

# APPENDIX TO MISCONDUCT COMPLAINT AGAINST MMC DR. RAGEN

## Implications of Dr. Ragen's June 17, 2012 'Private' Letter

The Becker, Press, and Allen 2011 paper asserted that NPS had "evidence" showing that the oyster farm caused a spatial displacement of harbor seals out of Drakes Estero. The paper was entitled:

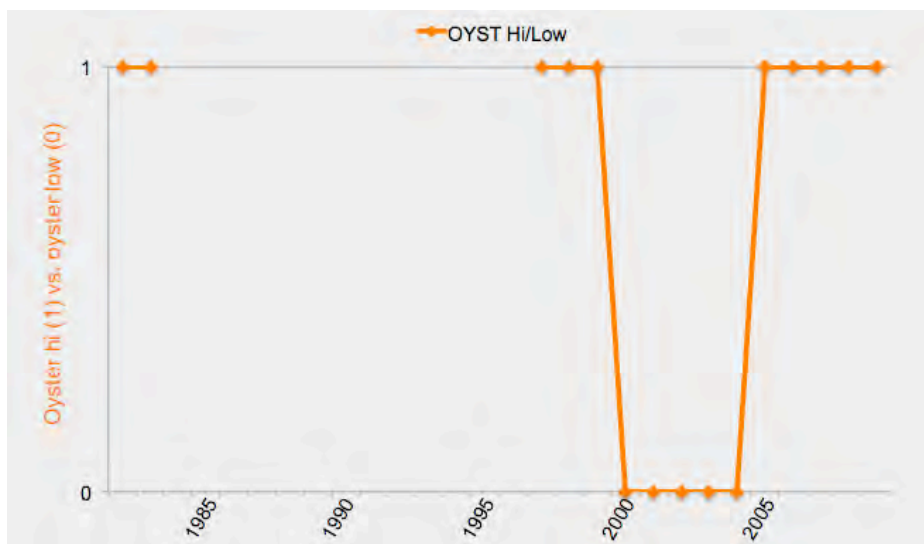
*Evidence for long-term spatial displacement of breeding and pupping harbour seals by shellfish aquaculture over three decades*

The paper used statistical analysis based upon fifteen years of harbor seal data (1982 and 1983, then a thirteen year gap, and 1997 to 2009) to conclude that as oyster farm activity increased (i.e., oyster boat and worker activity at sandbars UEN and OB during the three month pupping season March-May), harbor seals were displaced out of Drakes Estero as measured by the proportion of seal pups in Drakes Estero.

In the previous Becker et al. paper in 2009 (as reviewed by the previous NAS panel), oyster activity had been measured as annual oyster harvest (in pounds) for the entire estero. Put another way, Becker attributed 100% of the harvest from 43 designated growing areas to five designated beds at UEN and OB, a significant statistical distortion.

At public meetings, both the NAS panel in 2008, and the MMC panel in 2010, recognized that annual oyster harvest for the entire estero was not an appropriate statistical measure of oyster farm activity at sandbars UEN and OB (the harvest sites closest to – i.e. 600-700 yards from – the seal haul-out sites in Drakes Estero) during the three month pupping season (March-May). The estero contains over 40 sites for growing oysters. Having been told by two scientific panels, including the original NAS panel, that such a method was inappropriate, Becker nevertheless proceeded to do it anyway.

The Becker 2011 paper went one step further in coming up with an abstraction of oyster activity. Becker represented oyster activity as a categorical measure called OYST Hi/Low in which each year was assigned either a one (high activity) or a zero (no activity). The years 2000 to 2004 were assigned "low" or zero (activity at sandbars UEN and OB during pupping season), and all other years were assigned "high" or one. The entire NPS analysis in the Becker 2011 paper is contingent upon the validity of this categorical OYST Hi/Low designation. This categorical (and arbitrary) designation for oyster activity at sandbars UEN and OB during pupping season is shown below.



This OYST Hi/Low designation was the basis for the NPS Becker 2011 paper. Is it an accurate reflection of oyster activity at sandbars UEN and OB during pupping season? The answer is no.

Did OYST Hi/Low drive the best models in the Becker paper? The answer is yes.

Table 3. A priori models ranked by delta QAICc for the proportion of Point Reyes seal pups and total seals using Drakes Estero.  $\Delta_i$  indicates QAICc distance from the best model and  $w_i$  indicates model weight. Modeling oyster harvest as a continuous variable or low oyster during 1999–2004 gives similar results. Models ranking within the lowest 4 QAICc units (in bold) were used for multimodel inference in table 5. Oyst: low oyster harvest from 2000–2004; DP: Double Point Counts; A: proportion of Drakes Estero seals using subsite A; Dist: anthropogenic disturbance rate. Rankings are similar if using continuous oyster harvest rather than *categorical*

Age class	Model	$\Delta_i$	$w_i$	$r^2$
Pup	<b>Oyst+Double Point (DP)</b>	<b>0.0</b>	<b>0.35</b>	<b>0.51</b>
	<b>Oyst+DP+A</b>	<b>1.3</b>	<b>0.18</b>	<b>0.63</b>
	<b>Oyst+A</b>	<b>1.4</b>	<b>0.18</b>	<b>0.46</b>
	<b>Oyst+ENSO</b>	<b>2.6</b>	<b>0.09</b>	<b>0.42</b>
	<b>Oyst</b>	<b>3.3</b>	<b>0.06</b>	<b>0.26</b>
	Year	5.0	0.03	0.20
	Oyst+Dist	5.6	0.02	0.32
	Oyst+DP+Dist+A	6.3	0.01	0.66
	DP+Year	6.4	0.01	0.29
	DP	7.3	0.01	0.12
	Null	7.7	0.01	0.00
	Regional pup count	10.3	0.00	0.02

As shown above (Figure 3 from the Becker 2011 paper), all of the top five models in this ranking included the “Oyst” (OYST Hi/Low) independent variable. It was this ranking of models that led Becker et al. to conclude that they had evidence to show a correlation between oyster activity and harbor seals in Drakes Estero.

### Goodman-Lewis review of the NPS Becker 2011 paper

On August 29, 2011, and again on October 23 and November 6, Dr. Goodman and Mr. Lewis submitted reviews of the Becker 2011 paper. Goodman-Lewis concluded that:

- (1) The OYST Hi/Low categorical designation was arbitrary and incorrect in that ample oyster activity took place at sandbars UEN and ON during 2000-2004 and oyster activity did not increased at those sites in 2005 (based upon aerial photos, DBOC records, statements from previous owners and employees, and CA Department of Public Health records),
- (2) The so-called NPS correlation was in fact driven by a general trend in the regional population (that increased in 2000-2004 and then returned to baseline in 2005), and by a single stochastic (random) event that took place in 2003 (and that drove hundreds of seals into Drakes Estero during 2003 and 2004, away from danger),
- (3) The data were highly leveraged (unduly influenced) by a single year – 2003 – in which the confluence of both natural forces (the transient increase in the regional population and the lethal event at Double point) coincided,
- (4) The inclusion of 1982 & 1983 (with the subsequent thirteen year gap) biased the

data because of the intervening 1992 multi-agency protocols that prohibited the oyster boats from entering the lateral channel via the east end (and thus travelling within ten yards of the seals) and mandated that they enter via the west end of the lateral channel (and thus stay 600-750 yards away from the seals), and

- (5) The data were too thin (confirmed by MMC Report) – and too highly influenced by forces of Mother Nature (confirmed by Dr. Ragen’s June 17 letter) and the 92 protocol changes – to draw any meaningful conclusions about the impact of the oyster farm.

Goodman-Lewis concluded that the so-called NPS negative correlation of the proportion of seal pups in Drakes Estero with oyster activity (as modeled using OYST Hi/Low) was actually not driven by oyster activity (as concluded in the Becker 2011 paper), but rather driven by the total regional seal population and the 2003 lethal event at Double Point.

Inexplicably, Becker and his co-authors also excluded data (and the possibility of impact) in their analysis from shark predation, ocean food conditions, and other environmental factors about which some of the same authors were addressing in separate, unrelated reports (and about which the MMC panel members commented in Appendix F).

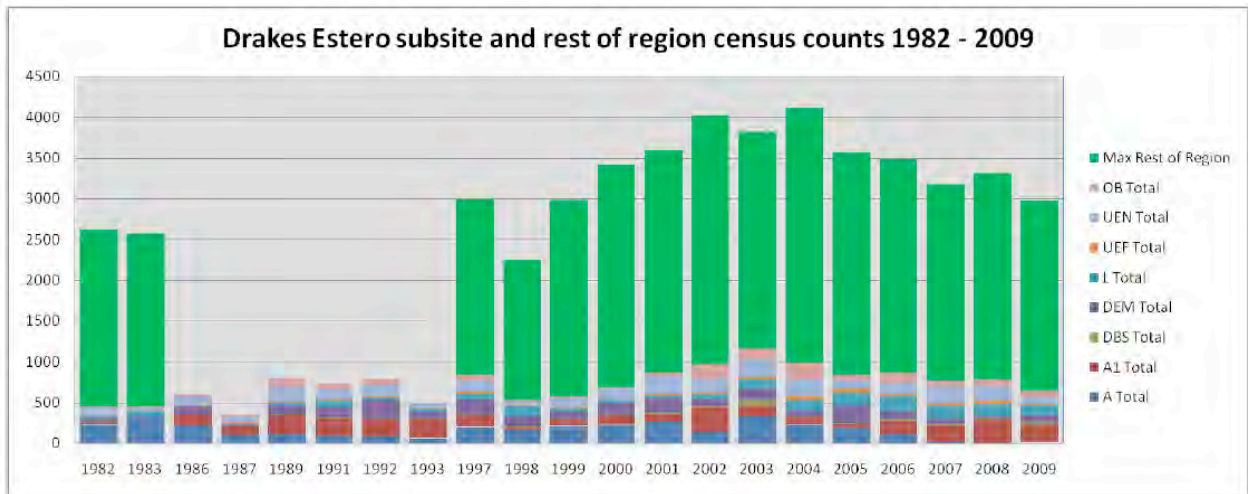
In brief, the NPS data showed that as the total regional seal population increased, due to the near-saturation of the coastal haul-out sites shared with elephant seals, a greater proportion of moms and pups entered into Drakes Estero. The regional population increased in 2000-2004, and then went back to normal in 2005 (coincidentally matching the arbitrary – and incorrect – “low” years in the NPS OYST Hi/Low categorical designation of oyster activity).

Dr. Goodman and Mr. Lewis were not the first scientists to point out that the regional population increased in 2000-2004 and then returned back to baseline in 2005. This was first noted by Dr. Francis O’Beirn (NAS panel member) in 2008 and further highlighted by Dr. Brian Kingzett (MMC panel member) in 2010.

