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COMMENT

Comment on Peterson et al. (2001) 'Sampling design begets conclusions'

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The review by Peterson et al. (2001) summarizes, compares and critiques 3 scientific studies of the effects of the Exxon Valdez oil spill: (1) The Trustee Coastal Habitat Injury Assessment (CHIA) study. (2) The Exxon-supported Shoreline Ecology Program (SEP). (3) The NOAA-sponsored Hazardous Materials (HAZMAT) shoreline study. Most of the impacts occurred in Prince William Sound (PWS) and all of the studies discussed here focused most of their effort there. The authors (p. 266) fail to recognize that the 3 studies had very different goals (see Point 6 below). The goal of the SEP was to assess the extent of recovery as of 1990 (Boehm et al. 1995, Gilfillan et al. 1995a,b, Page et al. 1995). The SEP consisted of 2 components, a 64 site PWS Stratified Random Sampling (SRS) part and a 12 site PWS 'worst-case' fixed site part. The CHIA study had a different objective: 'A primary goal of the CHIA study was to quantify injuries to intertidal and supratidal biota in the Exxon Valdez oil spill area.' (Sundberg et al. 1996). The CHIA study had 12 randomly chosen oiled study sites in PWS subjectively paired with 12 unoiled sites. The NOAA HAZMAT study was a limited site-specific program carried out over time that was not designed to draw general conclusions about the spill zone as a whole. A major criticism of the review is that the authors are asking the reader to accept the notion that the matched pair CHIA study design involving 12 oiled sites is superior in detecting ecological effects from an oil spill to a stratified random sampling design involving many more sites. This notion is wrong, as explained below.

We have found many factual errors and misinterpretations in the review of Petersen et al. (2001). Given space limitations, we can only comment on the major errors. For clarity, our comments follow the 18 numbered points in the authors' review. We urge the reader to review our work cited here and compare the numbered points in the authors' paper with our corresponding responses.

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(1) Area covered per sample. The authors (p. 261) state that the smaller sampling area for epibiota used by the SEP resulted in low statistical power (low ability to find differences if they are present). The smaller sampling-unit area in the SEP was more than compensated for by the increase in the number of sites. The CHIA study only sampled 144 transects in PWS, whereas the SEP sampled at a total of 228 transects. The SEP used count-based probability models, which can accommodate smaller observed counts for a sampling location than the models used in the CHIA and HAZMAT studies (see Points 5 and 15; Gilfillan et al. 1999). Therefore, the authors' criticism concerning sampling area misrepresents the SEP.

(2) Sample replication. The authors (p. 262) claim that sample replication was less adequate for the SEP than for either the CHIA study or HAZMAT study without describing the differences in study objectives and study design. Since the CHIA study's objective was primarily to quantify injury by comparing the oiled site to the reference site in each pair, more transects (6) per site were required. The SRS component of the SEP had the broader objective of making comprehensive spatial inferences about the severity and extent of oiling effect throughout PWS and had more oiled sites (48) than the CHIA study (12) and fewer transects per site (3). The SRS transects were sufficiently far apart so that observations behaved independently in most cases, giving the SRS more power than either the CHIA or HAZMAT studies (see Points 5 and 15).

The CHIA study required more transects per site than a randomized study since each oiled/control site pair constituted a separate experiment, each of which must have reasonable power or the meta-analytic conclusions will be misleading. This is not the case with only 6 observations per site. Meta-analyses are highly sensitive to the p-values of individual experiments and the CHIA p-values are not well determined even if it is assumed that the log-normal distributional assumptions are met, which was usually not the case (Gilfillan et al. 1995a). As a result, transect replication in the

CHIA study is far more important than, and not comparable to, the transect replication in the SEP. The authors' assertion that the study of the SEP had low statistical power is not only inaccurate, but inconsistent with their claim that the SEP had 'too much power' in Point 13 (see below).

