Items culled from elog on the 6/28/02 suspension wire breakage incident/
AJW, 8/28/02

Fri Jun 28 2002 (Local)

Fri Jun 28 19:08:10 2002 **UTC**

EARTHQUAKE:

China north east region
Mag 7.2
time 17:19 UTC

- ***barker*** (http:// <-- contains reference url for this entry.)

Sat Jun 29 03:27:47 2002 **UTC**

MMT2 PROBLEMS
****

After the earthquake this morning (plot 1) the SM2 damping was unstable. This problem was fixed by increasing the side damping from 1 to 30. Only about ~6 seconds later at 18:57:25 UTC (coincidence?) MMT2 shifted (plot 2). The POS sensor went from 1.21 to 1.01, PIT went from 0.47 to 1.48, YAW went from -0.3 to -0.07 and SIDE went from -1.0 to -2.13 (plot 3 and 4).

Further investigation showed that all sensors but UL are essentially stuck. LL may show some small motions. The upper sensors only show digitization noise. UL could be excited, however, its natural frequency has shifted to 7.37Hz (plot 5). The ring-down time is about 35 sec which would indicate a Q of 250 (plot 6).

Swapping the electronics (controller and satellite amplifier) yielded no improvements. Our best guess at this moment is a mechanical interference, such as an electric space charge which has pinned the mass to one of the earthquake stops. This effect was once been seen on the 4k. However,

there it happened after the masses were driven for hours.

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daniel
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- contains reference url for this entry.)

View External Object

Sat Jun 29 03:29:31 2002 **UTC**

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daniel
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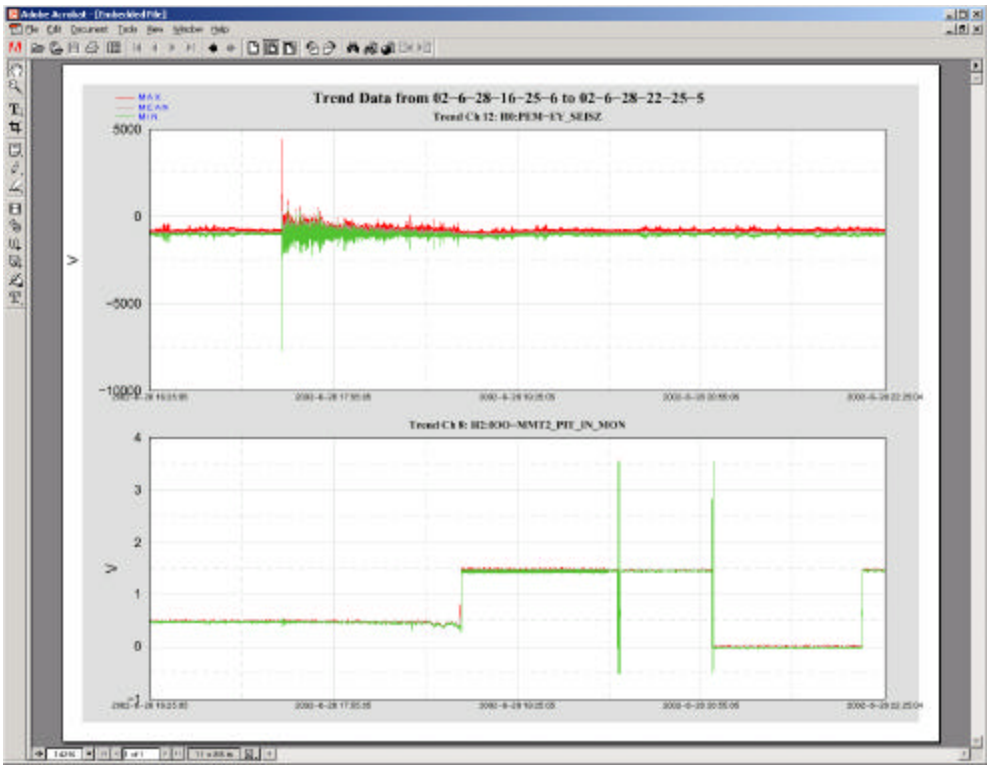
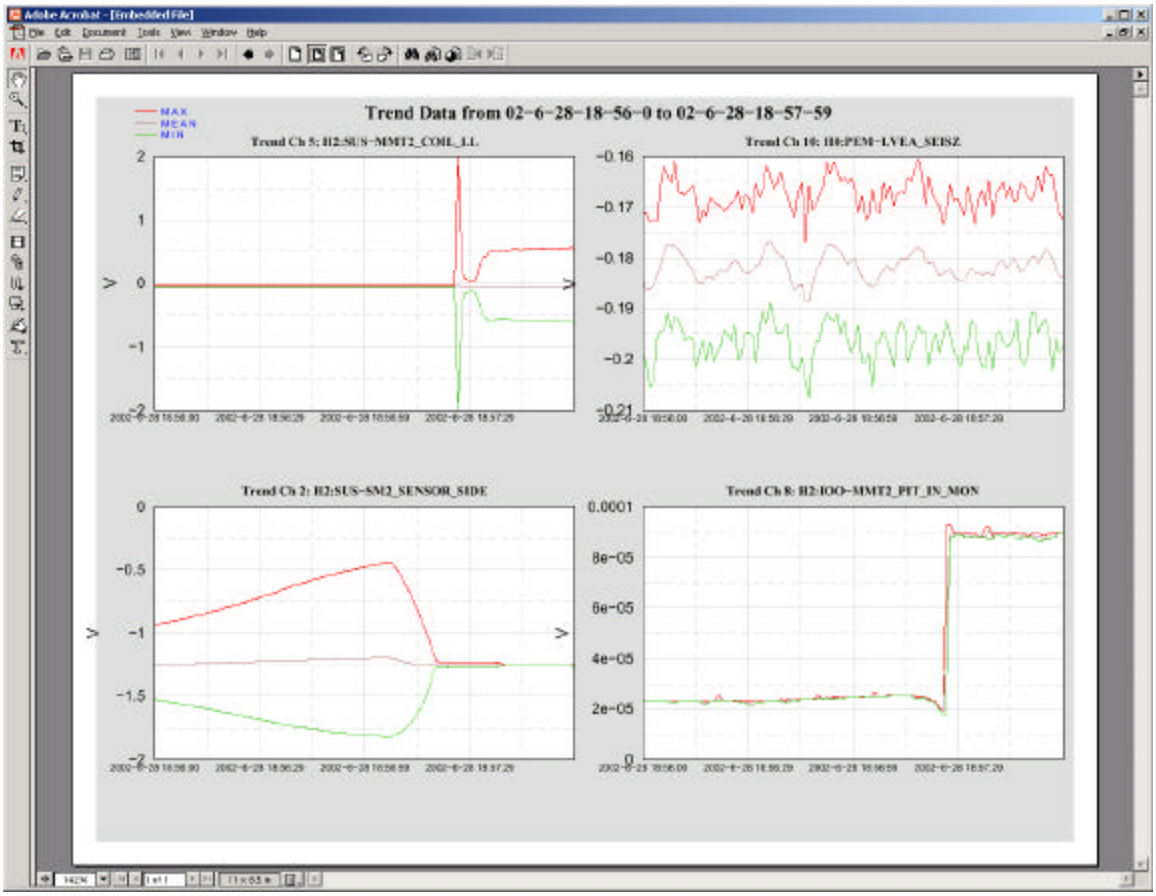
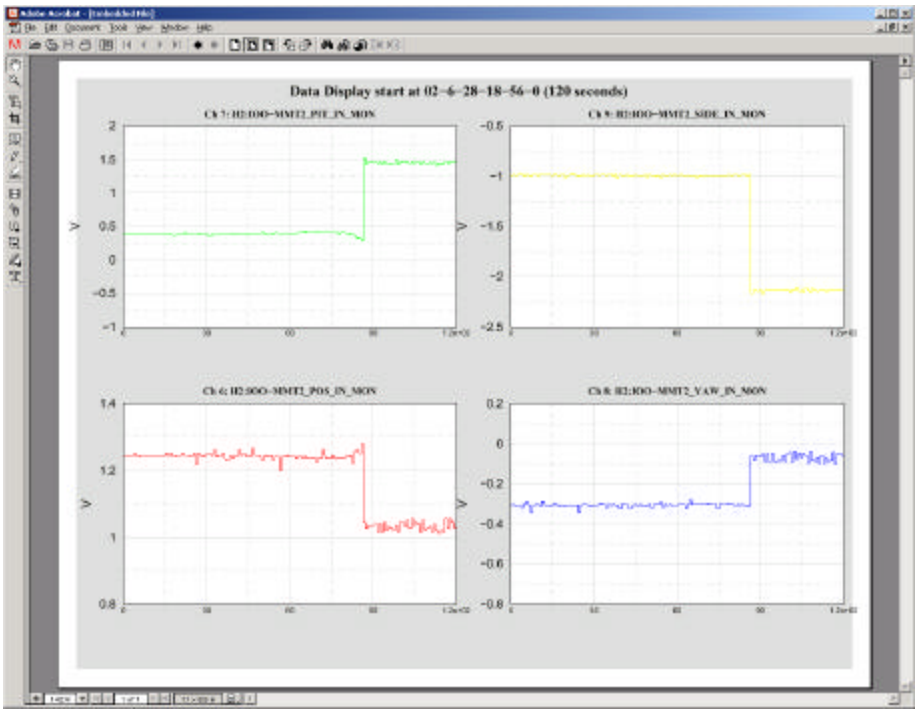
Plot 1: The earthquake was all but over when MMT2 shifted.