For example, Dr. O’Beirn (Trinity College Dublin and leader at the Marine Institute in Galway) said the following after NPS Dr. Becker’s presentation at the September 4, 2008 NAS panel meeting. Dr. O’Beirn noted that regional seals increased in 2002-2004 and then decreased in 2005 throughout the region, and was thus unlikely to be due to the oyster farm. Concerning the decreased in pups in Drakes Estero beginning in 2005, Dr. Francis O’Beirn said:

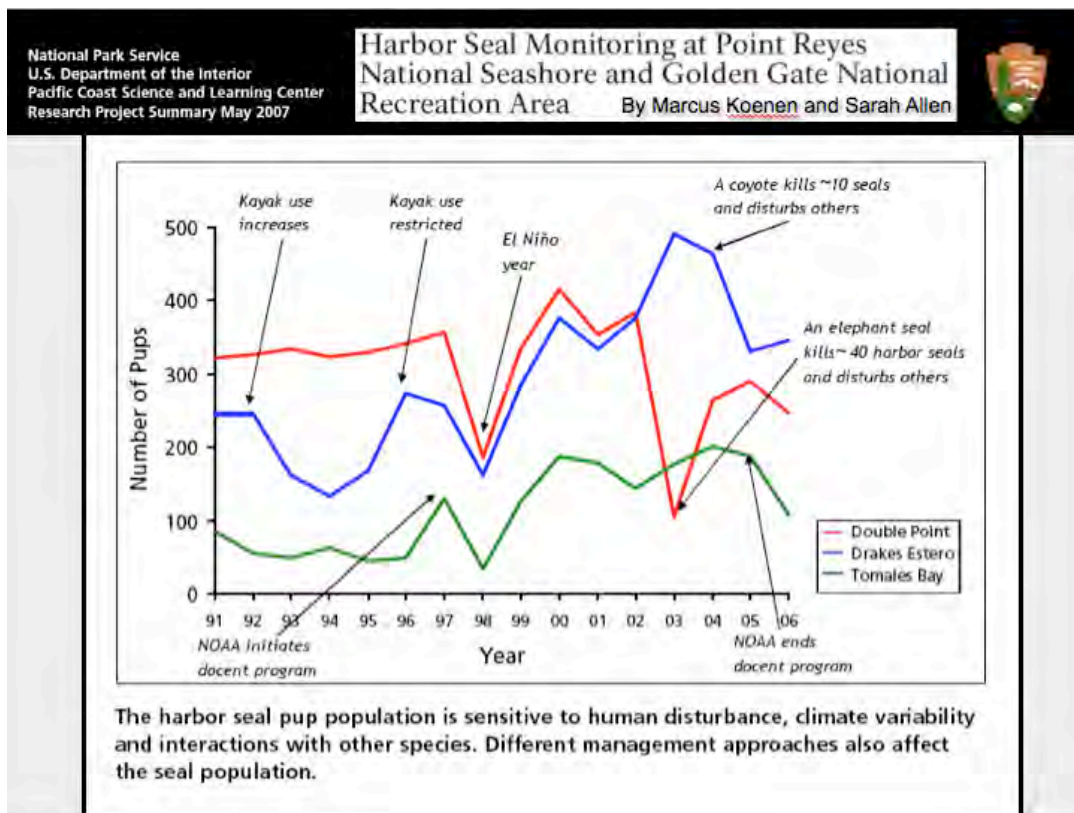
*“My point is that during the period that you’re apportioning it to aquaculture, which is the latter years, 2005, 2006, and 07, a similar decrease has been observed [at other regional sites].”*

Below is the figure from Dr. Brian Kingzett from Appendix F of the MMC Report showing the increase in the total regional seal count in 2002-2004 and the proportional increase in seal counts in Drakes Estero in those years.



On top of this ebb and flow that drove more seals into Drakes Estero from 2000-2004, was another dramatic – and more highly impactful – event. In 2003, according to NPS records, a single rogue elephant seal at Double Point (a shared haul-out site along the coast just outside the entrance to Drakes Estero) killed ~ 40 harbor seals and drove 600 off of the haul-out site; about 50% of those seals entered Drakes Estero. The normal population did not return to Double Point and re-establish the normal harbor seal distribution until 2005.

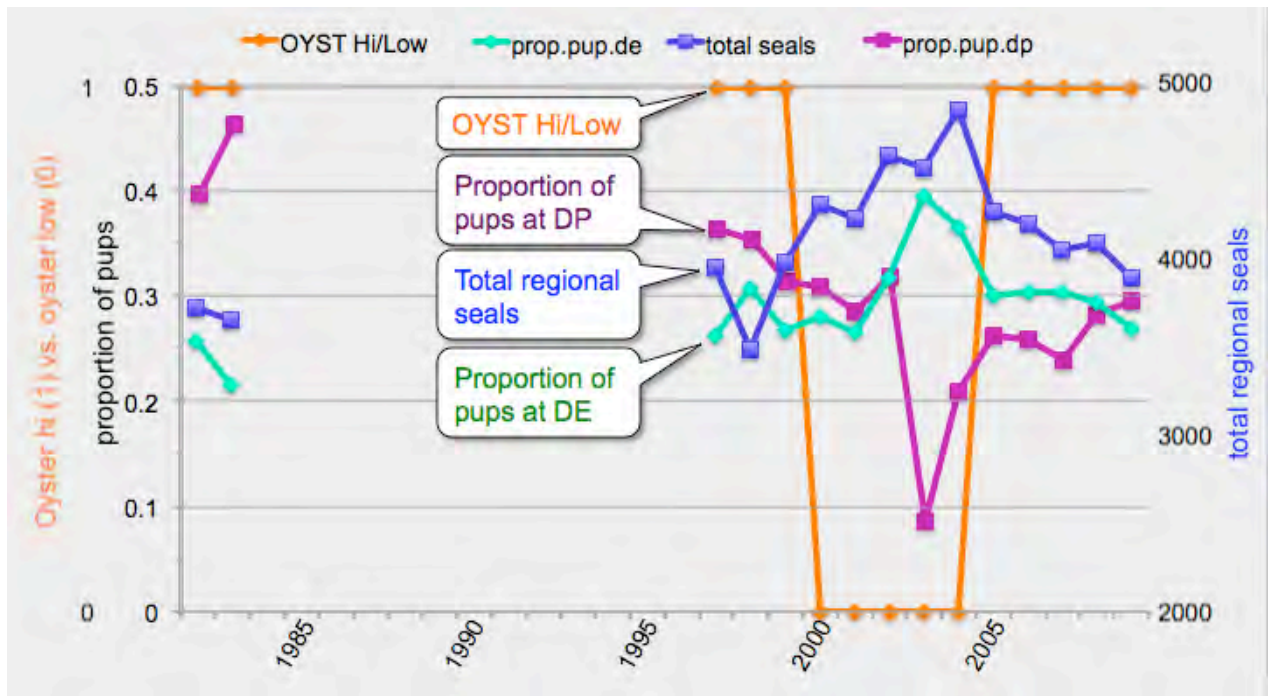
The importance of the event at Double Point in 2003 was first pointed out by NPS Dr. Sarah Allen and colleague in a May 2007 report, as shown below. Notice how the Double Point pup count (shown in red; the total seal count would be much greater) dramatically decreased in 2003, while the Drakes Estero pup count (shown in blue) increased by about 50% of the Double Point decline. Both returned back in 2005.



The Goodman-Lewis critique argued that those two natural factors – the ebb and flow of the regional population and the random lethal event at Double Point – led to the modest changes in the proportion of pups in Drakes Estero as measured in the Becker 2011 paper. The proportion went up between 2000-2004, particularly in 2003 and 2004, and then went down again in 2005, but this was not driven by the oyster activity at UEN and OB (which did not change in 2005), but rather by the forces of nature – the regional population and the lethal event at Double Point.

The arbitrary OYST Hi/Low model just happened to coincide with these natural trends by (incorrectly) designating 2000-2004 as “low” years and 2005 as a “high” year.

These relationships are shown in the figure on the next page.



The Goodman-Lewis review went on to show that better statistical models could be constructed that were far superior to NPS Becker’s best models, and that did not include OYST Hi/Low as an independent variable, but rather were based upon the total regional seal population, the pups at Double Point (DP), and the 92 protocols (i.e., before 1992, oyster boats went near seals during pupping season, whereas after 1992, they did not).

Goodman-Lewis used two methods – both generalized linear model (GML as run by Lewis) and multiple linear regression (MLR as run by Goodman) – to analyze these models. Given the small NPS dataset, the two methods gave the same results in terms of the ordering of models,  $R^2$  of the models, and relative significance of the models and individual terms.

The Goodman-Lewis best model for the proportion of pups in Drakes Estero (DE) was:

$$\text{DP pups} + \text{total regional seals} + \text{92 protocols} \quad \text{adjusted } R^2 = 0.87$$

The NPS Becker paper best model for proportion of pups in DE was:

$$\text{OYST Hi/Low} + \text{DP seals} \quad \text{adjusted } R^2 = 0.42$$

Goodman-Lewis also showed, using standard diagnostic outlier analysis, that when 2003 alone was removed from the statistics (to determine to what extent it had undue influence on the statistics), out model remained highly significant whereas the best Becker model did not.

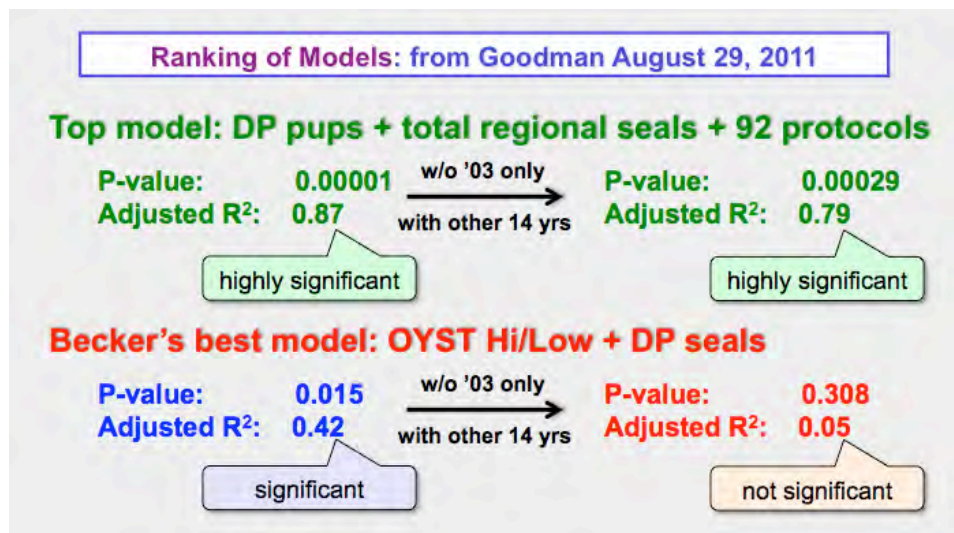
The Goodman-Lewis best model for the proportion of pups in DE without 2003 was:

DP pups + total regional seals + 92 protocols                      adjusted  $R^2 = 0.79$

The NPS Becker paper best model for proportion of pups in DE without 2003 was:

OYST Hi/Low + DP seals    adjusted  $R^2 = 0.05$

This is shown in the diagram on the next page. All together, we showed six models that were superior to any model that included OYST Hi/Low as a statistically significant, independent variable.



Put another way, NPS correctly observed in 2000-2004 (immediately before the Lunnys purchased the oyster farm) a spike in Drakes Estero harbor seal numbers, and then a dip in 2005 (immediately after the Lunnys took over the farm). NPS blamed the oyster farm for this return to normal levels. It conveniently fit the NPS “narrative of harm,” but was not supported by reasonable statistical analysis. Nature frequently pits one species against another and that happened along the Point Reyes coast. The numbers showed an ebb and flow of the total regional seal population in the middle of the decade – but when the threat (rogue elephant seal) subsided, the harbor seal herd returned to a more normal pattern of distribution.

Although under new management, little if anything changed operationally at sandbars UEN and OB in 2005. Rather, in March to May 2005, the Lunnys continued to harvest the oysters established by the Johnsons from whom they purchased the farm a few months earlier, in January 2005. They began growing new oyster seed in 2005, but those activities were not near OB or UEN (or anywhere near harbor seal haul-out areas). The new seed being processed would be transferred to UEN and OB beginning one and a half to two years later. The entire claimed correlation by NPS was manufactured. The regional seal population was ignored. The lethal event at Double Point was underestimated. DBOC operations were misrepresented.

## November 22, 2011 Marine Mammal Commission Report

The November 22, 2011 MMC Report concluded:

*“The Marine Mammal Commission believes that the data supporting the above analyses are scant and have been stretched to their limit. Nevertheless, the analyses in Becker et al. (2011) provide some support for the conclusion that harbor seal habitat-use patterns and mariculture activities in Drakes Estero are at least correlated.”*

This was Dr. Ragen’s key conclusion in his report. Dr. Ragen accepted the OYST Hi/Low designation in spite of many criticisms that there was both ample oyster activity at UEN and OB during the years 2000-2004, and that activity at those sites did not increase in 2005. Although Dr. Ragen’s panel members were critical of this categorical designation, Dr. Ragen nevertheless accepted it in his MMC Report and failed to explain how or why he contradicted conclusions from his MMC panel members.