(3) Numbers of study sites per category. The authors (p. 263) are misleading about the number of study sites per category. It is not clear that there are only 12 oiled CHIA study sites in PWS, with relatively few CHIA site pairs available for certain habitats (e.g. only 2 for the sheltered estuarine habitat). The SEP sampled many more sites, a total of 76 sites in PWS compared with 12 pairs of sites for the CHIA study. The authors consistently fail to report the fact that, in addition to the 64 SRS sites (including 12 control sites), the SEP had a worst-case site component involving 11 oiled sites and a soft-sediment reference site that were sampled through 1999. Therefore, the authors' comparisons and conclusions are based on an incomplete description of the study designs considered.

(4) Numbers of sampling dates. The authors (p. 264) state that the SEP had only one sampling date, ignoring the fact that the SEP had a 12 worst-case non-random site component that sampled locations of special concern over the period 1990–1999. The HAZMAT study showed that recovery (defined as parallel behavior between oiled and reference sites) generally occurred by 1992 (Hoff & Shigenaka 1999) and is consistent with our conclusion based on our data from the non-random SEP sites sampled over time (Page et al. 1999). When the results and design of the 3 studies compared in the review are fairly represented, the conclusions of these 3 studies are reasonably consistent.

(5) Total area sampled. The authors (p. 265) criticize the area sampled for epifauna in the SEP as being too small. There were many more of the smaller samples in the SEP (Point 1 above), yielding a higher overall statistical power when calculated by standard methods (Gilfillan et al. 1996, 1999; Point 15 below).

(6) Philosophical support for targeting putative affected areas. The authors (p. 266) fail to recognize the differences in the goals of the studies compared. The goal of the SEP was to assess the extent of recovery as of 1990 following the dual criteria set forth by CERCLA (US Code 1987): Comprehensive testing to see if injury has occurred and determining the extent of injury. Thus the SEP sampled pebble/gravel habitats representing 3.7% of the spill zone and lightly oiled areas representing 70% of the spill zone. The objective of the CHIA study was to quantify injuries to biota in the spill zone (Sundberg et al. 1996) and, as a result, focused on specific moderately and heavily oiled sites. By excluding lightly oiled areas, the CHIA study excluded the largest single classification of oiling, thus excluding areas that were uncleaned and where the SEP detected spill effects. The CHIA study used lightly oiled sites as unoiled reference sites for some matched pairs, an inappropriate practice for an oil spill fate and effects study. The failure of the authors to take into account the differences in the compared study designs makes their criticism of the SEP invalid.

(7) Random site selection versus match-pair design. The authors (p. 267) are asking the reader to accept the fact that a matched pair design, where the pairs are subjectively chosen, is superior to a study plan involving many more randomly chosen sites in a stratified sampling plan where groups of sites are compared. Unlike epidemiological studies where multivariate matching is conceptually possible because patient records are available, it is impossible to accurately match sites in environmental science without comprehensive information about the characteristics of the candidate sites. This is especially true if the match is made using subjectively assessed characteristics. The CHIA study never demonstrated that they paired sites successfully according to their criteria and there is clear evidence that many site pairs were mismatched (see Figs. 1 & 2). In the SEP, the use of covariates, including calculated wave energies for each site, in an appropriately determined generalized linear model is methodologically superior to pairing sites without adequate prior quantitative measurements forming the basis of

