WA MSP Science Panel Review of Draft Report "Overview of Methods and conceptual framework for Science Panel", and Ecological indicators of the pelagic zone habitat for Washington State's marine spatial planning process.

June 29, 2015

Overview

The two documents (Framework and application to coastal pelagic habitats) were reviewed by three members of the WAMSP Science Panel. We were very impressed with the overall framework and the detail with which it was applied to the coastal pelagic habitats. Below we highlight some areas where we see the potential for improvement. As per convention, we separate them according to major and minor comments.

Overview of Methods and Conceptual Framework

The authors have done a great job outlining the process of indicator development, the rationale for indicator use, and have done some interesting work finding out how people weight different characteristics of indicators. These authors have already been involved in very similar work, so it is no surprise that much of this is borrowed from IEA and DIPSER frameworks. This is not intended as a knock, but rather a strength of the approach. The overview is generally well written and logically supports the structure and content of the conceptual framework. The approach and rationale for selection and weighting of indicators is particularly well crafted.

Major Comments

Although it appears to be an accepted convention, from the standpoint of ecosystem ("habitat" here) classification, the OCNMS classes of "intertidal zone", "kelp forests", "rocky reefs", "open ocean" and "seafloor" are somewhat overlapping and could be difficult to attribute to biotic communities. The "intertidal zone" is a depth/elevation zone, while "rocky reefs" are defined by substrate, "kelp forests" occur on different substrates in both intertidal zones and seafloors, and "seafloor" is ambiguous relative to both depth/elevation and substrate. It should be noted that the chosen "habitat characterizations" are attributes of exposed and shallow coastal shores but don't necessarily have any relationship to the substrate of deeper waters offshore. Maping biota across these "habitat types" would likely be confusing, if not very coarse resolution.

This document might benefit by more clearly articulating how it will be used to guide marine spatial planning. We noted that none of the indicators had a strong explicit spatial component. Presumably the goal of MSP is to better coordinate offshore development so that we protect valuable species and habitats. Often, the goal is not the habitat itself, but the collection of species or other human benefit that we derive from them (e.g. the recreational benefit of beaches). The proposed framework put "habitat" up front, and used metrics like "quality and quantity", without consideration of either spatial arrangement, or whether the quality and quantity is sufficient to meet the goals of MSP. That is, perhaps the authors could go a bit further to articulate specifically how this process helps in the MSP process. We also note that this shortcoming seems to be built into the project from its inception, and the authors point out that focus and

limitation. Looking back on notes, one of us observed that the advisory panel had noted this issue when this project was first described at our meeting in Sept. 2014

Minor Comments

The narrative explaining ecosystem indicators is valuable, especially in utilizing the DPSIR context. It is still a bit unclear where "indictors of ecological components" fit in (as responses?).

The concept and use of "ecosystem health" in the conceptual framework is well discussed defined/described. It is recognized as a plastic word that has somewhat marginal scientific value. It will be important to explain to stakeholders and managers exactly how specific key attributes is reliably indicative of a change in ecosystem condition they care about. For instance, it would be valuable to explain how the 'direction' of change in an indicator of species diversity or trophic diversity relates to a desirable goal.

It would have been nice to have a reference list of the literature cited associated with this section.

Should there be indicators that are more directly linked to some of the biotic attributes, e.g., the actual sea surface temperatures or indices of primary productivity (e.g., Chl *a*) that vary as a function of upwelling and some climate drivers?

There's often an inconsistent presentation of Framework categories between the Overview document, Appendix 2, and the Pelagic report, and even within the Overview document. For example, the 3 major "Considerations" (Primary, Other and Data) are just as often described as "Criteria" instead, and the grouping of physical drivers as "sub-goals" or "key attributes" is inconsistent. While these may be small terminology issues, this project is quite dependent on a formalized set of categories; these inconsistencies detract from a clear understanding of the overall conceptual framework and individual indicators.

The document would benefit significantly from a harsh copy editor's review. Although it is generally well written, there is considerably awkward and run-on sentences and grammar/punctuation glitches.

Fig. 1. This map seems to present the WDFW habitat categories, not the set of 6 habitats chosen for the project and listed below the figure in Table 1. The figure caption doesn't clarify that distinction.

Page 6-7. It's not clear how Key Attributes were chosen and whether they are intended to be highly aggregated or fairly specific characteristics.

