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

Reply to Gary with attached Sept. 14 vetting report on risers, choruses etc., and lightning report

8 messages

robert schofield <rmssrmss@gmail.com> Wed, Aug 31, 2016 at 11:51 AM
To: Alan Weinstein <ajw@ligo.caltech.edu>, Rainer Weiss <weiss@ligo.mit.edu>, godfrey@slac.stanford.edu

Hi Gary, Rai,

I want to point out that we do monitor external observatories for choruses and a wide range of other RF signals was a part of the vetting process for each of the three gravitational wave candidates. Below I attach the portion of the environmental vetting report for our first detection that covers external RF observatories. Rai, I think you may not have been aware of this part of the vetting process. Data from observatories that cover the hundreds to thousands of Hz band are near the end of the report and show no evidence of choruses, risers, or other coincident events.

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Let me know if you have any questions.

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2 attachments

logSept14EMenvironment2.pdf
7029K

AALightningReport.pdf
3575K

Godfrey, Gary L. <godfrey@slac.stanford.edu> Wed, Sep 7, 2016 at 1:28 PM
To: Rainer Weiss <weiss@ligo.mit.edu>, "robert schofield <rmssrmss@gmail.com> (rmssrmss@gmail.com)" <rmssrmss@gmail.com>
Cc: "Alan Weinstein (ajw@ligo.caltech.edu)" <ajw@ligo.caltech.edu>

September 6, 2016

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These steps will make the ETM less sensitive to EM pulses, and with a good monitor of the electric field definitively rule out an EM pulse as the cause of any future GW detection.

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E = (1 amp) (6.9 x 10^-7 ohm-m)/(2 x 3.14 x .5 m x .0054 m) = 4 x10^-5 volts/m

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gravitational wave channel during our recent run produced fields of at least hundreds of volts per meter." If Robert's expectation is true, that the amplitude of the E field is 100's volts/m at 100 Hz (rather than at higher frequencies characteristic of the usec long strike), then the vac pipe shielding is working and is reducing the E field to below the .1 V/m at 100 Hz that would cause a clearly detectable .5 x 10^-21 strain. Maybe though the 100's volts/m are closer to 1 MHz where both the ETM is less sensitive to being driven, the interferometer noise is greater, and the vac pipe is a better shield.

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that could directly produce the event in DARM. PEM injections show that, over the frequency band of this event, the EM signal would have to appear on magnetometer channels with an SNR of at least 100 in order to produce an SNR of 1 in DARM."

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"PEM injections show that, over the frequency band of this event, the EM signal would have to appear on magnetometer channels with an SNR of at least **10-4 (assuming no vac pipe EM shielding of the ETM)** in order to produce an SNR of 1 in DARM. Thus SNR of up to **10^4 (assuming no vac pipe EM shielding of the ETM)** in DARM would not be vetoed by the magnetometer."

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From: robert schofield [mailto:rmssrmss@gmail.com]
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Rai Weiss <weiss@ligo.mit.edu> Sat, Sep 10, 2016 at 1:33 PM
To: "Godfrey, Gary L." <godfrey@slac.stanford.edu>
Cc: "robert schofield <rmssrmss@gmail.com> (rmssrmss@gmail.com)" <rmssrmss@gmail.com>, "Alan Weinstein (ajw@ligo.caltech.edu)" <ajw@ligo.caltech.edu>

Gary,
All of your points are well taken. Some are even in the process of being done.

We screwed up big time by letting a hugh insulator be the test mass without a conductive coating over the surface. I have complained about this for many years as it provides a mechanism for hopping of charges over the surface which change the electrostatic forces on the mass in a random way and will at some level dominate the noise budget. Evidently at the moment the discontinuous motion of the charges on the surface do not dominate the noise since changing the bias field does not change the noise. Nor do we see a difference in the noise when the entire electrostatic drive system is turned off and the mass is controlled like a marionette from the magnets on the mass above the test mass in the quadruple suspension.

All this is not an excuse to plan for a conducting coating on the mass. The main reason we do not currently have one is because of the difficulty of maintaining coating absorption at 1 micron less than 1 ppm. The standard conducting coatings such as tin oxide even at densities of 100s of megohms/square have too much optical loss. Marty Fejer in the Applied Physics Department at Stanford has been working with us to develop a coating with low optical loss. He may have had success with nickel oxide coating which is an electronic rather than an ionic conductor. This is still in the works. People would be loath to coat the fused silica suspension fibers because of the possibility of increased thermal noise at the bending points of the fibers. The fused silica violin modes have Q of 10^7 which is a measure of the mechanical loss of the mirror suspension. We probably can get away without grounding a conducting coated test mass and discharge it with the thermal plasma technique that seems to work quite well. I am sure we will need a conducting coating on the mass when we try to improve the sensitivity of the detector. There is at the moment a mystery noise that is limiting the performance of the detector between 40 to 150 Hz which seems not to be due to the charging of the test mass. This has the commissioners and system planners interest. The conducting coating is not yet considered a top priority issue. It would also be a major hit on the suspension requiring a disassembly.

I have designed some field meters that should be able to measure 10^-8 volts/cm between 10 to 500Hz. The electrometer preamps need to be in the
vacuum system in encapsulated UHV containers. We have done this with the seismometers and photo detectors placed in the vacuum system. I think these will be installed between the O2 and O3 runs. Valera Frolov at the Livingston site has measured the difference in induced voltage on some insulatable components separated by 10's of cm and has measured upper limits on fields at 10^-6 volts/cm inside the chamber.

The suggestion of making the electrostatic drive differential so that there is no net field at the test mass is a good idea. I had discussed this with Rich Abbott a while ago. The trouble with this is again a major change. It would require more conducting strips on the recoil mass that faces the test mass and more wiring. It would become a priority if the commissioners found that it could reduce the noise. They are not worried about the linear offsets in the force due to the charge if kept in bounds by the thermal plasma neutralizer.

Finally, currents in the chamber walls at 60 Hz and multiples need to compete with the external 60 Hz and multiples much of which come from an unbalance in the three phase transmission line that crosses the site at Livingston. At Hanford ground loops may dominate. At this time we tolerate the 60Hz and multiples as we can still remove them in the data analysis. As the sensitivity improves there may be an impetus to fix these as well.

You will notice a certain hesitancy in making big changes in the instrument. There are obvious flaws in the design but it has to be demonstrated that any proposed change must cure a dominant noise problem or be

effective in increasing the instrument run duty cycle. If neither can be shown and it is just a guess, the idea gets a low priority. Given
the hugh group of people depending on the data, this is unfortunately quite sensible.

RW

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September 15, 2016

Rai,

I understand the collaboration's hesitancy to make any major changes that might screw up the instrument or result in downtime. It sounds like you have found a good way forward by installing an electrometer inside the vac chamber to measure the actual electric field that might be present. The absence of any coincident EM chirp inside the vac chamber would alleviate any concern that future GW detections were just due to a charged ETM.

Based on the calculation in my last email, it seems a little strange that turning off the ESD system didn't affect the interferometer noise. Perhaps the ESD was not off for a long enough time to allow the ETM to completely discharge before the noise measurement was made? Perhaps also the charged mirror noise is subdominant and won't show up until another larger noise source is eliminated.

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Q= C1*V1 + C2*V2 + C3*V3 + C4*V4 + C*V = 0
-V= (C1/C)*V1 + (C2/C)*V2 + (C3/C)*V3 + (C4/C)*V4

Assuming that the A and B strips in all the quadrants have the same areas this reduces to:

V= -1/4 *(V1+V2+V3+V4)

This average V can be made by a simple addition to electronics which is outside the vac chamber. For example, a new box with 5 coax connectors (4 voltage inputs and 1 voltage output) would contain 4 equal resistors summing into a node followed by a unity gain inverting amplifier. If all the strip capacitances aren't the same, tweek the values of the 4 resistors.
Unfortunately, this will result in a small coupling between the quadrants. When you push on one quadrant, all the others will respond slightly too. If these adjustments are all in a feedback system, the system may be stable and just converge despite the small couplings. Otherwise, you could diagonalize a matrix and find which voltages need be applied to just move one quadrant.

Good luck, and I look forward to many more of your wonderful GW detections in the future.

Gary

-----Original Message-----
From: Rai Weiss [mailto:weiss@ligo.mit.edu]
Sent: Saturday, September 10, 2016 1:34 PM
To: Godfrey, Gary L.
Cc: robert schofield <rmssrmss@gmail.com> (rmssrmss@gmail.com); Alan Weinstein (ajw@ligo.caltech.edu)
Subject: RE: Reply to Gary with attached Sept. 14 vetting report on risers, choruses etc., and lightning report

Gary,
All of your points are well taken. Some are even in the process of being done.

We screwed up big time by letting a hugh insulator be the test mass without a conductive coating over the surface. I have complained about this for many years as it provides a mechanism for hopping of charges over the surface which change the electrostatic forces on the mass in a random way and will at some level dominate the noise budget. Evidently at the moment the discontinuous motion of the charges on the surface do not dominate the noise since changing the bias field does not change the noise. Nor do we see a difference in the noise when the entire electrostatic drive system is turned off and the mass is controlled like a marionette from the magnets on the mass above the test mass in the quadruple suspension.

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The main reason we do not currently have one is because of the difficulty of maintaining coating absorption at 1 micron less than 1 ppm. The standard conducting coatings such as tin oxide even at densities of 100s of megohms/square have too much optical loss. Marty Fejer in the Applied Physics Department at Stanford has been working with us to develop a coating with low optical loss. He may have had success with nickel oxide coating which is an electronic rather than an ionic conductor. This is still in the works. People would be loath to coat the fused silica suspension fibers because of the possibility of increased thermal noise at the bending

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> September 6, 2016
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> As Rai says, LIGO's unknown noise might also be reduced by making the
> ETM charge zero. Robert said the baseline electric field noise (in
> the low frequency region) inside a chamber has been measured to be ~ 1
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> the estimated .3 nC of charge (Veffective-bias=20 volts on 16 pf
> ETM-pusher capacitance) 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 /sqrt(Hz) at 100 Hz.
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> Another source of electric field inside the vac chamber is current
> running on the inside surface of the vac chamber. This current could
> be ~amps and results from electricians connecting the 120 VAC white
> wire to the bus bar building ground (as well as a white wire back to
> the transformer) at each electric breaker panel. Current comes out
> the black wire from a local transformer, goes through an appliance,
> returns via the white wire to the breaker panel, and then takes many
> parallel paths (including the building and LIGO's vac chamber) back to
> the transformer. The majority of the vac pipe current will be at 60
> Hz, but higher frequency noise from motors, fluorescent lights, and
> switching power supplies will also be there. A quick calculation for
> 1 amp flowing in one skin depth (5.4 mm @ 60 Hz) of a 1 m diam SS
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> E = (1 amp) (6.9 x 10^-7 ohm-m)/(2 x 3.14 x .5 m x .0054 m) = 4 x10^-5
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> This might well be part of the big peak in strain you see at 60 Hz.
> Plotting the amplitude of the 60 Hz strain peak versus Veffective-bias would reveal how much of the 60 Hz peak is due to charge on the ETM.
>
>
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> On the Cold Dark Matter Search at Soudan (CDMS) we had a funny similar
> noise story. A copper sheathed room was built to shield the detectors
> (50 mK Ge with SQUID current readouts) from RF noise. All the AC for
> lights and scopes was run through a fancy filter (series inductors and
> capacitors to ground). However, lots of low freq noise was seen
> coming from the detectors that paradoxically went away when the fancy
> filter was bypassed. It turns out the filter was doing its job and shunting noise currents to the green ground wire. We initially thought these currents went thru the green wire back to the electric panel 50 m away. However, the electric code requires that the green wire be connected to building metal at every opportunity, and therefore the filtered noise currents were actually being sent directly into our room girders and cryostat.
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> The prior comments were for the future LIGO. Now, can one
> conclusively prove GW150914 (and its two weaker successors) was not an EM event with the monitoring that was available? Than you Robert for the series of good arguments in your email to me. My comments:
>
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>
> 1)   Robert said that self-inflicted noise was a problem for the
> 5000 turn coil. I think this may mean 60 Hz pickup is saturating the
> output. I calculate a 2 nT magnetic field from a long powerline that
> is perpendicularly 100 km away and carrying 1000 amps. If there are
> AC lines in the room with the coil, the field could easily be ~100 nT. For both the coil and the new LEMI-120 the gain has to such that the AC pickup does not rail the electronics, and there have to be enough ADC channels to have <1 pT/chan. It would be unfortunate if the setup of the 5000 turn coil made it unable to use its sensitivity to rule out an EM chirp as the cause of GW150914.
>
>
>
> 2)   Robert asks: “If fields from geomagnetic events could get in
> via sneaky routes like the cable routes, why wouldn’t the much larger
> locally generated electric and magnetic fields also get in?” An

