Correcting the record on forage fish and their predators

A response from the Lenfest Forage Fish Task Force to recent publications

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Five years ago, we authored the report *Little Fish, Big Impact*, which included a suite of recommendations for the management of forage fish. The goal of our group, the Lenfest Forage Fish Task Force, was to bring attention to these important species and to provide recommendations for their sustainable management.

We are pleased to see that others continue to study forage fish and to reach conclusions similar to ours, including a recent publication by Ray Hilborn and others in *Fisheries Research* (Hilborn et al. 2017). That paper recommended, as we did, that fisheries management be tailored to individual species and ecosystems where possible. Our Task Force also went a step further, providing general advice for situations when information is insufficient for a tailored approach. We urged that this advice be used as an interim measure, in light of strong evidence that existing management approaches are not designed to take into account the variability in forage fish stocks, their unusual life-history characteristics, and the role they play in the ecosystem.

It is this general advice that the Hilborn et al. study took issue with. We are disappointed to see that it did so by mischaracterizing our work in several places and presenting technically questionable analyses. Moreover, there were a number of misleading and verifiably false statements in a video and press release associated with the study.

We feel compelled to correct the record promptly with the statement below, which addresses some of the worst falsehoods and examples of mischaracterization. We plan to address the many additional shortcomings of the paper in a separate publication.

Mischaracterizations in the press release and video

One of the obviously false statements in the press release is that our report "recommended slashing forage fish catch rates by 50 to 80 percent." In fact, we recommended reducing fishing *mortality*, not catch rates, an important distinction. And we recommended reductions of 25 to 50 percent relative to F_{MSY}, not 50 to 80 percent. This recommendation was part of a more detailed set of recommendations, which the materials did not acknowledge.

Another false statement in the video was that we concluded "that predators rise and fall with their prey populations as a generality." Our report contains many nuanced statements about the evidence for a strong link between forage and predator abundance, but none as simplistic as this quote.

The press release and video also seriously mischaracterize our work. For example, one of the study authors stated the following in the video:

> The Lenfest conclusion that forage fish fisheries can only be managed successfully to sustain predators as well as the forage fishery by very precautionary policies and reduced harvesting is not based on any fact. It's based on model predictions, on running alternative scenarios on models that we now understand have been fundamentally flawed.

The implication here is that if a conclusion is based on modeling, it has no sound basis. This is incorrect because this type of modeling uses empirical data and biological knowledge to construct a coherent picture of a species or system. It is generally accepted that all models have weaknesses, but when used with caution and good information, can provide a useful representation of a system. We are aware of the published critiques of the Ecopath with Ecosim (EwE) model, but we do not share the belief that it was not suitable for our purpose. We chose this modeling framework because it helped us to survey a large number of ecosystems and arrive at a reasonable approach for places where there is not yet enough data to tailor management of the system.

Moreover, our recommendations were not based exclusively on the output of a single type of model. We also relied on detailed case studies, analysis of the real-world impacts of various harvest strategies, and a comprehensive review of the literature on forage fish and their predators. One of the other modeling studies we considered (Smith et al. 2011), employed several different kinds of ecosystem models, including EwE, and came to similar conclusions about the need to greatly lower fishing mortality rates relative to F_{MSY}, a common fisheries benchmark, to sustain forage fish fisheries. Finally, we relied on empirical data suggesting global patterns that can be used as interim rules for management, such as the finding that successful seabird breeding requires forage fish abundance to be at least one-third of its long-term maximum (Cury et al. 2011). We based our recommendations on these multiple, independent lines of evidence, which the Hilborn et al. study does not acknowledge.

It should be noted that single-species models and simple predator-prey models also have limitations and uncertainties. They can yield unreliable predictions because they do not account for the full suite of complex ecosystem interactions. The Hilborn et al. study is therefore a contribution to an active area of research, not conclusive evidence that "reductions in fishing mortality rate would benefit predators less than argued by Pikitch et al. (2012)".

Our last example is the following statement by one of the authors in the video:

> What we found is there was essentially no relationship between how many forage fish there are in the ocean and how well predators do in terms of whether the populations increase or decrease.

This is a misleading overstatement of the Hilborn et al. study's findings because it asserts that the authors have proven a negative. This study only adds one piece of evidence to a large body of literature, much of which contradicts their paper. For example, empirical studies have already demonstrated a strong connection between forage fish and the abundance of predators in southern Africa, Norway,

Peru, and Antarctica (the species in the last example is krill, which is not a fish but fits our definition of a forage species). Others trace the link between forage abundance and breeding success, both through field data (e.g. Boersma & Rebstock 2009) and global analysis of existing data (Cury et al. 2011).

When studies come into conflict, the scientific community does not blindly accept the most recent. Instead, it investigates the reasons for the discrepancy. We were surprised at the sweeping assertion in the statement above, given that there is a large body of evidence that supports the finding that dependent predators are strongly impacted by the abundance of their forage fish prey.

Shortcomings of the Hilborn et al. study

We now turn to five of the numerous shortcomings of the Hilborn et al. study itself. First, the study makes broad claims about the adequacy of our advice for **forage fish** management, yet six of the 11 species in its empirical analysis (Pacific hake, chub mackerel, Atlantic mackerel, and three squids) do not meet the definition of "forage fish" that we used in our 2012 report. While our definition is now in common use, we do not claim that it is the only correct one, only that one needs to pay attention to such things when making comparisons.

Second, the study used estimated population levels generated by stock assessment models to identify correlations between predators and prey. Yet, the models that generated these estimates were not designed for this purpose. In effect, the rigor and attention to detail demonstrated in their examination of ecosystem models was not applied to these models.

Third, the Hilborn et al. study only used examples from U.S. fisheries, which they note are among the best managed in the world. It therefore ignores major systems where forage fish have been demonstrated to play an important role, including large upwelling systems such as the Humboldt and Benguela systems, and Antarctic ecosystems.

Fourth, the study looked across food webs with very different levels of predator dependency on individual prey items and concluded that there was no strong dependency overall. This ignores the results of our report and other studies, such as Smith et al. (2011), showing that dependencies are strong in some food webs and weaker in others. To conclude that fishery management need not change because dependencies are not always strong is faulty reasoning.

Fifth, Hilborn et al. mischaracterized a paper by two of us (Essington & Plagányi, 2013). That paper addressed "recycled" models, which are designed for one purpose but used for another. Hilborn et al. incorrectly presented this paper as a blanket rejection of "recycling," when in fact it is a guide on how to do it thoughtfully. The Task Force did in fact proceed thoughtfully in its use of models, for example by considering the potential distorting effects of aggregating species groups.

A path forward

One point of agreement between us and the Hilborn et al. authors is that more information about individual systems can enable better management. In the five years since our Task Force, the Lenfest Ocean Program has sought to provide this information through numerous grants focused on forage fish,

and by sponsoring a second Task Force on Ecosystem Based Fisheries Management (Essington et al. 2016). Past experience (e.g. Worm et al. 2009) has shown that scientists can make progress toward resolving disagreements through a process of open-minded collaboration.

Given the important role of forage fish from ecological, economic, and social perspectives, we welcome advances in improving management of these species, and our Lenfest team continues to support our default recommendations in situations where detailed information is lacking.

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