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Reply to 'Do oceanic zooplankton aggregate at, or near, the deep chlorophyll maximum?'

by P. B. Ortner,¹ P. H. Wiebe,² and J. L. Cox³

We appreciate the time and attention given our paper by Longhurst and Herman and the opportunity to reply to their critique. Unfortunately, we believe their difficulties with the presentation of our data are more subjective than substantive. In fact, we believe a careful reading of our text will show that our conclusions are suitably conservative.

We would first like to address points raised with regard to the methods we employed:

1) Longhurst and Herman state that the water bottles we subsampled for chlorophyll were placed at 25m intervals from 0 to 200m. In fact, *as described in the given citation*, additional water bottles were frequently added in the vicinity of the DCM. These additional bottles were spaced as closely as 5m apart and a single cast employed as many as twenty-one bottles. Thus, our knowledge of DCM position was much more accurate than Longhurst and Herman imply.

2) Longhurst and Herman state that the depths sampled by our Clarke-Bumpus samplers were imprecisely known because we used wire out and wire angle to estimate the depth strata sampled. It is true that information was all that was available during a Clarke-Bumpus tow, but, *as stated in our text*, depth recorders were used on all tows. For example, a 0-250m Benthos time-depth recorder was attached to the deepest Clarke-Bumpus sampler of each series. While wire out and wire angle do give a relatively poor indication of net depth, especially at greater depths (500-1000m), when used at shallow depths in conjunction with depth recorders fairly precise estimates of net depth can be obtained. Indeed the precision of our depth estimates was about ± 2.5 m. We are surprised that the above methodological information was overlooked since the above facts, only summarized in the text of our paper, are given in great detail in the background document

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(Ortner, 1978) which Longhurst and Herman have had in their possession for several years.

Methodology aside, we have substantial difficulties with Longhurst and Herman's re-interpretation of our data. Given the integrative nature of the zooplankton samplers we employed, the most reasonable treatment of our zooplankton data is to consider any given value as representing an interval average. However, if we arbitrarily decide each interval biomass value applies precisely to the midpoint of that interval, as in Longhurst and Herman's Table 1, what do we find? *As our text made clear*, the slope water DCM observed in August 1975 was erased by November 1975; therefore, Tows 12, 13, 36 and 38 must be excluded from Table 1. We then find on seven occasions this epizooplankton maximum was at or just below the exact centerpoint of the DCM while on eleven occasions it was just above it. The biomass data is at best barely suggestive of a systematic trend.

We agree, and *nothing in our text suggests otherwise*, that the shallow subsurface ATP maximum, when present, is only occasionally coincident with the exact centerpoint of the DCM. In our text ATP profiles were used to support other data indicative of microbial activity. In any case, we fail to see how a discussion of ATP distribution is germane to the present issue of the coincidence of epizooplankton and the DCM.

There are additional suppositions made with regard to our data which we wish to dispell. Our text and abstract strictly related epizooplankton and the other biological parameters measured to vertical temperature profiles and thereby inferred relationships. In certain respects the data set was inconclusive primarily for sampling considerations (different sampling intervals for different parameters and lack of synopticity). For example, internal waves can cause vertical displacement of a DCM 10m or more over periods of several hours; since water bottle casts and net tows were displaced in time any errors resulting from the lack of synopticity could only be corrected by reference to an independent hydrographic parameter such as temperature. Furthermore, although zooplankton biomass per se is an important parameter to consider in relation to DCM position and intensity, we would like to emphasize the importance of focussing upon specific DCM characteristics and zooplankton taxonomic groups. None of Longhurst and Herman's remarks appears directed at our findings with respect to the functional groups we were able to enumerate. The pattern of distribution of those groups, however, strongly supports our contention that ecological interactions at or near DCM depths may be unique. Independent work by Fairbanks and Wiebe (1980) provides additional evidence that this may be true for seasonally stratified regimes throughout the Northwestern Atlantic.

Indeed we agree fully as to the critical significance of what our colleagues aptly term "the newly understood complexities of the layer biocoenosis in the vicinity of the pycnocline." The critical importance of those "complexities" is, *and was*,

one of our principal conclusions. On various occasions, and in various hydrographic regimes, the most intense grazing pressure may be located at, above, or below the exact apex of an often rather broad DCM feature. We must make it clear, however, that our data cannot be used to rigorously determine if the DCM is, or is not, the focus of grazing pressure. Although Longhurst and Herman suggest otherwise we did not do so. Far more intense experimentation is required to confirm or deny that hypothesis. However, we believe we have clearly shown that with the onset of stratification various zooplankton parameters are intimately related to seasonal temperature and density structure which are in turn determinative of the occurrence and position of the DCM. Recently obtained high resolution zooplankton data given in Ortner *et al.* (in press) document a close relationship between vertical zooplankton distribution and seasonal pycnocline depth.

In our paper we discussed the degree to which our data agreed or disagreed with that given in a number of past studies including Longhurst's extensive analysis of the EASTROPAC data (Longhurst, 1976). We stand by that evaluation. It is manifestly clear, however, that the historical data base is inadequate to resolve many of the issues raised in our paper, in Longhurst and Herman's critique, and in our reply. Higher resolution data on the vertical distributions of plankton standing stock, turnover rate and grazing activity are clearly required. We do not, however, expect a single explanation to emerge which will adequately describe for different seasons and different hydrographic regimes the complicated relationship between the DCM, the epizooplankton, and the seasonal pycnocline. As we stated at the close of our paper, our own unpublished data indicates significant differences may be found between permanently stratified and seasonally stratified regions.

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