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DISCUSSION

Policy Uses and Limitations of the Ecopath Suite and Approach[a]

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This report documents a free-ranging discussion on issues raised during the workshop, structured by Carl Walters, who acted as facilitator. Specifically, this involved grouping the predictions of Ecosim/ Ecospace into three classes: 1) predictions which can trusted; 2) predictions, that require local testing/verification; and 3) predictions which, for various reasons, cannot be trusted.

Discussants are identified by their initials, and their interventions were regrouped and edited, thus making our discussion look more formal than it was.

1. Predictions that can be trusted.

CW: concerning predictions, there are two that are fairly robust:

- primary production limits possible total production, through it will not determine whether a system is stable or not. Here, Ecopath and its dynamic routines, Ecosim and Ecospace can help;
- equilibrium community composition responds to fishing: as you alter fishing mortality, a number of changes in abundance are predicted. Notably, the system will tend to become top heavy when fishing is very low. Conversely, at very high fishing mortalities, long-lived species will be lost, while the changes at intermediate level of fishing mortality are largely indeterminate, i.e., we cannot assess their reliability. In fact some of the results for intermediate levels of fishing are goofy, with several stable states of which we don't know if they are realistic or not. But the extreme predictions are reliable, including e.g., prediction of cascades.

DP: If so, one should be able to say that first-order predictions are largely reliable: groups that are exploited decline, and their major prey increase. OK?

CW: Yes, furthermore, we found, in some cases, that the fishing mortality which maximizes yield is much smaller than M, in some others it is larger. Gulland's rule, that F_{max} = M is #@%!!.

TP: How do you explain these differences? Perhaps we should stick to the most conservative relationship between F_{max} and M.

KC: We certainly need policies that are robust to uncertainty of this sort.

DP: Not only do we need to be cautious about the value of F_{max}, but also about the absolute value of the predicted yield. The point here is that in the 'development decades' of the 1960s and 1970s, what was done in several countries, notably in Southeast Asia, was to divide the catch of a typical trawler into the yield potential predicted by Gulland's equation, then deploy the number of boats that came out of the division**.**

TP: Does Carl's point about goofy results being obtained at intermediate levels of F imply that that our ecosystem work is worthless, or no better than single species assessments?

KC: We should do both type of work, as there is enough overlap for both to benefit from the other's results. We should also benefit from attempts to explain the causes of observed differences.

TP: If there are differences, we cannot say both sets of results are valid. Moreover, industry will also go against the more conservative results which tend to come out when you consider feeding interactions.

DP: In fact, you get a reduction of predicted yields in multispecies situations even when you don't consider feeding interactions; all you need is fixed ratios of F between the different species, as often occurs with trawlers. John Pope showed that very nicely in a study he did of the Gulf of Thailand trawl fishery. Incidentally, this is the very reason why we lose large, long-lived bycatch species.

CW: Trophic models cannot really provide practical answers to multispecies management problems.

TP: Then how about Multispecies VPA, which is used in Europe?

DP: They have made real progress with that in the North Sea, even if they don't use it directly to determine TAC for the various species. What they do is use MSVPA to refine the inputs to the single species assessments, e.g. the M values

[a] Present during the discussion were E. Buchary, A. Bundy, V.Christensen, K. Cochrane, F. Gayanilo, A. Jarre-Teichmann, J. Kitchell, J. Moreau, T. Okey, R. Olson, D. Pauly (Editor), T. Pitcher, A. Pongase, R. Sumaila, C. Walters (Facilitator), and R. Watson (Rapporteur).

used for young fishes, which are now set much higher than before.

KC: But there are still uncertainties; for example, the relationship between sardine and anchovy in upwelling system is competition, but this cannot be captured by trophic interactions. This creates huge uncertainties.

TP: Yes, but adaptive management, including 'pushing the system' is hard to sell as a way to find out more. Multispecies/ ecosystem models should be inherently more predictive than single-species methods. In addition to predictions of the type provided by single-species models, they make predictions which single-species models simply cannot make. We have to get people used to these kinds of predictions.

