<u>Annotations:</u>

Provides context by referencing the measurements made in the previous day. This helps to motivate this day's work.

Compares new measurement to previous measurement and provides interpretation of comparison.

Provides subjective interpretation of plotted results.

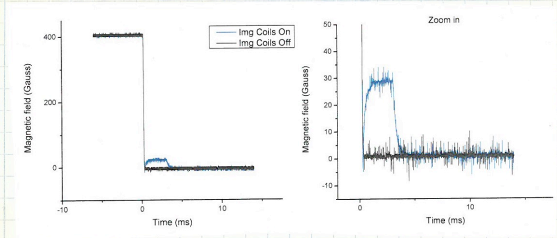
Plot is well labeled with axes, units, and legend.

Clearly states all objective information pertinent to the measurements.

Preliminary analysis of results, synthesized in well labeled plot and accompanied by evaluation of fitting.

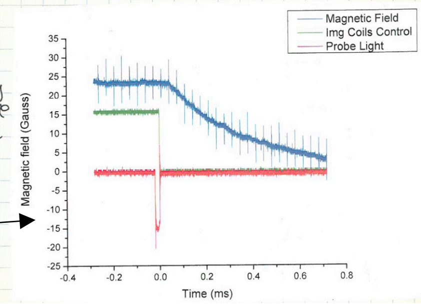
10/5/12

Yesterdays measurements were taken in the MOT load position. When the coils are in that position the λ/4 waveplate for the verticle MOT beams is right above the Top coil. This was causing an eddy current that made the magnetic field turn off much slower.
After repeating this measurement at the absorption imaging location the magnetic field turn off was much ~~more~~ quicker. so it looks like the imaging coils are not the problem. Also the measured field is pretty close to the calculated field (24 G)



Next looked at probe light timing and img coils timing to make sure they are happening at the same time.

Looks fine →



60s MOT load $N = 1.21 \times 10^9$ $\tau = 20.28$

decide to try a smaller magnetic field for imaging coils and try to tune probe to that.

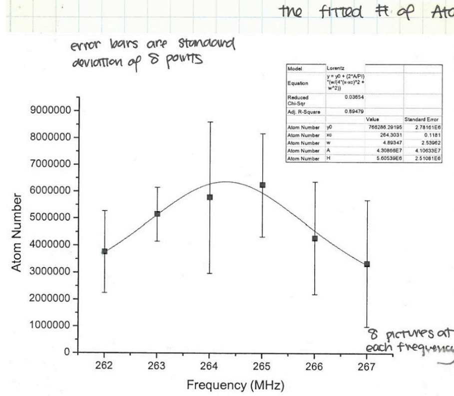
control voltage: 4V $B = 12$ Gauss $\Delta\nu = 17.8$ MHz

set AOM1 → 267.8 MHz and AOM2 → 250 MHz

Finally saw atoms in absorption imaging with imaging coils on!

Started tracing out a beam profile with $\sim\Delta\nu = 17.8$ MHz detuning however after 3 different frequencies the probe beam stopped locking above 269 MHz. At this point I also noticed that the lenses and mirror for focusing the image on the Andor camera were tilted and the beam was not centered on the lenses. Fixed this and decided to move on to imaging with a 24 Gauss field (8V control on img coils).

Coil control voltage: 8V $B_{calculated} = 24$ Gauss $\Delta\nu_{calculated} = 33.6$ MHz
AOM2 = 233.0 MHz (held constant)
AOM1 was varied
MOT load to {0.5V} though this could have been higher since the fitted # of Atoms never reached 10^7



Initially it looked like the image would saturate so I made the MOT load smaller but this turned out to be too small because the wings of the Lorentzian were not accessible because there its mostly noise and the program cannot fit to it.
This should probably be repeated with either a larger MOT load or once other parameters have been optimized

In this second example of a research notebook, the entry comes after a number of days of attempting to troubleshoot and understand a particular piece of equipment. The researcher has tried a number of different approaches to characterize the behavior of the piece of equipment over this time period. This entry was written at the conclusion of this process.

8/25/2009

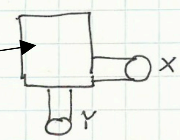
Still looking at Uzi valve stability : Valve = 2.0×10^{-5} torr (10 Hz)
940 μs detect time.

run histogram_5Hz.txt
histogram_10Hz.txt

The 0.1 Hz histogram from yesterday showed a slow decrease in signal over the run, could have been deuteration issues.

