be Majorana losses or maybe detection ~~eff~~ efficiency.

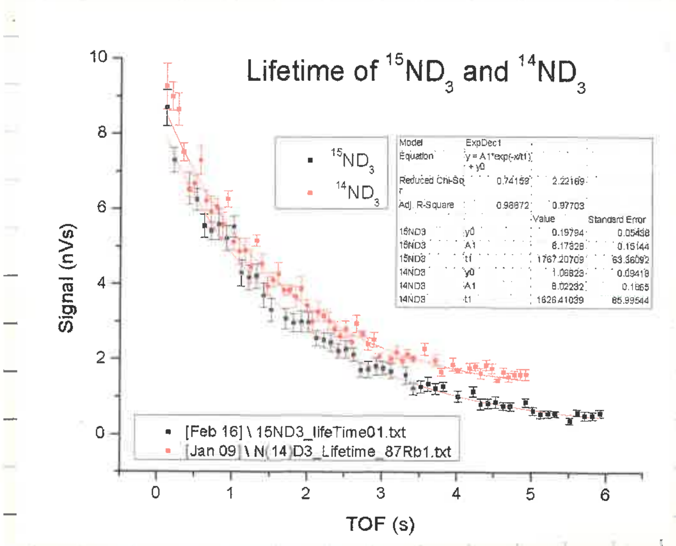
To test for Majorana I'm going to do free flight traces (see above ↑)

run 14ND$_3$_freeFlight01.txt   MCP = -4500 V

run 15ND$_3$_freeFlight01.txt

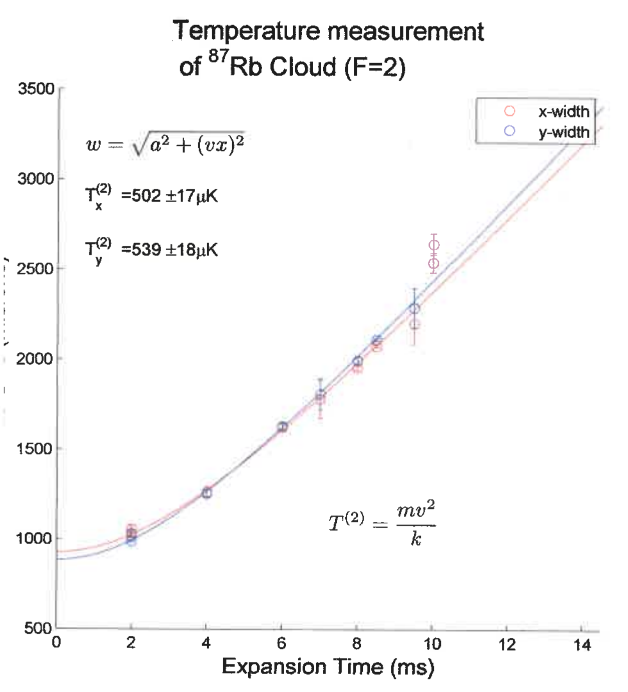
16 Feb 2010

Taking lifetime trace of $^{15}ND_3$



All parameters are from 15 Feb. MCP: -5000V

~~12 sec~~ 12 sec trap time
laser shutter was used



~~Rb lifetime is~~
Rb temperature in E-trap. 8kV
200 ms in trap.
200 ms "kill Rb" time in slower control array

0.5 V load
no waits anywhere.

imaging is saturated at less than 8 ms expansion, however the fits still match well on

17 Feb 2010

today we are set to try a [real] collision run

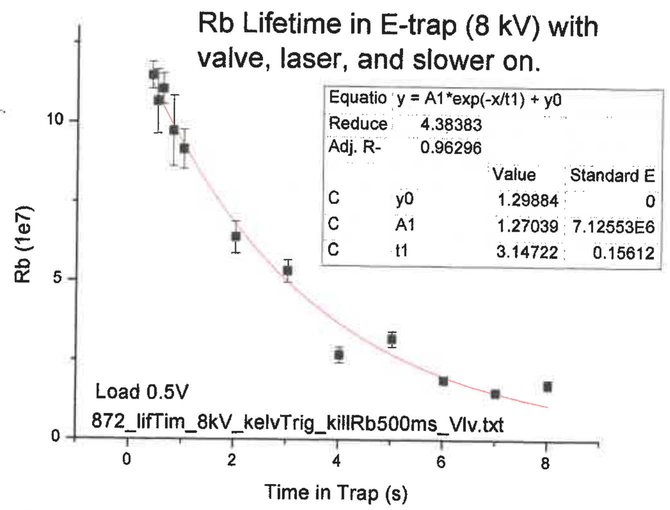
Because we are not certain ~~to~~ of the stability (long term) of certain things, we will be doing the following before each collision run:

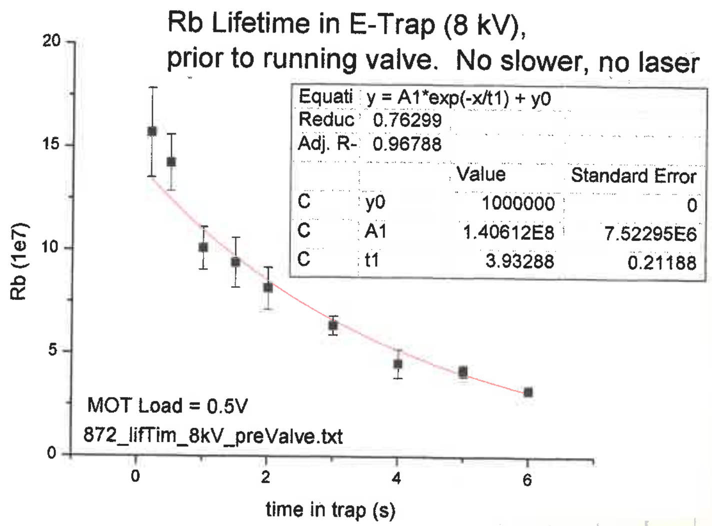
- measure ~~t~~ Rb lifetime w/ ~~all~~ full experiment running
- calibrate imaging
- measure initial Rb # in E-trap
- measure temperature.

I'm going to start by measuring Rb lifetime w/ E-fields, but no valve. Valve has been off for ~14 hours

run 872_lifTim_8kV_preValve.txt

run 872_lifTim_8kV.txt — normal experimental conditions. valve is firing once per track move, the valve has not been run at 10 Hz





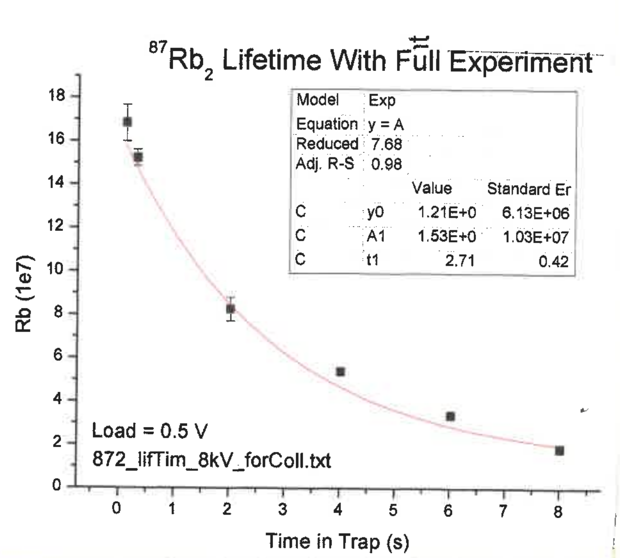
$\boxed{\text{18 Feb 2010}}$

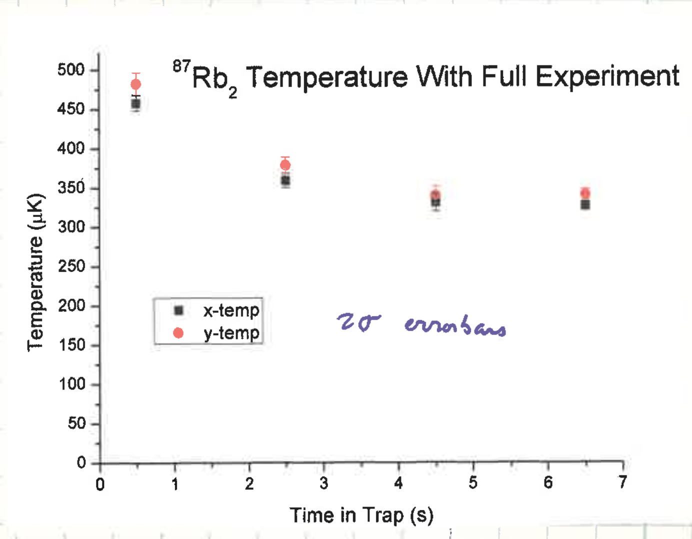
today I'm going to take collision data for ^{87}Rb (F=2) and $^{15}ND_3$

measuring lifetime of Rb: run 872_lifTime_forColl.txt
~~easy~~ full experiment running.

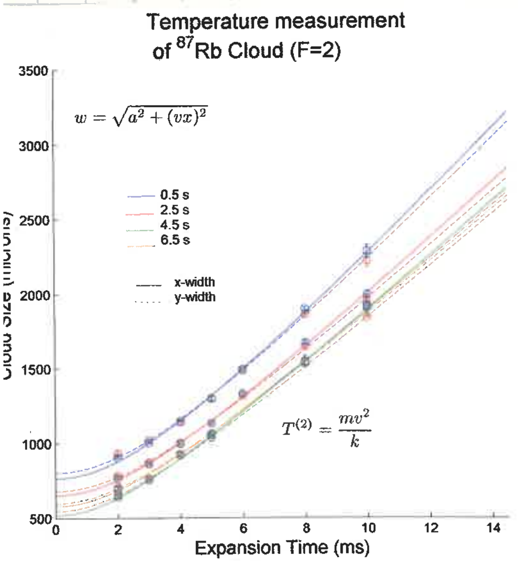
measuring temperature at different times:

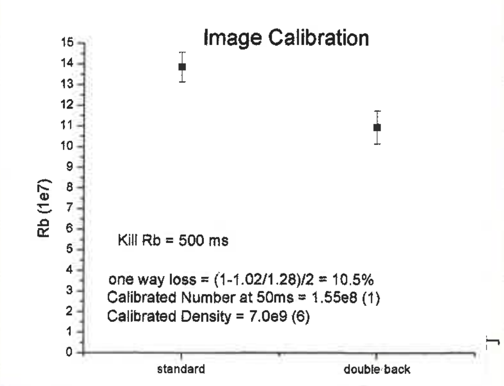
run 872_temp0.5s_8kV_forColl.txt — T @ 0.5 sec
872_temp2.5s_8kV_forColl.txt — T @ 2.5 sec
" "4.5 s" "
" "6.5 s" "





2σ errorbars





I calibration file,
1 ≡ standard imaging
2 ≡ double back

~~peak density~~
~~5.7e9~~
~~2 5.74e9 (6)~~

500 ms "kill Rb" time

P_{eff} = 1.9e14

Now to take collision data

Laser 31511 cm^{-1} 17mJ
31492.8 cm^{-1} 17mJ
MCP -5000V
valve 280V, 830 Torr backing
Gas 5% of $^{14}ND_3$ and 5% $^{15}ND_3$ in separate cylinders
both mixed w/ Kr
TOFMS 1200/800
TA 70% (350 mW)

run 15N_872_collisions.txt Kill Rb = 500 ms
run 15N_872_collisions.txt

I took the collision data down to 50ms detect time. New calibrated number @ 50ms is:

19 Feb 2010

modified program (Total control) to ensure a 50% duty ~~cyl~~ cycle with the "kill Rb" part. This created an issue of the arrays being to long to be handled by the clusters in Labview. I increased all cluster sizes to 190 from 180. Time stamp as well as bool clusters need to be updated in: "Build Slower control"; "Aux Board Control"; as well as in the various event controls inside "Total Control" for the time stamps.

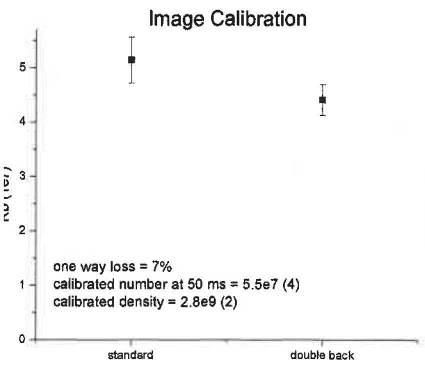
Today I'll take collision data at a different Rb density as yesterday. ~~I~~ The Rb density will be adjusted by adjusting the "Kill Rb" time.

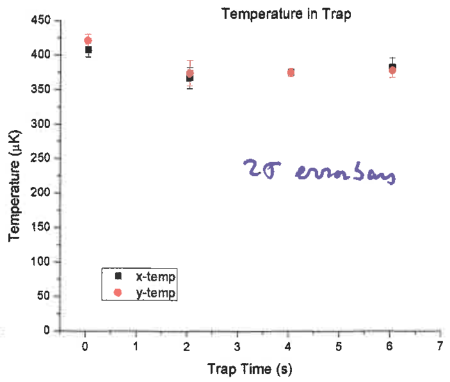
run 872_imageCalibration_exe
run 872_lifTime_for Coll_exe
run 872_temp_for Coll_exe

Load = 0.5 V

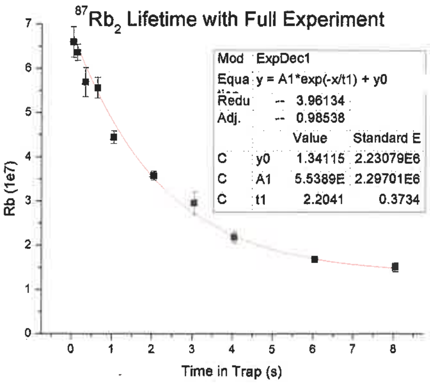
Kill Rb = 2000 ms

TA power = 70%
(350 mW)





2σ errorbars



$P_{eff} = 7.6E13$

19 Feb 2010

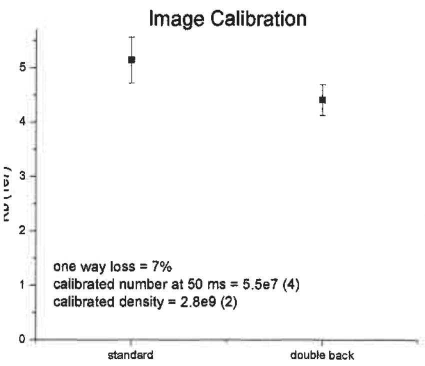
modified program (Total control) to ensure a 50% duty ~~cyl~~ cycle with the "kill Rb" part. This created an issue of the arrays being to long to be handled by the clusters in Labview. I increased all cluster sizes to 190 from 180. Time stamp as well as bool clusters need to be updated in: "Build Slower control"; "Aux Board Control"; as well as in the various event controls inside "Total Control" for the time stamps.

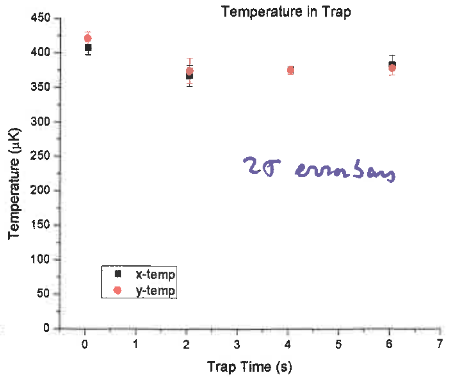
Today I'll take collision data at a different Rb density as yesterday. ~~I~~ The Rb density will be adjusted by adjusting the "Kill Rb" time.

run 872_imageCalibration.exe
run 872_lifTime_for_Coll.exe
run 872_temp_for_Coll.exe

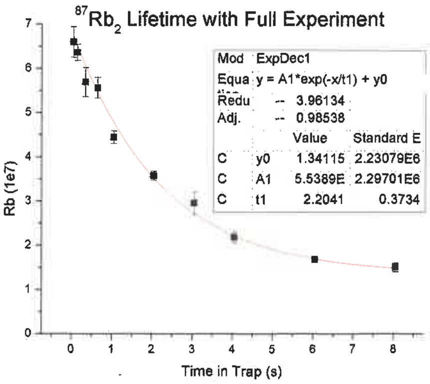
Load = 0.5V
Kill Rb = 2000 ms

TA power = 70%
(350 mW)

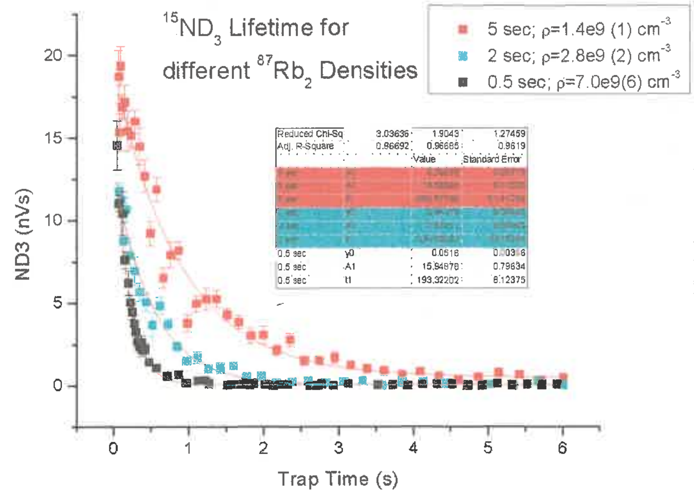


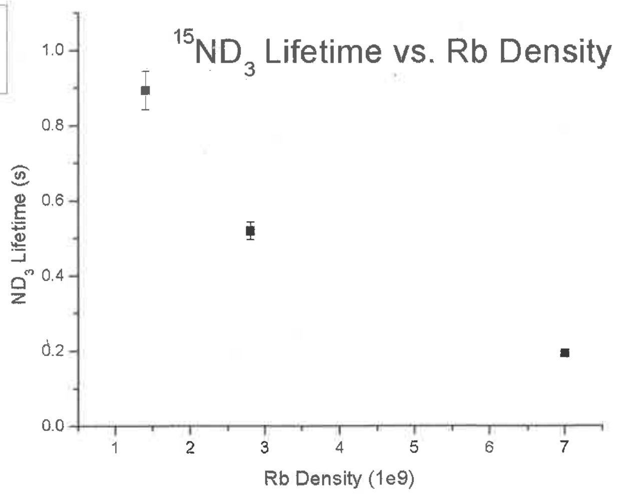


2σ errorbars



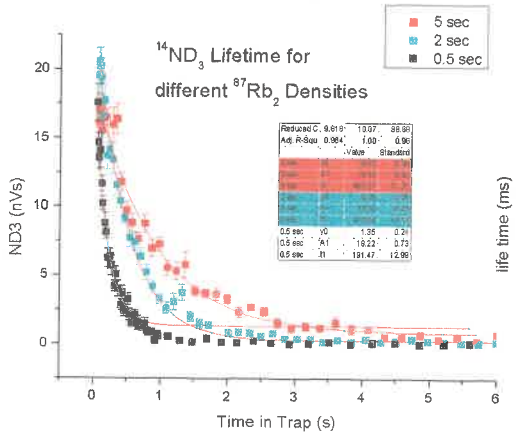
$P_{eff} = 7.6E13$

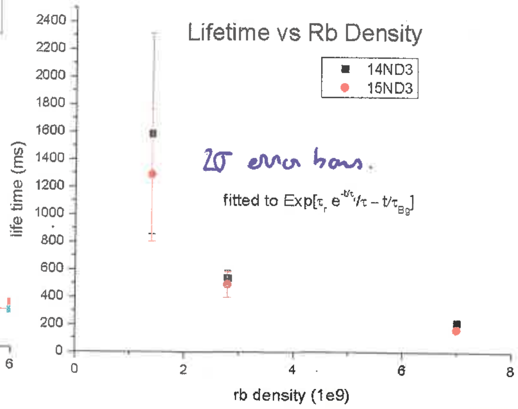




23 Feb 2010

Yesterday we did not get a chance to do ~~15~~ $^{14}ND_3$ w/ 5 sec wait. So that is what we are doing today.
The Rb lifetime and calibration are consistent w/ yesterday, we are not retaking the temperature

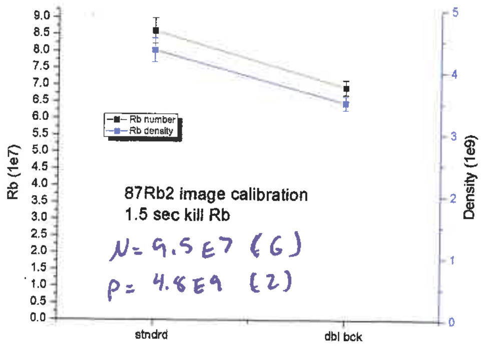


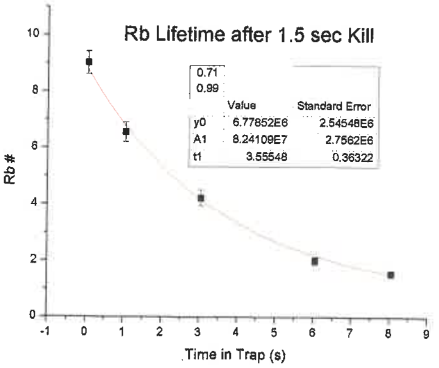


24 Feb 2010

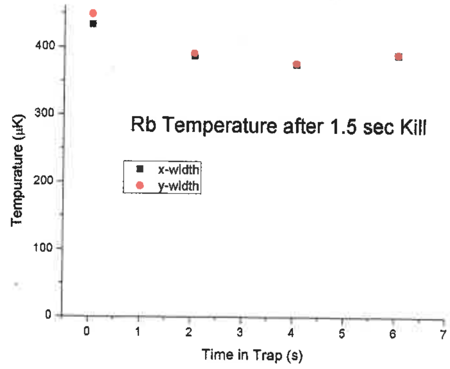
The 5-sec kill Rb density ~~...~~ may be too low to accurately calculate the lifetime. The error bars are very large. I'm going to pick a density b/t 7E9 and 3E9 and increase the number of averages. All collision runs up to this point were 10 avgs. This will be increased to 20 avgs.