Dr. Ragen then concluded that although the NPS data are “scant” and “stretched to their limit,” the Becker et al. 2011 paper is still correct and provides support for the NPS correlation. He went on to say that there was not sufficient evidence to prove causation, but he did find sufficient evidence – or proof – for the NPS correlation. [It is worth noting that since the release of the MMC Report, various media reports and NPS supporters have publicly equated correlation with causation, and concluded that the Becker paper showed causation, and the MMC Report agreed with NPS.]

Dr. Ragen criticized Dr. Goodman for having used the wrong method (multiple linear regression analysis) and said Dr. Becker’s method (generalized linear model) was better, when in fact Goodman-Lewis had used both methods and derived the same rank ordering,  $R^2$ , and relative statistical significance (as submitted to Dr. Ragen on October 23, 2011 and November 4, 2011). Dr. Ragen, in his MMC Report (and subsequently in his June 17 letter), either did not realize that Dr. Goodman and Mr. Lewis had conducted their analysis using both methods (including the method used by Dr. Becker), or he elected to exclude that knowledge for reasons only he can explain. The Goodman-Lewis work was not properly represented in either his MMC Report or his June 17 letter.

Dr. Ragen also dismissed the diagnostic outlier analysis as inappropriate, even though he presented such an analysis (removing 1982 and 1983) on pages 55-56 of his report. This diagnostic outlier analysis was in accord with statistics faculty at Stanford University and U.C. Davis, and multiple statistics textbooks (see our November 29, 2011 critique).

For example, the Rockefeller lecture “*Regression Diagnostics*” states:

*“Outliers can sometimes cause problems with regression results.” The problem is when “one observation is allowed to have too much influence over the regression (and any research or policy conclusions that flow from it). One solution is to report findings with and without outliers so that fair readers can make up their own minds.”*

Numerous other endorsements of the diagnostic outlier analysis are presented in the Goodman-Lewis November 29, 2011 critique of the MMC Report.

Dr. Ragen also heavily criticized the Goodman-Lewis models on page 52 of his report for having built-in co-dependencies (i.e., using the same term in more than one independent variable, or in a dependent and independent variable).

Models to explain the proportion of pups in DE	
1)	Explanatory variables: DP pups plus total regional seals plus '92 protocols $\text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}} = \text{Pups}_{\text{DP}} + \text{Seals}_{\text{Reg}} + 92$ $\text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}} = \text{Pups}_{\text{DP}} + (\text{Adults}_{\text{Reg}} + \text{Pups}_{\text{notDE}} + \text{Pups}_{\text{DE}}) + 92 \quad (\text{expand } \text{Seals}_{\text{Reg}})$
2)	Explanatory variables: DP pups plus total regional seals $\text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}} = \text{Pups}_{\text{DP}} + \text{Seals}_{\text{Reg}}$ $\text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}} = \text{Pups}_{\text{DP}} + (\text{Adults}_{\text{Reg}} + \text{Pups}_{\text{notDE}} + \text{Pups}_{\text{DE}}) \quad (\text{expand } \text{Seals}_{\text{Reg}})$
3)	Explanatory variables: DP proportional pups $\text{Pups}_{\text{DEprop}} = \text{Pups}_{\text{DPprop}}$ $\text{Pups}_{\text{DEprop}} = \text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}} \text{ and } \text{Pups}_{\text{DPprop}} = \text{Pups}_{\text{DP}}/\text{Pups}_{\text{Reg}} \quad (\text{note})$ $\text{Pups}_{\text{DP}} = \text{Pups}_{\text{Reg}} - \text{Pups}_{\text{notDP, notDE}} - \text{Pups}_{\text{DE}} \quad (\text{note})$ $\text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}} = (\text{Pups}_{\text{Reg}} - \text{Pups}_{\text{notDP, notDE}} - \text{Pups}_{\text{DE}})/\text{Pups}_{\text{Reg}} \quad (\text{substitute})$ $\text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}} = 1 - (\text{Pups}_{\text{notDP, notDE}}/\text{Pups}_{\text{Reg}}) - (\text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}}) \quad (\text{simplify})$
4)	Explanatory variables: DP pups plus total regional pups $\text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}} = \text{Pups}_{\text{DP}} + \text{Pups}_{\text{Reg}}$ $\text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}} = \text{Pups}_{\text{DP}} + (\text{Pups}_{\text{notDE}} + \text{Pups}_{\text{DE}})$
5)	Explanatory variables: DP seals plus total regional seals $\text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}} = \text{Seals}_{\text{DP}} + \text{Seals}_{\text{Reg}}$ $\text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}} = (\text{Adults}_{\text{DP}} + \text{Pups}_{\text{DP}}) + (\text{Adults}_{\text{Reg}} + \text{Pups}_{\text{Reg}})$ $\text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}} = (\text{Adults}_{\text{DP}} + \text{Pups}_{\text{DP}}) + (\text{Adults}_{\text{Reg}} + (\text{Pups}_{\text{notDE}} + \text{Pups}_{\text{DE}}))$
6)	Explanatory variables: DP proportional seals $\text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}} = \text{Seals}_{\text{DP}}/\text{Seals}_{\text{Reg}}$ $\text{Pups}_{\text{DE}}/\text{Pups}_{\text{Reg}} = \text{Seals}_{\text{DP}}/(\text{Adults}_{\text{Reg}} + \text{Pups}_{\text{notDE}} + \text{Pups}_{\text{DE}})$

Figure 20. Statistical models (single or combined explanatory variables used by Dr. Corey Goodman to explain the proportion of regional pups in Drakes Estero. The models are confounded because terms in the dependent variable are also part of the independent variable; thus, the models have a built-in dependency. (DE = Drakes Estero; DP = Double Point; Reg = Regional; notDE = not at DE; notDP = not at DP; notDP,notDP = not at DP or DE)

The problem with Dr. Ragen’s comments in his MMC Report is that, in contrast to what he said, Goodman-Lewis did not make errors or mistakes in their models. It is common in systems analysis – whether looking at the human body, complex environmental systems, social science, or economics – to find some correlation between different variables – what is called multicollinearity or correlated independent variables. It is a common statistical challenge that cannot be avoided in complex systems.

It is incumbent upon the scientist doing the analysis to make sure he/she understands what the variables are measuring, how they might inter-relate, to examine each variable alone as well as together, to make sure each variable maintains its statistical significance, and to conduct certain diagnostics to look at the impact. Typically, collinearity would lead to a lower significance of particular terms, not a higher significance, and yet our models were far superior to Becker’s, and all of our independent variables were statistically significant in our models.

Below are a few simple examples of common-sense statistics in which there are correlated variables.

Example #1: if we examine the percentage of male babies in China vs. the total Chinese population, we find that as the Chinese population increased, the percentage of male babies increased. This has led to the speculation of infanticide of female babies. We have all heard about this finding. But it contains a relationship that Dr. Ragen claims is fatal, when in fact it is not. The term “male babies” is included in the dependent variable



(percentage of male babies) and is also included in the independent variable (total Chinese population), but on one side it is part of a percentage, whereas on the other side, it is part of a numerical term. This is no different than what Goodman and Lewis did in correlation the proportion of pups in Drakes Estero to the total regional population.

Example #2: if we examine the percentage of Hispanics in California vs. the total California population, we find that as the California population increased, the percentage of Hispanics increased. This has led to the speculation that Hispanics are having more children (on average) than are Caucasian families. We have all heard about this finding as well. But it too contains the same relationship that Dr. Ragen claims is fatal, when in fact it is not. The term “Hispanics” is included in the dependent variable (percentage of Hispanics) and is also included in the independent variable (total California population), but on one side it is part of a percentage, whereas on the other side, it is part of a numerical term. This too is no different than what Goodman and Lewis did in correlation the proportion of pups in Drakes Estero to the total regional population.

Example #3: if we examine human blood pressure vs. age and number of blood pressure pills, we observe that together, these two independent variables explain some of the variability of blood pressure in the human population (along with other factors, including genetics, etc.). Blood pressure has a positive correlation with age, and a negative correlation with number of blood pressure pills. But age and number of pills are themselves highly correlated: as age increases, in general people take more blood pressure pills. But not everyone takes the medication, and the medication does not work perfectly in all people, especially as they get older, and thus the regulation of blood pressure depends upon both age and medication.

Even though these are correlated independent variables, this relationship is both valid and very informative (as are many similar statistical relationships considered in human health). It is hard to get away from correlated independent variables when considering the systems biology – the holistic interrelationships – of the human body. The natural world around us is no different.

This too is no different than what Goodman and Lewis did in looking at the relationship between the proportion of pups in Drakes Estero (our dependent variable) vs. the 2003 Double Point event (as measured by Double Point pups) and the total regional seal population. The first independent variable (Double Point pups, negatively correlated with proportion of pups in Drakes Estero) contains Double Point pups, as does the second independent variable (Total regional seals, positively correlated with proportion of pups in Drakes Estero). The sign of their correlation is opposite, so in principle, if this was a problem, they should cancel each other out. But they both make sense from a biological perspective. As shown below, when pups were removed from the second variable (by changing total regional seals to total regional adults), it made no difference – we observed the same correlation and strength of the model. Moreover, taking Double Point counts out of the total regional seal term also made no difference.

In complex systems such as medicine, the environment, social science, and economics, it is not uncommon to find multicollinearity or correlated independent variables. There are no absolute rules. Scientists must be cautious. Scientists have to carefully look at their variables individually and together, understand the relationships, look for significance of each variable, and consider the implications. In other words, it requires a deeper analysis. Dr. Goodman and Mr. Lewis did just that. Moreover, in the end, as explained below, it made no difference – the Goodman-Lewis models remained superior.

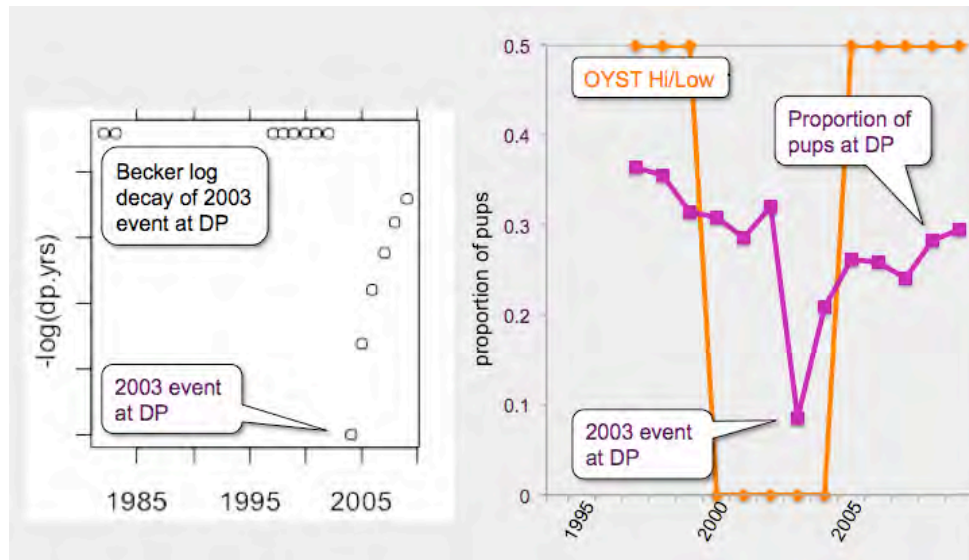
Dr. Goodman and Mr. Lewis did not have to argue about this point or get into a deep discussion of statistics: it was easy for us to modify the independent variables, get rid of

the built-in dependencies (what we viewed as non-errors), and show that the models were still far superior to Becker’s top model. Essentially, nothing changed. The top model remained DP pups + total regional adults + 92 protocols, and the adjusted  $R^2$  did not change (the second best model remained DP pups + total regional adults).

In summary, Dr. Goodman and Mr. Lewis submitted to Dr. Ragen on November 29, 2011 a modification of their models (something Dr. Ragen never gave them the chance to do prior to the MMC Report being published) – and it made no difference in the relative significance of the models. See the Goodman-Lewis critique below for a description of these models.

