From: Robyn Conmy/CI/USEPA/US
Sent: 8/6/2012 10:32:28 PM

To: Albert Venosa
CC:
Subject: Re: Fw: Assessment of Ortmann et al 2012 paper

Hi Al, I did see the email from Mace this morning but didn't have time to respond as I went to NHEERL AED this afternoon to meet with PIP 2 collaborators. I read the paper and here is my assessment.
There are a number of issues / questions that I have with the experimental design. (1) The concentration of oil used in the replicates is ridiculously high (500-1000 ppm). This is hard to compare to real world scenarios in the DWH spill. The authors do mention that this is a static dose and is not akin to what organisms may encounter in the real world, but they neglect to mention the high concentrations being unrealistic. (2) the oil used was not weathered. If the concern is about surface organisms then, weathered oil should have been used. (3) Along those lines, they used coastal biota for this experiment, not offshore organisms, so to be representative of dispersed oil that would have made it Alabama's coast, it needed to be very weathered. (4) There is no mention in how they made the chemically dispersed oil. Was it swirled in a flask for 30 sec or was it properly dispersed? (5) The mesocosms had no filtered sunlight. Both DOSS and dispersed oil photodegrade, particularly in summer months in Alabama in surface waters. This pathway could grossly impact the uptake rates and make these results less plausible in the environment. No one knows the impact, so a dark - light experiment would have addressed this important question.
I had some additional concerns about the zooplankton sampling and role of inhibiting ciliates. For that, I deferred to the zooplankton expert in residence, Drew (handy to have one of those). He pointed out that the ~10 ml used to estimate zooplankton abundance is too small to accomplish this. The authors report that zooplankton abundance was below the detection limit, but if you look at the math, 1 zooplanktor in 10 ml is equivalent to ~100000 in a cubic meter. That's a lot of zooplankton. Drew commented that their sampling volume is inadequate to gauge numbers. This is of course important because zooplankton preferentially graze on ciliates. There is no way with their results to know if ciliate inhibition was due to chemically dispersed oil or because they were being heavily grazed in the mesocosms. Their lack of effort to parameterize the grazers undermines the significance of the results.
All the above aside, I do also agree with John and Mike that even if these effects could represent the real word spill, the volume of the gulf impacted with high concentrations of dispersant is small relative to the unimpacted volume and the ciliate carbon transfer would remain in tact.
Cheers,
Robyn
<>/<>/<>/<>/<>/<>/<>/<>/<>/<>/<>/<>/<>/<>/<>/<>/<>/<>/<>/<>
Robyn N. Conmy, Ph.D.
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USEPA/NRMRL/LRPCD
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Cincinnati, Ohio 45268
513-569-7090 (office)
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conmy.robyn@epa.gov
-----Albert Venosa/CI/USEPA/US wrote: -----
To: "Robyn Conmy" <conmy.robyn@epa.gov>
From: Albert Venosa/CI/USEPA/US
Date: 08/06/2012 05:12PM
Subject: Fw: Assessment of Ortmann et al 2012 paper
Here is the article. I haven't had a chance to study it yet.

Albert D. Venosa
Albert D. Venosa, Ph.D.
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National Risk Management Research Laboratory
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Email: venosa.albert@epa.gov
----- Forwarded by Albert Venosa/CI/USEPA/US on 08/06/2012 05:12 PM -----
From: Rick Greene/GB/USEPA/US
To: Albert Venosa/CI/USEPA/US@EPA
Date: 08/06/2012 12:34 PM
Subject: Re: Assessment of Ortmann et al 2012 paper

Here you go. Hope all is well. BTW - Mace is in Portland this week at ESA.
Inactive hide details for Robert KavlockRobert Kavlock
(See attached file: Ortmann PloS Oil.pdf)
Inactive hide details for Albert Venosa---08/06/2012 11:03:03 AM---Hi, Rick, long time, no see! Thanks for the comments below,Albert Venosa---08/06/2012 11:03:03 AM---Hi, Rick, long time, no see! Thanks for the comments below, but could you also send me the Ortman a
From: Albert Venosa/CI/USEPA/US
To: Rick Greene/GB/USEPA/US@EPA
Date: 08/06/2012 11:03 AM
Subject: Re: Assessment of Ortmann et al 2012 paper

Hi, Rick, long time, no see!
Thanks for the comments below, but could you also send me the Ortman article? That's what I don't have.