Instead to just taking 20 averages at each point, I will take each point 2x w/ 10 avgs each. The points will still be randomized so ~~they~~ get the double point will be taken at different time of the day.





MOT Load: 0.5V



run ~~...~~ 14N_872_killRb1.5secEx6

25 Feb 2010

Same density (Rb) as we yesterday (1.5 sec hill Rb ~ 4.8E9 /cm³)
but w/ $^{15}ND_3$.

verifying Rb density and lifetime w/ yesterdays calibration

ρ = 4.5E9 (2) N = 8.9E7 (5)

I had to replace all the mosfets for the bottom coil. 2 were broken (one sparking) and 3 had their connectors to the drain broken.

We deuterated the valve line in ~10 psi of $^{14}ND_3$. We will let the deuteration go untill Monday morning (currently thursday)

1 March 2000

I stated pumping the $^{14}ND_3$ last night (~8pm) and let it pump untill this morning.

Today we will take the "1.5 s hill Rb" data w/ $^{14}ND_3$. Hopefully w/ the valve being deuterated over the weekend we will see less of a drop in ND_3 signal over the day.

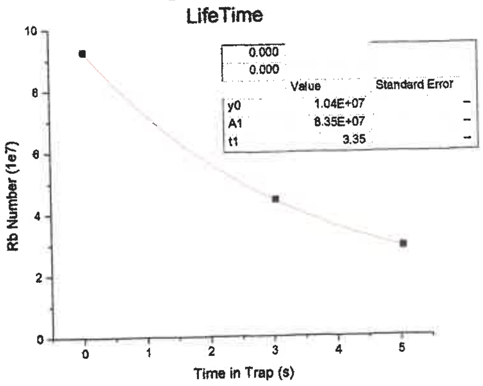
Start again by doing the calibration / lifetime / temperature.

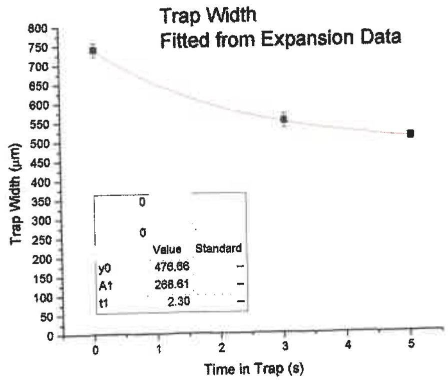
run 872_calibration_1.5sec.txt
872_lifetime_1.5sec.txt

Since we have re-deuterated and fixed MOSFETS, I'm going to take the same data at 24 Feb 2010 and see if we have the same drift / problem.

$^{14}ND_3$

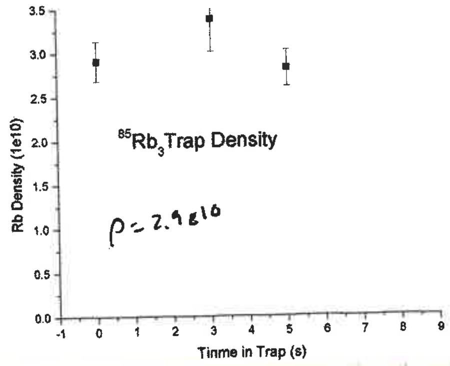
$^{87}Rb_2$ With 1.5 Sec Kill Time; 8kV eTrap





$\rho_{eff} = 1.35 \epsilon 14$

3/01/2010



ρ

$n_0 = 9 \epsilon 7$

2 March 2010

today is same as yesterday but w/ $^{15}ND_3$. I'm going to take 40 pts w/ 10 avgs each.

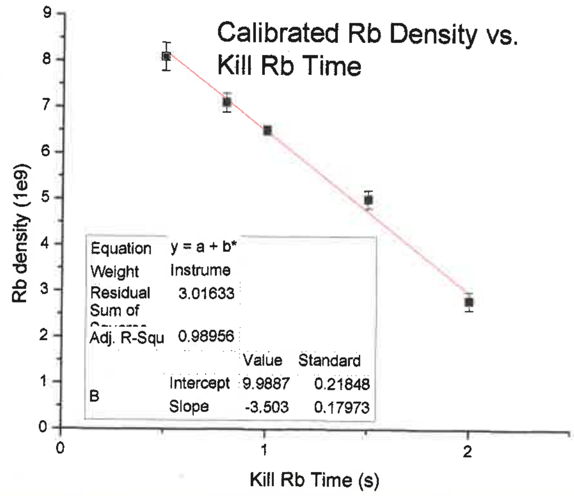
the density and lifetime are consistent w/ yesterday

run 15N 872_killRb1.5sec.exe — oops, this file is in previous days folder

there was no difference in the ^{14}N vs ^{15}N cases for the cross-section. Was there some systematic w/ dectection that previously showed a difference? or is todays point wrong in some way?

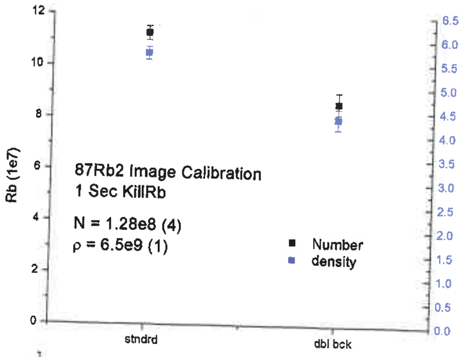
3 March 2010

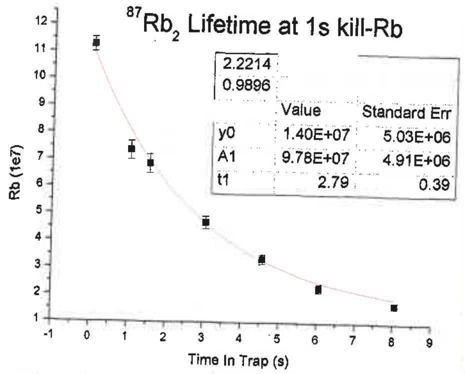
Because so far we do not have a consistent story for the cross sect of vs density, we will take another density point today.

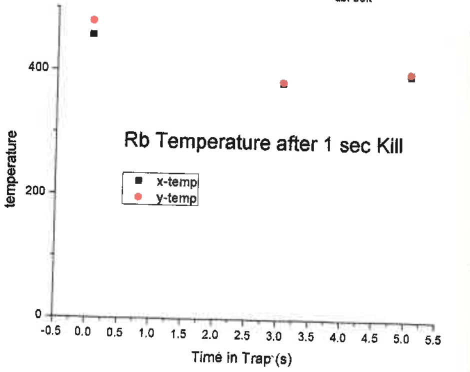


Today we will do a 1 sec kill time

$\rho = (10 - 3.5t) \times 10^9$



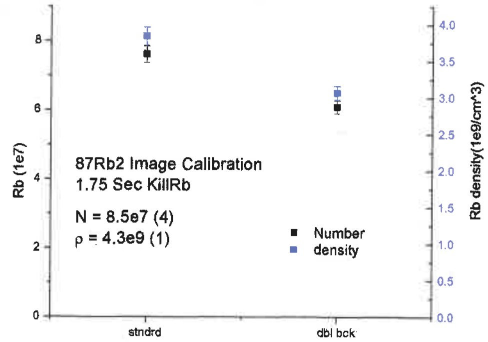


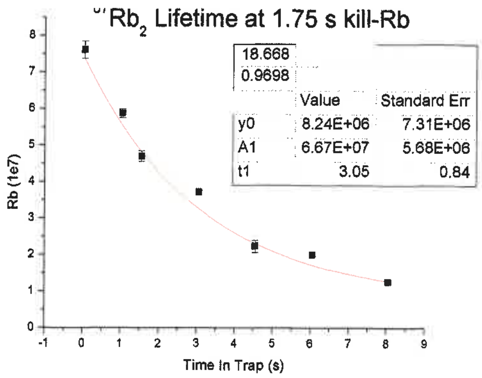


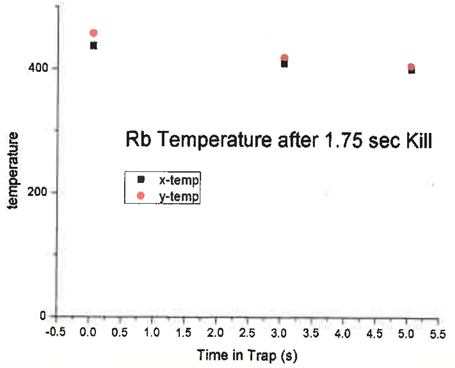
sub 15N_872_killRb1.0sec.txt
14N_872_killRb1.0sec.txt

4 March 2010

Rb Kill time of 1.75 seconds



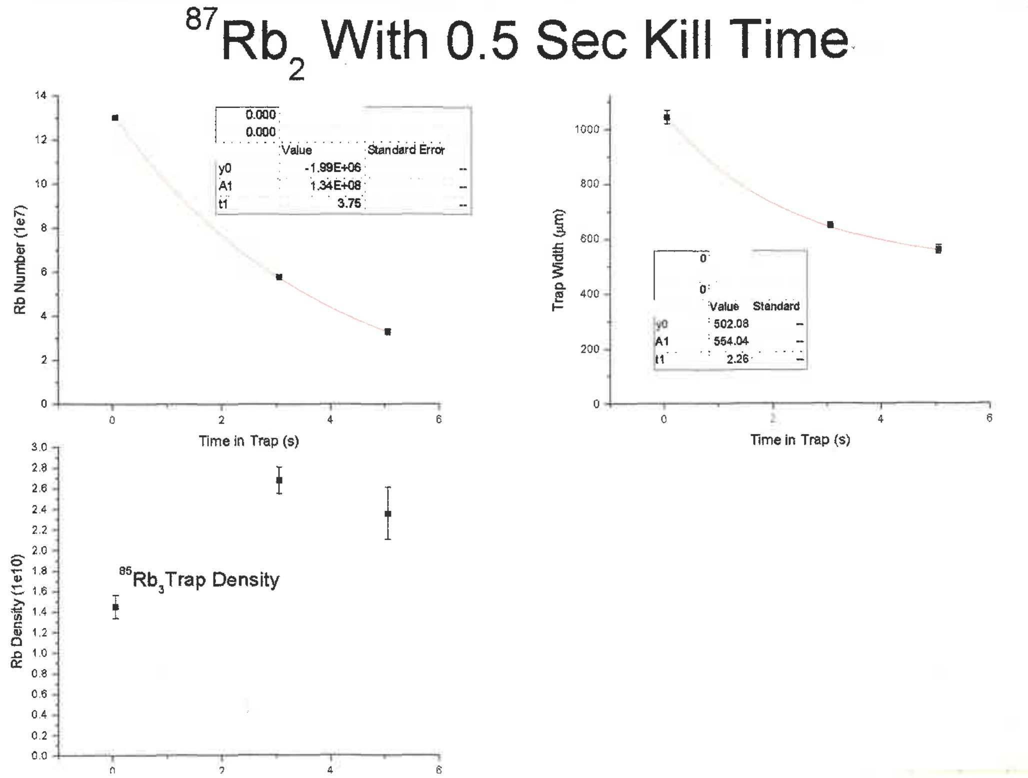




run 14N_872_killRb1.75sec.txt

run 15N_872_killRb 1.75 sec. txt

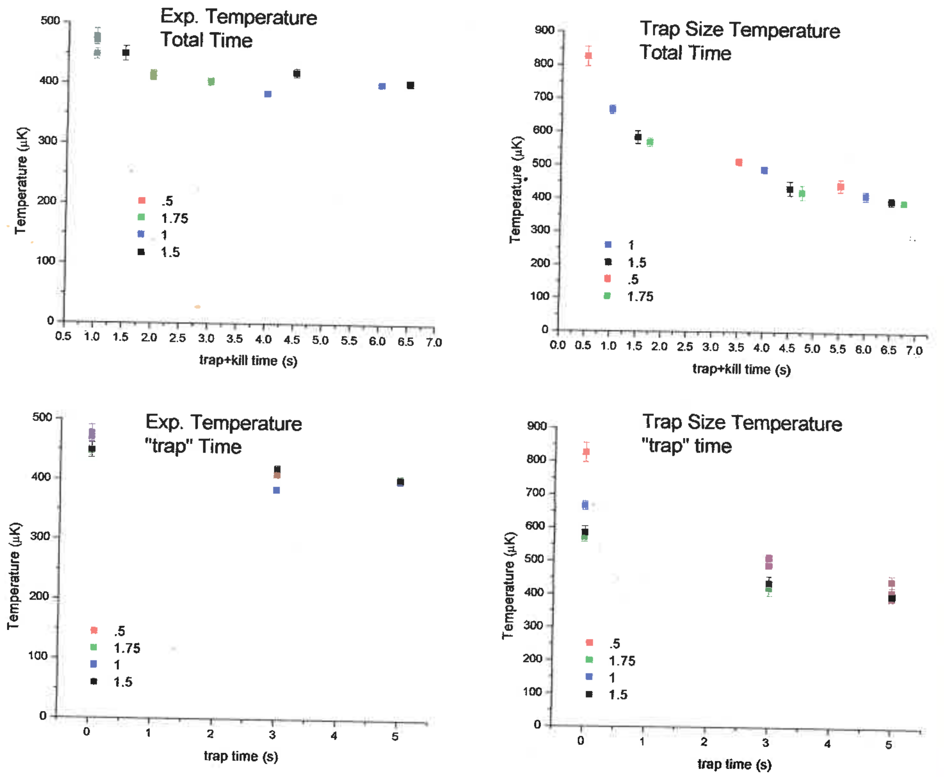
5 March 2010 0.5 sec kill Rb



8 March 2010

We discovered an error in getting the density from the Labview program. The size of the cloud was always fixed to 680 μm. All prior Density measurements are therefore inaccurate

In addition we measure temperature in two ways: measuring expansion rate and measuring in-trap size. Those two methods give different temperatures.

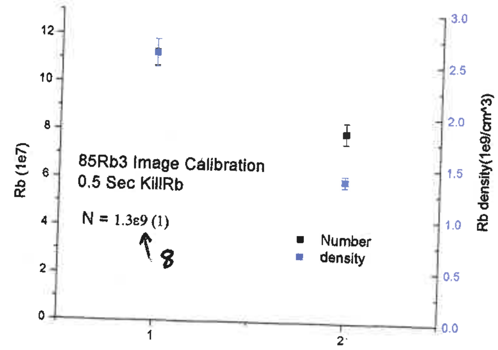


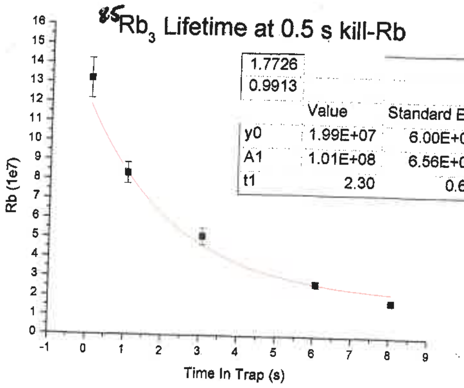
The in-trap size changes much more than the expansion time ~~the~~ Temperature would suggest. This is a problem for extracting a tot. cross-section since this effects the density distribution.