- did many histograms with various parameter values to try and improve stability, no effect.

- optimized valve XY position, Saturation! Turned MCP down to 2900V.
Vast improvement in signal size, no effect on stability
(x~2.5)



new valve position → [black #-5]
~~X: 13.80mm~~
Y: 15.65mm

run 10Hz_after_valve_optimization_mcp3000-5utorr.txt

Specifies files where results of measurements and analysis can be located.

Describes interpretation of results.

CONCLUSIONS ABOUT UZI VALVE FROM LITTLE BOY

(1) The PZT valve is more stable shot to shot, by a factor of ~ 2 to 3
[5% stdev vs. 10–15% stdev]
(2) The uzi valve loses less signal as repetition rate is decreased.
[only 25% drop from 10 Hz → 0.1 Hz for uzi, 50% drop for PZT]
(3) Free flight signals are comparable, with the uzi valve possibly being slightly better. The uzi valve also appears to have a more narrow velocity spread.

The researcher then synthesizes the results of the previous several days of characterization. This concise description makes the full picture clearer than if one had to read back over all previous entries.

With these conclusions in mind, we put the uzi valve on Kelvinator to try bunching/slowing/trapping. A significant difference made itself apparent, in that the uzi free flight signal was significantly smaller than the PZT free flight signal. This persisted for both bunching and slowing as well. The main chamber differences are

(a) Kelvinator has a much larger front chamber (~ a factor of 10), and much higher pumping speed there (~ another factor of 10).
(b) The flight distance from valve to detection is about twice as far in Kelvinator.

The researcher then goes on to describe the future direction for the experiment.

June 7, 2011

This concise description shows consideration for a broader audience.

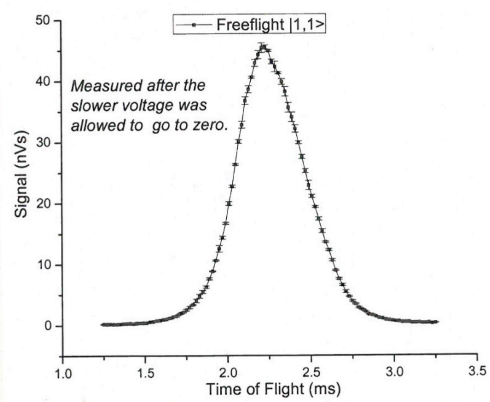
Started by checking signal on the first peak of slowing for A=1, detecting |1,1⟩. This peak was down to about 32 nVs from 38 nVs yesterday. Switched to slow and detect |1,1⟩ with the new timings and were down to ≈ 32 nVs from ≈ ~~44~~ 42 two days ago. Remixed gas and got 10% back, up to ≈ 38 nVs. Tried picomotor scan, found max at exact same location as yesterday (+2.65 mm).

Thorough description of process performed during the day and makes clear the comparison with previous day's results.

∗ Switched to freeflight of |1,1⟩ and found signal was enormous (>200 nVs). This was being caused by residual charge on the slower acting as a DC guide. If the slower is not allowed to discharge fully, it can hold charge long enough to give erroneous signal values for an entire freeflight scan. This may be the cause of the discrepancy between slowed and freeflight signal levels where freeflight has been substantially larger. With as little as 100V on the slower, the signal was ≈ 20% higher. After allowing the slower to completely discharge, freeflight was about 50 nVs, compared to ≈ 47 nVs for slowing, which is much more reasonable.

Speculation about potential cause of observed discrepancy.

Thoroughly labeled plot with all potentially relevant information.