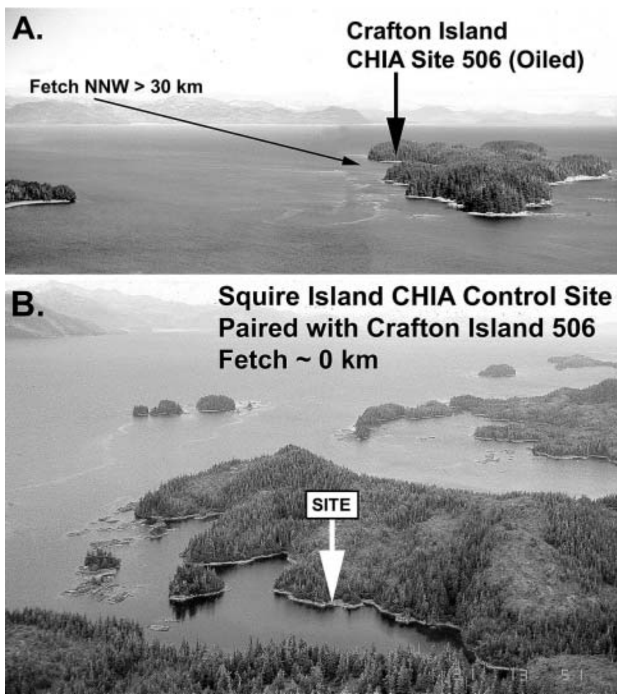

Fig. 1. Mis-matched pair of CHIA study sites, showing a sheltered control site mismatched with an exposed oiled site. These have been visited by one of the authors (DSP) to verify their character

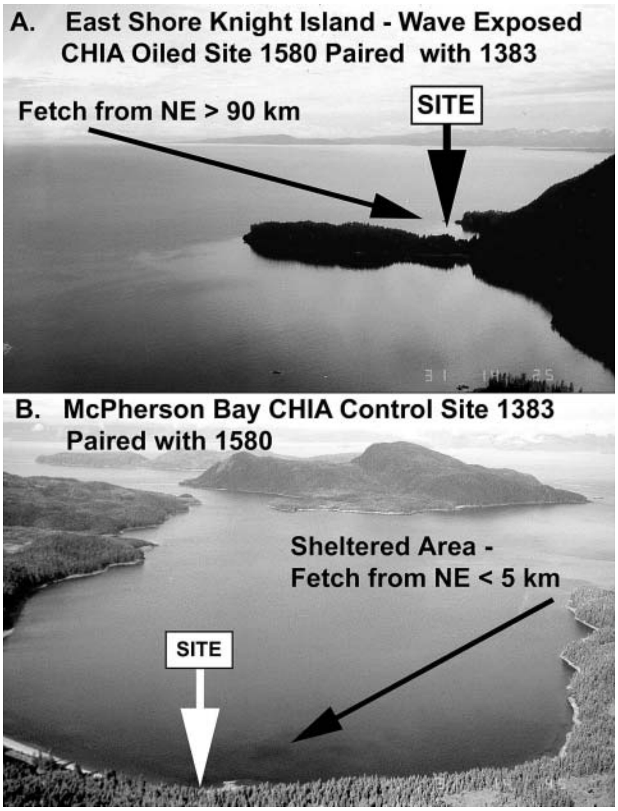

Fig. 2. Mis-matched pair of CHIA study sites, showing a sheltered control site mismatched with an exposed oiled site. The oiled site is a very exposed east-facing rocky shoreline, while the reference site is in a sheltered embayment. An oil spill response vessel is now permanently stationed in the reference bay due to its sheltered nature

the matching. Furthermore, it is methodologically infeasible to include covariates in the meta-analyses of a matched-pair design (Hedges & Olkin 1985).

(8) Sampling frame. The authors (p. 268) criticize our selection of certain reference sites and argue that we should have re-randomized when some of the sites were close together. We did not want to compromise a rigorous random sampling plan in the SEP study by subjectively picking reference sites. Random sampling means that one does not eliminate study sites for subjective reasons (Green 1979).

(9) Treatment of habitat heterogeneity within sites. The authors (p. 269) state that inclusion of secondary habitats in the SEP sampling artificially inflated the error variance and decreased power. The inference that the SEP included secondary habitat types within a given site is incorrect. Page et al. (1995) describe the site selection process where a key criterion was an uninterrupted habitat type in the upper intertidal zone over a distance ≥100 m. There were written sampling procedures that prevented sampling secondary habitats had they been encountered. This point is another example of the misrepresentation of the actual study plan of the SEP.

(10) Interspersion of sites. The authors (p. 269) criticize the SEP for having randomly chosen sites that were too close together. They cite the Perry Island sites as an example, incorrectly describing Perry Island, where a number of SRS reference sites were randomly chosen, as a 'single small island'. Perry Island is a large island with a shoreline length in excess of 60 km, making it probable that several SRS sites would be randomly chosen there, particularly in unoiled reference categories. This invalidates their criticism of the site selection process with regards to close proximity sites. The recommendation of the authors that we should have 're-randomized unsuitable sites' would have introduced bias into the site selection process, a major problem of the CHIA study.

