COMMENTS ON WHERE NEXT ON FISHERY "REFERENCE POINTS"/MANAGEMENT

What follows are a series of emails which developed from discussion around certain technical issues arising in a discussion during a meeting of the ICCAT scientific group involved in the development of a Management Procedure for North Atlantic bluefin tuna.

These broadened into the suggestion for, ultimately, an FAO workshop for a discussion on fisheries management at a broad and overarching level, as elaborated below.

From: Michael Sissenwine <m.sissenwine@gmail.com>
Sent: Thursday, 17 November 2022 11:50
To: Barange, Manuel (FIAX) <Manuel.Barange@fao.org>
Cc: Doug Butterworth <doug.butterworth@uct.ac.za>; Mark Dickey-collas <Mark.dickey-collas@ices.dk>; Ana Parma <anaparma@gmail.com>
Subject: Inertia in Fishery Science and Management

Dear Manuel,

I hope you are doing well. It has been a long time since we worked together as ICES chairs of ACOM and SCICOM, and also a long time since I've been engaged with FAO. At one time I chaired the Advisory Committee on Fisheries Research, and I was involved in negotiation of the Code of Conduct, Fish Stocks Agreement, and related action plans and guidelines.

 A string of messages shared with you by Doug Butterworth (below) motivated me to renew contact with you. As Doug's message described it, he was responding to my initiation of a discussion of fisheries on a "broad and overarching level." The messages included some big thinkers in the world of fisheries (several scientists that have worked with FAO, including Doug, Mark Dickie Collas, Ana Parma). I agree with Doug's suggestion that FAO might play a role in future discussions of this nature.

To be more specific, the overarching issue that concerns me most is the implicit or default assumption that fisheries exist within stationary (in a time series sense) ecosystems, which is increasingly indefensible with climate change. My most important stationarity concern is about the MSY concept that underlies fishery management, including status determinations. The MSY concept as applied to individual stocks assumes that production functions, which are the basis of fishery management reference points (e.g., FMSY, BMSY), don't change over decades unless there is statistically significant evidence otherwise.

An additional problem caused by non-stationarity is the implicit assumption that the functional relationship between stock size and relative abundance indices (either fishery dependent or fishery independent) is stationary over decades (almost 60 years in the case of bottom trawl survey of the Northeastern US). There are many reasons why this assumption is violated, including inaccurate calibration factors between survey vessels (even precise estimates cause a bias in stock assessments when point estimates are applied as a constant), changes in the timing of seasonal migrations and spatial distributions of fish stocks (likely with

climate change), and changes in the areas that can be surveyed (e.g., because wind energy development, operational restrictions on a larger vessel, an increases in fixed gear).

I think it is apparent that these default assumptions of stationarity are not valid for many, if not most fish stocks, especially with climate change. However, the power of statistical tests of the null hypothesis of stationarity are likely to be low power because of noisy data. I consider the "double whammy" of non-stationarity of production functions and relative abundance time series reasons to re-think stock assessment methods and the interpretation of the MSY concept so that the need for long (multiple decades) stationary time series is reduced.

These non-stationary problems may be the reason the 2014 US National Research Council report on US Fishery Rebuilding Plans (Co-chaired by Ana Parma, report attached) found that more than a third of the US Stock Rebuilding Plans it reviewed (all plans that had sufficient analytical detail for objective analysis at the time) were unnecessary because more recent assessments determine the stocks were not overfished when the Plans were initiated. Of the remaining stocks for which subsequent assessments confirmed rebuilding plans were appropriate, less than one third of the plans were successful (i.e., two thirds of the stocks had not been rebuilt in up to 13 years). In total, less than 20% of the rebuilding plans reviewed by the NRC Committee achieved their rebuilding objective. Reassessment of stocks and/or changes in the estimate of BMSY were by far the most common reason stocks were determined to be rebuilt (not because of management measures implemented according to the plans).

Of course, the uncertainty cuts both ways, which means that assessments failed to identify some stocks that needed to be rebuilt. The bottom line is that biomass reference points are so poorly estimated (e.g., non-stationarity), that, arguably, it is engineering malpractice to use them as fishery management performance measures. Yet biomass reference points are increasingly driving fishery management. Of course there are also problems with fishing mortality reference points, but my sense is they are more robust. I won't elaborate now.

I do not advocate abandoning current approaches without developing and critically evaluating alternative approaches to be confident they are an improvement. However, the clock is ticking (a 2014 report from an authoritative body isn't "breaking new!") and the credibility of current stock assessment methods and management approaches is in jeopardy.