CW: I agree that we can make broad predictions. We can predict that prey fishes will be affected if we remove their prey. This is not what some people want, though. What they want are detailed predictions of precisely what the biomass of each group will be next year. And this we cannot do. Let's move on to another category of predictions, concerning the efficacy and time requirements for transition policies. This involves, among other things, estimating what it takes for experimental programs, etc. to work. Some policies will never show effects, e. g. the setting up of very small MPAs.[d]

DP: In Alaska, the Exxon Valdez Oil Spill Restoration Council must certify whether the various elements of the Prince William Sound ecosystem have bounced back or not. We are now working on that using a consensus Ecosystem model as our starting point (see Okey, this vol.). In the oiled area, the slow groups (marine mammals, birds) have not returned to pre-spill levels, and I expect we will be able to find this to be the aftermath of a shock to the food web, i.e., there might be no need to assume continued effects of oil residues.

KC: I have a related question: what do you monitor in multispecies management, and how is the monitoring done?

CW: We should monitor F directly, by tagging everything.

KC: All species?

CW: All species that need direct monitoring. Estimating F is important because multispecies problems are due to the effects of a combination of gear, and attempts at effort reduction must not be limited to one gear. Multispecies problem occurs when one gear is cut back and other gears move in, like the sport fishery, and cause new problems.

JK: We should be testing for the effects of keystone species, and for effects caused by feeding triangles such as the Norway pout-euphausiids-copepod case that Villy Christensen presented (in Pauly et al. 1998). Alida Bundy's analysis of San Miguel Bay dynamics, where species interactions appear to be very strong is another case (Bundy, 1997).

CW: We will just have to accept that some type of changes is just not predictable. Also, we must be aware that Ecoranger can deal only with some types of uncertainties; it cannot deal with the uncertainty associated with the equilibrium predictions of Ecosim, which are due to the structure built into the systems' description. Anyway, let's now turn to our next topic.

2. Predictions that need local testing.

CW: As mentioned before, there are some complex webs for which the outcomes of Ecosim are indeterminate, i.e., the directions of response to gear changes can go either way. A case in point is Alida's model of the heavily exploited San Miguel Bay, where clear predictions could not be achieved for most groups, given policy changes. This is particularly pronounced for the intermediate trophic levels, while the top and lower levels behave as one would expect.

DP: This might be due to overaggregated pools at intermediate levels, with diet compositions that are too broad.

CW: It is true that, with overgeneralized piscivores, problems will occur, but Alida's mid-trophic level pools were not that generalized.

DP: Our partner in the Prince William Sound project, Stewart Pimm, says that overaggregated lower trophic levels are the main cause of instability and self-simplification in food webs, and that this is the first thing one should look for when problems occur; also feeding cycles between pools at the same trophic level should be avoided. Stuart said he is working on a diagnostic system, later to be incorporated into Ecopath, which will identify these problems (see Pauly, this vol.).

CW: This has been known a while; but this will not resolve the problem of indeterminacy in the intermediate trophic levels.

KC: In upwelling systems, notably in the Benguela system, sardine vs. anchovy competition is not well understood and is not predictable.

[d] Editors note: I can't resist pointing out here that some people believe (indeed: have shown) that very small MPAs can increase the biomass of relatively large, potentially very mobile fish, such as parrotfishes and snappers. See C.M. Roberts and J.P. Hawkins. 1997. How small can a marine reserve be and still be effective? Coral Reefs 16:150.