(11) Controls for shoreline treatment and oiling intensity. The authors (p. 270) criticize the SEP for not differentiating between oiled sites and sites that were oiled and then cleaned. The vast majority of moderately and heavily oiled areas in PWS were in fact cleaned, most by several different methods. In both the SEP and the CHIA studies, oiling effects and cleanup effects are confounded. While oiling intensity was a categorical variable that was not easily quantified in any study, all of the studies cited by the authors used the same systematic geographical database generated by the cleanup program overseen by the US Coast Guard to determine the level of oiling at a given location (Neff et al. 1995).

(12) ANCOVA with covariate affected by treatment. The authors (p. 270) incorrectly state that the SRS data analysis was an ANCOVA, implying normal theory models. This misrepresents what was done since (log)normal models were only used for certain high-abundance species. In the SEP data analysis plan, an exponential family model well suited to species abundance data (a binomial, negative binomial or Poisson model and the normal as a limiting case) was selected according to a model fitting protocol (Gilfillan et al. 1995a). The authors criticized the manner in which covariates were used in the SEP study design, which enhanced our ability to detect an oiling effect (Anderson et al. 1980). Covariates did not interact with the oiling level and thus simple additive models, on the inverse link scale, were used. Covariates will increase the power of detecting an oiling effect if the variability accounted for by the model more than offsets the loss in error degrees of freedom due to fitting the covariates, as was found to be the case for the SEP. The authors (p. 271) incorrectly state that we found correlations among wave exposure, oiling level and total organic carbon. The reference cited by the authors (Page et al. 1995, p. 283) states just the opposite. The authors' failure to accurately represent the data analysis procedures of the SEP invalidates their criticisms of the SEP and its conclusions.

(13) Pseudoreplication. The authors (p. 272) argue that we should have decreased our ability to detect an oiling effect, an assertion completely at variance with their criticism of our study in their Point 2 (see above). Using transect 'pseudoreplicates' to increase power is a sound and essential strategy for CERCLA-based comprehensive studies since only a limited number of sites can be sampled. In the SEP study design, the 3 transects at each site were placed sufficiently far apart that they were found to behave independently in most cases (in the sense that site-to-site variability was not significantly greater than transect-to-transect variability), thus increasing statistical power (Gilfillan et al. 1995a). The statistical test that was performed to determine whether the transects behaved in this manner had a moderately stringent criterion ($\alpha = 0.05$) for rejection. Had we used the rejection criteria of $\alpha = 0.2$ to 0.3, advocated by the authors, we would have decreased our ability to detect an oiling effect and understated the degree of injury and overstated the degree of recovery. The goal of the SRS was to use a liberal, but reasonable, policy for detecting oiling effects to the extent that we were willing to accept false positives. This goal is consistent with the Precautionary Principle, which basically states that in environmental science we should be comfortable with false positives. It seems odd to us that the authors, whose CHIA study tended to maximize the detection of injury (e.g. by the liberal nature of their meta-analyses, lack of independence, etc.), criticized us for doing so.

(14) Inferring degree of recovery. The authors (p. 272) criticize the methods used in the SEP to assess recovery as of the summer of 1990. The SEP took a dual approach to assessing recovery. The SRS focused on the objective of estimating the extent of recovery for all of PWS by 1990 based on individual and community analyses. The fixed-site program examined recovery over time (1989, 1990, 1991, 1998, and 1999 sampling periods). The authors also criticize the use of detrended canonical correspondence analysis (DCCA) on the grounds that it is based on normal theory. Their claim is false because DCCA is not based on normal theory. The underlying model is based on the Poisson distribution, not the normal distribution as stated by the authors (ter Braak 1986). The Poisson distribution is a reasonable assumption although in some cases over dispersion (as represented by a negative binomial model) or under dispersion (as represented by a binomial model) was present (see Gilfillan et al. 1995a). Because the authors did not accurately represent the SEP data analysis methods, their criticism of the SEP data analysis procedures are invalid.