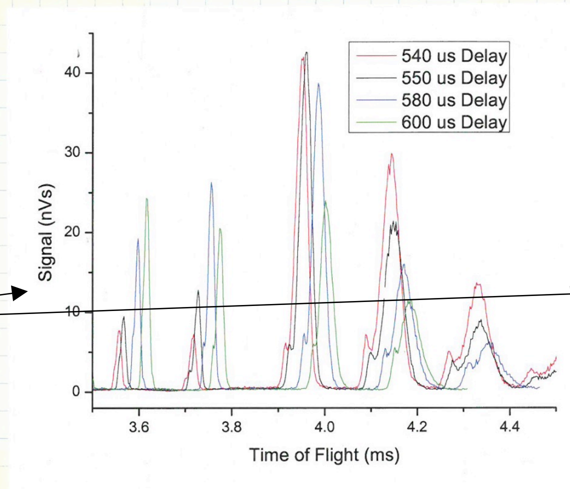


Freeflight |1,1>

Gas	ND_3
MCP (V)	-4000
Valve (V)	-461
Valve Temp (°C)	184
Pressure (10^{-5} Torr)	1.1
TOFMS (V)	1000
Phase Angle (deg.)	
Sirah λ (cm^{-1})	31511.5
Wavemeter λ (cm^{-1})	15755.51
Laser Power (mJ)	11.9 (Sirah)
Final Velocity (m/s)	
Detect Time (ms)	2.23
Step Size (μ s)	20
Scope Averages	56
Labview Averages	3
Date	6/7/2011

To test if the initial delay was a possible source of disagreement between experiment and simulation, the delay was varied and TOF spectra were measured.

Explicitly states the intended test to determine the cause of the discrepancy.



Varying the delay did not improve agreement!

Well labeled plot with interpretation.

TESTS

The small powermeter was used to check losses at mirrors, with little change found from Sirah output to the last mirror.

The MCP power supply was changed to check stability, with no change in signal.

After re-optimizing the Sirah, freeflight of |1,1⟩ was down to ≈ 36 from ≈ 50 earlier today. And slow/detect |1,1⟩ was down to ≈ 30 nVs from 38 earlier today.

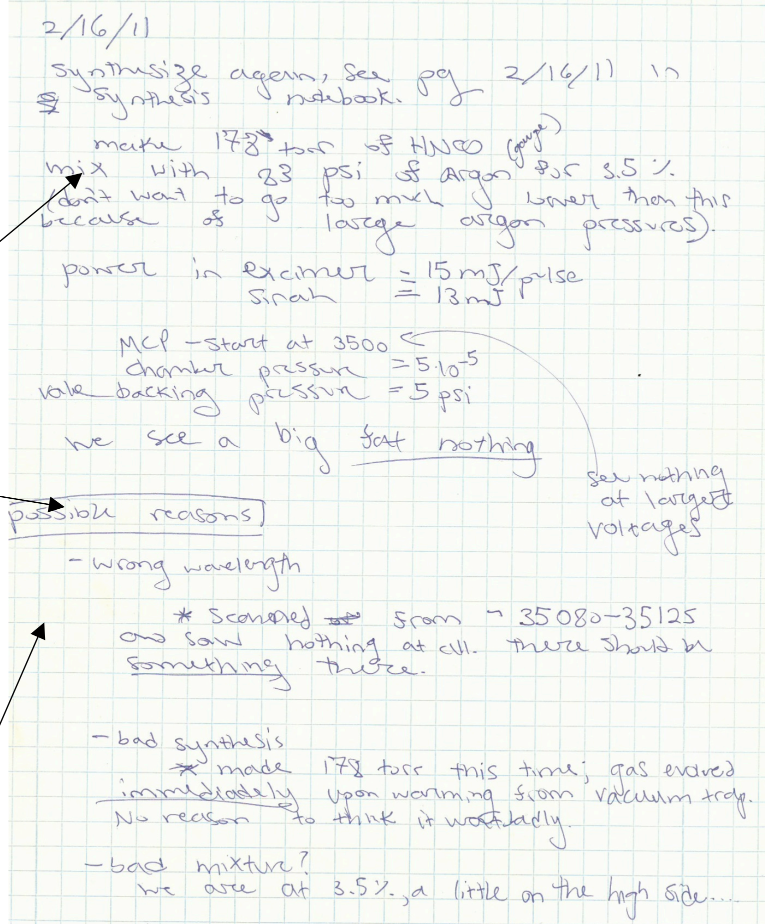
The concise synthesis of all tests performed allows for other researchers to quickly understand results.

Researcher connects the current day's work with previous results so that reader may easily reference and understand the background to the current work ("synthesize again")

Researcher makes it clear that they obtained a null result.

Makes thorough list of hypotheses about the cause of the null result. This list may serve to motivate subsequent days' measurements.

After testing all of their hypothesis the researcher then provides motivation for the direction of the subsequent day's work.