Note that this is a general problem of lack of knowledge, not a problem with Ecopath.
CW: People working on coral reefs think partitioning is the key problem to study, because it is what maintains reef diversity. Space, not food is the problem they focus on.
DP: I don't agree that it is their consensus. In fact, many of them, especially in Australia, believe recruitment limitation and variability to be the major structuring elements for coral reefs. In any case, don't we agree that space being important, we should construct food webs with subwebs referring to different parts of one's system, and especially separate the phyto- and zooplankton groups?
CW: This is unnecessary, and probably wrong; there are better ways to achieve stability.
TO: I believe it is wise to follow Pimm's advice; we are including over 40 groups in the Prince William Sound model we are constructing to analyze the impact of the Exxon Valdez Oil Spill (Okey, this vol.), and use these to define well-separated inshore and offshore webs, merging only at the top.
VC: Another way to generate stability is by using low movement rates in Ecospace; this leads to clear spatial separation.
CW: Space-structured models can allow co-existence of about anything at some scales, and prevent self-simplification of food webs.
DP: I still think Pimm's suggestions are useful, especially since they generate food webs whose subwebs correspond to the very spatial structure that you say is required.
CW: Even if you follow Pimm's suggestions, some webs will still self-simplify; there are many cases where we don't know what keeps the system stable. Clearly, spatial separation plays a role at some scales, but this is not the whole story. There are key processes we still do not understand. Even Villy's detailed Ecopath model of the North Sea is not stable when run on Ecosim.
KC: We have to avoid the reductionist trap, and not get into an endless process of digging deeper and deeper and never getting to the key process; clearly each model we use must fit specific circumstances.
DP: If we match our spatial separation with our food web, we should be OK. The point is to avoid the bias we have as fisheries people, to lump zooplankton and phytoplankton into great big boxes, because we don't know about them – though the planktologists do.
CW: Sub-models may be the answer, with linkages at the top, e.g. through the marine mammals feeding in the different sub-models.
DP: This is what has been done, in effect, in some of the coral reef models that have been published so far. There, subsystems were defined (sea grass, lagoon, crest, slope) and linked by groups that feed in two or more of the subsystems. In fact, I believe we should return to the Ecopath models that do not include this type of structures, and fix them before we use them for spatial modeling.
VC: Let's be cautious before we make such changes; first we must check that they really do what Pimm says they do.
CW: I checked the number of pools in Ecopath models vs. the maximum value of the vulnerability multipliers that could be accommodated without self-simplification of the food webs, which more of less corresponds to S. Pimm's test of stability. There was a general trend toward loss of mid-trophic level pools, except in models with very detailed diets. Medium-sized models were unstable.
DP: Pimm's routine will do more or less the same thing, for any model we want to analyze, as a part of the Ecopath diagnostic system.
CW: The way to go about this problem of stability is not necessarily via better ecology, but through a better look at the policy questions and the inherent credibility of answers.
KC: Some will want to assure themselves that the ecology included in their model is as good as possible.
VC: Certainly users should check for cycles in their models. This can be done easily using a routine of Ecopath.
KC: But we don't want a cookbook either, or people mindlessly generating numbers. Better perhaps to have a number of guidelines, such as Carl's check of the effect of changing the vulnerability schedules.
DP: But some of the guidelines we teach to Ecopath users do have cookbook character, e.g., 'avoid cannibalism', 'do not include less than 12-15 boxes spread over the whole food web', etc.
CW: Let's now move to the third group of predictions.

3. Predictions that cannot be trusted.

CW: The biggest question we have is that relating to animals with trophic changes during their life history. We have tried to resolve this through split pools, but generally, these split pools, in Ecosim, tend to predict too much compensatory change. This is disappointing. It could be due to a failure to describe the factors affecting survival or changes in life history, etc.
JK: Behaviour is context dependent, and rules should not be inflexible. One useful rule though is that the P/B value tells us how fast unex-

pected things can happen; groups with low P/B cannot change as fast as others.

KC: I can imagine, that there might be policy situations in which it would help to distinguish more than the two size/age groups presently allowed as 'split pools' in Ecosim.

CW: I don't believe having more size/age groups would make much of a difference. Besides, the data and computational requirements would be so enormous, we would lose all present advantages of the package. In any case, it would be very difficult to have a highly detailed size/age distribution consistent with the rest of Ecopath/Ecosim/Ecospace. In fact even our present, relatively simple representation leads, through various amplifications, to highly complex behavior, e.g., in the case of Jim Kitchell's model of the Central Pacific. Another source of problem is when we use the time-shapes in Ecosim to represent changes in productivity.

DP: If we build models without physical forcing, we can't well introduce such forcing though the back door, as it were, and expect it to work well.

VC: Yet we do that with the economic component, and it appears to be of some use.

KC: Is it really true, Carl, that these predictions are completely "hopeless"?