> answer is that the locally generated B fields may get in, but there is
> very little associated E field to move the charged mirror. Consider the E from the f=100 Hz B field at the center between some larger radius Helmholtz coils. The integral of E around an r=1 m radius circle at the center is the time derivative of the B flux thru the circle. Assuming approx. uniform B gives:
>
> 2 * pi * r * E = 2 * pi * f * pi * r^2 * B
>
> E = f * pi* r * B
>
> E = 300 * B
>
> For an EM wave (ie: far field from the source) the E field is 10^6
> times larger for the same B
>
> E = 3 x 10^8 * B
>
> When you measured the ETM movement due to a magnetic field, the
> Helmholtz coil B was probably in the arm direction (since you were
> thinking of the interaction with the magnets on the upper stages). The miniscule E field from this near field B would be perpendicular to the arm, even further reducing its ability to swing the charged ETM along the arm.
>
>
>
> 3) Robert says " I disagree that the electric fields from nearby
> (lightning) strikes are not large enough to test the system, we expect
> that some of the close strikes that did not effect the gravitational
> wave channel during our recent run produced fields of at least
> hundreds of volts per meter." If Robert's expectation is true, that
> the amplitude of the E field is 100's volts/m at 100 Hz (rather than at higher frequencies characteristic of the usec long strike), then the vac pipe shielding is working and is reducing the E field to below the .1 V/m at 100 Hz that would cause a clearly detectable .5 x 10^-21 strain. Maybe though the 100's volts/m are closer to 1 MHz where both the ETM is less sensitive to being driven, the interferometer noise is greater, and the vac pipe is a better shield.
>
>
>
> 4) The Sept14EnvironmentLog says:
>
> "Our primary defense against inter-site coincidences that might be produced by long-range electromagnetic signals are the PEM sensors.
> The magnetometers would detect any EM signal
>
> that could directly produce the event in DARM. PEM injections show
> that, over the frequency band of this event, the EM signal would have to appear on magnetometer channels with an SNR of at least 100 in order to produce an SNR of 1 in DARM."
>
>
>
> As I attempted to show in (2) above, because the PEM injections used
> near field B the E was 10^6 times smaller than in a travelling EM
> wave. Thus the Log Statement should read
>
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>
> "PEM injections show that, over the frequency band of this event, the

> EM signal would have to appear on magnetometer channels with an SNR of
> at least 10-4 (assuming no vac pipe EM shielding of the ETM) in order to produce an SNR of 1 in DARM. Thus SNR of up to 10^4 (assuming no vac pipe EM shielding of the ETM) in DARM would not be vetoed by the magnetometer."
>
>
>
> 5) The AALightning report was very thorough. The various
> atmospheric and solar processes, that were thought of and monitored,
> did not cause GW150914. At the least, none have the right evolution
> in time and frequency. Only the chorus makes rising chirps, but they
> come in groups and are at higher frequency. Perhaps for GW150914,
> without a direct measure of the electric field near the charged ETM, the circumstantial case is the best that can be done. The case is thorough, but maybe there is some generator of chirping E field (clearly rare) that LIGO, a very new, different, and highly sensitive instrument, has discovered for the first time? For future events, reducing the charge on the ETM and including E field measurement near the ETM within the vac chamber, will eliminate any remaining doubt.
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> Gary
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> From: robert schofield [mailto:rmssrmss@gmail.com]
> Sent: Wednesday, August 31, 2016 11:51 AM
> To: Alan Weinstein; Rainer Weiss; Godfrey, Gary L.
> Subject: Reply to Gary with attached Sept. 14 vetting report on
> risers, choruses etc., and lightning report
>
>
>
> Hi Gary, Rai,
>
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>
> I want to point out that we do monitor external observatories for
> choruses and a wide range of other RF signals was a part of the
> vetting process for each of the three gravitational wave candidates.
> Below I attach the portion of the environmental vetting report for our
> first detection that covers external RF observatories. Rai, I think you may not have been aware of this part of the vetting process. Data from observatories that cover the hundreds to thousands of Hz band are near the end of the report and show no evidence of choruses, risers, or other coincident events.
>
>
>
> I also want to point out that self-inflicted noise made the coil
> magnetometers that Rai mentioned, considerably less sensitive than
> their design. We are currently replacing them with commercial

> magnetometers, Lemi-120s, that have a noise floor of 0.01 pT at 100
> Hz. This is mainly for subtraction of the effects of coherent inter-site magnetic fields, such as associated with Schumann resonances, for the stochastic gravitational wave search. The reason for this is that the stochastic gravitational wave background search uses inter-site correlations to dig down below the local uncorrelated noise.
>
>
>
> The necessity of placing detectors for these global EM events at quiet
> sites is one birds- eye or detail-free argument for why it doesn't
> make sense to consider them a likely source of short transient events
> on our gravitational wave channel (even though we check for them
> anyway): the uncorrelated locally generated fields at our facilities
> tend to swamp the fields from these events and yet these local fields
> do not limit our sensitivity. If fields from geomagnetic events could
> get in via sneaky routes like the cable routes, why wouldn't the much larger locally generated electric and magnetic fields also get in? Even so, we make sure this is true by injecting even larger fields than our backgrounds.
>
>
>
> I also attach the lightning report associated with our first detection
> (this does not cover risers or choruses since they are not produced by
> lightning, unlike whistlers). Hopefully it will answer your questions
> about lightning coupling. I disagree that the electric fields from
> nearby strikes are not large enough to test the system, we expect that
> some of the close strikes that did not effect the gravitational wave channel during our recent run produced fields of at least hundreds of volts per meter. Thunder is more likely to show up on the gravitational wave channel than the lightning that produces it. Also, you will notice that we can detect and veto events with maximum amplitudes considerably lower than your estimates Gary, using time-frequency techniques.
>
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>
> The attached EM environment report and the lightning report were attachments to the main environmental vetting report in the event log.
>
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>
> Let me know if you have any questions.
>
>
>
> Robert
>
>
>

Rai Weiss <weiss@ligo.mit.edu> Thu, Sep 15, 2016 at 6:55 PM
To: "Godfrey, Gary L." <godfrey@slac.stanford.edu>
Cc: "robert schofield <rmssrmss@gmail.com> (rmssrmss@gmail.com)" <rmssrmss@gmail.com>, "Alan Weinstein (ajw@ligo.caltech.edu)" <ajw@ligo.caltech.edu>

Gary,
Thank you for your note. Our experience is that the charge on the mass is not uniformly distributed. Usually one quadrant excitation causes more linear electrostatic force than another. In fact one can find the centroid of the

charge distribution by driving all the quadrants and looking at the yaw and pitch of the test mass. To reduce the linear force on the test mass we would need to make each quadrant a differential driver. As you point out one can reduce the linear term somewhat by the trick you propose. In any case if there is charge hopping on the test mass we will get random noise from the hopping due to field lines that leave the charge and go the other surfaces around the test mass such as the heater ring and structure. We really need to get rid of the charge.
RW

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> September 15, 2016
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This might well be part of the big peak in strain you see at 60 Hz.
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On the Cold Dark Matter Search at Soudan (CDMS) we had a funny similar noise story. A copper sheathed room was built to shield the detectors (50 mK Ge with SQUID current readouts) from RF noise. All the AC for lights and scopes was run through a fancy filter (series inductors and capacitors to ground). However, lots of low freq noise was seen coming from the detectors that paradoxically went away when the fancy filter was bypassed. It turns out the filter was doing its job and shunting noise currents to the green ground wire. We initially thought these currents went thru the green wire back to the electric panel 50 m away. However, the electric code requires that the green wire be connected to building metal at every opportunity, and therefore the filtered noise currents were actually being sent directly into our room girders and cryostat.

The prior comments were for the future LIGO. Now, can one
conclusively prove GW150914 (and its two weaker successors) was not an EM event with the monitoring that was available? Than you Robert for the series of good arguments in your email to me. My comments:

1) Robert said that self-inflicted noise was a problem for the
5000 turn coil. I think this may mean 60 Hz pickup is saturating the
output. I calculate a 2 nT magnetic field from a long powerline that
is perpendicularly 100 km away and carrying 1000 amps. If there are
AC lines in the room with the coil, the field could easily be ~100 nT. For both the coil and the new LEMI-120 the gain has to such that the AC pickup does not rail the electronics, and there have to be enough ADC channels to have <1 pT/chan. It would be unfortunate if the setup of the 5000 turn coil made it unable to use its sensitivity to rule out an EM chirp as the cause of GW150914.

2) Robert asks: "If fields from geomagnetic events could get in
via sneaky routes like the cable routes, why wouldn't the much larger
locally generated electric and magnetic fields also get in?" An
answer is that the locally generated B fields may get in, but there is
very little associated E field to move the charged mirror. Consider the E from the f=100 Hz B field at the center between some larger radius Helmholtz coils. The integral of E around an r=1 m radius circle at the center is the time derivative of the B flux thru the circle. Assuming approx. uniform B gives:

2 * pi * r * E = 2 * pi * f * pi * r^2 * B

E = f * pi* r * B

E = 300 * B

For an EM wave (ie: far field from the source) the E field is 10^6
times larger for the same B

E = 3 x 10^8 * B

When you measured the ETM movement due to a magnetic field, the
Helmholtz coil B was probably in the arm direction (since you were
thinking of the interaction with the magnets on the upper stages). The miniscule E field from this near field B would be perpendicular to the arm, even further reducing its ability to swing the charged ETM along the arm.

3) Robert says " I disagree that the electric fields from nearby
(lightning) strikes are not large enough to test the system, we expect
that some of the close strikes that did not effect the gravitational
wave channel during our recent run produced fields of at least
hundreds of volts per meter." If Robert's expectation is true, that
the amplitude of the E field is 100's volts/m at 100 Hz (rather than at higher frequencies characteristic of the

usec long strike), then the vac pipe shielding is working and is reducing the E field to below the .1 V/m at 100 Hz that would cause a clearly detectable .5 x 10^-21 strain. Maybe though the 100's volts/m are closer to 1 MHz where both the ETM is less sensitive to being driven, the interferometer noise is greater, and the vac pipe is a better shield.

4) The Sept14EnvironmentLog says:

"Our primary defense against inter-site coincidences that might be produced by long-range electromagnetic signals are the PEM sensors.
The magnetometers would detect any EM signal

that could directly produce the event in DARM. PEM injections show
that, over the frequency band of this event, the EM signal would have to appear on magnetometer channels with an SNR of at least 100 in order to produce an SNR of 1 in DARM."

As I attempted to show in (2) above, because the PEM injections used
near field B the E was 10^6 times smaller than in a travelling EM
wave. Thus the Log Statement should read

"PEM injections show that, over the frequency band of this event, the
EM signal would have to appear on magnetometer channels with an SNR of
at least 10-4 (assuming no vac pipe EM shielding of the ETM) in order to produce an SNR of 1 in DARM.
Thus SNR of up to 10^4 (assuming no vac pipe EM shielding of the ETM) in DARM would not be vetoed by the magnetometer."

5) The AALightning report was very thorough. The various
atmospheric and solar processes, that were thought of and monitored,
did not cause GW150914. At the least, none have the right evolution
in time and frequency. Only the chorus makes rising chirps, but they
come in groups and are at higher frequency. Perhaps for GW150914,
without a direct measure of the electric field near the charged ETM, the circumstantial case is the best that can be done. The case is thorough, but maybe there is some generator of chirping E field (clearly rare) that LIGO, a very new, different, and highly sensitive instrument, has discovered for the first time? For future events, reducing the charge on the ETM and including E field measurement near the ETM within the vac chamber, will eliminate any remaining doubt.

Gary

From: robert schofield [mailto:rmssrmss@gmail.com]
Sent: Wednesday, August 31, 2016 11:51 AM
To: Alan Weinstein; Rainer Weiss; Godfrey, Gary L.
Subject: Reply to Gary with attached Sept. 14 vetting report on risers, choruses etc., and lightning report

Hi Gary, Rai,

I want to point out that we do monitor external observatories for choruses and a wide range of other RF signals was a part of the vetting process for each of the three gravitational wave candidates. Below I attach the portion of the environmental vetting report for our first detection that covers external RF observatories. Rai, I think you may not have been aware of this part of the vetting process. Data from observatories that cover the hundreds to thousands of Hz band are near the end of the report and show no evidence of choruses, risers, or other coincident events.

I also want to point out that self-inflicted noise made the coil magnetometers that Rai mentioned, considerably less sensitive than their design. We are currently replacing them with commercial magnetometers, Lemi-120s, that have a noise floor of 0.01 pT at 100 Hz. This is mainly for subtraction of the effects of coherent inter-site magnetic fields, such as associated with Schumann resonances, for the stochastic gravitational wave search. The reason for this is that the stochastic gravitational wave background search uses inter-site correlations to dig down below the local uncorrelated noise.