Page 9, "Initial Selection of Indicators". Here it is stated that a review of the literature identified 110 indicators for habitat, fisheries and focal taxa, and ecosystem health goals. Given that this number is not presented as being specific to the pelagic habitat type, the assumption is that these are the total number of indicators across all 6 habitat types. However, the list of such indicators for the pelagic habitat alone presented in Appendix 2 is approximately 160 long (some are probably reused across key attributes). This makes it unclear whether the discussion here is actually intended to span all habitat types or is in fact focused on pelagic habitats. It's also not

clear why this section does not discuss the selection of indicators for physical drivers and human activities – a focus that's also present in the section on "Conceptual Framework for Indicator Selection" (pages 4-6), whose rationale I also could not fully understand. The issue of whether the focus is across all habitat types or only the pelagic habitat is also relevant to the section on Scoring Indicators, given that it points to the pelagics-specific Appendix 2.

Table 3. Subdivide the Evaluation Criteria using the 3 major "Considerations" or criteria (Primary, Other and Data).

Ecological indicators of the pelagic zone habitat for Washington State's marine spatial planning process.

Overview

This is an impressively comprehensive document, that covers a wide range of climate, anthropogenic and oceanographic drivers as well as responses to key taxa in this system. It is well organized and thoughtful. If anything, it suffers from bloat – much of the dismissed indicators might be moved to an appendix.

Major Comments

Throughout, more explicit consideration of the spatial structure of the pelagic environment would be beneficial, and the scale of assessment (WA coast, US West Coast, entire upwelling region) does not appear to be consistent.This was particularly noted in several discussions that focus on the entire California Current (US West Coast) rather than WA coast regions, the WA coast as a whole, or the Pacific NW. For example, Data Consideration #4 in the Framework document states that an ideal indicator should be available "across a broad range of the California Current". There is also some degree of redundancy or unnecessary complexity. For instance, many (if not all?) of the Community Structure indicators could be distributed among the earlier functional groups.

We wondered if more attention can be given heterogeneity in pelagic habitats. This work treats the coastal pelagic zone as a fairly homogeneous area. At a minimum, some rationale for this choice is needed. Perhaps at a state level, the onshore – offshore or north / south gradient is relatively minor. My guess is that the southern parts of the state have some influence of the Columbia River and may be quite distinct from the northern parts. C Castal oceanographers have characterized numerous features that, albeit often transitory, are prominent in the coastal waters of Washington, e.g., gyres, estuarine plumes. See Hickey and Banas (2003; Estuaries 26: 1010-101) for instance. There is a section on Currents, Eddies and Plumes, but it does not reflect how this would fit into the gross spatial classification that is the basis for the framework?

Similarly, at least in the Physical Drivers and Habitat indicators, no consistent framework is presented for addressing vertical structure. The term "surface" is often used and applied to indicators without a discussion or definition of its vertical extent (e.g., upper 1 meter, photic

zone, mixed layer). As a relevant comparison and suggestion of a possible option, I would point to the study on ecological vulnerability in Canada's Pacific marine ecosystems by Okey et al (2015), and their consistent and well defined use of three water column zones: Surface (<200m), Midwater (> 200m) and Benthic habitats (the latter probably corresponds to the Seafloor habitat type in the WA report, so it would not be in the scope of the Pelagic report). They acknowledge when appropriate data are not available for a given zone.

In most cases, a rationale for the emphasis on the most recent years of a variety of the highly dynamic and variable ocean condition indices is not clear. Only in a few cases is there a clear evidence of a shift from normal frequencies and scales of change in any index. Much of the variation highlighted over the most recent time period is not recognizable, and sometimes considerably less so, than the preceding decades. An early explanation of this focus in the Status and Trends is warranted.

The authors appear to be making an explicit assumption that climate change will be affecting a change in the frequency, timing and intensity of oceanic regime phases (top of pg 4). I don't think that there is a consensus on this topic among climate scientists, certainly some level of uncertainty. At a minimum, definitive literature citations are needed for this logic train.

The scope of the indicators is very broad and ambitious. From Physical Drivers and Habitat sections, concerns were raised regarding the extent the authors consulted with a broad set of thematic experts, such as physical and chemical oceanographers with direct expertise in WA waters. While such an effort can be daunting, ultimately it is necessary, unless this work is to be considered as only an initial effort at selecting indicators (at least for Physical Drivers). Important datasets that are specific to WA were not considered; this is particularly true for the ocean acidification indicators, where there has been rapid development in the last few years; here, the only specific indicators discussed are from the Newport line in OR, not even in WA. The mooring array CTD dataset from OCNMS is also not considered, as far as I can tell. Assessing what datasets are being used is made more difficult by the fact that many of the indicator citations in Appendix 2 are not found in the Reference List in the Pelagics report.

Minor Comments

Figure P.1- The conceptual diagram would benefit from more thought and /or better annotation. It's not clear to me what this set of boxes and arrows mean.. Some arrows depict species interactions and those are obvious, other arrows are unclear. Basically, I encourage the authors to make the conceptual more thought out than "these are the things that matter" by thinking about "how do they matter and why". Perhaps consider distinguishing arrows that involve different interactions. In addition, some of the arrow pathways are indistinguishable, e.g., although seabirds may feed on zooplankton, it is impossible to determine whether an operational arrow/path connects the two.

