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## ***Interactive comment on “Influence of river discharge on phytoplankton absorption properties: a case study in the East China Sea and Tsushima Strait” by S. Wang et al.***

### **Anonymous Referee #1**

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This paper intended to investigate the influence of fresh water on phytoplankton absorption properties, using in situ measured phytoplankton absorption coefficients and HPLC pigment data at the surface and SCM. The authors used samples collected in waters influenced by the Changjiang discharge in the East China Sea (ECS), and also introduced samples beyond the influence of that discharge in the Tsushima Strait (TS) as comparison. However, I do not think the interpretation of the results are convincing and using the TS samples as comparison is a good idea. It is not surprised that the absorption properties in the TS are different from those in the ECS. It could be just resulted from different size structures of phytoplankton in those two water bodies. But what leads to the difference in structure? Isn't it a combination of various factors?

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How to justify the contribution from the Changjiang? One puzzling issue is that similar absorption properties and phytoplankton size structures were observed at the surface and the SCM beneath it, and the authors attributed all these to the Changjiang diluted water (CDW). I am wondering how thick the CDW is. Also, if the water under observation was used as an example to demonstrate river impact, it should retain most features of the river water. Was considerable fraction of pico-plankton in the samples one of the features of the Changjiang water? The authors at least should provide some references relevant to the phytoplankton community structures in the Changjiang estuary and its vicinity. I believe there should be some. In a word, there seems no solid evidence to support the conclusion: “The majority of ECS surface samples taken from the low-salinity Changjiang diluted water (CDW), and even most of SCM samples taken from waters beneath the CDW, displayed significant fresh water influences.” I would like to suggest the authors change the title accordingly. Those in situ data are certainly of value. However, improvement in the interpretation of the data is advocated. The flow of the paper is not good. It reads like a lengthy data report, particularly the result section. Some details are not required, for example, Table 1 could be removed, and there is no need to use different symbols in Figure 1. In summary, I do not suggest its publication on BG as its current form.

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Interactive comment on Biogeosciences Discuss., 10, 14475, 2013.

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