**Bureau of Safety and Environmental Enforcement Comment Response Document**
**OSRR Project #1016 Comparative Testing of Corexit EC9500A, Finasol OSR 52, Accell Clean DWD,**

This is the Bureau of Safety and Environmental Enforcement's (BSEE) Comment Response Document for BSEE's Project #1016. This document addresses the questions and concerns expressed by the External Peer Review Committee. The Peer Review Committee was tasked with evaluating the scientific merit of the research report, the appropriateness of the assumptions made, and the quality and relevance of the data. Per Johan Brandvik, Ph.D., SINTEF, Environmental Technology; Merv Fingas, Ph.D., Spill Science; Julien Guyomarch, Cedre; George Sorial, Ph.D., University of Cincinnati; and Albert Venosa, Ph.D., EPA (retired) provided their expertise in the review of this report.

The Comment Response Document addresses the following:

- BSEE's agreement or disagreement with views expressed in the Peer Review Report
- Reasons those actions satisfy key concerns stated in the Peer Review Report (if applicable)

Particular attention was given to significant comments that were not accepted for incorporation into the research product.

**1. Are the objectives of the report clearly Defined? If not, what are your recommendations for improving the description of objectives?**

| Reviewer | Reviewer Comment | BSEE Comment Response |
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| Brandvick | The objective of the study is clearly defined (to better understand and compare the effectiveness of various dispersants under simulated arctic test conditions) and very relevant for both the industry and authorities which need to improve Arctic oil spill contingency. | Noted |
| Fingas | Yes, and I believe that the report follows the study objectives. | Noted |
| Guyomarch | The objectives of the study are clearly defined | Noted |
| Sorial | The objectives of the study were clearly defined in the last paragraph of the introduction section. | Noted |
| Venosa | The objectives were presented in one sentence on p. 3, i.e., "This study compared the performance of Corexit and three other commercially available dispersant formulations, as measured by Dispersant Efficiency and the size distribution of dispersed oil droplets in the water column." Alas, the sentence displays early in the report the positive bias towards Corexit. Rather, it should state something like "This study compared the performance of four commercially available dispersant formulations…" So, the objectives were indeed clear but imposed a preconceived bias toward one dispersant just because it has been the most tested one on the NCP Product Schedule. All biases should be eliminated before BSEE publishes this report. | The perception of a bias is acknowledged. Multiple statements have been revised in the final version. |

**2. Was the methodology used to define the selection and testing conditions of the four dispersants clearly described?**

| Reviewer | Comment | BSEE Comment Response |
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| Brandvik | No, C9500/OSR-52 were selected due to their large volume in U.S. stockpiles, but the only criteria listed for selecting the other two products was because they were "not as well known". Selecting all products based on stockpiled amounts would be a better approach. | Additional details were provided in regards to the selection of the products tested (pg. 7) |
| Fingas | Yes, and the selection did follow the criteria set down. | Noted |
| Guyomarch | The methodology used to select the dispersants is clear, but the choice of the testing conditions remains largely unclear. The characteristics of the test oil were related to other arctic crudes but based on a very limited number of parameters (API gravity and sulfur content), whereas, in terms of behavior at sea, asphaltenes and wax contents, distillation curve, viscosity… are as relevant. | ANS crude was ordered for this test program, but did not arrive on time. Due to tank availability and weather constraints, testing was conducted with a blend of oils already in stock at Ohmsett as described in the report. This information will be added to the "Test Oil" section on page 9. |

| 2. Was the methodology used to define the selection and testing conditions of the four dispersants clearly described? | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Guyomarch | In addition, the pour point of the oils seems high in comparison with the test temperatures (and too high when considering dispersibility studies). A preliminary study should have been conducted in order to better identify the most important parameters and to get lower dispersant efficiencies. And the control experiment should have led to poor dispersibility. | The properties of the oil blend used for this experiment were independently tested, with results on pg. 9. The oils used for this experiment had been in storage for some time, and had likely weathered before testing. For this reason, the full properties of the oil were presented in this paper. The goal of this test program was a side-by-side comparison of the dispersant products and oil properties was not treated as a variable. |
| Sorial | The methodology used to define the selection and testing conditions were clearly defined in methods, test oil, and testing procedures. | Noted |
| Venosa | The methodology of selection was simple and not fully explained. The investigators confined their selections to the two most popular dispersants on EPA's Product Schedule and two other lesser known products without regard to whether any of the selected products have ever been reported in peer-reviewed literature citations either in lab, mesocosm, or field studies. This implies the researchers are unfamiliar with the literature. It also implies that the researchers did not study the MSDS sheets of the Product Schedule products that describe in greater detail how the products differ in constituents, properties, and performance from one another. Had they done any of this, they might have confined their selections to more effective products a priori rather than a non-effective one like ZI 400 (despite the fact that the latter product is on the Product Schedule). | Originally, the plan was to test Corexit, Finasol, Accell, and Dispersit. All of which appear in literature with positive results. Unfortunately, as documented in the report, Dispersit had operational issues which did not allow it to participate in the test. (pg. 23) This was not realized until shortly prior to the start of the tests. ZI400 was able and willing to supply us with product in the short time frame. Also, it was felt appropriate to test a relatively "unknown" product in a realistic setting since it has been listed on the NCP Product Schedule of approved products with similar or better effectiveness to the other products (EPA NCP). |

