Gmail **robert schofield <rmssrmss@gmail.com>**

[detchar] Fwd: Charged LIGO Mirrors and EM Chirps

13 messages

Alan Weinstein <ajw@ligo.caltech.edu> Tue, Aug 9, 2016 at 8:15 AM
Reply-To: detchar@sympa.ligo.org, Alan Weinstein <ajw@ligo.caltech.edu>
To: "detchar@ligo.org" <detchar@ligo.org>, "calibration@ligo.org" <calibration@ligo.org>

Hi all,

Here's an email from a very smart physicist I used to work with at SLAC, who asserts that the electrostatic charge on our mirrors are sufficiently susceptible to stray EM fields to fake a GW signal, without any PEM channels witnessing it.

I was under the impression that detchar folks have demonstrated that any stray fields large enough to do this would be witnessed by a PEM channel.

And, of course, the form of the signal, and the coherence between detectors, suppresses the chance that this can happen, without many more "glitches" that don't look like chirps and are not coincident, that we would have noticed.

I am not knowledgeable about the effect of charging and can't respond intelligently about this possibility, from the point of view of physical effects associated with "ELF radio waves". Jeff Kissel tried to address his concerns, but he did not convince Gary.

I'm sending this to detchar and calibration groups, if anyone has more insight. Anyone else who could address this, that's not on the detchar or calibration lists?

Thanks,

Alan

---------- Forwarded message ----------
From: **Godfrey, Gary L.** <godfrey@slac.stanford.edu>
Date: Mon, Aug 8, 2016 at 11:24 PM
Subject: Charged LIGO Mirrors and EM Chirps
To: "ajw@caltech.edu" <ajw@caltech.edu>
Cc: "weiss@ligo.mit.edu" <weiss@ligo.mit.edu>, "Godfrey, Gary L." <godfrey@slac.stanford.edu>

August 8, 2016

Hi Alan,

It's been a long time since we've talked, and I hope all is going well for you. I just heard that Charlie Peck passed away; a wonderful man whom I will miss. I've retired from SLAC and now have some time on my hands! Your LIGO results are very exciting. In thinking about GW150914 I came up with the following experimental concern.

The LIGO end mirrors are charged by the electrostatic pusher plates and electron+ion currents in the vacuum. An ELF radio wave (chirp) can therefore move the mirrors around. A reasonable estimate of the mirror charge is 1 to 16 nC. The magnitude of an EM wave that would cause the observed .5 x 10^-21 strain is much below the veto threshold of your magnetometer. GW150914 could therefore be a sferic from lightning or even some human made EM signal. An internet search reveals atmospheric recordings of rising chirps called "chorus" of about the correct magnitude, but of too high a frequency (1-3 kHz). I have not found 30-150 Hz chirps in my brief internet search. Fermi also saw a coincident signal of ~1 MeV gammas in its GBM.

The details of the calculation are in the attached pdf, but I will summarize them here. The pusher electrodes cover ~1/2 the area of the pusher face. The pusher and mirror mass are 5 mm apart and of 34 cm diameter. I calculate a capacitance of 80 pf between the electrodes and mirror mass. As I understand, some strips are at 0 volts (wrt vacuum chamber) and some are at 400 volts (unfortunately not -200 and +200 volts) for an average voltage of 200 volts. A naive over estimate of the charge on the mirror mass would therefore be Qmax=CV=(80 pf) (200 volts)=16 nC. However, no conductive wires connect the mirror to the vacuum chamber, and the vagaries of ion pump currents and UV liberated electrons will have to supply the conductive path for charge to get to the mirror mass. In March, I corresponded with Jeff Kissel (LIGO Hanford Controls Engineer), who told me that you were worried about the amount of charge on the mirrors because its change and the charge mirroring forces from the chamber walls could be a source of strain noise. Jeff was very polite and helpful, and explained that you measured a VeffectiveBias (which is the pusher bias where a small additional bias wiggle voltage causes no change in strain) that was proportional to the mirror mass charge. I can see from your Ligo logs that Jeff makes an effort to keep VeffectiveBias <10-20 volts by occasionally reversing the polarity of Vbias. Then an estimate of the minimum charge on the mirror mass (on 9/14/2015) might be Qmin=(80 pf)(10 volts)=.8 nC . A 16 nC mirror with a .5 x 10^-21 strain amplitude will result from a 1.0 mV/m amplitude 50 Hz EM sine wave, attenuated by ½ for passing thru 4 mm of SS vac chamber. A .8 nC mirror would require a 20 mV/m amplitude EM sine wave. For an EM wave the corresponding B fields are B=E/c =3 to 60 pT. Both these fields are less than a 2 sigma veto threshold of 142 pT that I estimate you applied from the magnetometer (with a 4 pT/sqrt(Hz) noise shown in your instrumental companion paper). Jeff thought that your Displacement versus Magnetic Field measurement (~2 x 10-10 m/T shown in your instrumental companion paper) would rule out such small 3-60 pT fields as causing the observed strain since (4 x 10^6 m)^-1 (2 x 10^-10 m/T)(60 x 10^-12 T)= 3 x 10^-27 strain. Unfortunately, the displacement measurement was made with something like a Helmholtz coil in which the near field E at 50 Hz is much smaller than E=cB of a travelling EM wave. I didn't re-correspond and take up more of Jeff's time with this explanation.

So, GW150914 may be a gravitational wave, but it also might not be one, instead being an un-vetoed EM chirp. As Advanced LIGO lowers its strain noise, the sensitivity to EM chirps faking GWs will increase. Some ameliorations for future data could include:

1) Add an ELF radio receiver+antenna with a low enough noise so that a low enough veto threshold may be set. Preferably, continuously digitize and save the receiver output.