The necessity of placing detectors for these global EM events at quiet sites is one birds- eye or detail-free argument for why it doesn't make sense to consider them a likely source of short transient events on our gravitational wave channel (even though we check for them anyway): the uncorrelated locally generated fields at our facilities tend to swamp the fields from these events and yet these local fields do not limit our sensitivity. If fields from geomagnetic events could get in via sneaky routes like the cable routes, why wouldn't the much larger locally generated electric and magnetic fields also get in? Even so, we make sure this is true by injecting even larger fields than our backgrounds.

I also attach the lightning report associated with our first detection (this does not cover risers or choruses since they are not produced by lightning, unlike whistlers). Hopefully it will answer your questions about lightning coupling. I disagree that the electric fields from nearby strikes are not large enough to test the system, we expect that some of the close strikes that did not effect the gravitational wave channel during our recent run produced fields of at least hundreds of volts per meter. Thunder is more likely to show up on the gravitational wave channel than the lightning that produces it. Also, you will notice that we can detect and veto events with

> maximum amplitudes considerably lower than your estimates Gary, using time-frequency techniques.
>
> The attached EM environment report and the lightning report were attachments to the main environmental vetting report in the event log.
>
> Let me know if you have any questions.
>
> Robert

---

robert schofield <rmssrmss@gmail.com> Sun, Sep 18, 2016 at 8:07 PM
To: Rai Weiss <weiss@ligo.mit.edu>
Cc: "Godfrey, Gary L." <godfrey@slac.stanford.edu>, "Alan Weinstein (ajw@ligo.caltech.edu)" <ajw@ligo.caltech.edu>

Hi Gary,

Sorry about the long delay in answering the second part of your previous response - lots of things to do as we are approaching our second run. However, I do really appreciate having this discussion with you. I'll second Rai's response to the first part of your original response and address the second part, relative to event vetting. Comments, in italics, follow your numbered responses below:

> 1) Robert said that self-inflicted noise was a problem for the 5000 turn coil. I think this may mean 60 Hz pickup is saturating the output. I calculate a 2 nT magnetic field from a long powerline that is perpendicularly 100 km away and carrying 1000 amps. If there are AC lines in the room with the coil, the field could easily be ~100 nT. For both the coil and the new LEMI-120 the gain has to such that the AC pickup does not rail the electronics, and there have to be enough ADC channels to have <1 pT/chan. It would be unfortunate if the setup of the 5000 turn coil made it unable to use its sensitivity to rule out an EM chirp as the cause of GW150914.

The problem with the magnetometer system was not saturation, it was that currents in the signal acquisition and processing electronics produce magnetic fields that were locally larger than the geomagnetic fields. As an example, magnetic fields from disc access (which is magnetically actuated) in a data logger once dominated the non-60 Hz signal from that magnetometer. We have tested the LEMI setup and it does not have these self-inflicted problems. However, this magnetometer is not needed for simply detecting correlated fields since any fields that are observable at our sites, 3000 km apart, should also be observed at external observatories that are often more sensitive than our noisy sites could be. We want the magnetometers for precisely following the correlated fields for subtracting their effects from our gravitational wave channel.

The difficulty of detecting the fields you are worried about illustrates the point that locally generated electric and magnetic fields swamp the signals that you are worried about and would show clearly in our spectrum and limit our sensitivity if the coupling were within orders of magnitude of your attenuation-free estimate.

The argument that local fields swamp the geomagnetic fields, along with comparisons of internal and external fields, lightning studies, and electric field injections at chamber ports, is confirmation that there is no justification for assuming that the electric fields are six orders of magnitude greater than what would be predicted from the simple attenuation model for a chamber.

And this chamber model leaves out several more orders of magnitude of attenuation: the chambers and all associated equipment are located inside Faraday cages, grounded steel-skinned buildings. Especially considering this additional ~10e6 E-field attenuation, the magnetometers are much better at detecting far-field ELF EM radiation inside the buildings than an electric field sensor would be.

As an aside, we don't have a saturation problem, even though we do have a ~700 amp transmission line that roughly parallels the Washington site at closest distance of 2km. This transmission line produces about half of our 60 Hz magnetic peak in the buildings, but that is only a few nT. I'm not sure why your estimate is so high, my best guess is that you are not considering the partial cancelation due to the return current: an estimate using the typical spacing of 20m or so between out and back currents in these transmission lines yields the measured values. In the buildings we try to twist the power lines or at least make sure that out and back are close and so we don't really have fields much over 10 nT.

2) Robert asks: "If fields from geomagnetic events could get in via sneaky routes like the cable routes, why wouldn't the much larger locally generated electric and magnetic fields also get in?" An answer is that the locally generated B fields may get in, but there is very little associated E field to move the charged mirror. Consider the E from the f=100 Hz B field at the center between some larger radius Helmholtz coils. The integral of E around an r=1 m radius circle at the center is the time derivative of the B flux thru the circle. Assuming approx. uniform B gives:

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When you measured the ETM movement due to a magnetic field, the Helmholtz coil B was probably in the arm direction (since you were thinking of the interaction with the magnets on the upper stages). The miniscule E field from this near field B would be perpendicular to the arm, even further reducing its ability to swing the charged ETM along the arm.

I think you may have missed my point here. I was not referring to magnetic field injections. I was referring to the electric field background, which is much larger than the electric fields from far field geomagnetic events. The question I was asking is why would these tiny geomagnetic electric fields get in when the large local electric fields do not?

Nevertheless, to be needlessly redundant, less than a month after our first detection, I did check coupling for electric fields. I wanted to be absolutely sure about attenuation of "fringing" fields and so I injected electric fields at what I thought would be the most sensitive port of a test mass chamber. The glass port was covered with a large plate with a p-p oscillation of 22 V, relative to the chamber and ground, at the mid-frequency of the GW detection from weeks before. The response in the gravitational wave channel was only 1.5 e-21 m (it took many hours of integration to see it). This test, by itself, shows that your mechanism is not a concern.

3) Robert says " I disagree that the electric fields from nearby (lightning) strikes are not large enough to test the system, we expect that some of the close strikes that did not effect the gravitational wave channel during our recent run produced fields of at least hundreds of volts per meter." If Robert's expectation is true, that the amplitude of the E field is 100's volts/m at 100 Hz (rather than at higher frequencies characteristic of the usec long strike), then the vac pipe shielding is working and is reducing the E field to below the .1 V/m at 100 Hz that would cause a clearly detectable .5 x 10^-21 strain. Maybe though the 100's volts/m are closer to 1 MHz where both the ETM is less sensitive to being driven, the interferometer noise is greater, and the vac pipe is a better shield.

The peak E field from lightning strikes is in the thousand of Hz not MHz, and is not much lower at 100 Hz (I think that the characteristic time that dominates is the milliseconds for the electric field development time between the multiple strokes in the lightning strike, not the microseconds of the actual current flow and field reversal in each of the constituent strokes). This peak E-field frequency is found in several studies that can be Googled. The strikes discussed in the lightning report are indeed a good confirmation of the large attenuation that we expect for the fields you are concerned with, and I think that the lightning strike data set, by itself, eliminates your concerns.

4) The Sept14EnvironmentLog says:

"Our primary defense against inter-site coincidences that might be produced by long-range electromagnetic signals are the PEM sensors. The magnetometers would detect any EM signal

that could directly produce the event in DARM. PEM injections show that, over the frequency band of this event, the EM signal would have to appear on magnetometer channels with an SNR of at least 100 in order to produce an SNR of 1 in DARM."

As I attempted to show in (2) above, because the PEM injections used near field B the E was 10^6 times smaller than in a travelling EM wave. Thus the Log Statement should read

"PEM injections show that, over the frequency band of this event, the EM signal would have to appear on magnetometer channels with an SNR of at least **10-4 (assuming no vac pipe EM shielding of the ETM)** in order to produce an SNR of 1 in DARM. Thus SNR of up to **10^4 (assuming no vac pipe EM shielding of the ETM)** in DARM would not be vetoed by the magnetometer."

This argument is based on the assumption that the grounded steel-sheathed building and the vacuum chamber inside provide no E-field attenuation. With a simple model of attenuation, at 100 Hz, the ratio of fields from far-field EM seen inside the buildings would be roughly six orders of magnitude smaller than your figure (the attenuation of the magnetic fields by the building is only a factor of a few, while the E-field attenuation is more than 6 orders of magnitude):

*E ~= 3 x (10^8/ 1 x10^6) * B ~= 3 x 10 ^2 B,*

and, as seen by the test mass, something like 12 orders of magnitude smaller. It makes much more sense to try and detect far field ELF EM inside the buildings using magnetometers rather than electric field sensors. If we were trying to detect far field ELF EM inside the chamber, it would make much more sense to put a magnetometer in the chamber than an E-field sensor.

E-field injections from lightning strikes, our large-plate E-field injections and simple comparisons of internal and external fields, confirm that there are many orders of magnitude of attenuation.

As an aside, I want to remind you that imperfect conducting shells are relied on and work well for van de Graaff generators and accelerators, and people in fire lookouts and airplanes stake their lives on imperfect conducting shells during direct lightning strikes. The imperfections do not eliminate the attenuation and there is no justification for assuming no attenuation even in a worst-case scenario.

> 5) The AALightning report was very thorough. The various atmospheric and solar processes, that were thought of and monitored, did not cause GW150914. At the least, none have the right evolution in time and frequency. Only the chorus makes rising chirps, but they come in groups and are at higher frequency. Perhaps for GW150914, without a direct measure of the electric field near the charged ETM, the circumstantial case is the best that can be done. The case is thorough, but maybe there is some generator of chirping E field (clearly rare) that LIGO, a very new, different, and highly sensitive instrument, has discovered for the first time? For future events, reducing the charge on the ETM and including E field measurement near the ETM within the vac chamber, will eliminate any remaining doubt.

Ill agree that we can not reduce the possibility of false detections to zero (even with an in-chamber electric field sensor!); what we try to do is to make sure that the probability of false detections from known and unknown couplings is below the false detection probabilities given in the paper. I am convinced that test mass motion from your suggested coupling mechanism is more than eight orders of magnitude smaller than your calculation and thus the probability of a false detection from this mechanism is vanishingly small. I think the false detection probabilities are much higher for other mechanisms, and, with limited resources, these are the mechanisms that we should focus on. For example, a much bigger concern for me is the possibility of inter-site correlations through the GPS-linked hardware and software of our control and data acquisition systems.

That being said, we have been discussing in-chamber E-field monitoring for some time in order to study and reduce self-inflicted noise, but I do not think it is a top priority for our second run.

Robert

On Thu, Sep 15, 2016 at 6:55 PM, Rai Weiss <weiss@ligo.mit.edu> wrote:

> Gary,
> Thank you for your note. Our experience is that the charge on the mass is not uniformly distributed. Usually one quadrant excitation causes more linear electrostatic force than another. In fact one can find the centroid of the charge distribution by driving all the quadrants and looking at the yaw and pitch of the test mass. To

reduce the linear force on the test mass we would need to make each quadrant a differential driver. As you point out one can reduce the linear term somewhat by the trick you propose. In any case if there is charge hopping on the test mass we will get random noise from the hopping due to field lines that leave the charge and go the other surfaces around the test mass such as the heater ring and structure. We really need to get rid of the charge.
RW

On Thu, 15 Sep 2016, Godfrey, Gary L. wrote:

September 15, 2016

Rai,

I understand the collaboration's hesitancy to make any major changes that might screw up the instrument or result in downtime. It sounds like you have found a good way forward by installing an electrometer inside the vac chamber to measure the actual electric field that might be present. The absence of any coincident EM chirp inside the vac chamber would alleviate any concern that future GW detections were just due to a charged ETM.

Based on the calculation in my last email, it seems a little strange that turning off the ESD system didn't affect the interferometer noise. Perhaps the ESD was not off for a long enough time to allow the ETM to completely discharge before the noise measurement was made? Perhaps also the charged mirror noise is subdominant and won't show up until another larger noise source is eliminated.