(15) Power analysis. The authors (p. 274) state that we did not use actual data to analyze the statistical power. This statement is false. Data-based SRS power studies were run during both the study design phase (based on 1989 data) and the data analysis phase subsequent to 1990 (Gilfillan et al. 1995a). Following these analyses, extensive power simulations were run on the SRS, CHIA, and the HAZMAT studies where real data from each study formed the basis for these simulations. The results of these analyses have been reported (Gilfillan et al. 1996, 1999). Overall, the SRS had moderate power based on the site model and high power based on the pooled transect model. The power for the CHIA study suffered from problems that are inherent in the meta-analysis used (Hedges & Olkin 1985). First, since only 2 to 5 site pairs are available for any given habitat, a single significant difference or even no significant differences among site pairs can result in a significant overall test. This is a well-known deficiency of meta-analysis approaches. This deficiency invalidates the practical use of meta-analysis as a methodology for CERCLA-based studies. The rejection of the overall hypothesis by a single significant site-pair test would only be desirable if the principal interest is whether an impact occurred somewhere, whereas rejecting the overall hypothesis when no site-pair tests are significant is never desirable. Second, reversals in the direction of significance among the paired analyses, which often occurred in the CHIA data set, essentially kill the power (depending on the pair weights) to detect oiling effects (Gilfillan et al. 1999). Third, the direction of the alternative hypothesis in the CHIA study was determined by a data-based vote counting algorithm, which has the effect of doubling the significance level, which gives the artificial appearance of power. Vote counting has another deleterious effect. Increasing the number of site pairs tends to give more wrong decisions (Hedges & Olkin 1985), particularly for low-power experiments. Fourth, the statistical power heavily depends on the weights, which vary widely among the site-pair analyses. (Gilfillan et al. 1999). The authors' conclusions about the relative statistical power of the studies compared are invalid because they fail to accurately represent the statistical properties of the various analyses.

(16) Taxonomic level used for analysis. The authors (p. 274) state that: '…SEP rarely reports results by species and instead tend to pool data for univariate analyses into higher taxonomic categories…'. This statement is totally false. For example, Gilfillan et al. (1995a, p. 411) state that 'overall 83 (18.7%) of the total of 443 species tests showed an oiling effect.' In fact, generalized linear models were fit to all individual species that did not exceed 80% missing values. This misrepresentation of the results presented by Gilfillan et al. (1995a) is the foundation of the rest of the argument in Point 16, which is that species level tests are needed to detect changes in possibly sensitive species which would be masked if species were grouped. When the grouping argument fails so does the rest of the argument in Point 16 because the authors fail to accurately represent what was actually done.

(17) Pooling of disparate communities. The authors (p. 275) criticize the use of 1.0 mm mesh for sampling epifauna and infauna. The target populations of epifauna and infauna for the SEP were those animals retained on a 1.0 mm mesh. The same was true for the CHIA study. The criticism that both macro- and meiofauna were incorrectly included in the SEP sampling design is invalid. All comparisons were based on internally consistent data. Eliminating the meiofauna from the data set would have excluded nematodes, one of the most abundant groups of animals and one most likely to exhibit an oiling effect.

(18) Scope of communities and habitats examined. The authors (p. 276) criticize the SEP because it did not study all possible oil spill effects. This criticism is unwarranted and misleading. The purpose of the SEP was to estimate the extent of recovery for the intertidal and shallow subtidal infauna and epifauna of PWS (see Point 6). The SEP was a part of a much larger group of studies (22 total), not cited by the authors, that investigated oil spill impacts on a very wide variety of organisms. Most of these studies were reported in the same volume that the authors have cited for our work.

Criticism of the SEP for not sampling soft sediment sites is unwarranted because the authors ignored the important non-random fixed site component of the SEP where a soft sediment site was sampled, a rare habitat accounting for considerably less than 1% of the oiled area in PWS.