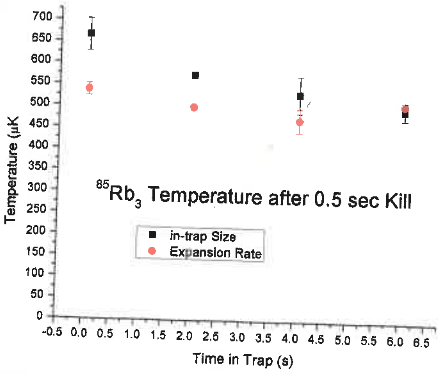
$\boxed{\text{9 March 2010}}$

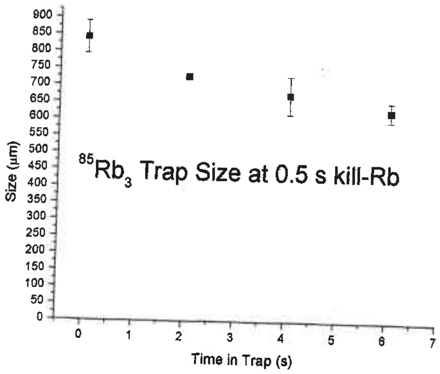
Today we are going to do collisions w/ 85 Rb f=3 (f=3)

loading MOT to 0.25V









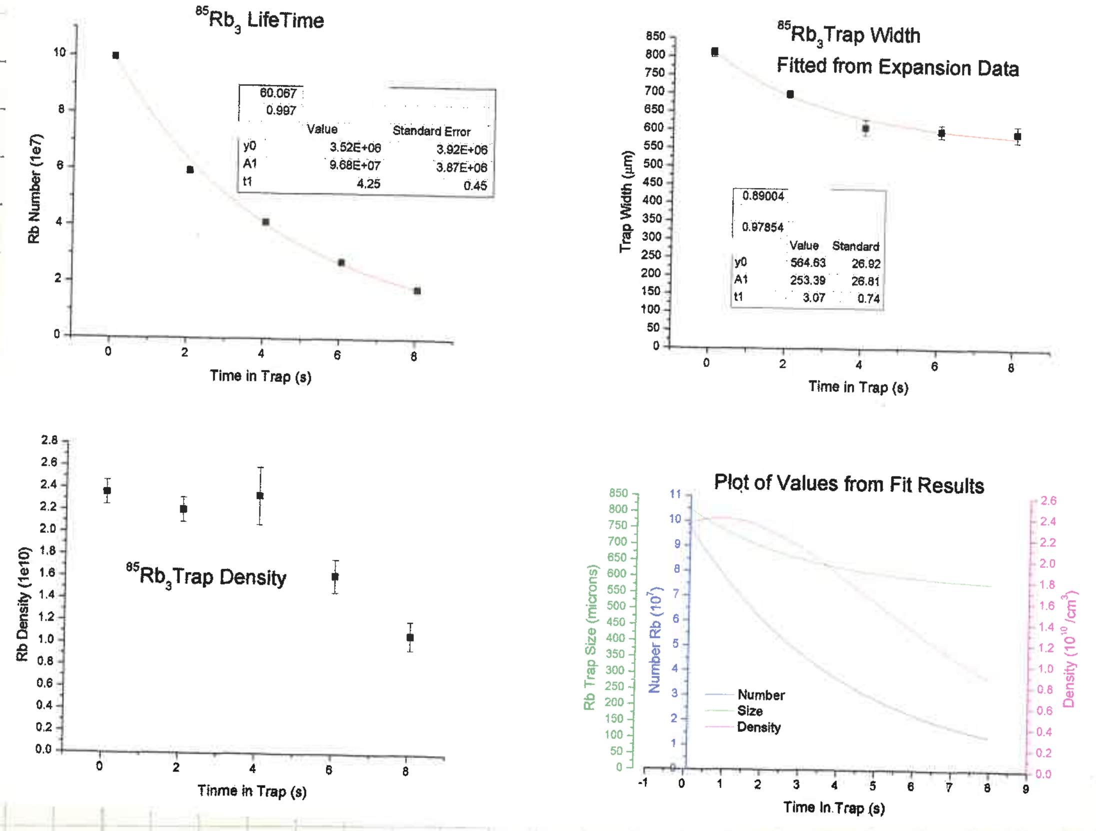
run ~~14_8~~ 14N_853_killRb0.5Sec.txt

run 15N_883_kill Rb_0.5 Sec.txt

10 March 2010

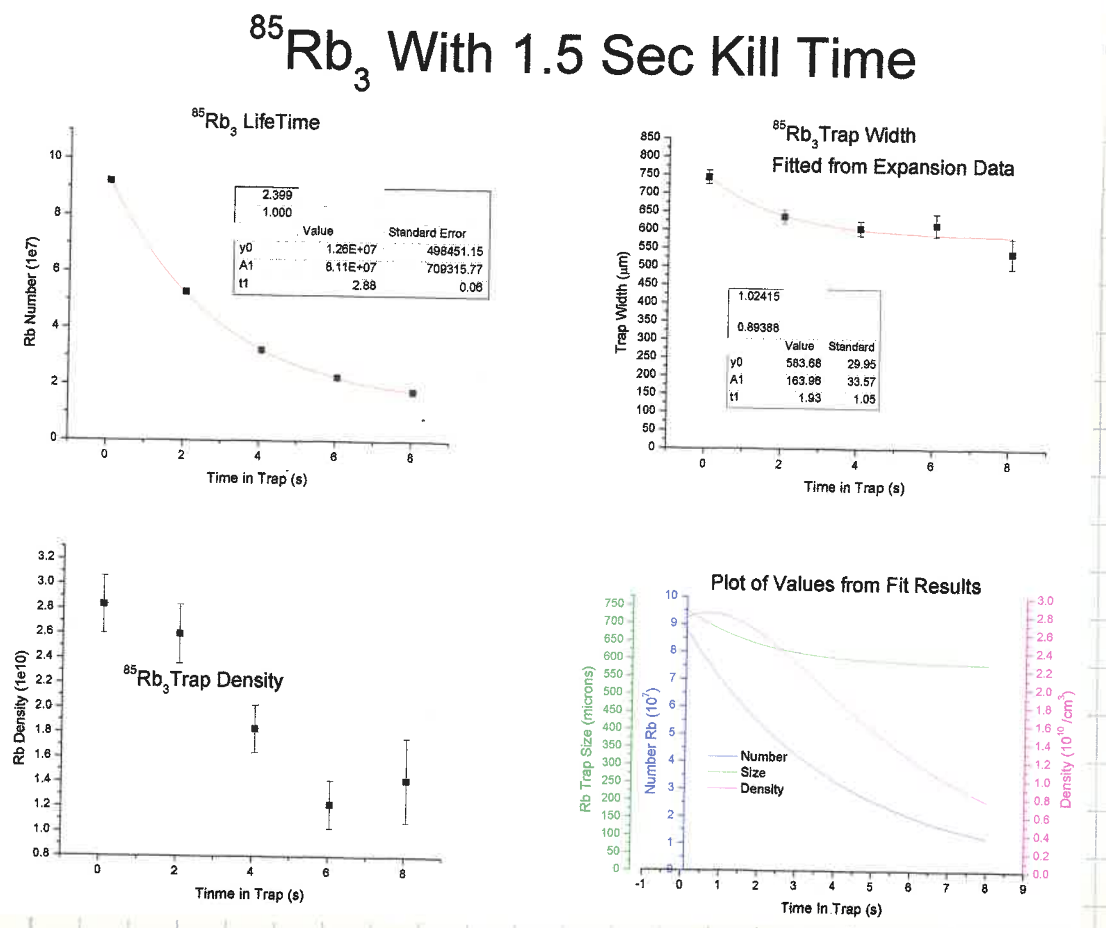
today I will keep doing $^{85}Rb_3$, using a kill time of 1 sec. I'm going to store the calibration data a little differently however. the lifetime and the expansion data will be taken together and all stored in a single file. Column 1 will be the expansion time, and column 2 will be the time in trap. this way I can get a density as a function of time.

$^{85}Rb_3$ With 1 Sec Kill Time



11 March 2010

~~Dat~~ Calibration data taken in the same format as yesterday. Doing a 1.5 sec kill time

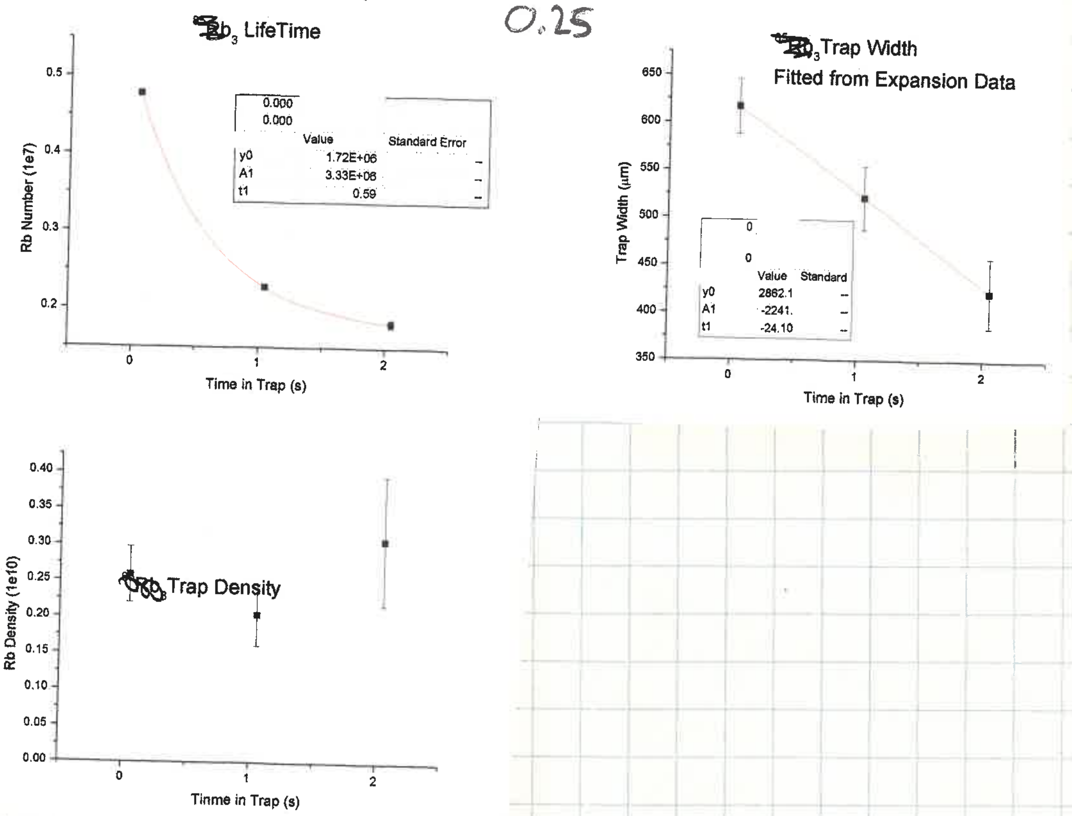


12 March 2010

Today I want to do collisions w/ the non-stretch state of Rb. I started by looking at $^{85}Rb_2$, but there is no atoms after a 0.5 sec wait in electric fields. Before entering fields, there are low-mid 10^7 atoms.

I switched to $^{87}Rb_1$. I also cut the kill time down to 0.25 seconds.

$^{87}Rb_1$ With ~~0.5~~ 0.25 Sec Kill Time



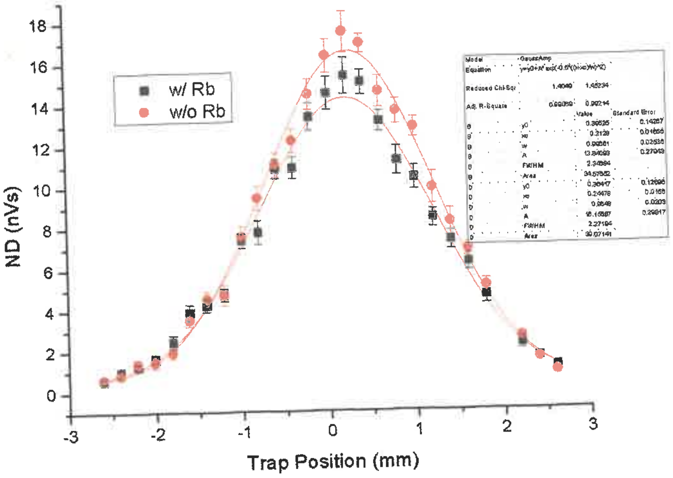
~~14N~~ 14N_871_killRb_0.25s.txt
15N_871_killRb_0.25s.txt

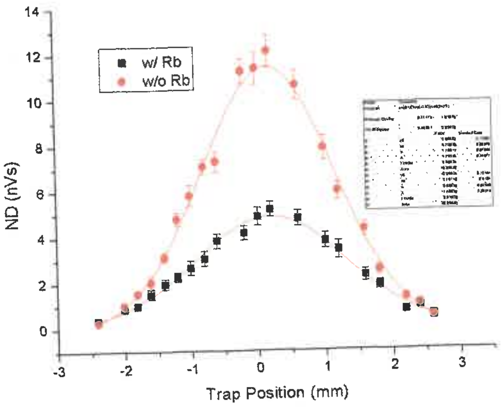
15 March 2010

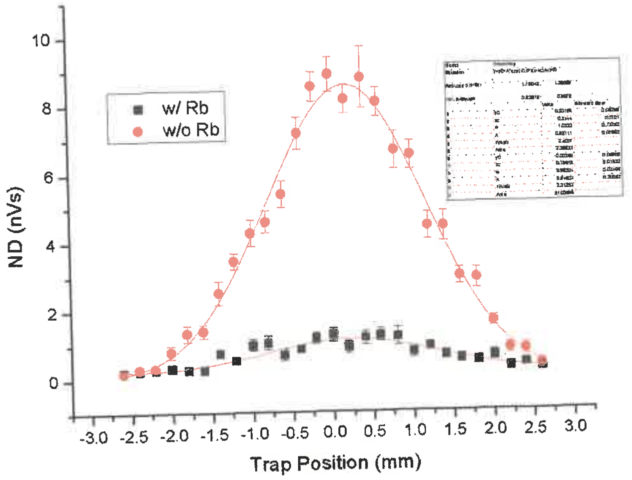
Today I'm going to take ND$_3$ width measurements at different trap times.

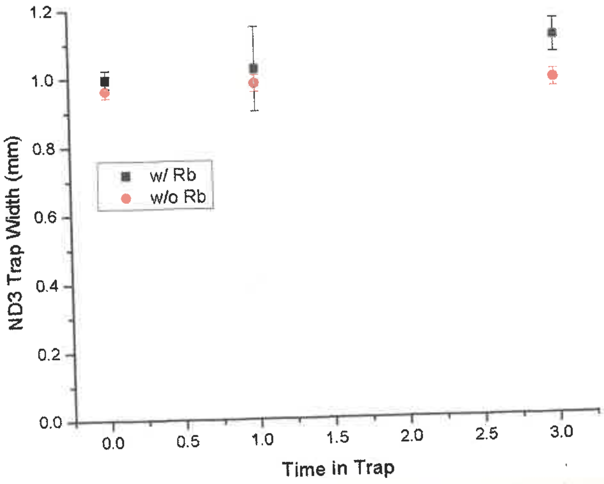
using $^{87}Rb_2$ and $^{14}ND_3$ Mot load = 0.5V
500 ms kill Rb

run pico_14N_872_0.05s.txt 50 ms trap time.
pico_14N_872_1.05s.txt 1.05 s in trap
pico_14N_872_0.35s.txt









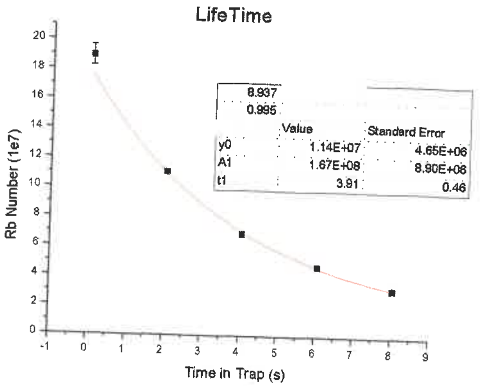
the trap widths are a factor of 2 large in size. See page 73 for explanation

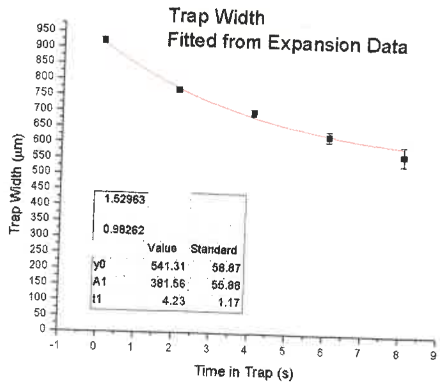
16 March 2016

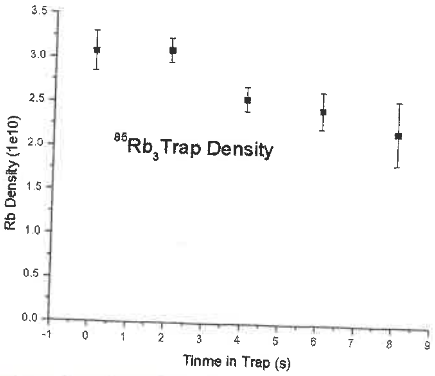
looking at collisions w/ E-trap at 7kV. ~~OT~~ Increased Rb density and probably longer lifetime

$^{87}Rb_2$ With 0.5 Sec Kill Time

7kV eTrap







17 March 2010

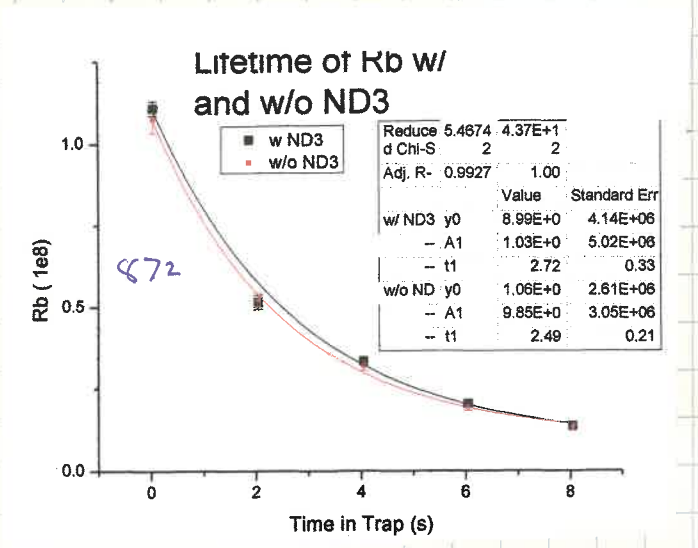
the cross-sections we are measuring are. One possibility, ~~[illegible]~~, is that Rb-ions are shortening the lifetime. (~~[illegible]~~ ions have big cross-sections). Its a hard thing to test for, but perhaps if we ~~we~~ have Rb ions, we also have ND_3 ions, and the ~~[illegible]~~ presence of ND_3 ions will shorten the Rb trap lifetime.

Two Rb lifetime measurements: 1 w/ ~~Rb~~ ND_3 and 1 w/o ND_3.

trapping at 8kV, no laser. All other parameters ~~[illegible]~~ the same as the collision parameters on page 56.

500 ms kill Rb time

using $^{15}ND_3$



I tried looking for Rb ions. With laser firing, they ~~[illegible]~~ arrive at ~5.3 us. Without the laser we see no Rb ions. We still don't ~~[illegible]~~ know for sure if ~~we~~ they are there, but we ~~don't~~ can't see them.

18 March 2010

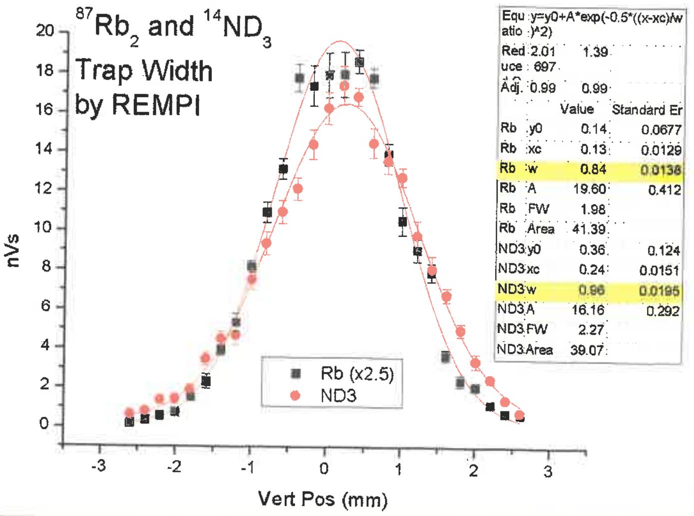
The ~~S~~ simulations show a different trapwidth from the data. This is a problem because the ND_3 distribution from the simulation goes into determining the weighted Rb density. If we are off on this measurement, then we are off also off on our cross-section measurement.

To check the pico motor calibration, I'm going to do a vertical scan of the Rb to compare that width w/ the ND_3 width.

MCP = -4600V, MOT Load = 0.5V, kill Rb = 500 ms
"detect time" = 50 ms

run pico_872_size_0.05sec.txt

8kV E fields ; 17mJ Laser



From previous temperature measurements we expect a width of 780 ± 22 μm in the weak dimension.