Albert D. Venosa

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Email: venosa.albert@epa.gov

Inactive hide details for Rick Greene---08/06/2012 10:35:03 AM---Bob, Here are comments from GED staff.Rick Greene---08/06/2012 10:35:03 AM---Bob, Here are comments from GED staff.
From: Rick Greene/GB/USEPA/US
To: Robert Kavlock/DC/USEPA/US@EPA
Cc: Albert Venosa/CI/USEPA/US@EPA, "Mace Barron" <barron.mace@epa.gov>, "bennett rick" <bennett.rick@epa.gov>, Lek Kadeli/DC/USEPA/US@EPA, Robyn Conmy/CI/USEPA/US@EPA
Date: 08/06/2012 10:35 AM
Subject: Re: Assessment of Ortmann et al 2012 paper

Bob,
Here are comments from GED staff.
Comments from John Lehrter;
Funny, I heard a story about this on NPR this morning. I know the lead author, Alice Ortmann. She is pretty good. To me, the results are like so many of these types of studies. There is likely an impact while the dispersant is around at sufficient concentration, but once it's gone these organisms are so plentiful and grow so quickly that I really can't imagine there are long term shifts in their population structures or in the trophic food chain. If you were to continuously pump dispersant or any surfactant into an enclosed system like Pensacola Bay, yes, maybe, there would be a measurable trophic transfer impact. In the open Gulf, though, it doesn't seem likely. As an anecdote, I went fishing last week with my buddy Will Patterson, a fishery biologist at DISL, who is funded to investigate potential impacts to the deep-water pelagic and demersal fish populations. We fished a wide area near the DWH and we slayed them.
Comments from Mike Murrell;
I agree with John's assessment. I find it curious that adding oil and dispersant caused a big uptick in bacterial biomass, but adding 30mM of C as glucose (a HUGE amount, realizing that DOC of 5-10 M is typical for marine waters) had no measurable effect. I looked at their methods and learned that they used flow cytometry to count microbes (prokaryotes and viruses). Being an old microscope person, I have a bit of trouble trusting flow cytometry counts without at least some independent measures as a 'reality' check. It is possible that this result is an artefact of the physical effect that dispersants have on fluid flow characteristics and particle distribution. I've used dispersants for microscope preps to more evenly disperse bacteria on the surface of the filter (reduces clumping/aggregation)....maybe the Corexit had the same effect and somehow screwed the poop. I don't think they took the approp. controls to rule out that possibility (i.e. take samples immediately after adding dispersant but before bugs can respond). Probably WAY too much detail for DC folks, but that's my 2c. In the final analysis, notwithstanding these intriguing results, it stretches credulity that marine microbial food webs could be profoundly/persistently altered in such a large open system.
****

Dr. Richard M Greene
Acting Director
Gulf Ecology Division
National Health and Environmental Effects Research Laboratory
US EPA, Office of Research and Development

1 Sabine Island Drive., Gulf Breeze FL 32561-5299
tel: 850-934-2497; cell: 850-426-2532
Inactive hide details for Mace Barron---08/06/2012 09:23:54 AM---Rick Greene had shared those, and am sure can compile for you.Mace Barron---08/06/2012 09:23:54 AM---Rick Greene had shared those, and am sure can compile for you. Sent by EPA Wireless E-Mail Services
From: Mace Barron/GB/USEPA/US
To: Robert Kavlock/DC/USEPA/US@EPA
Cc: "Mace Barron" <barron.mace@epa.gov>, "bennett rick" <bennett.rick@epa.gov>, Robyn Conmy/CI/USEPA/US@EPA, Lek Kadeli/DC/USEPA/US@EPA, Albert Venosa/CI/USEPA/US@EPA, "Rick Greene" <Greene.Rick@epamail.epa.gov>
Date: 08/06/2012 09:23 AM
Subject: Re: Assessment of Ortmann et al 2012 paper