2) Definitively measure the charge on the end mirrors. For example, with 2 plates in the vacuum pipe, apply a known electric field as a function of frequency and record the mirror motion.

3) Minimize the mirror charge by making the average pusher plate voltage = 0.

4) Develop a method to discharge the mirror to the vacuum chamber potential (UV light?).

5) Add EM shielding around the end mirrors (eg: increase the EM attenuation by making the vacuum pipe thicker).

6) Perhaps you could prove GW150914 is not an EM chirp by fitting the exact GW150914 pulse shape (assumed to be EM and corrected for its passage thru the vac pipe walls) at the exact time to the digitized magnetometer outputs from both end stations. This digging into the noise would be similar to how you use a template to pull a GW event out of the strain time series.

Alan, I have sent a copy of this email to Ray Weiss so, if interested, you would have someone to talk to. Jeff also told me that for a long time Ray has been concerned about charge on the mirrors affecting the strain noise.

Be well,

Gary Godfrey
SLAC
godfrey@slac.stanford.edu

LigoEmWave_2.pdf
3457K

Jeff Kissel <jkissel@ligo.mit.edu> Tue, Aug 9, 2016 at 9:16 AM
Reply-To: detchar@sympa.ligo.org, Jeff Kissel <jkissel@ligo.mit.edu>
To: "<detchar@sympa.ligo.org>" <detchar@sympa.ligo.org>, "<calibration@sympa.ligo.org>" <calibration@sympa.ligo.org>

Hey All,
I forward my (rather long!) conversation with Gary back in April. Start at the bottom if you're really interested. I'm happy to have anyone else try a different angle of attack for convincing Gary on this; I've convinced myself that it's not an issue. I'm not sure if he's contacted Brian Lantz.

Cheers,
Jeff Kissel
Controls Engineer
LIGO Hanford Observatory
Mailing Address: P.O Box 159, Richland, WA 99352-0159
Shipping Address: 127124 N Route 10, Richland, WA 99354
Office Phone: +1 509 372 8108

> Begin forwarded message:
>
> **From:** "Godfrey, Gary L." <godfrey@slac.stanford.edu>
> **Subject: RE: How much electric charge is on an aLIGO end mirror?**
> **Date:** April 4, 2016 at 3:25:53 PM PDT
> **To:** Jeff Kissel <jkissel@ligo.mit.edu>
>
> Hi Jeff,
>
> Thank you for your thoughtful answer to my questions and in particular for the companion paper reference <https://dcc.ligo.org/LIGO-P1500238/public>. The 3rd plot of the paper's Figure 2 shows aLigo's measured coupling [meters/Tesla] between an applied osc B and mirror motion for the corner mirror (I don't think this 45 degree splitter has an electrostatic pusher on it since the reaction mass would obstruct the various beams). The coupling measurement is a great way to determine the total charge on the mirror. In the attached pdf file I have transferred the measured data points (blue) to the first plot and overlaid a calculation the response (red curve) of a charged mirror. A charge of 50 pC best goes thru the points. Not knowing what the actual vacuum vessel materials and wall thicknesses are, I used attenuation in 3 cm of aluminum walls to get the freq dependence correct. This 50 pC is consistent with ~1 V on the mirror (work function difference between silica and surrounding metal and what your electrometer measurements showed) and ~50 pf capacitance.
>
> What would really be interesting (and aLigo has probably already done this??) would be the coupling

measurements [meters/Tesla] for the end mirrors with the electrostatic pusher voltages set as they are during normal running (eg: like they were set when GW150914 was detected). This would answer the question of what the charge on the end mirrors was, and would the magnetometers have been sensitive enough to veto an EM wave as the cause of the 10^-21 strain.

In the second plot of the attached pdf, I plotted the amplitude of B that could be responsible for the observed 10^-21 strain for both a 50pC charge (red line) on the mirror and a 16nC charge(dashed red line). The thick black line is a 2 noise sigma veto threshold for aLigo's magnetometer. As you said in your email, a causative B for 50pC would have been vetoed. However, the necessary B is smaller for a 16nC charge, and is below (at 50 Hz) or only slightly above (at 100 Hz) the veto threshold.

I left the estimated charge at 16nC because the pusher traces cover ~1/3 the area of the disk but the average pusher voltage could be 200-400 volts which is 3 times the 100 volts used in my previous email. You could minimize the total charge on the mirror by being sure "Sum over all pusher traces(Area of trace x Voltage on trace)=0 wrt vac chamber walls" . You are absolutely right that the calculation of charge densities is a devil of a task with all the unknown high resistances, nearby surfaces, and vac ion currents. Fortunately, you could just leave the Helmholtz coils installed near the end mirrors, and periodically measure the charge. This will measure the total charge on the mirror and help prove aLigo is not just seeing EM effects, but I don't think it helps measure the distribution of charge on the mirror that you are interested in for noise and pusher performance.

Atmospheric effects (Schumann waves, Q-bursts, sferics) make EM waves at low frequencies and various pulse shapes that bounce around the world in the cavity between the earth and the ionosphere. Charge particles spiral along field lines between the earth's poles, emitting EM waves as they go. The solar wind compresses the earth's B field causing coincident EM effects over large areas. A ~100 Hz EM wave in coincidence at Hanford and Livingston is very possible. However, an EM wave with GW150914's rising chirp profile I have not found in Google searches. I'm not sure this is relevant, but the wavelength of a 100 Hz wave is 3000 km which is the same as the distance between Hanford and Livingston. It would be best just to eliminate all of this by showing the charge on a mirror is so low, that an EM wave able to cause the measured strain, would be vetoed by the magnetometers.

You may be able to measure the total mirror charge by putting the same offset voltage on all the pusher electrodes. This would move the mirror but a) maybe the charge is not opposite the electrodes where you know the electric field and are able to calculate the force, and 2) maybe your new offset voltage causes the total charge on the mirror to change during the measurements.

