

Daryl Boness, Editor, Marine Mammal Science

Dear Dr. Boness:

Thank you for the opportunity to respond to the comments recently sent (9/8/2008) to you regarding our paper in press: "Modeling the effects of El Niño, density-dependence, and disturbance on harbor seal counts in Drakes Estero, California: 1997-2007." Prior to addressing the comments directly, I would like to point out that we appreciate this opportunity to modify three parts of the MS, and these three modifications/corrections/updates should also satisfy some of Dr. Goodman's concerns as they directly pertain to his points 1-3 below. First, a new oyster harvest statistic for 2007 was released about six weeks ago. Our paper used the projected 2007 values obtained from Cal Fish and Game and we just learned of this new value on Sept. 4, 2008. We have redone the analyses with this new value for 2007 and it does not change any of our conclusions. Akaike weights are equally strong for the best models and r^2 values decreased by 0.04 for one model and increased by 0.03 for the other (details on new models are presented below). We would like the opportunity to include this updated analysis in the paper. Second, Dr. Goodman discusses a contested trip report that was used in the disturbance data analysis. A lengthy and thorough Inspector General's report (July 2008) did not support the oyster company's allegations of a falsely reported disturbance, but rather as Dr. Goodman states, "they found no determination." We consider this data point to be true and correct, but if you or the reviewers feel that this data point should be removed or asterisked, we are happy to oblige. The loss of the single data point has no impact on the conclusions or patterns seen in the paper. Lastly, we are incorporating Dr. Goodman's recommendation that we use the "rate of disturbance", rather than "percentage mariculture related". The IG Report can be found at: <http://www.doi.gov/>. Click on "[Recently Released Reports](#)".

We also would like to take the opportunity to add 2 short paragraphs to the discussion. The first will concern why ENSO may have been important for the upper estero. This may be since the upper estero is used mainly for pupping (at a higher frequency than the rest of the estero) since the upper estero are islands, have reduced current, and generally lower disturbance and are predator free. A second paragraph would briefly discuss newly collected 2008 data. For the 2008 pupping season, the California Coastal Commission (CCC) imposed a rule greatly reducing use of the lateral channel at OB by oyster boats and oyster bags were also pulled farther away from traditional seal haul out sites. Also, only one mariculture disturbance was detected in the upper estero in 2008, therefore it appears that conditions that were reducing seal counts may have abated since counts at OB were up from about 65 in 2007 to 97 in 2008. We therefore suggest that an adaptive management approach be investigated since increasing and reducing activity in the area close to seals appears to have immediate effects. It is not appropriate to model the 2008 data at this time because a large management shift occurred due to CCC restrictions on use of channels near the seal haul outs.

We would also like to highlight that publishing disturbance impacts on marine mammal distribution and habitat use (but not necessarily population level impacts) has recent precedent in the literature:

Bejder et al. 2006. Decline in relative abundance of Bottlenose dolphins exposed to long-term disturbance. *Conservation Biology* 20:1791-1798.

Watson-Capps and Mann. 2005. The effects of aquaculture on bottlenose dolphin ranging in Shark Bay, Western Australia. *Biological Conservation* 124:519-526.

We thank you for your time in this additional (and unexpected) review and hope that we can satisfy any concerns. We will respond to each of the criticisms and concerns below, leaving Dr. Goodman's text in arial, and our responses are preceded by "BHB RESPONSE" and are in Times Italic

BEGIN 9/8/2008 GOODMAN LETTER TO DR. BONESS

It is with deep regret that I write to you to ask you to hold and not publish the paper by National Park Service scientists Benjamin Becker, David Press, and Sarah Allen entitled "Modeling the effects of el Niño, density-dependence, and disturbance on harbor seal counts in Drakes Estero, California: 1997-2007." I realize that this paper is due to be published within a few weeks, but I urge you to pull it until the analysis is finished with

the proper numbers and the paper can be peer-reviewed once again. Once you read this letter, I hope you will agree that it is best for your journal that this paper be held and not published until the controversy around it is resolved and the authors have addressed certain questions and entered the correct numbers into their analysis.

I am a biologist, an elected member of the National Academy of Sciences, and was a Professor at Stanford and U.C. Berkeley for 25 years (I am now an adjunct Prof at U.C. San Francisco and am in the private sector). I have published over 200 peer-reviewed papers in my academic scientific career. I chaired the National Research Council's Board on Life Sciences for 6 years and today serve on the California Council on Science and Technology.

In May of 2007, I presented a series of reports to the Marin County Board of Supervisors alleging that the National Park Service had misrepresented the data on the potential impact of the oyster farm (Drakes Bay Oyster Company or DBOC) on the ecology of Drakes Estero. The NPS has since retracted many of the claims that I alleged they had misrepresented (I would be glad to provide you with details backing up these assertions).

BHB Response – The NPS has not retracted any claims regarding harbor seal data, other than to indicate that we referred to an incorrect baseline year of 2004 rather than 2005. We recommend that you read the entire IG report so that you can understand the nature of the accusations and the findings by the IG.

The NPS was investigated by the Inspector General of the Department of Interior and found to have misrepresented some of the science.

BHB Response – This is true, the IG found one serious NPS misrepresentation regarding sedimentation and oyster pseudofeces that was completely accidental and without intent (out of about 14 allegations). The incorrect statement had already been retracted by the NPS in July 2007. This accidental misrepresentation was not related to harbor seals or harbor seal data.

Last summer, on July 21, Senator Dianne Feinstein held a meeting in which she asked that they give me access to their harbor seal database (from which they had made provocative claims but had withheld access) and that an independent scientific review take place and compare the NPS claims vs. the NPS data. That review just began this past week. The Ocean Studies Board of the National Research Council (NRC, of the National Academy of Sciences) has begun a study on "Best Practices for Shellfish Mariculture and the Effects of Commercial Activities in Drake's Estero, Pt. Reyes National Seashore, California."