Rick Greene had shared those, and am sure can compile for you.
Sent by EPA Wireless E-Mail Services
Inactive hide details for Robert KavlockRobert Kavlock
----- Original Message -----

From: Robert Kavlock

Sent: 08/06/2012 09:22 AM EDT

To: Mace Barron

Cc: "Mace Barron" <barron.mace@epa.gov>; "bennett rick" <bennett.rick@epa.gov>; Robyn Conmy; Lek Kadeli; Albert Venosa

Subject: Re: Assessment of Ortmann et al 2012 paper

Mace
Can you get us the comments by John/Mike on eco?
Thanks
Bob
Inactive hide details for Mace Barron---08/05/2012 12:55:11 PM---There were some good comments from a couple of the GED water qMace Barron---08/05/2012 12:55:11 PM---There were some good comments from a couple of the GED water quality experts (John, Mike), so I defe
From: Mace Barron/GB/USEPA/US
To: Robert Kavlock/DC/USEPA/US@EPA, "bennett rick" <bennett.rick@epa.gov>, "Mace Barron" <barron.mace@epa.gov>, "Lek Kadeli" <Kadeli.Lek@epamail.epa.gov>
Cc: "Robyn Conmy" <Conmy.Robyn@epamail.epa.gov>, "Albert Venosa" <Venosa.Albert@epamail.epa.gov>
Date: 08/05/2012 12:55 PM
Subject: Assessment of Ortmann et al 2012 paper

There were some good comments from a couple of the GED water quality experts (John, Mike), so I defer to them on the ecological perspective.
From a tox perspective, I had a few issues that should have been addressed prior to publication, listed below.
Bottomline for me: the paper is one of the hundreds of papers that will come out from the spill. The results of these will likely be dependent on the specific conditions of the experiments. For this one, the environmental relevance is highly uncertain.
1) I did not see where the oil was weathered prior to experimental application.
*this makes no sense, as all the oil reaching the surface was at least 20 percent weathered. This affects the hydrocarbon composition and thus there is a large uncertainty if the results are relevant.
2) It was unclear what mixing enegy was used (seems to be very low), which would have a significant effect on droplet size and dissolved phase hdrocarbon composition
*note that dispersant application guidelines require a minimal sea state for effective mixing.
*I think this was a fatal flaw
3) Its unclear if the oil droplet data are valid.
*i could not tell how they calibrated the instrument or accuracy of the results because generally required information were missing.
*they used a cutoff of 30 um: Not sure how it would influence interpretation if they had used a higher cutoff
4) Unfortnately, the results maybe more an artifact of the experimental design.
*with the lack of weathering and apparent low mixing energy, the relevance of the particle distribution /hydrocarbon composition to oil exposures during the spill is unclear
*not sure if they are extrapolatable beyond the test conditions
Sent by EPA Wireless E-Mail Services
Inactive hide details for Robert KavlockRobert Kavlock
----- Original Message -----

From: Robert Kavlock

Sent: 08/01/2012 03:57 PM EDT

To: bennett.rick@epa.gov; barron.mace@epa.gov
Subject: dispersant effect publication

Rick/Mace
The attached article was featured in an article in the New Orleans press this week and it have caught the attention of some here in DC. Lek asked that you guys check into it and get some feedback. Beyond the article, is there any evidence in the Gulf that such an impact may have actually happened?
Thanks
Bob
[attachment "Ortmann PloS Oil.pdf" deleted by Mace Barron/GB/USEPA/US]
[attachment "Ortmann PloS Oil.pdf" removed by Robyn Conmy/CI/USEPA/US]