

# ***Interactive comment on “Distributions of mixed layer properties in North Pacific water mass formation areas: comparison of Argo floats and World Ocean Atlas 2001” by F. M. Bingham and T. Suga***

**Anonymous Referee #1**

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This study examines how well the World Ocean Atlas 2001 (WOA01) climatology depicts the winter mixed layer in the North Pacific, by comparing with in-situ observation data from Argo profiling floats during 2004 and 2005. It focuses on the locations and properties of the winter mixed layer corresponding to the formation regions of several water masses, namely the subtropical, central, and eastern subtropical mode waters and the tropical water. I think this work is important because, as the authors mention, the WOA series has been widely used for the surface boundary conditions in numerical studies. The authors' conclusion that the water mass formation regions match well between Argo data and WOA01 except for the two types of CMW (particularly the denser one) is not surprising because each of CMWs is formed in a narrow-band region be-

tween fronts, specifically the Kuroshio Extension front, the Kuroshio bifurcation front, and the subarctic front, which have high spatial and temporal variability (Oka and Suga, 2005, JPO). Nevertheless, I believe this work is worth publishing, since the formation of CMW, which carries mid-latitude SST anomalies to the subsurface subtropical gyre, is least well reproduced in the numerical models among the North Pacific mode waters. This study is expected to provide a key to improvement for such numerical studies. I therefore recommend that this manuscript be accepted, after the following issues are addressed.

The most questionable part in this manuscript for me is the latter half of Sec.3 (p.10, l.5-), which strays from the main theme of this manuscript that the winter mixed layer in WOA01 is evaluated by using Argo data. In Sec.1, the authors clearly write, “the main question to be addressed in this paper is: How well does the WOA01 depict the T-S properties and outcropping regions of some of the important water masses in the North Pacific?” (p.4, l.24-27), and the analysis proceeds with that theme until the middle of Sec.3. Then, to the contrary, the authors start describing the mixed layer status in 2004 and 2005 on the basis of WOA01 in the latter part of Sec.3. This part obscures the theme of this paper and confuses the readers. Of course if the authors try to evaluate the WOA01 winter mixed layer on the basis of Argo data, there should be some discussion about interannual variability because some of the difference in the winter mixed layer status between Argo data in each winter and WOA01 must be due to interannual variability. I think the current description in the latter part of Sec.3 is beyond this need. My suggestion is to separate the latter part of Sec.3 from this manuscript, to make the theme of this paper consistent. The separated part is worth pursuing, but I think it's better not to do it in this paper.

Another concern for me is the method of interpolating mixed layer properties from Argo data onto 1 degree grid. With the method explained in Sec.2, aren't the mixed layer properties over the North Pacific biased strongly to those at particular Argo observation points, because the Argo observation density in each winter of 2004 and 2005 (Fig.1)

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is much lower than 1 per 1-degree square for the most part of the North Pacific? There might be a better method of interpolation, using optimal interpolation or whatever, although I understand that no method is perfect. Even if the authors will keep using the current method, they should consider that some of the features they obtained might be (at least partly) due to that method. Specifically, the clearer delineation of volume distribution of the water masses with Argo data (Fig. 2b) might result from this interpolation. Also, the absence of water located to the fresh side of the main thermocline in the case of Argo (Fig. 2b) might be due to the fact that the Argo float did not make observations in the vicinity of the North American coast where mixed layer salinity is very low, rather than due to the averaging process in WOA01.

Other specific comments:

p.2, l.11, “whose T-S properties vary from what is shown in the WOA01”: This is not correct. The authors write that in WOA01 there is “very little indication of DCMW as a maximum in volume” (p.6, l.19). What they are actually doing is to compare the DCMW properties from Argo data with those from the reference (= Oka (personal comm.) ).

p.3, l.22-23, “the Kuroshio Extension front and the subtropical front (Suga and Hanawa, 1995): Did Suga and Hanawa mention the relation between the STMW formation region and the subtropical front? Maybe better to add a relevant reference for that relation.

p.3, l.27 - p.4, l.1, “Eastern Subtropical Mode Water (ESTMW) ... Thompson, 2000)”: What about its formation dynamics?

p.4, l.8, “to an unprecedented degree”: Can the authors write more specifically, presenting the spatial and temporal resolution/coverage of Argo float array?

p.4, l.9: Delete “at the surface”.

p.4, l.12-14, “MLDs generally agreed with ... north of the front”: It is better to explain why these underestimation and overestimation occur.