BHB RESPONSE – This is correct, however there are no marine mammal (or vertebrate) experts on the panel.

I tell you all of this because this is matter of pre-existing scientific dispute. The NPS has already retracted many of their previous claims against the oyster farm, and two members of the National Academy of Sciences (me and Dr. Peter Gleick, the President of the Pacific Institute) have said that the NPS is misrepresenting their own science.

The Ocean Studies Board panel met in Marin County, California, this past Thursday and Friday (Sept 4 & 5). Part of their mission is to investigate the allegations that the NPS has misrepresented their own data. Dr. Ben Becker presented his paper in press in Marine Mammal Science. The panel was critical of the statistical analysis and interpretation.

BHB RESPONSE – The panel asked many insightful questions, and it is their job to be critical. However, from my perspective I would not characterize the panel as questioning any of the paper's conclusions. Conversely, the Ocean Studies Board director rather asked for some more detailed summaries of the declines in subsite use from 2002 - 2007.

But they did not know that some of the data used for the paper was either incorrect or controversial. We informed Dr. Becker that his paper used incorrect and controversial data. He assured the NRC panel that

he would correct his paper, but how can he do so without pulling it, redoing the analysis, and getting the paper re-reviewed?

BHB RESPONSE - We agree, we are happy for the opportunity to correct the oyster harvest data and discuss the disturbance data. Dr. Goodman gave a presentation after mine detailing many of the issues he cites below. The panel is well aware of all allegations, Inspector General investigations, etc., so there is no attempt to mislead, we merely use the data we consider correct. We have also redone all the analyses and they are presented below.

At the same time, the National Park Service and others in the community were already citing this paper within one day as showing clear evidence in a peer-reviewed journal that the oyster farm is having a negative impact on the seals.

BHB RESPONSE - The park service represented the paper's conclusions: Subsite use decline in the upper estero is best explained by increase in oyster production.

This paper has such important flaws, that I strongly encourage you to hold it back until the National Academy of Sciences can get their own statistician to analyze the data and determine whether the conclusions are correct or incorrect.

BHB RESPONSE - The oyster harvest value changing for one year simply makes the association slightly stronger. I would not characterize this as an important flaw, but rather something that should be corrected. The 2007 data point is still the highest in since 1997. Furthermore, the panel was presented with the new information on oyster harvest counts for 2007 on that day, and I presented how I thought it might impact the results (stronger association at 2 subsites, weaker association at 1 subsite) which has been confirmed by my subsequent analyses with the new oyster harvest data. I did agree to redo the analysis for the NRC, and directly incorporating into this MMS paper is probably the best way to do that.

Regarding an NRC statistician analyzing the data. I do believe they plan to do this but I do not have any details. I welcome any way that MMS would like to proceed on this issue. However, it is also true that this paper (when published) may inform the NRC panel on the relationship between mariculture and harbor seal haul out patterns, so I think it would be important to ensure that the NRC was actually going to do their own thorough and timely analysis before holding back this paper (once revised with the suggestions below). We would also like reemphasize that a couple of well respected marine mammal statisticians guided us with these analyses as is noted in the acknowledgments.

Your referees did not know that many of the basic numbers in the paper (e.g., in Table 1 and Figure 2) were either incorrect or controversial.

BHB RESPONSE - Dr. Goodman is referring to the oyster harvest for 2007 as incorrect and this is corrected below. This is also an unfair criticism; these new values were released 1-2 months ago, prior to submission and revision of the paper. Controversial values include the April 26, 2007 disturbance date, which as explained before is up to the editor if it should be dropped (we provide analysis below showing the impact of keeping or removing this datapoint - none). Nevertheless, we consider the data to be correct and the Inspector General did not support the allegation that it was falsified. If the Inspector General had concluded back in July 2008 that the data point was found to be erroneous or fabricated, then we would have certainly notified MMS immediately and removed the data point from subsequent drafts and analyses.

The authors picked only a subset of years, focusing on the 2005-2007 general decrease in seals up and down the coast of California when Dr. Sarah Allen has harbor seal data for Drakes Estero back to the 1970's.

BHB RESPONSE - This statement is incorrect for several reasons. We clearly looked at all available data from 1997 - 2007, the entire time series for NPS monitoring that has subsite data in Drakes Estero. Dr. Allen does indeed have a small set of subsite data in field notebooks from the 1980s and 1990s, but this was part of a master's thesis or other

reports and not part of the NPS monitoring program. The Inspector General investigation also came to the same conclusion. Much of the data are also from before harbor seals recovered in N. California after the MMPA. All other data collected since the 1970s are mainly for the entire estero with shifts in subsite naming conventions and would not be comparable with the high quality 1997 – 2007 subsite dataset we used. Also, we only reported (and tested for) a 2005 – 2007 change at a subsite near the oyster farm, after the modeling indicated that this was best explained by a tapering ENSO effect and a building oyster harvest effect. These dates were in no way random or cherry picked, and justification is clearly explained in the ms.

The authors picked on a limited subset of subsites: disturbance data were analyzed for only 3 of 8 subsites in Drakes Estero.

BHB RESPONSE – The initial drafts of this manuscript began looking at all eight subsites in a multinomial fashion. Two statisticians (including Dr. Jeff Laake – NOAA-NMML) and an ecologist (from USGS) strongly criticized this approach in that this was simply data dredging, i.e., by looking at all sites, and then wherever we find a decline in seals near oyster culture, calling that an effect. We of course agree with Dr. Laake that that was an incorrect approach. Dr. Laake then suggested (as reaffirmed by Jay Barlow at NOAA, and another USGS researcher, plus the numerous friendly reviews we obtained before submitting the MS)) that an a priori hypothesis approach, only on sites suspected of being influenced by mariculture was a correct and robust approach. This perfectly fits with prevailing philosophy of science in ecology where large scale experiments are simply not possible and confronting a priori models with data as advocated by authors such as Hilborn and Mangel 1997, and Buckland and Anderson 2002.