Also, thank you for pointing me at Brian Lantz.

Gary

(Physicist retired from SLAC as of 1 month ago!!)

-----Original Message-----
From: Jeff Kissel [mailto:jkissel@ligo.mit.edu]
Sent: Thursday, March 24, 2016 6:47 PM
To: Godfrey, Gary L.
Cc: Dale Ingram; katzman_w@ligo-la.caltech.edu; burtnyk_k@ligo-wa.caltech.edu
Subject: Re: How much electric charge is on an aLIGO end mirror?

Hey Gary,
Would you like to come work for LIGO? Your questions are particularly on point, and expose exactly our areas of concern and uncertainty. We've indeed performed similar back-of-the-envelope predictions of potential noise sources from charge distribution on the test masses, but have yet to experimentally confirm anything. In

> fact, this is a medium-priority, hot topic in the collaboration. We both want
> (a) more-sophisticated-than-back-of-the-envelope models of how charge can couple to the electrostatic drive system, and
> (b) how mobile and/or impulse charge currents can cause noise on the instrument.
> Why?
> Exactly because the back-of-the-envelope says we should be seeing such things, but all measurements have yet to convincingly produce any results.
>
> Here're the more pointed answers to your question. Look for concluding statements below these.
> -----------
> You've done the same back-of-the-envelope calculation as us:
>
>> The reaction mass is 5 mm away from the mirror mass. I calculate the capacitance between the mirror and reaction mass is 160 pf (using 34 cm as the mirror and reaction mass diameter). If the average voltage on the reaction mass electrodes is 100 volts, then the capacitor is charged and the mirror has a charge of 16 nC.
>
> But, again, we have been dismayed / reminded by experiment that the electrostatic drive is not as simple as a parallel plate capacitor with infinite area. The electrostatic drive system relies on the attractive, *dipole* interaction between the test mass and two adjacent electrodes on the reaction mass. There are no electrodes on the test mass. So it's not a capacitor, per se. Further, the electrode pattern is an annulus on the outer edge of the reaction mass, so fringe fields cannot be ignored. Even further, the test mass and reaction mass are surrounded by a metal support structure, or "cage" at comparable distances to the reaction mass / test mass gap which can create image fields that interact with the drive system itself, and also charge where ever it may be (either in between the gap [near field interactions], or on the test mass [far field interactions]). The reality of the surrounding electric field, the geometry of the reaction mass electrodes, and the added complexity of dipole interactions makes the back-of-the-envelope calculation inaccurate quite quickly.
>
>> 1) Have you measured the mirror charge?
>>
>> 2) From the actual voltage on the traces have you more accurately calculated the mirror charge?
>
> Yes. Prior to closing up the vacuum chambers, we discharge the test mass and reaction mass using small bits of electrostatic material, and well as blowing ionized nitrogen across the surfaces of the masses for a long duration (~10 minutes) to further neutralize any remaining charge. We then measure near the masses with an electrometer, but as you may know, the sensitivity of such hand-held electrometers is not awesome, and they merely tell you the residual voltage at whatever distance one measures (we measure several of course), not the desired charge density. With these electrometers, after discharge, we measure a residual charge of a few Volts.
>
> Our best handle on the charge while the instrument is under vacuum is to measure the electrostatic drive actuation strength as a function of applied bias voltage and location. While this, again, is not a direct measure of the charge density, it's a measure of the impact of charge density and current on the interferometer. We fit actuation strength vs. bias voltage to a line, and find the zero crossing. That zero crossing we call the "effective bias voltage from charge on the test mass." Granted, given the complex electrical environment, this is actually a measure of several different things, but in some sense the changing actuation strength is one of the most important end-results of changing charge distribution.
>
> As such, we've measured that the effective bias voltage from charge is comparable to what is measured by the electrometer just before pump-down, in the few Volts region. However, we indeed see this change over

time, at the accumulation rate of ~10 V/month. Because our applied bias voltage is in the ~200-400 Volt range, it's not a huge impact, but it is non-negligible if we want to calibrate our instruments to better than the 10% level.

We believe that this charge accumulation is merely due to ion currents (as you suspect) in the chambers. Indeed, a year ago, we had had giant ion pumps within ~20 ft of the test mass, and we saw the effective bias voltage swing around in the 100s of volts per week. Ridiculous, but in retrospect not surprising. We've sense moved these ion pumps 250 meters away, and now have the charge accumulation rates I mention above.

Several collaborators of mine have taken the estimate of effective bias voltage, as measured by actuation strength, and tried to turn that into estimates of surface charge density, but all attempts at modeling (thus far more sophisticated than back-of-the-envelope, but still analytic) have failed to explain measurement. I've just launched a colleague at Moscow University on putting together a finite-element analysis (Comsol) model of the masses and surrounding electrical environment, and we'll see what that produces.

> This is a relevant question because charged mirrors can be moved by the electric field of an EM wave. For 16nC, a B=10 pT (E=cB) amplitude EM wave at 50-100 Hz would give the 10^-21 strain observed for the GW chirp. This 10 pT is below the noise threshold of ~100 pT that I calculate for your veto magnetometer. Q-bursts (associated with lightning caused Sprites) have 10 pT amplitudes of about the correct freq but are decaying chirps rather than the increasing amplitude chirps you saw, but maybe there is some other rarer atmospheric pulse with the correct shape.
>
> 3) What is the minimum B amplitude pulse (with the shape of the GW chirp that you saw) that would have been vetoed by the magnetometer?

While I highly recommend you read our detection's companion paper, <https://dcc.ligo.org/LIGO-P1500238/public>
which will give you more quantitative information on the magnetic susceptibility of the detectors (roughly, the estimated coupling of the ambient magnetic field as measured by local magnetometers is a factor of 10 or more below the current strain sensitivity), the biggest take away is that one would need to see a coherent, coincident, magnetic pulse in *both* detectors in order to look anything like a gravitational wave.