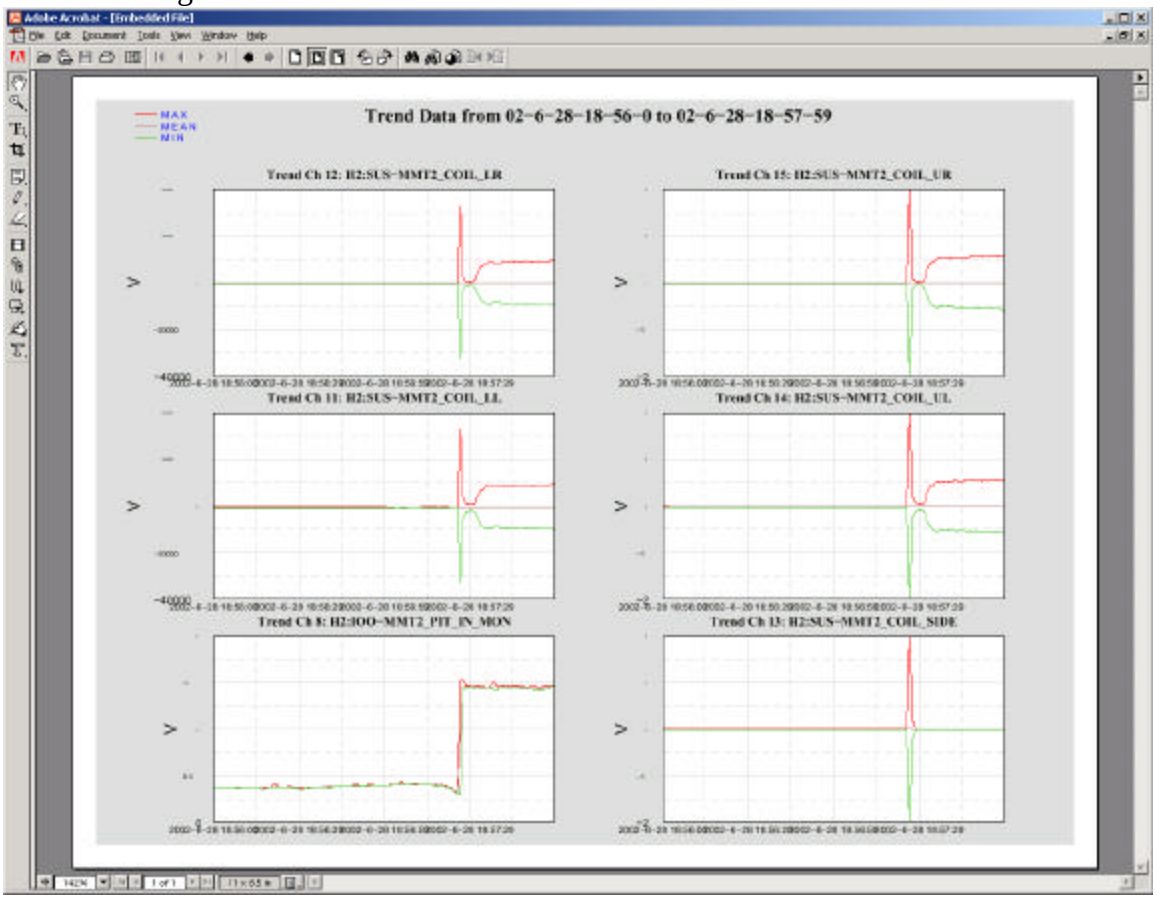
Plot 2: MMT2 shifted after SM2 calmed down.



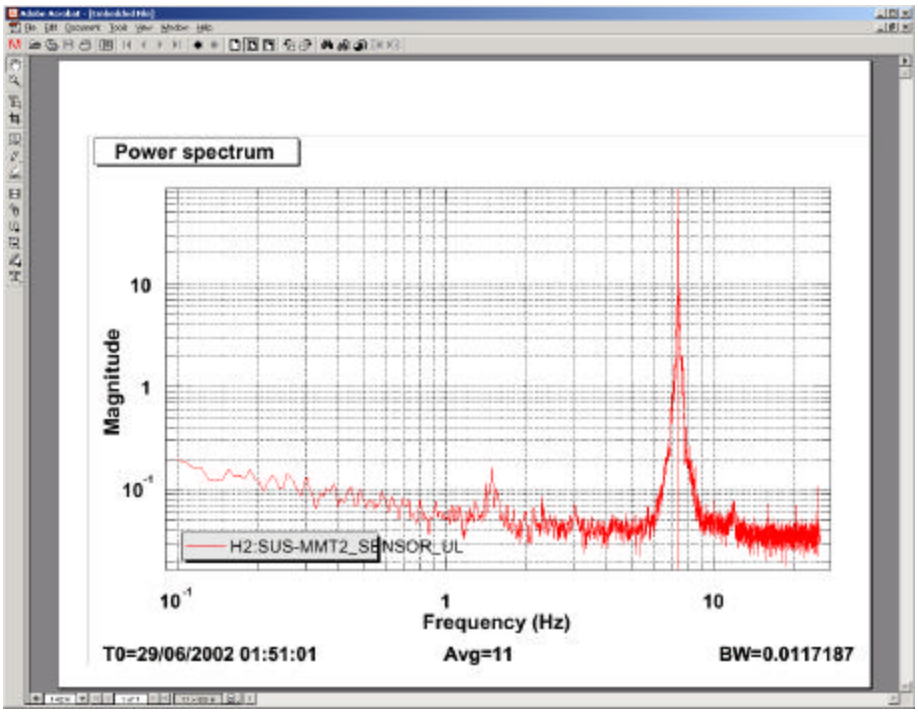
Plot 3: Shift of sensor POS, PIT, YAW and SIDE.