The authors made mistakes in what they entered; the oyster production listed is 63% too high for 2007 or 267% too high when 2007 is compared to 2006.

BHB RESPONSE – This was not a mistake. We used the latest available oyster harvest information from Cal. Dept of Fish and Game on oyster harvest in Drakes Estero for 2007 when submitting and revising the paper. The actual 2007 value was released about a month ago. The new value for 2007 (as stated earlier) was still the highest since the mid 1990s, and minimally affects the modeling (i.e., in the modeling, a smaller increase in oyster harvest led to the same decrease in seal use, meaning the effect was stronger, although r^2 declined by 0.04 in the best OB model and increased by 0.03 in the best UEN model).

The authors entered controversial data; the disturbance data for 2007 includes only 3 records for oyster farm disturbances, two of which are controversial with a formal complaint filed for one – the Apr 26 Trip Report. The authors did not report on disturbances per survey (i.e., observation date), which when calculated shows that disturbances per survey actually went down in 2007 vs. 2006 for islands UEF, OB, and UEN. Nevertheless, the authors derived strong conclusions based on regression analysis to show that disturbances from the oyster farm led to fewer seals on a specific subsite, island OB.

BHB RESPONSE - The GLM (Goodman refers to this as regression analysis) showed that there were fewer seals at OB when oyster harvest was higher. Density dependence was not important. We would be happy to investigate additional plausible alternative hypotheses. However, we feel density dependence should incorporate other population level processes that might have been hypothesized to be related to counts in the upper estero.

What is worrisome is that the NPS, and certain members of the local community, are already (as of the day of the NRC panel meeting last Thursday) claiming that the NPS has shown conclusively in a peer-reviewed scientific article that the oyster farm is harming the harbor seals.

BHB RESPONSE – The NPS has stated only what is in the paper, subsite use declined near the oyster operations as oyster harvest increased, and that among the candidate models tested, ENSO and Oyster harvest best explain these counts. We presented very directly in the paper and to the NRC the r^2 values and degree of confidence (Akaike weights) in all models. We believe strongly in Burnham and Anderson's (2002) modeling approach, and believe we are true to not overstating the results (which happen to be quite strong). Furthermore, the term "regression analysis" greatly simplifies the a priori, information-theoretic, generalized linear modeling approach we used. Since the 2007 number

has changed and the oyster model coefficient is a bit stronger. Below we also recalculate the predictive model explaining counts at subsite UEF.

This is a false conclusion given the mistakes of data entry and selection, and data analysis.

BHB RESPONSE – We disagree, the conclusions and patterns are the same, even with the corrected data. There is no evidence of data entry errors in the dataset which was thoroughly error checked.

First, NPS got one of the major variables wrong – oyster production – for 2007. The number they listed is 63% too high for 2007, and when you compare 2007 to 2006, it shows an increase that is 267% too high. The correct number for 2006 is 292,000 pounds.

BHB RESPONSE – We used the correct value for 2006 (291,538 lbs.) in all modeling and all versions of the ms. Dr. Goodman is incorrect. See current Figure 2.

Becker claimed 760,000 lbs of oysters in 2007 when the real number is 466,503 lbs. Whereas the correct number went up 60% from 2006 to 2007, Becker used numbers that went up 160% from 2006 to 2007 (2.67 fold higher than the real number). The correct numbers come from Tom Moore at California Dept. of Fish & Game. Clearly, Becker et al. need to redo their statistics using the correct numbers.

BHB RESPONSE – This has been addressed above. To recap, the new 2007 oyster harvest value was released a month ago (Mid-late summer 2008), we have rerun the analyses, and the new data results in more negative coefficients, but slightly lower r^2 (0.44 vs. 0.48) for the models for OB, and a slightly weaker association (but higher r^2 : 0.21 vs. 0.18) at UEN, which is farther from mariculture and we would have predicted to have less of an affect. Below are the new Akaike weights and r^2 for the best model and plots of the relationships from the best model. Again, this does not in any way affect the conclusions of the paper, other than perhaps to say that mariculture has a slightly stronger negative association with seal counts and explains slightly less variation (although we consider a 2 parameter model with an r^2 of 0.44 to be pretty good).

Below is a corrected table using the updated 2007 oyster harvest value. All models have been rerun in S-plus. Table 3 (in the current ms) coefficients will be corrected accordingly, but only involve slightly more negative “oyst” coefficients. ENSO and Oyst still $P \ll 0.05$ at both subsites and seals (density dependence) not significant ($P > 0.10$).

Table 2. (A) Ranking of candidate models at subsite OB by AICc and Akaike weights (w_i).

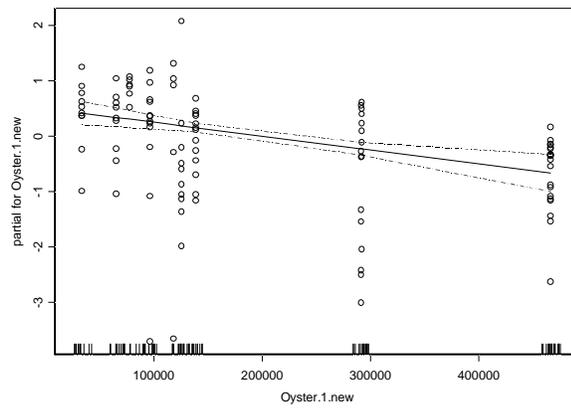
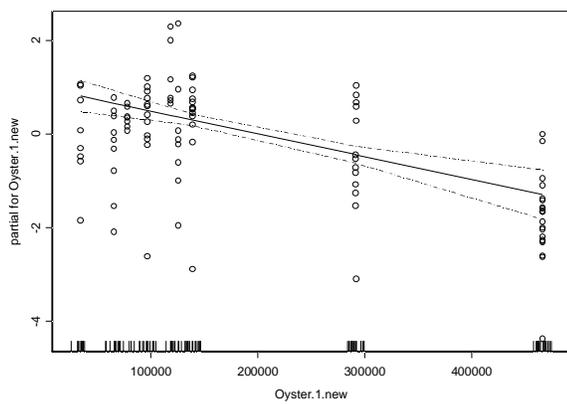
Dependent variable is the total seals at the subsite and independent variables tested are year as linear trend (Year), Sum of all other seals in estero (Seals), years since last El Nino event (ENSO),

and lbs. of oysters harvested in following year (Oyst). Column headings are:

