



Alan Weinstein &lt;alan.j.weinstein@gmail.com&gt;

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## [Aligo\_systems] Request to remove the vacuum bake-out

21 messages

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**Dennis Coyne** <coyne@ligo.caltech.edu>

Tue, Mar 28, 2006 at 3:27 PM

To: Fred Raab <fjr@ligo-wa.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, Rai Weiss <weiss@ligo.mit.edu>, John Worden <worden@ligo.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>

Cc: GariLynn Billingsley <gari@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, Bill Kells <kells@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>, aligo\_sys@ligo.caltech.edu, lsc-owg@ligo.caltech.edu

LIGO Vacuum Review Board:

Attached is a memo requesting that the planned ADL vacuum system bake-out be removed from the baseline. Please review and comment. We would like to remove this activity and cost from the plan if technically warranted. A

quick reply would be appreciated. Thank you,

Dennis

**T060065-00.pdf**

86K

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**Fred Raab** <raab\_f@ligo-wa.caltech.edu>

Tue, Mar 28, 2006 at 4:30 PM

Reply-To: raab\_f@ligo-wa.caltech.edu

To: Dennis Coyne <coyne@ligo.caltech.edu>

Cc: Fred Raab <fjr@ligo-wa.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, Rai Weiss <weiss@ligo.mit.edu>, John Worden <worden@ligo.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, Bill Kells <kells@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>, aligo\_sys@ligo.caltech.edu, lsc-owg@ligo.caltech.edu

Dennis,

I agree that we have no firm indication that we need a bakeout of the vacuum system. It would be useful to understand where the aliphatics and diallyl pthalate come from. Technically, it makes sense to remove the bakeout from the baseline plan.

As a management issue, I expect you want to move this from your baseline budget to contingency in some way. Does the previous contingency-rate calculation rise because you are taking on more risk? Does the bakeout move onto a contingency-liens list? I don't have a strong opinion, except that someone needs to incorporate this into the

contingency management for AdLIGO.

Fred

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[Quoted text hidden]

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Dr. Frederick J. Raab, Head  
LIGO Hanford Observatory  
P.O. Box 159  
Richland, WA 99352

phone: 509-372-8125  
FAX: 509-372-8137

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Aligo\_systems mailing list

[Aligo\\_systems@ligo.caltech.edu](mailto:Aligo_systems@ligo.caltech.edu)

[http://mm.ligo.caltech.edu/mailman/listinfo/aligo\\_systems](http://mm.ligo.caltech.edu/mailman/listinfo/aligo_systems)

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**Michael Zucker <zucker\_m@ligo.mit.edu>**

**Tue, Mar 28, 2006 at 4:30 PM**

To: Dennis Coyne <coyne@ligo.caltech.edu>

Cc: John Worden <worden@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, aligo\_sys@ligo.caltech.edu, Bill Kells <kells@ligo.caltech.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Rai Weiss <weiss@ligo.mit.edu>, Fred Raab <fjr@ligo-wa.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, lsc-owg@ligo.caltech.edu, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>

Dennis-

I'm sorry to have missed the LSC discussion. I may therefore be asking stupid questions.

The CR seems to indicate that the bound on cumulative deposition rate from the replaced ITM has been shown to be compatible with AdL requirements, scaling to the required AdL life cycle. (Is this a correct interpretation?)

On the other hand, this optic was incompatible with operation of initial LIGO; so is the AdL requirement itself perhaps

suspect?

Finally maybe part of the argument is that the VE itself (and Flourel seals) is exonerated by the chemical composition of the contamination. Is this a factor in our security? (and indeed, are we taking steps to purge the true offenders, e.g., diallyl phthalate connector bodies if that's what it is)

BTW I looked at the SEM document and it looks like it is missing the pictures. Also, if there's a technical report in preparation I wonder if we could have a look at a draft, or if a decision is really needed on a timescale faster than the report can be completed?