While Dr. Ragen did not accept the importance of the total regional seal population in his MMC Report, he did accept the importance of the event at Double Point in 2003. Having dismissed our modeling of the Double Point event, Dr. Ragen asked NPS to model the 2003 event using two different methods (either a logarithmic = log, or exponential decay = exp) and as presented on page 55 of the MMC Report, these two methods of modeling the 2003 event had  $R^2$  values of 0.72 and 0.67, far greater than the 0.26 shown in Becker’s paper for OYST Hi/Low alone.

Dr. Ragen failed to point out how much better was the model for the 2003 Double Point event than OYST Hi/Low. He also failed to point out that whereas he had criticized the Goodman-Lewis use of proportion of pups at Double Point as an independent variable, that the NPS model that he accepted in his report was essentially a mathematical model for the proportion of pups at DP (as shown statistically by showing the high correlation between the two). They are essentially the same (correlation with adjusted  $R^2 = 0.70$ ).



Finally, Dr. Ragen showed that when OYST Hi/Low was added on top of either the log or exp model of the 2003 Double Point event, that the  $r^2$  value went up (as shown below the  $R^2$  goes from 0.72 to 0.83, see Table 9 on page 55 in the MMC Report).

Table 9. QAICc ranking of models with exponentially decaying function for Double Point

Model	QAICc	$\Delta$ QAICc	$r^2$
Oyster (High/Low) + log(Double Point.yrs)	81.3	0.0	0.83
Oyster (continuous)+ log(Double Point.yrs)	84.1	2.8	0.78
Log(Double Point.yrs)	84.7	3.4	0.72
Exp(Double Point.yrs)	88.2	6.9	0.67

The problem is that Dr. Ragen broke a basic statistics rule when he accepted and published the NPS data in Table 9 in the MMC Report. The OYST Hi/Low independent variable, when added on top the exp DP variable, was not on its own statistically significant (not shown in the MMC Report, but repeated by us). Thus, the correct conclusion from Table 9 should have been that a model of the 2003 event (just as Goodman and Lewis had shown with their model based upon the proportion of pups at DP – the very relationship the mathematical models were mimicking) is far more significant than OYST Hi/Low, and that the addition of OYST Hi/Low is not meaningful because it is not significant. Dr. Ragen, however, failed to draw this conclusion.

Finally, although Dr. Ragen wrote that the MMC did not endorse the diagnostic outlier analysis, on page 55, Dr. Ragen asked NPS to perform an outlier analysis – he asked them to eliminate 1982 and 1983 and rerun their model. We had already shown that if you run their model (as in the Becker 2011 paper) without 1982 and 1983, the NPS correlation disappears. But Dr. Ragen let them run their model using new data and new metrics.

NPS did not run their models based upon the data used in their published Becker 2011 paper (the paper cited in the DEIS and the MMC Report). Rather, NPS altered the data and metrics and used numbers to which we were not provided access.

Dr. Ragen let NPS change from the maximum count during the three-month pupping season to the mean count during a one-month period. Under these conditions, the OYST Hi/Low model was still statistically significant. Notice that the OYST Hi/Low variable on its own had increased from 0.26 using the data and metrics in the Becker paper, to 0.42 using the new data and metrics (note – data never provided to us).

Thus, it was a switch of data and metrics that allowed Dr. Ragen to conclude that the NPS correlation was not dependent upon 1982 and 1983. This was no longer an analysis of the Becker 2011 paper, but rather Dr. Ragen allowed NPS to determine under what conditions of data and metrics they could still achieve statistical significance (using data we had never reviewed, and thus analyses we could not replicate).

## **November 29, 2011 Goodman-Lewis critiques of MMC Report**

On November 29, 2011, David Lewis and I submitted two detailed scientific critiques to Dr. Ragen concerning the MMC Report.

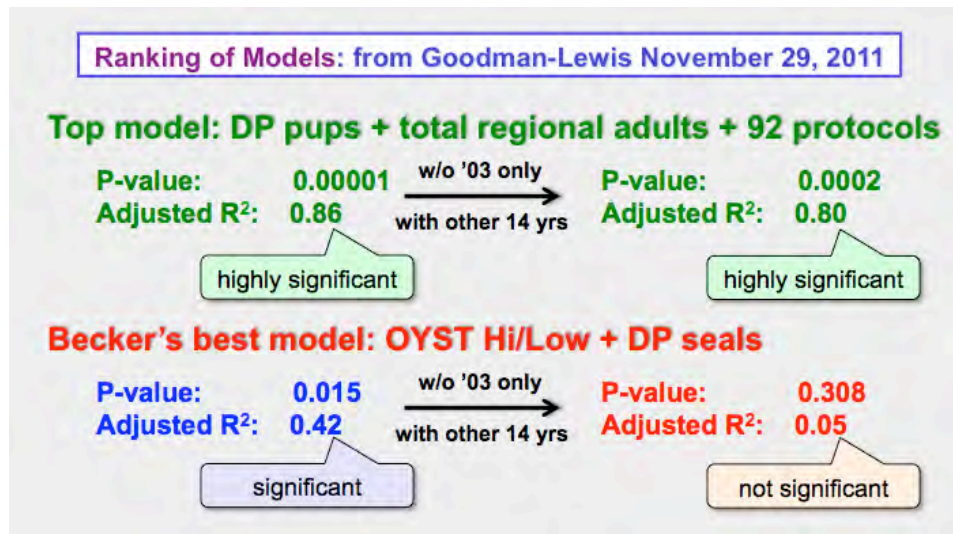
In part 1 of the Goodman-Lewis critique, we reaffirmed that the so-called NPS negative correlation of the proportion of seal pups in Drakes Estero with oyster activity (as modeled using OYST Hi/Low) was actually not driven by oyster activity, but rather driven by the total regional seal population and the 2003 lethal event at Double Point.

In part 2 of the Goodman-Lewis critique, they modified their models (according to the criticisms of them in the MMC Report, although they did not agree that such modifications were necessary) and came up with the same ranking of models:

- DP pups + total regional adults + 92 protocols was first,
- DP pups + total regional adults was second, and
- Both were far superior to any of the OYST Hi/Low models in Becker's paper.

They also showed that the exp model of the 2003 event at DP, as presented in the MMC

Report, was simply a substitute for the proportion of pups at DP (as shown above).



Dr. Goodman also submitted part 3 of their critiques to Dr. Ragen on January 6, 2012. In part 3, he examined the so-called disturbance events that Dr. Ragen accepted in the MMC Report, and showed that neither he nor NPS had the scientific evidence to conclusively support those disturbance events (more on that below).

It is worth taking a look at the conclusions Dr. Goodman and Mr. Lewis sent to Dr. Ragen on November 29, 2011. In light of his June 17, 2012 letter, they are as correct today as they were on November 29. In fact, Dr. Ragen's June 17, 2012 letter validated many of these – i.e., he has retracted his MMC Report.

Dr. Goodman and Mr. Lewis stand by every one of these conclusions, and Dr. Ragen's June 17, 2012 letter to Dr. Goodman largely confirms the Goodman-Lewis conclusions.

To conclude part 1 on November 29, 2011, Dr. Goodman and Mr. Lewis wrote:

- 1) *The elimination of the outlier data point (2003) to determine if this event highly leveraged (i.e., unduly influenced) the NPS correlation was a legitimate and appropriate diagnostic test. MMC was incorrect to reject this diagnostic test.*
- 2) *The Becker 2011 statistics are leveraged (i.e., unduly influenced) by a single stochastic (random) event -- the rogue elephant seal at Double Point in 2003: when 2003 alone is eliminated, Becker's best models are no longer significant.*
- 3) *The same conclusions are derived regardless of statistical method: Goodman and Lewis provided head-to-head comparison of GLM using AIC vs. MLR using R squared analysis, and derived the same conclusion: the NPS oyster activity model is entirely supported by the stochastic (and lethal) 2003 event.*
- 4) *Having rejected the request to ask NPS to do the outlier diagnostic test of their model, MMC instead accepted a different analysis from NPS that was purported to determine the leverage of the 2003 event on the NPS correlation. Rather than subtracting the 2003 event from their correlation, NPS instead added another independent variable that was leveraged by the 2003 event. Instead of effectively testing  $2 - 2 = 0$ , NPS in essence tested  $2 + 2 = 4$ . MMC accepted an incorrect test.*
- 5) *By adding another independent variable entirely leveraged by the lethal 2003 event, NPS essentially doubled down – they tested two variables that were both leveraged by the same event – and no surprise, found an increased R squared.*
- 6) *Although MMC stated to Kevin Lunny that it had not asked NPS to do the outlier diagnostic test, it actually had asked NPS to test their model without 2003 and 2004. In the*

*absence of 2003, the NPS model was no longer significant.*

- 7) *The MMC mistakenly accepted the NPS correlation that was highly leveraged by the random 2003 event at Double Point. The MMC made two compounded errors. First, MMC did not request the outlier diagnostic test. Second, MMC allowed NPS to do an inappropriate test that added rather than subtracted the 2003 event and never determined whether it leveraged the oyster activity model.*
- 8) *The NPS data are too thin, and too highly leveraged by a stochastic event in 2003, to be able to support the NPS correlation between harbor seals and oyster activity. Moreover, the NPS data are inadequate for MMC to affirm the NPS claim of a correlation between harbor seals and oyster activity.*
- 9) *What was called a long-term displacement OUT of Drakes Estero was actually a short-term displacement INTO Drakes Estero caused by events at Double Point. There is no evidence for long-term spatial displacement of seals and pups OUT of Drakes Estero that can be related to shellfish aquaculture.*
- 10) *The MMC mistakes could have been avoided had the MMC proceeded with their original open process rather than the insular closed process they conducted. Open dialogue, open discussion, and open exchange could have helped avoid these mistakes. Unfortunately, the closed process led to a flawed MMC Report.*

**To conclude part 2 on November 29, 2011, Dr. Goodman and Mr. Lewis wrote:**

- 1) *The Becker 2011 statistics are leveraged (i.e., unduly influenced) by a single stochastic (random) event -- the rogue elephant seal at Double Point in 2003: when 2003 alone is eliminated (the diagnostic outlier test), Becker's best models are no longer significant.*
- 2) *In his August 29 report to the MMC, Goodman presented models relying on the seals at Double Point, the total regional seal population, and the 1992 protocols that were three orders of magnitude (1000X) more statistically significant than Becker's best models, and remained statistically significant when 2003 alone was removed (i.e., were robust to the diagnostic outlier test).*
- 3) *The MMC panel members in their reports in Appendix F of the MMC report cited the ecologic importance of both the total regional harbor seal population dynamic (peaking in 2002-2004) and the stochastic event at Double Point (in 2003 with residual impact in 2004) as potential major influences on the harbor seal population in Drakes Estero, and cautioned that both tended to artificially coincide with the NPS measure of oyster activity.*
- 4) *The MMC Report rejected Goodman's top models due to built-in and linked dependencies. It is troubling that Dr. Ragen failed to raise the issue with either Dr. Goodman or Mr. Lewis. It is equally troubling that neither MMC nor NPS modified the models themselves.*
- 5) *The variable dependencies in Goodman's original models were not fatal as stated by the MMC, were easily adjusted, and when modified, gave rise to the same overall statistics and conclusions. In other words, MMC dismissed the Goodman analysis based upon variable dependencies that in the end made no difference. At the same time, when NPS contained similar dependencies, MMC was silent.*
- 6) *Goodman's modified best model (DP pups + total regional adults + 92 protocols), substituting adults for seals, has an adjusted  $R^2 = 0.86$  and a P-value = 0.00001. These values are virtually identical to those generated from the original model, and drive the same overall statistics and conclusions as in Goodman's August 29 report and the Goodman and Lewis October 23 report.*
- 7) *Dr. Goodman's top model (modified from seals to adults) is three orders of magnitude more statistically significant than Becker's best model (from Becker 2011). Moreover, it remains statistically significant when 2003 alone is removed (the diagnostic outlier test), whereas Becker's best models fail that test.*

- 8) *Becker's new models, including a mathematical model of the 2003 event at Double Point, may be guilty of the same linked dependency as Goodman's top models, but it appears as if they were not scrutinized in the MMC Report. By adding another independent variable entirely leveraged by the stochastic 2003 event, NPS essentially doubled down – they tested two variables that were both leveraged by the same event – and no surprise, found an increased R squared.*
- 9) *The NPS data are too thin, and too highly leveraged by a stochastic event in 2003, to be able to support the NPS correlation between harbor seals and oyster activity. Moreover, the NPS data are inadequate for MMC to affirm the NPS claim of a correlation between harbor seals and oyster activity.*
- 10) *What was called a long-term displacement OUT of Drakes Estero was actually a short-term displacement INTO Drakes Estero caused by events at Double Point. There is no evidence for long-term spatial displacement of seals and pups OUT of Drakes Estero that can be related to shellfish aquaculture.*
- 11) *The MMC mistakes could have been avoided had the MMC proceeded with their original open process rather than the insular closed process they conducted. Open dialogue, open discussion, and open exchange could have helped avoid these mistakes. Unfortunately, the closed process led to a flawed MMC Report.*

### **Dr. Tim Ragen's June 17, 2012 letter to Dr. Corey Goodman**

Buried in Dr. Ragen's 20-page letter of June 17, 2012, Dr. Ragen reversed the major conclusion of the MMC Report.