> 4) If the mirrors are charged, you must see lots of Q-burst type decaying chirps ... do you? ... I would guess these would only be seen in aLIGO since LIGO had magnets on the mirror for positioning and hence no charge. These would have been less sensitive to the B of the EM wave.

We do not see any such events regularly.

I see that you're at SLAC in Stanford. Coincidentally, one my colleagues that has been most interested in charge near the test mass (and one of the few who've attempted an analytical model) is also at Stanford: Brian Lantz <BLantz@stanford.edu> You should get in touch with him; I suspect you'll get a higher-bandwidth discussion of what you're asking, if I haven't answered all of your questions. But, be careful - like I'd said in my preamble - if you reveal that you know too much about electrostatics in precision experiments, you might get conscripted!

Let me know if I can be of further help.

Cheers,
Jeff Kissel

Controls Engineer
LIGO Hanford Observatory
Mailing Address: P.O Box 159, Richland, WA 99352-0159 Shipping Address: 127124 N Route 10, Richland, WA 99354 Office Phone: +1 509 372 8108

On Mar 23, 2016, at 8:51 AM, Dale Ingram <ingram_d@ligo-wa.caltech.edu> wrote:

Jeff:

Could I trouble you to reply to Gary regarding his questions about charging, when you have a chance?

Thanks,

Dale

On 3/23/2016 3:17 AM, Godfrey, Gary L. wrote:

Hi,

I read that aLIGO can put up to a plus/minus few hundred volts onto traces on the reaction mass. The reaction mass is 5 mm away from the mirror mass. I calculate the capacitance between the mirror and reaction mass is 160 pf (using 34 cm as the mirror and reaction mass diameter). If the average voltage on the reaction mass electrodes is 100 volts, then the capacitor is charged and the mirror has a charge of 16 nC. Ion currents in the vacuum may also effect the charge since the mirror must have a pretty high resistance path up thru the suspension fiber to the power supply ground.

1) Have you measured the mirror charge?

2) From the actual voltage on the traces have you more accurately calculated the mirror charge?

This is a relevant question because charged mirrors can be moved by the electric field of an EM wave. For 16nC, a B=10 pT (E=cB) amplitude EM wave at 50-100 Hz would give the 10^-21 strain observed for the GW chirp. This 10 pT is below the noise threshold of ~100 pT that I calculate for your veto magnetometer. Q-bursts (associated with lightning caused Sprites) have 10 pT amplitudes of about the correct freq but are decaying chirps rather than the increasing amplitude chirps you saw, but maybe there is some other rarer atmospheric pulse with the correct shape.

3) What is the minimum B amplitude pulse (with the shape of the GW chirp that you saw) that would have been vetoed by the magnetometer?

4) If the mirrors are charged, you must see lots of Q-burst type decaying chirps ... do you? ... I would guess these would only be seen in aLIGO since LIGO had magnets on the mirror for positioning and hence no charge. These would have been less sensitive to the B of the EM wave.

Thank you in advance for answering all these questions,

Gary

godfrey@slac.stanford.edu

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Dale Ingram
Education and Outreach Coordinator
LIGO Hanford Observatory
P.O. Box 159
127124 N Route 10
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Phone: 509-372-8248
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Web: www.ligo-wa.caltech.edu <http://www.ligo-wa.caltech.edu/>

LigoEmWave_3.pdf
38K

Brian O'Reilly <brian@ligo-la.caltech.edu> Tue, Aug 9, 2016 at 12:48 PM
Reply-To: detchar@sympa.ligo.org, Brian O'Reilly <brian@ligo-la.caltech.edu>
To: detchar@sympa.ligo.org
Cc: Alan Weinstein <ajw@ligo.caltech.edu>

Alan,

One immediate problem observation is that the thickness of the BSC chamber walls is actually half an inch or ~0.0127 m, so Gary's number is off by a factor of ~3, which changes the calculation by about a factor of 10 in favour of us seeing the field in the magnetometers.

Also the capacitance is probably too large by a factor of 4, but at least a factor of 2 (the ESD pattern partially covers an annular area on the test mass with an inner diameter of 226 mm and an outer diameter of 336 mm).

At LLO we had one mirror which had a large charge on 2 quadrants of ETMX (~400 V) . ETMY at LLO was under 20V and both end test masses at LHO were similar to ETMY at LLO (by flipping the bias voltage sign on a weekly basis we found we could control charge build up).

So the q=C.V term (charge on the mirror) is on average considerably smaller by at least a factor of 20 and probably more.

In short I think it is quite reasonable to estimate the B field needed outside the chamber at > 3.34e-10 Tesla i.e. 2 orders of magnitude at least above Gary's value. That also puts it a factor of 2 or more above the veto threshold.

Once you add in the coincidence requirement, the waveform morphology requirement, the fact that we don't see such events in single interferometer studies, I would conclude that we are in the clear.

Brian
[Quoted text hidden]
--
Senior Staff Scientist
LIGO Livingston Observatory
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Cell: 225-241-6146
Fax : 225-686-7189
http://www.ligo-la.caltech.edu/~irish

---

robert schofield <rmssrmss@gmail.com> Tue, Aug 9, 2016 at 1:50 PM
To: "detchar@sympa.ligo.org" <detchar@sympa.ligo.org>, Alan Weinstein <ajw@ligo.caltech.edu>
Cc: "detchar@ligo.org" <detchar@ligo.org>, "calibration@ligo.org" <calibration@ligo.org>

Hi Alan,

I can try fielding this too; at least some of his arguments were rebuffed in the PEM event vetting report. In addition, we have plenty of injections (natural and anthropogenic) showing that this doesnt happen. I agree with Brian's shielding comments and other problems with his estimates as well. But I wont be able to get to it for a few days.

