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Comment

Interactive comment on “Distributions and assemblages of larval fish in the East China Sea in the northeasterly and southwesterly monsoon seasons 2008” by W. Y. Chen et al.

Anonymous Referee #1

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General comments:

The manuscript showed ichthyoplankton communities in relation to hydrographic conditions in winter (northeasterly monsoon season) and summer (southwesterly monsoon season) 2008 in the East China Sea. This type of studies has been reported in many papers in the waters around Taiwan and the East China Sea (e.g., Okazaki and Nakata 2007; Lo et al. 2010; Hsieh et al. 2012). Unfortunately, the current manuscript fails to show properly new findings, so authors may need to emphasize differences from the previous outcomes and discuss more relevance to the previous ones. Since few studies have been conducted in the center of the East China Sea, it would be advantage of this study.

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As no clear statements on the aims were mentioned in introduction, it would be hard for readers to understand focus of this study. What are motivations of this study? Although authors addressed that this study was a part of the Long-term Observation and Research of East China Sea (LORECS) to assess the impacts of the reduction in Changjiang River discharges on marine environments, it would be difficult to show contribution of this single year-study in the project LORECS. So, it would be better to address this in acknowledgement. The last paragraph of introduction requires clear aims of this study to discriminate from the previous studies.

Authors concluded that environmental factors affecting larval abundance are water temperature during winter and food availability during summer. Results supporting the conclusions, however, include ambiguity and arbitrariness. First, no definition on primary production and zooplankton as a food source for larval fish was mentioned in the text. Does zooplankton wet weight mean zooplankton collected simultaneously with fish larvae or another zooplankton ring net for examining food availability, such as density of copepod nauplii? In the case of the former procedures (at least the context means so), it would be difficult to evaluate food availability for larval fish. Even though authors noted its difficulty in discussion, they concluded that food availability affects spatial distribution of larvae in summer based only on a positive relationship between larval abundance and primary production.

On the other hand, larval abundance in winter positively correlated with zooplankton wet weight in addition to SST, but authors concluded that larval abundance is affected by water temperature rather than food availability. As authors noted difficulties on evaluating food availability, it would be plausible that SST affected larval abundance than did food availability in winter. Differences in conclusions between summer and winter, however, are based on the arbitrary logics. Authors need to clarify definition of primary production and zooplankton wet weight and understand what these indices mean.

Analysis for ichthyoplankton communities are OK but their distribution patterns in relation to environmental factors includes many problems. I suggest that the current

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manuscript need major changes including a critical focus on this study, new data analysis in results and much more detailed discussion on relevance to the previous studies. Based on the above, I do not recommend this manuscript for publication at this time.

Specific comments:

The last paragraph of 4.2 misleads the outcomes. It is described that larval (abundance?) increased with primary production but not SST and zooplankton wet weight. . . This is incorrect, just larval abundance positively correlated with primary production but not with SST and zooplankton wet weight. . . . If authors would like to mention changing states, it would need to compare seasonal differences in larval abundances between winter and summer.

Although distribution patterns (neritic, oceanic benthic pelagic) in the adult stage are shown as superscripts in Table 3, these information do not contribute in discussion at all. It might be better to show spawning season of each species to compare it with the larval production.

In results of hydrographic conditions in p. 7079, lines 18-24 and Fig. 3, it would be hard to identify changes in current structures from winter to summer in Fig. 3 as explained in the texts. Is the flow rate shown using not only length but also color of arrows?

P. 7077, line 20, LORECES should be LORECS.

Interactive comment on Biogeosciences Discuss., 10, 7075, 2013.

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