CW: They are 'just so stories' and cannot be validated. Just like the various empirical models that link recruitment and some environmental parameter, which all break down the year after they are published.

DP: Seems to me that the Cury-Roy (Cury and Roy, 1989) hypothesis of dome-shaped recruitment windows did not break down. In fact, it has so far survived every test to which it was put.

TP: Could not the 'time shaping' in Ecosim be replaced by a proper, if somehow generic oceanographic model to simulate production at the lower trophic levels?

DP: The folks working on Prince William Sound have a good physical model, which nicely predicts phytoplankton blooms, the growth of the zooplankton that feeds on it, and the effect on the juveniles of some fishes. The problem is that they cannot put the system higher up in the same modeling framework. Thus, they cannot deal, e.g., with killer whales.

TP: Something like that would make lots of sense in Peru and other upwelling ecosystems.

DP: Such model would make sense there, as not much of interest to the fishery happens at the highest trophic levels, now that the birds, etc. are gone.

VC: Clearly, a model must respond to a specific need for predictions.

CW: Exactly; in our Grand Canyon work, we must model things on an hourly basis, because this is the scale at which interventions (water releases) happen, and most of the ecological impact flows from there.

KC: Can we not use Ecoranger to deal with some of the uncertainties here?

JK: I don't think so; the uncertainty is the structure of the system itself, so the true uncertainty will no be captured by a sensitivity or Monte-Carlo analysis of an existing model.

DP: So we delude ourselves when we use Ecoranger to define prior distribution, generate 'random' models, look at the posterior distributions, etc?

JK: It's like painting with a broad brush. It gives a broad picture; whether this is 'real' or not is another question. Perhaps it is 'halfway' correct.

CW: This makes me think of these various estimates of speed of light: all had confidence intervals about them. Yet the next estimate was invariably outside of the interval. This is similar to ecosystems, where qualitatively new behaviors emerge which are outside of the range of prediction of previous models.

DP: In Ecoranger, we can resample not only parameters such as biomass, P/B ratios, etc, but also the diet compositions. This means that by randomly generating new linkages between groups (where such linkages are possible), we can, in principle generate new system behaviors. However, nobody has used this routine to that end yet.

KC: So we all agree that model uncertainty cannot be overcome, but that Ecoranger is useful, in that it provides a measure of the minimum amount of uncertainty one has to accept in a given model.

VC: I certainly agree with that. Indeed, Ecoranger has now been accepted by lots of colleagues who previously were critics of the Ecopath approach.

DP: This should conclude our debate of Carl's three types of predictions. Let's now briefly talk about what comes next. For one, Ecospace will be presented at the next annual ICES Science Conference, in Portugal (see abstract in Walters, this vol.). Also, Ecosim and Ecospace will now be incorporated as elements of the Ecopath training courses to be given in the context of a large international project funded by the European Commission. Perhaps I should ask Villy to briefly describe this.

VC: The project Daniel just mentioned is called "Placing fisheries in their ecosystem context". It involves 31 institutions in Europe, West and South Africa, the Caribbean and Latin America as partners. It is the intention over the next 4

years to arrange a number of training workshops and conferences aimed initially at developing Ecopath models, and next on comparing published Ecopath models across latitude, degree of exploitation, and so on. Colleagues interested in the activity are very welcome to contact me for details.

DP: Then there is the conference, sponsored by IOC/SCOR's Working Group 105, and ICES, on the Ecosystem Impacts of Fisheries, to be held next year in Montpellier. I should chair its session on 'Trophic Impacts'. My own contribution to that will probably be to review the trophic level concept, so important in Ecopath.

VC: There is also the FAO-sponsored workshop to which the present workshop was the preparation. I think this should be organized like an ICES working group, and involve very knowledgeable colleagues, willing and capable to perform a systematic examination of the features of key predictions.

TP: This will require a more formal structure than we had during this workshop, with steps clearly outlined beforehand.

KC: I agree. Indeed, there should be dummy policies for people to test, resembling those which are usually evaluated.

AP: Recently, the U.S. National Research Council released a report on methods for fish stock assessment in which the methods were all applied to a dummy data set.