Below is the major conclusion from the MMC Report:

*"The Marine Mammal Commission believes that the data supporting the above analyses are scant and have been stretched to their limit. Nevertheless, the analyses in Becker et al. (2011) provide some support for the conclusion that harbor seal habitat-use patterns and mariculture activities in Drakes Estero are at least correlated."*

Dr. Ragen concluded in the MMC Report that the Becker 2011 paper provided some support for the conclusion that harbor seal distribution was correlated with oyster activity in Drakes Estero.

Does Dr. Ragen still support that conclusion? Does Dr. Ragen's letter support that conclusion? The answer is no. The quote below is from page 2 in Dr. Ragen's letter. It needs to be compared to Table 2 on page 4 of Dr. Ragen's letter (see below).

*"I asked the Park Service to analyze the mean values from counts conducted from 4/15 to 5/15 each year using their methods (generalized linear models) and yours (multiple regression). The results (Table 2) indicate that the Double Point event and regional population size may have had a significant influence on harbor seals in Drakes Estero. The results also identify the oyster low/high variable as a potential influence. Given the uncertainty associated with the analyses, the results are not proof of a correlation, but they also do not provide a basis for dismissing such a relationship."*

Dr. Ragen states that there is no proof of a correlation, a far cry from what NPS Becker, Press, and Allen said in their 2011 paper, and what Dr. Ragen supported in his MMC Report.

Dr. Ragen states that the regional population size and Double Point event may have had a significant influence on the harbor seals in Drakes Estero (an understatement – see the

best models below). This is the first time Dr. Ragen acknowledged the impact of the total regional population size.

Dr. Ragen states that the OYST Hi/Low variable is only a potential influence – a reversal of his earlier acceptance of the Becker 2011 paper claim of evidence and proof, and the way in which his MMC Report supported that correlation. What was solid is now potential, and a lesser potential than the regional population and the event at Double Point. Below is Table 2 (page 4) from Dr. Ragen’s June 17, 2012 letter. The “evidence” as Becker claimed in the title of his paper is now gone and replaced by “potential.”

Circled in green is the top model based upon regional population and Double Point counts (the 7<sup>th</sup> best model from Goodman-Lewis). Circled in red are the two methods (on the left, Becker’s generalized linear model as also run by David Lewis, and on the right, multiple linear regression as run by me). In the NPS analysis shown here, both methods give the same rankings and values. This is in agreement with what Dr. Goodman and Mr. Lewis previously showed, i.e. that both models give the same ranking and relative significance.

Table 2. Potential factors influencing the hauling patterns of harbor seals in Drakes Estero in 1982, 1983, and 1997 to 2010 based on mean counts of all harbor seals during the period from 15 April to 15 May (inclusive) at the main colonies in the Point Reyes area. Results are based on generalized linear models that produce corrected Akaike Information Criteria (AICc) or multiple regression models that produce p values. The models are ordered by their AICc values. Data and analytical results are from the National Park Service. The oyster values were updated by the California Department of Fish and Game after the Park Service requested the Department review its numbers.

Model	AICc	ΔAICc	r <sup>2</sup>	Summer 2011 oyster harvest values				Updated oyster harvest values			
				P cov 1	P cov 2	P model	Adj r <sup>2</sup>	P cov 1	P cov 2	P model	Adj r <sup>2</sup>
Regional population+ Double Point counts	213.2	0.0	0.50	0.01	0.01	0.01	0.50	0.01	0.01	0.01	0.50
oyst(L/H) + log(Double Point event)	214.2	1.0	0.46	0.01	0.06	0.01	0.46	0.01	0.01	0.01	0.46
oyst(L/H) + regional population size	214.8	1.6	0.44	0.02	0.08	0.01	0.44	0.02	0.08	0.01	0.44
oyst(L/H)	215.0	1.8	0.34	0.01	0.01	0.01	0.34	0.01	0.01	0.01	0.34
year	217.2	4.0		0.03		0.03	0.24	0.03		0.03	0.24
oyst(L/H) + exp(Double Point event)	217.5	4.3		0.02	0.36	0.03	0.34	0.02	0.36	0.03	0.34
regional population size	218.3	5.1		0.05		0.05	0.19	0.05		0.05	0.19
ENSO	219.4	6.2		0.09		0.09	0.13	0.09		0.09	0.13
oyster harvest lag + regional population	220.0	6.8		0.17	0.06	0.06	0.25	0.22	0.05	0.07	0.23
exp(Double Point event)	220.6	7.4		0.17		0.17	0.07	0.17		0.17	0.07
log(Double Point event)	220.7	7.5		0.19		0.19	0.06	0.19		0.19	0.06
oyster harvest	220.8	7.6		0.07		0.07	0.15	0.19		0.19	0.05
oyster harvest + regional population size	221.1	7.9		0.15	0.10	0.06	0.26	0.41	0.10	0.11	0.17
oyster harvest lag	221.3	8.1		0.15		0.15	0.08	0.26		0.26	0.02
oyster harvest lag + log(Double Point event)	221.8	8.6		0.06	0.07	0.07	0.23	0.15	0.12	0.15	0.14
oyster harvest + double point counts	222.2	9.0		0.04	0.19	0.09	0.21	0.08	0.18	0.17	0.12
Double Point counts	222.3	9.1		0.51		0.51	-0.04	0.51		0.51	-0.04
oyster harvest + log(Double Point event)	222.4	9.2		0.07	0.17	0.08	-0.21	0.22	0.21	0.20	0.10
oyster harvest + exp(Double Point event)	223.1	9.9		0.07	0.17	0.08	0.21	0.22	0.21	0.20	0.10
oyster harvest lag + exp(Double Point event)	223.2	10.0		0.21	0.23	0.18	0.12	0.38	0.02	0.27	0.06
oyster harvest lag + Double Point counts	224.0	10.8		0.10	0.28	0.20	0.10	0.21	0.37	0.37	0.01

<sup>1</sup> Excludes seal counts at Drakes Estero and Double Point. All other references to “regional population” exclude only Drakes Estero counts.

[Keep in mind that all of the analysis in Dr. Ragen’s June 17 letter uses data and metrics that we do not have, and that are not the same as in the Becker 2011 paper. NPS is no longer using the maximum count during pupping season (three months March-May), but rather than mean values during one month (4/15 to 5/15). A new year (2010) has been added. As a result, the OYST Hi/Low model has gotten much stronger, going from an r<sup>2</sup> of 0.26 in the Becker paper to 0.34 here in Table 2.

I remain puzzled as to how this value keeps changing, and why. If you look at the MMC Report on page 55, you will find the OYST Hi/Low model has an r<sup>2</sup> of 0.42. Perhaps this

is due to the addition of a further year in Dr. Ragen's letter compared to the MMC Report. I am not completely sure why their single most important independent variable keeps changing in every report and letter. It is a moving target, impossible for us to replicate, and difficult to compare between each new iteration and to determine precisely what NPS has done. What we have are the NPS data used for the Becker paper, and that is what we continue to use.]

Although cleverly written, this is a reversal and is in agreement with what Dr. Goodman and Mr. Lewis wrote beginning last August 2011.

Dr. Ragen's statement on page 2 is an eye-opener. It should be his key sentence and in bold font. He should have told the NAS panel on July 11, 2012. It is a cleverly written reversal. Between that paragraph and Table 2, Dr. Ragen admitted (without saying as much) that Goodman-Lewis were right all along, that the MMC Report got it wrong, and that the NPS Becker 2011 paper got it wrong.

In light of this complete change in Dr. Ragen's conclusions, it is difficult to reconcile with what he writes at the very end in his summary when he states:

*My view of this case has not changed—I continue to believe the Commission's report summarized the situation accurately. The Park Service has provided "some support for the conclusion that harbor seal habitat-use patterns and mariculture activities in Drakes Estero are at least correlated." The evidence is not overwhelming, but also cannot be dismissed.*

It is puzzling how Dr. Ragen could write this conclusion when on page 2, and in Table 2, he acknowledged that the best model (our 7<sup>th</sup> best model) contained regional seal population and the Double Point event, but not OYST Hi/Low, and that OYST Hi/Low was only a "potential" influence, and that there was no proof of correlation – the NPS claim of evidence for a correlation between oyster activity and harbor seals had collapsed.

In the complaint, I used the 'two faces of Tim Ragen' as a metaphor to describe Dr. Ragen's conduct that was contradictory and inexplicable. This is a perfect example of that phenomenon. Dr. Ragen said one thing in the MMC Report, contradicted himself in a 20-page (overdue) response letter, and then masked his reversal in the summary to that letter.

It is not necessary for us to dismiss a potential influence, but rather the burden is on NPS and Dr. Ragen to provide sufficiently strong evidence to demonstrate a correlation, which they have not. Given what is written in Dr. Ragen's June 17, 2012 letter, the Becker 2011 paper would not be published (and should be retracted), and the MMC Report should simply conclude that the NPS data are too scant and thin to draw any conclusions.

In the first sentence of Dr. Ragen's comment on page 2, he told us that he asked NPS to run their models using both methods (what Dr. Ragen cites as Becker's generalized linear method (AIC) vs. Goodman's multiple linear regression analysis (MLR). In the next paragraph, he claims that Dr. Goodman and Mr. Lewis had both run the multiple regression analysis. How did Dr. Ragen get it so wrong? Did he not read what was submitted to him? Keep in mind that Dr. Goodman and Mr. Lewis reported to Dr. Ragen on October 23, 2011, November 4, 2011, and November 29, 2011 that they had indeed run both methods in parallel, and had derived the same answer.

On October 23, Dr. Goodman and Mr. Lewis wrote to Dr. Ragen:



*“In contrast to Dr. Richard’s criticism that Dr. Goodman should have used AIC analysis (with the implied suggestion that the analysis would have been different than Dr. Goodman’s MLR analysis), we find that both statistical methods give the same answer when comparing the models: Goodman’s top models are valid and far superior to those in Becker 2011, regardless of whether one analyzes them using MLR vs. GLM. The unique influence of 2003 is clear regardless of what method is used (MLR vs. GLM) – what was called a long-term displacement OUT of Drakes Estero was actually a short term displacement INTO Drakes Estero caused by events at Double Point.”*

On November 6, Dr. Goodman and Mr. Lewis wrote to Dr. Ragen:

*“We disagree that any one statistical method is more robust given the relatively small dataset used for this analysis (15 years at most). Nevertheless, there is no need to debate the relative merits of the different approaches, since we have done parallel analysis and find both give the same results, as reported to Dr. Ragen on October 23.*

*In summary, we find that both statistical methods give the same answer when comparing the models.”*

Dr. Ragen continually criticized Dr. Goodman and Mr. Lewis for using multiple regression analysis. What is shown in Table 2 of Dr. Ragen’s June 17 letter is that both models give precisely the same result. The AIC method is shown in the left-hand three columns, and the multiple regression method is shown in the middle four columns under the banner “Summer 2011 oyster harvest values.” Note that both the order of the top four models and the precise  $r^2$  vs. adjusted  $R^2$  are identical. It makes no difference. That is what Dr. Goodman and Mr. Lewis had been telling Dr. Ragen since last October, and now Dr. Ragen and NPS Becker have agreed with them, without saying so. Why didn’t Dr. Ragen acknowledge that it made no difference which method was used? Dr. Ragen remained silent on this, but continued to claim that Dr. Goodman and Mr. Lewis used only one method. Why did Dr. Ragen misrepresent this point, and mislead the reader?

