DISCUSSION OF SESSION 1

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Each panel presentation was followed by time for discussion, which is reported in sequence here.

In response to *Jock Young's* recommendation that much further work on larval feeding ecology is needed, Jeff Leis pointed out that information on feeding was available in a thesis by Lou Hock Chark from James Cook University. Jock Young then referred to work on feeding of larvae which was proposed in NSW in the near future. Peter Gehrke pointed out that there are already several studies in progress, such as those by NSW Inland Fisheries. Jock Young acknowledged that there is probably a backlog of work about to be published and predicted that there will be a dramatic increase in future studies in this area.

In response to a question by Don Hancock, Jock Young defined the "critical period hypothesis" as relating to the time period between the transition from endogenous to exogenous feeding. The availability of food at this time influences the level of mortality by starvation. Iain Suthers mentioned that an extension of this theory is the match/mis-match hypothesis; for example mixing of the water column in spring brings nutrients to the surface layers which results in an increase in the availability of food. If this event coincides with larval hatching then it is known as a match; if not, the events are mismatched.

Jeff Leis suggested that food limitation is more likely in the tuna species spawning in the tropics due to the highly patchy distribution of larvae. He further suggested that this situation may be the exception rather than the rule in the tropics. Jock Young agreed that work on other tropical species would be of great interest.

Pancho Neira questioned Ron Thresher on the application of the critical period hypothesis to live bearers such as clinids. Ron Thresher responded that the situation is the same except that larvae do not have a yolk-sac. As an example he referred to his own work in Storm Bay, where recruitment by clinids to rock platforms corresponded to phytoplankton blooms 6-7 weeks prior. It appears that survival to recruitment largely depends on the occurrence of a phytoplankton bloom at the time the larvae are released.

Daniel Gaughan was asked whether there were any general studies on freshwater fish diets. He replied that there are no specific examples but suggested that, like marine fish, they would feed on the most abundant species present which would include rotifers, cladocerans and cyclopoid copepods.

Iain Suthers suggested that it may only be necessary to examine size structure, as opposed to classifying individual species, to answer questions relating to food limitation. Jeff Leis pointed out that the nutritional value, behaviour and

pigmentation (and hence visibility to predators) of prey are equally as important as size in determining the diet of larvae. If just size frequency was analysed the nutritional value of each species would have to be assumed to be equal. He went on to suggest that confining the size groups to within classes, such as Crustacea or Chordata, would overcome this problem. Iain Suthers agreed that although there are some problems with such a model, it would have some use in bulk-sorting of larvae when investigating feeding rates. Mike Sinclair commented that the data required would depend on the question being asked, for example for some species you may only need size data but for other, more selective, species more detailed data may be required.

Ron Thresher believed that the fundamental problem in gut content analysis is bias created from variability in retention rates; for example, the importance of copepod and tintinnid tests in blue grenadier larvae may be overestimated due to the relative length of time retained in the gut. Recent work has also shown the importances of ciliates in these diets which were previously overlooked due to inadequate preparation techniques. He stressed the importance of accurate tests of clearance rates and the usefulness of stable isotope analysis which should be run in conjunction with gut analysis studies. Peter Gehrke suggested that the importance of rotifers to freshwater larvae may be overestimated if the difficult-to-resolve mouth-parts are overlooked. Greg Jenkins made the point that bivalve veliger shells may be visible in the gut for a relatively longer period than other plankters while naked ciliates are not well preserved in straight plankton samples!

Barry Bruce asked *Nigel Preston* why penaeids in the Gulf have two spawning periods when the recruitment from one event is much more successful than the other. Nigel Preston, in the spirit of the workshop, put the question over to Peter Rothlisberg, who replied that reproduction of penaeids appears to be conservative; studies throughout the Indo-Pacific have shown that penaeids consistently have two distinct spawning periods, but the output from these spawnings is variable. In Malaysia the two spawning periods coincide with two wet seasons, and there is significant recruitment from both. In a much harsher environment such as the Gulf of Carpentaria where there is only one wet season, the two peaks are still evident. He argued that this reproductive strategy allows great flexibility in adapting to new or changing environments. The Gulf is < 6000 years old, yet penaeids have coped with this environment very well, albeit differently to other places.