In vertical dimension, expected width 390 ± 11 μm
measured width 840 ± 13 μm } $^{87}Rb_2$

So we also see a factor of 2 error in measuring Rb width w/ ~~Rump~~ REMPI

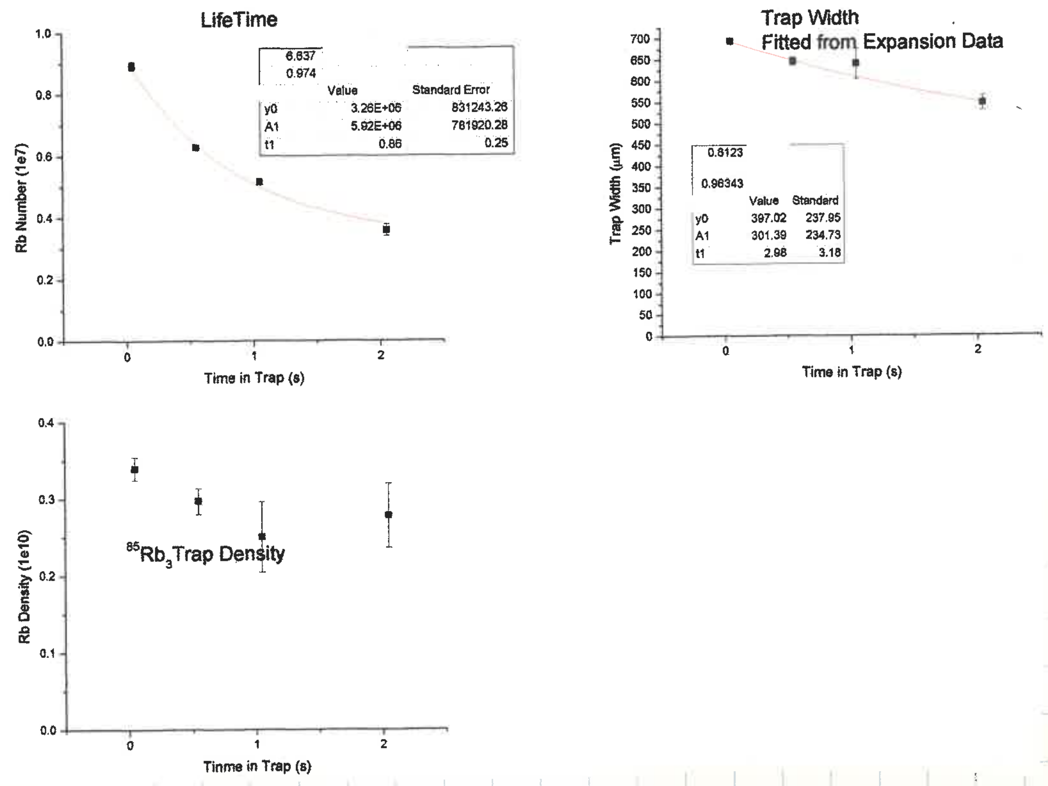
$\boxed{\text{I found the error in the pico motor program and fixed it.}}$

19 March 2010

Trying ^{87}Rb F=1 again. The last $^{87}Rb_1$ data did not fit very well, because of the very low density of $^{87}Rb_1$. I'm going to try it again except this time w/ 7kV electric fields to increase the density.

$^{87}Rb_1$ With 0.25 Sec Kill Time

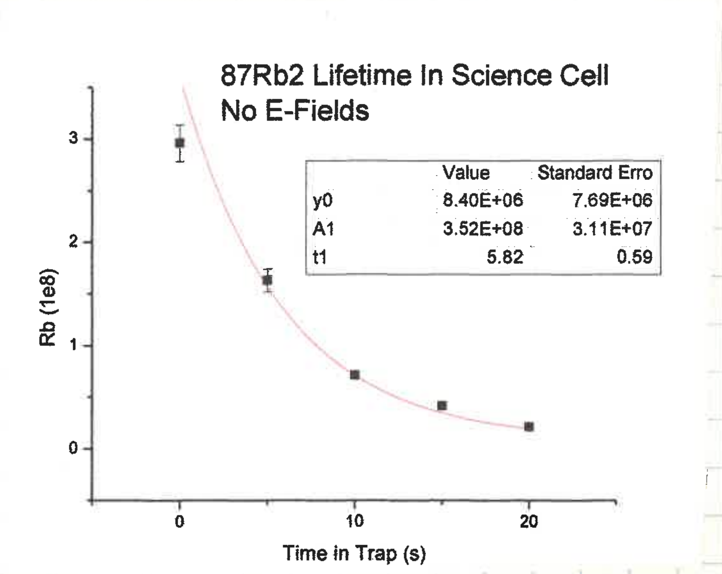
7kV eTrap

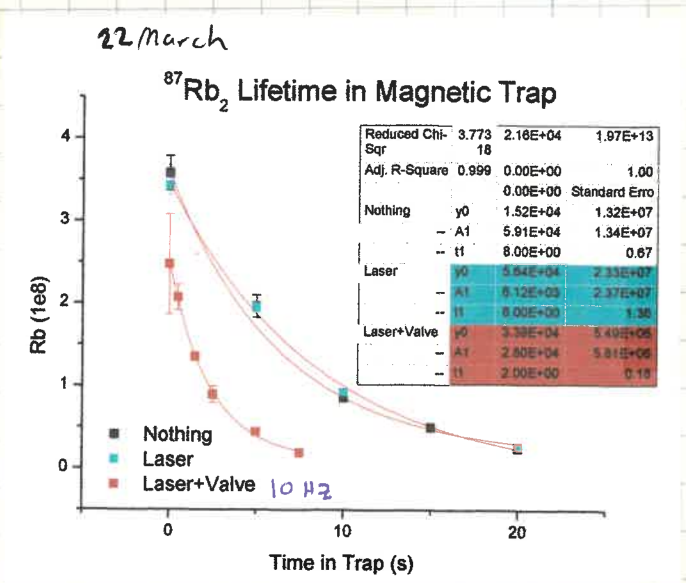


Run 14_87L_7kV killRb0.25sec.txt
15_87L_7kV killRb0.25sec.txt

If our cross-sections are really so large, then we should be able to see these collisions in a beam experiment. I'm going to measure the Rb lifetime in the science cell w/ no E-fields.

run 872_lifTime_noEfields_noValve.txt MOT Load = 0.5V





22 March 2010

Measuring lifetimes of Rb in preparation for beam experiment.

need to know: lifetime due to laser, valve, background.

I set up the laser shutter to open when Rb triggers the track position sensor.

run 872_lifTime_laser.txt - only laser on (17mJ)
↳ includes TOFMS

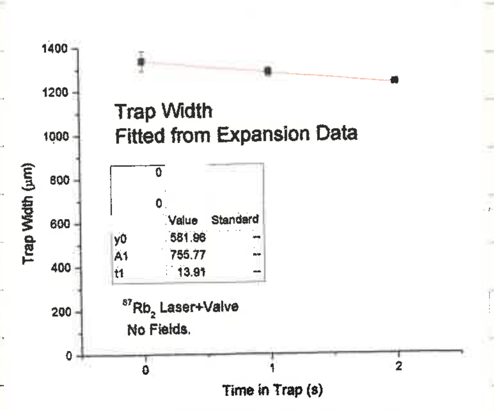
run 872_lifTime_laser_valve.txt valve at 10 Hz
Source chamber 1e-5

Next I want to verify the arrival of ND_3

Laser 31511 cm⁻¹ 17mJ
Slower 12kV
TOFMS 1200/800
Valve 280V
mcp 5000V

run 14_TOF_32.txt TOF trace at 32 m/s final velocity

to do the beam studies, we want to detect the molecules after they pass through the Rb ~~cloud~~ cloud.



W/ the Rb cloud being ~1.4mm, (gaussian width), we ~~will~~ will want to move it ~1 HWHM (~1.6mm) from the center.

← also includes TOFMS

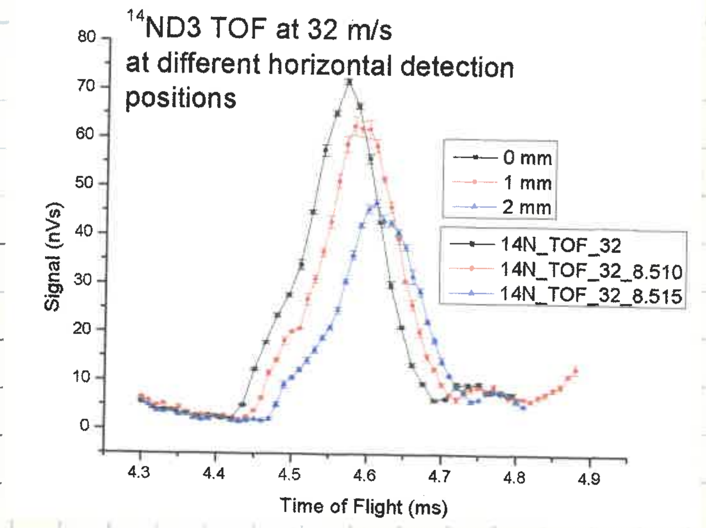
I'm going to take a few TOF traces at different horizontal laser positions.

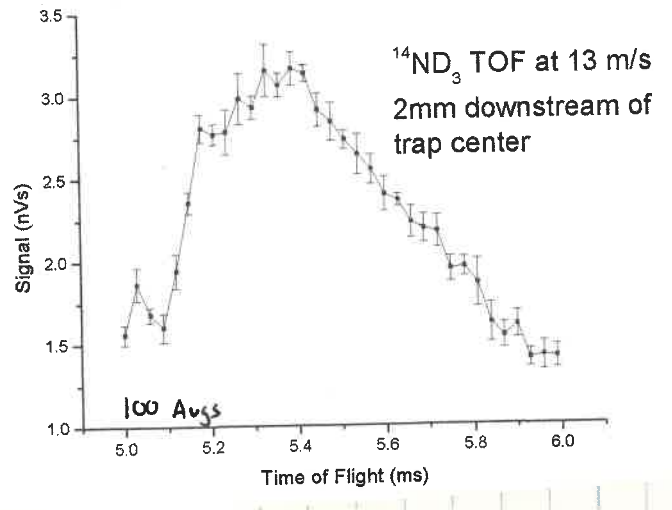
The zero position on the micrometer is ~~8.55 mm~~ 8.505mm

45.8 µm on the micrometer translates to ~1 mm in laser position.

micrometer position (horizontal)

run 14_TOF_32_8.510.txt
14_TOF_32_8.515.txt





23 March 2010

I modified the histogram in TotalControl_beta09_newDT.vi to be ~~able to take~~ handle an Rb point and a null point.

As a trial run, I'm going to slow to 13 m/s and do some histogramming

run 14_872_coll_13MperS.exe

$\phi = 61.8°$
detect time = ~~5.4 ms~~ 5.35ms
other parameters from previous page

24 March 2010

Doing another beam collision experiment. Getting a late start because of leaking cables and other problems.

run 14_872_coll_13MperS.txt same parameters as yesterday

Rb #. = 3.15e8 (calibrated 90%)
w/ a width of 1.3 mm
gives a density of 2E10

29 March 2010

Water cooled cables failed again (last friday). Hose was too short.

Today another collision run. Same as on the 24th, but ~~so~~ at 32m/s (better signal-to-noise)

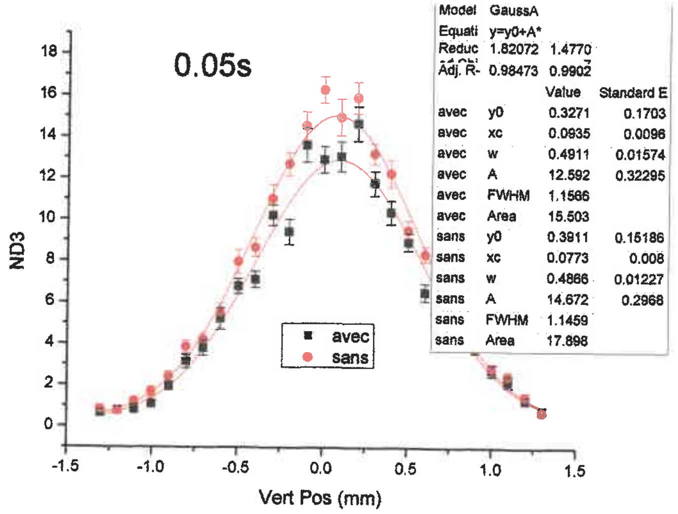
$\phi = 61.4°$

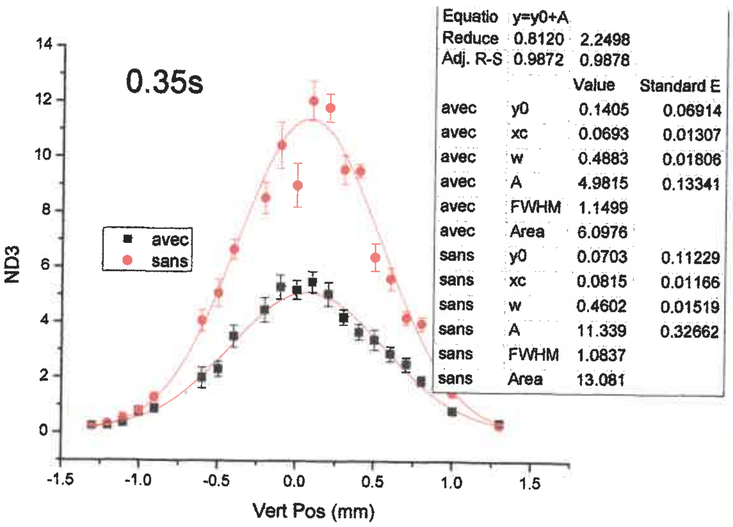
run 14_872_coll_32MperS.txt

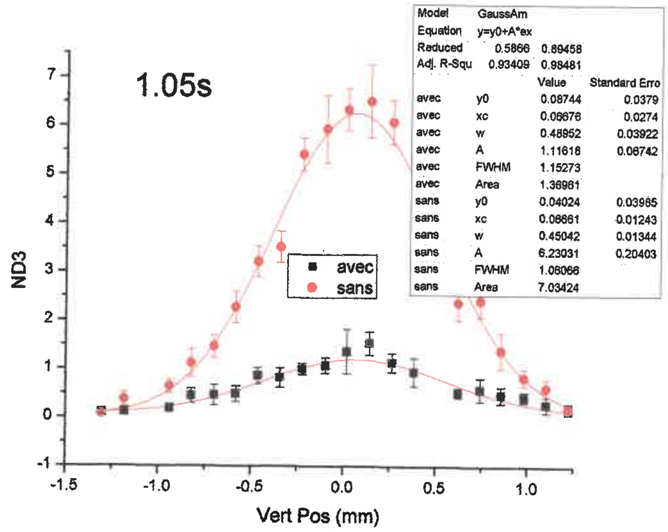
We have a column density on the order of 10^9. For a 1% effect we ~~expect~~ would need $O(10^{-11} cm^2)$ cross-sections. We are not going to see anything w/ the beam experiment.

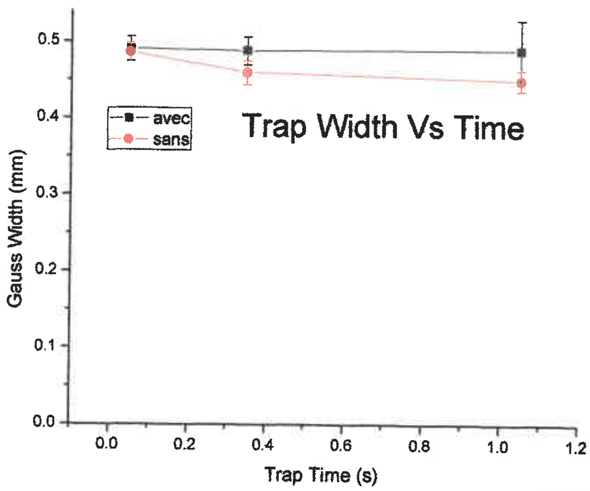
30 March 2010

I'm going to try another ND_3 trap temperature measurement with lower Rb density than before. This time it will be a 1 second kill Rb time.









31 March 2010

I want to double check that the magnetic field does not effect the measurement of the trap width.

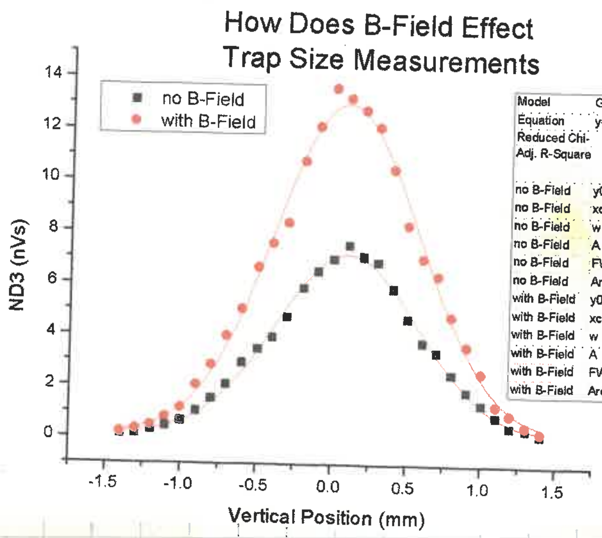
Still 14 ND_3

~~Laser 31511 cm^{-1} P: 17 mJ~~

Still running w/ same parameters as on pg. 56

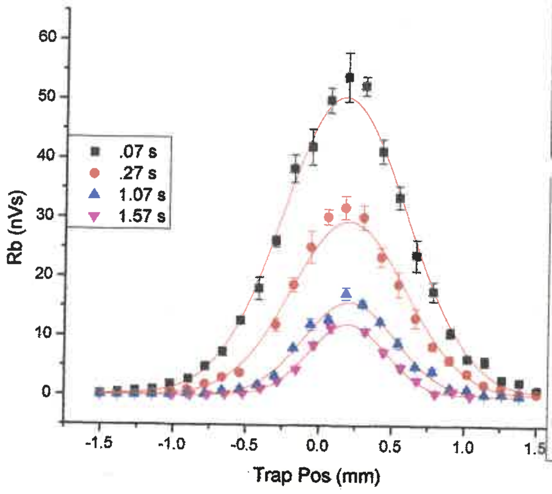
Sequence for taking data:
- move track to science cell,
- take 10 Avgs 3 times
- move track back

detect time = 70 ms
Everything at 10 Hz, scope is "anded" w/ track position.

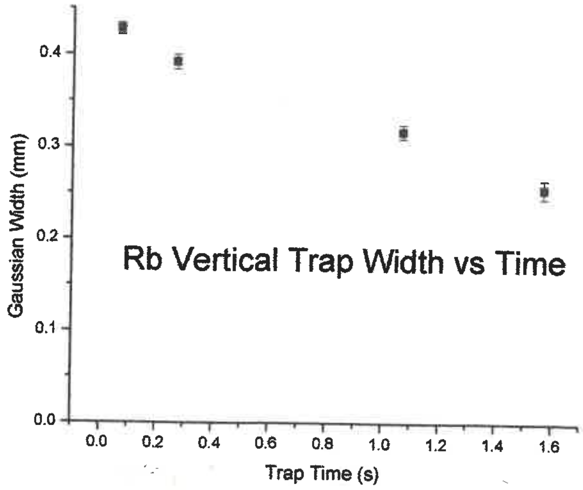


Model	GaussAmp	
Equation	y=y0+A*exp(-0.5*((x-xc)/w)^2)	
Reduced Chi-	0.04239	0.16156
Adj. R-Square	0.99321	0.9923

		Value	Standard Erro
no B-Field	y0	0.18959	0.09477
no B-Field	xc	0.09389	0.00683
no B-Field	w	0.47455	0.01104
no B-Field	A	6.98527	0.11166
no B-Field	FWHM	1.11747	
no B-Field	Area	8.30908	
with B-Field	y0	0.19118	0.19305
with B-Field	xc	0.08741	0.00734
with B-Field	w	0.48483	0.01221
with B-Field	A	12.8320	0.22084
with B-Field	FWHM	1.14168	
with B-Field	Area	15.5945	



Equation	y=y0+A*exp(-0.5*((x-xc)/w)^2)		
Reduced	2.188	2.12989	2.47875
Adj. R-Sq	0.989	0.97779	0.98305
		Value	Standard E
.07 s	y0	0.25173	0.06577
.07 s	xc	0.171	0.00808
.07 s	w	0.42799	0.00642
.07 s	A	50.29215	1.3697
.07 s	FWH	1.00784	
.27 s	y0	0.17415	0.04341
.27 s	xc	0.1983	0.0109
.27 s	w	0.39219	0.00812
.27 s	A	29.4013	1.13606
.27 s	FWH	0.92353	
1.07 s	y0	0.03004	0.00942
1.07 s	xc	0.20699	0.00863
1.07 s	w	0.3167	0.00749
1.07 s	A	15.845	0.56476
1.07 s	FWH	0.74578	
1.57 s	y0	0.09202	0.02665
1.57 s	xc	0.19493	0.00763
1.57 s	w	0.26295	0.00653
1.57 s	A	12.06385	0.46908
1.57 s	FWH	0.61919	



the Rb vertical position has been displaced (200 µm). This probably happened after the water cooled cable broke last ~~week~~ week.

two possible causes:

1) new water cooled cable is a couple inches too long
2) plate that mounts water cooled cable was lifted up to check connections.