I looked at some LIGO design dwgs (hopefully the most recent), and I saw that a flex with 5 wires comes from the traces on the pusher to 5 coax connectors that penetrate the vac pipe. Four wires are from the A fingers in each of the four quadrants. One wire is from the interlaced B fingers which are all connected together. As you said, this precludes the use of separate bipolar drive of each quadrant. However, without changing the wiring, I believe a trick can be played that will make the total charge zero on the pusher plate (and therefore zero on the ETM side of the capacitor too). Let V1, V2, V3, V4 be the voltages (wrt vac pipe ground) on the A strips, V be the voltage on the B strips. Each set of strips has some capacitance (C1, C2, C3, C4, C) across the d=5 mm gap to the ETM. The total charge, which we set equal to zero, on the pusher plate is:

Q= C1*V1 + C2*V2 + C3*V3 + C4*V4 + C*V = 0
-V= (C1/C)*V1 + (C2/C)*V2 + (C3/C)*V3 + (C4/C)*V4

Assuming that the A and B strips in all the quadrants have the same areas this reduces to:

V= -1/4 *(V1+V2+V3+V4)

This average V can be made by a simple addition to electronics which is outside the vac chamber. For example, a new box with 5 coax connectors (4 voltage inputs and 1 voltage output) would contain 4 equal resistors summing into a node followed by a unity gain inverting amplifier. If all the strip capacitances aren't the same, tweek the values of the 4 resistors.
Unfortunately, this will result in a small coupling between the quadrants. When you push on one quadrant, all the others will respond slightly too. If these adjustments are all in a feedback system, the system may be stable and just converge despite the small couplings. Otherwise, you could diagonalize a matrix and find which voltages need be applied to just move one quadrant.

Good luck, and I look forward to many more of your wonderful GW detections in the future.

Gary

-----Original Message-----
From: Rai Weiss [mailto:weiss@ligo.mit.edu]
Sent: Saturday, September 10, 2016 1:34 PM
To: Godfrey, Gary L.
Cc: robert schofield <rmssrmss@gmail.com> (rmssrmss@gmail.com); Alan Weinstein (ajw@ligo.caltech.edu)
Subject: RE: Reply to Gary with attached Sept. 14 vetting report on risers, choruses etc., and lightning report

Gary,
All of your points are well taken. Some are even in the process of being done.

We screwed up big time by letting a hugh insulator be the test mass without a conductive coating over the surface. I have complained about this for many years as it provides a mechanism for hopping of charges over the surface which change the electrostatic forces on the mass in a random way and will at some level dominate the noise budget. Evidently at the moment the discontinuous motion of the charges on the surface do not dominate the noise since changing the bias field does not change the noise. Nor do we see a difference in the noise when the entire electrostatic drive system is turned off and the mass is controlled like a marionette from the magnets on the mass above the test mass in the quadruple suspension.

All this is not an excuse to plan for a conducting coating on the mass.
The main reason we do not currently have one is because of the difficulty of maintaining coating absorption at 1 micron less than 1 ppm. The standard conducting coatings such as tin oxide even at densities of 100s of megohms/square have too much optical loss. Marty Fejer in the Applied Physics Department at Stanford has been working with us to develop a coating with low optical loss. He may have had success with nickel oxide coating which is an electronic rather than an ionic conductor. This is still in the works. People would be loath to coat the fused silica suspension fibers because of the possibility of increased thermal noise at the bending points of the fibers. The fused silica violin modes have Q of
10^7 which is a measure of the mechanical loss of the mirror suspension.
We probably can get away without grounding a conducting coated test mass and discharge it with the thermal plasma technique that seems to work quite well. I am sure we will need a conducting coating on the mass when we try to improve the sensitivity of the detector. There is at the moment a mystery noise that is limiting the performance of the detector between
40 to 150 Hz which seems not to be due to the charging of the test mass.
This has the commissioners and system planners interest. The conducting coating is not yet considered a top priority issue. It would also be a major hit on the suspension requiring a disassembly.

I have designed some field meters that should be able to measure 10^-8 volts/cm between 10 to 500Hz. The electrometer preamps need to be in the vacuum system in encapsulated UHV containers. We have done this with the seismometers and photo detectors placed in the vacuum system. I think these will be installed between the O2 and O3 runs. Valera Frolov at the Livingston site has measured the difference in induced voltage on some insulatable components separated by 10's of cm and has measured upper limits on fields at 10^-6 volts/cm inside the chamber.

The suggestion of making the electrostatic drive differential so that there is no net field at the test mass is a good idea. I had discussed this with Rich Abbott a while ago. The trouble with this is again a major change. It would require more conducting strips on the recoil mass that faces the test mass and more wiring. It would become a priority if the commissioners found that it could reduce the noise. They are not worried about the linear offsets in the force due to the charge if kept in bounds by the thermal plasma neutralizer.

Finally, currents in the chamber walls at 60 Hz and multiples need to compete with the external 60 Hz and multiples much of which come from an unbalance in the three phase transmission line that crosses the site at Livingston. At Hanford ground loops may dominate. At this time we tolerate the 60Hz and multiples as we

can still remove them in the data analysis.
As the sensitivity improves there may be an impetus to fix these as well.

You will notice a certain hesitancy in making big changes in the instrument. There are obvious flaws in the design but it has to be demonstrated that any proposed change must cure a dominant noise problem or be effective in increasing the instrument run duty cycle. If neither can be shown and it is just a guess, the idea gets a low priority. Given the hugh group of people depending on the data, this is unfortunately quite sensible.

RW

On Wed, 7 Sep 2016, Godfrey, Gary L. wrote:

> September 6, 2016
>
> Hi Rai and Robert,
>
> Congratulations Rai on receiving the Kavli Prize! It was certainly well deserved.
>
> Yes, your idea of continuous measurement of the electric field inside
> the vacuum chamber is a good one. In order to eliminate the effects of charged mirrors in the future I would prioritize:
>
> 1) Make the average voltage zero on the pusher mass. For the
> interlaced traces, one set should go to –V when the other set goes to
> +V (certainly not 0 and 400 volts like now). To be more exact, if the
> +sets of strips don't cover exactly the same area, Area1*V1 +
> Area2*V2 = 0. Since the areas are proportional to the capacitances
> between the strips and the ETM, this is just saying C1*V1+C2*V2=Q =0 to make the charge=0 on the pusher mass // ETM capacitor. This would seem to be just changing the drive electronics to bipolar outputs.
> The only remaining voltage on the capacitor would now be ~1 volt due
> to contact potentials, much less than the Veffective-bias ~20 volts that you get now by occasionally flipping the drive polarity.
>
> 2) Put a sensor inside the vacuum pipe to measure electric field,
> in the arm direction, and near as practical to the mirror mass. Perhaps this would just be a dipole antenna that of course would be much shorter than the wavelengths being looked at.
>
> 3) Put a conductive coating on the support fibers and on the
> non-mirror surfaces on the ETM. This will connect the ETM surfaces to
> the vacuum pipe ground. However, it's not a good idea to do this

before completing step 1, since it will speed up (compared to vac ion currents) the charging of the ETM surface to whatever opposite charge is on the pusher plate.

These steps will make the ETM less sensitive to EM pulses, and with a good monitor of the electric field definitively rule out an EM pulse as the cause of any future GW detection.

As Rai says, LIGO's unknown noise might also be reduced by making the ETM charge zero. Robert said the baseline electric field noise (in the low frequency region) inside a chamber has been measured to be ~ 1 x 10^-4 V/m-sqrt(Hz). Such an electric field noise near an ETM with the estimated .3 nC of charge (Veffective-bias=20 volts on 16 pf ETM-pusher capacitance) 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 /sqrt(Hz) at 100 Hz.

Another source of electric field inside the vac chamber is current running on the inside surface of the vac chamber. This current could be ~amps and results from electricians connecting the 120 VAC white wire to the bus bar building ground (as well as a white wire back to the transformer) at each electric breaker panel. Current comes out the black wire from a local transformer, goes through an appliance, returns via the white wire to the breaker panel, and then takes many parallel paths (including the building and LIGO's vac chamber) back to the transformer. The majority of the vac pipe current will be at 60 Hz, but higher frequency noise from motors, fluorescent lights, and switching power supplies will also be there. A quick calculation for 1 amp flowing in one skin depth (5.4 mm @ 60 Hz) of a 1 m diam SS pipe gives

E = (1 amp) (6.9 x 10^-7 ohm-m)/(2 x 3.14 x .5 m x .0054 m) = 4 x10^-5 volts/m

This might well be part of the big peak in strain you see at 60 Hz. Plotting the amplitude of the 60 Hz strain peak versus Veffective-bias would reveal how much of the 60 Hz peak is due to charge on the ETM.

On the Cold Dark Matter Search at Soudan (CDMS) we had a funny similar noise story. A copper sheathed room was built to shield the detectors (50 mK Ge with SQUID current readouts) from RF noise. All the AC for lights and scopes was run through a fancy filter (series inductors and capacitors to ground). However, lots of low freq noise was seen coming from the detectors that paradoxically went away when the fancy filter was bypassed. It turns out the filter was doing its job and shunting noise currents to the green ground wire. We initially thought these currents went thru the green wire back to the electric panel 50 m away. However, the electric code requires that the green wire be connected to building metal at every opportunity, and therefore the filtered noise currents were actually being sent directly into our room girders and cryostat.

The prior comments were for the future LIGO. Now, can one conclusively prove GW150914 (and its two weaker successors) was not an EM event with the monitoring that was available? Than you Robert for the series of good arguments in your email to me. My comments:

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2) Robert asks: “If fields from geomagnetic events could get in via sneaky routes like the cable routes, why wouldn’t the much larger locally generated electric and magnetic fields also get in?” An answer is that the locally generated B fields may get in, but there is very little associated E field to move the charged mirror. Consider the E from the f=100 Hz B field at the center between some larger radius Helmholtz coils. The integral of E around an r=1 m radius circle at the center is the time derivative of the B flux thru the circle. Assuming approx. uniform B gives:

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When you measured the ETM movement due to a magnetic field, the Helmholtz coil B was probably in the arm direction (since you were thinking of the interaction with the magnets on the upper stages). The miniscule E field from this near field B would be perpendicular to the arm, even further reducing its ability to swing the charged ETM along the arm.

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that could directly produce the event in DARM. PEM injections show that, over the frequency band of this event, the EM signal would have to appear on magnetometer channels with an SNR of at least 100 in order to produce an SNR of 1 in DARM."

As I attempted to show in (2) above, because the PEM injections used near field B the E was 10^6 times smaller than in a travelling EM wave. Thus the Log Statement should read

"PEM injections show that, over the frequency band of this event, the EM signal would have to appear on magnetometer channels with an SNR of at least 10-4 (assuming no vac pipe EM shielding of the ETM) in order to produce an SNR of 1 in DARM. Thus SNR of up to 10^4 (assuming no vac pipe EM shielding of the ETM) in DARM would not be vetoed by the magnetometer."

5) The AALightning report was very thorough. The various atmospheric and solar processes, that were thought of and monitored, did not cause GW150914. At the least, none have the right evolution in time and frequency. Only the chorus makes rising chirps, but they come in groups and are at higher frequency. Perhaps for GW150914, without a direct measure of the electric field near the charged ETM, the circumstantial case is the best that can be done. The case is thorough, but maybe there is some generator of chirping E field (clearly rare) that LIGO, a very new, different, and highly sensitive instrument, has discovered for the first time? For future events, reducing the charge on the ETM and including E field measurement near the ETM within the vac chamber, will eliminate any remaining doubt.

Gary

From: robert schofield [mailto:rmssrmss@gmail.com]
Sent: Wednesday, August 31, 2016 11:51 AM
To: Alan Weinstein; Rainer Weiss; Godfrey, Gary L.
Subject: Reply to Gary with attached Sept. 14 vetting report on risers, choruses etc., and lightning report

Hi Gary, Rai,

I want to point out that we do monitor external observatories for choruses and a wide range of other RF signals was a part of the vetting process for each of the three gravitational wave candidates. Below I attach the portion of the environmental vetting report for our first detection that covers external RF observatories. Rai, I think you may not have been aware of this part of the vetting process. Data from observatories that cover the hundreds to thousands of Hz band are near the end of the report and show no evidence of choruses, risers, or other coincident events.

I also want to point out that self-inflicted noise made the coil magnetometers that Rai mentioned, considerably less sensitive than their design. We are currently replacing them with commercial magnetometers, Lemi-120s, that have a noise floor of 0.01 pT at 100 Hz. This is mainly for subtraction of the effects of coherent inter-site magnetic fields, such as associated with Schumann resonances, for the stochastic gravitational wave search. The reason for this is that the stochastic gravitational wave background search uses inter-site correlations to dig down below the local uncorrelated noise.

The necessity of placing detectors for these global EM events at quiet sites is one birds- eye or detail-free argument for why it doesn't make sense to consider them a likely source of short transient events on our gravitational wave channel (even though we check for them anyway): the uncorrelated locally generated fields at our facilities tend to swamp the fields from these events and yet these local fields do not limit our sensitivity. If fields from geomagnetic events could get in via sneaky routes like the cable routes, why wouldn't the much larger locally generated electric and magnetic fields also get in? Even so, we make sure this is true by injecting even larger fields than our backgrounds.