Maria Milicich questioned *Greg Jenkins* as to whether patterns of growth with larval age may be developmentally determined rather than a result of feeding success etc. Greg Jenkins agreed that you would probably get a species-specific growth trajectory based on development pattern but pointed out that in the case of southern bluefin tuna larvae the pattern appeared to be significantly different inside and outside the patch, suggesting an environmental factor was operating. John Gunn pointed out that because the tuna were only studied for an 8-9 day period they may not have reached the inflection point of increased growth which was observed in the growth trajectories of other species mentioned in the talk. Ron Thresher found that blue grenadier showed an initial linear growth period of variable duration before 'launching' into exponential growth. This was interpreted at the time as variation in the onset of successful feeding. Greg Jenkins responded that even at the eight day point of growth, differences were noticeable between growth trajectories inside and outside the patch, ie larvae inside the patch had mostly linear trajectories, in some cases negative, whilst larvae from outside the patch already showed a tendency to exponential growth. He agreed that later exponential growth was probably likely for many larvae from within the patch.

Following *Iain Suther's* contribution, Mike Sinclair proposed that the larval stage may not be the most vulnerable stage (maybe a myth

started by Hjort and Cushing!) and that many studies suggest food is not a limiting factor. Iain Suthers disagreed and stated that only three papers (Peterman, Suthers and Bailey) have looked at the pelagic juvenile stage (supposedly the all-important stage) and all concluded that food availability is important in explaining growth and presumably survival. Mike Sinclair responded that in the paper by Peterman on pelagic juvenile salmon, there was an impact of food limitation on larval survival but not on year-class size. Greg Jenkins further questioned the importance of the early larval stages by pointing out that the majority of work has been on these early stages and that more work needs to be done on the later larval, early juvenile, and juvenile stages which may perhaps be of greater importance.

Bryce Stewart began the discussion on *Maria Milicich's* panel presentation by pointing out that in a study he conducted on juvenile King George whiting, where the fish were on a cycle of 15 days of food and then 5 days of no food, the otoliths responded by forming 15 wide rings followed by 5 narrow rings. He also achieved accurate results when back-calculating. Maria Milicich responded that such results may indicate that the utility of this method varies between species and should be validated for each species. She suggested that in her case there may have been a lag effect exceeding the duration of the experiment, but suggested that if this were the case it would limit the application of the method anyhow. Greg Jenkins made the point that it is now well known that the relationship between otolith-size and increment size varies with growth rate and that you could have similar increment spacings in fish growth at different rates due to differences in this relationship. If this difference is accounted for, back-calculation may still work. Maria Milicich responded that in such cases it would then be reasonable to proceed with back-calculation. She suggested that a lag in the response of otoliths to changes in somatic growth may explain her results but that it would have to be a lag of at least 10 days and such a lag would make back-calculation difficult.

From his own experience, Ron Thresher warned against generalising from the results of laboratory-based studies because increment formation was often abnormal in the laboratory. However, by the same token, he suggested that when laboratory studies gave negative results it would usually be put down to laboratory artefacts and the studies would not be published, with the published literature being biased towards positive results. He also mentioned that the fish size / otolith size relationship in Maria Milicich's experiment was not strictly linear and therefore the experimental results would depend on the part of the curve you were working with. Tony Fowler questioned how it was known that increments were laid down in starved fish at all. Maria Milicich responded that increment deposition in these treatments had been verified using tetracycline marking. Bryce Stewart mentioned that starved whiting had increments so narrow as to be barely detectable with the light microscope. Greg Jenkins suggested that scanning electron microscopy is often needed to resolve increments in slow growing fish.

Barry Bruce commented that his work on larval whiting indicated almost no lag between the decrease in ring spacing and reduction in food. Greg Jenkins responded that otolith growth in young larvae may respond more quickly to change in food intake than older individuals which would have larger energy reserves. Iain Suthers then asked Ron Thresher how validation studies could be designed to overcome these problems and suggested mesocosms as a possible solution. Ron Thresher responded that while mesocosms may be an answer, field-based studies were the ideal solution (i.e. tracking cohorts etc.), but these were difficult to conduct.

In a final general question, Peter Doherty asked if Greg Jenkins would reconsider his earlier statement that coastal fish larvae were generally not food limited, particularly in light of the examples given by the keynote speakers. Greg Jenkins responded by saying that he was basing his comment on the small amount of work done in coastal waters of Australia (mainly Victoria and Tasmania) and work from overseas on highly productive bays and estuaries. He suggested that he was not trying to generalise to all coastal waters, and agreed that there would be many coastal situations were food limitation would probably occur.