Alternative approaches might involve new technologies for resource surveys, model based analysis of relative abundance including covariants, application of dynamic reference points (which are applied in some situation), more Management Strategy Evaluation, refinement of the Transitional SPR approach applied in the Southeastern region of the US in the 1990s (which was abandoned prematurely in my opinion), changes in policy about determination of the status of fisheries or Ecosystem Based Fishery Management. These are only ideas as a starting point. Others probably have ideas that may be better. What's necessary is to acknowledge the problem of non-stationarity and to give some of the best and brightest people time to work on solutions.

Over the last few years, I've been involved in numerous discussions about the problem of non-stationarity. No one seems to disagree with my concerns, but no one seems to have time to address the problem. Another response by many scientists is to agree with my concerns, but the scientists believe advice based on a framework built on a foundation of stationary time series is required (e.g., according to legal interpretation of legislation). They fail to recognize that it is scientists that invented the current fishery management framework they are laboring to serve, not managers, politicians or lawyers. Accordingly, I think it is up to scientists to offer the leadership necessary to overcome inertia in the current system. While I do not think the non-stationarity problem is new or solely a result of climate change, broad recognition of the importance of climate change may provide the impetus to address non-stationarity now.

I'd be happy to communicate further on this issue. I am mostly retired now, but I remain interested in contributing to efforts to overcome inertia and advance fishery science and management.

All the best, Mike

---------- Forwarded message ---------
From: **Doug Butterworth** <doug.butterworth@uct.ac.za>
Date: Sun, Sep 11, 2022 at 3:20 AM
Subject: RE: Communications group discussion of LD levels - widening now on how to "reform/re-consolidate" worldwide fisheries assessment and management practice
To: Michael Sissenwine <m.sissenwine@gmail.com>
Cc: Fernández <carmen.fernandez@ieo.es>, Ana Parma <anaparma@gmail.com>, T Carruthers OBS <tom@bluematterscience.com>, André Punt <aepunt@uw.edu>, John Walter (John.F.Walter@noaa.gov) (John.F.Walter@noaa.gov) <John.F.Walter@noaa.gov>, Enrique Marin (enrique.rmarin@ieo.csic.es) <enrique.rmarin@ieo.csic.es>, Greg Donovan - private (corkblue1o@gmail.com) <corkblue1o@gmail.com>, Jose De Oliveira <jose.deoliveira@cefas.co.uk>, Hillary Richard <Rich.Hillary@csiro.au>, DeBruyn, Paul (FAOSC) <Paul.DeBruyn@fao.org>, Kitakado Toshihide <kitakado@kaiyodai.ac.jp>, James Ianelli <jim.ianelli@gmail.com>, Sean Cox <sean_cox@sfu.ca>, hawaiimars@hotmail.com <hawaiimars@hotmail.com>, Carryn de Moor <carryn.demoor@uct.ac.za>, kevern.cochrane@gmail.com <kevern.cochrane@gmail.com>, Preece Ann Ms <ann.preece@csiro.au>, Andrea Ross-Gillespie <andrea.ross-gillespie@uct.ac.za>, rebecca.rademeyer@gmail.com <rebecca.rademeyer@gmail.com>, Bergh Mike Dr <mike@olrac.com>, Malcolm Haddon - CSIRO (malcolm@haddon.net.au) <malcolm@haddon.net.au>, Wilberg Michael (wilberg@umces.edu) <wilberg@umces.edu>, Regular Paul (paul.regular@dfo-mpo.gc.ca) <paul.regular@dfo-mpo.gc.ca>, Davies Campbell <campbell.davies@csiro.au>, Melvin Gary (gary.d.melvin@gmail.com) <gary.d.melvin@gmail.com>, Jenna Munden <herringsciencecoordinator@gmail.com>, Miller Shana (smiller@oceanfdn.org) <smiller@oceanfdn.org>, Ray Hilborn <hilbornr@gmail.com>, (johann@sadstia.co.za) <johann@sadstia.co.za>, Michael Copeland <copeland.fishconsult@gmail.com>, Kim Prochazka <KProchazka@dffe.gov.za>, Janet Coetzee <janetccoetzee@gmail.com>, Marius Deon Durholtz <MDurholtz@dffe.gov.za>, Nakatsuka Shuya <snakatsuka@affrc.go.jp>, Tomoyuki Itoh <itou@fra.affrc.go.jp>, Nathan

Taylor <nathan.taylor@iccat.int>, Karen Dwyer (Karen.Dwyer@dfo-mpo.gc.ca) <karen.dwyer@dfo-mpo.gc.ca>, Dickey-Collas Mark (29 May) <mark.dickey-collas@ices.dk>, (Manuel.Barange@fao.org) <manuel.barange@fao.org>

Thanks Mike – good to initiate discussion on fisheries management at this broader and overarching level.