| 3. Were the testing procedures used appropriately describe and properly implemented? | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Brandvik | The Ohmsett facility offers unique possibilities regarding near field conditions for dispersant testing and the description of the experimental procedures are clear and concisely described. All the data needed to evaluate the data analysis and conclusions in the report are available in tables in the report and in the appendices. | Noted |
| Fingas | The report followed the established procedures; however, comments should have been made on the need to heat the dispersant. It is unclear if the oil was heated and at what pressure it was discharged, further the viscosity of the oil appears to be only calculated. | The need to add heat to the dispersants for consistent spraying is detailed on pg. 11. The physical measurement of the oil viscosity at standard temperature and on a temperature gradient are detailed on pg. 9. The oil was stored and applied at ambient temperature. |

| 3. Were the testing procedures used appropriately describe and properly implemented? | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Fingas | The time after the test (20 min.) that sweeping was started is too short and would result in missing much of the resurface oil. | Sweeping was conducted immediately following the 20min test duration. This was in an attempt to collect the oil which remained on the surface during the 20min when the waves were introducing mixing energy. This project was designed to conduct a side-by-side comparison of dispersants. As such, all dispersants were exposed to the same conditions. This is consistent with previous DE tests conducted at Ohmsett. (SLRoss 2011, 2009, 2008, 2007) |
| Guyomarch | The testing procedure was well explained but some parameters of the experiments were not consistent with the methodology. In particular, for the Finasol OSR 52, the Dispersant to Oil Ratio was the same for any conditions of dosage (lowest, mean, and highest). A scheme of the test protocol would also have been added, with the location of the oil slick and LISST equipment. The protocol for the oil concentrations measurements (type of measurement, calibration, uncertainty…) should have been better described, as well as the method used to assess the remaining oil at the end of each experiment. | Pump rates were set so a DOR of 1:20 would be applied if the full volume of dispersant was applied to the full volume of oil. Due to the operational realities of Ohmsett, some of the dispersant missed the slick and the actual DOR applied was calculated through the measurements described in the report. Additional details were added to pg. 12. LISST location information was added to appendix A and the "test Procedures" section of the report. |
| Sorial | The testing procedures described within the report and Appendix A were described accurately and properly implemented. | Noted |
| Venosa | The testing procedures were adequately described, but unfortunately not in an appropriate fashion. What I mean by this is that recoalescence is an extremely important outcome of using any oil dispersant. Droplet sizes < 70 µm should generate droplets that are neutrally buoyant and will not likely recoalesce. However, a good scientific method should attempt to confirm this outcome by studying it in greater detail than described rather than assuming it should. Halting the wave experiments after 20 min and then collecting whatever oil was not dispersed does not allow sufficient time for recoalescence to take place, which would add to the amount of oil that was not dispersed to rise and be collected with the rest of the non-dispersed oil. Granted, three more hours were allowed to pass by before preparing for the next run, but no description was presented that any more recoalesced oil was collected at that time. I would recommend that a full 24 hours should be allowed to pass before confirming no more oil surfacing from the water column. This was another major weakness in the report. | While BSEE agrees that the rapid collection of surface oil does not allow for recoalescence of droplets, data collected showed the ability of the products to create droplets at the initial breakup of the oil under a relatively consistent wave action. It can be assumed that 'Dispersed' oil clouds containing larger droplets will see much more coalescence than one with a smaller median droplet size. Additionally Ohmsett is an outdoor wave tank affected by wind induced currents as well as a 6" per minute current caused by the filtration system. Long term periods between testing and collection would not be logistically possible. This method is consistent with previous DE tests conducted at Ohmsett. The goal of testing was not to establish a definitive dispersant effectiveness rating for any product, but rather to establish a dispersant effectiveness rating for the test conditions. This allowed for a side-by-side comparison of the products; the goal of the project. (SLRoss 2011, 2009, 2008, 2007) |