VC: The participants, if from the ICES area, will need data sets which enable comparisons between the outputs of Multispecies VPA and those of the Ecopath suite. They might otherwise not be interested.

KC: But real Ecopath users will also be needed.

TP: Experts having experience with other methods are important, lest the workshop might be preaching to the converted. The NRC approach that Daniel mentioned might be best, as it introduces a degree of objectivity in the evaluations.

DP: So what do we conclude for this workshop?

RS: If we can trust the type of predictions Carl identified as trustworthy, then we should be in good shape.

AP: But we need more than Carl's opinion.

CW: Here it comes, nevertheless: one thing we will have to watch is the possibility of bugs as explanation for some of the strange patterns we got with split pools. But in any case, we should not give too much attention to time transient patterns. Generally, I am not surprised by our results, which are mostly O.K.

JK: When split pools gave us problems, we assigned the two stages to separate 'species'. This helped. Another observation I have is that the MPA scale determined by Ecospace seems to work, and can be used to screen policies and eliminate bad thinking about economic trade-offs.

KC: My comment about the workshop as a whole is that I'm happy with the way it went, and with its results.

JK: I can only recommend that the software now be made ready for beta testing by as many users as possible, so its remaining bugs can be ironed out. Then, let's organize blind round robins for further definition of its capabilities, as suggested before. The software will then grow and reach its potential.

VC: We will do that. We would be thankful if people sent us detailed bug reports, which we will fix in the version that can be downloaded from the Web. The address from which to download Ecopath is www.ecopath.org. or you can contact me at v.christensen@cgnet.com.

AB: Will there be a separate user guide, as for the DOS version, or an online guide, as for version 3.0 and 3.1?

VC: The beta test version, to be distributed some months from now, will have an online guide, updated from that in version 3.1. This workshop and some others we ran provided ideas as to what to add to the available material. We will have to reverse-engineer a few parts of the text, based on the routines we now have, and their documentation in the primary literature.

KC: This has been a very successful workshop and I thank Carl and Villy for their contributions and Daniel and Gunna for the workshop organization. Far more has been achieved than I anticipated, and I can see that we now have a useful tool at hand, which will make it to possible for critical 'what-if' questions to be asked in a multispecies spatial context, without the big guns who developed Multispecies VPA having to be consulted. Making this power widely available will be very valuable. I'm very happy and learnt a lot and am looking forward to the final report soon.

DP: We thank you and FAO for having made this possible. We are quite proud that such an important player as FAO expressed interest in the work done at the Fisheries Centre; I would like to thank Gunna for her help with the workshop preparations and Carl Walters, for the fascinating lectures, and for having turned the Ecopath suite into the dynamic tool it now is. Finally, I would like to thank those colleagues who came on their own, such as Jim Kitchell, to share their ideas with us.

References[c]

Anon, 1995. The Kyoto Declaration and Plan of Action. International conference on the sustainable contribution of fisheries to food security. Kyoto, Japan, 4-9 September 1995. Fisheries Agency, Japan. 21 p. [6]

Asila, A. and J. Ogari.. 1987. Growth Parameters and Mortality rates of Nile perch (*Lates niloticus*), estimated from length-frequency data in the Nyanza Gulf (Lake Victoria). FAO Fish. Rep. 389:1272-287. [31]

Bakun, A. 1996. Patterns in the ocean: ocean processes and marine population dynamics. Centro de Investigaciones Biologica del Nordeste, La Paz, Mexico/ University of California Sea Grant, San Diego, USA. [27]

Bundy, A. 1997. Assessment and Management of Multispecies, Multigear Fisheries: A case study from San Miguel Bay, the Philippines. Ph.D. Thesis. University of British Columbia, Vancouver, B.C. Canada. 395 p. [39]

Butterworth, D.S., Cochrane, K.L. and J.A.A. de Oliviera. 1997. Management procedures: a better way to manage fisheries? The South African Experience. In: E.K. Pikitch. D.D. Huppert and M.P. Sissenwine (eds.). Global Trends: Fisheries Management. American Fisheries Society, Symposium 20, Bethesda. [7]