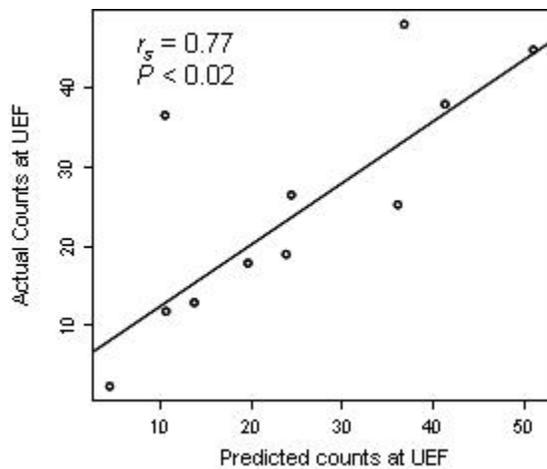
small sample AIC (AICc), distance from best model (Δ_i), and akaike weight (w_i).

Model	AIC _c	Δ_i	w_i	pseudo r^2
Subsite OB				
ENSO+Oyst	325.96	0.00	0.73	0.44
Seals+ENSO+Oyst	327.99	2.03	0.27	0.44
Seals+ENSO	339.19	13.23	0.00	0.29
ENSO	340.36	14.40	0.00	0.25
Year+Seals	342.82	16.86	0.00	0.25
Year	344.90	18.93	0.00	0.19
Year+Oysters	345.21	19.24	0.00	0.21
Seals	349.69	23.72	0.00	0.11
Seals+Oyst	351.72	25.75	0.00	0.11
Null model	353.68	27.72	0.00	0.00

Oyst	355.80	29.84	0.00	0.00
Subsite UEN				
ENSO+Oyst	316.37	0.00	0.59	0.21
Seals+ENSO+Oyst	318.48	2.10	0.21	0.21
ENSO	321.13	4.75	0.05	0.11
Year	321.68	5.30	0.04	0.10
Seals+ENSO	322.00	5.63	0.04	0.13
Year+Seals	322.20	5.83	0.03	0.13
Year+Oyst	322.69	6.31	0.03	0.12
Null model	325.60	9.22	0.01	0.01
Seals	326.12	9.75	0.00	0.04
Seals+Oyst	327.11	10.74	0.00	0.05
Oyst	327.72	11.34	0.00	0.01



Figures 1A & B. Above: (These are not in the ms, but rather for clarification of the negligible impact of the new 2007 oyster harvest value). Partial plots from best GLM models effect on seal counts due to Oyster harvest at OB (left) and UEN (right) using the updated 2007 official oyster harvest numbers. Coefficients are -0.49 ± 0.1 and -0.35 ± 0.6 , respectively. Time since ENSO still has a strong positive affect in both models and density dependence has no explanatory power and is a random scatter plot in both models. These are from the best models in the table above.



ABOVE: New Figure 3: The new 2007 oyster value also changes slightly the predictive value of the best OB model for subsite UEF. The new figure below would replace the existing Fig 3. The decline in fit is mainly due to an outlier in 1997.

Second, the statistics on disturbances plots percentage due to DBOC which hides the fact that these statistics are based on very few observations. I do not believe that a percentage of only 7 disturbances is a valid statistical measure that is meaningful to plot as is done on Figure 2B. For 2007, there were only three dates during which they claim to have observed DBOC disturbing the seals: Apr 26, Apr 29, and May 8, 2007. The reader or referee needs to analyze the database to find that out (the paper doesn't tell the referees that the 7 disturbances in 2007 are based on only three observation days over the entire year). But Apr 26 is based on the Apr 26 Trip Report which is controversial and disputed. It was not in the original Aug 2007 NPS harbor seal database (the NPS protocol says that disturbances get filled out on a certain form, which Apr 26 was not, and get entered into the database within one week, which Apr 26 was not). The engine was broken on the boat described in the Apr 26 Trip Report and the workers observed had already clocked out on shore. Lunny, the owner of DBOC, filed a complaint with the California Coastal Commission and NPS about this Apr 26 data in Sept 2007. NPS never responded. But 9 months after the date, in January 2008, NPS entered the Apr 26 Trip Report data into the database (violating several of their own protocols), and included it here in the Becker paper. At the top of page 10 in his manuscript, Becker tells us that for quality control, he excluded "data from observers with less than one year of survey experience", yet he included data that had a formal complaint filed questioning its authenticity, and that the Dept. of Interior Inspector General was investigating, as he submitted his paper. [The DOI IG was subsequently unable to render a decision on the veracity of that data.] Did Becker tell the editor or referees that some of the data was disputed and under investigation? It winds up that the May 8 data are also controversial – these observations took place on the morning of the Marin Board of Supervisors hearing. The entry claims that at the same minute (8:45 am that morning), a single oyster boat disturbed seals on three different islands (UEF, OB, and UEN) over one mile apart (giving rise to 3 of the 7 total disturbances listed in Table 1 for 2007). If Becker did the analysis without Apr 26, or without both Apr 26 and May 8, then it appears as if no meaningful increase in disturbances in 2007 vs. previous years would be found.