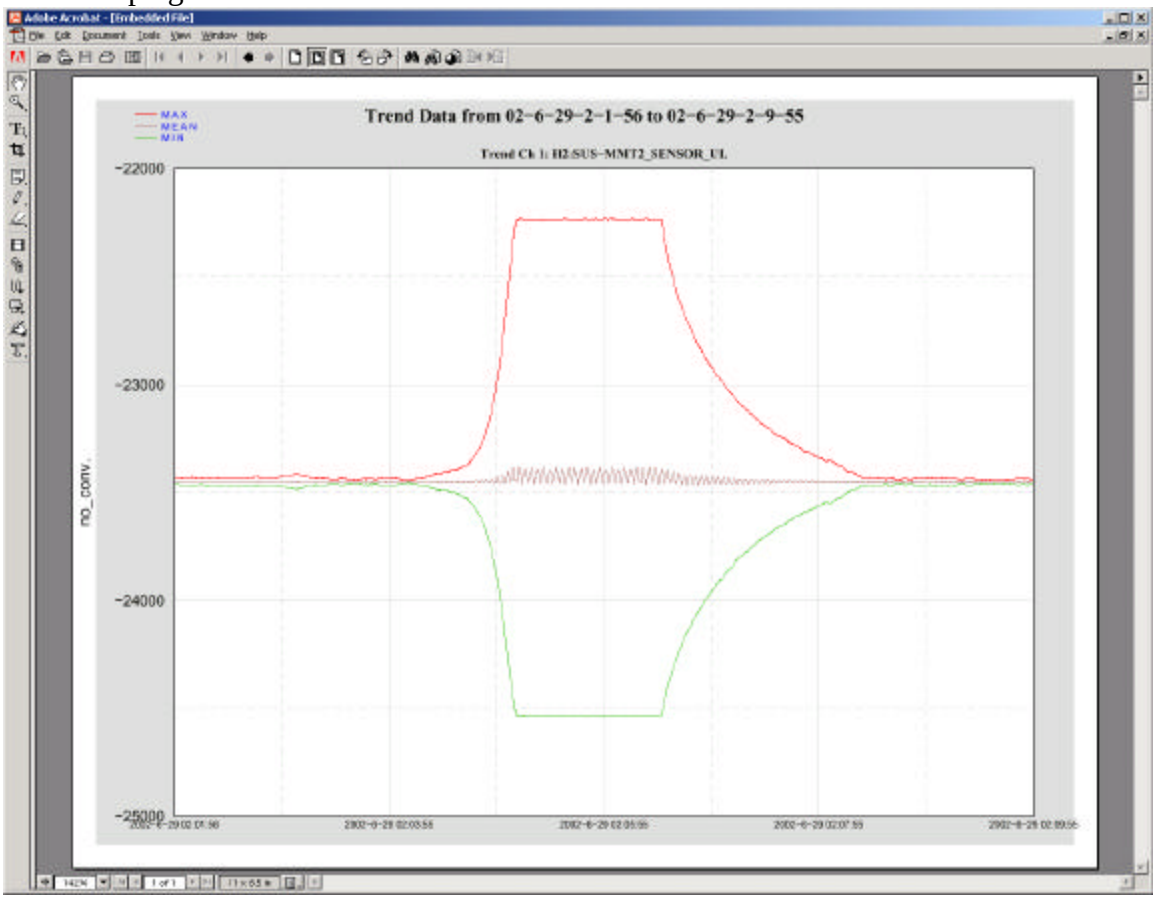
Plot 4: Coil readouts of MMT2. The local damping is unstable in the new configuration!



Plot 5: The new 7.4Hz eigenfrequency of MMT2.



Plot 6: Ring-down of the 7.4Hz mode after it was excited with the local damping turned on.



Sat Jun 29 06:57:03 2002 **UTC**

A visual inspection showed that the 2K MMT2 wire is broken.

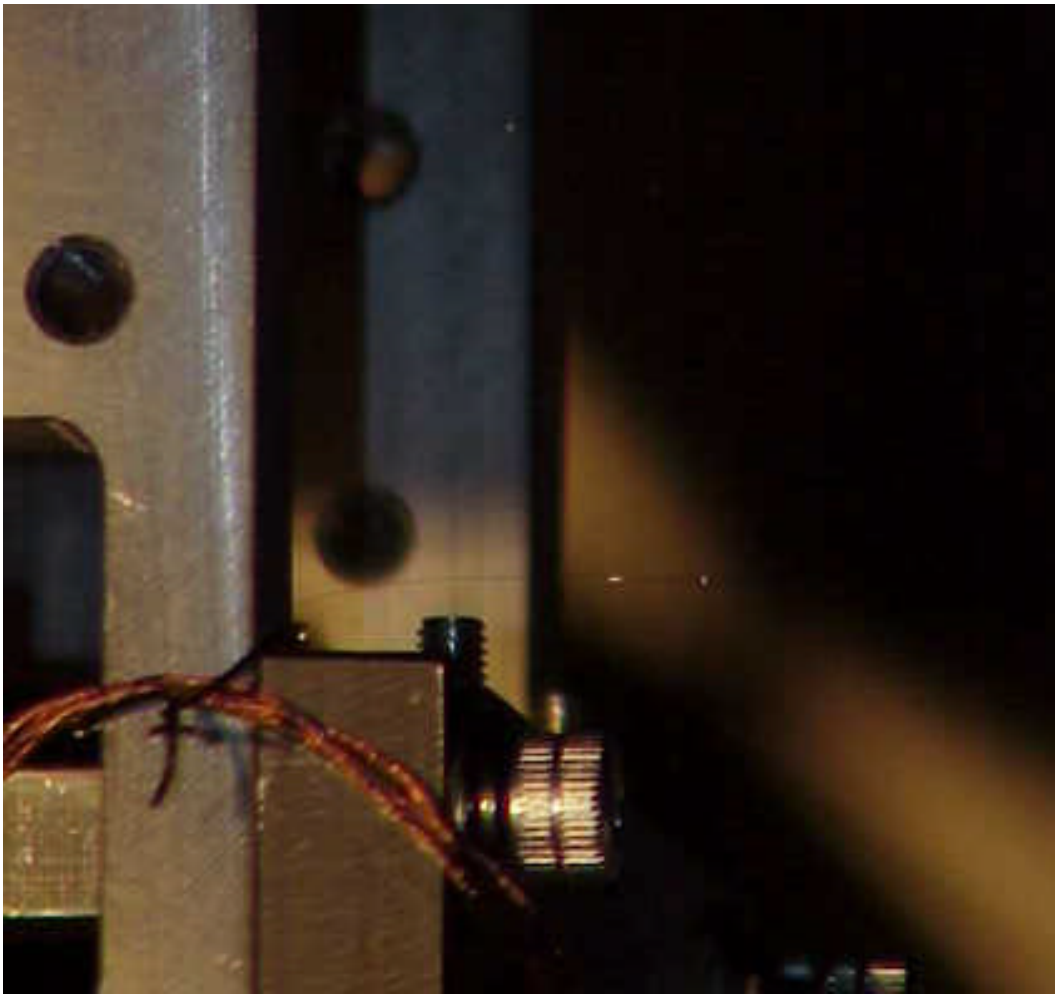
- daniel (
http:// <--
contains reference url for this entry.)