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p.4, l.24-27, “For that reason, the main question ... North Pacific?”: I think it’s better to mention just after this sentence that the authors examine this by comparing the winter mixed layer between WOA01 and Argo data. Also, it might be better to state clearly how this study is new, compared to Ohno et al. (2004).

p.5, l.5-6: “Oka (personal communication)”: There are too many “Oka (personal communication)” in this manuscript. I believe some of them (for example, those in Table 1) can be substituted by other relevant references.

p.5, l.7-9, “This criterion is less strict ... Kara et al. (2000).”: This sentence should be more specific. What criterion did de Boyer Montegut et al. (2004) and Kara et al. (2000) recommend or determine? What was the reason for that?

p.5, l.25, “up to 10 degree”: 10 degree is quite large. How large is the maximum search radius that the authors actually used for each winter?

p.6, l.12: Put “from WOA01” just after “mixed layer volume”.

p.6, l.13: Put “in the subtropics” just after “in the main thermocline”.

p.6-7, 3rd paragraph of Sec.3 (starting with “In the pictures of”): I understand that the discussion in this paragraph is important, but can’t the authors move it to another place in the manuscript? This paragraph is cutting off the connection between the two paragraphs just before and after this paragraph. (The same applies to 10th paragraph of Sec.3 in p.8, starting with “One feature”).

p.7, l.12: Put “(Fig. 2b)” just after “major water masses”.

p.7, l.13, “There are also peaks for NPTW”: I think the difference in volume distribution for NPTW between WOA01 and Argo is remarkable. Why is that?

p.7, l.14-16, “There is a volume mode ... it is normally thought of”: According to Oka and Suga (2005, JPO), the core temperature of DCMW changes interannually, between about 6.5C and 11C for the period of 1991-2003 (their Fig. 8a). I therefore consider that

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the temperature range of 9.5-11C is within the normal range, although it is relatively high.

p.7-8, 7th paragraph of Sec.3 (starting with “We now focus on”): In this paragraph and in the caption of Table 1, the authors should write clearly that the discussion is limited to the mixed layer. Phrases such as “the volumes of the various water masses” can remind the readers of the volumes from all depths (including those below the mixed layer), and are somewhat misleading.

p.8, l.9-10, “This result is probably not reliable ... as shown below.”: Which part does “as shown below” indicate?

p.8, l.11-12, “it makes sense to compare individual profile T-S properties”: This is not correct. Figure 3 does not compare T-S properties for the various water masses. It compares the formation locations for the water masses.

p.8, l.17, “The other water masses show similar distributions.”: Unclear sentence. Similar between what?

p.8, l.18-20, “There appears to be ... outside the gray area.”: It might be better to put “From Argo data,” at the top of this sentence.

p.9, 11th paragraph of Sec.3 (starting with “There are two types”): The content of this paragraph partly overlaps that of 8th and 9th paragraphs of the same section. Is it possible to integrate these paragraphs?

p.9, 12th paragraph of Sec.3 (starting with “Because surfacing floats”): I do not understand well why the authors calculate the medians and standard deviations shown in Fig. 2c. Since there are a certain percentage of green symbols in the gray areas for both LCMW and DCMW (Figs. 3d and 3e), it is naturally expected that the medians of T-S properties might not correspond to those from WOA01. How is the content of the last sentence of this paragraph (“Most floats ... in Table 1.”) interpreted?

p.12-13, 2nd paragraph of Sec.4: I do not understand the necessity of this paragraph.

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How are the Suga et al.'s (submitted, PiO) results important to those of this study?

p.17, Table 1, footnote: The authors divide STMW and ESTMW by the dateline. How is this validated? Any reference?

p.21, Figs. 3b and 3c: Show the latitudes at the top and bottom of each map.

p.21, Fig.3, caption, l.3, "the T-S properties given in the WOA01": At what depth are these T-S properties?

Technical comments:

p.4, l.12: Replace "2001" after "Conkright" by "2002".

p.5, l.5: Replace "is" after "control" by "are".

p.6, l.3: Replace "MLD" by "MLS".

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