I also attach the lightning report associated with our first detection (this does not cover risers or choruses since they are not produced by lightning, unlike whistlers). Hopefully it will answer your questions about lightning coupling. I disagree that the electric fields from nearby strikes are not large enough to test the system, we expect that some of the close strikes that did not effect the gravitational wave channel during our recent run produced fields of at least hundreds of volts per meter. Thunder is more likely to show up on the gravitational wave channel than the lightning that produces it. Also, you will notice that we can detect and veto events with maximum amplitudes considerably lower than your estimates Gary, using time-frequency techniques.

The attached EM environment report and the lightning report were attachments to the main environmental vetting report in the event log.

Let me know if you have any questions.

Robert

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robert schofield <rmssrmss@gmail.com> Wed, Sep 21, 2016 at 12:30 PM
To: David Shoemaker <dhs@mit.edu>

---------- Forwarded message ----------
From: **robert schofield** <rmssrmss@gmail.com>
Date: Sun, Sep 18, 2016 at 8:07 PM
Subject: Re: Reply to Gary with attached Sept. 14 vetting report on risers, choruses etc., and lightning report
To: Rai Weiss <weiss@ligo.mit.edu>
Cc: "Godfrey, Gary L." <godfrey@slac.stanford.edu>, "Alan Weinstein (ajw@ligo.caltech.edu)" <ajw@ligo.caltech.edu>

Hi Gary,

Sorry about the long delay in answering the second part of your previous response - lots of things to do as we are approaching our second run. However, I do really appreciate having this discussion with you. I'll second Rai's response to the first part of your original response and address the second part, relative to event vetting. Comments, in italics, follow your numbered responses below:

1) Robert said that self-inflicted noise was a problem for the 5000 turn coil. I think this may mean 60 Hz pickup is saturating the output. I calculate a 2 nT magnetic field from a long powerline that is perpendicularly 100 km away and carrying 1000 amps. If there are AC lines in the room with the coil, the field could easily be ~100 nT. For both the coil and the new LEMI-120 the gain has to such that the AC pickup does not rail the electronics, and there have to be enough ADC channels to have <1 pT/chan. It would be unfortunate if the setup of the 5000 turn coil made it unable to use its sensitivity to rule out an EM chirp as the cause of GW150914.

The problem with the magnetometer system was not saturation, it was that currents in the signal acquisition and processing electronics produce magnetic fields that were locally larger than the geomagnetic fields. As an example, magnetic fields from disc access (which is magnetically actuated) in a data logger once dominated the non-60 Hz signal from that magnetometer. We have tested the LEMI setup and it does not have these self-inflicted problems. However, this magnetometer is not needed for simply detecting correlated fields since any fields that are observable at our sites, 3000 km apart, should also be observed at external observatories that are often more sensitive than our noisy sites could be. We want the magnetometers for precisely following the correlated fields for subtracting their effects from our gravitational wave channel.

The difficulty of detecting the fields you are worried about illustrates the point that locally generated electric and magnetic fields swamp the signals that you are worried about and would show clearly in our spectrum and limit our sensitivity if the coupling were within orders of magnitude of your attenuation-free estimate.

The argument that local fields swamp the geomagnetic fields, along with comparisons of internal and external fields, lightning studies, and electric field injections at chamber ports, is confirmation that there is no justification for assuming that the electric fields are six orders of magnitude greater than what would be predicted from the simple attenuation model for a chamber.

And this chamber model leaves out several more orders of magnitude of attenuation: the chambers and all associated equipment are located inside Faraday cages, grounded steel-skinned buildings. Especially considering this additional ~10e6 E-field attenuation, the magnetometers are much better at detecting far-field ELF EM radiation inside the buildings than an electric field sensor would be.

As an aside, we don't have a saturation problem, even though we do have a ~700 amp transmission line that roughly parallels the Washington site at closest distance of 2km. This transmission line produces about half of our 60 Hz magnetic peak in the buildings, but that is only a few nT. I'm not sure why your estimate is so high, my best guess is that you are not considering the partial cancelation due to the return current: an estimate using the typical spacing of 20m or so between out and back currents in these transmission lines yields the measured values. In the buildings we try to twist the power lines or at least make sure that out and back are close and so we don't really have fields much over 10 nT.

2) Robert asks: "If fields from geomagnetic events could get in via sneaky routes like the cable routes, why wouldn't the much larger locally generated electric and magnetic fields also get in?" An answer is that the locally generated B fields may get in, but there is very little associated E field to move the charged mirror. Consider the E from the f=100 Hz B field at the center between some larger radius Helmholtz coils. The integral of E around an r=1 m radius circle at the center is the time derivative of the B flux thru the circle. Assuming approx. uniform B gives:

2 * pi * r * E = 2 * pi * f * pi * r^2 * B

E = f * pi* r * B

E = 300 * B

For an EM wave (ie: far field from the source) the E field is 10^6 times larger for the same B

E = 3 x 10^8 * B

When you measured the ETM movement due to a magnetic field, the Helmholtz coil B was probably in the arm direction (since you were thinking of the interaction with the magnets on the upper stages). The miniscule E field from this near field B would be perpendicular to the arm, even further reducing its ability to swing the charged ETM along the arm.

I think you may have missed my point here. I was not referring to magnetic field injections. I was referring to the electric field background, which is much larger than the electric fields from far field geomagnetic events. The question I was asking is why would these tiny geomagnetic electric fields get in when the large local electric fields do not?

Nevertheless, to be needlessly redundant, less than a month after our first detection, I did check coupling for electric fields. I wanted to be absolutely sure about attenuation of "fringing" fields and so I injected electric fields at what I thought would be the most sensitive port of a test mass chamber. The glass port was covered with a large plate with a p-p oscillation of 22 V, relative to the chamber and ground, at the mid-frequency of the GW detection from weeks before. The response in the gravitational wave channel was only 1.5 e-21 m (it took many hours of integration to see it). This test, by itself, shows that your mechanism is not a concern.

3) Robert says " I disagree that the electric fields from nearby (lightning) strikes are not large enough to test the system, we expect that some of the close strikes that did not effect the gravitational wave channel during our recent run produced fields of at least hundreds of volts per meter." If Robert's expectation is true, that the amplitude of the E field is 100's volts/m at 100 Hz (rather than at higher frequencies characteristic of the usec long strike), then the vac pipe shielding is working and is reducing the E field to below the .1 V/m at 100 Hz that would cause a clearly detectable .5 x 10^-21 strain. Maybe though the 100's volts/m are closer to 1 MHz where both the ETM is less sensitive to being driven, the interferometer noise is greater, and the vac pipe is a better shield.

The peak E field from lightning strikes is in the thousand of Hz not MHz, and is not much lower at 100 Hz (I think that the characteristic time that dominates is the milliseconds for the electric field development time between the multiple strokes in the lightning strike, not the microseconds of the actual current flow and field reversal in each of the constituent strokes). This peak E-field frequency is found in several studies that can be Googled. The strikes discussed in the lightning report are indeed a good confirmation of the large attenuation that we expect for the fields you are concerned with, and I think that the lightning strike data set, by itself, eliminates your concerns.

4) The Sept14EnvironmentLog says:

"Our primary defense against inter-site coincidences that might be produced by long-range electromagnetic signals are the PEM sensors. The magnetometers would detect any EM signal that could directly produce the event in DARM. PEM injections show that, over the frequency band of this event, the EM signal would have to appear on magnetometer channels with an SNR of at least 100 in order to produce an SNR of 1 in DARM."

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This argument is based on the assumption that the grounded steel-sheathed building and the vacuum chamber inside provide no E-field attenuation. With a simple model of attenuation, at 100 Hz, the ratio of fields from far-field EM seen inside the buildings would be roughly six orders of magnitude smaller than your figure (the attenuation of the magnetic fields by the building is only a factor of a few, while the E-field attenuation is more than 6 orders of magnitude):

$$E \sim= 3 \times (10^8 / 1 \times 10^6) * B \sim= 3 \times 10^2 B,$$

and, as seen by the test mass, something like 12 orders of magnitude smaller. It makes much more sense to try and detect far field ELF EM inside the buildings using magnetometers rather than electric field sensors. If we were trying to detect far field ELF EM inside the chamber, it would make much more sense to put a magnetometer in the chamber than an E-field sensor.

E-field injections from lightning strikes, our large-plate E-field injections and simple comparisons of internal and external fields, confirm that there are many orders of magnitude of attenuation.

As an aside, I want to remind you that imperfect conducting shells are relied on and work well for van de Graaff generators and accelerators, and people in fire lookouts and airplanes stake their lives on imperfect conducting shells during direct lightning strikes. The imperfections do not eliminate the attenuation and there is no justification for assuming no attenuation even in a worst-case scenario.

5) The AALightning report was very thorough. The various atmospheric and solar processes, that were thought of and monitored, did not cause GW150914. At the least, none have the right evolution in time and frequency. Only the chorus makes rising chirps, but they come in groups and are at higher frequency. Perhaps for GW150914, without a direct measure of the electric field near the charged ETM, the circumstantial case is the best that can be done. The case is thorough, but maybe there is some generator of chirping E field (clearly rare) that LIGO, a very new, different, and highly sensitive instrument, has discovered for the first time? For future events, reducing the charge on the ETM and including E field measurement near the ETM within the vac chamber, will eliminate any remaining doubt.

Ill agree that we can not reduce the possibility of false detections to zero (<u>even with an in-chamber electric field sensor!</u>); what we try to do is to make sure that the probability of false detections from known and unknown couplings is below the false detection probabilities given in the paper. I am convinced that test mass motion from your suggested coupling mechanism is more than eight orders of magnitude smaller than your calculation and *thus the probability of a false detection from this mechanism is vanishingly small. I think the false detection probabilities are much higher for other mechanisms, and, with limited resources, these are the mechanisms that we should focus on. For example, a much bigger concern for me is the possibility of inter-site correlations through the GPS-linked hardware and software of our control and data acquisition systems.*

That being said, we have been discussing in-chamber E-field monitoring for some time in order to study and reduce self-inflicted noise, but I do not think it is a top priority for our second run.

Robert

On Thu, Sep 15, 2016 at 6:55 PM, Rai Weiss <weiss@ligo.mit.edu> wrote:

> Gary,
> Thank you for your note. Our experience is that the charge on the mass is not uniformly distributed. Usually one quadrant excitation causes more linear electrostatic force than another. In fact one can find the centroid of the charge distribution by driving all the quadrants and looking at the yaw and pitch of the test mass. To reduce the linear force on the test mass we would need to make each quadrant a differential driver. As you point out one can reduce the linear term somewhat by the trick you propose. In any case if there is charge hopping on the test mass we will get random noise from the hopping due to field lines that leave the charge and go the other surfaces around the test mass such as the heater ring and structure. We really need to get rid of the charge.
> RW
>
> On Thu, 15 Sep 2016, Godfrey, Gary L. wrote:
>
>> September 15, 2016
>>
>> Rai,
>>
>> I understand the collaboration's hesitancy to make any major changes that might screw up the instrument or result in downtime. It sounds like you have found a good way forward by installing an electrometer inside the vac chamber to measure the actual electric field that might be present. The absence of any coincident EM chirp inside the vac chamber would alleviate any concern that future GW detections were just due to a charged ETM.
>>
>> Based on the calculation in my last email, it seems a little strange that turning off the ESD system didn't affect the interferometer noise. Perhaps the ESD was not off for a long enough time to allow the ETM to completely discharge before the noise measurement was made? Perhaps also the charged mirror noise is subdominant and won't show up until another larger noise source is eliminated.
>>
>> I looked at some LIGO design dwgs (hopefully the most recent), and I saw that a flex with 5 wires comes from the traces on the pusher to 5 coax connectors that penetrate the vac pipe. Four wires are from the A fingers in each of the four quadrants. One wire is from the interlaced B fingers which are all connected together. As you said, this precludes the use of separate bipolar drive of each quadrant. However, without changing the wiring, I believe a trick can be played that will make the total charge zero on the pusher plate (and therefore zero on the ETM side of the capacitor too). Let V1, V2, V3, V4 be the voltages (wrt vac pipe ground) on the A strips, V be the voltage on the B strips. Each set of strips has some capacitance (C1, C2, C3, C4, C) across the d=5 mm gap to the ETM. The total charge, which we set equal to zero, on the pusher plate is:

Q= C1*V1 + C2*V2 + C3*V3 + C4*V4 + C*V = 0
-V= (C1/C)*V1 + (C2/C)*V2 + (C3/C)*V3 + (C4/C)*V4

Assuming that the A and B strips in all the quadrants have the same areas this reduces to:

V= -1/4 *(V1+V2+V3+V4)

This average V can be made by a simple addition to electronics which is outside the vac chamber. For example, a new box with 5 coax connectors (4 voltage inputs and 1 voltage output) would contain 4 equal resistors summing into a node followed by a unity gain inverting amplifier. If all the strip capacitances aren't the same, tweek the values of the 4 resistors.
Unfortunately, this will result in a small coupling between the quadrants. When you push on one quadrant, all the others will respond slightly too. If these adjustments are all in a feedback system, the system may be stable and just converge despite the small couplings. Otherwise, you could diagonalize a matrix and find which voltages need be applied to just move one quadrant.