2/16/11

Synthesize again, see pg 2/16/11 in synthesis notebook.

make 178 torr of $HNCO$ (gauge)
mix with 83 psi of Argon for 3.5 %
(don't want to go too much lower than this because of large argon pressures).

power in excimer ≅ 15 mJ/pulse
Sirah ≅ 13 mJ

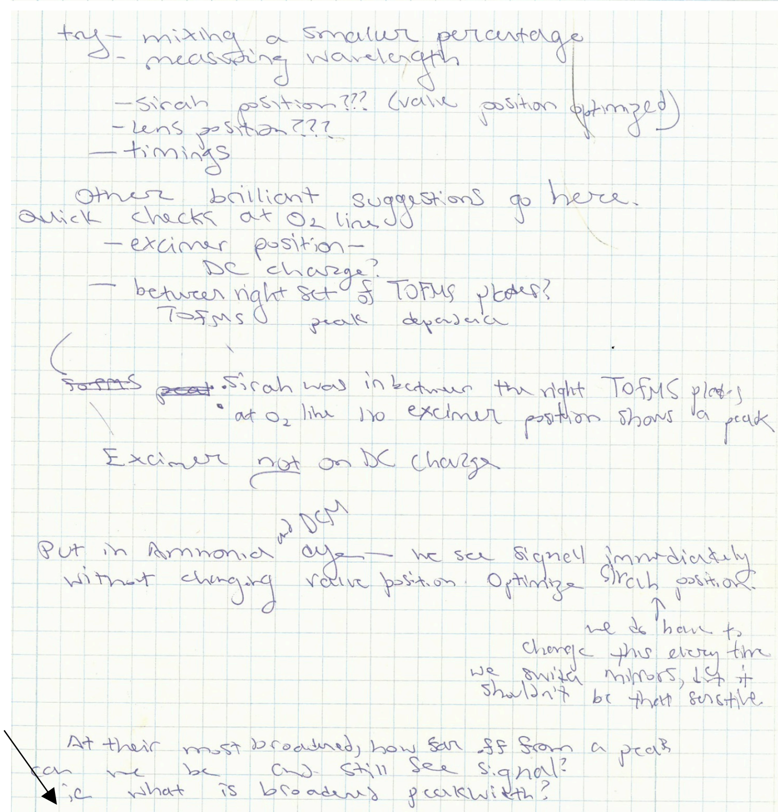
MCP - start at 3500
chamber pressure = $5 \cdot 10^{-5}$
valve backing pressure = 5 psi

we see a big fat nothing

See nothing at largest voltages

possible reasons

- wrong wavelength
  * Scanned from ~35080-35125 and saw nothing at all. there should be something there.
- bad synthesis
  * made 178 torr this time; gas evolved immediately upon warming from vacuum trap. No reason to think it worked badly.
- bad mixture?
  we are at 3.5 %, a little on the high side...



try - mixing a smaller percentage
- measuring wavelength
  - Sirah position??? (valve position optimized)
  - lens position???
  - timings

Other brilliant suggestions go here.
Quick checks at O_2 lines
- excimer position -
  DC charge?
- between right set of TOFMS plates?
  TOFMS peak dependence

Sirah was in between the right TOFMS plates
at O_2 line no excimer position shows a peak