| 4. Were the results of the sampling and statistical testing adequately characterized and clearly described? Were there any critical results or limitations that were not discussed or addressed in the report? | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Brandvik | The definition of Dispersant Effectiveness (DE) and volume fraction below 70 microns are well defined and relevant. The documentation of released oil, applied dispersant and DOR are all relevant and well described. The documentation of droplet sizes measured is also well documented. The data from replicate testing of each dispersant are available both for DE and volume of droplets < 70 microns. | Noted |
| Brandvik | The use of the droplet size data is a weak point in the report. The method used to quantify oil droplet sizes is based on laser scattering (LISST 100X). This instrument has an upper detection limit of 500 microns. The shape of (most) cumulative distribution curves shows that there is a significant amount of particles larger than 500 microns. These cumulative curves should not be used to calculate the median droplet diameter, since they only describe a fraction of the present droplets (see table 5). This is not at all discussed in the report. | It is agreed that the LISST has an upper limitation of 500 microns and that the median droplet size calculations are only based on the LISST data. This was briefly noted on pg16, but has been relocated and expanded on pg. 13. |
| Brandvik | The need for some of the statistical methods applied is not clear. I think simpler and better known statistical methods could have been applied. My understanding is that the statistic methods presented; Stepwise Regression, Breusch-Godfrey test and the Tukey Honestly Significant Difference (Tukey HSD) test is used to: 1) Find the significant experimental variables that influence DE (Stepwise regressions), 2) Find if there is a surfactant memory in the system affecting DE (Breusch-Godfrey), 3) Find significant difference between products and control (DE and vol. < 70 microns) Tukey HSD. The first and second methods are appropriate, but the third seems over complicated. A simpler student t-test comparing each dispersant with the control would be simpler, sufficient and maybe most important, and much easier to communicate to the reader. | The rationale for using the Tukey post-hoc test over alternate tests (such as the Dunnett's t-test comparing treatments only to the mean) has been added to p. 14 of the final version of this report. A simpler explanation and graph of the results has been added to facilitate communication to the reader |
| Guyomarch | Results of oil concentrations are not clearly explained, and there are no units on figures. The way the two sets of data resulting from the LISST measurements were combined is not explained. The statistical analysis is also not clear and figure (with error bars) would have been preferred to tables with too many significant numbers. | Additional detail have been provided to describe how the LISST data was handled (pg13). Oil concentration was used simply to identify which droplet sizes distribution data points were collected in the oil plume versus the background samples. Units have been added to these plots.(Appendix D) |
| Guyomarch | The threshold value of 70 µ m is also not discussed whereas it would have been interesting to reassess this limit based on a big set of data obtained at a large scale. | Additional details have been provided (pg13) discussing the 70µm threshold (Lunel 1993, National Research Council. 2005) . This threshold is generally accepted as a cutoff for neutrally buoyant oil droplets. |
| Guyomarch | One important result has not been discussed: the control is very high, and the effects of dispersants are expressed as a gain in percentage compared to this condition. Moreover, the control shows a significant variability (from 43.0 to 59.7), thus suggesting that the comparison between dispersants should include this key point. | Additional detail have been added to highlight the variability of the data. It is acknowledged that the natural dispersion measured for the control runs is higher than expected, but the droplet size distribution data for the controls shows that many of the droplets were quite large. |
| Guyomarch | A comment or an explanation on the increase of the oil concentration over time for the lower LISST (test 8) should be added. | The increasing concentration for the lower LISST in test 8 was believed to be sensor creep or fouling. The LISST was removed after this test and cleaned. A clear pattern was still identifiable to capture data points with relevant droplet size distributions. A comment was added to the appendix. |