The difficulty is that current "practice" is so widely spread and somewhat diverse around the world. Possibly what is needed is a high level international workshop, similar to the one in San Diego that I and some others put together with Pew (especially Shana Miller) some five years back to try to pull diverging MSE/MP practice back to a more common and effective track. This was reported in a paper in CJFAS with Shana as lead author, though I've been somewhat disappointed by its impact. We fishery scientists tend to be a strong willed (pig-headed?) lot, and Sinatra-like want to do things "our (current) way" in many organisations, instead of being prepared to learn more from experience elsewhere and adapt. Though we are, of course, immensely time-constrained by the dictates of current management system requirements.

Perhaps we do need some ideas on this published first before moving further forward along such a workshop line – maybe I'll find some time for this nearer the end of this year to contribute my bit there.

Cheers

Doug

Cc: Manuel – a possible FAO initiative?

From: Michael Sissenwine <m.sissenwine@gmail.com>
Sent: Sunday, 11 September 2022 04:03
To: Doug Butterworth <doug.butterworth@uct.ac.za>
Cc: Fernández <carmen.fernandez@ieo.es>; Ana Parma <anaparma@gmail.com>; T Carruthers OBS <tom@bluematterscience.com>; André Punt <aepunt@uw.edu>; John Walter (John.F.Walter@noaa.gov) (John.F.Walter@noaa.gov) <John.F.Walter@noaa.gov>; Enrique Marin (enrique.rmarin@ieo.csic.es) <enrique.rmarin@ieo.csic.es>; Greg Donovan - private (corkblue1o@gmail.com) <corkblue1o@gmail.com>; Jose De Oliveira <jose.deoliveira@cefas.co.uk>; Hillary Richard <Rich.Hillary@csiro.au>; DeBruyn, Paul (FAOSC) <Paul.DeBruyn@fao.org>; Kitakado Toshihide <kitakado@kaiyodai.ac.jp>; James Ianelli <jim.ianelli@gmail.com>; Sean Cox <sean_cox@sfu.ca>; hawaiimars@hotmail.com; Carryn de Moor <carryn.demoor@uct.ac.za>; kevern.cochrane@gmail.com; Preece Ann Ms <ann.preece@csiro.au>; Janet Coetzee <janetccoetzee@gmail.com>; Andrea Ross-Gillespie <andrea.ross-gillespie@uct.ac.za>; rebecca.rademeyer@gmail.com; Bergh Mike Dr <mike@olrac.com>; Malcolm Haddon - CSIRO (malcolm@haddon.net.au) <malcolm@haddon.net.au>; Wilberg Michael (wilberg@umces.edu) <wilberg@umces.edu>;

Regular Paul (paul.regular@dfo-mpo.gc.ca) <paul.regular@dfo-mpo.gc.ca>
Subject: Re: Communications group discussion of LD levels - general difficulties for MSE in practice in selecting an MP

Thanks Doug. I generally agree. Yes, publication on this topic would be valuable.

In general, I think it is up to those of us that have been at "this" for many decades to lead change because we are responsible for the prevailing current stock assessment and management framework. It wasn't invented by managers or lawyers, and it wasn't specified in legislation until scientists promoted it.   For example, scientist invented single species MSY based management frameworks with the default assumption of stationarity, the Kobe matrix and the PKG performance measure, the precautionary approach, and the illusion that stock size can be predicted to achieve targets according to a schedule, instead of iterative approach to move stocks in the right direct (essential what "empirical MPs do).   This ambitious framework was useful for raising money for more data and scientists (as NMFS Senior Scientists I initiated the Stock Assessment Improvement Plan that generated at least $100 million of budget increases, but money is not a substitute for a design that harmonizes scientific understanding, policy goals, and the realities of nature.

Keep up the good work, Mike

On Sep 10, 2022, at 2:39 AM, Doug Butterworth <doug.butterworth@uct.ac.za> wrote:

Thanks Mike. Quickly (or perhaps not quite so quickly) some general points, because this is not a simple (or simply solved issue) – plus it is of wide relevance in fisheries generally, hence I've sent this out to a wider audience.

1. At some stage in the MP selection process, there has to be specification of a "resource risk threshold" below which one does not want to drop.

2. This has to involve a probability level choice, because it must somehow relate to the lower tail of some probability distribution. Hence choices are needed on:

    a.   How to calculate that probability

    b.   Distribution of what

    c.   What probability level

3. To my mind, the key problem here is that most persons in the ICCAT discussions (and IOTC, if I understand correctly what is happening there) are used to best assessment paradigm choices here, and are falling back onto those, when in many cases they aren't appropriate and/or directly translatable in terms of their values when it comes to MSE.