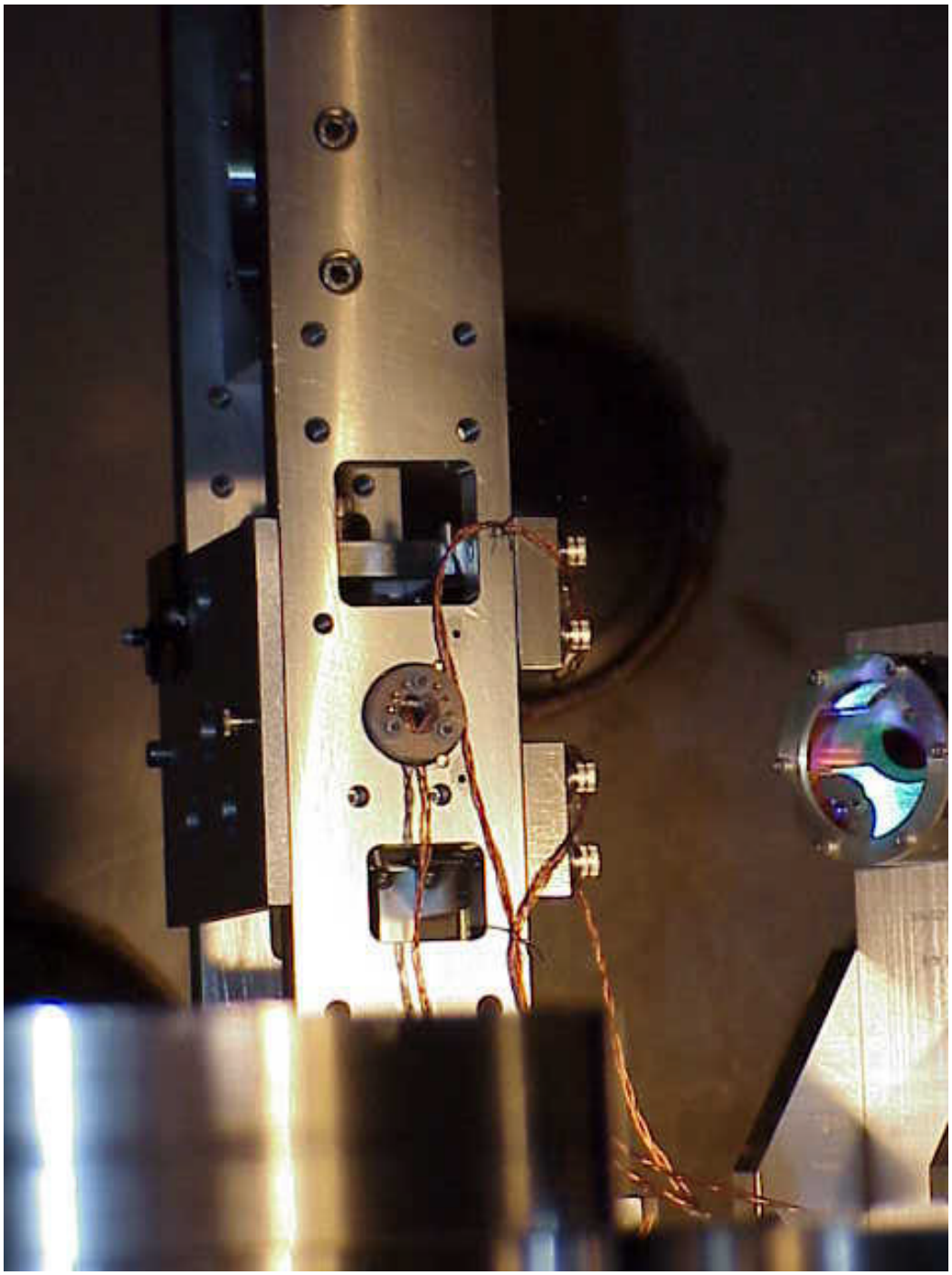
Sat Jun 29 08:44:38 2002 **UTC**

Suspension Wire Broken on 2k MMT2

Betsy and I went out to look into HAM-7 as we were venting it. We convinced ourselves that we could see a slack wire, using modified binoculars, but it would have been tough to convince others. However, when John turned up the purge air we saw the wire blow out the back of the support structure where it was quite evident and, with Virginio's help, we photographed it.

Robert, Betsy, Virginio and a host of witnesses





Sat Jun 29 12:37:21 2002 **UTC**

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I think all of the 2k large optics have a clean bill of health since yesterday's earthquake sent them swinging. I looked at high resolution spectra of their sensors and see that the pendulum, pitch, and yaw peaks are in the appropriate places on all large optics (Fred has the plots).
As well, aside from a little bit of long term (over 2 days) drift on the ITMs and FMs (maybe due to operator tweeking), most of the optics still reside around the zero of their optical levers. They also damp and respond well to bias adjustments.
I say "almost" because although the MMT3 also damps and responds well, it took a step in both pitch and yaw some 12 hours after the earthquake and then has held that position. The conlog shows no operator input for adjusting biases on this optic at that time... I'm highly suspect of an inappropriate RM adjustment (via operator input) in pitch that might have steered the beam to heat the MMT3 somewhere as we've seen before. In anycase, the MMT3 has not drifted back to it's original "locking" spot, so I drove it there with bias. Attached is a plot showing this step and the correlation to the RM motion.
Original biases were:
Pitch = 4.859, Yaw = 0.576
Now:
Pitch = 3.909, Yaw = -1.434

Wed Jul 3 04:53:35 2002 **UTC**

Photographs of the Ends of the Broken 2k MMT2 Suspension Wires

The short piece extended 17cm below the edge of the clamp to the
break point. The ends appear to have been drawn and the tips partly
shaped by surface tension. Thin film colors were most obvious near
the tip of the long piece, although they appeared elsewhere on the wire.
The end of the long piece had little blobs of what looked like metal
loosely attached (see photo). These brushed off when the loose wire
end brushed the wipe on which it was sitting. The two pieces have been
packed to send to Dennis.

Robert, Doug, Erika, Ron

[redacted] - ***robert*** (http:// <- - contains reference

[redacted] url for this entry.)

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Fri Jul 19 19:12:24 2002 **UTC**

SCANNING ELECTRON MICROSCOPE PICTURES OF BROKEN WIRE FROM SM2

Attached is an unfortunately large (7MB) pdf file of SEM pictures of the broken wire at both ends as well as reference wires that were broken by cutting and by tension at room temperature in air. We are indebted to Libby Shaw MIT Surface Laboratory Megan Goldman MIT Undergraduate Research Student in Physics Marie Woods MIT/LIGO Group administrative assistant for these pictures. To me it is clear that the SM2 wire was actually heated.

[redacted] - ***RaiW*** (http:// <-- contains reference url for this entry.)

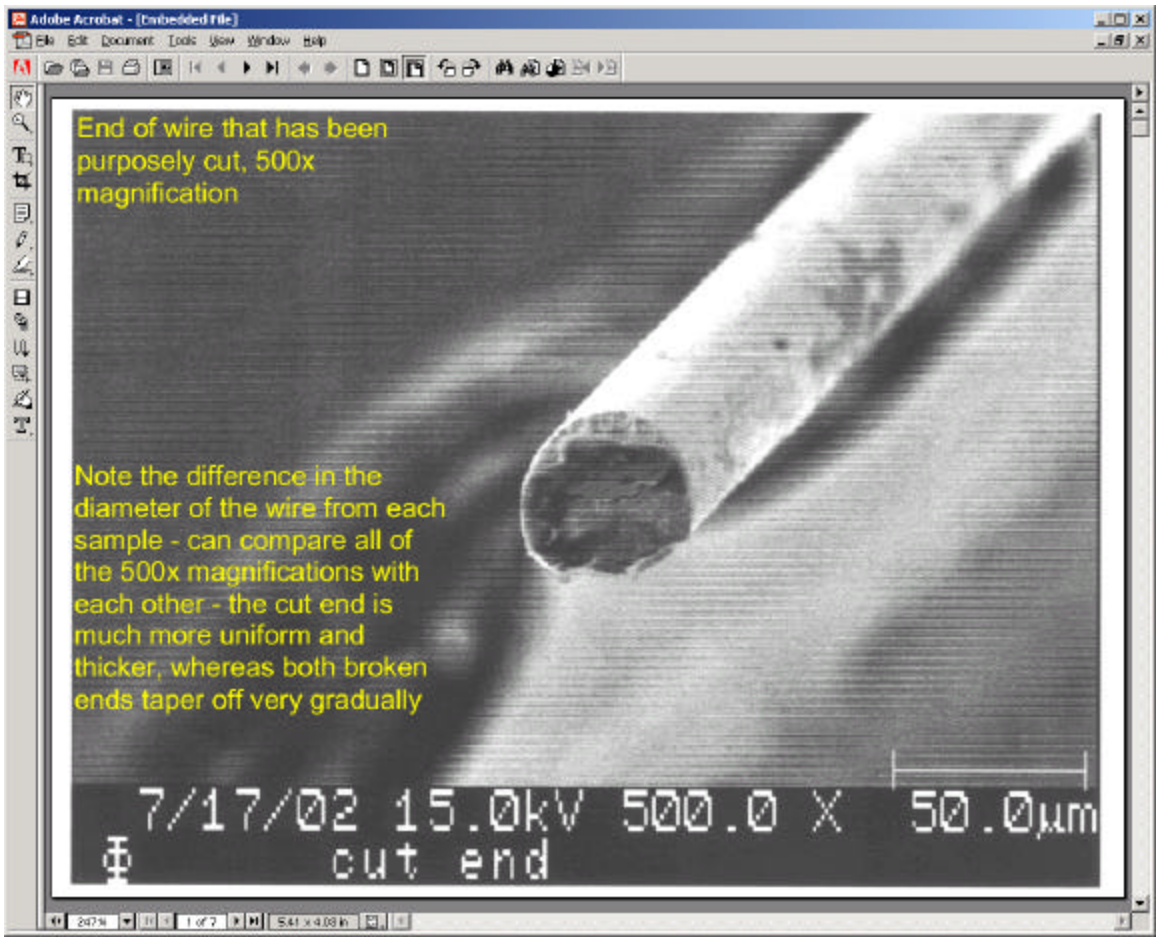
16:13:34

Thu Jul 25 2002 (Local)

Thu Jul 25 23:13:34 2002 **UTC**

Note typo; the broken wire is from MMT2.