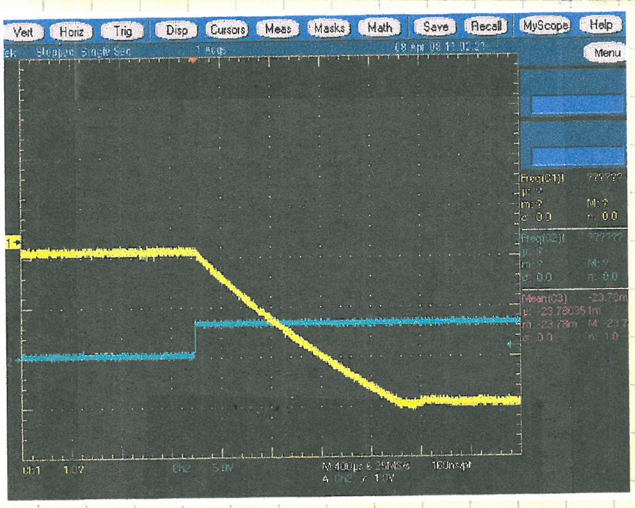
Excimer not on DC charge

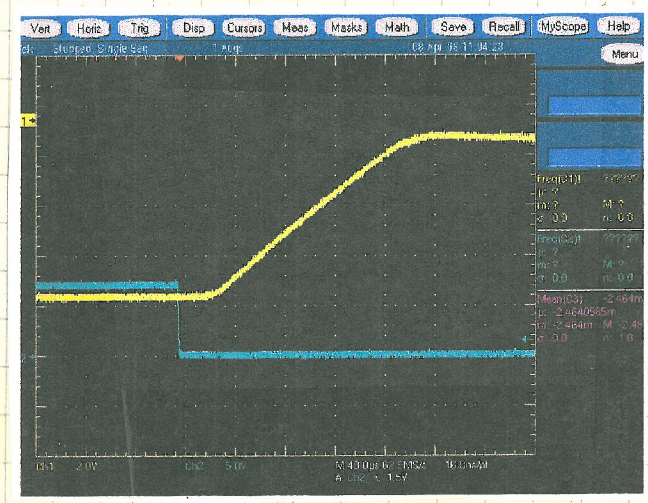
Put in Ammonia and DCM dye - we see signal immediately without changing valve position. Optimize Sirah position.
we do have to change this every time we switch mirrors, i.e. it shouldn't be that sensitive.

At their most broadened, how far off from a peak can we be and still see signal?
i.e. what is broadening peak width?

8 April 2008

We are trying to resolve why our cloud is so "cold" (80 μK). It happens that the magnetic trap is not turning off as fast as we think (pg 10). This is ~~beca~~ because there are 10 $k\Omega$ resistors on the gate of the mosfets that are slowing down the signal. We replaced the 10 $k\Omega$ resistors with 100 Ω resistors.





on time ~~400 μs~~ 1.6 ms

off time ~~40 μs~~ ~160 μs

Temperature of Trap:

$$k_B T = \frac{4}{5} \mu_0 g h B'_x x_{HWHM}$$

$\mu_0 = 1.4 \frac{MHz}{gauss}$

$g = 1/2$

$B'_x = 105.5$ gauss/cm

$x_{HWHM} = ?$

$= 1.65$ mm

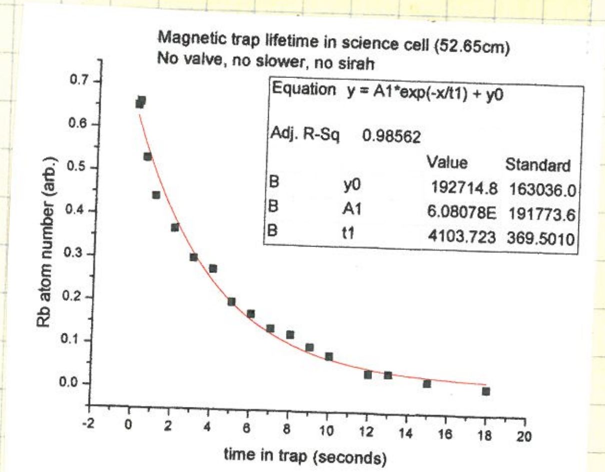
$x_{width} =$ ~~31.5~~ 34 pixels

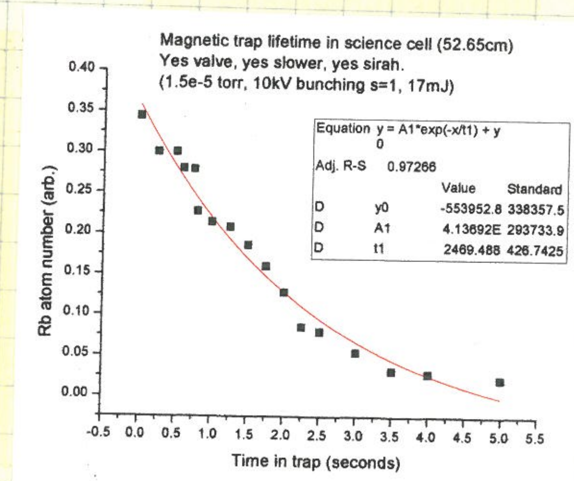
$x_{FWHM} = 2 \cdot 2.35 \cdot 34 \cdot 21 = 3.3$ mm

$T = 466\ \mu K$

24 May 2008

Chamber baked overnight at 105°, only MCP and (isolation valve closed) TOFMS sections. Pressure in the back is now ~ 1×10^{-9} torr. Magnetic trap lifetime w/ no valve etc is now about <u>4 seconds</u>, up from ~ 1.5 seconds with pressure in the mid -9's from last week. Lifetime measurements follow, unsure as to the cause of lifetime depletion with full aparatus running, we'll investigate that after DAMOP. It could be the valve, sirah, or slower.