Christensen, V. and D. Pauly 1992a. Ecopath II - A system for balancing steady-state ecosystem models and calculating network characteristics. Ecol. Modeling 61: 169-185. [10]

Christensen, V. and D. Pauly 1992b. A guide to the Ecopath II software system (version 2.1). ICLARM Software 6, 72 p. [10]

Christensen, V. and D. Pauly 1995. Fish production, catches and the carrying capacity of the world ocean. Naga, the ICLARM Quarterly. 18(3): 34-40. [10]

Christensen, V. and D. Pauly 1996. Ecological modeling for all. Naga, the ICLARM Quarterly. 19(2):25-26. [10]

Cury, P. and C. Roy. 1989. Optimal environmental window and pelagic fish recruitment success in upwelling areas. Can. J. Fish. Aquat. Sci. 46: 670-680. [41]

Dalsgaard, J. and D. Pauly 1997. Preliminary Mass-Balance Model of Prince William Sound, Alaska for the Pre-Spill Period 1980-1989. Fisheries Centre Research Report 1997, 5(2), 34 p. [32]

FAO 1995. Code of Conduct for Responsible Fisheries. FAO, Rome. 41p. [6]

Froese, R. and D. Pauly (Editors). 1998. FishBase 98, Concepts, Design and Data Sources. ICLARM. [9]

Gayanilo, F.C., M. Soriano and D. Pauly. 1989. A draft guide to the Compleat ELEFAN. ICLARM Software 2, 65 p. [31]

Haggan, N. 1996. Integration of Traditional Environmental Knowledge p. 88-89 In: D. Pauly and V. Christensen (eds.) Mass-Balance Models of North-eastern Pacific Ecosystems. Fisheries Centre Research Reports 4(1). [26]

Haggan, N. 1998. Reinventing the Tree. p. 19-39. In: T. Pitcher, D. Pauly & P. Hart (Eds.) Reinventing Fisheries Management. Chapman & Hall. London. [26]

[c] The page(s) where each reference is (are) given in square brackets, and thus these references may be used as an authors' index.

Hughes, N.F. 1986. Changes in the feeding habits of Nile perch *Lates niloticus* (L.) in Lake Victoria, East Africa since its introduction in 1960 and its impact on the native fish community of the Nyanza Gulf. J. Fish Biol. 29: 541-548.[31]

Jarre, A., M.L. Palomares, M.L. Soriano, V.C. Sambilay, Jr. and D. Pauly. 1990. MAX-IMS: A computer program for estimating the food consumption of fishes from diel stomach contents data and population parameters. ICLARM Software 4 27 p.[31]

Jarre-Teichmann, A. 1995. Seasonal mass-balance models of carbon flow in the central Baltic Sea, with emphasis on the upper trophic levels. ICES C.M. 1995 T:6.[27]

Jarre-Teichmann, A., K. Wieland, B.R. MacKenzie, H.H. Hinrichsen, M. Plikshs and E. Aro. 1997. Stock recruitment relationship for Central Baltic cod (*Gadus morhua*) incorporating environmental variability. ICES Symposium on the Recruitment Dynamics of Exploited Marine Populations: Physical-Biological Interactions, 22-24 September 1997, Baltimore, USA. [submitted to ICES Mar. Sci. Symp.] [27]

Jarre-Teichmann, A., L.J. Shannon, C.L. Moloney and P. Wickens. Comparing trophic flows in the Southern Benguela to other upwelling ecosystem. South African *Journal of Marine Science* (in press) [27]

MacKay, A. 1981. The generalized inverse. Practical computing. September 1981: 108-110. [10]

Moreau, J., W. Ligtvoet and M.L.D. Palomares. 1993. Trophic relationship in the fish community of Lake Victoria, Kenya, with emphasis on the impact of Nile perch (*Lates niloticus*), p. 144-152. In V. Christensen and D. Pauly (eds.) Trophic models of aquatic ecosystems. ICLARM Conf. Proc. 26. [30,31]

Munro, G. and U.R. Sumaila. 1998. Assessing the economic benefits of artificial reefs and marine protected areas in Hong Kong waters. In T. Pitcher et al. (eds) *Fisheries Centre Research Reports* (in press). [19, 20]