4. Probability – Fairly standard for best assessment, e.g. bootstrap the estimation, though one might question about whether to condition on a fixed value of e.g. M. However this is problematic for MSE, as one has not a single OM, but usually a

weighted set of OMs in a "Reference Set/Grid". The choices of these OMs can change over time, and with that these probabilities too. Furthermore distributions will change (get wider) compared to those for the single model in best assessment. So this is hardly a robust process for MSE. Hence CCSBT has tended to move away from probability-based targets as much as possible: for example, the OMs in grids should always be reasonably optimistic/pessimistic balanced, so distribution medians at least should be more robustly estimated (less change-able over time) in these circumstances. Hence, if one wants to be "more sure" of reaching a particular recovery target say, rather than (e.g.) 70% (instead of 50%) probable by 2035, instead require 50% by 2030.

This however still doesn't get away of needing some lower tail value somewhere to address the biological uncertainties issue associated with "resource risk".

5. What measure? PGK may work for the best assessment paradigm and constant catch projections, calculated for typically 1-3 years (much though I don't like F for various reasons, including definition difficulties, but let's not get into that here). But it makes no sense for MSE. The F in PGK is there as a surrogate for stock trend (no good to have the stock at Bmsy if the level of fishing mortality implicitly suggested would drive it downwards from there), but one doesn't need that in MSE as one projects stock trajectories anyway, and it is the acceptability of those simulated trajectories on which MP choice should be based, not any F value. The general MSE norm is to use the (spawning) biomass at the end of the projection period, usually relative to some dynamic B0, or better the lowest value (LD) on the trajectory throughout the projection period (as the objective is not to drop to the range where recruitment success may be impaired – the LRP, though we have very little reliable information on what that is – your complaint about the arbitrary nature of the 0.4*Bmsy for ABFT). Thus I think the focus needs to be on these two measures of "depletion", not PGK – LD is "better" (though can be confounded if some OMs estimate the resource to be already below the LRP – why we are looking at years 11-30 only in the projections to get LD* for ABFT) but final depletion at the end of the projection period is more easily grasped by many stakeholders.

6. What probability level – arbitrary as you say, but choices have been made for the best assessment paradigm (5%, 10%, 15% …) which are equally "arbitrary". My main concern here is that those values are not "comparable" with the values generated in MSE (for the reasons I give above). Decision makers (in ICCAT and elsewhere) are tending to treat them as if they are, and this leads to more conservative (rather than the intended equivalent) choices to those being made under the best assessment paradigm.

[Much of the above what I've been intending for years to put into a paper for Marine Policy, if I ever get the time!!]

Cheers

Doug

From: Michael Sissenwine <m.sissenwine@gmail.com>
Sent: Friday, 09 September 2022 17:13
To: Fernández <carmen.fernandez@ieo.es>; Ana Parma <anaparma@gmail.com>; Doug Butterworth <doug.butterworth@uct.ac.za>
Subject: Fwd: Communications group discussion of LD levels

I totally agree with your concern about the technical nature of the LD calculations. See my message below. Ana and Doug may also be interested. Mike

---------- Forwarded message ---------
From: **Michael Sissenwine** <m.sissenwine@gmail.com>
Date: Fri, Sep 9, 2022 at 9:59 AM
Subject: Communications group discussion of LD levels

I found the discussion troubling. It seems that "we" (the ICCAT MSE process including scientists, managers and stakeholders) are attempting to optimize CMPs within the constraint of less than a 15% probability of B<0.4BMSY, or maybe 10% or even 5%. Is this based on science or emotion or the loudest voice in the room?   40% is arbitrary.   The probabilities are arbitrary. The calculation of the probabilities are dependent on arbitrary decisions in the OMs (weightings, timing of productivity shifts), such that 5% differences in the tails of distributions may not be meaningful.   Furthermore, a constraint of less than 5-15% probability of B<0.4BMSY seems more precautionary than usual (e.g., rebuilding US fisheries is triggered by a 50% probability of being less than 0.5BMSY; I'd guess the corresponding probability of B<0.4BMSY is higher than 15%).

I think LD levels are valuable performance statistics, but I would use them to rank CMPs (e.g., a higher LD is better in terms of precaution or safety), or to guard against extremes (even a small probability of B lower than the historic range might be a reason to reject a CMP).   As I recall, this is the approach the technical group was pursuing before managers and stakeholders insisted on a Blim constraint. I have shared my perspective on Blim previously.

I understand that we are where we are, but have I missed something or is my description of the situation reasonable?

No rush to respond, but thanks, Mike

Michael Sissenwine