Now let’s focus on Dr. Ragen’s reversal. As shown at the top of Table 2, one of the Goodman-Lewis models (Regional population + Double Point counts) was the best model on Becker’s new table, even using the switched data and metrics. The Becker 2011 paper said that the top five models all included OYST Hi/Low. The fact that OYST Hi/Low was included in all of Becker’s top five models was the reason Becker and his colleagues concluded that they had evidence to conclude that increased oyster activity led to a spatial displacement of harbor seals out of Drakes Estero.

Dr. Ragen’s MMC Report supported the NPS correlation by concluding that the top models all included OYST Hi/Low. Here for the first time, Dr. Ragen shows a new version of the NPS top models with the top model being “Regional population + Double Point counts.” That is one of our weaker models (dating back to my August 29 review of the Becker paper), not one of the models from Becker’s paper, and it does not include OYST Hi/Low.

[To account for the so-called error of including pups and adults from the same location in different terms, Becker excludes Drakes Estero and Double Point from the “Regional population” here – thus weakening the relationship by excluding a lot of the numbers – this was unnecessary – as the Chinese proportion of male babies vs. Chinese population example shows – but nevertheless, we have now done the same, just to silence Dr. Ragen’s final criticism. This is not a problem, but we have done it regardless, just to show that our models are superior to

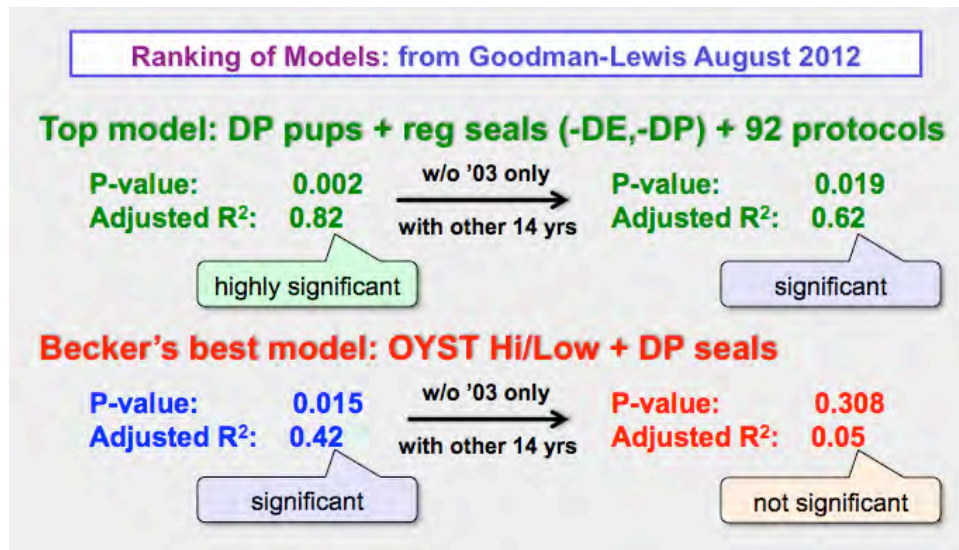
Becker's.]

In other words, on page 2 of his letter, Dr. Ragen acknowledged that the best model includes regional population and the 2003 Double Point event. That is precisely what David Lewis and I have been saying since August 29, 2011. Both Drs. Ragen and Becker have embraced one of our weaker models as better than any of Becker's models, but they don't explicitly say so, and don't point out the implications of such a revelation.

It winds up that Drs. Becker and Ragen have only included one of the Goodman-Lewis weaker models. Dr. Goodman and Mr. Lewis have six models that are stronger yet, and far superior to Becker's best model.

If they make the same correction for pups vs. adults (and thus exclude Drakes Estero and Double Point from the regional seals variable) for their best model, they come up with the following:

DP pups + regional seals (-DE, -DP) + 92 protocols = adjusted R<sup>2</sup> 0.82



## Ranking of Models: from Goodman-Lewis August 2012

notes	In Becker 2011?	model	P-value	Adj R <sup>2</sup>
Goodman model	no	DP pups + reg seals (-DE,-DP) + 92 protocols	0.002	0.82
Goodman model	no	exp DP + 92 protocols	0.001	0.77
Goodman model	no	exp DP + reg seals (-DE,-DP) + 92 protocols	0.005	0.75
Goodman model	no	DP pups + reg seals (-DE,-DP)	0.005	0.66
Goodman model	no	exp DP + reg seals (-DE,-DP)	0.006	0.65
MMC Report model	no	exp DP	0.002	0.63
Goodman model, Ragen letter	no	DP total + reg seals (-DE,-DP)	0.01	0.58
Ragen letter	no	OYST Hi/Low + log DP	0.01	0.46
Ragen letter	no	OYST Hi/Low + reg seals (-DE)	0.01	0.44
Ragen letter	yes	OYST Hi/Low	0.01	0.34
Ragen letter	no	OYST Hi/Low + exp DP	0.03	0.34

With each round of Dr. Ragen's criticism, and each modification from Dr. Goodman and Mr. Lewis, we've seen the Goodman-Lewis top model remain the top model:

- August 29, 2011 (Goodman):
  - DP pups + regional seals + 92 protocols                      adjusted R<sup>2</sup> 0.87
- November 29, 2011 (Goodman and Lewis):
  - DP pups + regional adults + 92 protocols                      adjusted R<sup>2</sup> 0.86
- August 2012 (Goodman and Lewis):
  - DP pups + regional seals (-DE, -DP) + 92 protocols      adjusted R<sup>2</sup> 0.82

For all of Dr. Ragen's repeated criticisms, the Goodman-Lewis best model remains the best model, and is far superior to any of Becker's best models that include OYST Hi/Low. Becker and Dr. Ragen just happened to use a lesser Goodman-Lewis model [Regional population (-DE, -DP) + Double Point counts] that wasn't as good as the best models (using Double Point pups instead of total seals, and the 92 protocols), and yet they still found that it was the best model.

Dr. Ragen and Dr. Becker should be asked to explain their justification for selecting a lesser Goodman-Lewis model to include in Table 2, and not the best model or models.

Dr. Goodman and Mr. Lewis have been saying over and over again since August 29 that the best models included Double Point pups (not adults or total seals), and the 92 protocols. Drs. Ragen and Becker chose to ignore our best models, but still found that a weaker Goodman-Lewis model was better than Becker's.

That is what has happened over the past year. None of these modifications made any difference, and none of them was necessary. Dr. Ragen was wrong about the statistics. Nevertheless, Dr. Goodman and Mr. Lewis changed from total regional seals (adjusted R<sup>2</sup> = 0.87) to total regional adults (adjusted R<sup>2</sup> = 0.86) and now to total regional seals

minus Drakes Estero and Double Point (adjusted  $R^2 = 0.82$ ). It didn't make any difference. That model – involving the 2003 Double Point event, the total regional population, and the 92 protocols – reigns supreme over any of the NPS Becker model involving OYST Hi/Low.

Keep in mind, the OYST Hi/Low categorical measurement is not an appropriate measure of oyster activity, so the Becker models are irrelevant to begin with. All of this is much ado about nothing, since the basic tenet of the NPS correlation – that the OYST Hi/Low categorical model is a reasonable measure of oyster activity at sandbars UEN and OB during pupping season – is incorrect, and NPS and MMC have been told this.

Imagine what Dr. Ragen might have been compelled to write on page 2 of his letter had he and Becker included the Goodman-Lewis best models (of which they have six models that are superior to the one Dr. Becker picked). Dr. Ragen would have found it impossible to say that OYST Hi/Low was still a potential influence. But even writing that it was a potential influence is far different from the Becker 2011 paper concluding that they had conclusive evidence to support the correlation, and also to suggest causation.

Moreover, Dr. Ragen's MMC Report supported the NPS correlation, something Dr. Ragen now admits is only a "potential influence," does not constitute "proof of a correlation," but cannot yet be completely dismissed. How would those words change when confronted with six better models?

**In summary, Dr. Ragen's statement, and the results shown in Table 2 (June 2012), constituted a reversal of his major conclusion from the MMC Report (November 2011), and were in agreement with what Goodman and Lewis had been saying for nearly one year (August 2011).**

## **Goodman's so-called statistical mistakes highlighted in Dr. Ragen's June 17 letter**

Dr. Ragen's reversal is cleverly disguised in a document that claims to show that Goodman made errors. The major errors are documented on page 6 (see below).

What is shown as Dr. Goodman's errors on page 6 of Dr. Ragen's letter are in fact not from the Goodman-Lewis November 29, 2011 critiques, but from Dr. Goodman's August 29, 2011 review of Becker's paper. Dr. Ragen presented his analysis as if he were analyzing the Goodman-Lewis November 29 critique, but he did not. Dr. Ragen switches documents. Dr. Ragen comments in his June 17, 2012 letter are on the August 29, 2011 review of NPS data in Becker 2011, not the November 29, 2011 critique of his MMC Report that he was asked to respond to, and said his June 17 letter addressed.

Dr. Ragen misled the readers of his June 17, 2012 letter. This is simply a repeat (cut and paste) of the exact equations in Figure 20 on page 52 of the MMC Report. Dr. Ragen misled the readers of his letter to think that we had made these so-called mistakes in our November 29, 2011 critiques, but Dr. Ragen got it wrong. It's not clear that Dr. Ragen understands what he was reviewing. This doesn't matter.

Dr. Goodman and Mr. Lewis made the modifications, and it made no difference, and they said so in part 2 of their November 29, 2011 critiques. Thus, why did Dr. Ragen repeat these criticisms in his June 17 letter and mislead the reader to think they were relevant to the Goodman-Lewis November 29 critique? They were not. The fact that their (7<sup>th</sup> best)

model comes to the top of Dr. Ragen’s Table 2 shows just how irrelevant all of this is. Below are Dr. Ragen’s criticisms on page 6 of his June 17, 2012 letter.

- First, you included built-in dependencies between the dependent and independent variables (as illustrated in Figure 20 of our report). For example, your analysis of the “independent” variables  $Pups_{DP}$  plus total regional seals can be written—

$$Pups_{DE}/Pups_{Rg} = f(Pups_{DP} + Seals_{Rg})$$

or expanded to—

$$Pups_{DE}/Pups_{Rg} = f(Pups_{DP} + (Adults_{Rg} + Pups_{Rg}))$$

or to—

$$Pups_{DE}/Pups_{Rg} = f(Pups_{DP} + (Adults_{Rg} + Pups_{notDE} + Pups_{DE}))$$

In essence, your dependent and independent variables are mathematically linked, rendering your interpretation of  $R^2$  and associated  $p$ -value incorrect and invalid.

- Second, in other cases you linked your independent variables. For example, your analysis of the “independent” variables  $Seals_{DP} + Seals_{Rg}$  can be written—

$$Pups_{DE}/Pups_{Rg} = f(Seals_{DP} + Seals_{Rg})$$

or expanded to—

$$Pups_{DE}/Pups_{Rg} = f(Seals_{DP} + (Seals_{DP} + Seals_{notDP}))$$

In this case, the linkage between independent variables means that the same data are used in fitting more than once—a violation of the usual assumptions for this kind of model. The linkage does not bias the magnitude of the relationship between the dependent and “independent” variables, but it does need to be taken into account to calculate correct  $p$ -values.

Dr. Ragen’s assertion that Dr. Goodman and Mr. Lewis made a mistake is gratuitous and misleading. Dr. Ragen intentionally spent an entire page of his letter talking about mistakes Dr. Goodman had made, when in fact, the issue (or non-issue) had long since been addressed in the Goodman-Lewis November 29, 2011 critiques. These criticisms did not apply to what Dr. Goodman and Mr. Lewis submitted on November 29, 2011. Why did Dr. Ragen mislead the readers of his letter? Did Dr. Ragen understand what was being reviewed? What was his intention here?