BHB RESPONSE - As discussed earlier, we are happy to go with the editor's discretion on this April 26 data point. While we consider the April 26 data point to be correct and the Inspector General did not support allegations that it was falsified, it is nonetheless, a data point in our database. If the editor would like it removed, we can remove it completely, or leave it in with an asterisk. I imagine removing it completely would be the cleanest way, but removing a data point due to an unsubstantiated (but thoroughly investigated) allegation would set a bad precedent. Incidentally, the 3 lowest pupping season counts (recorded by different surveyors) at this subsite since at least 2001 were seen the week of the April 26th disturbance and an undisputed disturbance later in the week by a different observer on April 29.

The second disturbance that Dr. Goodman would like to see removed is May 8, 2007. During this disturbance, the oyster boat simply traveled by three different subsites while the data only has one recorded time. Disturbances can often affect several harbor seal subsites when grouped closely together. In reality, the three disturbances may have occurred over some unspecified period of time (likely several minutes), but the recorder simply listed a single time entry as the disturbance unfolded. The fact that a single time is listed in no way suggests that the disturbances did not occur. This data point has not been formally disputed (that we are aware of), except in Dr. Goodman's comments above. The data were collected by a highly experienced surveyor (not the same person as the April 26 disturbance) and is considered by NPS to be highly reliable. Regarding the fact that the May 8 disturbance was on the same day as a Marin Board of Supervisor's Meeting discussing mariculture impacts, I believe that Dr. Goodman is implying that for this reason the data may have been falsified, but there is no evidence or allegation for this.

Dr. Goodman also implies that we are relying on only a few days of actual disturbances, this is true, but surveys occurred throughout the pupping/molting season, meaning sample size was very large (~60-160 hours/year. We believe that the implication that we should throw out data and not use it simply because the detection events are rare is not sound. Furthermore, there is no biological basis for the argument to remove them. It is also possible that oyster disturbances could be prolonged since the oyster personnel stay on the sand bars for prolonged periods, and that the

there is also displacement in the form of oyster bags left on or near the haul out sites. Please see new proposed figure 2 on Page 9 below.

Regardless, the proportion of mariculture related disturbances per hour surveyed is significantly greater after 2005 (even omitting Apr 26 data point): using proportion tests:

2-sample test for equality of proportions with continuity correction
data: c(1, 5) out of c(488, 183)
##1 oyster related disturbance in 488 hours vs. 5 oyster related disturbances in 183 hours.
X-square = 6.9528, df = 1, p-value = 0.0084
prop'n in Group 1 prop'n in Group 2
0.00204918 0.0273224

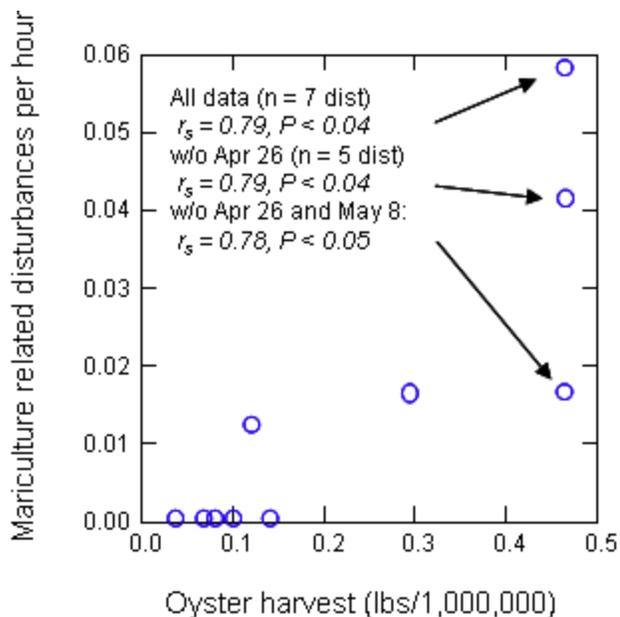
Thus, in response to Dr. Goodman's second concern, we show that even removing Apr 26th there is still a significant relationship between oyster harvest and mariculture related disturbance, and mariculture related disturbance significantly increased in 2006-2007 over previous years.

Third, In Table 1 in the Becker et al. paper, they examine total human and mariculture-related disturbances in 2006 and 2007 without calculating the disturbances per observation days (i.e., they ignore the sample size). It winds up that they made many more observations in 2007 vs. 2006. In we compare observations during the broadest definition of the pupping season (Mar-June, the months of the most observations), then the difference is 24 observations in 2006 vs. 48 in 2007 (mean for 2000-2005 = 36.8). If we compare the total number of observations over the entire year, then the difference is 39 in 2006 vs. 56 in 2007 (they stopped considering 2007 data on July 31). In either case, if we use the 7 human disturbances in 2006 and 2007, the number of disturbances per observation goes down in 2007 vs. 2006. And of course, if we eliminate the controversial Apr 26 Trip Report, and/or the May 8 record, then the number goes down even further. Finally, the one human disturbance attributed to the oyster farm in 2006 at island UEF is ambiguous and probably should not have been listed (using the QA/QC described on page 10 of Becker's manuscript). It comes from May 6 2006 and is listed in the database as "blue-yellow motorboat" and then as "possibly oyster related" whereas all other mariculture-related disturbances are definitively identified as oyster boat or oyster workers. This record is ambiguous and should not have been included. The enclosed slides show a new analysis of Table 1 considering these various issues.

BHB RESPONSE - It is untrue that we ignored sample size. In the first round of reviews at MMS, two of the three referees suggested that we perform power analyses and statistical tests on these data. Subsequently, in the current version of the paper we performed power analyses indicating high power (>0.91) and proportions tests (which explicitly model sample size) for the differences in disturbance rates from 2000-2005 versus 2006-2007 (when mariculture related disturbances began to be observed). Dr. Goodman focuses on the figure and percents and ignores our statistical tests that considered sample size explicitly.