7/2010

I'm going to try to re-align the Rb coils, the magnetic trap is about 200 µm too low according to the above data.

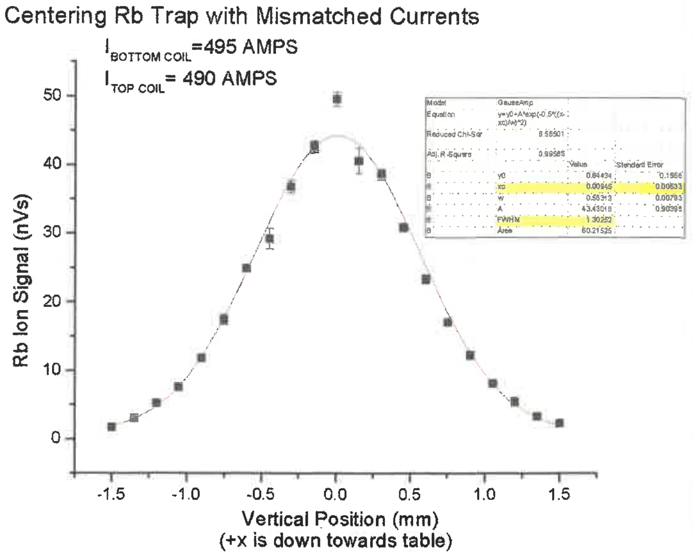
Physically applying force to the coils for short periods (~1 min) appeared to have little to no affect on trap center height [~10 µm if any difference at all].

According to pg 59 of the Rb book, I can correct the offset vertically by slightly misbalancing the current in the coils. The displacement should be ~25 µm/amp.

Need to push the atoms up, so more current in the bottom coil, 5 amps should do it.

see next pg.

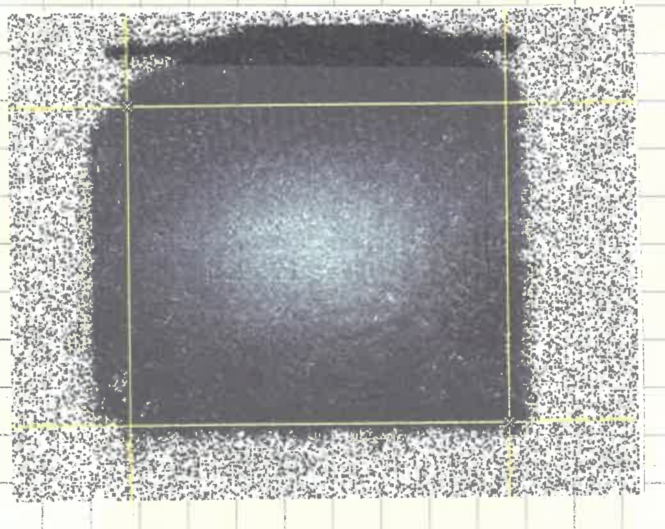
Vertical Alignment



I'll stick with this.

[picomotor scan,
MCP = 4700 V, ~16 mJ 31511.00 cm⁻¹

I now want to check the other two dimensions. The longitudinal (molecular beam dimension) alignment is done with absorbtion pictures in the science cell with electric fields on, using symmetry of picture as the metric.



This dimension looked good in all similar pictures, with both longer and shorter expansion times.

The Rb track dimension is aligned by Rb ion detection at different track positions. old center is 52.36 cm. See top of next page.

12 April 2010

Trapped molecule signal has steadily been dropping over the last 6 months or so.
I want to investigate to see if possibly the MCP is dying.

I'll start with free-flight traces. New gas ($^{14}ND_3$, 5% in Kr) mixed on friday. Settings are as follows:

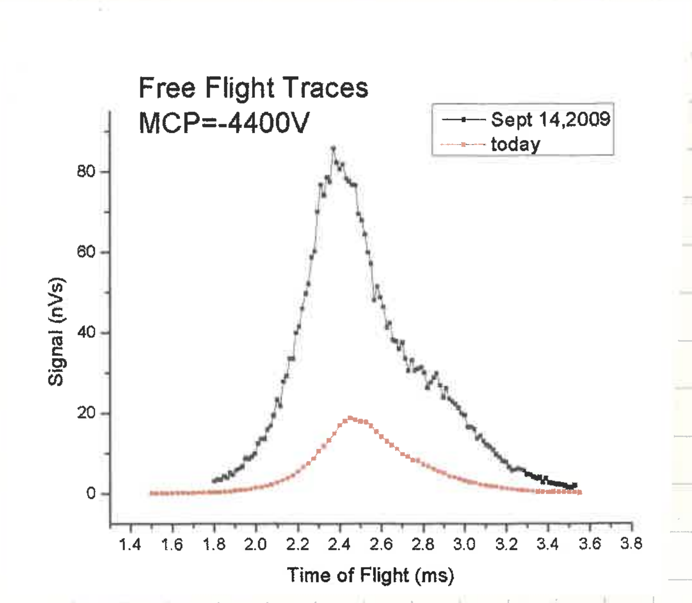
laser: 3151 cm^{-1}, 16 mJ
valve: −280V, P_{source} = 8.0×10^{-6} torr, 4 psi backing
TOFMS: 1200V, 800V, 0V, 0V
MCP: −5000V unless specified otherwise.

I'll be comparing to 14 Sep 2009, ~100 nVs at MCP = −4400V.

run freeFlight01.txt — signal is definitely down, only 35 nVs on peak. ~~(also arriving early)~~
flushing the valve...

freeFlight02.txt — significant improvement, arrival time later.
SATURATION @ ~90 nVs
Turning MCP down to −4400V to compare w/ previous data

freeFlight03.txt



I'll now try a couple of valve tweaks.

freeFlight04.txt ← valve up to −330V, 1.5×10^{-5} torr
no affect on signal

Signal Appears to be down by a factor of ~4 since sept 2009.

80 nVs Vs. 20 nVs → now we have ~14 nVs

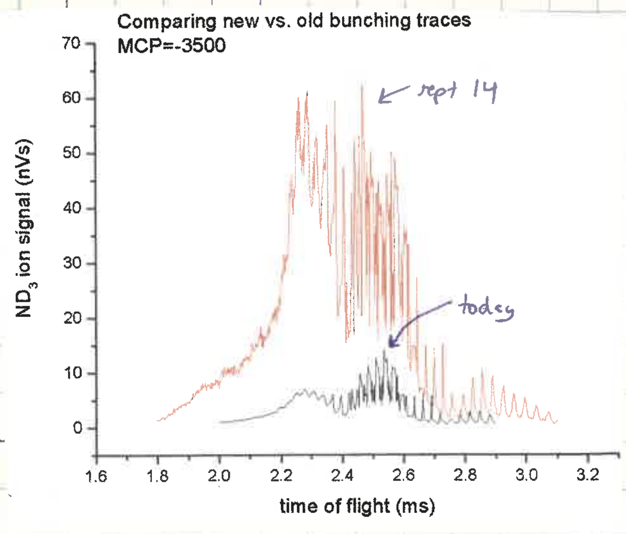
next trapping data had ~3.5 nVs trapped w/ B-fields

So roughly, $\dfrac{\text{free flight signal @ -4400v}}{\text{trapped signal @ -5000v, w/o B-fields}}$ hasn't really changed much since about 5 months ago. But we have lost about a factor of 4 across the board. Testing free flight removes most variables, signal could be down because of:

1) MCP efficiency is dying
2) gas mixture / deuteration
3) some other valve change/problem.

I would also like to look at a bunching trace to verify. MCP = -3500V, No B-fields
Again, on sept 14 2009, we had ~ 50 nVs peak heights at these settings.
(± 12kV slower).

run bunch_s1_v415_d550us.txt



Loss in free flight shows up as loss in bunching, signal heights down by about a factor of 4 $\left(\frac{60}{15}\right)$

19 April, 2010

Replaced MCP plates on friday and baked the chamber over the weekend.
After cooling down, science cell is in the mid -10's.

Conditioned MCP, going to take a free-flight trace...

run free_flight_01.txt MCP = -4400V

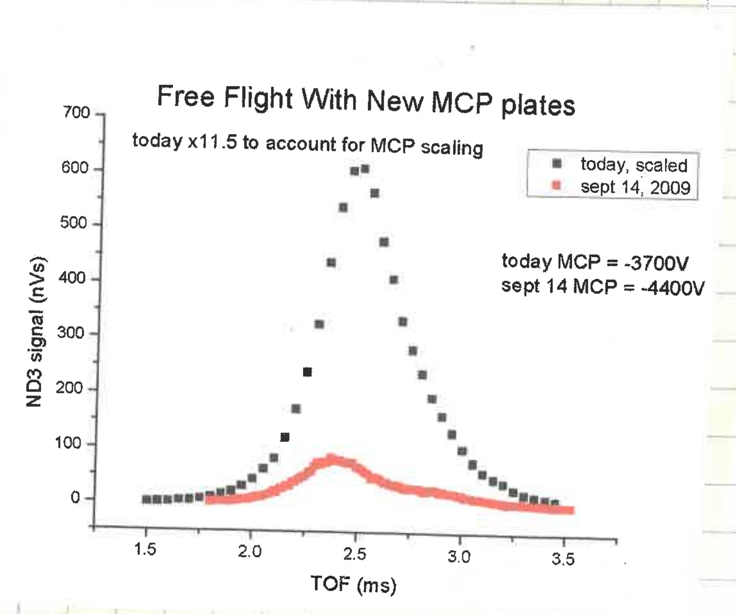
Signal is back up! Saturated above ≃ 70 nVs

turning MCP down to -3700V

@ 1.9ms (off peak ⇒ no saturation) $\dfrac{-4400v}{-3700v} = \dfrac{21.7\ nVs}{1.89\ nVs} \simeq 11.5\times$ gain run free-flight_02.txt MCP = -3700V

I'll assume that 11.5x gain is linear so that I can compare our new signal to september 14, 2009.

With this scaling, we have ~30 times more signal than last week. Once the slower is fully conditioned, I'll take a bunching trace.



Now I'll condition the trap and finish conditioning the slower. See decelerator chamber logbook.

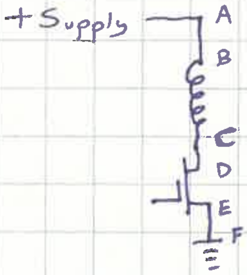
April 26, 2010

Experiment is now totally re-assembled. A slightly worrying note: some time during the box re-assembly the science cell ion gauge ~~startin~~ started reading in the low -9's. It had been happy in the mid -10's. I can't find any leaks. Rb lifetime in the science cell should tell us if we have a problem. However, when I went to measure the Rb lifetime I noticed that the supplies were sagging after ~1-2 seconds. Measuring the cooling water flow:

booster inlet : 59 psi
booster outlet : 94 psi

⇒ resulting flow through coils: 0.5 gallons/minute.
just top : 3:31 / gallon
just bottom : 3:56 / gallon

Measuring voltage drops along the current path:



top coil path   bottom coil path

@ 490 Amps (top), 495 Amps (bottom)

	top path	bottom path
AB	1000 mV	790 mV
BC	5.95 V	6.36 V
CD	190 mV	190 mV
DE	900 mV	750 mV
EF	40 mV	34 mV
AF:	8.12 V	8.12 V

← these seem very high (AB)

← could indicate 1 FET on top is blown (DE)

Seems as though we are just on the edge of our operating capability.

(1) Adding a fifth mosfet to each bank (top + bottom)
(2) Built a "counter-flow heat exchanger" from 0.5" OD copper tubing inserted in 3/4" garden hose, 20 ft long. To be used with refridgerated recycler, filled with ethanol.

(1) seemed to help a bit
(2):

chiller set to 5°C

outside temp: 52°F = 11.1 °C

[A] No booster, (52 psi outlet) base water temp no chiller: 13.3 °C
w/ chiller: 10.3 °C

[B] YES booster, (87 psi outlet) base water temp no chiller: 13.6 °C
w/ chiller: 12.6 °C

not doing much, worse still, it causes 10 psi drop.

28 April, 2010

The last day or so we have been dealing with what we thought to be a leak in the decelerator chamber. After baking/conditioning was complete the pressure in the science cell was in the mid-to-low -10's. After box assembly and checking laser alignment (sirah), the pressure was around $\sim 1.0 \times 10^{-9}$ torr. It has stayed there over the past ~4 days. We also saw that opening the gate valve seemed to hurt the MOT, yesterday we could only load to ~0.02 volts. Closing the gate valve, the MOT improved over the day (slowly), to about 0.05 volt load. Turning the getter up to 3.75 A, then, 4.0 A, then 4.25 A had no effect on the mot-load. We switched to getter #1 and within 1/2 hour, mot loaded to 0.1 V

(inally … Amps)

Getter 1 was left at 3.25 A overnight. This is our last getter.

This morning the mot loaded to 3.6 V with $\tau \sim 7$ seconds. (:))
I turned the getter down to 3.0 amps.

We may still have a leak in the decelerator chamber, but at least the mot-problem is fixed. I'm going to open the gate valve and take lifetimes at various positions with no valve, no E-fields, just the magnetic trap.
The supplies are still sagging, I ordered a larger booster (+45 psi instead of our current +30 psi) to try and remedy this.

Getter #1 down to 2.85 Amps.
MOT LOAD ($^{87}Rb_{F=2}$) is now:

$\tau = 10$ seconds
$N = 3.2 \times 10^9$
$L = 2.8$ V

I'm currently running the supplies at ~~[illegible]~~, (top, bottom) = (475A, 480A) to avoid any sagging (good for $T \geq 10$ seconds)

2 have ... unlocked. →

lifetime in science cell: $\tau \sim 1.5$ seconds — 52.36 cm, $P = 1 \times 10^{-9}$ torr
in bellows: $\tau \sim$ ~~3~~ ~~seconds~~ 2.8 sec — 42 cm
in MOT imaging position ~~[illegible]~~ $\tau \sim 10$ seconds.

⇒ Looks like we need to bake the molecule chamber again.

---

30 April, 2010

Chamber has been baking for a couple of days now (just molecule chamber). Pressure stagnated at 1.4×10^{-8} torr, turned off the bake this morning. 3 hours later saw 1.0×10^{-9} torr in the science cell.

Thought: Perhaps the pressure jump in the molecule chamber earlier in the week was a result of the getter dying in the Rb cell.

I've also made some improvements to the magnetic trap coils:

***

1) Installed new water pressure booster (+45 psi, old one was +30 psi). Pressure after the booster is now ~115 psi, inlet pressure is 60 psi.
2) Replaced welding cables that connect + supply to coils [both, the long ones]. This was a result of seeing how resistive the old cables were [table, last pg]
Upon opening up the old cables ~ 1/[illegible] of the wires had broken off

on the coil connection side. This caused high resistance.

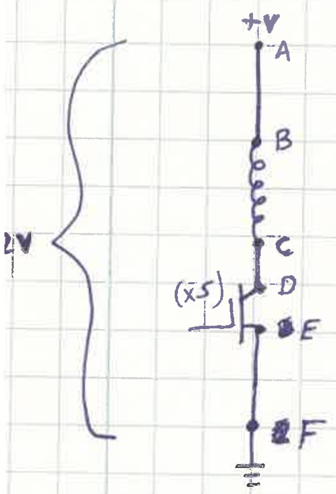
The new trap coil behavior is:

flow through coils: 1:43 /gallon ⇒ 103 seconds/gallon ⇒ $\boxed{0.58 \text{ gallons/minute}}$
base water temp: $\boxed{15.2^\circ C}$

[No counter-flow chiller being used at this time]

I'll look again at the voltage drops in the system to have a baseline for the future, by the way...

$\boxed{\text{COILS NOW HOLD 500 A for} \geq 10 \text{ seconds}}$ (cables were biggest problem).



500 amps	top coil	bottom coil
	mV	
AB	287	271
BC	6020	6640
CD	187.5	195
DE	1580	972
EF	40.5	30

remember that the voltage across the mosfet bank will ~~do whatever it can to~~ not be as low as it can be until railing/sagging begins. Each FET is rated to [fully open] 7.5 mΩ on resistance, there are 5 per bank, so $R_{FETs} = 1.5\ m\Omega$.
At 500 amps, the ~~most~~ min voltage across the FETs should then be ≃ 750 mV.
(corresponding to them being fully open).

7 hours after turning off the bake, $P_{science} = 5.5 \times 10^{-10}$ and falling
$P_{slower} = 1.7 \times 10^{-9}$

Re-assembled the box, no change in pressure, $P_{science} = 4.9 \times 10^{-10}$ torr.

3 May 2010

It looks like there has been a power outage this past weekend. All diode lasers were off. Rb and Sarah computer were also off. Everything else was fine though. The big scope was still on. Can't talk to UPS to verify what happened.

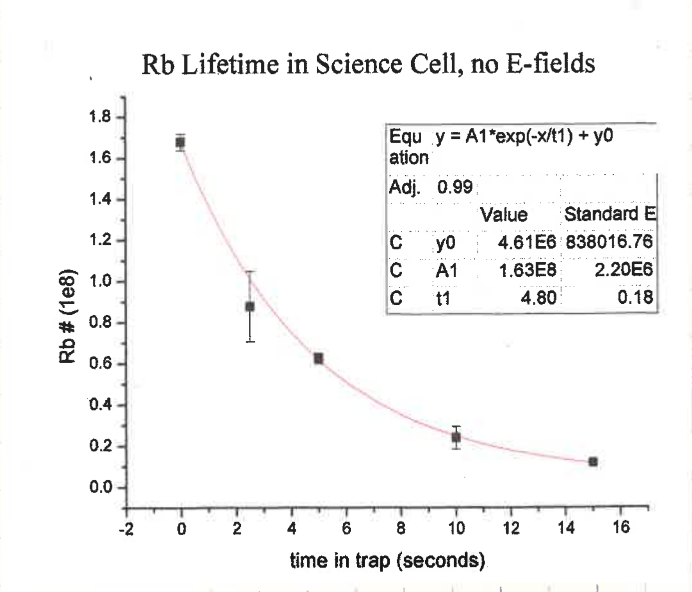
MOT Parameters: Load time: 16.8 second
Amplitude: 1.2 V
N: 1.4e9

Gate valve still closed

getters at 2.99A since friday.

after opening gate valve τ = 16. sec
A: 1.2V
N: 1.37e9

turned getter up to 3.1 Amps



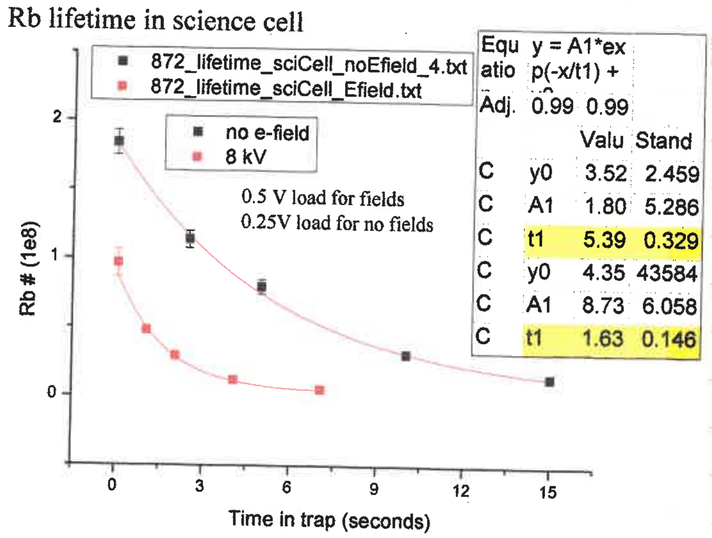
0.5V load, various expansion times

"872_Rblifetime_sciCell_noEfield_1.txt"

* While attempting to measure Rb lifetime w/ E-fields we had what appears to be a trap discharge event, $P_{science}$ jumped up to the low -7's, then (now) is slowly dropping. Now in the high -10's, Rb lifetime w/o E-fields has dropped to ~4 seconds. Event occured @ ±8kV, 270 ms on time (15% duty cycle) [3rd time at this parameter set]

Now trying to switch condition the trap. 70 ms on time, 250 ms total time, slowly increasing voltage. Then increasing trap time.