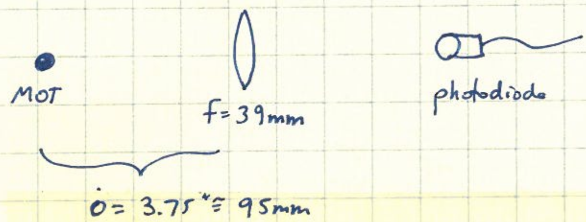




The "everything on" data fits a quadratic ~ t^2 much better than an exponential. Also of note: in the two plots above the MOT loading parameters were changed from a load point of 0.8 to 0.4 since the MOT would not load that high with the sirah on. So despite earlier conclusions,

<u>the sirah does effect the MOT!</u>

imaging of the mot with a photo diode



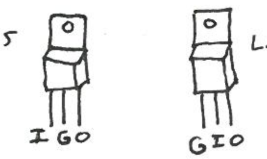
$\frac{1}{f} = \frac{1}{i} + \frac{1}{o}$

$\Rightarrow i \cong 66\,mm$

note: for MOT load program to work, it needs a ~~negative signal from diode~~ ~~⇒ positive signal~~ ~~so from~~ ~~amplifier~~, negative offset of ~ 2 volts. (amplifier output)

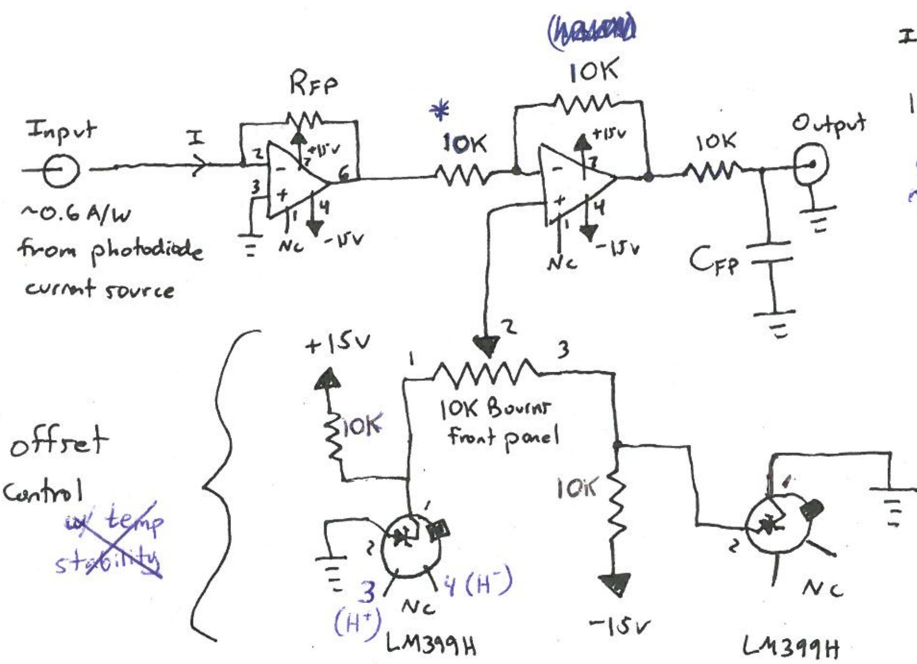
Photodiode Amplifier for MOT Imaging

power connections: ± 18 V supply



10 μF bypass capacitors

LF356 op-amps



output $\tau = RC \in (0, 34\,ms)$

offset control ~~w/ temp stability~~

R_{FP} controls gain (2nd Amp is unity gain)

C_{FP} controls roll on/off speed. (low pass filter on output)

} both variable via multi-position switch, full CCW = position 1, full CW = position 10

roll on/off time: $\tau \sim RC$, R = 10K

switch position	R_{FP}	C_{FP}
1	1K	NC - 0
2	3.3K	1 nF
3	10K	4 nF
4	33K	10 nF
5	100 K	37 nF
6	330 K	100 nF
7	1 M	300 nF
8	3.3 M	1.6 μF

essentially, gain = R_{FP}

(I → V converter also sometimes called a transimpedance amp)

* actually a 1KΩ ← see page 97