[redacted] - ***landry_m*** (http:// <-- contains reference url for this entry.)



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Fri Aug 2 00:07:59 2002 **UTC**

EARLIER CALCULATION ON RADIATIVE BALANCE OF WIRE IN LASER BEAM

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RaiW
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The wire break at MMT2

It is clear both from the pictures taken and the simple calculation given below that the wire broke because the laser beam hit it and melted it. I suggest two things to test this. First, to simply pass a wire of the same radius through a 5 watt beam of spot diameter 2mm. In air the thermal conduction by the gas will eventually dominate but the wire probably will glow. In vacuum, as used in the calculation below, the only loss mechanism is radiation and the wire will get hotter and melt. Second, is to look at the residual pressure record from the discharge gauges in the vicinity of the wire and see if there is a desorbed gas burst near the time of the wire break.

The calculation is the same one that Stan and I have done for dust on the mirror which also showed an alarming increase in dust temperature and caused us to ask that the mirrors be cleaned in situ.

The calculation:

Use the symbols and values

a = wire radius = 3 x 10^-3 cm
w = laser spot radius at the wire = 0.1 cm = 1 mm
P = power in the laser beam at the wire = 5 watts
sigma = Stefan-Boltzmann radiation constant = 5 x 10^-12 watts/cm^2/K^4
e = emissivity of emission and absorption by the wire = 0.1
c = specific heat of the steel wire = 0.2 Joules/gm/K
rho = density of the steel = 8 grams/cm^3

The radiative balance temperature of the wire is

$$T^4 = \frac{4*P}{sigma*(pi*w)^2}$$

using the values given in the table

T = 2.5 x 10^3K above the melting point of the steel.

The time it takes to reach this temperature is calculated using only the heat capacity of the steel, the thermal conductivity down the steel wire does not enter in first approximation. The rate of change of wire temperature with time is then:

$$\frac{dT}{dt} = \frac{4*e*P}{a*c*rho*(w*pi)^2} = 4.2 \times 10^3 \text{ K/sec}$$

A 1/3 second exposure to the beam is probably enough to do it.

This problem of heating the wires by the laser beam is most acute in the mode cleaner and the middle of the telescope where the w is small. It is not an issue at the large test masses.

The forensics should concentrate on how it is possible for the beam to get to the wire. Is there a combination of misalignments that could do it?

Tue Jul 9 16:02:08 2002 **UTC**

Venting to replace MMT2 on 2K:

Began ~7:45 AM PDT, and door taken off at ~9:00 AM.

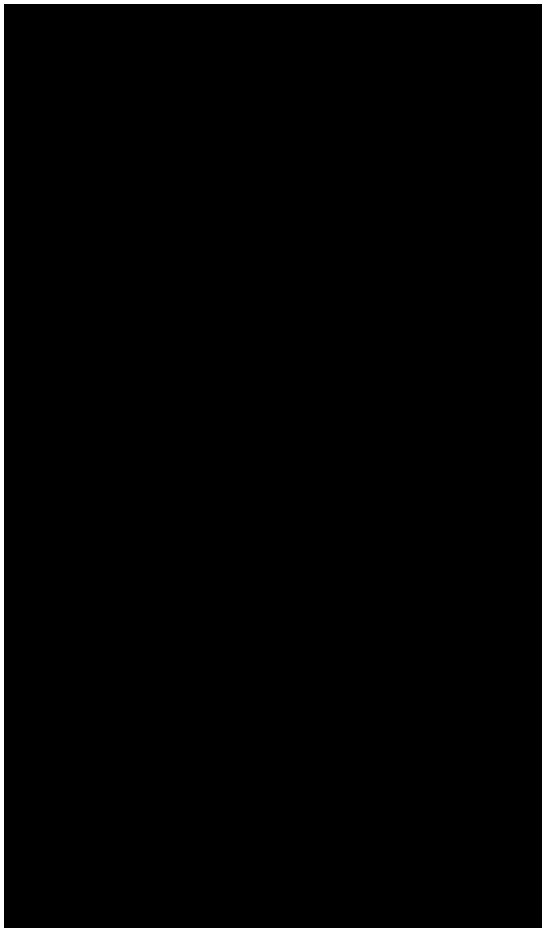
- *Vorvick* (http:// <-- contains reference url for this entry.)

11:56:55

Tue Jul 9 2002 (Local)

Tue Jul 9 18:56:55 2002 **UTC**

Work inside HAM8 completed at 11:45am PST. Preparing to re-seal chamber.



- *fjr* (http:// <-- contains reference url for this entry.)

13:49:43

Tue Jul 9 2002 (Local)

Tue Jul 9 20:49:43 2002 **UTC**

Pumping commences at 12:35 PST (19:35 UTC).

- *fjr* (http:// <-- contains reference url for this entry.)

15:36:13

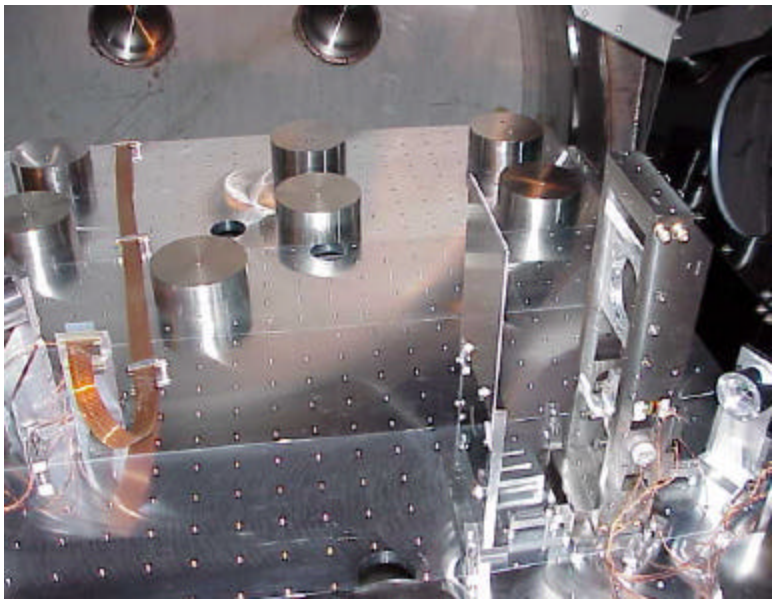
Tue Jul 9 2002 (Local)

Tue Jul 9 22:36:13 2002 **UTC**

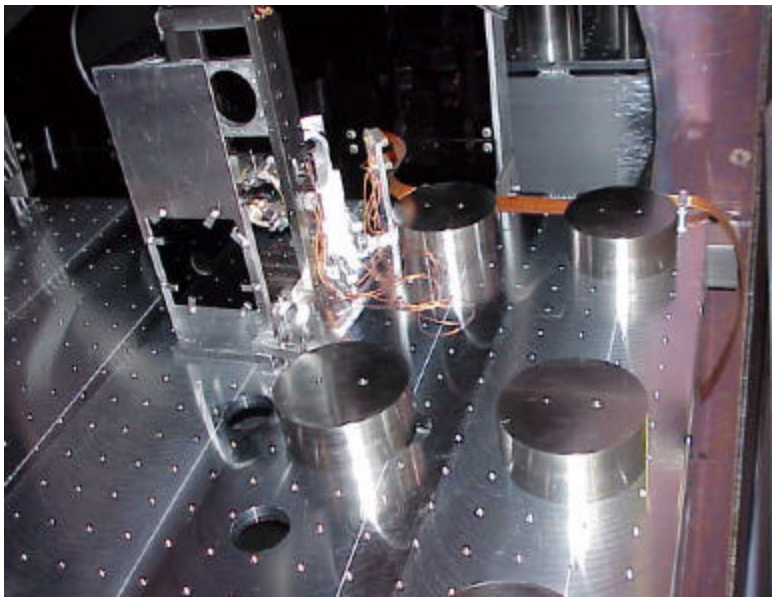
Pressure check at 3 hours into pumpdown is 1.4e-5 Torr.