⇒ Saw no events up to ±8kV for 7 seconds. When



872_lifetime_sciCell_Efield.txt
872_lifetime_sciCell_noEfield_4.txt

Those lifetimes seem long-enough to run [barely], so I'm going to move on.

I'm now going to determine how large a single ion signal is on the new MCP plates. With their specified gains, I expect 1 ion ⇒ ~ 12 nVs. I'll do this with free flight + purposefully off timing. [@ −5000V]

We are really interested in why our signal is larger than with the old plates. This could be a result of (1) detecting more ions, or, (2) simply have more gain. (2) won't help S/N, but (1) certainly will. We're also interested to see if we can change the saturation voltage (which right now corresponds to ~ 80 nVs) by defocusing the ion beam [by adjusting the TOFMS plate voltages].

SEE PG 92/93

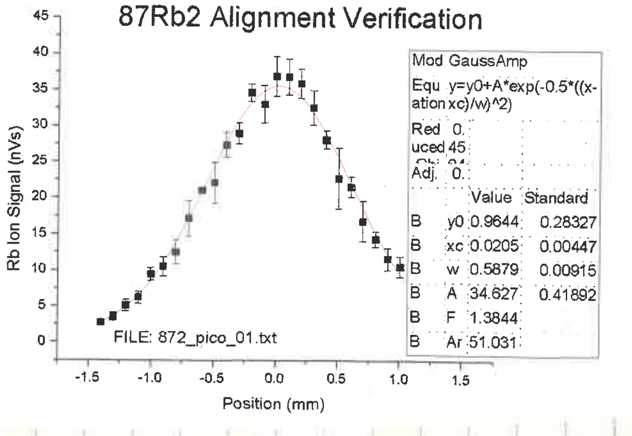
4 May 2010

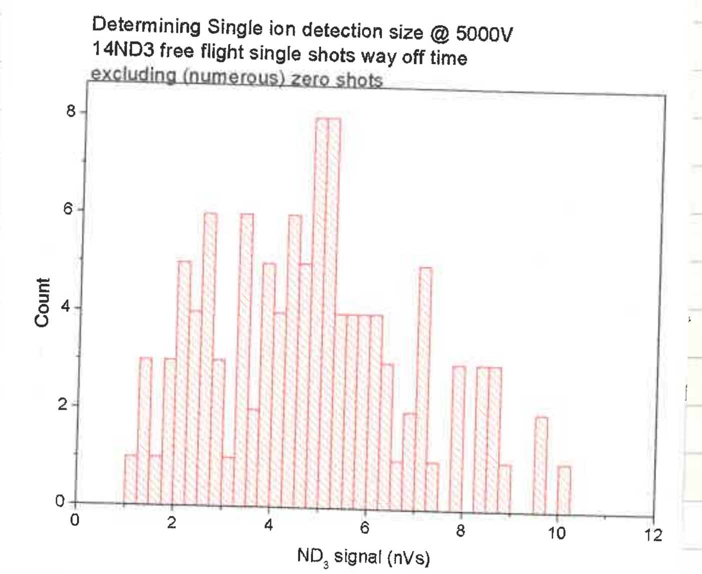
I'm going to check the Rb alignment. ~~in~~ Starting with the vertical dimension,

Laser 18 mJ at 31511 cm⁻¹
MCP −3800V 1st stage gain
MOT load to 0.7V

(5 scope Avgs) x (3 labview Avgs)

top coil : 495 Amps
bot coil : 500 Amps



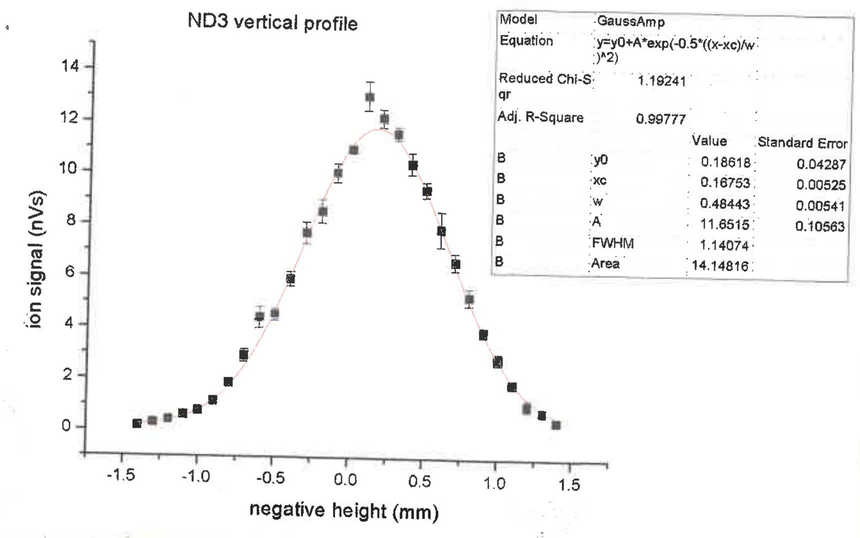


Not enough stats, but this appears to be a single distribution, so,

$\boxed{1 \text{ ion} \simeq 5 \text{ nVs}}$

I don't believe this #

Now testing saturation/defocusing : $\boxed{\text{saturates at } 115 \text{ mV}}$
No adjustment on back TOFMS plate changes this



Strange
Appears that trap vertical center has shifted down slightly. (~170 μm).

We'll have to re-align the Rb vertically, balancing the coils (500A, 500A) should fix ≈ 125 μm of this offset.

Now looking at B-field effect on signal. Effect is huge. Not only that, but the saturation voltage is now ~ 1 volt.

Back to free flight, saturation is @ ~1.6 volts. Something here doesn't make sense.

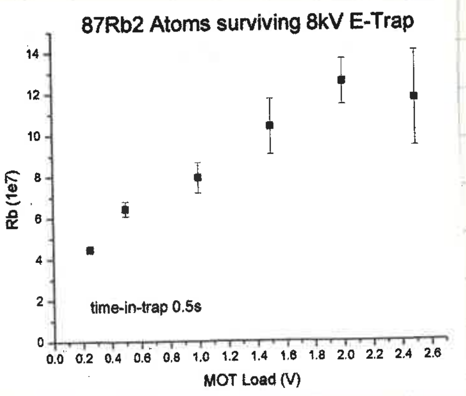
5 May 2010

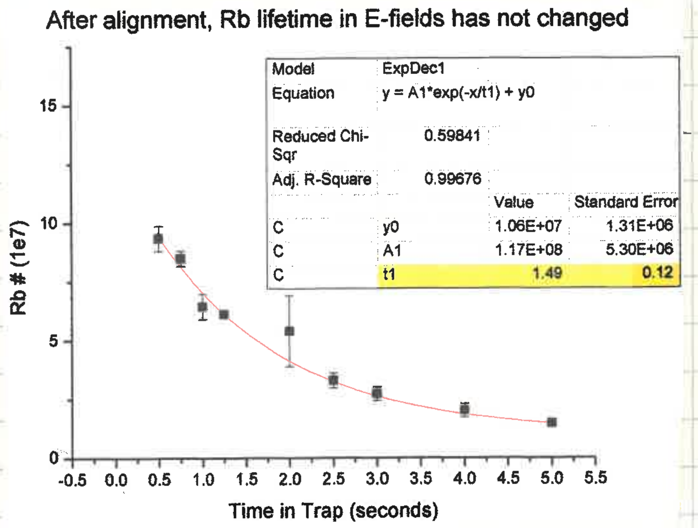
I'm going to take vertical Rb profiles w/ electric fields on.

Laser 31511 cm^{-1} 16mJ   MOT Load ≈ 1 V
MCP −4800V
Etrap 8000 V
B-trap 500/500 Amps

Run 872_pico_0.07s_exc profile at 70 ms

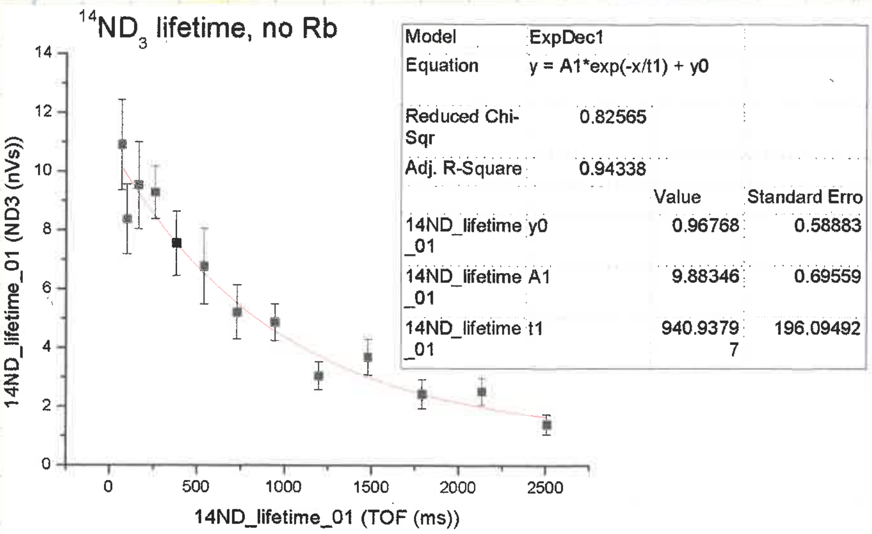
Actually before we do this we need to decide ~~at~~ what MOT load level we want to use.





Using 1.5V mot-load

At one point the lifetime of $^{87}Rb_{F=2}$ in electric fields was ~3 seconds. That was, however, after not breaking vacuum for many months. Is ND_3 lifetime down as well?



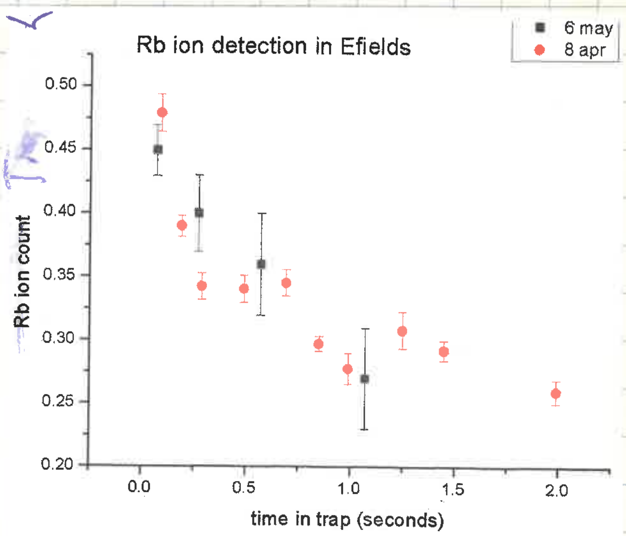
Yes, also down. Used to be 1.5 secs.

6 May 2010

Yesterday we tried to take Rb profile traces. Unfortunately the laser was losing power throughout the day and since these profiles take ~1 hour to run, the power sag was ~~so~~ effecting the results.

Therefore we replaced the Yag FL (to get more power) and replaced the dye as well (the Sirah is what was sagging). We got 23 mJ, but then it sagged to 14 mJ. We opened the air intakes above the desks and dropped the ~~air~~ room temp by 10°F. The laser power increased to 17 mJ and held for the afternoon.

The Rb lifetime is short, ~1.5 seconds. (in E-fields)



(10 avgs)

7 May 2010

Sirah power is 18.5 mJ, steady so far.

We're going to need a better set of data (more points, smaller error bars) than just above for simulation verification.

First I'm going to look at our molecule (trapped) signal to noise, both with and without B-fields.

run hist_B-fields_50ms.txt — 50 ms trap time, MCP = -4600 V
$^{14}ND_3$, 3 sec trap ~~time~~ period.

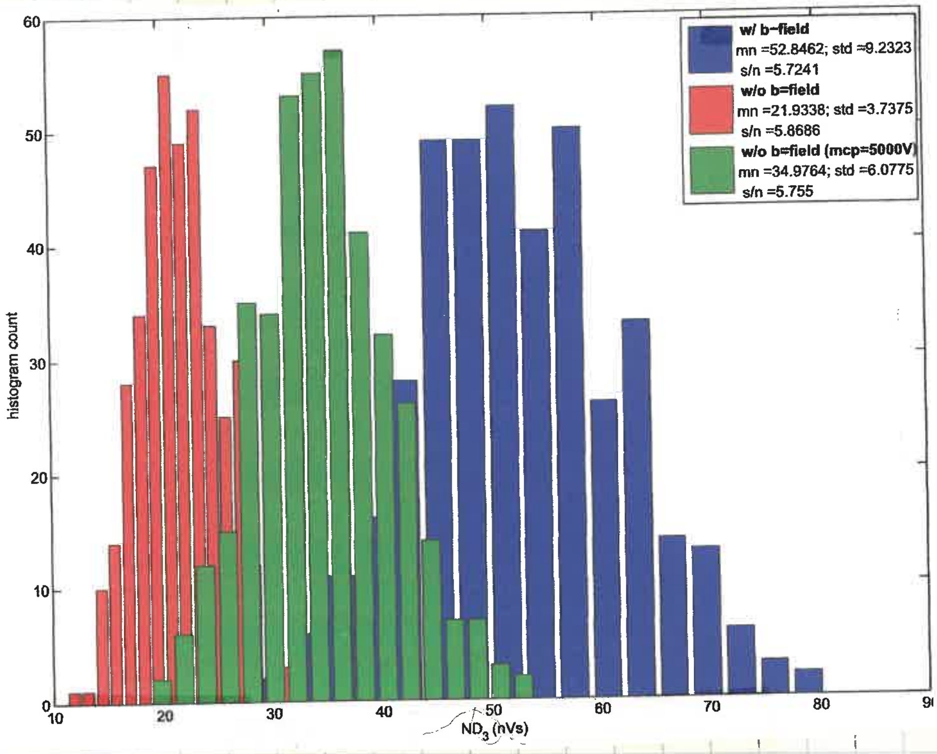
Compare histograms to Aug 17, 2009, @ ±8 kV (no B-fields, -5000 V MCP)
signal = 24.55 nVs, std dev = 6.861 nVs

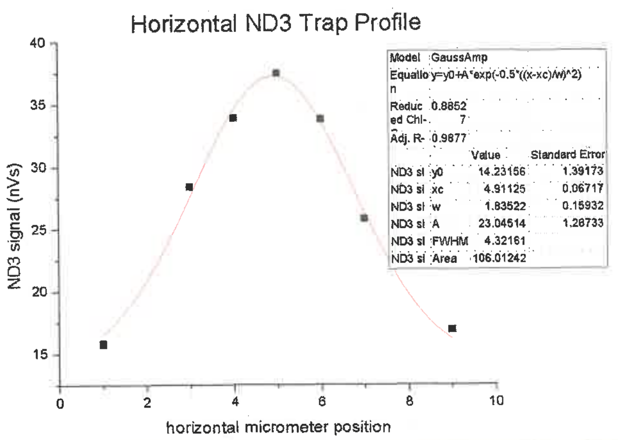
now to do it w/o the B-fields. Keeping everything the same.

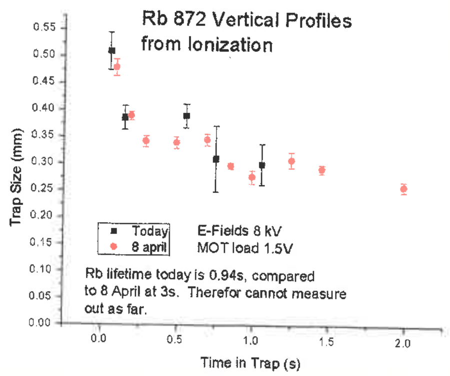
mcp = -4600V

run hist_noB-fields_50ms.txt

run hist_noB-fields_50ms_02.txt   mcp= -5000V





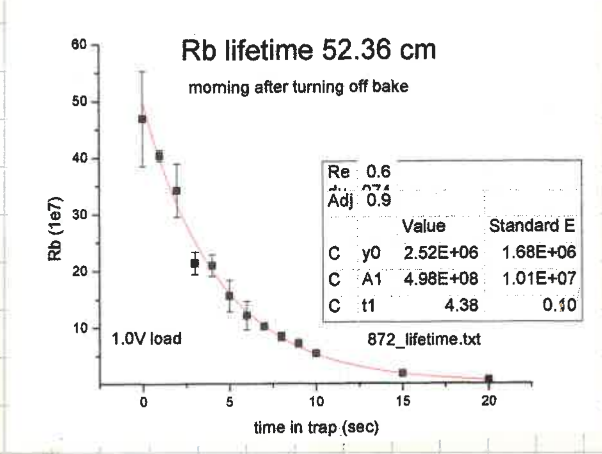


14 May 2010

The last 3 days we spent baking the chamber ~180°C hoping to increase the Rb lifetime in the trap region

The MOT beams were mis-aligned so we made small tweaks

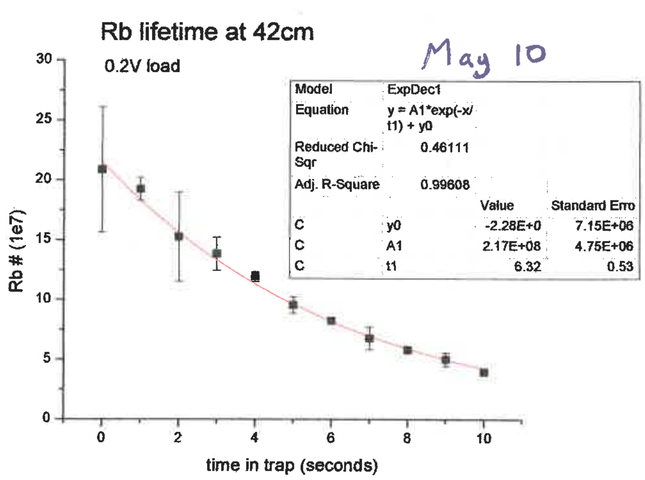
MOT Load = 3.3V (w/ gate valve open)
τ = 6.8 sec

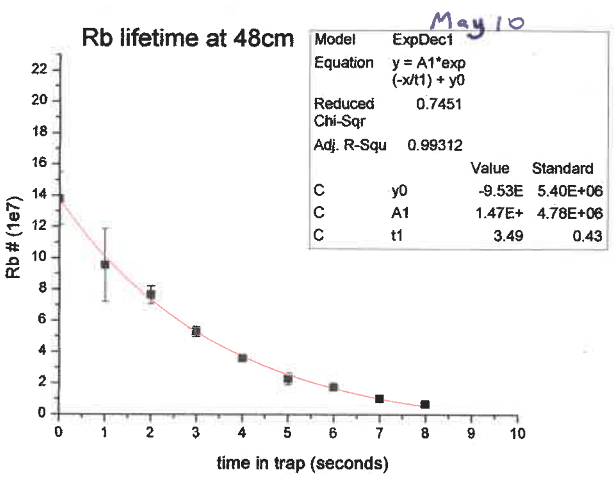


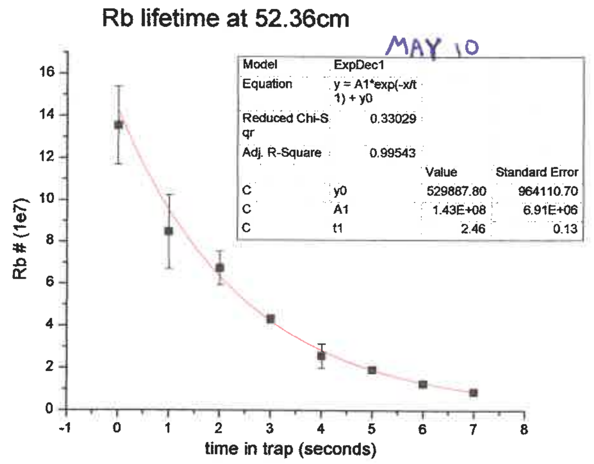
The lifetime is still short. We expect after running the valve it will be even shorter.