| 4. Were the results of the sampling and statistical testing adequately characterized and clearly described? Were there any critical results or limitations that were not discussed or addressed in the report? | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Venosa | The sampling and statistical testing were adequately described, but unfortunately the methods were not fully appropriate in my opinion. First of all, an analysis of variance (ANOVA) should have been performed at the conclusion of all tests. This is usually what researchers do to determine if there were any significant differences noted in the experiment. If there were none, then there would be no need to conduct any further statistical tests. Only when statistical differences are determined, then the HST test should be done to determine where the significances existed. Instead, the authors used stepwise multiple regression analysis to establish a best-fit model to keep only those variables that explain the variability in the data. They then used the HST to identify pair-wise differences in regard to DE, % oil droplet sizes < 70 µm, and oil concentration having droplet sizes < 70 µm. In their stepwise multiple regressions, they asked how significant salinity, interfacial tension, and viscosity were in explaining the variability. But, since the same seawater and tank were used in all experiments without emptying the tank and refilling it with fresh seawater, studying the influence of salinity is a waste of time. It turned out (Table 6) that salinity played a role for the dependent variable "% below 70 µm", which I do not understand, as did viscosity. None of those 3 variables significantly affected DE or concentration below 70 µm (unless I'm reading Table 6 incorrectly). Frankly, I don't understand the blank cells in the table. If they tested all 3 variables, then there should be probability values (p-values) in all table cells. | The results have been re-presented using the ANCOVA framework. Additional explanation for why the analysis is appropriate for this situation has been added (pg.16). |
| Venosa | Tables 7-9 contained the critical statistical data. In Tables 8 and 9, only 2 pairwise significances were observed. For DE (Table 8), only the pairs ZI400-Corexit and ZI400-Finasol were statistically different from each other. For the < 70 µm concentrations (Table 9), only the pairs Finasol-Control and ZI400-Finasol were statistically different from each other. Table 7 shows 7 different pairs showing significant differences from each other. These results beg to be discussed and explained in greater detail but were not. | Tables 7, 8, and 9 were removed and the information is now displayed in figures 5, 9, and 10. The Tukey HSD test results were adjusted using an alpha value of 0.1 rather than 0.05 to better demonstrate the groupings of statistically different treatments when using a slightly less conservative criteria. |

| 5. Are the findings and overall discussion of the results for each product tested clearly discussed? | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Brandvik | No, results from the statistical methods (discussed above) are presented in table 6, 7, 8, and 9, they should have been be used in the subsequent discussion and conclusion sections. That's unfortunately not the situation. The results in these tables are presented, but I can't find a single reference to these tables in the "Discussion" or "Conclusion" sections. The discussion section is dominated by a justification of why a fifth dispersant was omitted and additional operational observations like spray patterns, challenges with high viscosity, and possible foaming. None of the statistical material is discussed. | References and additional discussion of the statistical methods and results have been further integrated into the narrative of the report, and are no longer presented in a table format. |
| Brandvik | One example is that table 8 shows that DE for neither C9500 nor OSR-52 is significantly higher than the control. This is probably due to the deviation (±6) for the two products is large compared to the difference between the products and the control (22-23). This seems strict. An ordinary T-test would have probably shown that they were significant! However, this and several other results from the statistical analysis, are not discussed or referred to in the conclusions. | Additional discussion of this consideration has been added to the discussion and conclusions sections. (pg. 17) |
| Fingas | There should have been more discussion on the removal of data point from one of the dispersants. This should have been done on a statistical basis, not on an ad hoc basis. | The data was revisited and it was determined that this data point was not a statistical outlier. Results have been updated to reflect the inclusion of this data point. |
| Guyomarch | Results and conclusions obtained for each product are generally well discussed, with the limitations mentioned above. Once again, the high level of the efficiency for the control, combined with a great variability, should prompt reservations to conclusions. | Noted |

| 5. Are the findings and overall discussion of the results for each product tested clearly discussed? | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Venosa | The dispersant product selection was inadequate since two dispersants that have been well tested in the literature were not tested in the project, namely, SPC 1000 and JD2000. Admittedly, the authors had problems with SPC 1000, but I am not convinced, based on their reporting, that they tried hard enough to overcome the viscosity and application problems of the product. As for JD2000, this product should have been chosen based on the literature, but the authors demonstrated an unawareness of the literature by not even mentioning this product in their selection procedure. | (pg. 23) SPC 1000 was part of the original test plan. When discrepancies between our sample's properties and the published properties were discovered, the manufacturer was contacted and verified that the product could be diluted with water but was unable to provide any assistance in suggesting nozzle sizes or pump pressures to use. JD2000 was a possible alternative, but to time constraints, ZI 400 was chosen to replace SPC1000 because they were able and willing to supply us with product in the short time frame. Also, it was felt appropriate to test a relatively "unknown" product in a realistic setting since it has been listed on the NCP Product Schedule of approved products with similar or better effectiveness to the other products (EPA NCP). |