Robert
[Quoted text hidden]

---

robert schofield <rmssrmss@gmail.com> Sun, Aug 14, 2016 at 5:44 PM
To: "detchar@sympa.ligo.org" <detchar@sympa.ligo.org>, Alan Weinstein <ajw@ligo.caltech.edu>
Cc: "detchar@ligo.org" <detchar@ligo.org>, "calibration@ligo.org" <calibration@ligo.org>

Hi Alan, DetChar,

I got a chance to look at his calculations. The biggest issue is that he doesnt account for reflection and this makes his estimate off by many orders of magnitude. I plan to send the following response on Monday. Let me know if there are comments or suggestions.

Robert

Hi Gary,

Let me address your concerns from the point of view of one of the LIGO environmental coupling specialists. It is a pleasure communicating with someone who cares about some of the same issues that I do. And I really appreciate having an expert outsider who takes the time to seriously challenge us. I don't think that this service is provided nearly as often as would be ideal in science.

My biggest difference with your coupling estimate is that I think that you underestimate, by many orders of magnitude, the attenuation by the vacuum chamber walls. I think that the estimate does not account for the very high reflectivity of steel for E-fields at audio-band frequencies. I think that the fallacy in this omission is illustrated in the resulting prediction that the chamber walls would not attenuate the E-field at all as the frequency approaches 0 Hz, while Gauss's law would predict complete attenuation.

The following web page calculator estimates that the transmission at 50 Hz would be less than 1e-6, much less than the 0.5 that your estimate gives for the same frequency. http://www.clemson.edu/ces/cvel/emc/calculators/SE_Calculator/index.html

My understanding is that the attenuation for high-impedance near-field electric fields is even greater than for the far-field plane waves of the estimate. It is the low-impedance near-field magnetic fields that penetrate the chamber, hence our focus on magnetic coupling.

Our EM attenuation is reduced by view ports, but even with the view ports, our in-band E-field attenuation is at least several orders of magnitude. For example, we have measured fields of 1e-4 V/m sqrt(Hz) inside of a chamber, while typical outside fields are above 1 V/m (we could never have reached our sensitivity if your attenuation calculations were correct). And a significant part of the internal fields may be produced inside of the chamber.

Even though we are confident of the large degree of attenuation afforded by our chambers, we also studied the coupling of EM fields from lightning strikes (you may find it interesting that I have the time/location/current data for nearly every cloud-to-ground and the larger of the cloud-to-cloud strikes in the US during our observation run on my laptop, as well as nearly all strikes globally for certain time periods). The fields from many lightning strikes during the run would have been much larger, 4 or 5 orders of magnitude larger, than your estimated threshold of visibility on the gravitational wave channel. Yet we did not see them in the gravitational wave channel (though some where huge on magnetometers).

And, to be triply sure, we monitor for the EM choruses that you propose as the source of the EM fields (even though choruses come in multiples, are at higher frequencies and chirp for much longer than binary black holes). We have our own RF scanner at LHO that was working for the boxing day event, and we consulted external low- and high-frequency RF observatories for all three vetted events.

Please contact me if you have any further questions on any of these details. There was actually significant internal discussion of these matters since there was a very large lightning strike over Burkina Faso that coincided with GW150914, our first detection.

Dr. Robert Schofield

Department of Physics

University of Oregon

[Quoted text hidden]

David Shoemaker <dhs@mit.edu> Sun, Aug 14, 2016 at 5:54 PM
To: detchar@sympa.ligo.org, robert schofield <rmssrmss@gmail.com>
Cc: Alan Weinstein <ajw@ligo.caltech.edu>, "detchar@ligo.org" <detchar@ligo.org>, "calibration@ligo.org" <calibration@ligo.org>

I like it.

thanks!

d.
[Quoted text hidden]

Kai Staats <kai.staats@ligo.org> Sun, Aug 14, 2016 at 7:00 PM
Reply-To: kai.staats@ligo.org
To: detchar@sympa.ligo.org

Personally, I learned a great deal.
[Quoted text hidden]

Alan Weinstein <ajw@ligo.caltech.edu> Sun, Aug 14, 2016 at 7:20 PM
To: robert schofield <rmssrmss@gmail.com>
Cc: "detchar@ligo.org" <detchar@ligo.org>, "calibration@ligo.org" <calibration@ligo.org>

Hi all,

Thanks Robert, and also Jeff, Anamaria, Brian!

I forwarded your responses to Gary, and also David's invitation for Gary to visit a site. I gave him the arguments from the data analysis perspective (coincidence, agreement with GR over many cycles, absence of single-detector chirps, no correlation with any PEM channel...). I also told him to expect your email, Robert.

We'll see how he responds...

Thanks again,

Alan

[Quoted text hidden]

Albert Lazzarini <lazz@ligo.caltech.edu> Sun, Aug 14, 2016 at 11:13 PM
To: detchar@sympa.ligo.org, Alan Weinstein <ajw@ligo.caltech.edu>
Cc: robert schofield <rmssrmss@gmail.com>, "detchar@ligo.org" <detchar@ligo.org>, "calibration@ligo.org" <calibration@ligo.org>

What about inviting him to join the LSC?

LIGO Laboratory
California Institute of Technology
1200 E. California Blvd.
Pasadena, CA 91125 USA

626-395-8444 (Office)
626-628-3847 (Facsimile)
[Quoted text hidden]

Brian O'Reilly <brian@ligo-la.caltech.edu> Wed, Aug 17, 2016 at 2:33 PM

To: robert schofield <rmssrmss@gmail.com>

Duh! Good one. I didn't think about the reflected field either.