Figure P.1 and Pelagic Habitat I: These two figures should be consistent. For instance, I would advocate using the term "forage fishes" as in Figure P 1, instead of "sardines, anchovy, herring & smelt" in Pelagic Habitat I because some species such as Pacific sandlance (*Ammodytes hexapterus*) are not listed. Similarly, I would advocate renaming "Euphausiids, Copepods,

Pteropods" as "Zooplankton" because there are a lot of other taxa involved and connected to other ecological components. "bacteria" in "Phytoplankton & bacteria" might also be changed to "microbes"? The specific taxa of most interest can certainly be elaborated upon in the text.

Page 3 .Is climate change a driver? Isn't "climate" a driver? Climate has two parts, natural variation and anthropogenic changes. The distinction between natural and anthropogenic forcings is briefly discussed in the Framework document when describing the DPSIR approach (Page 3), but the ramification for climate drivers is not clearly discussed. In fact, the paragraph presents "climate conditions" as an example of natural forcings to differentiate natural and anthropogenic forcings. It would be helpful to present a more fleshed out discussion of direct, regional human pressure vs. anthropogenic climate forcings that are globally driven; the latter is relevant for multiple climate and oceanic drivers discussed, including ocean acidification. I note that the authors acknowledge that for climate, "separating anthropogenic from natural processes is difficult" (Pelagic chapter, Page 4).

Pages 4-11, Sea Surface Temperature and El Nino attributes.

- The rationale for distinguishing these two sets of attributes is not well described, particularly when the same MEI & NOI indicators are listed under both. This is made more confusing by the fact that while Table P1 lists 4 indicators for SST (including MEI & NOI), the detailed indicator subsection discussions only address SST and PDO without explaining why.
- Is including both SST data directly and large-scale indices of climate forcings (PDO, MEI, NOI) as parallel indicators of SST something of a mixing of apples and oranges? SST data proper are widely available (from surface "skin" temperature accessible to satellite derived SST; from a subset of buoys, not just NDBC Cape Elizabeth; etc). The indices seem more appropriate as a separate category of climate drivers that are not, and can not, be spatially explicit, not even specifically to WA pelagic waters in the aggregate.
- NANOOS partners have created a MODIS-derived temperature (and chlorophyll) monthly anomaly product that has undergone more refined processing than the datasets used here. See http://www.nanoos.org/documents/announcements/nanoos-climatology-announcement.pdf

Throughout – how is "trend" determined (e.g. page 4,5,6)? Is it from some sort of smoothing function, a time series analysis, regression, eye-ball? Many of these have trends that go up or down from data that look at first glance to be random fluctuations. it would be helpful to also mention, at least briefly, why an interval of 5 years (as opposed to, say, 3 or 10) was chosen to represent current/recent conditions.

Accordingly, the figure legend(s) could be a bit more explanatory, at the minimum explaining the relevance of graphical features such as the green shading in Figure 2, which the reader would have to delve into the text to interpret. And why were the last five years chosen? Similarly for Figure P 4 and the associated text on Pg 7-8, what is the point of highlighting the 2010-2014 time series of an exceedingly variable time PDO time series; why isn't the cited December 2014 index level on the figure?

Throughout Physical Drivers section: Background text on key attributes could be strengthened. Only 1 paragraph is written about the relevance of ENSO events on the ecosystem (and most of this is a description of the phenomenon).

Page15. Mean age is a tricky indicator, because it can go down for two totally different reasons – a strong recruitment event or an increase in mortality in older ages (e.g. fishing or diseases). This may make it a problematic indicator. It might be better to simply track juvenile and adult stages separately, to avoid the problems with ratio or "mean" indicators.

Page 17. Ocean acidification is listed as a key attribute, but much like "climate change", a more appropriate label would be "water pH". Acidification isn't an attribute, but rather the response to anthropogenic CO2 emission.

As discussed by the authors, the effects of OA are not expressed by pH alone; aragonite saturation, though challenging to assess, has been shown to be a more direct indicator of actual stress on calcifying organisms.

OA research and monitoring have greatly expanded in the last several years, but this more recent work, both in terms of available data and research findings, is not reflected in this section. At a minimum, the authors should examine and glean from the WA OA Blue Ribbon report (Feely et al 2012). But there are other highly relevant studies that should inform this indicator selection, such as Boehm et al. (2015), Hauri et al (2013) and Waldbusser & Salisbury (2014), in addition to ones already mentioned. The Background section also seems to be in need of strong editorial review; it repeats itself, repeating explanations on the carbonate system; it introduces values for aragonite saturation without first explaining what the values mean; it drops the sentence "When dissolved oxygen concentrations fall below 1.4 ml L-1, the waters are considered to be 'hypoxic.'" apparently out of context.