| 6. Are the conclusions logical and appropriate based on the results? Are there any additional conclusions that could be drawn? | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Brandvik | No, the conclusions are not logical. The report has a comprehensive statistic chapter, but very little of this information is used in the conclusions. | The conclusions and results have been edited to incorporate the statistical analysis. |
| Brandvik | My evaluation of the presented data is that two of the dispersants have a significantly better effectiveness than the control (C9500 and OSR-52) based on both DE and volume of droplets < 70 microns.<br>However, the report concludes (abstract) that:<br>"Corexit EC9500A performed very well …. producing the highest average Dispersant Effectiveness (DE)"<br>"Finasol OSR 52 demonstrated a performance close to that of Corexit"<br>"Highest average" and "Close to" are terms which are not very comparable to the statistical ambitions earlier in the report. Based on the presented data, it is not possible to see a significant difference between DE of C9500 (73% ±6) and OSR-52 (72±6), see table 3. The C9500 has also a higher DOR (5%) than OSR-52 (3%), which is not discussed in the report. This ranking is highly surprising based on the presented data and especially based on the use of advanced statistical methods in other sections in the report. | These points have been clarified in the discussion and conclusions. |
| Fingas | Notwithstanding the arithmetic of the output values, more emphasis might have been put on discussing the practicality of this in the Arctic. One cannot heat the oil and dispersants there, so this reviewer does not see the application to field. This should have at least been discussed. | The oil was not heated in any way for these tests. The fact that the dispersants were heated does provide information about their practical use in the field. Additionally, and vessel or aircraft with power generation or auxiliary heat could provide energy to warm dispersants for pumping. |
| Sorial | The conclusions were clearly provided for the four dispersants studied. | Noted |

| 6. Are the conclusions logical and appropriate based on the results? Are there any additional conclusions that could be drawn? | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Venosa | No, the conclusions are not logical and appropriate. As I stated early in this review, the statistical analyses were flawed to begin with, but even if one accepts the analyses, the authors clearly displayed positive biases toward Corexit and Finasol. Although, based on their qualitative observations about the lack of foaming by Corexit and Finasol and the presence of foaming by ZI 400 and Accell that may have negatively impacted efficient and effective applications of the latter products to the slick, such observations are indeed helpful in allowing good decisions to be made by decision-makers against the use of the latter two products in the Arctic and in favor of the former two. However, the lack of statistical significances between Finasol, Corexit, and Accell do not help in such decision making. | Noted. The statistical analyses were refined and further integrated into the body of the report. |

| 7. Does this report present sufficient new data and knowledge, and are the findings useful for informing oil spill response planning in the Arctic regions? | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Brandvik | The main operational important messages extracted by me from this data material are:<br>1. There are larger differences in effectiveness between the dispersant on the EPA list.<br>2. Some of them perform excellent and should be preferred (stockpiled) by industry and authorities.<br>3. Some of the dispersants on the list have a very low effectiveness (since the threshold to get into the list is very low) and should NOT be used or stockpiled as marine oil spill dispersants.<br>4. More realistic and challenging testing is needed to get into the EPA list. This should be presented more clearly in the report. | Comparisons between dispersants performance on a large scale vs the NCP required testing was outside the scope of this report. |
| Brandvik | Another operational relevant fact is that dispersants are in a real situation not applied to fresh oils. In most cases it takes a couple of hours to take a decision (obtain a permit?), get the dispersant to the site, detect the slick and initiate dispersant spraying. This mean that dispersant usually are sprayed on slightly weathered oil, were emulsification is the most significant process, raising the viscosity due to increased water content. This gives the dispersant other challenges than dispersing water free fresh oil (penetrating viscous emulsions, increased oil pour point due to evaporation, breaking emulsions before dispersing the oil etc.). The reduced relevance of performing the testing on fresh oil is not addressed and this is a major weakness of the report. | Some of this information has been added to the summary on dispersants" section (pg5), but it is felt that this would not be appropriate to add to the discussion about the findings of this test program which only aimed to provide a comparison between products at a large scale (pg4). The introduction has been reworded to further stress this point. |
| Fingas | As noted, the applicability to the Arctic is in question. Furthermore the presentation of the correct results (value – control) would have been helpful. The correct results, even with their weaknesses, show that dispersant application to the Arctic would be questionable, although this does not come out in the report. | The goal of this study was not to determine the appropriateness of dispersant use in the Arctic. The goal of this test was to simple compare multiple products which appear on the NCP Product Schedule against each other in a simulated arctic environment. The introduction has been reworded to further stress this point. |
| Guyomarch | The questions raised by the Arctic conditions, in terms of oil spill response, in relationship with dispersant application, is based on environmental issues, and also on efficiency of techniques. This study well address the possibility of applying dispersants, from a technical point of view. However, even if tests are performed at low temperatures, these temperatures are not totally representative of arctic conditions. In particular, only low pour point oils can be treated, while it is not the case of this study. It should have been interesting to compare dispersants at the laboratory scale at very low temperatures, to clearly identify the influence of this parameter, and then to conduct large scale experiments by varying the oil condition and not only the dispersant products. | It is agreed that multiple oil properties would have greatly benefited this testing, but the availability of various oils in the volumes needed were not feasible at the time. |
| Sorial | Yes, the report provides new data that could be representative to actual spill scenario. | Noted |