Mike

[Quoted text hidden]

> Dennis<T060065-00.pdf>

[Quoted text hidden]

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**John Worden <worden\_j@ligo-wa.caltech.edu>**

**Tue, Mar 28, 2006 at 4:48 PM**

To: Dennis Coyne <coyne@ligo.caltech.edu>, Fred Raab <fjr@ligo-wa.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, Rai Weiss <weiss@ligo.mit.edu>, John Worden <worden@ligo.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>

Cc: GariLynn Billingsley <gari@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, Bill Kells <kells@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>, aligo\_sys@ligo.caltech.edu, lsc-owg@ligo.caltech.edu

I support the removal of the bake-out from the plan for the following reason:

We have no proof that the vacuum quality is any worse than it was when we took delivery from PSI. If we were to rebake with the same care that was taken originally, we may or may not do better than PSI. Also, a single failure of a heater circuit could overheat an o-ring and distribute the outgassing products throughout the vacuum system. Likewise, a mechanical failure resulting from thermal cycling could end in repair work with risks of further contamination and negating of the bake. In other words, I feel the risks are great and even a "successful" bake would very likely leave us where we are now.

John

[Quoted text hidden]

[Quoted text hidden]

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**Dennis Coyne <coyne@ligo.caltech.edu>**

**Tue, Mar 28, 2006 at 6:50 PM**

To: Michael Zucker <zucker\_m@ligo.mit.edu>

Cc: John Worden <worden@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, aligo\_sys@ligo.caltech.edu, Bill Kells <kells@ligo.caltech.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Rai Weiss <weiss@ligo.mit.edu>, Fred Raab <fjr@ligo-wa.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, lsc-owg@ligo.caltech.edu, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>

At 04:30 PM 3/28/2006, Michael Zucker wrote:

Dennis-

I'm sorry to have missed the LSC discussion. I may therefore be asking stupid questions.

The CR seems to indicate that the bound on cumulative deposition rate from the replaced ITM has been shown to be compatible with AdL requirements, scaling to the required AdL life cycle. (Is this a correct interpretation?)

On the other hand, this optic was incompatible with operation of initial LIGO; so is the AdL requirement itself perhaps suspect?

No, the average absorption of the ITM that was removed is 12 ppm and far higher than acceptable in either INL or ADL. This high absorption level is due to particulate contamination. The scattering and absorption loss associated with the particulate contamination is a serious concern for ADL (... but that is not in the scope of the subject memo -- others are proposing means to mitigate the particulate problem).

The background level of the contaminated ITM is 1 ppm. Once cleaned the average absorption is 0.7 ppm. There may be a small contribution to the absorption (say ~0.3 ppm) from the uniform deposition of a sub-monolayer contaminant film. This level of contamination may be acceptable for ADL.

Finally maybe part of the argument is that the VE itself (and Flourel seals) is exonerated by the chemical composition of the contamination. Is this a factor in our security? (and indeed, are we taking steps to purge the true offenders, e.g., diallyl phthalate connector bodies if that's what it is)

Mark Anderson (JPL chemist that did the FTIR analysis) says that the contaminant is dially phthalate or a mixture of similar ester based plasticizers that are used in many plastics. Aliphatic hydrocarbons are common oil with a distribution of branched and straight chain alkanes. Since the FTIR sampling occurred after shipment and after months of testing in a plastic sealed container, it is not clear that the contaminant film was deposited while in the vacuum system or subsequently. In addition, only residues which are dissolved in dichloromethane would be detected by the FTIR analysis. I suspect (but am not sure) that this exonerates the flourel spring seats and the viton o-rings. For example, the mold release agents

I think that the only other polymers that we intentionally put in initial LIGO are:

- PEEK (connectors and threads in the ribbon cable)
- Kapton (insulation)
- PTFE Teflon (Kunert wire insulation)
- PTFE 440HP (low evolvable fluorine version of Teflon used for a connector)
- vacseal epoxy (for bonding magnets and standoffs)

I'm no chemist, but I don't think any of these materials have plasticizers added.

One contaminant that may have entered the vacuum system unintentionally and which might have plasticizers, are the flexible plastic soles of the clean room shoes which are known to abrade on the rough chamber floor.