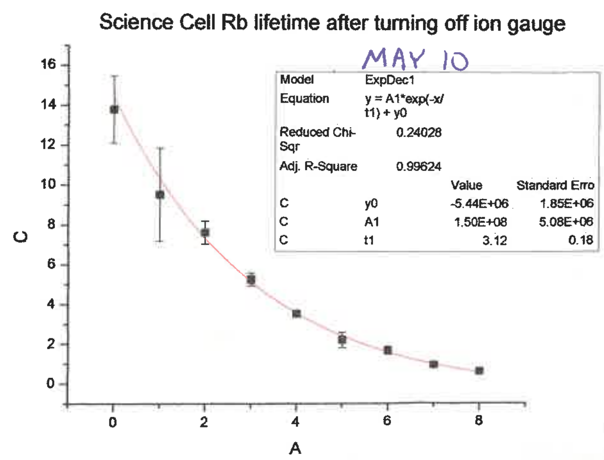
We will wait a few days and measure it again, maybe it will improve still.

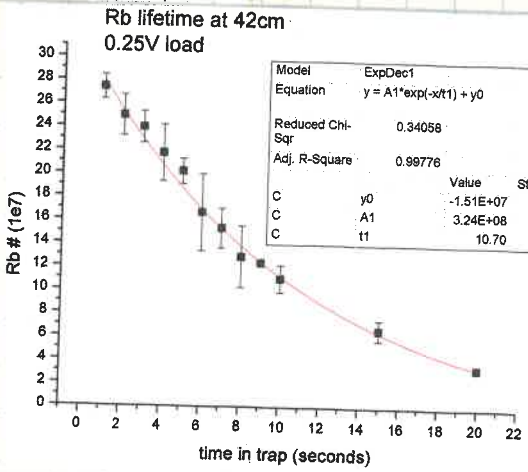
I'll look at a couple of other track positions to compare to last week:

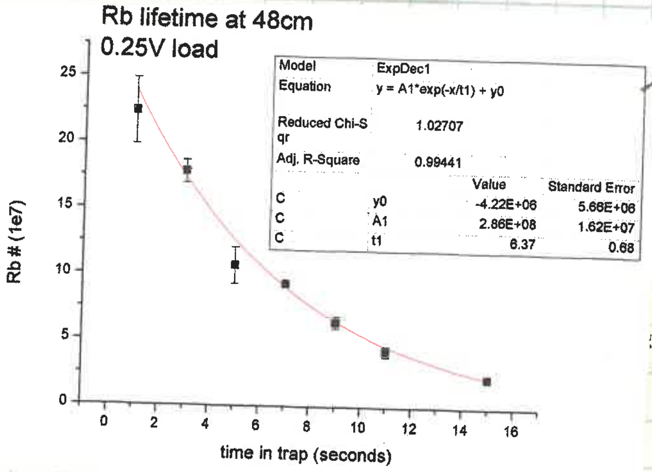












Lifetimes at these positions have improved.

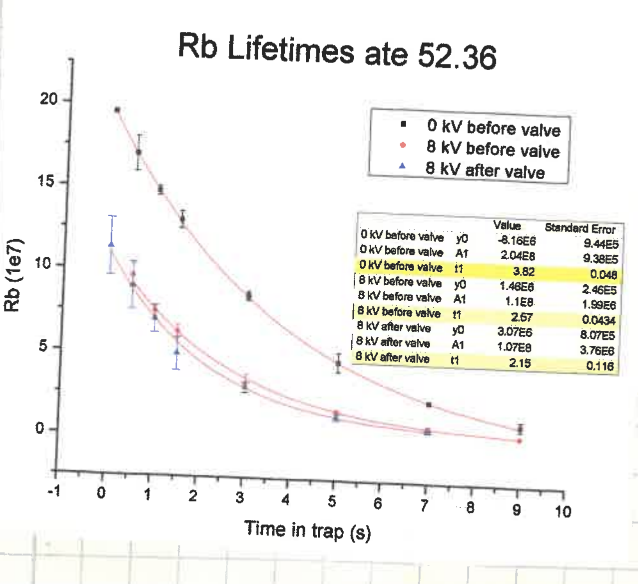
Now conditioning the slower, then the trap.

17 May 2010

The trap and slower have been conditioned. We still have not run the valve since we last baked, and its been 3 days since we last measured lifetime. I'm going to see if the lifetime has improved since then.

MOT Load : 0.25V

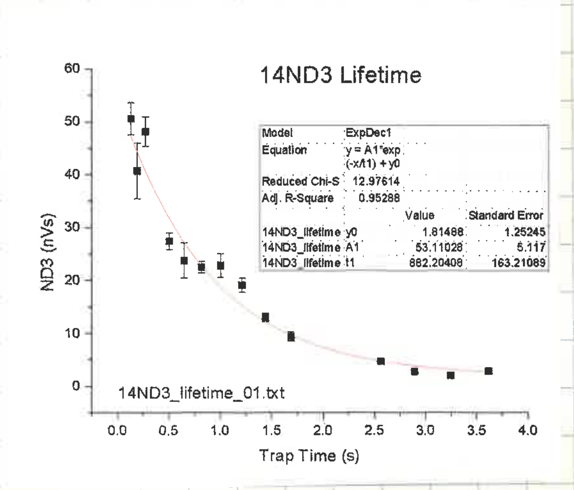
872_lifetime_52.36_0kV.txt
872_lifeTime_52.36_8kV.txt
872_lifeTime_52.36_8kV_afterValve.txt ... ran valve at 10 Hz for 10 minutes also fire single stage pulse when Rb arrives (standard running procedure)



coils @ 500A / 500A

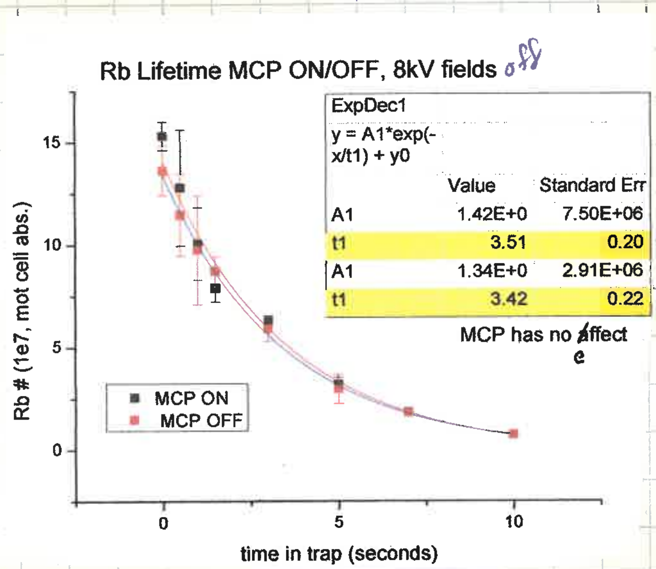
Now we want to see how the ND_3 lifetime is.

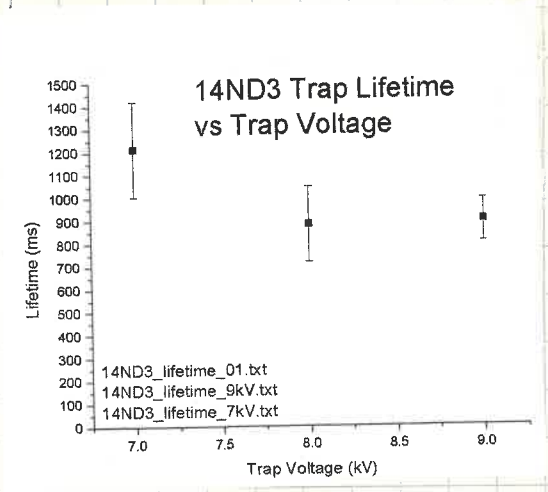
Laser 31511 cm^{-1}, 19 mJ
MCP -5000 V, TOFMS 800/1200
Trap 8 kV



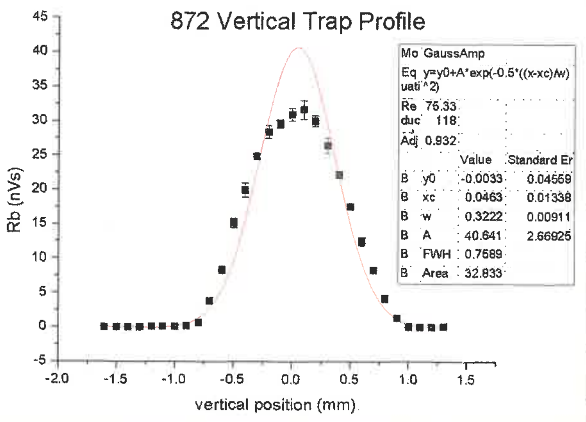
the ~~ND3~~ Rb lifetime is slightly up from the last bake, but the ~~Rb~~ ND_3 lifetime is not improved at all.

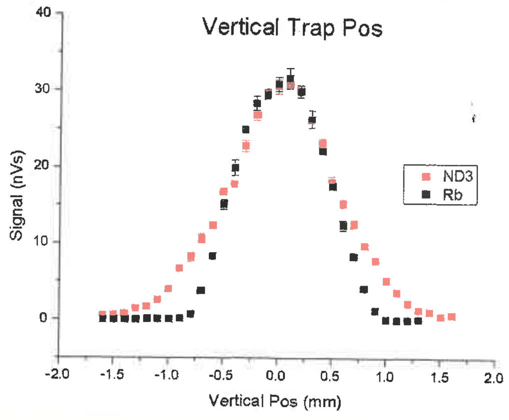
there is a question as to if the MCP being on has some effect on the lifetime.



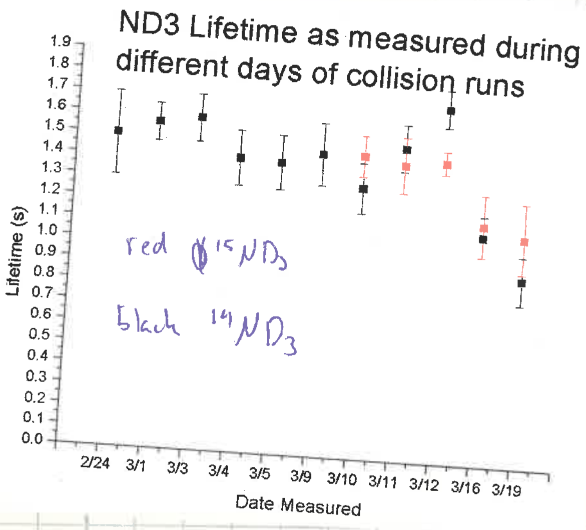


Again to check the Rb alignment to the trap





ND_3 is scaled

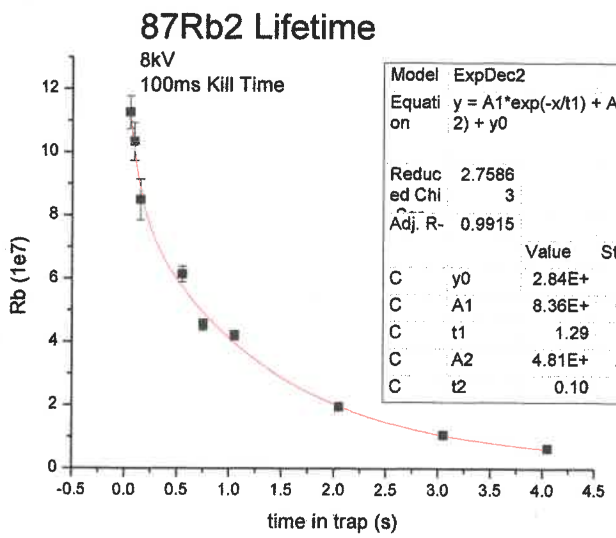


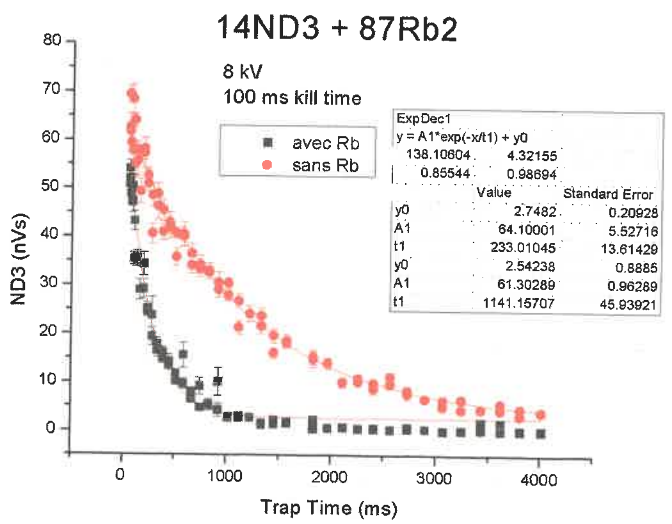
$\boxed{\text{18 May 2010}}$

Going back to collisions today $^{87}Rb (F=2)$ w/ $^{14}ND_3$

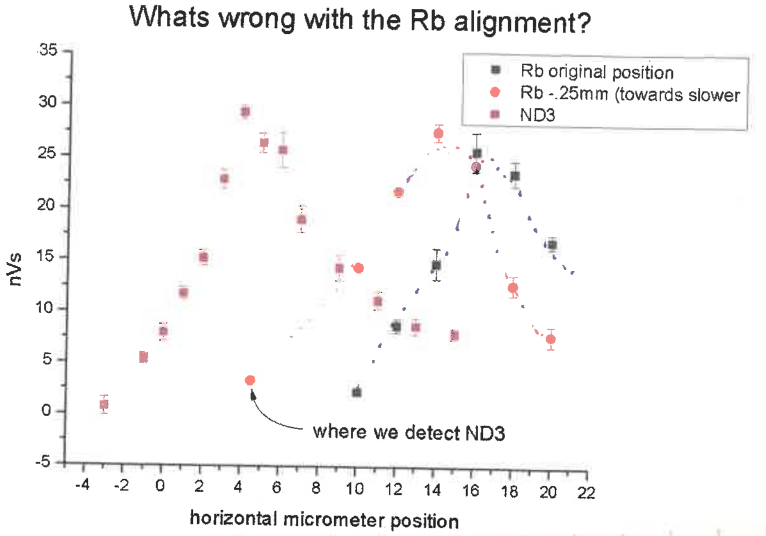
Laser 31511 cm^{-1} P= 19mJ
MCP -4500V
Trap 8kV
MOT loads 1V
Valve 280V
kill Rb 100 ms

40 points taken @ 2X
15 Amps
~7 hours



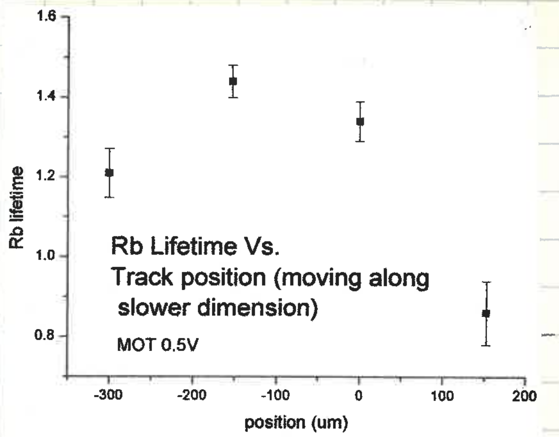


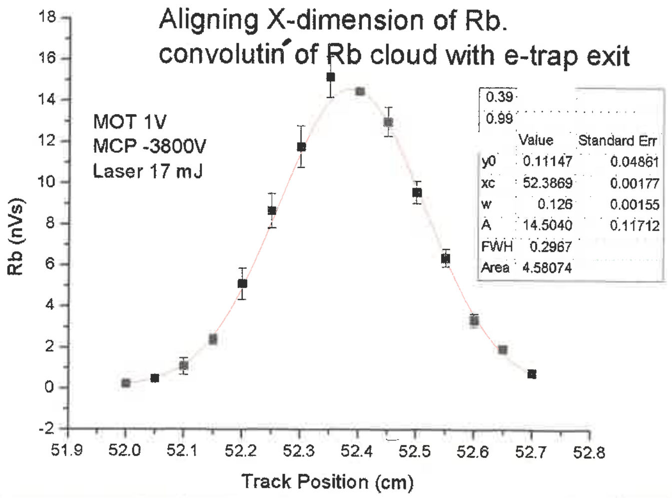
19 May 2010



from doing horizontal laser scans of the Rb in the e-trap, it looks like we are completely misaligned

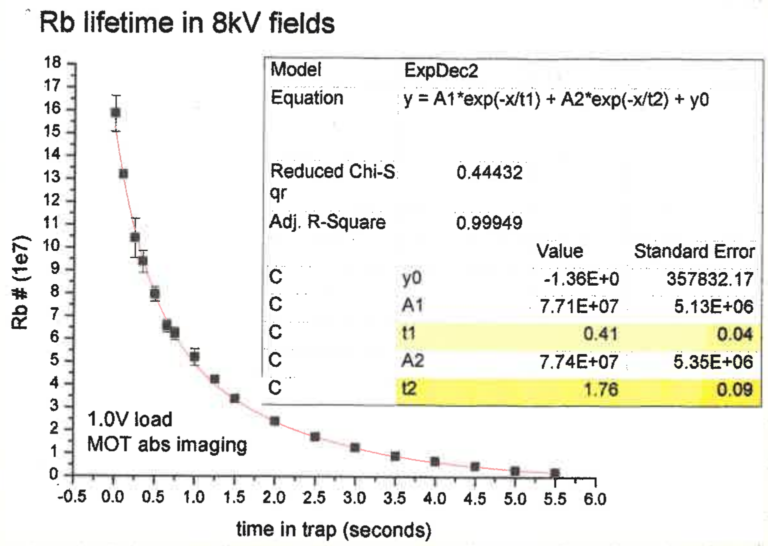
However when we move the track such that it looks aligned from the laser scans, the Rb barely survives the electric fields.





Repump power is down to ~ 7 mW. Tweaked fiber coupling, now ~ 14.5 mW.

And now another lifetime measurement of Rb in electric fields, fits poorly to a single exponential, but a double exponential does a good job.