- *fjr* (http:// <-- contains reference url for this entry.)

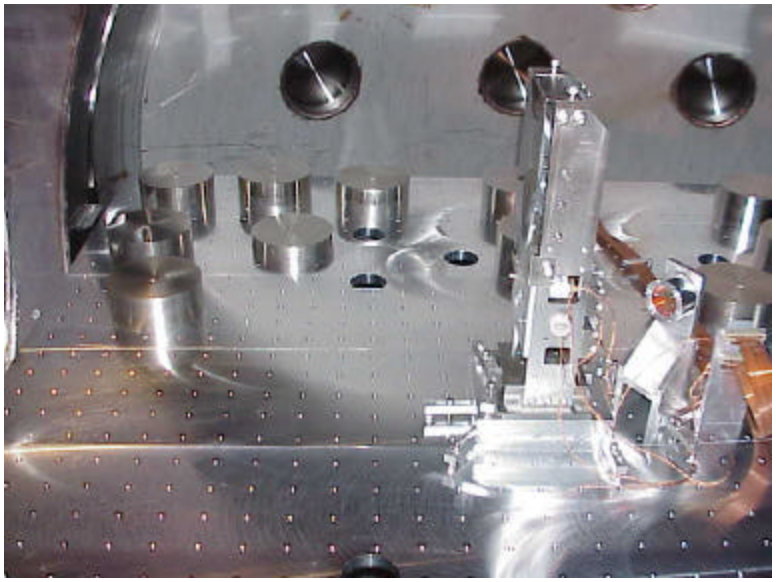
MMT2 and MC2 pics.