I should add in the interest of full disclosure that for ADL we have more polymers that we intend to put into the vacuum system, including:

- a black dyed Teflon (for cable insulation associated with ADE capacitance position probe)
- two types of low-outgassing epoxy (potting compounds associated with the ADE position probe & the PSI electro-magnetic actuator)
- a flexible circuit (DuPont Pyralux Series - Kapton / Acrylic Adhesive system) for use in the OSEM
- liquid crystal polymer per MIL-M-24519 for connector (Glenair micro-D) for the OSEM

All of the high-irradiance cavity exposure testing have passed these materials.

BTW I looked at the SEM document and it looks like it is missing the pictures.

The pictures are included as a pdf attachment to the pdf file. However it seems (for me at least) if I open the link within Adobe I can't see the attachment. However if you open the link in a browser, you will see the attachment:

<http://www.ligo.caltech.edu/docs/T/T060061-00.pdf>

Also, if there's a technical report in preparation I wonder if we could have a look at a draft, or if a decision is really needed on a timescale faster than the report can be completed?

We need to make a decision ASAP so that we can figure out how to accommodate the bake out costs for the upcoming ADL baseline review.

[Quoted text hidden]

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**Peter Fritschel <pf@ligo.mit.edu>**

**Wed, Mar 29, 2006 at 7:21 AM**

To: Dennis Coyne <coyne@ligo.caltech.edu>

Cc: Michael Zucker <zucker\_m@ligo.mit.edu>, John Worden <worden@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, aligo\_sys@ligo.caltech.edu, Bill Kells <kells@ligo.caltech.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Rai Weiss <weiss@ligo.mit.edu>, Fred Raab <fjr@ligo-wa.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, lsc-owg@ligo.caltech.edu, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>

On Tue, 28 Mar 2006, Dennis Coyne wrote:

> At 04:30 PM 3/28/2006, Michael Zucker wrote:

> Dennis-

>

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> The background level of the contaminated ITM is 1 ppm. Once cleaned the average absorption is 0.7 ppm. There may be a small contribution to the absorption (say ~0.3 ppm) from the uniform deposition of a sub-monolayer contaminant film. This level of contamination may be acceptable for ADL.

>

>

I think the text in the CR is kind of misleading. For example, it says that the absorption and scattering losses for the H1 ITMs are normal, following replacement and cleaning. What is normal? I would have said normal absorption is below 1 ppm (0.5-0.6ppm is our canonical number), and normal scattering (of the type measured) would be below 10 ppm (1-2 angstrom microroughness). But the absorption measurements showed about 2ppm for ITMY (above 'normal'), and an upper limit on ITMX of about 1ppm (maybe 'normal'). The scatter measurements suggest total scatter loss of several tens of ppm (up to 70 ppm), certainly above 'normal'.

Also the text says that after cleaning, 4IMT07 shows an absorption of 1ppm, whereas you say above it is 0.7ppm. The difference is important for AL power levels -- which is it?

I think this last point is most relevant for the issue at hand. The bulk of the 4ITM07 absorption was due to particulates, but what I hadn't known was that there may have also been a uniform contaminant contributing 0.3ppm of absorption. This is big enough to worry about for AL, and says to me that we need more data -- the witness mirrors that were put in at LHO will help with this.

And an FYI -- diallyl phthalate was the insulator material in the Positronic D-connectors that we used in the PNI vacuum system. It passed the RGA testing at the time; I don't know if this material was ever put into a contamination cavity test, but it might have and would be worth looking into the test records.

Peter

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**Helena Armandula <ahelena@ligo.caltech.edu>**

**Wed, Mar 29, 2006 at 7:30 AM**

To: raab\_f@ligo-wa.caltech.edu, Dennis Coyne <coyne@ligo.caltech.edu>

Cc: Fred Raab <fjr@ligo-wa.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, Rai Weiss <weiss@ligo.mit.edu>, John Worden <worden@ligo.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, Bill Kells <kells@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>, aligo\_sys@ligo.caltech.edu, lsc-owg@ligo.caltech.edu

Attached is a recent article on the origin of plasticizers.