Ogari, J. and S. Dadzie 1988. The food of Nile perch *Lates niloticus*, after the disappearance of the haplochromine cichlids in the Nyanza Gulf of Lake Victoria Kenya. J. Fish. Biol. 32, 571-577. [31]

Paine, R.T., J.L. Ruesink, A. Sun, E.L. Soulanille, M.J. Wonham, C.D.G. Harley, D.R. Brumbaugh and D.L. Secord. 1996. Trouble on Oiled Waters: lessons from the *Exxon Valdez* oil spill. Annu. Rev. Ecol. Sys. 27:197-235 [32]

Pauly, D. 1986. A simple method for estimating the food consumption of fish population from growth data and food conversion experiments. U.S. Fish Bull. 84: 827-840. [31]

Pauly, D. and M.L. Palomares. 1987. Shrimp consumption by fish in Kuwait water: a methodology, preliminary results and their implication for management and research. Kuwait Bull. Marine Sciences 9:101-125. [31]

Pauly, D. and V. Christensen. 1996. Mass-Balance Models of North-eastern Pacific Ecosystems. Fisheries Centre Research Reports 4(1) 131 p. [26]

Pauly, D., V. Christensen, J. Dalsgaard, R. Froese and F. Torres Jr., 1998. Fishing down marine food webs. Science 279: 860-863. [21, 26, 39]

Pitcher, T. and D. Pauly, 1998. Rebuilding ecosystems, not sustainability, is the proper goal of fisheries management. p. 311-329. In: T. Pitcher, D. Pauly & P. Hart (eds.) Reinventing Fisheries Management. Chapman & Hall. London. [21, 26]

Pitcher, T.J. Fisheries management that aims to rebuild resources can help resolve disputes, reinvigorate fisheries science and encourage public support. In: Beckett (ed.) (in press a). [21]

Pitcher, T.J. Rebuilding as a New Goal for Fisheries Management: Reconstructing the Past to Salvage the Future. In: Eckschmitt, K. (ed.) Managing Ecological Systems: Challenges, Successes and Failures. John Wiley, NY. (in press b). [21]

Polovina, J.J. 1984. Models of a coral reef ecosystem I: the Ecopath model and its application to French Frigate Shoals. Coral Reefs 3(1): 1-11. [10]

Polovina, J.J. 1985. An approach to estimating an ecosystem box model. U.S. Fish. Bull. 83(3):457-560. [10]

Polovina, J.J. and M.D. Ow 1983. Ecopath: a user's manual and program listings. NMFS/NOAA, Honolulu Admin. Rep. H-83-23, 46 p. [10]

Roberts, C.M. and J.P. Hawkins. 1997. How small can a marine reserve be and still be effective. Coral Reefs 16:150. [39]

Sumaila, U.R. 1997. Cooperative and non-cooperative exploitation of the Arcto-Norwegian cod stock in the Barents Sea. Environm. Res. Econ. 10:147-165. [19]

Sumaila, U.R. Protected marine reserves as fisheries management tools: A bioeconomic analysis. Fish. Res. (in press). [19]

Ulanowicz, R.E. 1986. Growth and development: ecosystems phenomenology. Springer Verlag, New York, 203 p. [10]

Walters, C., V. Christensen and D. Pauly. 1997. Structuring dynamic models of exploited ecosystems from trophic mass-balance assessments. Rev. Fish. Biol. Fish. 7, 139-172. 6, 11, 15, 30, 31]

Walters, C., D. Pauly, V. Christensen and J. Kitchell. 1998. Representing density dependent consequences of life history strategies in an ecosystem model. Ecosim II. (under review by CJAFS). [11]

Walters, C., D. Pauly and V. Christensen. 1998. Ecospace: a software tool for predicting mesoscale spatial patterns in trophic relationships of exploited ecosystems, with special reference to impacts of marine protected areas. To be presented at Theme Session (S) on 'Visualization of Spatial (including Survey) Data', of the ICES Annual Science Conference, Cascais, Portugal, September 1998. [6, 11, 13]