## Dr. Ragen accused Dr. Goodman of misrepresenting DBOC data and NPS testimony

There are two issues concerning disturbance records, and how they are described in Dr. Ragen’s June 17, 2012 letter. First, was Dr. Ragen correct in accepting a few of the NPS disturbance records? Second, was Dr. Ragen correct in his harsh criticisms?

In the MMC Report, Dr. Ragen wrote:

*“After examining individual disturbance records, the Commission concludes that, from time to time, mariculture activities have disturbed the seals.”*

Dr. Ragen accepted four mariculture-related disturbances over the past seven and one half years as evidence that, from time to time, mariculture-related activities have disturbed the seals.

In spite of what Dr. Ragen wrote in his June 17, 2012 letter, the evidence continues to be

very weak, and in many cases both controversial and suspicious, concerning these four so-called disturbance events. Two of them were previously examined by the NAS panel back in 2008-2009 and were not accepted. Dr. Ragen did a poor job of defending his acceptance of these disturbances. Perhaps to deflect attention from the records themselves, and his arguments for accepting them, his letter was full of harsh criticisms – and accusations of bias and lack of integrity directed against Dr. Goodman.

Here is brief synopsis of the four disturbance events that Dr. Ragen continues to support.

**April 26, 2007:** Sarah Allen's Trip Report -- Dr. Ragen says Dr. Goodman misrepresented the DBOC electronic time clock records. One of his main examples involved the time clock record of Jorge Mata Jr. as shown below. It was Dr. Ragen who misrepresented the DBOC records. Dr. Goodman carefully scrutinized them and interviewed DBOC owner Kevin Lunny. Dr. Ragen got it wrong. There are many reasons why this record is highly suspicious. The engine was broken and the oyster workers had clocked out by the time the so-called disturbance took place. Many details in the Trip Report are inconsistent with the physical conditions of the lateral channel and with the NPS photographic record. It may have been fabricated. NAS called it controversial. The MMC Report should have said the same. See Dr. Goodman's January 6, 2012 critique to Dr. Ragen for further details.

**April 29, 2007:** This was a Sunday. DBOC records show that none of the oyster workers were working that Sunday (which is typical – they rarely work on Sundays). There are many other reasons why this record is suspicious at best. NAS called it controversial. The MMC Report should have said the same. See Dr. Goodman's January 6, 2012 critique to Dr. Ragen for further details.

**March 14, 2008:** A NPS volunteer claimed a disturbance took place right as she arrived at her viewpoint nearly one mile away. A second person, NPS staffer Sarah Codde, was at the same time 1000 feet away (~ one fifth the distance) from the seals, witnessed the oyster boat come and go, was focused on the seals and potential disturbances, and said no disturbance took place. The volunteer arrived late. Sarah Codde wrote down that no disturbance took place. Her written records are clear. She gave testimony under oath to DOI Field Solicitor Gavin Frost and told him that no disturbance took place. DOI Field Solicitor Gavin Frost wrote this in his scientific misconduct report. Nevertheless, Dr. Ragen concluded that this was a bonafide disturbance.

**May 15, 2008:** This NPS disturbance was NOT presented at Dr. Ragen's June 7, 2010 MMC meeting with NPS to discuss all disturbance records. The NPS scientists never mentioned it, and never put it on their disturbance list. This so-called disturbance was not asserted in Dr. Ragen's June 2011 draft version of his MMC Report. Rather, it suddenly appeared in the final MMC Report as evidence for a disturbance. The NPS scientists Dr. Sarah Allen and Sarah Codde told Gavin Frost that the NPS photographs did not show any bona fide DBOC disturbances, yet this assertion of a disturbance is based upon the NPS photographs. Nevertheless, without discussion with Kevin Lunny or me, Dr. Ragen uses the NPS photographs to claim a disturbance. He is wrong. Dr. Ragen bases his conclusion on Sarah Codde's photos. In this case, Sarah Codde told Gavin Frost that none of her photos showed a bona fide disturbance. Nevertheless, Dr. Ragen concluded that this was a bonafide disturbance based upon Ms. Codde's photos.

## **Dr. Ragen claimed Dr. Goodman misrepresented DBOC records concerning the April 26, 2007 disturbance record**

In the final paragraph of Dr. Ragen's letter on page 20, he accused Dr. Goodman of having misrepresented the information on time cards. He was citing the DBOC records from April 26, 2007 as shown on page 11 of his letter.

It was his criticism of Dr. Goodman's lack of scrutiny of the DBOC records from April 26, 2007 that led the Point Reyes Light, on the front page of its July 26, 2012 issue, to paraphrase Dr. Ragen as saying:

*"He also argued that Dr. Goodman did not apply the same level of scrutiny to the oyster company's records as he did to those of the National Park Service."*

If this were a true statement, then it would show that Dr. Goodman is biased and failed to review DBOC business and operational records. But it is not correct.

Concerning April 26, 2007, Dr. Ragen asserted that Dr. Goodman did not properly scrutinize the DBOC records. The fact is that it was Dr. Ragen who did not properly scrutinize the DBOC records, and misrepresented them in his June 17 letter. The case in point is the time card of Jorge Mata Jr. on the bottom of page 11 on Dr. Ragen's letter.

Dr. Ragen told us that Jorge Jr. clocked out at 6:54 pm, well after the so-called disturbance that took place between 4:30 and 5 pm. But look at the "in" column to the left of the "out" column. Jorge Jr. clocked in at 5:11 pm, and the location of the so-called disturbance was 20 minutes away by boat, and it took place long before he clocked in on shore.

Jorge Mata Jr., as Dr. Ragen was told, is the deliveryman. He does not work on the oyster boats. He is not an oyster worker. He was loading his truck for delivery to restaurants and distributors throughout the Bay Area the next morning.

The NAS panel in 2009 did not accept the April 26, 2007 Trip Report for a plethora of reasons. Dr. Ragen insists upon considering it valid, and so he misrepresented the DBOC records, and incorrectly accused me of being biased in my analysis of data.

Dr. Ragen's whole analysis of disturbance records on pages 11-20 is suspect. The more you dig into the details, the more Dr. Ragen's statements fall apart. Some of Dr. Ragen's explanations are so far-fetched as to make you scratch your head and wonder what Dr. Ragen is thinking, and whether this is politically- rather than scientifically-motivated. He is on very thin ice in his defense of the NPS disturbance records.

Below are the time card records shown on page 11 of Dr. Ragen's letter. Below that is a larger image of Jorge Mata Jr.'s time card. Look at the seventh line that is the April 26 record.

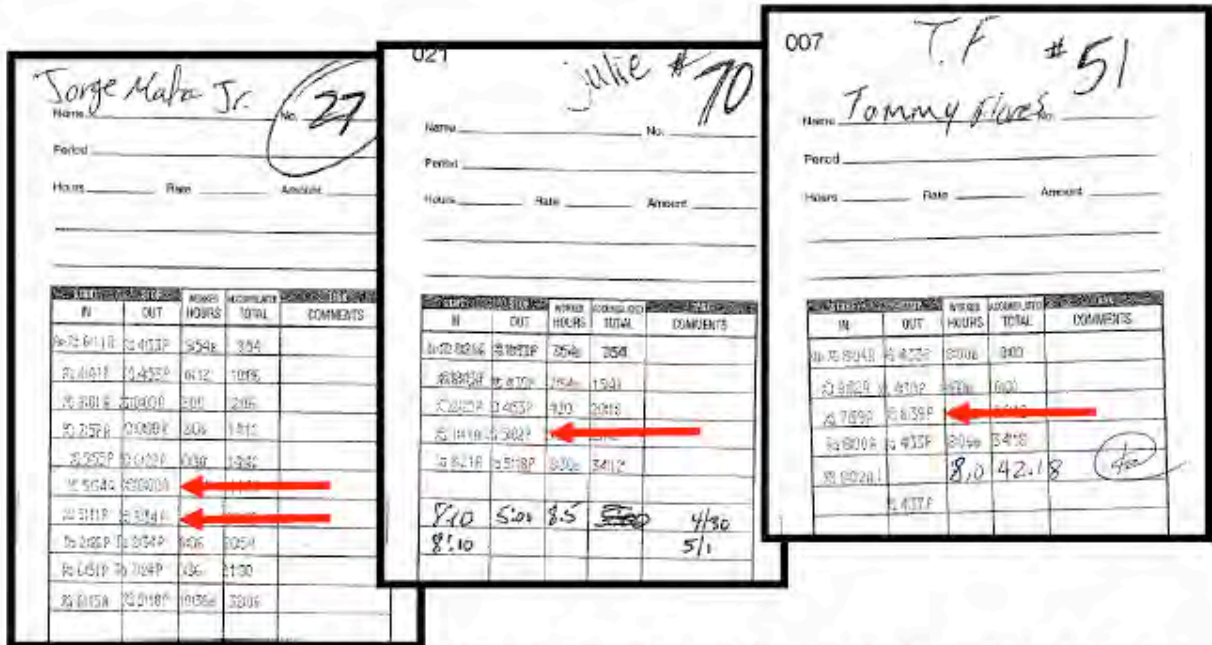
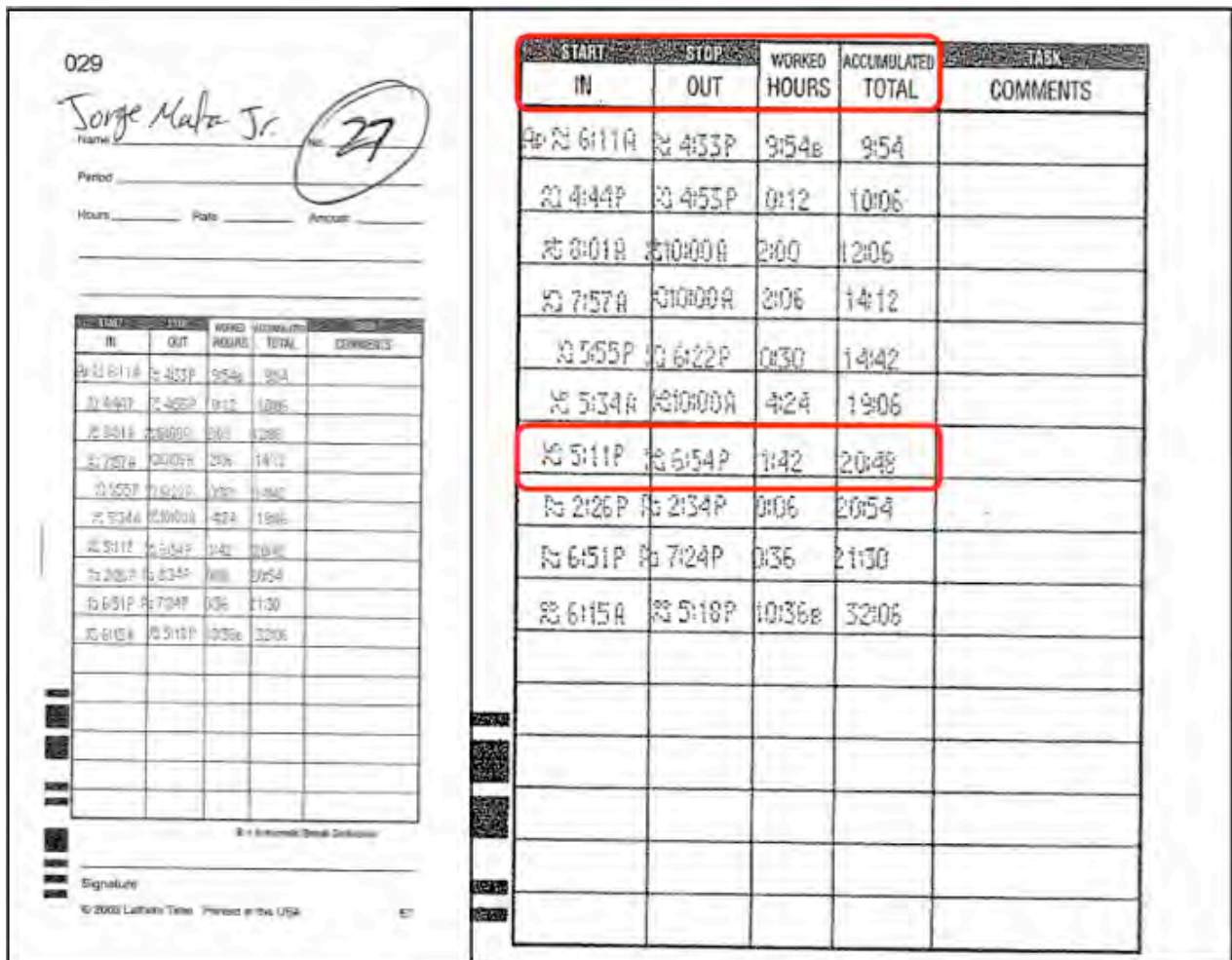


Figure 5. Time cards for Jorge Mata, Jr., Julie, and Tommy Flores. On 26 April they last punched out at 6:54 pm, 5:02 pm, and 6:39 pm, respectively. Day of month is located in "IN" column and rotated 90° counterclockwise.