Discussion. In their discussion, the authors incorrectly state that the results of the SEP are inconsistent with those of the CHIA and HAZMAT studies. In the first place, comparisons should be made between comparable studies. The HAZMAT study was a fixed site study carried out at the most heavily oiled and cleaned rocky shoreline locations in PWS. Its results are not comparable with the SRS component of the SEP, but are comparable with the non-random worst-case fixed site component of the SEP that the authors ignored. The comparison with the CHIA study compares to a study done at moderately and heavily oiled sites with the SRS component of the SEP, which was done at all oiling levels. Specifically, the authors claim that the results of the SEP '…are discordant…' when compared to those of the CHIA and HAZMAT studies. This is false, as we indicated in our response to Point 4 above. In fact, the overall conclusions of the CHIA and SEP studies were remarkably consistent. Overall, no significant differences between site pairs were found by the CHIA study in 79% of all the tests that they performed. The SEP found no differences in 83% of the tests performed. The similarities between the CHIA study and the SEP are far greater than the differences.

The results of the SEP are highly credible and consistent with other studies when compared fairly. The SEP was based on the sediment quality triad approach, where sediment chemistry, toxicity and ecology are all studied concurrently. The results of this approach showed that those sites where there was the most remaining oil in 1990 had the greatest sediment toxicity and showed the most deleterious ecological effects. These were the upper- and middle intertidal zones of boulder beaches.

The results of the SEP are internally consistent and are also consistent with the results of the CHIA study. The CHIA study used sites that had been lightly oiled as reference sites (see Point 6 above). Lightly oiled sites represent 70% of the spill zone in PWS. The SEP estimated that by 1990, 70 to 90% of the spill zone in PWS had recovered, consistent with most of the spill zone having been lightly oiled in 1989. Thus the CHIA study and SEP estimates of recovery are reasonably consistent, since the CHIA study, by including lightly oiled areas as reference sites, assumed that at least 70% of the spill zone had recovered by 1990.

Finally, in their Table 5, the authors (p. 279) present a 'scientific ranking' of the 18 elements for each of the 3 studies that they have reviewed. Each of the elements is given a subjective numerical ranking, giving the appearance of scientific method. Tables like Table 5 are only useful if they are based on an accurate representation of the studies involved. Based on our presentation of the errors and misstatements in the review that are given above, we will present some examples of misleading elements of Table 5. These examples are arranged by the row number and title taken from Table 5:

(2) Sample replication. The SEP had a higher degree of sample replication than either the CHIA or NOAA studies, but received the lowest grade. This element of the Table should be reversed, with the SEP having the highest score.

(4) Number of sampling dates. The SEP has 5 sampling dates (1 SRS; 4 fixed) yet gets a score of 1.5. The CHIA study has 2 dates and gets a score of 3. The NOAA study has 4 sampling dates and gets a score of 4. Clearly, the SEP, with the most sampling dates, should receive a sampling date score of at least 4.

(9) Treatment of habitat heterogeneity within sites. The SEP had explicit written sampling procedures to prevent sampling heterogeneous habitats within a site, yet it is awarded a score of 1; the CHIA study is given a score of 4 with no explanation for the difference. These scores should be reversed.

(14) Inferring degree of recovery. The SEP is given a score of 1.5. For the CHIA study, which can infer recovery only for moderate and heavily oiled locations, if at all, the authors award themselves a score of 3. The NOAA study, whose design allows no inferences beyond the sites that were sampled, is awarded a score of 4. Clearly, the rankings should be reversed in this category as well.

Given the many factual errors and errors of interpretation that occur throughout the review, the subjective and erroneous 'rankings' in Table 5 have no scientific value, are grossly misleading and have no place in a review in a respected scientific journal. Given more space to develop our points, we could easily justify rankings, which would reverse the findings in Table 5, but there is little point in doing this since the objectives of the studies are very different and thus they are not directly comparable, making Table 5 meaningless, except in an advocacy context.

Environmental science is not well served by published work that fails to accurately present the studies in the references cited. Study design in environmental impact assessment is a very important and timely topic. We find that the review does not constructively contribute to the discussion of this topic because it does not fairly and accurately represent our work and that of others.

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