Helena

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 **Plasticizers.pdf**  
378K

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**David Reitze <reitze@phys.ufl.edu>**

**Wed, Mar 29, 2006 at 8:08 AM**

To: Helena Armandula <ahelena@ligo.caltech.edu>, raab\_f@ligo-wa.caltech.edu, Dennis Coyne <coyne@ligo.caltech.edu>  
Cc: Fred Raab <fjr@ligo-wa.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, Rai Weiss <weiss@ligo.mit.edu>, John Worden <worden@ligo.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, Bill Kells <kells@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>, aligo\_sys@ligo.caltech.edu, lsc-owg@ligo.caltech.edu, aligo\_io@ligo.caltech.edu

Helena, et al.,

Interesting report.

I'll venture a guess that at some point in the life cycle of the COC mirrors before entering the vacuum, they were sitting in a non-metallic container that exposed the mirror to them to polymeric materials (certainly this was true of the IO mirrors, which were shipped to us in plastic containers by REO). And the H1 ITM\_Y was cleaned with methanol (presumably from a squeeze bottle).

Certainly hot spots due to sticking particulate matter will be problematic for Adv LIGO, but the sub-nm level coating that Dennis referred to is equally problematic. The whole problem of thermal compensation gets easier if the coating absorption levels can be reduced to 0.4 ppm (best effort to date by LMA for silica/tantala).

For AdL, real effort (and \$) will need to be spent on maintaining the integrity of the surface of the mirrors from the coating chamber to 'first light'. In light of comments from others, this seems more important than baking the beam tube.

My two cents,

Dave

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David Reitze

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**Lazzarini Albert <lazz@ligo.caltech.edu>**

**Wed, Mar 29, 2006 at 8:19 AM**

To: Peter Fritschel <pf@ligo.mit.edu>, Dennis Coyne <coyne@ligo.caltech.edu>

Cc: Michael Zucker <zucker\_m@ligo.mit.edu>, John Worden <worden@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, aligo\_sys@ligo.caltech.edu, Bill Kells <kells@ligo.caltech.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Albert Lazzarini <lazz@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Rai Weiss <weiss@ligo.mit.edu>, Fred Raab <fjr@ligo-wa.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, lsc-owg@ligo.caltech.edu, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>

Is the level of contamination accretion observed after the long term in situ exposure such that the RGA and contamination cavity tests would have detected it?

On 29 Mar 2006, at 7:21 AM, Peter Fritschel wrote:

> And an FYI -- diallyl phthalate was the insulator material in the  
> Positronic D-connectors that we used in the PNI vacuum system. It  
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> the RGA testing at the time; I don't know if this material was ever  
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> into a contamination cavity test, but it might have and would be worth  
> looking into the test records.  
>

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626-304-9834 (Facsimile)

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---

**Dennis Coyne <coyne@ligo.caltech.edu>**

**Wed, Mar 29, 2006 at 12:57 PM**

To: Helena Armandula <ahelena@ligo.caltech.edu>, raab\_f@ligo-wa.caltech.edu

Cc: Fred Raab <fjr@ligo-wa.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, Rai Weiss <weiss@ligo.mit.edu>, John Worden <worden@ligo.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, Bill Kells <kells@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>, aligo\_sys@ligo.caltech.edu, lsc-owg@ligo.caltech.edu

Good article Helena. Seems we should seriously consider replacing our plastic solvent dispenser bottles with glass bottles.

Dennis

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**Doug Cook <cook\_d@ligo-wa.caltech.edu>**

**Wed, Mar 29, 2006 at 2:05 PM**

To: Dennis Coyne <coyne@ligo.caltech.edu>

Cc: Helena Armandula <ahelena@ligo.caltech.edu>, raab\_f@ligo-wa.caltech.edu, Fred Raab <fjr@ligo-wa.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, Rai Weiss <weiss@ligo.mit.edu>, John Worden <worden@ligo.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, Bill Kells <kells@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>, aligo\_sys@ligo.caltech.edu, lsc-owg@ligo.caltech.edu

Dennis,

Along with that same thinking we need to be using spectral grade solvents and not just reagent grade. It made the difference with cleaning ITMy.

