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Bob –

Thanks for passing along Richard Nerf's artificial (his word) but interesting model and his comments on statistical analysis.

We have at hand two models of the effects of nearshore built stressors on habitat factors. The models differ in a number of significant ways. Both are based on unverified embedded production and impact functions.

And both illustrate the dispersion that results from strongly differing mixes of stressor types, their concentrations, and their impacts among shoreline reaches. Hence the birdshot appearance of, for instance, Mr. Nerf's figures 1, 2, and Table 3.

I should summarize what I did and didn't do. I'll use the context of the Bainbridge data set and Assessment.

First, I responded to the authors' obvious premise that a stressor index ("Controlling Factor" score), a composite of multiple presumed-causal relations, reasonably predicts a composite score ("Ecological Functions") reflecting habitat welfare. One can certainly imagine that possibility.

However the scatter plots reproduced by Mr. Nerf and me were the basis for a null hypothesis that there is no significant correlation between the composite stressor and habitat scores. I confirmed that with correlation analysis.

At page 4, top, Mr. Nerf wonders about the regression results for low-bank stressors, portrayed in his Figure 1. r^2 for the 31 low-bank observations is .032, not significantly different from zero. The coefficient on the slope variable is also not significantly different from zero. This information was not in the paper seen by Mr. Nerf. This means that a high stressor index for a shoreline reach is not associated with a high level of impact on habitats. Nor a low level. The predicted levels are inconsistent and unpredictable.

That part of the analytical work ended there, with some (I think) reasonable conclusions about the implications for basket-of-stressor-type causality. There may have been high correlations, between pairs of stressors and impacts, embedded in the baskets. That was irrelevant to my inquiry, which related to the received totals. Harm/harmed pairing is discussed later.

Next, because the authors emphasized the role of bulkheads in their text and within their model, I considered that looking at bulkheads alone might highlight their impacts. As I reported, the regression, with the aggregate habitat score as the response variable, revealed a negligible bulkhead role relative to the combination of habitat indicators ("Ecological Functions").

As for the model and its predictions, the former was devised by marine scientists, well-known in their field. The latter were founded on responsibly collected data. The findings relative to dispersion and trend of the aggregate stressor/habitat correlations appear to be supported, not refuted, by Mr. Nerf's model.

In the third part of my analysis I followed up on the authors' assumptions about causal relations underlying components of the stressor and habitat gross indexes, hidden among the clouds of scattered points in the reproduced figures. There were several reasons to suspect that such relations may exist. Mr. Nerf's tinkering with a contrived set of response curves further supports that possibility.

Ignoring model formulations altogether, I regressed the field data on bulkhead intensity against eelgrass welfare and, separately, other individual habitat elements among the authors' data set. No model-based accumulation of stressor indexes here, in fact no model, just bulkhead and habitat data. No addition nor multiplication, just bulkhead intensity relative to individual habitats, the null hypothesis again being no significant correlation. Some of the results were reported in an endnote. I've enclosed some of the graphics. Bulkheads account for almost none of the variance in the habitat factors. Nor, for that matter, do docks.

My summary statement, in the text, that bulkheads clearly play a statistically trivial role in nearshore habitat welfare is correct, given the data set. And my three main conclusions (p. 6) still stand upright.

There is mention in Mr. Nerf's comments about intermediate effects: for instance the effect of bulkheads on eelgrass is seen as indirect,

through reflective forces. Here he ventures into causation territory. In any case, if bulkheads affect, say, sediment drift strongly and the effect telegraphs through to eelgrass, then eelgrass welfare will be correlated with bulkheads. Truly so. If whipped mules regularly kick down barns that destroy water troughs, there's a correlation of trough welfare with whipping.

With two models before us, there may be some questions. Does the Nerf model invalidate the Assessment authors'? No. The models differ in structure, data arrays, and products. Both are conjectural. Neither has been validated. Is one 'better' than the other? We don't know until both are data-driven and their predictive ability can be gauged against field-measured results. The Nerf model points to the possibility of nonlinear responses and joint impacts. The authors' model is informed by data in the box.

Another inter-model comparison that may be relevant. The Assessment authors perceived a very high impact of bulkheads relative to other built nearshore features. That is reflected in several of their "Controlling Factors", which are summed. In the Nerf model a high bulkhead presence and corresponding impact would, I assume, be shown by a response curve mostly concave into the origin in his Figure 3, spilling steeply close to the response (vertical) axis. The presence of any other stressor in that reach serves, via multiplication by some number less than 1, to lessen the absolute impact of bulkheads.

Suppose a bulkhead (stressor) level of, say, .8, yielding a remaining-grass figure of, say, .2. If unblemished grass's level is 100 units, the bulkhead takes it down to 20, an absolute loss of 80. Now add turbidity, with a (stressor) level of, say, .6, per Mr. Nerf's example on pages 4-5. The remaining-grass figure for turbidity is .5. In my example that's an absolute loss of 50 units. Using the Nerf multiplicative approach we deal with .2 x .6 = .12 for total grass remaining, which is a loss of 88 units. But we've already allocated a loss of 80 units to bulkheads and 50 to turbidity. So clearly we cannot say that bulkheads account for a loss of 80 but rather a good deal less, to leave room for turbidity impact. The Assessment model deals with that impossible total of 130 units lost (80 + 50) by scaling them downward. To me these two sentences are the essence of the difference between the models.

I hold no brief for either model; the one I relied on was the model of choice for the scientist-authors and their client jurisdictions.

Concerning my analyses, Mr. Nerf says that regression analysis "...cannot detect the presence or absence of a causal link..." I agree, as does every statistics text from which I've taught. However when correlation is abysmally low, causality is not apt to be high.

Late in his paper he lists, from Wikipedia and WikiDoc, some widely-understood limitations on certain statistical measures. There are others. If Mr. Nerf perceives a particular problem here he hasn't said so.

So I will suggest a (dim) possibility: collinearity. There might be a factor that is highly correlated with, say, bulkhead presence and so, by extension, correlated as well or better than bulkheads with habitat welfare. If it's within our equation we can recognize and deal with it. The issue in my one-predictor equations would be an unknown factor that steals some of the steam from the bulkhead's explanatory power.

A real-world collinearity situation exists with lumber export volumes. Canada exports to the U.S.; the U.S. exports to Japan, as does Canada. There are three fairly obvious elements of collinearity. (The solution is simultaneous equations, on which I can declaim another day.) For bulkheads there may be some nature-based force that rivals and moves up and down in concert with bulkheads. I don't know of one.

Finally, another call for more research. It is significant, I think, that Mr. Nerf had to rely on fabricated response curves, signaling a dearth of real research-developed nearshore relationships. Now if only NOAA, DNR, UW, DOE, DFW, PSP, and/or USCE would pursue numbers as vigorously as notoriety. For instance, replicating the Thurston County beach-profile work here in the central Sound would help show whether there's meat on the bones of Mr. Nerf's hypothesized beach-steepening framework.

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