However, we do agree with Dr. Goodman that using (disturbances / survey time) might be more appropriate. Dr. Goodman actually suggests (disturbances/survey), but since individual survey times may vary, we chose to divide by total time surveying. Regardless, either method reaches essentially the same relative values. We have therefore produced a new figure 2 that is a function of Mariculture related disturbance divided by number of hours surveyed each year (during the extended pupping/molting season: May – July). We then illustrate via spearman rank correlation (Kendall's tau also provides similar significant results) that oyster harvest is positively correlated with mariculture related disturbance from 2000 – 2007. Because the disturbance increase is so closely tied with oyster harvest and disturbance was essentially zero at low oyster harvest, this result is not sensitive to whether we include disputed Apr 26 or even the May 8 data points ($r_s = 0.78 - .079$ and all $P < 0.05$). Thus, we thank Dr. Goodman for clarifying the analysis. It is important to point out that each of the years has between 61 and 122 hours of survey effort, so sampling was quite extensive meaning that zeros are likely to actually be zeros, even though disturbance events are rare. (See response to concern #2 above for statistical tests) The correlations are highly significant because most years with low oyster harvest had no oyster related disturbance, and when oyster mariculture increased, oyster related disturbance

increased, even when dropping the points Dr. Goodman is skeptical of. Lastly, note that non-parametric rank correlation (Spearman) significance values are still significant, even with only 8 years in the time series for pairwise comparisons. Spearman tests are robust to very small sample sizes and significance tables for Spearman tests of course take into account sample size. We used S-plus for these calculations but also did 1000 bootstrap Monte Carlo simulations of correlations using Systat 12 and got similar results for r_s .



ABOVE: Proposed new Figure 2. Rather than showing percent mariculture related (current figure 2), we will merely show the relationship between oyster harvest and mariculture related disturbances. Of course, the new figure would only include the data points agreed to with the editor (i.e., for 2007 including the Apr 26 data point or not).

We feel the 2006 data point is valid, there is no reason to drop it and it has not been formally challenged anywhere except in Dr. Goodman's comments. The estero is managed as a wilderness area, meaning no other power boats are allowed in the area without explicit park service permission and paperwork, so the likelihood of a non-mariculture boat in the estero without NPS knowledge or permission is unlikely. It is also important to note that a few oyster related disturbances were omitted from the analysis in the paper because they occurred in tandem with another natural or human disturbance, so some likely mariculture related disturbances have already been omitted to be conservative.

Lastly, we hope that it is clear that the disturbance rate data and oyster harvest associations are only presented to see if there is a reasonable justification for testing the a priori hypothesis in the GLM that oyster harvest levels might affect counts in the upper estero. This a priori hypotheses, of course, explained counts quite well, but it is more robust since we have already demonstrated a significant association between oyster harvest activities and levels of mariculture related disturbance. Also, disturbance alone might not be the whole story, simple placement of bags and other gear on sandbars (see fig 1 in current MS) may discourage seals from using them.

Fourth, Becker et al. focused on the disturbances by the oyster farm but neglected to mention that they represent only a small subset of all disturbances.

BHB RESPONSE - We clearly show in table 1 that human related disturbances are higher in the middle and lower estero (higher mean per year), and that other human disturbances occur in the upper estero. We did not consider natural (predator, other seal, bird etc.) disturbances, because we were only interested in potential associations in the upper estero at the isolated sandbars (that enjoy lower general human disturbance than the rest of the estero) which are close to mariculture, where mariculture related disturbance has been increasing and appears related to oyster

harvest volume and activities. Non-mariculture human related disturbance ranged from about 0.05 per hour to 0.12 per hour from 2000-2007, but is not at all related to mariculture.

As of Apr 26 2007, in the 2 ½ years that Lunny had owned DBOC, there was not a single FW (seal getting flushed into the water, the most serious disturbance according to Sarah Allen) caused by DBOC out of a total of over 2000 FW's (if we do not consider the 24 FW's on May 6 which are listed ambiguously as "possibly oyster related").

BHB RESPONSE - The actual datasheet entry for May 6, 2006 is: "Blue-yellow motorboat, Oyster related? Zipped through channel and headed direction Johnson's (Oyster farm)". Any other boats on the estero require wilderness exemption permits and NPS permits all research on the estero, so it is very unlikely that this was not an oyster related boat. No other motorboats are allowed in the estero. Thus, we consider this a high quality data point.

For the three years 2005-2007, the database records 2,864 FW's. 38.8% were caused by Park visitors, 25.9% by low flying aircraft, 16.8% by birds, and 5.7% by predators (for many, they observed FW's but do not know the cause). DBOC represented an even smaller number. If we count the only non-controversial observation – Apr 29 – then the number of FW's caused by DBOC during 2005-2007 is 5 which is 0.2% of the total. If we include May 8, the number goes up to 118 FW's or 4.1%. If we include the Apr 26 Trip Report, the number goes up to 132 FW's or 4.6%. Regardless of whether you include only Apr 29 (0.2% of total FW's) or all three dates (4.6% of FW's), the data reveal that Becker only included less than 5% of the disturbances from 2005-2007 in his paper.

BHB RESPONSE - This paper is about three subsites in the upper estero that are near mariculture activity. We compare mariculture related disturbance to oyster harvest in this area. We point out in the paper (Table 1) that the human disturbance rate is higher in the middle and lower estero showing annual means for each subsite and then the percentage that are mariculture related. Of course, there are no mariculture related disturbances in the middle and lower estero since they are far away from mariculture activities and outside their mariculture permit. The paper states there are many disturbances in the middle and lower estero that are both anthropogenic and natural (e.g., predators and unknown causes) that certainly affect subsite use as stated in the paper. Nowhere in the paper do we imply that human related or natural disturbances elsewhere in the estero are unimportant or minimal, rather, they are not the purview of this paper. Finally, we again return to the fact that the upper estero is a group of sand islands that likely afford extra protection for pups and might be considered high quality habitat.

Second, Dr. Goodman suggests that we should be pooling all human disturbance (both mariculture and non-mariculture related) to investigate how all human disturbances might affect the upper estero. But total human disturbance should not be (and apparently is not) related to mariculture, so it is not logical to make this step to relate oyster harvest and all human disturbance.