Brian

[Quoted text hidden]

[Quoted text hidden]

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robert schofield <rmssrmss@gmail.com> Thu, Aug 18, 2016 at 11:08 AM
To: Brian O'Reilly <brian@ligo-la.caltech.edu>

And not a bad omission for BOE calculations at the usual RF frequencies... -R

[Quoted text hidden]

Godfrey, Gary L. <godfrey@slac.stanford.edu> Mon, Aug 22, 2016 at 12:15 PM
To: Rai Weiss <weiss@ligo.mit.edu>, Alan Weinstein <ajw@ligo.caltech.edu>, robert schofield <rmssrmss@gmail.com>
Cc: "Godfrey, Gary L." <godfrey@slac.stanford.edu>

Ray, Alan, and Robert,

Thank you Alan, Jeff, Anamaria, Brian, Robert, and Ray for your replies. As many of you pointed out, my attenuation estimate thru the vacuum pipe wall was not nearly large enough. I agree now that it is ~10^-7 for the SS pipe with no penetrations. My mistake was not including reflection of the EM wave from the metal pipe. I had only calculated the attenuation by skin depth of a vacuum pipe whose wall should also have been 3 times thicker. If the attenuation is 10^-7, then the B of the EM chirp required to give GW150914 would have been easily seen by the magnetometers. However, Ray and Robert say the attenuation might be much less because of penetrations (such as windows) in the vacuum pipe.

Robert says the baseline noise (in the low frequency region) inside a chamber has been measured to be ~ 1 x 10^-4 V/m-sqrt(Hz). He thinks the ambient noise outside the chamber is a few orders of magnitude higher (because outside fields are ~1 V/m) which would indicate effective shielding by the chamber. It is interesting that such an electric field noise near an end mirror with the estimated .3 nC of charge would cause a strain noise of 2 x 10^-24 /sqrt(Hz) . This is getting close to your measured baseline strain noise of ~8 x 10^-24 at 100 Hz, and I guess this is the reason you are keeping V_EffectiveBias<20 volts (which I think means .3 nC on the mirror).

So, I have included your updated numbers for pusher strip area (now gives a capacitance of 16 pf instead of 80 pf) and effective bias voltage (now using 20 volts instead of 200 volts). This gives a mirror charge of .3 nC instead of 16 nC. I have recalculated the EM chirp magnitude for NO ATTENUATION, that would give GW150914. The attached calculation shows it to be 80 pT which is below my estimated 142 pT veto threshold of the magnetometers. Wonderfully, Ray says there are 1 meter diam x 5000 turn coils which were being recorded at Hanford. I agree with his ~.001 pT/sqrt(Hz) thermal noise estimate for this coil. This is way below the 4 pT/sqrt(Hz) noise of the magnetometers. If the baseline noise really is thermal, then the coil, with a noise sigma of .001 x sqrt(350-35)=.02 pT should be able to definitively rule out a coincident 80 pT chirp. Of course, additional attenuation from the vacuum pipe would require a larger B chirp that would be even more easily vetoed.

Ray's ancillary argument rightly notices that the mirror charges times the E-fields would have to be miraculously the same at both the Livingston and Hanford sites in order to give the observed same amplitude strain signals. If the ELF chirp comes from something like a sprite causing a coincidence between the two sites, the sprite must be between the Livingston and Hanford sites since the 7 msec delay between the two events is less than the 10 msec speed of light between the two sites. Sprites occur at an altitude of 50-90 km above thunderstorms, and have been associated with a chorus of rising chirps (though at higher frequencies). An ELF radiation at 60 km altitude would be below the ionosphere (good, it doesn't get reflected out into space!). It is also not line of sight to either site and would require some bounces in the ionosphere-earth wave guide. This waveguide is~100 km high and the 100 Hz EM wave has a wavelength of 3000 km. Thus the waveguide is below cutoff, and the wave exponentially attenuates as it passes thru. Fortunately the sites are only 3000 km apart, so the wave only has to travel ~one wavelength. The wave can also take different numbers of bounces to get from the source to the detector. The waves from these different paths interfere and perhaps explain Ray's comment about there being a lot of dispersion which would make the chirps at both sites look different. Gamma rays (<~1 MeV) from the sprite would have passed thru the ionosphere and perhaps caused the coincident (within ~.4 sec) signal in the GBM on Fermi. Fermi has actually seen lots of TGFs (terrestrial gamma flashes) coming from the region of thunder storms. Schumann resonances, which are lightning excited standing waves in the earth-ionosphere cavity at 8, 14, 21, 27, 34 Hz, are not being proposed as the ELF chirp, because they don't look like a 50-150 Hz chirp. Perhaps there is a new or un-thought of old source of ELF chirps.

Robert said some huge lightning strikes were seen on the magnetometers but never in the gravitational wave channel. This would settle the question if 1) the lightning strikes were for aLIGO where there are electrostatic pusher plates charging up the mirrors, 2) the lighting was far enough away (~one wavelength =3000 km) to have generated a travelling wave with E=cB since E<<cB for near field, and 3) the lightning generated ~100 Hz rather than just higher frequencies for which the test mass sensitivity drops off (~freq^-2) and the shielding is more effective.

Alan and Brian correctly point out that the GW150914 event is highly unlikely to be a coincidence between two random data streams of chirps, because neither site sees a high enough singles rate of chirps. However, the hypothesis is that one ELF event makes the mirrors move at both sites. Few or no singles events are seen, but perhaps this is because the sites are too close together for any E field to drop off in amplitude (the sites are only 1 wavelength apart).

I expect the coil data, being sufficiently more sensitive than the magnetometers, will completely lay any ELF pulse acting on a charged mirror concerns to rest, and I am looking forward to what you find. Once again, thank you for educating me on the work you have done and the care you have taken.