| 7. Does this report present sufficient new data and knowledge, and are the findings useful for informing oil spill response planning in the Arctic regions? | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Venosa | I partially answered this question above in Question 6. I think the only "data" that might be useful in future decision-making for Arctic oil spills are the qualitative observations on foaming of two of the four products. | Noted |

| I. General Impressions | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Brandvik | The Ohmsett facility offers unique possibilities regarding large-scale dispersant testing and the description of the experimental procedures are clear and concisely described. The amount of work performed is impressive and all experiments are performed with three replicative. All the data needed to evaluate the data analysis and conclusions in the report are available in tables in the report and in the appendices. | Noted |
| Brandvik | The need for and relevance of some of the advanced statistical methods applied are not clear, simpler and more well-known statistical methods could have been applied. | A full rationale for the statistical methods applied has been added to the methods section. |
| Brandvik | Very little of the statistics presented in multiple tables are used in the discussion and conclusions | Noted. The statistical analysis was further included into the discussion and conclusions of the report. |
| Brandvik | The main conclusion is conflicting with the statistical analysis. | Noted. Statistical analyses were refined and additional information was included in the discussion and conclusion sections of the report. |
| Brandvik | There is no evidence in the data material supporting a (significant) difference in performance between C9500 and OSR-52 | Noted. Statistical analyses were refined and now reflect this. |
| Brandvik | The method used to quantify dispersed oil droplets has an upper detection limit of 500 microns, but there is a substantial fraction of larger droplets present. The cumulative distribution curves should not be used to calculate the median droplet diameter, since they only describe a fraction of the droplets. | This limitation of the study is noted. Discussions of the limitations of the equipment have been added (pg. 13). |
| Brandvik | The main conclusions stating that C9500 is best, followed by OSR-5 is not justified by the data. They are both significantly better than the control and the other product tested. The other products are not significantly better than the control. | The results have been edited to further incorporate the statistical analysis. |
| Fingas | The report addresses the terms of reference and is generally accurate, but could be improved in several ways:<br>Calculation of values – It is standard practice to report values as the test value less the control value, thus the values here should be:<br>Reported Value (RV) Test Value (TV)<br>Corexit RV 22.9 TV 72.7<br>Finasol RV 22.4 TV 72.2<br>Accell RV 1.5 TV 51.3<br>ZI 400 RV -4.1 TV 45.7<br>Control RV 0 TV 49.8<br>(Table 4 tries to show this, but the math is unclear or incorrect.) | Table 4 (pg.17) provides the test value DE, which is the percent of the original spill volume which was not collected due to being dispersed into the water column or other factors. It also provides the gain in increased improvement over the untreated oil caused by each product. This Normalizes the data in a way that allows large scale tests to be compared to each other. The DE (TV or RV) is only relevant to this particular oil, temperature, test facility, wave profile, etc. and cannot be representative of any other scenario. |