Fifth, why didn't Becker et al. examine the other 5 subsites? Some of them, like sandbar A in 2005 and 2006 or site L in 2007, had many more disturbances due to humans than did OB or UEN. Why not run the analysis on all 8 subsites, for all years for which they have subsite disturbance records (i.e., 1997-2007 as the title implies)?

BHB RESPONSE - As discussed above, statisticians argued strongly against this approach. Also, the upper estero islands are isolated (predator free) and important mainly for pupping, the most sensitive part of seals life cycle.

Sixth, why did the NPS ignore sandbar A which had an 80% decline from 2005-2007? This was the 80% decline described by PRNS Superintendent Neubacher and Dr. Sarah Allen at the Marin County Supervisors hearing on May 8 as of "national significance". Isn't it significant enough for the analysis of the data in this paper?

BHB RESPONSE - We feel this comment is outside the scope of evaluation of this paper. Nevertheless, Dr. Goodman is incorrect. NPS never referred to subsite A as being an area of concern in any reports or testimony. Dr. Goodman has accused NPS of using data from subsite A (which is away from Mariculture and outside the lease) in testimony and

reports from 2007. In fact, NPS was describing declines at subsite OB (one of the sites modeled in the paper) which is close to mariculture and did indeed undergo an ~80% decline from a peak in 2004 (prior to mariculture activities increasing) to the point at which testimony and reports were prepared in 2007. Dr. Goodman has inferred that we were referring to subsite A (and accused us of scientific misconduct because of this) since there was also a recent large decline in seal use (actually closer to 90%) at subsite A, likely due to connection of the sandbar to land and subsequent increases in human and natural (predator disturbance). Nonetheless, by the same reasoning as our response to Dr. Goodman's fourth point above, subsite A is outside the purview of this paper since it is not subject to mariculture disturbance and modeling all subsites could be considered a form of data dredging (e.g., Laake, and other's advice).

Seventh, the title is wrong – the paper doesn't look at Drakes Estero from 1997-2007, but rather it looks at 3 out of 8 subsites in Drakes Estero, and in its statistics relies too heavily on a general decline in seals from 2005-2007. The NPS has, or has access to via Dr. Sarah Allen and her former employer (the Point Reyes Bird Observatory), the complete Drakes Estero harbor seal data from the 1970's to present. She published in a May 2007 NPS report a graph on her seal data going back to 1991. Tom Moore from California Department of Fish & Game has the oyster production numbers for all of those years. Instead of comparing three recent years in which all Pt. Reyes National Seashore harbor seal populations went down (most having no oyster farm nearby) while oyster production was going up in Drakes Estero (and thus deriving misleading conclusions), why not compare seals to oysters over a 20-30 year period and determine if there is any relationship? If you look at figures in published papers (e.g., Sydeman and Allen, 1999), you will see that during the 1990's the number of seals increased along the PRNS coast, while during the 1990's up until 1997, the number of oysters in Drakes Estero remained very high (in fact, higher than DBOC numbers in 2007).

BHB RESPONSE – We feel the paper title is adequate, however, if the editor and reviewers would prefer the word “upper” to be inserted before “Drakes Estero”, we would be happy to edit the title. Regarding too much reliance on a general decline from 2005 – 2007, this is again incorrect for two reasons. First, this is an 11 year time series, so there is equal reliance on data from all 11 years. There happens to be higher levels of oyster harvest at both the very beginning (1997-1998) and the end (2006-2007) of the time series, so this also indicates that there is representation from earlier in the dataset, and not a sole reliance on 2005-2007. Second, we explicitly modeled density dependence for each subsite which was a function of the rest of the seals at other subsites in the entire estero (excluding the subsite being modeled, of course). If it were true that a general population decline in the estero or the region explained (or even partially explained) counts at upper estero subsites, then density dependence would have been a factor in the best (lowest AIC, highest r^2) models. However, density dependence was never important and had no relationship whatsoever to counts for the two subsites modeled (and also the third subsite counts predicted by the OB model).

I have encouraged the NRC panel to get the historic data on the numbers of seals from Dr. Sarah Allen (she has all of it, and can also get it from the Pt. Reyes Bird Observatory) and the number of oyster pounds from Tom Moore of California Department of Fish & Game, and have an independent statistician run the analysis for the 1970's-2007, or at least 1991-2007. I don't think they will find much of a relationship.

BHB RESPONSE - Dr. Goodman seems to suggest here that the best way to study this system is a simple correlation test between oyster harvest and total seals in the estero. It would be nice if ecological patterns were that simple and well understood. Of course, myriad processes may affect populations, and as investigated here, counts at subsites near mariculture were best explained by ENSO and oyster harvest (as a proxy for disturbance/displacement). We are happy to make any and all of our data available should NRC or MMS wish to perform an independent analysis. However, we do NOT have subsite data consistently collected prior to 1997 except for a few (2-3) years in the mid 1980s that were part of a masters theses and do not have ready access to the original data. This was also prior to harbor seal population increases after passage of the MMPA, so it might be difficult to combine the datasets without the caveat that the Northern California population was still exponentially increasing in the mid 1980's (Sydeman and Allen 1999).

I thought that one of the most informative statements at the NRC panel meeting last Thursday Sept 4 was from Grey Pendleton in Alaska when he described recent NOAA guidelines, and said he agreed, that you can't look at single local subsites in which seals move back and forth on a day-to-day basis between different neighboring subsites, but rather you have to look at the overall population. If you consider Drakes

Estero as one functioning population as Pendleton did, then it is meaningless to consider site OB when for the greater Drakes Estero population there is no problem of any kind, as Becker himself answered in the Q&A session after his talk. The harbor seal population in Drakes Estero decreased by less than 20% from 2005 to 2007 (a normal variance), but this trend was equally reflected in and consistent with most of the other harbor seal populations along the Northern California coast.