Doug

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**Michael Zucker <zucker\_m@ligo.mit.edu>**

**Wed, Mar 29, 2006 at 3:04 PM**

To: Doug Cook <cook\_d@ligo-wa.caltech.edu>

Cc: Dennis Coyne <coyne@ligo.caltech.edu>, John Worden <worden@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, raab\_f@ligo-wa.caltech.edu, Helena Armandula <ahelena@ligo.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, aligo\_sys@ligo.caltech.edu, Bill Kells <kells@ligo.caltech.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Rai Weiss <weiss@ligo.mit.edu>, Fred Raab <fjr@ligo-wa.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, lsc-owg@ligo.caltech.edu, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>

Hmmm...I thought this all was standard procedure... isn't it?

In the early 80's we standardized on using only spectral grade reagents from freshly opened bottles (we wasted the rest of the bottle, or used it as paint thinner, after discovering the remainder absorbed crud from the air once open). We also used glass syringe or drop dispensers, and Kodak lens tissues.

Please tell me we aren't cutting corners here!

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**John Worden <worden\_j@ligo-wa.caltech.edu>**

**Wed, Mar 29, 2006 at 3:52 PM**

To: Michael Zucker <zucker\_m@ligo.mit.edu>, Doug Cook <cook\_d@ligo-wa.caltech.edu>

Cc: Dennis Coyne <coyne@ligo.caltech.edu>, John Worden <worden@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, raab\_f@ligo-wa.caltech.edu, Helena Armandula <ahelena@ligo.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, aligo\_sys@ligo.caltech.edu, Bill Kells <kells@ligo.caltech.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Rai Weiss <weiss@ligo.mit.edu>, Fred Raab <fjr@ligo-wa.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, lsc-owg@ligo.caltech.edu, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>

Mike,

This is the Hanford contamination control plan. It refers to "60 and 250 ml dispensing bottles" but does not specify glass. As long as I can remember we have used plastic bottles. - and the filled bottles may sit for a long period of time before use. Only reagent grades are spelled out in the appendix.

[M990034-C.pdf](#)

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**David Shoemaker <dhs@ligo.mit.edu>**

**Wed, Mar 29, 2006 at 5:52 PM**

To: David Reitze <reitze@phys.ufl.edu>, Helena Armandula <ahelena@ligo.caltech.edu>, raab\_f@ligo-wa.caltech.edu, Dennis Coyne <coyne@ligo.caltech.edu>

Cc: Fred Raab <fjr@ligo-wa.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, Rai Weiss <weiss@ligo.mit.edu>, John Worden <worden@ligo.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, Bill Kells <kells@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>, aligo\_sys@ligo.caltech.edu, lsc-owg@ligo.caltech.edu, aligo\_io@ligo.caltech.edu

Do we have any way of judging how contaminated the vacuum equipment might be -- have we taken any wipes or tried to do an FTIR recently? if this is practical but not yet done, it should be on checklists for any (unhoped-for) vacuum incursions. The witness plates are of course also interesting, but I think they have not been in for long. Could we do the same with the as-yet unused unopened spares we have, to try to determine if there are contaminants on the surfaces of the optic or the carrier and if that could

explain what we see on the mirrors?

Can we make a guess, if the original source of the contaminants are in fact in the detector parts in the vacuum, how much of the problem remains after we remove the 'dirty' detector parts? e.g., if we assume that there is a coating of net absorptivity of 0.3 ppm on the walls of the chambers, how long it would take to re-plate back on to the new clean optics?

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John's comment on the risks of a bakeout are interesting. My sense on the balance is that we should remove the bakeout from the baseline plan. We would be left with the risk that we later decide to bake out; it would be a valid (and significant) call on contingency, and could be carried out if needed. We should lay out a plan to collect additional information to better assess the risk, with a schedule that is tied to critical points in our schedule -- e.g., 1 year from now for our final baseline review, and then again at the point where we would need to start to mobilize a bakeout.

And we should certainly be using glass bottles!

d.