| I. General Impressions | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Fingas | Secondly, although the established protocol at Ohmsett is used, comments should be made on its applicability to this study. The study was to establish dispersibility of oils at low temperatures; however, the temperature and pressure of oil release were not noted. This could be very important. Importantly, the dispersants had to be heated to be discharged. Does this not rule out Arctic dispersion? There is no practical way that dispersants could be heated in the Arctic and certainly they would cool to frozen droplets (snow) by the time they hit the water. This should at least be highlighted. | The need to heat the dispersants was to regulate the viscosity of the products to ensure consistent dosage rates, none of the products became frozen and non-operational. Additional details have been provided regarding the need to heat the products (pg. 11). The oil was not heated and viscosity was calculated based on the viscosity curved created in the Ohmsett lab and surface temperature of the water at the time of the test. The oil is discharged at very low pressure at a very low elevation over the water's surface. The goal was not to establish dispersibility of oils at low temperatures, but to simply compare the performance of various EPA approved products in a cold environment. |
| Fingas | Another point about the protocol is that the 20 minutes of tests is certainly very rapid. The rise time of droplets would be well above this time, thus the test does not count semi-stable droplets that would rise after the time of the water sweeping. A time of about 3 hours might be more appropriate | While BSEE agrees that the rapid collection of surface oil does not allow for recoalescence of droplets, data collected showed the ability of the products to create droplets at the initial breakup of the oil under a relatively consistent wave action. It can be assumed that 'Dispersed' oil clouds containing larger droplets will see much more coalescence than one with a smaller median droplet size. Additionally Ohmsett is an outdoor wave tank affected by wind induced currents as well as a 6" per minute current caused by the filtration system. Long term periods between testing and collection would not be logistically possible. |
| Fingas | The 70 µm cutoff for droplets may be accepted, but the origin (from Lunel, 1993) is less than scientific. This cutoff point happens to be the range of the particular instrument, Lunel was using. This is a comment on a value that has suddenly become magical. A better cut-off might be 50 µm. | As stated, 70 µm is generally accepted. Additional details will be provided discussing the 70µm threshold (Lunel 1993, National Research Council. 2005) . This threshold is generally accepted as a cutoff for neutrally buoyant oil droplets. Additionally, because the project aims to compare the products to each other, any reasonable cutoff could be used to accomplish this goal. 70 µm was chose for consistency with previous Ohmsett testing. (SLRoss 2011, 2009, 2008, 2007) |

| I. General Impressions | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Fingas | The SPC 1000 should have not been diluted with water. This is certainly part of the reason for the foam production. | SPC 1000 created a more stable foam in its neat form. Diluting the product with water was done after confirming with the manufacturer that the product is water soluble. This was done in an effort to reduce the viscosity to something close to the stated specification on the product's technical bulletin. |
| Fingas | More justification is needed for the removal of the data point for Accell test. Perhaps a statistical test should be used. | The data was revisited and it was determined that this data point was not a statistical outlier. Results have been updated to reflect the inclusion of this data point. |
| Fingas | Appendix B is hardly needed in full as now presented. The size data is not particularly useful in graphical form. | Noted. Appendix B (Moved to Appendix D in the final version of the report) was simply there to show the separation of plume data vs background data in a visual matter. |
| Guyomarch | This test program, conducted in order to better understand the effect of various dispersants under arctic test conditions, is interesting as there is definitely a need in this field. In addition, experiments were conducted at a large scale, with replicates and controls, and the quality of the dispersion was monitored through many measurements, in particular the droplets size distribution. It also appears that significant efforts were made to control the dispersant application, which was then realistic. Syntheses of results were well presented in synthetic tables, thus clearly showing the effect of the various factors. However, considering the importance of the work, more details on the experimental results would be useful. In particular, the way the concentrations were determined is not clear, as well as the date concerning the processing of the two sets of droplet sizes. Appendix B would also benefit from details about the experiment rather than the test number. The statistical analysis is also unclear, and a graphical presentation of the results would be preferable (rather than a series of figures with (too) many significant results). | The statistical analysis has been further included into the discussion and conclusions of the report. Also, the axis labels have been updated to be more descriptive. |
| Guyomarch | Finally, regarding the plan of experiment, it seems that a preliminary study, preferably at a small scale, would have clearly demonstrated that some of the dispersants were significantly less efficient, and should then not have been included in the large scale tests. A reduced number of dispersants could then have allowed different conditions of tests, in particular a more viscous (or weathered) oil as differences between products are more interesting when the dispersibility is reduced (and the control could also be lower...). | This study used the EPA NCP Product Schedule to select dispersants which have met the minimum criteria for consideration for use in a U.S. spill response. (EPA NCP) |
| Sorial | Strength: The authors blended two crude oils into one homogeneous test oil that could be used for the full study – 15 planned tests. They measured the viscosity and its API gravity and compared to actual region oil. | Noted |
| Sorial | Strength: Each dispersant was tested in three separate replicates to provide detailed statistical analysis. Even the control tests (Oil without dispersant) were tested in replicates in order to determine the effect of the test conditions. | Noted |
| Sorial | Strength: Detailed statistical analysis was provided. | Noted |
| Sorial | Strength: The test runs could represent a real field scenario. | Noted |
| Sorial | Weakness: Data selection: The plots provided for the data selections are labelled as Run number without specifying the dispersants type or oil control. The concentration units should be provided in the plot not within the text. | These changes have been made. |