Good luck, and I look forward to many more of your wonderful GW detections in the future.

Gary

-----Original Message-----
From: Rai Weiss [mailto:weiss@ligo.mit.edu]
Sent: Saturday, September 10, 2016 1:34 PM
To: Godfrey, Gary L.
Cc: robert schofield <rmssrmss@gmail.com> (rmssrmss@gmail.com); Alan Weinstein (ajw@ligo.caltech.edu)
Subject: RE: Reply to Gary with attached Sept. 14 vetting report on risers, choruses etc., and lightning report

Gary,
All of your points are well taken. Some are even in the process of being done.

We screwed up big time by letting a hugh insulator be the test mass without a conductive coating over the surface. I have complained about this for many years as it provides a mechanism for hopping of charges over the surface which change the electrostatic forces on the mass in a random way and will at some level dominate the noise budget. Evidently at the moment the discontinuous motion of the charges on the surface do not dominate the noise since changing the bias field does not change the noise. Nor do we see a difference in the noise when the entire electrostatic drive system is turned off and the mass is controlled like a marionette from the magnets on the mass above the test mass in the quadruple suspension.

All this is not an excuse to plan for a conducting coating on the mass.
The main reason we do not currently have one is because of the difficulty of maintaining coating absorption at 1 micron less than 1 ppm. The standard conducting coatings such as tin oxide even at densities of 100s of megohms/square have too much optical loss. Marty Fejer in the Applied Physics Department at Stanford has been working with us to develop a coating with low optical loss. He may have had success with nickel oxide coating which is an electronic rather than an ionic conductor. This is still in the works. People would be loath to coat the fused silica suspension fibers because of the possibility of increased thermal noise at the bending points of the fibers. The fused silica violin modes have Q of
10^7 which is a measure of the mechanical loss of the mirror suspension.
We probably can get away without grounding a conducting coated test mass and discharge it with the thermal plasma technique that seems to work quite well. I am sure we will need a conducting coating on the mass when we try to improve the sensitivity of the detector. There is at the moment a mystery noise that is limiting the performance of the detector between
40 to 150 Hz which seems not to be due to the charging of the test mass.

This has the commissioners and system planners interest. The conducting coating is not yet considered a top priority issue. It would also be a major hit on the suspension requiring a disassembly.

I have designed some field meters that should be able to measure 10^-8 volts/cm between 10 to 500Hz. The electrometer preamps need to be in the vacuum system in encapsulated UHV containers. We have done this with the seismometers and photo detectors placed in the vacuum system. I think these will be installed between the O2 and O3 runs. Valera Frolov at the Livingston site has measured the difference in induced voltage on some insulatable components separated by 10's of cm and has measured upper limits on fields at 10^-6 volts/cm inside the chamber.

The suggestion of making the electrostatic drive differential so that there is no net field at the test mass is a good idea. I had discussed this with Rich Abbott a while ago. The trouble with this is again a major change. It would require more conducting strips on the recoil mass that faces the test mass and more wiring. It would become a priority if the commissioners found that it could reduce the noise. They are not worried about the linear offsets in the force due to the charge if kept in bounds by the thermal plasma neutralizer.

Finally, currents in the chamber walls at 60 Hz and multiples need to compete with the external 60 Hz and multiples much of which come from an unbalance in the three phase transmission line that crosses the site at Livingston. At Hanford ground loops may dominate. At this time we tolerate the 60Hz and multiples as we can still remove them in the data analysis.
As the sensitivity improves there may be an impetus to fix these as well.

You will notice a certain hesitancy in making big changes in the instrument. There are obvious flaws in the design but it has to be demonstrated that any proposed change must cure a dominant noise problem or be effective in increasing the instrument run duty cycle. If neither can be shown and it is just a guess, the idea gets a low priority. Given the hugh group of people depending on the data, this is unfortunately quite sensible.

RW

On Wed, 7 Sep 2016, Godfrey, Gary L. wrote:

> September 6, 2016
>
> Hi Rai and Robert,
>
> Congratulations Rai on receiving the Kavli Prize! It was certainly well deserved.
>
> Yes, your idea of continuous measurement of the electric field inside
> the vacuum chamber is a good one. In order to eliminate the effects of charged mirrors in the future I would prioritize:
>
> 1)   Make the average voltage zero on the pusher mass. For the
> interlaced traces, one set should go to –V when the other set goes to
> +V (certainly not 0 and 400 volts like now). To be more exact, if the
> +sets of strips don't cover exactly the same area, Area1*V1 +

Area2*V2 = 0. Since the areas are proportional to the capacitances between the strips and the ETM, this is just saying C1*V1+C2*V2=Q =0 to make the charge=0 on the pusher mass // ETM capacitor. This would seem to be just changing the drive electronics to bipolar outputs.
The only remaining voltage on the capacitor would now be ~1 volt due to contact potentials, much less than the Veffective-bias ~20 volts that you get now by occasionally flipping the drive polarity.

2) Put a sensor inside the vacuum pipe to measure electric field, in the arm direction, and near as practical to the mirror mass. Perhaps this would just be a dipole antenna that of course would be much shorter than the wavelengths being looked at.

3) Put a conductive coating on the support fibers and on the non-mirror surfaces on the ETM. This will connect the ETM surfaces to the vacuum pipe ground. However, it's not a good idea to do this before completing step 1, since it will speed up (compared to vac ion currents) the charging of the ETM surface to whatever opposite charge is on the pusher plate.

These steps will make the ETM less sensitive to EM pulses, and with a good monitor of the electric field definitively rule out an EM pulse as the cause of any future GW detection.

As Rai says, LIGO's unknown noise might also be reduced by making the ETM charge zero. Robert said the baseline electric field noise (in the low frequency region) inside a chamber has been measured to be ~ 1 x 10^-4 V/m-sqrt(Hz). Such an electric field noise near an ETM with the estimated .3 nC of charge (Veffective-bias=20 volts on 16 pf ETM-pusher capacitance) 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 /sqrt(Hz) at 100 Hz.

Another source of electric field inside the vac chamber is current running on the inside surface of the vac chamber. This current could be ~amps and results from electricians connecting the 120 VAC white wire to the bus bar building ground (as well as a white wire back to the transformer) at each electric breaker panel. Current comes out the black wire from a local transformer, goes through an appliance, returns via the white wire to the breaker panel, and then takes many parallel paths (including the building and LIGO's vac chamber) back to the transformer. The majority of the vac pipe current will be at 60 Hz, but higher frequency noise from motors, fluorescent lights, and switching power supplies will also be there. A quick calculation for 1 amp flowing in one skin depth (5.4 mm @ 60 Hz) of a 1 m diam SS pipe gives

E = (1 amp) (6.9 x 10^-7 ohm-m)/(2 x 3.14 x .5 m x .0054 m) = 4 x10^-5
volts/m

This might well be part of the big peak in strain you see at 60 Hz.
Plotting the amplitude of the 60 Hz strain peak versus Veffective-bias would reveal how much of the 60 Hz peak is due to charge on the ETM.

On the Cold Dark Matter Search at Soudan (CDMS) we had a funny similar noise story. A copper sheathed room was built to shield the detectors (50 mK Ge with SQUID current readouts) from RF noise. All the AC for lights and scopes was run through a fancy filter (series inductors and capacitors to ground). However, lots of low freq noise was seen coming from the detectors that paradoxically went away when the fancy filter was bypassed. It turns out the filter was doing its job and shunting noise currents to the green ground wire. We initially thought these currents went thru the green wire back to the electric panel 50 m away. However, the electric code requires that the green wire be connected to building metal at every opportunity, and therefore the filtered noise currents were actually being sent directly into our room girders and cryostat.

The prior comments were for the future LIGO. Now, can one conclusively prove GW150914 (and its two weaker successors) was not an EM event with the monitoring that was available? Than you Robert for the series of good arguments in your email to me. My comments:

1) Robert said that self-inflicted noise was a problem for the 5000 turn coil. I think this may mean 60 Hz pickup is saturating the output. I calculate a 2 nT magnetic field from a long powerline that is perpendicularly 100 km away and carrying 1000 amps. If there are AC lines in the room with the coil, the field could easily be ~100 nT. For both the coil and the new LEMI-120 the gain has to such that the AC pickup does not rail the electronics, and there have to be enough ADC channels to have <1 pT/chan. It would be unfortunate if the setup of the 5000 turn coil made it unable to use its sensitivity to rule out an EM chirp as the cause of GW150914.

2) Robert asks: “If fields from geomagnetic events could get in via sneaky routes like the cable routes, why wouldn’t the much larger locally generated electric and magnetic fields also get in?” An answer is that the locally generated B fields may get in, but there is

very little associated E field to move the charged mirror. Consider the E from the f=100 Hz B field at the center between some larger radius Helmholtz coils. The integral of E around an r=1 m radius circle at the center is the time derivative of the B flux thru the circle. Assuming approx. uniform B gives:

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(lightning) strikes are not large enough to test the system, we expect
that some of the close strikes that did not effect the gravitational
wave channel during our recent run produced fields of at least
hundreds of volts per meter." If Robert's expectation is true, that
the amplitude of the E field is 100's volts/m at 100 Hz (rather than at higher frequencies characteristic of the usec long strike), then the vac pipe shielding is working and is reducing the E field to below the .1 V/m at 100 Hz that would cause a clearly detectable .5 x 10^-21 strain. Maybe though the 100's volts/m are closer to 1 MHz where both the ETM is less sensitive to being driven, the interferometer noise is greater, and the vac pipe is a better shield.

4) The Sept14EnvironmentLog says:

"Our primary defense against inter-site coincidences that might be produced by long-range
electromagnetic signals are the PEM sensors.
The magnetometers would detect any EM signal

that could directly produce the event in DARM. PEM injections show
that, over the frequency band of this event, the EM signal would have to appear on magnetometer channels with an SNR of at least 100 in order to produce an SNR of 1 in DARM."

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near field B the E was 10^6 times smaller than in a travelling EM
wave. Thus the Log Statement should read

"PEM injections show that, over the frequency band of this event, the

EM signal would have to appear on magnetometer channels with an SNR of
at least 10-4 (assuming no vac pipe EM shielding of the ETM) in order to produce an SNR of 1 in DARM. Thus SNR of up to 10^4 (assuming no vac pipe EM shielding of the ETM) in DARM would not be vetoed by the magnetometer."

5) The AALightning report was very thorough. The various atmospheric and solar processes, that were thought of and monitored, did not cause GW150914. At the least, none have the right evolution in time and frequency. Only the chorus makes rising chirps, but they come in groups and are at higher frequency. Perhaps for GW150914, without a direct measure of the electric field near the charged ETM, the circumstantial case is the best that can be done. The case is thorough, but maybe there is some generator of chirping E field (clearly rare) that LIGO, a very new, different, and highly sensitive instrument, has discovered for the first time? For future events, reducing the charge on the ETM and including E field measurement near the ETM within the vac chamber, will eliminate any remaining doubt.

Gary

From: robert schofield [mailto:rmssrmss@gmail.com]
Sent: Wednesday, August 31, 2016 11:51 AM
To: Alan Weinstein; Rainer Weiss; Godfrey, Gary L.
Subject: Reply to Gary with attached Sept. 14 vetting report on risers, choruses etc., and lightning report

Hi Gary, Rai,

I want to point out that we do monitor external observatories for choruses and a wide range of other RF signals was a part of the vetting process for each of the three gravitational wave candidates. Below I attach the portion of the environmental vetting report for our first detection that covers external RF observatories. Rai, I think you may not have been aware of this part of the vetting process. Data from observatories that cover the hundreds to thousands of Hz band are near the end of the report and show no evidence of choruses, risers, or other coincident events.

I also want to point out that self-inflicted noise made the coil magnetometers that Rai mentioned, considerably less sensitive than their design. We are currently replacing them with commercial magnetometers, Lemi-120s, that have a noise floor of 0.01 pT at 100 Hz. This is mainly for subtraction of the effects of coherent inter-site magnetic fields, such as associated with Schumann resonances, for the stochastic gravitational wave search. The reason for this is that the stochastic gravitational wave background search uses inter-site correlations to dig down below the local uncorrelated noise.

The necessity of placing detectors for these global EM events at quiet sites is one birds- eye or detail-free argument for why it doesn't make sense to consider them a likely source of short transient events on our gravitational wave channel (even though we check for them anyway): the uncorrelated locally generated fields at our facilities tend to swamp the fields from these events and yet these local fields do not limit our sensitivity. If fields from geomagnetic events could get in via sneaky routes like the cable routes, why wouldn't the much larger locally generated electric and magnetic fields also get in? Even so, we make sure this is true by injecting even larger fields than our backgrounds.