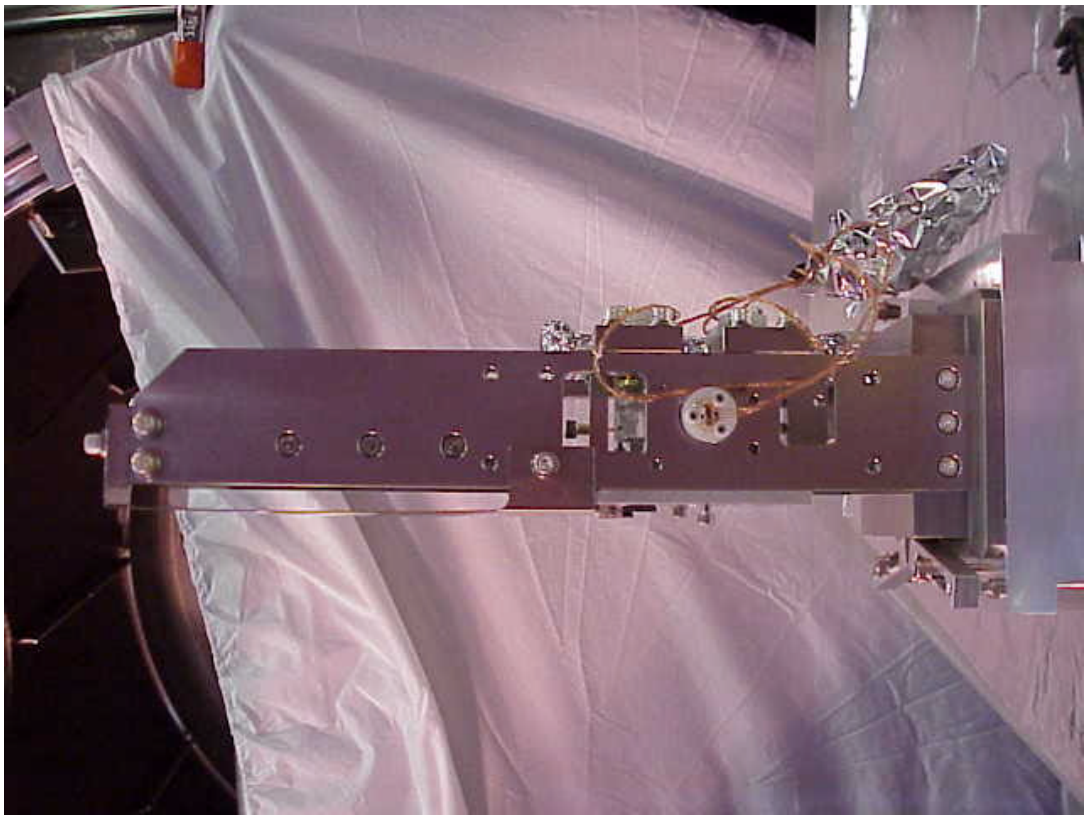
MC2 and baffle extension

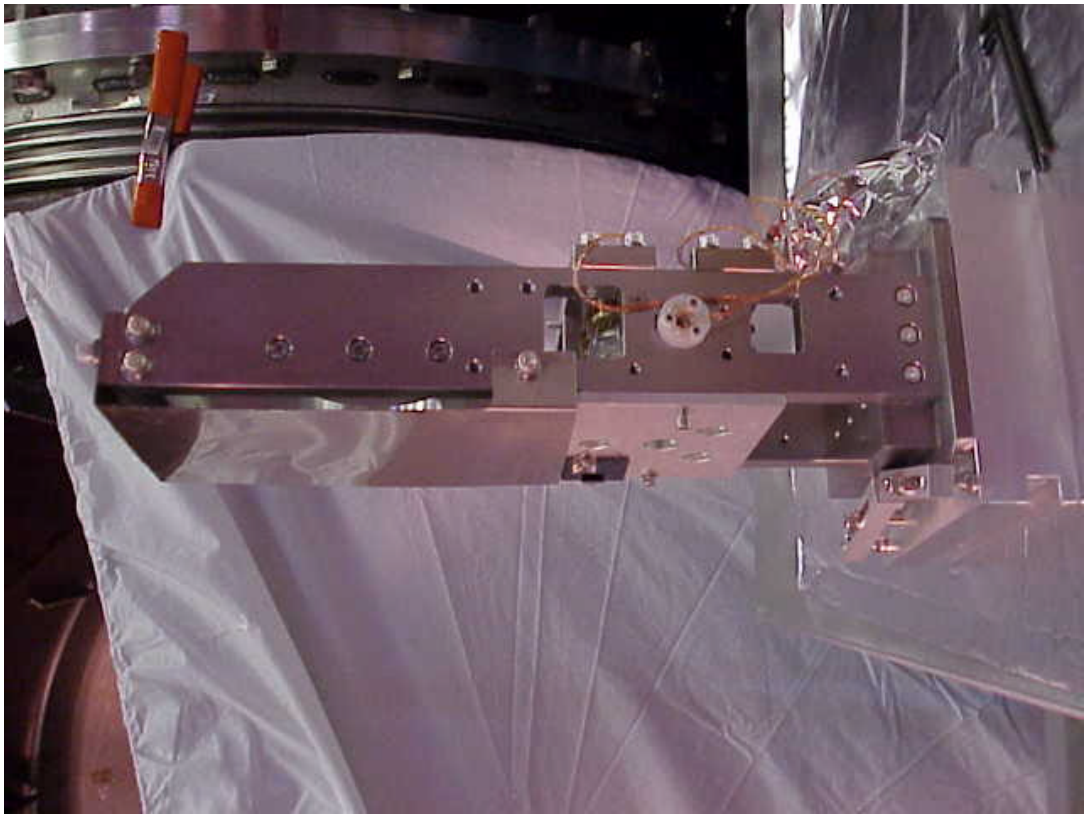


MC2 and baffle



MMT2 and baffles





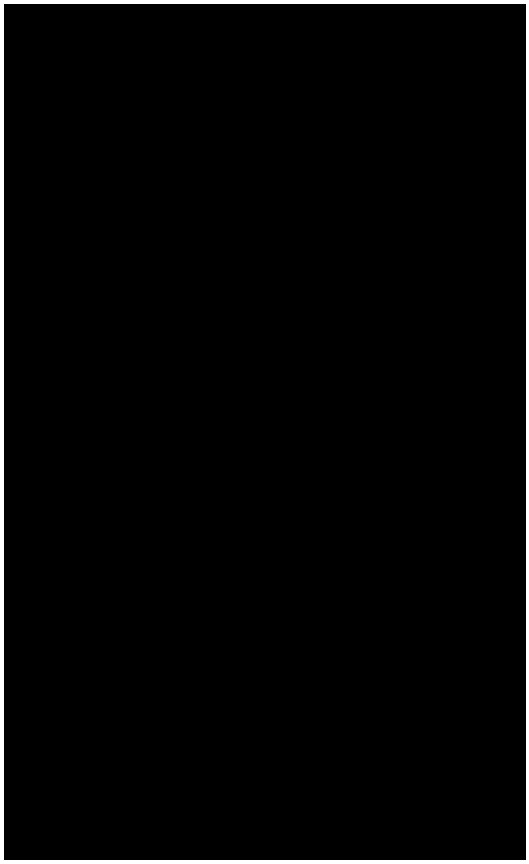
MMT2 with baffles

Tue Jul 23 01:57:28 2002 **UTC**

WATER LOAD INTO BEAM TUBE

At 5:00PM calculate 1.0 torr liters/day into the beam tube on each arm.
The fraction of water is f = 0.277
The total pressure is 1.3 x 10^-8 torr at the entrance to the trap.

The pressure is low enough to keep the beam tube open to the interferometer since the expectation is that the gas load will drop as 1/t.



- ***RaiW*** (http:// <-- contains reference url for this entry.)

15:03:08

Tue Jul 23 2002 (Local)

Tue Jul 23 22:03:08 2002 **UTC**

Rai has used the wrong gauge pressure here.
The correct gauge shows a pressure of about 1/2 this
so that the water load into the tube is more like 0.5 torr -liter/day.
Updated RGA data is in Tuesday July 23 OPS log.

john

- ***john*** (http:// <-- contains reference url for this entry.)

Sun Jun 30 04:42:55 2002 **UTC**

EVENTS LEADING UP TO THE FAILURE OF THE 2K MMT2 SUSPENSION
and some wild speculations

Two significant events are evident in the channels monitoring MMT2 prior to failure:
the Russian-Chinese border quake (1.5 hours before failure) and a slow drift in pitch, yaw, and position beginning 15 minutes before the final jump (see plot 1). The optic appears to recover from the quake within a couple of minutes and neither
the slow drift nor the final jump appear to be correlated with quake signals or with other seismic signals in the LVEA (plot 2). I dont think that the quake or other
seismic signals directly triggered this event. MC2 shows no correlation with the final
events in the life of the MMT2 suspension.

I havent found anything that correlates precisely with the beginning of the slow drift. The slow drift, with increasing and decreasing pitch, yaw and pos, is quite unusual in the
history of the optic and leads right up to the final jump. The conlog showed no MMT2 adjustments
during this period and I speculate that the drift is a result of temperature changes, due
to a stray main beam or, less likely, that the drift is due to the suspension wire slipping
very slowly in the clamp. We have seen thermally induced alignment changes in this ballpark
in the past.

The only other optic with unusual signals during this period was SM2. SM2 began to ring up
within 2 seconds of the quake signal arrival (plot 3 and zoom in - not shown), possibly because the quake swung it into a positive
feedback regime. This swinging continued until Josh and others halted the swinging just 5 seconds
before the final jump in MMT2 (plot4). Again, Mike showed that the conlog recorded these
activities but nothing that should directly affect MMT2.

My speculation is that temperature cycling of the wires or clamp region triggered slippage of
a loosely held suspension wire from the clamp, or, triggered an imminent fracture in the wire.
The temperature cycling would be associated with the resonant frequency of SM2, or, more likely, with the longer term variations in the pointing of SM2.

Robert

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robert
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http://
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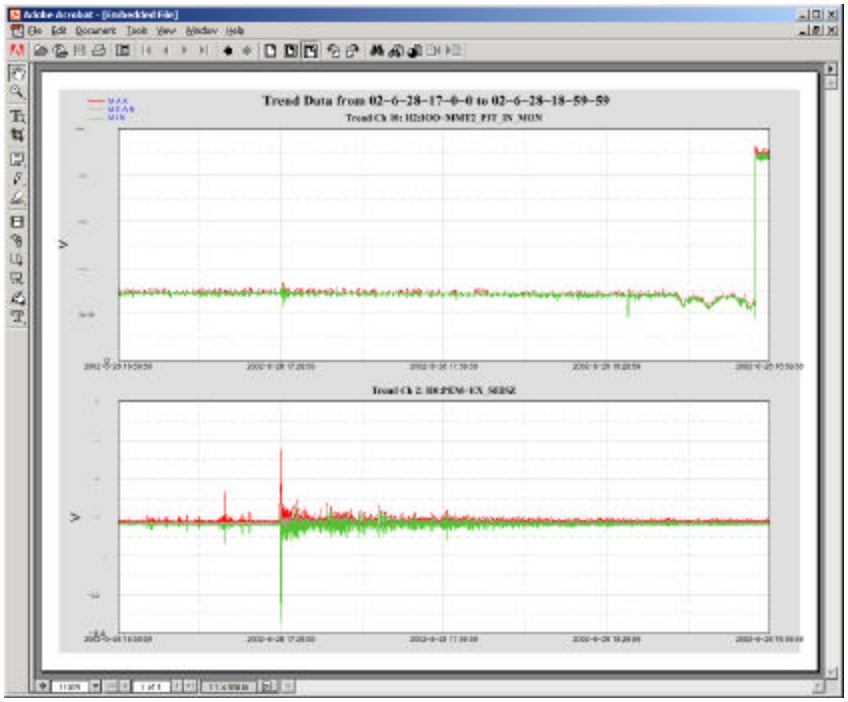
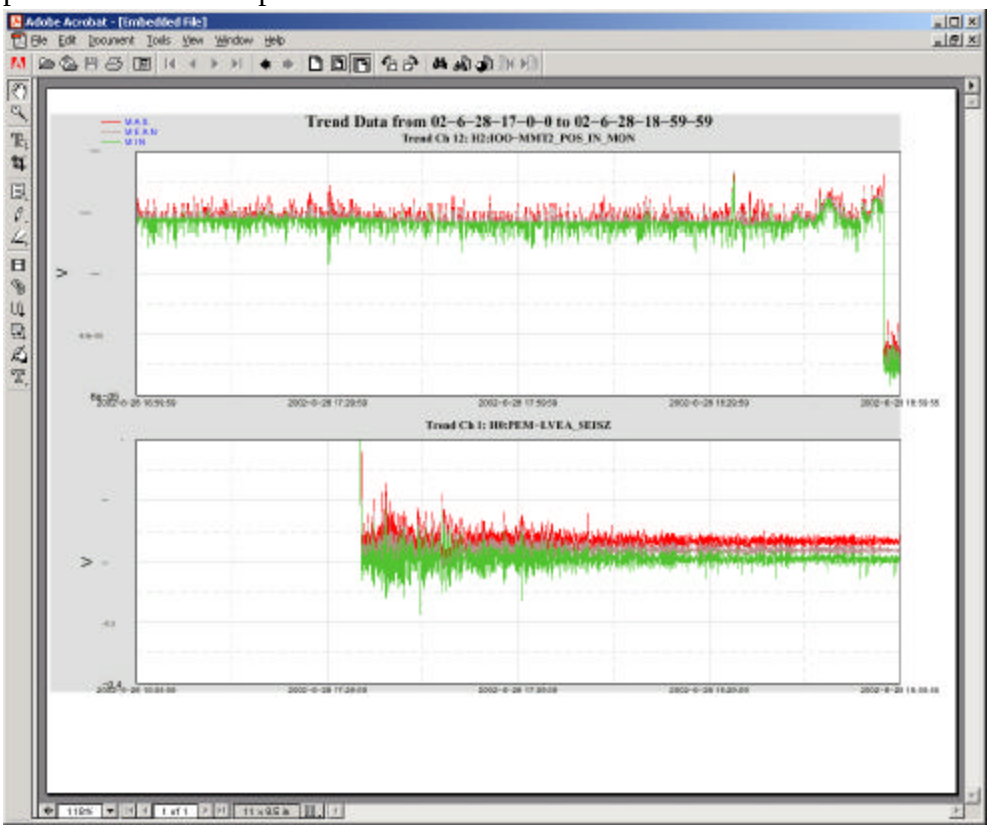
- contains reference url for this entry.)