| I. General Impressions | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Sorial | Weakness: It is not clear why Finasol provided similar dispersant effectiveness as Corexit while the droplet size distribution curves are significantly different. More elaborations need to be provided. | Dispersant effectiveness and droplet size distribution are two separate and independent criteria. Effectiveness is a measure of the volume of oil which has been dispersed. The droplet size distributions show the make-up of that dispersed oil. It is possible for a products to disperse a large volume of oil, but have the created plume consist of mostly large droplets. |
| Sorial | Weakness: The droplet size distributions were measured at two depths, how was the plot in Figure 5 generated from these two depths | Figure 5 is now figure 7. Additional detail has been provided to describe how the LISST data was handled to generate this data (pg. 13). |
| Sorial | Weakness: All the tables should be provided to two decimal places. | Where appropriate, data has been presented to two decimal places. |
| Sorial | Weakness: A nomenclature for all symbols used and acronyms should be provided after the table of contents | Agreed. A table of symbols and acronyms was added. |
| Venosa | My overall impression is mediocre. The accuracy of information, as evident in my review comments below, is subpar. The researchers seemed to be unfamiliar with the literature, or they simply did not review it and include their review comments in the report. | It is unclear what literature is being referred too. The author is very familiar with many of the recent large scale tests which investigated dispersant effectiveness, many of which were conducted at Ohmsett and sponsored by BSEE and are referenced in the report. |
| Venosa | Their methodology was lacking in regard to the allowance of sufficient time for recoalescence to occur, a major weakness. | While BSEE agrees that the rapid collection of surface oil does not allow for recoalescence of droplets, data collected showed the ability of the products to create droplets at the initial breakup of the oil under a relatively consistent wave action. It can be assumed that 'Dispersed' oil clouds containing larger droplets will see much more coalescence than one with a smaller median droplet size. Additionally Ohmsett is an outdoor wave tank affected by wind induced currents as well as a 6" per minute current caused by the filtration system. Long term periods between testing and collection would not be logistically possible. This method is consistent with previous DE tests conducted at Ohmsett. (SLRoss 2011, 2009, 2008, 2007) |
| Venosa | Statistical methodology was less than appropriate since no analyses of variance were used to establish significant differences. Thus, the quantitative, statistical findings and overall discussion of results were inadequately presented. Pair-wise statistical differences were inadequately discussed or explained. | The results have been re-presented using the ANCOVA framework. Additional discussion of the pairwise comparisons has been added to the discussion. |

| I. General Impressions | | |
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| **Reviewer** | **Comment** | **BSEE Comment Response** |
| Venosa | As for product selection, it was disappointing that the researchers did not select two products for testing at Ohmsett that had been previously tested and reported in the literature as being excellent dispersants in laboratory and wave tank studies, JD2000 and SPC1000 (they tried with SPC1000, but they seemed unable to overcome the viscosity problems they faced with the product, which they might have more successful with if they had communicated with the manufacturer more fully). | The SPC1000 manufacturer was in contact with the project team to try and resolve the issues but to not appear bias towards SPC 1000 because other manufacturers where not consulted, SPC1000 was ultimately removed. Additional information regarding the selection of the products has been added to the final report on pg. 6 |
| Venosa | Their conclusions were not logical or appropriate due to the flawed statistical analyses and the bias they had toward Corexit 9500 and Finasol (Finasol is a product that is very similar if not identical to Corexit 9500). | Both the MSDS sheets and the NCP Technical Bulletins for Finasol and Corexit provide many differences. (40 CFR §300.920 (e), 2014) Additionally, published reports demonstrate the performance differences between these products.(Resby,Brandvik,Daling,Guyomarch,Eide 2007)(SLRoss 2013) |
| Venosa | In my opinion, the only “data” that might be useful in future decision-making for Arctic oil spills are the qualitative observations on foaming of two of the four products. | Noted |