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**Bill Kells <kells@ligo.caltech.edu>**

**Wed, Mar 29, 2006 at 6:14 PM**

To: Michael Zucker <zucker\_m@ligo.mit.edu>, Doug Cook <cook\_d@ligo-wa.caltech.edu>  
Cc: Dennis Coyne <coyne@ligo.caltech.edu>, John Worden <worden@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, raab\_f@ligo-wa.caltech.edu, Helena Armandula <ahelena@ligo.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, aligo\_sys@ligo.caltech.edu, GariLynn Billingsley <gari@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Rai Weiss <weiss@ligo.mit.edu>, Fred Raab <fjr@ligo-wa.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, lsc-owg@ligo.caltech.edu, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>

To all,

Yes, I too vividly recall the "ultra" cleaning days of the 40m!

However I believe a confusion is evolving with regard to fear of contaminant films (also the article unearthed by Helena, the trace residues detected on LIGO I HRs by JPL ftir, and the proposal to apply a commercial strippable protective coating to ADL HRs). Certainly the LIGO I optics will always be found to have *some* chemical residue on them (given the far from stringent chemical grades, environment, storage, and procedures we have de facto adopted). Back in the era of 514 nm ifo light, any level of organic contamination became justifiably

synonymous with absorptive loss. Although we understood that this association would be [much] less at 1064nm, I believe it has only slowly become apparent through the now considerable LIGO I history how dramatically less sensitive we are to modest environmental contamination. By 514nm 40m standards it takes one's breath away to even consider applying some commercial, solvent based polymer coating to Adl optics! Yet it may be that the almost certain trace residue this will leave is entirely acceptable. Similarly, the trace residues we find via FTIR on our current optics very likely have no bearing on the absorption levels we operationally contend with. These residues indeed may originate in the grade of solvent, its age and bottles stored. However, at ~ monolayer surface coverage level they probably do not contribute to 1064 absorption.

A mechanism I do worry about is "carbonized" dust (of some chemical content that does not itself absorb at 1064nm). R. DeSalvo has pointed out that surface contaminant particles ("dust") in poor thermal contact with the coating/substrate can anomalously heat up (burn?). Then one is left with a semi *conductive* residue thread which can be very lossy (in a broad band way !). This can be very little: one 10micron<sup>2</sup> [completely] absorptive particle per cm<sup>2</sup> of HR surface could induce the level of performance degradation seen in the H1 X arm.

Bill K.

At 03:04 PM 3/29/2006, Michael Zucker wrote:

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**Michael Zucker <zucker\_m@ligo.mit.edu>**

**Wed, Mar 29, 2006 at 6:10 PM**

To: John Worden <worden\_j@ligo-wa.caltech.edu>

Cc: Doug Cook <cook\_d@ligo-wa.caltech.edu>, Dennis Coyne <coyne@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, John Worden <worden@ligo.caltech.edu>, raab\_f@ligo-wa.caltech.edu, Helena Armandula <ahelena@ligo.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, aligo\_sys@ligo.caltech.edu, Bill Kells <kells@ligo.caltech.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Rai Weiss <weiss@ligo.mit.edu>, Fred Raab <fjr@ligo-wa.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, lsc-owg@ligo.caltech.edu, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>

John- I understand this doc to cover internal vacuum components (metallic, glass, etc.). I was talking specifically of optical surfaces (whether inside or outside vacuum). I guess I don't know if we have a separate document covering optic cleaning procedures...at one point I think the folklore below was in the appendices of some optics catalog, maybe it was Newport or Melles Griot?

On Mar 29, 2006, at 17:52, John Worden wrote:

> Mike,  
> This is the Hanford contamination control plan. It refers to "60 and  
> 250 ml dispensing bottles" but does not specify glass. As long as I  
> can remember we have used plastic bottles. - and the filled bottles  
> may sit for a long period of time before use. Only reagent grades are  
> spelled out in the appendix.  
>  
> M990034-C.pdf  
>  
> At 03:04 PM 3/29/2006, Michael Zucker wrote:  
>> Hmmmm...I thought this all was standard procedure... isn't it?  
>>  
>> In the early 80's we standardized on using only  
>> spectral grade reagents from freshly opened bottles (we wasted  
>> the rest of the bottle, or used it as paint thinner, after  
>> discovering the  
>> remainder absorbed crud from the air once open). We also used glass  
>> syringe or drop dispensers, and Kodak lens tissues.  
>>  
>> Please tell me we aren't cutting corners here!  
>>