BHB RESPONSE – I believe Dr. Pendleton’s comments were directed at looking for changes in the entire population, which is not what we have done, we are only looking at subsite use. My characterization of Dr. Pendleton’s comments are that of course population changes are more important than subsite changes and that subsites need to be counted together (pooled) for population analysis since animals move around (and this also is how NPS tracks population size). It is also true that NOAA guidelines focus on populations, as they should, but our paper and analysis is not about the population, but rather subsite use. It is also correct that we have not shown a decline in the entire estero population related to mariculture (or anything else) that is outside the purview of this paper. Furthermore, 2 recent papers (below) deal solely with habitat use and not “population” level impacts. Such information is important to understand, even if it does not directly demonstrate population impacts. In the management of marine mammals, for example, pupping areas within a larger colony may require a higher level of protection.

Bejder et al. 2006. Decline in relative abundance of Bottlenose dolphins exposed to long-term disturbance. Conservation Biology 20:1791-1798.

Watson-Capps and Mann. 2005. The effects of aquaculture on bottlenose dolphin ranging in Shark Bay, Western Australia. Biological Conservation 124:519–526.

Several other papers cited in our ms also discuss shifts or declines in habitat use by harbor seals or other marine mammals in relation to chronic disturbance events.

Concluding comments.

This paper will be read by the lay public. Becker presented it to many members of the lay public in his open talk to the NRC panel last Thursday. Many in the community are already confusing Becker et al.'s results of site utilization in Drakes Estero into believing that the paper demonstrates significant harm by the oyster farm to the harbor seal population in Drakes Estero. Thus, the paper should be worded clearly to state that it only draws conclusions on site utilization, and in no way demonstrates that the oyster operation is harming the seal population.

BHB RESPONSE – Here Dr. Goodman states that our results are indeed about site utilization, not the entire estero population, which seems to make some of his earlier criticisms moot. Both the paper and the associated presentation have been very clear in focusing on subsite use only and there is no implication that the paper suggests overall population loss in the estero due to mariculture (although that is an interesting question that might warrant a future study or analysis).

Given the way in which this NPS Becker et al. paper in press is already being presented by NPS and others in the community as showing definitive harm to harbor seals by the oyster farm, and given that Becker discovered at the NRC panel meeting on Thursday Sept 4 that he had used the wrong oyster production numbers, I would encourage you to ask Becker et al. to redo their analysis.

BHB RESPONSE – The paper shows decline in subsite use, nothing else. I agree about changing the oyster number for 2007, have done so earlier in this document, and am happy to incorporate these new numbers (new analyses presented above, released one month ago) in an update of the ms.

In his new analysis, he should use the complete 25-30 year seal and oyster production numbers beginning in the 1970's,

BHB RESPONSE – This would only be applicable to overall counts for the estero, subsite counts are only consistently available for 1997 – 2007, which is why the paper was limited to that time period. Additional counts done in the 70s and 80s were part of theses or special projects and probably not comparable to this dataset. If they were, we would have absolutely incorporated them from the outset. This is not a case of selective omission (as I think Dr. Goodman is implying), but rather a robust 11 year time series AFTER the harbor seal population in Northern California recovered after the implementation of the Marine Mammal Protection Act. There are varying seal counts, ENSO, density dependence, and mariculture production throughout the time series, creating a very complete and robust dataset that is tested against null models and time (year) effects.

use all 8 subsites,

BHB RESPONSE – As stated earlier, this was not our question (although it is an interesting one) and would probably devolve to a correlation analysis and guesswork about different subsites. Jeff Laake, USGS, and other statisticians rejected this idea (quite bluntly) from the outset.

consider the Drakes Estero population compared to other Northern California populations, and have the statistical analysis re-reviewed.

BHB RESPONSE – This was done by testing density dependence in all models. Any affects due to overall changes within the estero, or counts in other areas that may have covaried with overall estero counts would have been incorporated in the best models for OB and UEN if they were important. It turned out that ENSO and oyster harvest were far more predictive, and density dependence was a very poor predictor. This actually surprised us a bit, but it is clear that ENSO and oyster harvest were of far greater importance in this case. Perhaps since ENSO affects pupping, and the upper estero seems to be used primarily for pupping, this is why it was such a powerful predictor?

I also encourage Becker to follow his QA/QC guidelines on page 10 of his manuscript and thus exclude the controversial Apr 26 Trip Report data (and May 8 data as well). I don't think it is good science to include data for which the authenticity has been questioned (and for which an investigation could not reach a determination), and that was not entered into the database in a timely fashion and using the proper protocols (i.e., the Apr 26 Trip Report).

BHB RESPONSE – Again, we consider the Apr 26 trip report to be valid, but we defer to the Editor for guidance. The Inspector General report concerning the issue and their lack of conclusion for the allegation can be found on pages 25-27 of the Inspector General Report at: <http://www.doi.ig.gov/>. Click on “[Recently Released Reports](#)”. Regarding protocols, David Press, data manager for the project and second author of the paper, does not feel that the data point violates any protocols. In fact, the NPS pinniped monitoring protocol remains in draft form pending outside, formal peer review. Furthermore, the date that data is entered into our database has no bearing on the validity on the data itself. As stated and demonstrated earlier, removing these data points would not affect the conclusions of the paper.

I look forward to discussing these issues with you. Please let me know when we can talk by phone.

Sincerely yours,

Corey S. Goodman, Ph.D.

Member, National Academy of Sciences
Adjunct Professor, University of California San Francisco

BHB NOTE – I again would like to thank the Editor and reviewers for taking the time to evaluate these issues and proposed corrections/updates to the ms. If the editor so chooses, we can quickly incorporate the changes suggested here and into a new version of the ms.

Becker et al.
9/22/2008

Response to Goodman Comments on MMS paper

Sincerely,

Ben Becker
David Press
Sarah Allen
ben_becker@nps.gov
415-464-5247