This manuscript is a revised version of egusphere-2022-254 that I also reviewed. Dr. Debyser and coauthors have put a great deal of work into improving the manuscript, including (a) modifying the fractionation models and (b) changing the approach to evaluate the composition of PSW DSi.

In general, the revisions have significantly strengthened the manuscript, and my recommendation is to

accept after my two concerns are addressed as detailed below, including

1) some clarification in the method part, and 2) more rigorous statistical analyses in section 4.1.1

1) Method part on the Si isotopes measurement:

The authors should clarify, if they produced duplicate and triplicate measurements and mention that in the method part. It is very uncommon for stable Si isotopes in seawater if all the samples were only measured ones (using a standard-bracketing method with 3-4 replicates) as they are normally not volume limited. It is very important to have at least parts of the samples measured as full replicates including the MAGIC precipitation step and column chemistry and run in different analytical sessions (eg. De Souza et al., 2012; Grasse et al., 2013; Liguori et al., 2021). This is especially important in this study, as 1) δ^{30}Si in the study underlies many different processes, 2) samples with low DSi concentrations are difficult to measure correctly due to high matrix/Si ratio, 3) The authors did not measure δ^{30}DSi directly, but instead only measured δ^{29}Si.

And also, please report the uncertainties as 2SD, which is a common practice in reporting seawater Si isotopes data, and which will be useful for lab comparison.

2) Statistical analyses for discussions in section 4.1.1

The authors stated “AW follows the traditional isotopic effect of 5‰ and PSW follows the particularly low isotopic effect of 2‰”, but it is not obvious in Figure 6. If the authors plot fractionation curves with ε=2‰ on the left upper plot, they should also fit the dataset as much as the curves with ε=5‰. The authors should do statistical analyses for example curve fitting to support this statement.

Same goes to the statement “In PSW… A shift towards a mostly linear trend in summer is observed, suggesting open system kinetics below the mixed layer”, it is difficult to see this trend from the right upper plot in Fig.6. There are also some triangle datapoints visually follow the exponential trend. So please use statistical analyses to support this statement. This is very important for the follow up discussion to be convincing.

Minor comments:

1) The authors should also be careful with some terms/concepts that are discussed in the manuscript, for example: “$\delta^{30}Si(OH)_4$ in PSW does not show a good fit with either of the Rayleigh models”

Not both models are called Rayleigh model. One is called a “Rayleigh model”, and the other is called a “open system/steady state model”

2) In the paragraph Line 458 to 463, there's a logic gap from previous discussion to "this suggests that….PSW carries a remote signal of partial DSi uptake, and is controlled by mixing and dilutive effects rather than local biological processes". In fact, this is a conclusion to draw after the discussion in section 4.2. So, it came a bit too early to place this conclusion already in section 4.1.1, or more discussions/arguments are needed here.

References:

De Souza, G.F., Reynolds, B.C., Rickli, J., Frank, M., Saito, M.A., Gerringa, L.J.A., Bourdon, B., 2012. Southern Ocean control of silicon stable isotope distribution in the deep Atlantic Ocean. Global Biogeochem. Cycles 26, 1–13. https://doi.org/10.1029/2011GB004141

Grasse, P., Ehlert, C., Frank, M., 2013. The influence of water mass mixing on the dissolved Si isotope composition in the Eastern Equatorial Pacific. Earth Planet. Sci. Lett. 380, 60–71. https://doi.org/10.1016/j.epsl.2013.07.033

Liguori, B.T.P., Ehlert, C., Nöthig, E.M., van Ooijen, J.C., Pahnke, K., 2021. The Transpolar Drift Influence on the Arctic Ocean Silicon Cycle. J. Geophys. Res. Ocean. 126. https://doi.org/10.1029/2021JC017352