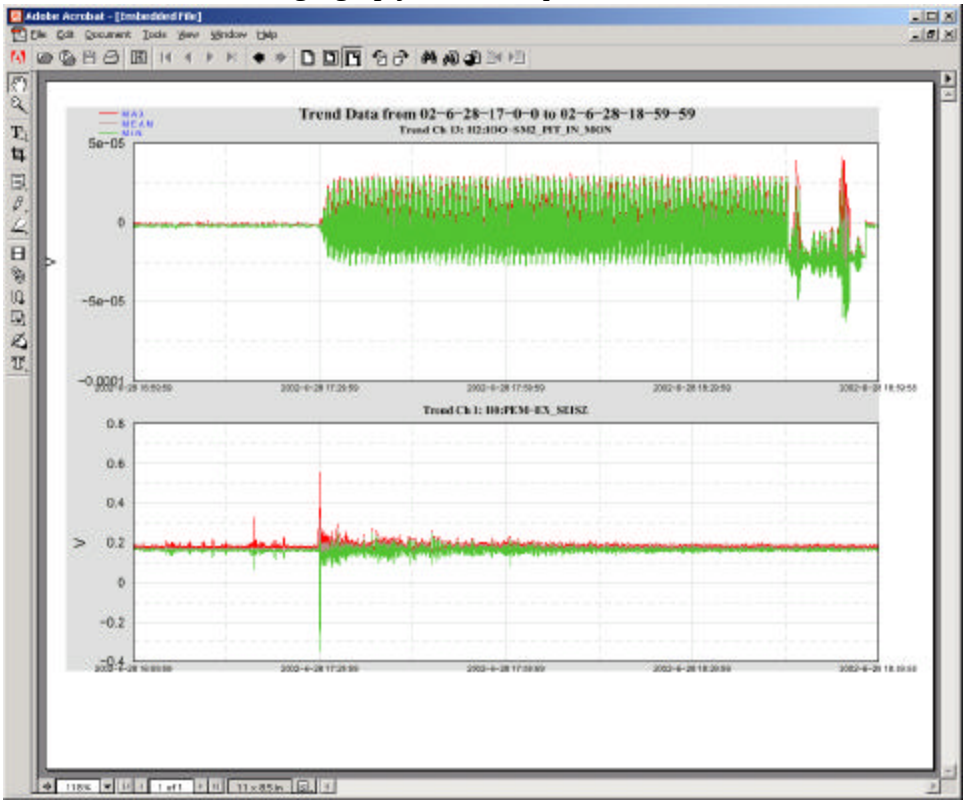
Plot 1. Earthquake is at the left, slow drift of MMT2 is at the right, and the final dive is at the far right.



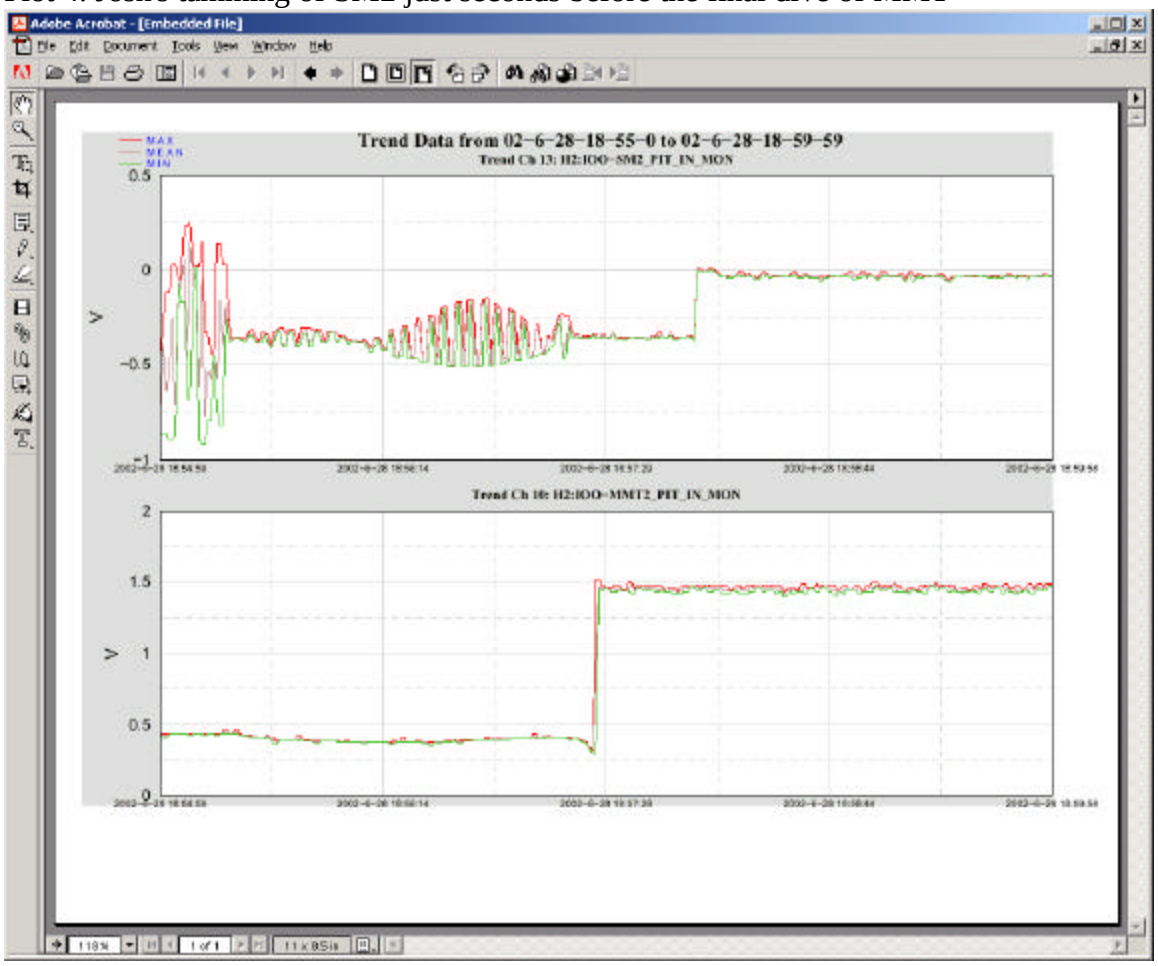
Plot 2. Shows LVEA seismometer, which wasnt on for the arrival of the quake, and position rather than pitch of MMT2



Plot 3. Shows SM2 ringing up just as the quake strikes.



Plot 4. Josh's tamming of SM2 just seconds before the final dive of MMT



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Tue Jul 2 18:31:57 2002 **UTC**

This plot shows that the Chinese quake was the third or fourth largest peak seismic velocity in the last 40 days.
There are a couple of other quakes in this 40 day trend
that may have had similar frequency composition and amplitude.
The quake before the failure is
the last glitch at the right side of the trend. I have picked the MY seismometer because it doesnt show as many of the bangs typical in the LVEA that may produce high velocities but which are mainly at high frequencies that get filtered by the stack.

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robert
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http://
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- contains reference url for this entry.)