

From: Fred Dobbs <[REDACTED]>

Subject: Continued discussion--All former members of the SAB Panel on ballast water treatment technology

Date: February 13, 2015 11:16:38 AM PST

To: Fred Benfield [REDACTED], Edward Lemieux

[REDACTED], Jim Sanders <[REDACTED]>, Lisa Drake [REDACTED], Kevin Reynolds [REDACTED], JoAnn M. Burkholder [REDACTED], Andrew Cohen [REDACTED], Allen Burton [REDACTED], William Clements [REDACTED], L. Conquest [REDACTED], Charles Haas [REDACTED], Thomas La Point [REDACTED], Wayne Landis [REDACTED], David Lodge [REDACTED], Judith Meyer [REDACTED], Mario Tamburri [REDACTED], Nick Welschmeyer [REDACTED], Chapman, Peter (Canada) [REDACTED], Robert Diaz [REDACTED] Amanda Rodewald [REDACTED]

Dear former SAB Panel members,

This is all helpful, open discussion.

As Lisa and Mario indicate, last October, after we discovered the error in the SAB Ballast Water Report's conclusion, Fred sent that information (as contained in our February 2 email message to former panel members) to Mario (as the chair of Subgroup 1, which analyzed the test data and produced the conclusion) and to Lisa (also a member of Subgroup 1). Fred then had a conversation with Mario, from which Fred emerged with the understanding that Subgroup 1 had conducted an analysis demonstrating there wasn't sufficient statistical confidence to conclude that any of the treatments met the 10x IMO D-2 standard, despite test data that appeared to show some treatment systems had met that standard.

The methods described in Chapter 4 of the SAB Report—the chapter prepared by Subgroup 1 members describing their analysis—do not mention a statistical approach. Instead, the methods indicate that treatment systems were scored as either meeting or not meeting a particular standard, based on whether the sample concentrations reported in the test data were below or above the concentrations defining the standard.

Andy wrote Mario, reporting Fred's understanding that Subgroup 1 had rejected, because of insufficient statistical confidence, test data that appeared to show some treatment systems met 10x IMO D-2. Andy asked if Fred's understanding was correct, and if so, what confidence level Subgroup 1 had used to distinguish between data that provided sufficient confidence and data that did not.

Mario didn't answer either question. Instead, he asked what our objective was. Andy replied that the test data appeared to show that the Report's conclusion was in error, and we felt that if the conclusion were wrong, we should acknowledge the error and correct it. Andy again asked Mario what confidence level Subgroup 1 had used to distinguish between data that provided sufficient confidence and data that did not. Mario did not reply.

That brought us to November. (We should mention that we also sent the information to Judy Meyer, who replied that she had talked to Mario and to Tom Armitage at EPA, and didn't believe there was any error.) With our discussion with Mario in mind, Andy re-examined the test data. As explained in our Feb. 2 email message, the spread in performance demonstrated by the five treatment types was so wide that no confidence level would support a finding that all five treatment types met the IMO D-2 standard, but none met the 10x IMO D-2 standard. Wanting to be sure of this conclusion before writing the panel, we sent the analysis to Loveday Conquest, who reviewed and verified it. (This analysis can be downloaded at www.bioinvasions.com/sab, along with other supporting documents.)

So that's where things stand. To summarize, we argue that the test data Subgroup 1 reported as reliable in fact show some treatment systems met the 10x IMO D-2 limits (for example, all trials of the Ecochlor system yielded concentrations of one-third or less than the 10x IMO D-2 limit for >50 micron organisms, and all trials of the Ecochlor, BalPure and Peraclean systems yielded concentrations of less than one-ninth of the 10x IMO D-2 limit for 10-50 micron organisms). When shown these data, Mario implied Subgroup 1 had done a statistical assessment that led it to reject this argument, but he would not answer questions about the assessment. The methods described in the SAB Report do not mention any such assessment, and statistical considerations, checked by Loveday, show that no such assessment could have supported the conclusion in the Report. Mario and Lisa have responded, criticizing our concerns, but without explaining how Subgroup 1 reached its conclusion in the face of data showing otherwise, and stating they have no time to discuss this matter further.

Mario, Lisa, and the other former panel members who have declined to join in a correction letter all stated in one way or another that a new analysis should be done, one that includes the additional test data that have become available since the Panel did its work. We agree wholeheartedly. But a new analysis is not a substitute for correcting the error we made. The SAB Report's erroneous conclusion has been cited by EPA and USCG as the basis for the discharge limits they adopted, and unless corrected, will continue to influence policy until a new analysis is conducted.

Are there questions or comments from panel members who have not yet weighed in?

Sincerely,
Fred Dobbs and Andy Cohen