I also attach the lightning report associated with our first detection (this does not cover risers or choruses since they are not produced by lightning, unlike whistlers). Hopefully it will answer your questions about lightning coupling. I disagree that the electric fields from nearby strikes are not large enough to test the system, we expect that some of the close strikes that did not effect the gravitational wave channel during our recent run produced fields of at least hundreds of volts per meter. Thunder is more likely to show up on the gravitational wave channel than the lightning that produces it. Also, you will notice that we can detect and veto events with maximum amplitudes considerably lower than your estimates Gary, using time-frequency techniques.

The attached EM environment report and the lightning report were attachments to the main environmental vetting report in the event log.

Let me know if you have any questions.

Robert

---

robert schofield <rmssrmss@gmail.com> Wed, Oct 26, 2016 at 3:25 PM
To: Rai Weiss <weiss@ligo.mit.edu>, David Shoemaker <dhs@mit.edu>
Cc: "Godfrey, Gary L." <godfrey@slac.stanford.edu>, "Alan Weinstein (ajw@ligo.caltech.edu)" <ajw@ligo.caltech.edu>

Hi Gary,

If we are mostly done with this email discussion, I wonder if you would mind if I posted the thread to our event log? I think it would make a valuable contribution to the detection documentation.

Robert

On Sun, Sep 18, 2016 at 8:07 PM, robert schofield <rmssrmss@gmail.com> wrote:

> Hi Gary,
>
> Sorry about the long delay in answering the second part of your previous response - lots of things to do as we are approaching our second run. However, I do really appreciate having this discussion with you. I'll second Rai's response to the first part of your original response and address the second part, relative to event vetting. Comments, in italics, follow your numbered responses below:
>
> 1) Robert said that self-inflicted noise was a problem for the 5000 turn coil. I think this may mean 60 Hz pickup is saturating the output. I calculate a 2 nT magnetic field from a long powerline that is perpendicularly 100 km away and carrying 1000 amps. If there are AC lines in the room with the coil, the field could easily be ~100 nT. For both the coil and the new LEMI-120 the gain has to such that the AC pickup does not rail the electronics, and there have to be enough ADC channels to have <1 pT/chan. It would be unfortunate if the setup of the 5000 turn coil made it unable to use its sensitivity to rule out an EM chirp as the cause of GW150914.
>
> *The problem with the magnetometer system was not saturation, it was that currents in the signal acquisition and processing electronics produce magnetic fields that were locally larger than the geomagnetic fields. As an example, magnetic fields from disc access (which is magnetically actuated) in a data logger once dominated the non-60 Hz signal from that magnetometer. We have tested the LEMI setup and it does not have these self-inflicted problems. However, this magnetometer is not needed for simply detecting correlated fields since any fields that are observable at our sites, 3000 km apart, should also be observed at external observatories that are often more sensitive than our noisy sites could be. We want the magnetometers for precisely following the correlated fields for subtracting their effects from our gravitational wave channel.*
>
> *The difficulty of detecting the fields you are worried about illustrates the point that locally generated electric and magnetic fields swamp the signals that you are worried about and would show clearly in our spectrum and limit our sensitivity if the coupling were within orders of magnitude of your attenuation-free estimate.*
>
> *The argument that local fields swamp the geomagnetic fields, along with comparisons of internal and external fields, lightning studies, and electric field injections at chamber ports, is confirmation that there is no justification for assuming that the electric fields are six orders of magnitude greater than what would be predicted from the simple attenuation model for a chamber.*
>
> *And this chamber model leaves out several more orders of magnitude of attenuation: the chambers and all associated equipment are located inside Faraday cages, grounded steel-skinned buildings. Especially considering this additional ~10e6 E-field attenuation, the magnetometers are much better at detecting far-field*

ELF EM radiation inside the buildings than an electric field sensor would be.

As an aside, we don't have a saturation problem, even though we do have a ~700 amp transmission line that roughly parallels the Washington site at closest distance of 2km. This transmission line produces about half of our 60 Hz magnetic peak in the buildings, but that is only a few nT. I'm not sure why your estimate is so high, my best guess is that you are not considering the partial cancelation due to the return current: an estimate using the typical spacing of 20m or so between out and back currents in these transmission lines yields the measured values. In the buildings we try to twist the power lines or at least make sure that out and back are close and so we don't really have fields much over 10 nT.

2) Robert asks: "If fields from geomagnetic events could get in via sneaky routes like the cable routes, why wouldn't the much larger locally generated electric and magnetic fields also get in?" An answer is that the locally generated B fields may get in, but there is very little associated E field to move the charged mirror. Consider the E from the f=100 Hz B field at the center between some larger radius Helmholtz coils. The integral of E around an r=1 m radius circle at the center is the time derivative of the B flux thru the circle. Assuming approx. uniform B gives:

2 * pi * r * E = 2 * pi * f * pi * r^2 * B

E = f * pi* r * B

E = 300 * B

For an EM wave (ie: far field from the source) the E field is 10^6 times larger for the same B

E = 3 x 10^8 * B

When you measured the ETM movement due to a magnetic field, the Helmholtz coil B was probably in the arm direction (since you were thinking of the interaction with the magnets on the upper stages). The miniscule E field from this near field B would be perpendicular to the arm, even further reducing its ability to swing the charged ETM along the arm.

I think you may have missed my point here. I was not referring to magnetic field injections. I was referring to the electric field background, which is much larger than the electric fields from far field geomagnetic events. The question I was asking is why would these tiny geomagnetic electric fields get in when the large local electric fields do not?

Nevertheless, to be needlessly redundant, less than a month after our first detection, I did check coupling for electric fields. I wanted to be absolutely sure about attenuation of "fringing" fields and so I injected electric fields at what I thought would be the most sensitive port of a test mass chamber. The glass port was covered with a large plate with a p-p oscillation of 22 V, relative to the chamber and ground, at the mid-frequency of the GW detection from weeks before. The response in the gravitational wave channel was only 1.5 e-21 m (it took many hours of integration to see it). This test, by itself, shows that your mechanism is not a concern.

3) Robert says " I disagree that the electric fields from nearby (lightning) strikes are not large enough to test the system, we expect that some of the close strikes that did not effect the gravitational wave channel during our recent run produced fields of at least hundreds of volts per meter." If Robert's expectation is true, that the amplitude of the E field is 100's volts/m at 100 Hz (rather than at higher frequencies

characteristic of the usec long strike), then the vac pipe shielding is working and is reducing the E field to below the .1 V/m at 100 Hz that would cause a clearly detectable .5 x 10^-21 strain. Maybe though the 100's volts/m are closer to 1 MHz where both the ETM is less sensitive to being driven, the interferometer noise is greater, and the vac pipe is a better shield.

The peak E field from lightning strikes is in the thousand of Hz not MHz, and is not much lower at 100 Hz (I think that the characteristic time that dominates is the milliseconds for the electric field development time between the multiple strokes in the lightning strike, not the microseconds of the actual current flow and field reversal in each of the constituent strokes). This peak E-field frequency is found in several studies that can be Googled. The strikes discussed in the lightning report are indeed a good confirmation of the large attenuation that we expect for the fields you are concerned with, and I think that the lightning strike data set, by itself, eliminates your concerns.

4) The Sept14EnvironmentLog says:

"Our primary defense against inter-site coincidences that might be produced by long-range electromagnetic signals are the PEM sensors. The magnetometers would detect any EM signal

that could directly produce the event in DARM. PEM injections show that, over the frequency band of this event, the EM signal would have to appear on magnetometer channels with an SNR of at least 100 in order to produce an SNR of 1 in DARM."

As I attempted to show in (2) above, because the PEM injections used near field B the E was 10^6 times smaller than in a travelling EM wave. Thus the Log Statement should read

"PEM injections show that, over the frequency band of this event, the EM signal would have to appear on magnetometer channels with an SNR of at least **10-4 (assuming no vac pipe EM shielding of the ETM)** in order to produce an SNR of 1 in DARM. Thus SNR of up to **10^4 (assuming no vac pipe EM shielding of the ETM)** in DARM would not be vetoed by the magnetometer."

This argument is based on the assumption that the grounded steel-sheathed building and the vacuum chamber inside provide no E-field attenuation. With a simple model of attenuation, at 100 Hz, the ratio of fields from far-field EM seen inside the buildings would be roughly six orders of magnitude smaller than your figure (the attenuation of the magnetic fields by the building is only a factor of a few, while the E-field attenuation is more than 6 orders of magnitude):

*E ~= 3 x (10^8/ 1 x10^6) * B ~= 3 x 10 ^2 B,*

and, as seen by the test mass, something like 12 orders of magnitude smaller. It makes much more sense to try and detect far field ELF EM inside the buildings using magnetometers rather than electric field sensors. If we were trying to detect far field ELF EM inside the chamber, it would make much more sense to put a magnetometer in the chamber than an E-field sensor.

E-field injections from lightning strikes, our large-plate E-field injections and simple comparisons of internal and external fields, confirm that there are many orders of magnitude of attenuation.

As an aside, I want to remind you that imperfect conducting shells are relied on and work well for van de Graaff generators and accelerators, and people in fire lookouts and airplanes stake their lives on imperfect *conducting shells during direct lightning strikes. The imperfections do not eliminate the attenuation and there is no justification for assuming no attenuation even in a worst-case scenario.*

5) The AALightning report was very thorough. The various atmospheric and solar processes, that were thought of and monitored, did not cause GW150914. At the least, none have the right evolution in time and frequency. Only the chorus makes rising chirps, but they come in groups and are at higher frequency. Perhaps for GW150914, without a direct measure of the electric field near the charged ETM, the circumstantial case is the best that can be done. The case is thorough, but maybe there is some generator of chirping E field (clearly rare) that LIGO, a very new, different, and highly sensitive instrument, has discovered for the first time? For future events, reducing the charge on the ETM and including E field measurement near the ETM within the vac chamber, will eliminate any remaining doubt.

Ill agree that we can not reduce the possibility of false detections to zero (even with an in-chamber electric field sensor!); what we try to do is to make sure that the probability of false detections from known and unknown couplings is below the false detection probabilities given in the paper. I am convinced that test mass motion from your suggested coupling mechanism is more than eight orders of magnitude smaller than your calculation and thus the probability of a false detection from this mechanism is vanishingly small. I think the false detection probabilities are much higher for other mechanisms, and, with limited resources, these are the mechanisms that we should focus on. For example, a much bigger concern for me is the possibility of inter-site correlations through the GPS-linked hardware and software of our control and data acquisition systems.

That being said, we have been discussing in-chamber E-field monitoring for some time in order to study and reduce self-inflicted noise, but I do not think it is a top priority for our second run.

Robert

On Thu, Sep 15, 2016 at 6:55 PM, Rai Weiss <weiss@ligo.mit.edu> wrote:

Gary,
Thank you for your note. Our experience is that the charge on the mass is not uniformly distributed. Usually one quadrant excitation causes more linear electrostatic force than another. In fact one can find the centroid of the charge distribution by driving all the quadrants and looking at the yaw and pitch of the test mass. To reduce the linear force on the test mass we would need to make each quadrant a differential driver. As you point out one can reduce the linear term somewhat by the trick you propose. In any case if there is charge hopping on the test mass we will get random noise from the hopping due to field lines that leave the charge and go the other surfaces around the test mass such as the heater ring and structure. We really need to get rid of the charge.
RW

On Thu, 15 Sep 2016, Godfrey, Gary L. wrote:

September 15, 2016

Rai,

I understand the collaboration's hesitancy to make any major changes that might screw up the instrument or result in downtime. It sounds like you have found a good way forward by installing an electrometer inside the vac chamber to measure the actual electric field that might be present. The absence of any coincident EM chirp inside the vac chamber would alleviate any concern that future GW detections were just due to a charged ETM.

Based on the calculation in my last email, it seems a little strange that turning off the ESD system didn't affect the interferometer noise. Perhaps the ESD was not off for a long enough time to allow the ETM to completely discharge before the noise measurement was made? Perhaps also the charged mirror noise is subdominant and won't show up until another larger noise source is eliminated.