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**Riccardo DeSalvo <desalvo@ligo.caltech.edu>**

**Wed, Mar 29, 2006 at 8:48 PM**

To: Bill Kells <kells@ligo.caltech.edu>

Cc: Michael Zucker <zucker\_m@ligo.mit.edu>, Doug Cook <cook\_d@ligo-wa.caltech.edu>, Dennis Coyne <coyne@ligo.caltech.edu>, John Worden <worden@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, raab\_f@ligo-wa.caltech.edu, Helena Armandula <ahelena@ligo.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, aligo\_sys@ligo.caltech.edu, GariLynn Billingsley <gari@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Rai Weiss <weiss@ligo.mit.edu>, Fred Raab <fjr@ligo-wa.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, lsc-owg@ligo.caltech.edu, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>

Somehow I tend to believe what Bill says.

After all we successfully cleaned and reduced the thermal absorption on the unremoved ITM with the same, supposedly plastic-bottle-contaminated, solvent.

And the replacement mirror, which also behaves well, was probably cleaned with similarly "polluted" solvent before installation.

If the supposed residue was so bad, how could we have achieved such a low absorption?.

And if just keeping the solvent in glass bottles was to dangerously contaminate the solvent, all surfaces of all mirrors would be badly absorbing, which is obviously not the case.

What strikes me most is that:

- 1) on the extracted mirror we found particulates that are compatible to having the correct amount of absorption.
- 2) the ITM that remained inside was successfully cleaned with a quick drag wipe
- 3) a uniform film was observed on the AR side and removed by many drag wiping.
- 4) no film was observed after drag wiping with "contaminated" solvent.

Somehow I tend to believe that the film, had other origins (to be on one side only must have had an evaporative and projective origin) and that it is less important.

Finally, the only event of well documented bad absorption on the two ITM was connected with particulates. Therefore I am much more worried of them, since we have proof that they harm us, than of traces of plasticizers, of which we have evidence that they probably do not, or do much less harm.

Ric

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**David Shoemaker <dhs@ligo.mit.edu>**

**Thu, Mar 30, 2006 at 6:03 AM**

To: John Worden <worden\_j@ligo-wa.caltech.edu>, Dennis Coyne <coyne@ligo.caltech.edu>, Fred Raab <fjr@ligo-wa.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, Rai Weiss <weiss@ligo.mit.edu>, John Worden <worden@ligo.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>  
Cc: GariLynn Billingsley <gari@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, Bill Kells <kells@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>, aligo\_sys@ligo.caltech.edu, lsc-owg@ligo.caltech.edu

John, do we have any RGA measurements of the current vacuum which can be compared with the pre-installation and the immediate post-installation state? I don't know if this would tell us anything useful about the contamination, but it would be nice to put a number to the notion that it is not worse now in any way we can in fact measure it.

David

At 19:48 28-03-06, John Worden wrote:

I support the removal of the bake-out from the plan for the following reason:

We have no proof that the vacuum quality is any worse than it was when we took delivery from PSI. If we were to rebake with the same care that was taken originally, we may or may not do better than PSI. Also, a single failure of a heater circuit could overheat an o-ring and distribute the outgassing products throughout the vacuum system. Likewise, a mechanical failure

resulting from thermal cycling could end in repair work with risks of further contamination and negating of the bake. In other words, I feel the risks are great and even a "successful" bake would very likely leave us where we are now.