Gary

From: robert schofield <rmssrmss@gmail.com>

Hi Gary,

Let me address your concerns from the point of view of one of the LIGO environmental coupling specialists. It is a pleasure communicating with someone who cares about some of the same issues that I do. And I really appreciate having an expert outsider who takes the time to seriously challenge us. I don't think that this service is provided nearly as often as would be ideal in science.

My biggest difference with your coupling estimate is that I think that you underestimate, by many orders of magnitude, the attenuation by the vacuum chamber walls. I think that the estimate does not account for the very high reflectivity of steel for E-fields at audio-band frequencies. I think that the fallacy in this omission is illustrated in the resulting prediction that the chamber walls would not attenuate the E-field at all as the frequency approaches 0 Hz, while Gauss's law would predict complete attenuation.
The following web page calculator estimates that the transmission at 50 Hz would be less than 1e-6, much less than the 0.5 that your estimate gives for the same frequency. http://www.clemson.edu/ces/cvel/emc/calculators/SE_Calculator/index.html
My understanding is that the attenuation for high-impedance near-field electric fields is even greater than for the

far-field plane waves of the estimate. It is the low-impedance near-field magnetic fields that penetrate the chamber, hence our focus on magnetic coupling.
Our EM attenuation is reduced by view ports, but even with the view ports, our in-band E-field attenuation is at least several orders of magnitude. For example, we have measured fields of 1e-4 V/m sqrt(Hz) inside of a chamber, while typical outside fields are above 1 V/m (we could never have reached our sensitivity if your attenuation calculations were correct). And a significant part of the internal fields may be produced inside of the chamber.

Even though we are confident of the large degree of attenuation afforded by our chambers, we also studied the coupling of EM fields from lightning strikes (you may find it interesting that I have the time/location/current data for nearly every cloud-to-ground and the larger of the cloud-to-cloud strikes in the US during our observation run on my laptop, as well as nearly all strikes globally for certain time periods). The fields from many lightning strikes during the run would have been much larger, 4 or 5 orders of magnitude larger, than your estimated threshold of visibility on the gravitational wave channel. Yet we did not see them in the gravitational wave channel (though some where huge on magnetometers).
And, to be triply sure, we monitor for the EM choruses that you propose as the source of the EM fields (even though choruses come in multiples, and are at higher frequencies then our binary black holes). We have our own RF scanner at LHO that was working for the boxing day event, and we consulted external low- and high-frequency RF observatories for all three vetted events.

Please contact me if you have any further questions on any of these details. There was actually significant internal discussion of these matters since there was a very large lightning strike over Burkina Faso that coincided with GW150914, our first detection.

Dr. Robert Schofield
Department of Physics
University of Oregon

-----Original Message-----
From: Rai Weiss [mailto:weiss@ligo.mit.edu]
Sent: Sunday, August 14, 2016 9:47 PM
To: Alan Weinstein
Cc: Godfrey, Gary L.
Subject: Re: Charged LIGO Mirrors and EM Chirps

Alan and Gary,
I am still in the middle of pulling together a response to the suggestion being made that we might be perturbed by sferics. I have worried about this for many years in the search for a stochastic background. That is the reason we have much more sensitive magnetometers installed at both sites in vaults a goodly distance from the buildings. These are 5000 turn coils
1 meter in diameter. Each site has three mutually orthoganal ones near one of the end stations. They are limited by thermal noise at about 10^-15
tesla/sqrt(Hz) at 100 Hz. Unfortunately these coil magnetometers were not connected at Livingston but were conneceted at Hanford. I have asked Anne Marie to help find the time series from these magnetometers at the moment of the first black hole merger. I have talked with people at Goddard Space Flight Center who have access to elf records to see if there is any excitations at the time of our detection in the world wide net. There was a significant thunderstorm in Africa within the hour of our detection.

The sferics which rattle around the earth in the toroidal cavity between the ionosphere and the earth's surface usually have a frequency derivative that is negative - opposite to the black hole merger. This is one of the pieces of information I would like to get for the event in the hour around our detection.

As to the actual mechanics of the test mass excitation. I think Gary has raised an interesting point with worrying about the mechanism being through the charge on the test mass. I have worried about our vulnerability due to

this bad design of the suspensions and this is the reason why we have made the plasma discharge system. I worry primarily because of the wiring that penetrates the stainless steel shell of the test mass tanks. The shell shields the electric field from an elf wave at
100 Hz by at least 10^-7 (ratio of field inside to outside), but it does not shield the magnetic field well. There is therefore time varying B field in the tank which through Faraday's law makes circular E fields in the tank. These at best would make test mass motions of 10^-27 m/sqrt(Hz) motions of the testmass with a charge of 10^-8 coulombs on the test mass.
So of no consequence. What does worry me however are the potentials and fields brought in by the not well shielded wiring. It is unlikely to provide enough force to cause our signal but as Gary points out our magnetic testing would not have shown this mechanism. That is why I would still like to know the actual elf excitations at the time of our discovery.

Even so there are some ancillary arguments which make it quite unlikely that elf is the source of our signal. The dominant argument is the almost equality of the waveforms at the two sites and their seperation in time by
7 milliseconds. The charge on the test masses would need to be close to the same at both sites and the elf wave dispersion in the toroidal cavity would need to be much smaller than what has been measured before. The source would need to have been a thunderstorm in a location somewhere south of Louisiana.