I looked at some LIGO design dwgs (hopefully the most recent), and I saw that a flex with 5 wires comes from the traces on the pusher to 5 coax connectors that penetrate the vac pipe. Four wires are from the A fingers in each of the four quadrants. One wire is from the interlaced B fingers which are all connected together. As you said, this precludes the use of separate bipolar drive of each quadrant. However, without changing the wiring, I believe a trick can be played that will make the total charge zero on the pusher plate (and therefore zero on the ETM side of the capacitor too). Let V1, V2, V3, V4 be the voltages (wrt vac pipe ground) on the A strips, V be the voltage on the B strips. Each set of strips has some capacitance (C1, C2, C3, C4, C) across the d=5 mm gap to the ETM. The total charge, which we set equal to zero, on the pusher plate is:

Q= C1*V1 + C2*V2 + C3*V3 + C4*V4 + C*V = 0
-V= (C1/C)*V1 + (C2/C)*V2 + (C3/C)*V3 + (C4/C)*V4

Assuming that the A and B strips in all the quadrants have the same areas this reduces to:

V= -1/4 *(V1+V2+V3+V4)

This average V can be made by a simple addition to electronics which is outside the vac chamber. For example, a new box with 5 coax connectors (4 voltage inputs and 1 voltage output) would contain 4 equal resistors summing into a node followed by a unity gain inverting amplifier. If all the strip capacitances aren't the same, tweek the values of the 4 resistors.
Unfortunately, this will result in a small coupling between the quadrants. When you push on one quadrant, all the others will respond slightly too. If these adjustments are all in a feedback system, the system may be stable and just converge despite the small couplings. Otherwise, you could diagonalize a matrix and find which voltages need be applied to just move one quadrant.

Good luck, and I look forward to many more of your wonderful GW detections in the future.

Gary

-----Original Message-----
From: Rai Weiss [mailto:weiss@ligo.mit.edu]
Sent: Saturday, September 10, 2016 1:34 PM
To: Godfrey, Gary L.
Cc: robert schofield <rmssrmss@gmail.com> (rmssrmss@gmail.com); Alan Weinstein (ajw@ligo.caltech.edu)
Subject: RE: Reply to Gary with attached Sept. 14 vetting report on risers, choruses etc., and lightning report

Gary,
All of your points are well taken. Some are even in the process of being done.

We screwed up big time by letting a hugh insulator be the test mass without a conductive coating over the surface. I have complained about this for many years as it provides a mechanism for hopping of charges over the surface which change the electrostatic forces on the mass in a random way and will at some level dominate the noise budget. Evidently at the moment the discontinuous motion of the charges on the surface do not dominate the noise since changing the bias field does not change the noise. Nor do we see a difference in the noise when the entire electrostatic drive system is turned off and the mass is controlled like a marionette from the magnets on the mass above the test mass in the quadruple suspension.

All this is not an excuse to plan for a conducting coating on the mass.
The main reason we do not currently have one is because of the difficulty of maintaining coating absorption at 1 micron less than 1 ppm. The standard conducting coatings such as tin oxide even at densities of 100s of megohms/square have too much optical loss. Marty Fejer in the Applied Physics Department at Stanford has been working with us to develop a coating with low optical loss. He may have had success with nickel oxide coating which is an electronic rather than an ionic conductor. This is still in the works. People would be loath to coat the fused silica suspension fibers because of the possibility of increased thermal noise at the bending points of the fibers. The fused silica violin modes have Q of 10^7 which is a measure of the mechanical loss of the mirror suspension.
We probably can get away without grounding a conducting coated test mass and discharge it with the thermal plasma technique that seems to work quite well. I am sure we will need a conducting coating on the mass when we try to improve the sensitivity of the detector. There is at the moment a mystery noise that is limiting the performance of the detector between
40 to 150 Hz which seems not to be due to the charging of the test mass.
This has the commissioners and system planners interest. The conducting coating is not yet considered a top priority issue. It would also be a major hit on the suspension requiring a disassembly.

I have designed some field meters that should be able to measure 10^-8 volts/cm between 10 to 500Hz. The electrometer preamps need to be in the vacuum system in encapsulated UHV containers. We have done this with the seismometers and photo detectors placed in the vacuum system. I think these will be installed between the O2 and O3 runs. Valera Frolov at the Livingston site has measured the difference in induced voltage on some insulatable components separated by 10's of cm and has measured upper limits on fields at 10^-6 volts/cm inside the chamber.

The suggestion of making the electrostatic drive differential so that there is no net field at the test mass is a good idea. I had discussed this with Rich Abbott a while ago. The trouble with this is again a major change. It would require more conducting strips on the recoil mass that faces the test mass and more wiring. It would become a priority if the commissioners found that it could reduce the noise. They are not worried about the linear offsets in the force due to the charge if kept in bounds by the thermal plasma neutralizer.

Finally, currents in the chamber walls at 60 Hz and multiples need to compete with the external 60 Hz and multiples much of which come from an unbalance in the three phase transmission line that crosses the site at Livingston. At Hanford ground loops may dominate. At this time we tolerate the 60Hz and multiples as we can still remove them in the data analysis.
As the sensitivity improves there may be an impetus to fix these as well.

You will notice a certain hesitancy in making big changes in the instrument. There are obvious flaws in the design but it has to be demonstrated that any proposed change must cure a dominant noise problem or be effective in increasing the instrument run duty cycle. If neither can be shown and it is just a guess, the idea gets a low priority. Given the hugh group of people depending on the data, this is unfortunately quite sensible.

RW

On Wed, 7 Sep 2016, Godfrey, Gary L. wrote:

September 6, 2016

Hi Rai and Robert,

Congratulations Rai on receiving the Kavli Prize! It was certainly well deserved.

Yes, your idea of continuous measurement of the electric field inside
the vacuum chamber is a good one. In order to eliminate the effects of charged mirrors in the future I would prioritize:

1) Make the average voltage zero on the pusher mass. For the
interlaced traces, one set should go to –V when the other set goes to
+V (certainly not 0 and 400 volts like now). To be more exact, if the
+sets of strips don't cover exactly the same area, Area1*V1 +
Area2*V2 = 0. Since the areas are proportional to the capacitances
between the strips and the ETM, this is just saying C1*V1+C2*V2=Q =0 to make the charge=0 on the pusher mass // ETM capacitor. This would seem to be just changing the drive electronics to bipolar outputs.
The only remaining voltage on the capacitor would now be ~1 volt due
to contact potentials, much less than the Veffective-bias ~20 volts that you get now by occasionally flipping the drive polarity.

2) Put a sensor inside the vacuum pipe to measure electric field,
in the arm direction, and near as practical to the mirror mass. Perhaps this would just be a dipole antenna that of course would be much shorter than the wavelengths being looked at.

3) Put a conductive coating on the support fibers and on the
non-mirror surfaces on the ETM. This will connect the ETM surfaces to
the vacuum pipe ground. However, it's not a good idea to do this
before completing step 1, since it will speed up (compared to vac ion
currents) the charging of the ETM surface to whatever opposite charge is on the pusher plate.

These steps will make the ETM less sensitive to EM pulses, and with a
good monitor of the electric field definitively rule out an EM pulse as the cause of any future GW detection.

As Rai says, LIGO's unknown noise might also be reduced by making the
ETM charge zero. Robert said the baseline electric field noise (in
the low frequency region) inside a chamber has been measured to be ~ 1

x 10^-4 V/m-sqrt(Hz). Such an electric field noise near an ETM with the estimated .3 nC of charge (Veffective-bias=20 volts on 16 pf ETM-pusher capacitance) 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 /sqrt(Hz) at 100 Hz.

Another source of electric field inside the vac chamber is current running on the inside surface of the vac chamber. This current could be ~amps and results from electricians connecting the 120 VAC white wire to the bus bar building ground (as well as a white wire back to the transformer) at each electric breaker panel. Current comes out the black wire from a local transformer, goes through an appliance, returns via the white wire to the breaker panel, and then takes many parallel paths (including the building and LIGO's vac chamber) back to the transformer. The majority of the vac pipe current will be at 60 Hz, but higher frequency noise from motors, fluorescent lights, and switching power supplies will also be there. A quick calculation for 1 amp flowing in one skin depth (5.4 mm @ 60 Hz) of a 1 m diam SS pipe gives

E = (1 amp) (6.9 x 10^-7 ohm-m)/(2 x 3.14 x .5 m x .0054 m) = 4 x10^-5 volts/m

This might well be part of the big peak in strain you see at 60 Hz.
Plotting the amplitude of the 60 Hz strain peak versus Veffective-bias would reveal how much of the 60 Hz peak is due to charge on the ETM.

On the Cold Dark Matter Search at Soudan (CDMS) we had a funny similar noise story. A copper sheathed room was built to shield the detectors (50 mK Ge with SQUID current readouts) from RF noise. All the AC for lights and scopes was run through a fancy filter (series inductors and capacitors to ground). However, lots of low freq noise was seen coming from the detectors that paradoxically went away when the fancy filter was bypassed. It turns out the filter was doing its job and shunting noise currents to the green ground wire. We initially thought these currents went thru the green wire back to the electric panel 50 m away. However, the electric code requires that the green wire be connected to building metal at every opportunity, and therefore the filtered noise currents were actually being sent directly into our room girders and cryostat.

The prior comments were for the future LIGO. Now, can one conclusively prove GW150914 (and its two weaker successors) was not an EM event with the monitoring that was available? Than you Robert for the series of good arguments in your email to me. My comments:

1) Robert said that self-inflicted noise was a problem for the 5000 turn coil. I think this may mean 60 Hz pickup is saturating the output. I calculate a 2 nT magnetic field from a long powerline that is perpendicularly 100 km away and carrying 1000 amps. If there are AC lines in the room with the coil, the field could easily be ~100 nT. For both the coil and the new LEMI-120 the gain has to such that the AC pickup does not rail the electronics, and there have to be enough ADC channels to have <1 pT/chan. It would be unfortunate if the setup of the 5000 turn coil made it unable to use its sensitivity to rule out an EM chirp as the cause of GW150914.

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When you measured the ETM movement due to a magnetic field, the Helmholtz coil B was probably in the arm direction (since you were thinking of the interaction with the magnets on the upper stages). The miniscule E field from this near field B would be perpendicular to the arm, even further reducing its ability to swing the charged ETM along the arm.

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5) The AALightning report was very thorough. The various atmospheric and solar processes, that were thought of and monitored, did not cause GW150914. At the least, none have the right evolution in time and frequency. Only the chorus makes rising chirps, but they come in groups and are at higher frequency. Perhaps for GW150914, without a direct measure of the electric field near the charged ETM, the circumstantial case is the best that can be done. The case is thorough, but maybe there is some generator of chirping E field (clearly rare) that LIGO, a very new, different, and highly sensitive instrument, has discovered for the first time? For future events, reducing the charge on the ETM and including E field measurement near the ETM within the vac chamber, will eliminate any remaining doubt.

Gary

From: robert schofield [mailto:rmssrmss@gmail.com]

Sent: Wednesday, August 31, 2016 11:51 AM
To: Alan Weinstein; Rainer Weiss; Godfrey, Gary L.
Subject: Reply to Gary with attached Sept. 14 vetting report on risers, choruses etc., and lightning report

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I want to point out that we do monitor external observatories for choruses and a wide range of other RF signals was a part of the vetting process for each of the three gravitational wave candidates. Below I attach the portion of the environmental vetting report for our first detection that covers external RF observatories. Rai, I think you may not have been aware of this part of the vetting process. Data from observatories that cover the hundreds to thousands of Hz band are near the end of the report and show no evidence of choruses, risers, or other coincident events.

I also want to point out that self-inflicted noise made the coil magnetometers that Rai mentioned, considerably less sensitive than their design. We are currently replacing them with commercial magnetometers, Lemi-120s, that have a noise floor of 0.01 pT at 100 Hz. This is mainly for subtraction of the effects of coherent inter-site magnetic fields, such as associated with Schumann resonances, for the stochastic gravitational wave search. The reason for this is that the stochastic gravitational wave background search uses inter-site correlations to dig down below the local uncorrelated noise.

The necessity of placing detectors for these global EM events at quiet sites is one birds- eye or detail-free argument for why it doesn't make sense to consider them a likely source of short transient events on our gravitational wave channel (even though we check for them anyway): the uncorrelated locally generated fields at our facilities tend to swamp the fields from these events and yet these local fields do not limit our sensitivity. If fields from geomagnetic events could get in via sneaky routes like the cable routes, why wouldn't the much larger locally generated electric and magnetic fields also get in? Even so, we make sure this is true by injecting even larger fields than our backgrounds.

I also attach the lightning report associated with our first detection (this does not cover risers or choruses since they are not produced by lightning, unlike whistlers). Hopefully it will answer your questions about lightning coupling. I disagree that the electric fields from nearby strikes are not large enough to test the system, we expect that some of the close strikes that did not effect the gravitational wave channel during our recent run produced fields of at least hundreds of volts per meter. Thunder is more likely to show up on the gravitational wave channel than the lightning that produces it. Also, you will notice that we can detect and veto events with maximum amplitudes considerably lower than your estimates Gary, using time-frequency techniques.

The attached EM environment report and the lightning report were attachments to the main environmental vetting report in the event log.

Let me know if you have any questions.

Robert