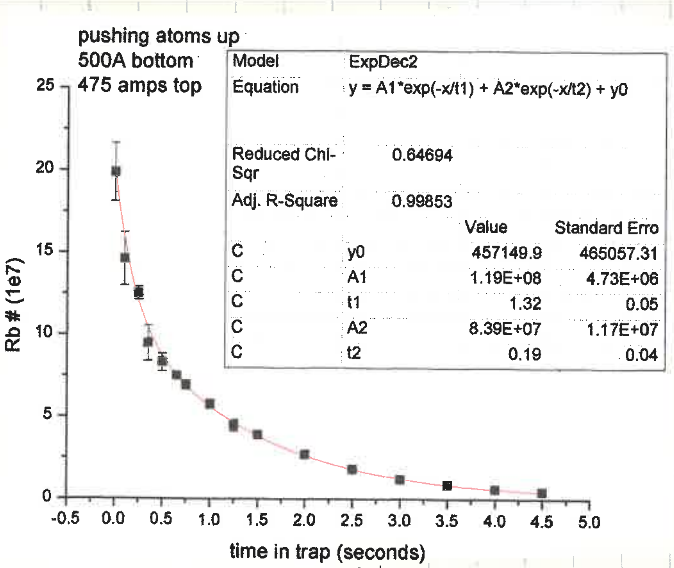


872_lifetime_52.387 cm_after..._03.txt

Now going to take vertical profiles. We have lots of Rb^+ signal.

872_pico_8kV_0.05s.txt : vertical profile at 50ms ; MCP = -4000V
872_pico_8kV_0.55s.txt MCP = ~~-4000V~~ -4300V
14ND3_pico 8kV_0.05s.txt : verify laser alignment
872_pico_8kV_1.05s.txt MCP = ~~-4500V~~ -4400V

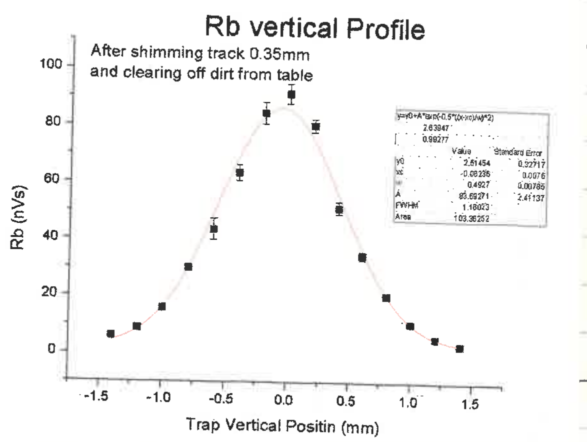
the B-trap is ~400 μm low. Corresponding to ~25 Amp difference in currents.

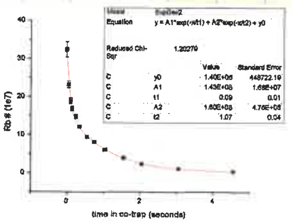


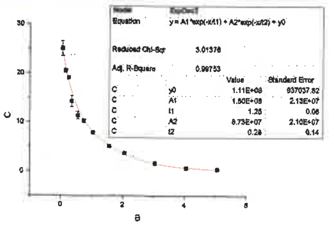
lifetime is shorter w/ misbalanced currents. Probably its better to shim the track.

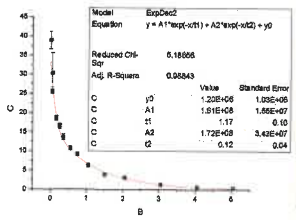
Shimmed the track up ~350 μm, dust under the track makes precision difficult.

New vertical profile at balanced trapping currents:

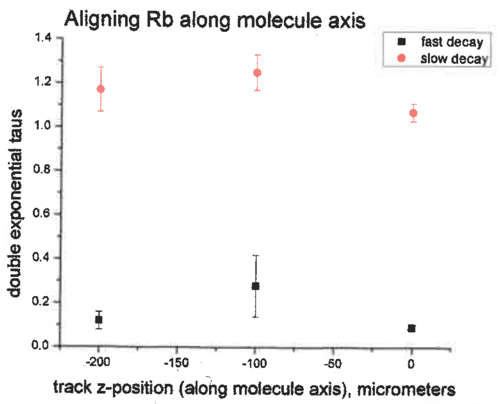






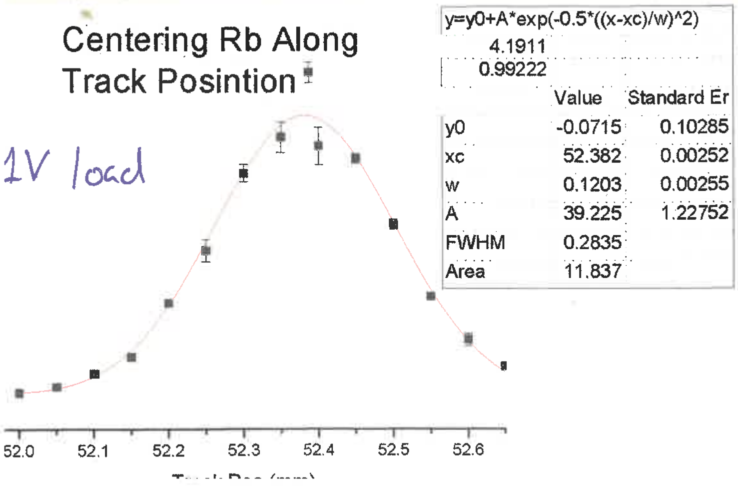


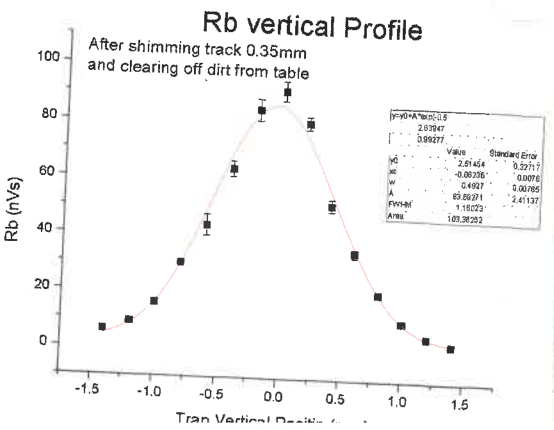
horizontal_lifetime_aligning.opj



Choosing final z-position here @ -100 μm relative to where we started today

1V load



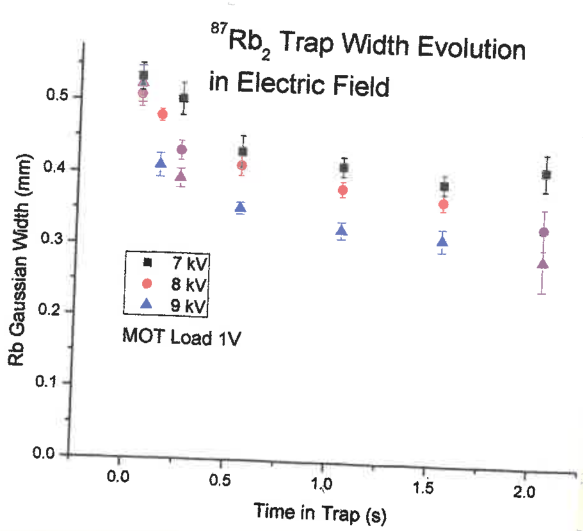


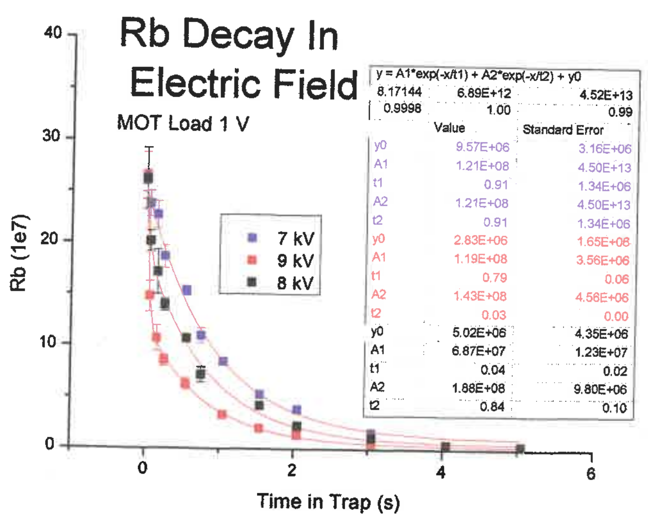
24 May 2010

Now finally I going to try to take Rb trap widths in electric fields again.

Laser 31511 cm⁻¹ P - 17 mJ
MOT 1V

run	872_pico_8kV_1050ms.txt	mcp :- 4100V	tit : 1.05 seconds
	872_pico_8kV_0050ms.txt	mcp :- 3800V	tit : 50 ms
	872_pico_8kV_0550ms.txt	mcp : - 4000V	
	872_pico_8kV_0250 ms.txt	mcp : - 3500V	
	872_pico_8kV_1550ms.txt	mcp : - 4600V	
	872_pico_8kV_2050ms.txt	mcp : - 4700V	noisy trace
	872_pico_7kV_0050ms.txt	mcp : - 3800V	
	872_pico_7kV_1050ms.txt	mcp : ~~- 4~~ -4100V	
	872_pico_7kV_0550ms.txt	mcp : - 4000 V	
	872_pico_7kV_1550ms.txt	mcp : -4400V	
	872_pico_7kV_250 ms.txt	mcp : -3500	
	872_pico_7kV_2050ms.txt	mcp : -4600V	
	872_pico_9kV_1550ms.txt	mcp : -4800V	
	872_pico_9kV_0050ms.txt	mcp : - 4000V	
	872_pico_9kV_0280ms.txt	mcp : -4100 V	
	872_pico_9kV_1050ms.txt	mcp : ~~-4300V~~ -4500V	
	872_pico_9kV_0150ms.txt	mcp : -4000V	
	872_pico_9kV_0550ms.txt	mcp : -4300V	
	872_pico_9kV_2050ms.txt	mcp : - 5000V	





25 May 2010

collisions!

Start time = 50ms

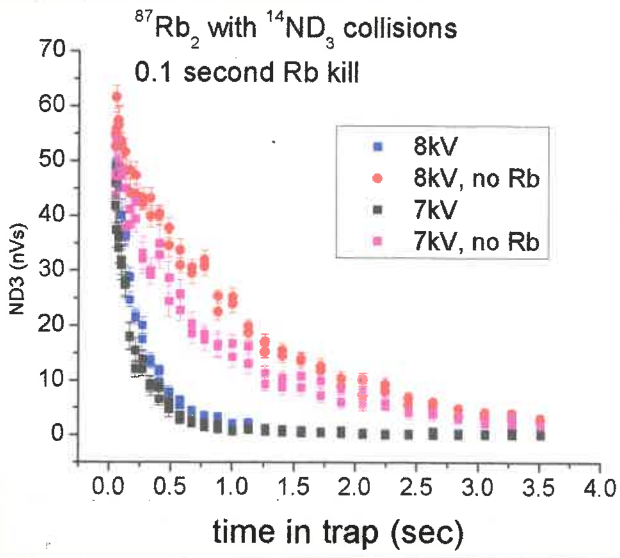
image calibration is taken at start time.

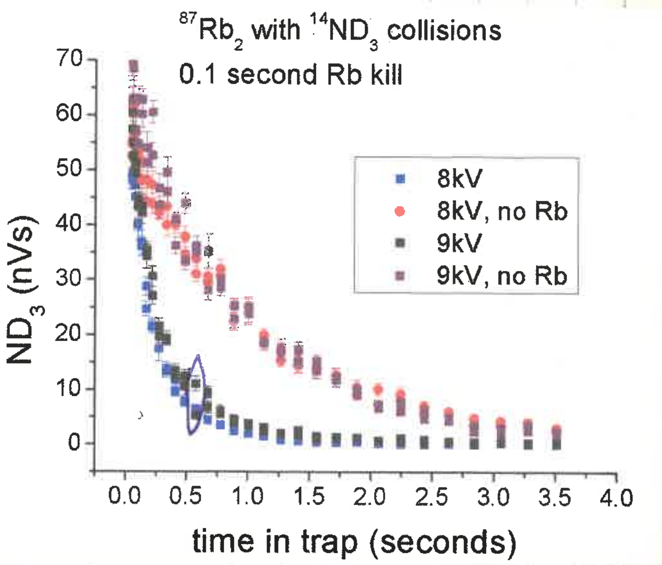
MOT Load = 1.0 V

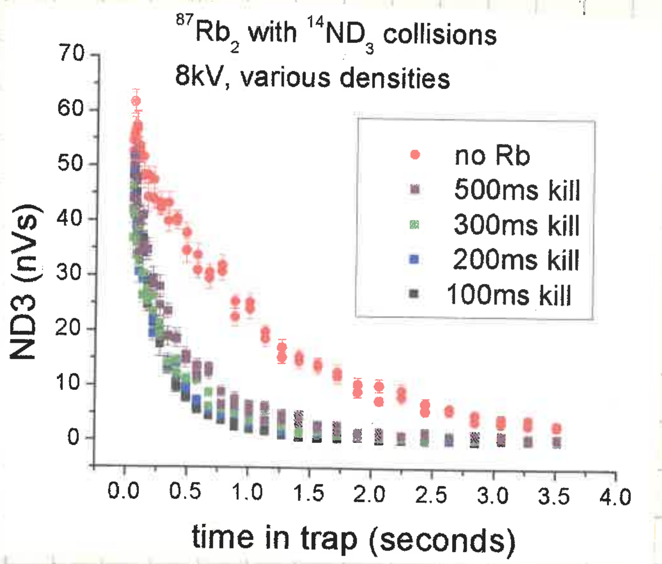
Laser 31511 cm^{-1} P = 17 mJ

wrong files used

Run	14N_872_8kV_kill Rb 0.1 Sec.txt	mcp = -4600V	~~N_0 = 1.55e8 ± .154e8~~
	14N_872_7kV_kill Rb 0.1 Sec.txt	mcp = -5000V	~~N_0 = 1.55e8 ± .156e8~~
	14N_872_9kV_kill Rb 0.1 Sec.txt	mcp = -4700V	~~N_0 = 1.56e8 ± .148e8~~
	14N_872_8kV_kill Rb 0.2 Sec.txt		~~N_0 = 1.46e8 ± .07e8~~
	14N_872_8kV_kill Rb 0.3 Sec.txt	N_0 = 1.23e8 ± .06	~~N_0 = 1.46e8 ± .07e8~~
	14N_872_8kV_kill Rb 0.5 Sec.txt		~~N_0 = .77e8 ± .046e8~~







Run	N0	Error
872_calibrate_7kV_0.1Sec	4.62E+08	1.66E+07
872_calibrate_8kV_0.1Sec	4.60E+08	1.66E+07
872_calibrate_8kV_0.2Sec	4.19E+08	7.43E+06
872_calibrate_8kV_0.3Sec	3.43E+08	6.24E+06
872_calibrate_8kV_0.5Sec	2.19E+08	4.84E+06
872_calibrate_9kV_0.1Sec	4.47E+08	1.56E+07

1 Jun 2010

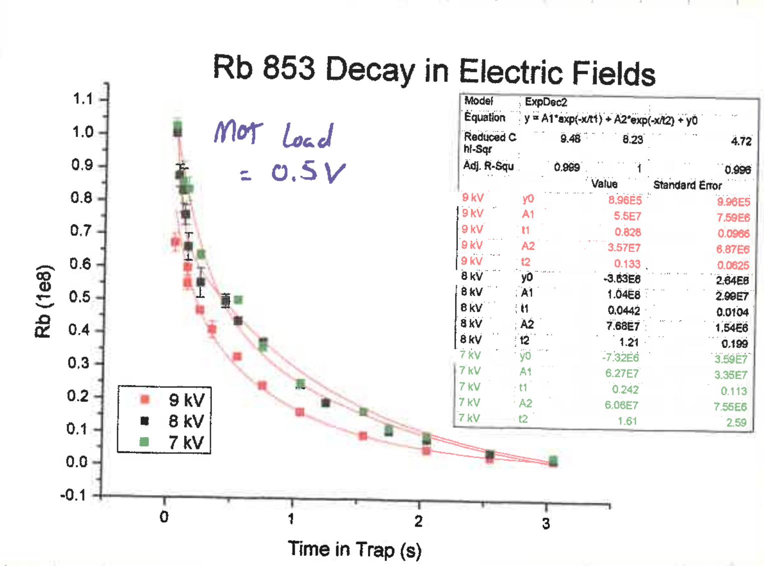
Switched to ^{85}Rb today. Mot looks very small.

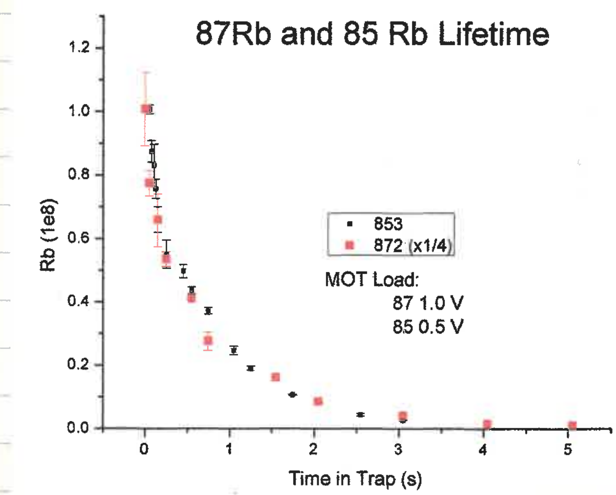
Switched back to ^{87}Rb and the mot is small, only loads to ~1.4V. I last week it loaded to ~2.6V.

turned getters up to 3.25V. Now ^{87}Rb Mot loads to ~ 2.6V

^{85}Rb Mot loads to ~~to~~ 1.07V

run 853_lifetime_8kV.txt
853_lifetime_7kV.txt
853_lifetime_9kV.txt noisey, crappy.
853_lifetime_9kV_2.txt





2 June 2010

pico motor scans of ^{85}Rb F=3 Laser 17mJ MOT load: 0.5V

853_pico_9kV_0050ms.txt mcp: -4200V
853_pico_9kV_1050ms.txt mcp: -5000V
853_pico_9kV_0550ms.txt mcp: -4800V

the Rb center is off ~200 μm. I checked this with the $^{14}ND_3$ and it looks like the laser was bumped. I recentered the laser, but the first 3 Rb traces look off-center.

recentered laser (up 240 μm)
horizontal center @ 6.5

853_pico_9kV_0250ms.txt mcp: -4300V
853_pico_9kV_1550ms.txt mcp: -5000V

853_pico_9kV_0135ms.txt   mcp = -4300V

all the same time were then taken for 7 and 8 kV

---

3 June 2010   collision runs today are saved in 25-May directory.

doing collisions of ~~N~~ ^{85}Rb.

MOT load 0.5V   Laser 18mJ

~~start time~~ ~~taken~~

30pt b/t 50ms and 3.5 sec

Rb calibration data is in folder andor\06Jun\01

sun	14N_853_8kV_killRb 0.1sec.txt	mcp = -4800V	N_0 = 8.3E7 ± .79E7
	14N_853_9kV_killRb 0.1sec.txt	mcp = ~~-5000V~~ -4800V	N_0 = 5E7 ± .57E7
	~~14N_853_7kV_killRb 0.1sec.txt~~	~~mcp = -5000V~~	

4 June 2010

14N_853_7kV_killRb 0.1sec.txt	mcp: -5000V	N_0 = 1.6E8 ± .16E8
15N_835_8kV_killRb 0.1sec.txt	mcp: -4800V	N_0 = 8.3E7 ± .79E7

7 June 2010

today we are going to look at ^{87}Rb F=1.

We are down by a factor of 200 in F=1 vs F=2 number at 50 ms.

We are not going to do F=1 - too little Rb

18 June 2010

We want to see if we can detect the symmetric ND_3 that results from the ND_3 (Asym) collision w/ Rb

Laser 31136.90 cm^{-1} P= 18mJ
MOT ^{87}Rb (F=2) load to 1V
we are on getter #3 of the mot. We are running out of Rb!! getters 1 and 2 already have no Rb

~~Steps~~

87

Run lookingForSymState02.txt

No symmetric state molecules detected