John

At 03:27 PM 3/28/2006, Dennis Coyne wrote:

LIGO Vacuum Review Board:

Attached is a memo requesting that the planned ADL vacuum system bake-out be removed from the baseline. Please review and comment. We would like to remove this activity and cost from the plan if technically warranted. A quick reply would be appreciated. Thank you,

Dennis

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**Eric D. Black <black\_e@ligo.caltech.edu>**

**Thu, Mar 30, 2006 at 10:56 AM**

To: Michael Zucker <zucker\_m@ligo.mit.edu>

Cc: Doug Cook <cook\_d@ligo-wa.caltech.edu>, Dennis Coyne <coyne@ligo.caltech.edu>, John Worden <worden@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, raab\_f@ligo-wa.caltech.edu, Helena Armandula <ahelena@ligo.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, aligo\_sys@ligo.caltech.edu, Bill Kells <kells@ligo.caltech.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Rai Weiss <weiss@ligo.mit.edu>, Fred Raab <fjr@ligo-wa.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, lsc-owg@ligo.caltech.edu, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>

This is commonly accepted practice in the atomic physics community.

Cleaning with spectral grade - we always called it "spectroscopic grade" - solvents in glass bottles is standard procedure in most optics labs, as far as I know. We do it at the TNI. The standard argument is that the plastic leeches into the solvent and coats the optic. Scientific supply houses sell glass bottles with glass droppers for just this purpose. I think we got ours from Thorlabs.

Eric

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> Lsc-owg mailing list

> [Lsc-owg@ligo.caltech.edu](mailto:Lsc-owg@ligo.caltech.edu)

> <http://mm.ligo.caltech.edu/mailman/listinfo/lsc-owg>

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**Helena Armandula <ahelena@ligo.caltech.edu>**

**Thu, Mar 30, 2006 at 11:17 AM**

To: Michael Zucker <zucker\_m@ligo.mit.edu>, Doug Cook <cook\_d@ligo-wa.caltech.edu>

Cc: Dennis Coyne <coyne@ligo.caltech.edu>, John Worden <worden@ligo.caltech.edu>, Helena Armandula <armandula\_h@ligo.caltech.edu>, raab\_f@ligo-wa.caltech.edu, Helena Armandula <ahelena@ligo.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, aligo\_sys@ligo.caltech.edu, Bill Kells <kells@ligo.caltech.edu>, GariLynn Billingsley <gari@ligo.caltech.edu>, Liyuan Zhang <zhang\_l@ligo.caltech.edu>, Dave Reitze <reitze@phys.ufl.edu>, Rai Weiss <weiss@ligo.mit.edu>, Fred Raab <fjr@ligo-wa.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>, lsc-owg@ligo.caltech.edu, Carol Wilkinson <wilkinson@ligo-wa.caltech.edu>

On the subject of optical surface contamination, please, see doc # T050055-00. I performed several test to evaluate optical absorption vs. different cleaning methods.

The tests were done on parts coated at REO and stored for over a year. Even prior to cleaning, there was not signs of high absorption.

We have FTIR tests done in optics removed from the interferometer (ITM04), in a mirror stored in the aluminum cake pan for a long time, 1-2 years and, the levels of contamination are pretty similar (T050143).


Helena


At 03:04 PM 3/29/2006, Michael Zucker wrote:

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**2 attachments**

 **T050055-00-D .pdf**  
124K

 **T050143-00.pdf**  
44K

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**John Worden <worden\_j@ligo-wa.caltech.edu>**

**Thu, Mar 30, 2006 at 12:14 PM**

To: David Shoemaker <dhs@ligo.mit.edu>, Dennis Coyne <coyne@ligo.caltech.edu>, Fred Raab <fjr@ligo-wa.caltech.edu>, David Shoemaker <dhs@ligo.mit.edu>, Rai Weiss <weiss@ligo.mit.edu>, John Worden <worden@ligo.caltech.edu>, Mike Zucker <mike@ligo.mit.edu>

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Dave,

Kyle is looking for PSI's RGA pre-installation results - we may have moved a large stack of PSI docs to a mid station. If so we will look on Tuesday's maintenance.

I am pretty sure that all we will find post installation is faraday mode scans on a wet system. More recently Kyle has managed one or two uncalibrated scans in the LVEA in high sensitivity mode. It makes sense to compare these with PSI's data but keep in mind that there was different plumbing, different RGAs and different motivations. I am not sure that we would be comparing apples with apples.

We'll try to put something together.

John

RAI - have you ever done this comparison at LLO?

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