## **Dr. Ragen claimed Dr. Goodman mischaracterized statements by NPS scientists concerning the March 14, 2008 disturbance record**

In the final paragraph of Dr. Ragen's letter, he wrote:

*"In my view, your arguments against that conclusion misrepresent mariculture activity in the OB/UEN area from March to August 2007, misrepresent the information on time cards, and mischaracterize statements by Park Service staff as to the utility of their data on disturbance events."*

His statement that Dr. Goodman mischaracterized statements by NPS staff refers to the March 14, 2008 event. On page 15 of Dr. Ragen's letter, he accused Dr. Goodman of mischaracterizing the Park Service views and says this is "*more troubling*." More troubling? What Dr. Goodman concluded in his January 6, 2012 critique concerning the March 14, 2008 event was the following:

- *This disturbance record from an NPS volunteer is inconsistent with the photographs.*
- *This record is contradicted by the observations of NPS Sarah Codde who was closer in distance to the harbor seals and noted no disturbance in her database.*
- *Sarah Codde told Field Solicitor Gavin Frost that no disturbance took place.*
- *The Frost Report stated this record was not supported by Codde's observations.*
- *This record is suspicious and controversial, and should not be included in the MMC conclusion.*

These conclusions are as true today as they were on January 6. The basis for Dr. Goodman's conclusions came largely (but not entirely) from the Frost Report from DOI Field Solicitor Gavin Frost. All of the details can be found in Dr. Goodman's January 6, 2012 critique of these disturbance records. Quotes from the Frost Report are included below. Does Dr. Goodman's critique deserve to be called a mischaracterization and troubling? Was this a fair and reasonable statement on Dr. Ragen's part?

According to Gavin Frost, NPS scientists Dr. Sarah Allen and Sarah Codde told him that NPS had no record of disturbances in both the photographs and Sarah Codde's independent database (videos and logs). Concerning the March 14, 2008 disturbance record in the MMC Report, the Frost Report is clear: Sarah Codde told Frost that her mission was to observe disturbances, that she witnessed the oyster boat come and go that day, that she was close to the seals, and that no disturbance took place.

Dr. Ragen's letter implied that NPS scientists told him something else. Did NPS scientists tell Field Solicitor Gavin Frost one thing under oath, and then tell the MMC Executive Director something else? Dr. Ragen then criticized Dr. Goodman (and called Dr. Goodman's actions "troubling") for raising their testimony as reason to question the validity of this so-called disturbance record.

The volunteer who claimed to witness the disturbance arrived right at the beginning of the so-called event, was 4500 feet from the seals (Sarah Codde was 1000 feet away), got the arrival time of the oyster boat wrong (Sarah Codde got it right as verified by the NPS photographs), got other details wrong, but said a disturbance took place. Sarah Codde was much closer to the seals – nearly five-fold closer.

The volunteer who got the times wrong authored another controversial disturbance record – May 8, 2007 – that Dr. Ragen did not support and that was not verified by the

NPS photographs. The volunteer at 4500 feet claimed to witness a disturbance whereas the NPS staffer at 1000 feet and focused on potential disturbances noted that no disturbance took place. Moreover, these records are being treated as if they are regulatory records, when they are not. NPS disturbance records are part of a long-term monitoring program, with the data being entered largely by community volunteers.

Below is what Dr. Goodman wrote about March 14, 2008 in his January 6, 2012 critique.

Ragen concluded that the NPS photographs for March 14, 2008 neither confirmed nor refuted the reported disturbance (the photos show the oyster boat but not the seals), and thus he apparently accepted the observer's record of a mariculture-related disturbance. Ragen's narrative made his conclusion a bit ambiguous.

DBOC acknowledged that this was likely its oyster boat and workers, but pointed out that although the observer got the time of departure of the boat correct (as indicated by the time-stamped photos), that the observer's records were otherwise inconsistent with many other aspects in the photos, including the arrival time of the boat (i.e., when the disturbance was said to have occurred). The boat arrived ten minutes after the observer recorded the arrival. The observer wrote that the "man in white jacket does all work" whereas the man in the dark blue jacket did all of the work at the beginning. There are many inconsistencies at the beginning of this record, right when the disturbance was said to have occurred, and right when the observer said she first arrived at the distant observation point. Given these inconsistencies, we did not find the record "sufficient" to conclude that a mariculture-related disturbance took place.

But you need not take our word for it. The NPS scientists refuted this disturbance record in one of their databases (appendix E of MMC report), and told DOI Field Solicitor Gavin Frost (March 22, 2011 Frost Report), that no disturbance took place. The NPS denial of the disturbance to Gavin Frost raises serious questions about the veracity of this observer (the same person responsible for the controversial May 8, 2007 record that Ragen concluded was inconclusive and that we have disputed).

In December 2010, NPS released yet another photographic and observer database for the 2008 pupping seasons (that they had never disclosed to the NAS), this one focused solely on the potential disturbance of harbor seals by the oyster farm. The person responsible was Sarah Codde who reported to Dr. Sarah Allen. Codde was responsible for taking care of the two secret cameras which were in a location on the east shore of Drakes Estero far closer to the OB seals and oyster boats and workers at UEN/OB than the normal volunteer observation point on the western bluffs across from sandbar A. According to documents obtained through FOIA, Codde and others helping her spent many hours, many days each week, at the camera location, watching, recording, and filming the oyster boats and seals.

In the Frost Report, S1 = Dr. Sarah Allen and V1 = Sarah Codde. On page 11 of the Frost Report, Gavin Frost wrote:

*"S1 also takes full responsibility for the 2008 actions of, and data gathered by V1, whom S1 had instructed to attend the cameras three to four times each week from March-June 2008, to replace batteries and memory cards as needed, to adjust the focus and angle of the cameras as needed, to review the photographic images collected, and to prepare a written log of that review, through which V1 strictly confined her/his attention to DBOC mariculture activities, disturbances, and potential disturbances of harbor seals in upper Drakes Estero. As part of a now discontinued pilot study, S1 further directed V1 to monitor (while at, approaching, and departing the cameras) the harbor seals visually for population increases and decreases at favored haul-out subsites, to document observations, to monitor and memorialize all visible DBOC mariculture operations, and to record, in writing and with still camera and video equipment, all pinniped disturbances that V1 observed in upper Drakes Estero."*

As shown on page E-2, Sarah Codde (the observer whose name is blackened out on the left hand margin) wrote in records 6-8 that she was present from 12:33 pm to 13:02 pm, the time period fully including the arrival and departure of the oyster boat and the occurrence of the so-called disturbance. Yet in the column marked "disturbance" at the right, Codde marked N, N, N (N = no). She fully acknowledged the arrival and departure of the oyster boat, noting in the "route #" column the boat traveling down route 1A, 1B, and 2 between 12:33 to 12:41, and then returning up route 2, 1B, and 1A from 12:50 to 13:02. She was clearly focused on the oyster boat, and much closer to it and the seals than the observer at the distant observation point. Codde witnessed the arrival and departure of the oyster boat.

According to the Frost Report, Codde's job was to "monitor and memorialize all visible DBOC mariculture operations, and to record, in writing and with still camera and video equipment, all pinniped disturbances ..." that she observed at OB and UEN. Yet Codde wrote that she observed no disturbance.

The Frost Report went one step further. Gavin Frost interviewed Sarah Codde specifically about the March 14, 2008 so-called disturbance event. Frost wrote:

*Notably, the absence of any mariculture-caused disturbances observed or documented by V1 and the cameras applies to March 14, 2008, the date on which a volunteer observer, standing a significant distance away from the camera locations on the opposite side of Drakes Estero, witnessed a DBOC boat disturb four seals from a group of nineteen in upper Drakes Estero. Without question, V1 was present at the camera locations on that date, at the exact time, and was closer to the disturbance site than the volunteer observer, but s/he neither saw nor documented or filmed any compatible anthropogenic disturbance. The PC85 camera, aimed at the area of alleged pinniped disturbance, confirmed the presence of a DBOC boat at the relevant time, but photos did not confirm any harassment of harbor seals on that date. With regard to the harbor seal disturbance on March 14, 2008, the only date during that pupping season when DBOC activities allegedly harassed pinnipeds, SI relied heavily, but without clear explanation, on the volunteer observer's report and completely dismissed, without timely analysis or review, the direct or indirect contradiction of that data as presented by the negative implications of VI's observations, the photographic images, and the video clips.*

*Indeed, much like the approach to digital photos taken in 2007, S1 accorded little weight to the 2008 photographic data and related information gathered by V1. Deemed "incompatible" with the long term data collected from the traditional visual monitoring site in the southwest corner of Drakes Estero, the 2008 camera project and information associated therewith received little scientific attention from S1 and did not alter SI's subjective belief that DBOC mariculture operations either disturb harbor seals in upper Drakes Estero or dissuade the marine mammals from using established haul-out subsites.*

In summary, there are three reasons why the March 14, 2008 so-called disturbance record cannot be accepted and is suspicious and controversial.

1. The March 14, 2008 observer record is inconsistent with the photographic data in terms of the time of arrival of the oyster boat and which worker got out of the boat and did the initial work.
2. The March 14, 2008 observer record is inconsistent with Sarah Codde's database (appendix E in MMC report) which covered the entire time period, from a closer vantage point, and documented the arrival and departure of the oyster boat, and yet recorded no disturbance.
3. The March 14, 2008 observer record is inconsistent with Sarah Codde's interview with Gavin Frost, and what is reported in the Frost Report: "Without question, V1 [Sarah Codde] was present at the camera locations on

*that date, at the exact time, and was closer to the disturbance site than the volunteer observer, but s/he neither saw nor documented or filmed any compatible anthropogenic disturbance."*

The data from and interview with Sarah Codde refute the March 14, 2008 NPS observed disturbance record. Sarah Codde's database was included in the MMC report (appendix E), but Ragen failed to cite it. Moreover, Ragen failed to cite the Frost Report, even though he was alerted by me in early January 2011 that, according to Gavin Frost, both Dr. Sarah Allen and Sarah Codde told Frost during extensive interviews in December 2010 that the photographic evidence showed no harbor seal disturbances caused by the oyster farm.

What is troubling about that characterization? Why did Dr. Ragen use this characterization to raise questions concerning Dr. Goodman's integrity? Why did Dr. Ragen attack his major critic in this fashion?

NPS Sarah Codde's notes (see Appendix E in the MMC Report) and her testimony under oath to DOI Field Solicitor Gavin Frost raised enough concern to disqualify this disturbance record. Dr. Ragen, on the other hand, believed the volunteer, said the NPS staff told him something different, and is highly critical of Dr. Goodman. This is the basis for Dr. Ragen's assertion that Dr. Goodman mischaracterized the views of NPS scientists. It sounds as if the NPS scientists gave two different answers – one under oath to DOI Field Solicitor Gavin Frost, and the other in private discussions with Dr. Ragen.