RW

On
Sun, 14 Aug 2016, Alan Weinstein wrote:

> Hi Gary,
> Nice to hear from you! And apologies for the delayed reply.
>
> As I gather you know (having chatted recently with Jeff Kissel), a
> great deal of effort and study has gone into understanding the origins, effects, and potential remedies for the charge buildup on our core optics.
>
> I am by no means an expert on the subject, and have not made any
> contributions to this effort, although I follow it fairly closely, and have learned about it through many discussions with the people who have been working on it in the last year or so.
>
> Although I entirely understand and appreciate your argument, the crux
> is in the numbers; and I have no expertise for judging the validity of your numerical estimates.
>
> So, since receiving your email, I have solicited input from a variety
> of experts within LIGO working on the problem. I will pass these on to
> you now, verbatim. Some argue on the basis of experimental measurements (our "environmental EM injections"). Others argue that you may be way off in your estimates of the attenuation by the vacuum chamber walls.
>
> I will then give my own perspective from the point of view of the data, without reference to your arguments about the EM couplings.
>
> ____
> You've already hear from Jeff Kissel at LHO, who claims that his
> response to you holds, and should be sufficient... One of his main
> arguments is that we have reduced, and changed the sign of, our charge buildup several times, and have seen no changes in overall behavior (other than the predicted significant change in the ESD actuation strength). Work on this is continuing.
>
> ____

> From Anamaria Effler at LLO:
>
> 1) We do measure the EM coupling at all test masses with ESDs
> functioning. If the chirp is magnetic and in band (ie not some
> downconversion effect) the magnetometers will see it. We need an SNR of 1000 or so in our injections (in the sensors) to see anything in DARM.
> 2) We do measure charge and discharge when possible. We have ion guns
> mounted on the chamber which allow us to not open the vacuum system when discharging. Worked well last time we did it for ETMX, for example, a couple weeks ago.
> 3) Most times the charge is the equivalent of a few tens of volts
> applied, but we have had a few hundred on ETMX, hence the latest discharge.
> 4) We do have radio stations in the corner tuned to our main RF
> frequencies in case of some radio downconversion. We injected signals
> at all RF frequencies we use, since they could downconvert theoretically and the only place we saw anything at all was at 9 and 45 MHz, hence the sensors settings.
>
> ____
> From Brian O'Reilly at LLO:
>
> One immediate problem observation is that the thickness of the BSC
> chamber walls is actually half an inch or ~0.0127 m, so Gary's number
> is off by a factor of ~3, which changes the calculation by about a factor of 10 in favour of us seeing the field in the magnetometers.
>
> Also the capacitance is probably too large by a factor of 4, but at
> least a factor of 2 (the ESD pattern partially covers an annular area
> on the test mass with an inner diameter of 226 mm and an outer diameter of 336 mm).
>
> At LLO we had one mirror which had a large charge on 2 quadrants of
> ETMX (~400 V) . ETMY at LLO was under 20V and both end test masses at
> LHO were similar to ETMY at LLO (by flipping the bias voltage sign on a weekly basis we found we could control charge build up).
>
> So the q=C.V term (charge on the mirror) is on average considerably
> smaller by at least a factor of 20 and probably more.
>
> In short I think it is quite reasonable to estimate the B field needed
> outside the chamber at > 3.34e-10 Tesla i.e. 2 orders of magnitude at
> least above Gary's value. That also puts it a factor of 2 or more above the veto threshold.
>
> Once you add in the coincidence requirement, the waveform morphology
> requirement, the fact that we don't see such events in single interferometer studies, I would conclude that we are in the clear.
>
> ____
> Like Brian, Robert Schofield at LHO believes that you are
> underestimating, by many orders of magnitude, the attenuation by the vacuum chamber walls. He says he will email you directly in the next days.
> ____
> My perspective is from the data; Brian refers to it briefly, above.
>
> - The chirp signals we see in our two strong events, GW150914 and
> GW151226, as well as the signal we see in the less strong but still
> convincing LVT151012, are coincident in time, amplitude, and phase
> evolution between the two detectors, separated by 300 km. This is very non-trivial. In fact, I sorta remember,

as a green graduate student, learning about the power of coincidence in experimental physics from you.
>
> - If this were an accidental coincidence, we would see tens of
> thousands more such events in one detector but not another. We see nothing like these events in single-detector data.
>
> - The phase evolution is very non-trivial, over (in the case of
> GW151226) tens of cycles. It agrees well with GR. Of course, we look
> specifically for these kinds of events, and we look over four
> parameters (two masses and two spins), but our search pipelines are sufficiently loose that if they didn't agree well, we'd still see them. If these were not of astrophysical origin, we'd see many more events that agree poorly with GR. We see nothing other than these three events.
>
> - And, I am convinced, by our experts, that our EM monitors outside
> our vacuum chambers would witness any environmental EM pulse with much
> greater sensitivity than anything that could be picked up by our
> in-vacuum mirrors. These are checked routinely and studied carefully
> by those of us working directly with the data. There is no evidence of EM pulses correlating with the strain data in our detectors, at least in advanced LIGO (there definitely was, in initial LIGO, especially at LLO, but never anything that looked like a chirp). Again, I expect that you will hear much more expert opinion on this from Robert Schofield in the coming days.
>
> ____
> Finally, I recall fondly your talent at taking your careful, clear,
> critical eye to many different technical problems when we were working
> together at SLAC. If, in your retirement, you feel inspired to spend
> some time at either of the two LIGO sites and contribute to our efforts to isolate our detectors from environmental influences, you have a standing invitation to do so! We have a Visitor's Program to support such efforts.
>
> Best,
>
> Alan
[Quoted text hidden]

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Alan Weinstein <ajw@ligo.caltech.edu> Tue, Aug 23, 2016 at 9:46 AM
Reply-To: detchar@sympa.ligo.org, Alan Weinstein <ajw@ligo.caltech.edu>
To: "detchar@ligo.org" <detchar@ligo.org>

The latest from Gary Godfrey, who remains very interested in charging and "EM chirps".
For context, see earlier emails following this one.

Alan

---------- Forwarded message ----------
From: **Godfrey, Gary L.** <godfrey@slac.stanford.edu>
[Quoted text hidden]

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