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Laser Interferometer Gravitational Wave Observatory (LIGO) Project

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Subject: **Magnet size considerations; interference and coil power dissipation**

*Peter Fritschel and Alex Marin found errors which I decided to correct (along with adding a real DCC number) before re-releasing this note. --MEZ 7/22/96*

Seiji--

I raised some concerns at the SUS PDR about the size of magnets, and so I went back to our Suspension Concept writeup of April 1992[1] to see what we felt were safe dipole moments given environmental magnetic fields. Briefly, we found that if the 10 kg mass has a remanent unbalanced dipole moment of $\mu_{net} = 3 \times 10^{-3}\ \mathrm{A \cdot m^2}$ and the local conductors cause nonuniform shielding with a scale length of 10 cm, a $\vec{B}$ field fluctuation of about $10^{-11}\ \mathrm{T}/\sqrt{\mathrm{Hz}}$ would induce a force equal to the target pendulum thermal noise. We looked at various sources, including fluctuating local currents due to our own instrumentation; for example, $50\ \mu\mathrm{A}/\sqrt{\mathrm{Hz}}$ current noise in a wire 1 meter away would produce this field. We also did a back-of-the-envelope calculation of spurious pulses due to lightning strikes; though there are many uncertain parameters, we found lightning bolts 1000 km away could produce force pulses exceeding the target pulse sensitivity (and, worse yet, potentially correlated between the sites). I'm not sure why, but our current PEM magnetometer specifications seem to call out a sensitivity of only 1 $\mathrm{nT_{rms}}$ from DC - 1 kHz, which is not adequate to veto things at this level. In our document we suggested coil magnetometers about 300 times more sensitive. One thing we didn't look at was the level of 60 Hz and multiples; we should have. If I take the "typical" 60 Hz field level of 10 mG = 1 $\mu$T described in the LIGO EMI Control Plan (E960036-02-E, page 8), I think we will have a problem without very extensive shielding; under the above conditions, such 60 Hz fields would result in 20 fm RMS motion of each test mass. This is a sizeable fraction of the total RMS motion allowance, at a frequency where little or no loop gain is available to suppress it.

It may be that the expected net dipole moment of each mass will be lower, either due to careful balancing (which is obviously a good idea in any case) or due to smaller magnets. But some caution is needed; while the dipole cancellation of the assembly might in principle be perfect (you would need to measure and select matched magnets to get better than 5% I think), the field gradi-

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[1] *Test Mass Suspension and Control Concept for Initial LIGO Receivers* (rev. A) by Kawamura, Sievers and Zucker, 10 April 1992 (LIGO-T920003-A-D).

ent is strongly modified by local metal and is unlikely to be uniform over the diameter of the mass. My personal judgement is that it is prudent to expect a residual magnetic moment of about one full magnet's worth to account for this nonuniformity.

In the end I found it too taxing to compute the moment of your design magnets from the material data sheet Janeen sent me, but from the dimensions on your drawing it looks like the volume is about 0.009 cc, or about 24% of the volume of the old OSEM magnets. Those old ones had a dipole moment of about 0.03 A $m^2$ and were made of SmCo alloy, which has about 60% the magnetization per unit volume of the NdFeB material you want to use; so my best guess is that your design magnet has $\mu \approx 0.01\ \text{A m}^2$, about three times the moment assumed in our memo (can you verify this?). This makes the susceptibility to stray fields even higher.

So, what stops us from using really small magnets? Assume the peak force required per magnet is fixed. Clearly the smaller the magnet, the more power the electronics need to provide to achieve this force. More power is obviously more difficult and more costly to provide, but scaling up the power might make the required *dynamic range* easier to achieve, depending on how it's done. So it isn't obvious to me where the true optimum will lie from the electronics standpoint. It is also subject to optimization of the coil winding wire diameter (or turn count in a given volume), to achieve the best driving impedance compromise.

Let's assume that these electronics considerations provide no constraints, i.e. we can supply any amount of power to achieve the required force and still get the noise we need. Then the dominant remaining constraint is the heating of the coils in vacuum. I would be very nervous about permitting anything that close to the mirrors to rise significantly in temperature, especially given that it has so many questionable materials (epoxy, solder, wire insulation, etc) and was not baked very hot after assembly (because the electronics can't take it). I would propose a maximum temperature rise of 20 K over ambient[1].

With the coil form geometry in your design, filled with copper magnet wire of round cross-section, I calculated Joule heat power dissipation for 1 Newton force to be

$$P \approx 16\ \text{kW} \times \left(\frac{F}{1\ \text{N}}\right)^2 \left(\frac{0.01\ \text{A m}^2}{\mu}\right)^2 .$$

The Design Requirements motion allowance of 80 microns peak-to-peak (i.e. 40 microns DC or max. RMS) requires a DC force of 0.009 N, or 2.3 mN/coil for 4 coils; at the above magnet size you'd dissipate 85 mW in each coil. Note that this is considerably less than the 0.7 W shown in the PDR viewgraphs; you appeared to be using only one coil instead of four, and power/coil goes like $1/n^2$ for fixed total force (still leaves a factor of 2 somewhere... your actual magnetic moment, coil R and turn count would be helpful here).

To estimate the temperature rise, I looked at a few cooling paths. Radiation is not very effective, providing about 0.5 K/mW effective thermal resistance if the emissivity is 1/2. The lead wires are

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[1] I understand the LED's die at 70C, so this is already uncomfortably close to the maximum temperature the assembly could ever be degassed at.

roughly comparable (unless they are designed to carry heat!). The conduction gradient along the bulk of the Macor form is low, but the kinematic contacts to the holder will have a very small total area (I estimated 0.1 $mm^2$ based on deforming annealed stainless, Brinell 200, with a 5 lbf load from the setscrew spring plunger). This also gives me of order 1 K/mW temperature rise, still pretty weak cooling. You could do much better by bedding the OSEM on indium or another soft metal to increase the contact area.

If these estimates are even close, it's telling us the peak force requirement is too ambitious, the coils need some kind of explicit cooling provision, and/or the magnets need to be bigger. If the old interference calculations above are right too, the magnets in fact should probably be made smaller. My conclusion is that it should be a very high priority to test one of these coils in a realistic holder in vacuum, checking both outgassing and T as a function of current. I would also like it if someone could go through the 1992 magnetic interference calculations with me to be sure they make sense, and see if we have any more data on measured fields and gradients. I remember Fred wanted to do a direct test of the magnetic susceptibility of one of the 40m mirrors; was this ever done?

mez:mez

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