Page 19 Some information on how DO is an indicator of pH is needed. As an indicator, DO needs to be explicitly linked in a causal manner not a coincident, confounding factor. Not that DO is not a good indicator, but there are many other processes that factor into DO levels that suggest it would be a much better indicator of (more biotic) other processes. The comment re. Figure P 12 about data from shelf waters (pg. 20) also poses a question of its relevance? There has been work developing commonly measured variables (particularly oxygen AND temperature) as proxies for aragonite saturation, but this work is not cited clearly, particularly the most recent and extensive work (see Alin et al 2012). The use of such proxies has limitations that need to be carefully considered, as discussed by Alin et al.

Page 20 – See point about P.15.

Page 21. The Currents, Eddies and Plumes is a valuable section on more finite, "mesoscale" features that (a) deserves to be incorporated more explicitly into the spatial framework of the assessment and (b) should be focused on the WAMSMP. The dimensions and indicators of other such features, such as the Juan de Fuca Eddy should also be considered as indicators. For the Juan de Fuca eddy, the impact on productivity of interactions with Salish Sea outflow is highlighted in the recent work by Davis et al. (2014).

Page 24 – low DO can also be associated with strong upwelling events. For this reason, DO is a much more logical indicator under this section.

Page 26 – Some spatial characteristics would be useful – percent of area with DO < 2 mg / l, vertical extent of oxygen minimum layer, etc.

Page 27 Pacific sand lance (*Ammodytes hexapterus*) should probably be included in this forage fish list. They do tend to be a bit more shelf-nearshore than the other species, but occur as larvae offshore and are definitely food web components. (Just because scientists can't catch them doesn't mean they aren't out there!)

It might be questioned whether invertebrates are routinely considered forage fish?

Much of this Background presentation seems to be taken from the freshwater or nearshore literature and is not specific to marine pelagic habitats. Discussions of TDML don't seem to belong here (as opposed to Human Activities); the same is true for detailed discussions on watershed characteristics, SAVs, and drinking water. In Page 27-28, two central citations (Isaak et al 2011 and Peterson et al 2013) are not listed in the references, and some of that presentation appears to be from rivers and streams rather than the marine environment (but it's hard to tell).

Page 28 – presumably Spawner – egg relationships are more robust than spawner – juvenile / recruit relationships

P 30- Why wouldn't the incidence of forage fish in salmon or seabird diets be a good indicator of regional population size of the prominent species?

Page 33. Are the authors concerned that salmon dynamics are driven by a suite of processes, on some of which take place in pelagic habitats? It seems like using them to characterize the status of the pelagic habitat could potentially be misleading if there is a trend, positive or negative, that is due to nearshore or riverine processes.

There is a strong gradient in the composition of salmon prey with increasing salmon size, which is also confounded by time. The description here is a rather simplified/generalized version that should be expanded upon to describe this variability (and the utility thereof as an indicator).

Given the plethora of confounding factors affecting salmon population abundance, it is questionable hos the spawning escapement (which, for instance, could be entirely scaled to commercial/recreational catches and watershed effects) could be a dependable indicator of pelagic ocean conditions? It is surprising that incidence of juvenile salmon in diets of marine birds wouldn't be considered as (much more) viable indicators? Fortunately body growth rate of juvenile is considered to be a valuable indicator.

Page 42 The issue stated on this page is of legitimate concern, and as well it should be noted that a significant portion of Columbia River salmon (particularly coho in some ocean conditions) move south to rear along the Oregon and northern California coast, not Washington's.

Page 50 Shouldn't the hake acoustic survey data for distribution of (backscatter) biomass be associated just with the WAMSMP?

Pages 50-51 Much of the background on seabirds is based on coastal breeding colonies along the Washington coast. Although this might be an indicator of general use of the pelagic zone, isn't there data on offshore occurrences and abundances that would be more direct indicators?

Page 55 It is a misstatement to say that "Phytoplankton is the base of the food web for the entire marine community" although that is the case for the pelagic ecosystem; suggest revising first sentence to clarify.

Page 58 Similarly as stated for phytoplankton, suggest that the first sentence on this page be revised to "zooplankton are the foundation of the ocean's pelagic food web".

Page 84 – Seafood demand seems a difficult thing to measure at the right scale. Much of the seafood captured in the region is exported, and that level of demand is due to a range of processes totally separate from the WA coast. I suggest dropping this attribute, because I can't imagine how it would help decision makers think about WA coastal planning.

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