Author response to all referees' comments for Manuscript BG-2011-31 "Climate dependent diatom production is preserved in biogenic Si isotope signatures"

We really thank two anonymous referees and George Swann for providing us very insightful and constructive comments, which helped improve this manuscript. We have tried our best to carefully consider and respond to all the comments raised by the referees. We have now revised the manuscript substantially to improve the presentation of our data from the Bothnian Bay. Below, each review comment is listed in the left column of tables and our response/action is written in the right column of the tables. The necessary revised figures for this response letter are shown at the end.

Anonymous Referee #1

Comments	Response/Actions
Major comments	
My biggest concern is related to the temperature model (Eq.3); How are air and sea surface temperature related? The temperature model (Eq.3) uses air temperature, which appears to be more variant than the surface water temperature. For instance, considering the period between 1961-1990; According to figure 6b, the air temperature during the summer months varied somewhere between values as low as 6degC and as high as 17degC. For the same period the authors state in section 2: "During the periods of 1961-1990, the summer time ... average maximum surface temperature ... varied between 16 and 16.5 degC". While air temperature varied considerably, the mean sea surface temperature was very constant. If nutrients were not limited due to other processes, or bio-productivity was not impaired by seasonal ice-cover, it is not obvious to me how diatom productivity should related (or respond) to air-temperature, whilst sea surface temperature remains more or less constant. Once would assume that kinetics of Si utilization and accompanied isotopic fractionation by diatoms were a first-order response to the sea water temperature and not the air temperature. This is in my opinion the weakest point of this study and the authors need to drastically improve the train of thought here. It is mentioned somewhere in section 4.4 that colder (air or water?) temperatures might impair stratification of the water column hence dampening bio-productivity - is there any evidence for that?	Regarding the comment on "While air temperature varied considerably, the mean sea surface temperature was very constant", this observation is very common in open oceans. However, the Bothnian Bay can be considered as a lake system with the salinity of less than 2 psu, in which the air temperature is strongly correlated the water temperature. In the revised manuscript, we add new Fig. 8 to support this. Air and water temperatures are there shown to correlate very well in a strong linear regression. The correlation between the air and water temperature shows r^2=0.78 and 0.93 for daily measurements and average summer temperature, respectively between 1948 and 1963. Regarding the question about stratification of the water column, we add Fig. 9 where the pycnocline depth vs. surface temperature is plotted to clarify that surface temperatures are correlated with air temperature and therefore have influence on the depth of the mixed layer (above pycnocline) where diatom production mainly occurs. High surface water temperature leads to a shallow mixed layer and low surface water temperature expands the deep mixed layer. The development of the mixed layer has been shown by Wasmund et al. (Wasmund N., Nausch G., Matthäus W. 1998. Phytoplankton spring blooms in the southern Baltic Sea—spatio-temporal development and long-term trends. J. Plankton Research, 20, 6, 1099-1117) to control diatom blooms in the Baltic Sea.
Figure 6a shows the calculated f values vs. time depending on different river Si isotope compositions. From reading the manuscript it is not quite clear to me how the "measured f" values were derived - please add a brief discussion. The only river data for this area are from the Kalix River (Engstroem et al 2010), and average close to 1.1pmil. The Kalix is	We have clarified how to calculate f values by changing the text in Section 4.4: "The f values can be calculated by using δ^{30}Si values according to Eq.3 ... $\delta^{30}Si_{BSi}$ is the values shown in Fig. 4 and initial δ^{30}Si river input value used is +1.1‰ derived from measurements of an unregulated boreal river, the Kalix River, data from other rivers are not available".

unregulated and gives a constraint on the natural Si isotope composition delivered to the Bothnian Bay. In order to match their calculations to observation in figure 6a, the authors assume a shift in river Si isotope composition due to anthropogenic impact on watersheds. Although this is possible, we have however no idea what the effects of hydraulic engineering would be. Without having data for these rivers, it is difficult to say what the effect might be. Hence the assumption that the post 1950 river have higher Si isotope compositions is somewhat unjustified and in my opinion not valid. The Si isotope composition of rivers is, albeit some good research, still a very complex system and we are far from understanding the interactions between different Si-carrying pools. I'm not saying that enhanced diatom productivity due to dammed lakes is impossible, I just don't think that large scale river systems are that easy and straight forward to interpret.	We know that it is difficult to interpret river system in such a simple way and it is also a pity that we do not have water samples from other rivers to prove there is a shift. However, according to the current studies of the Bothnian Bay, there are no notable changes in its land cover including the vegetation and bedrock, which means there is no source change of Si isotope signatures. Therefore, if there is a change in the Si isotope composition, it is likely to occur during the transport to the Bothnian Bay, which in our case damming on the major rivers could be considered as the most important effect. We do not deny any other possibilities in such a complex system, here we would like to emphasize that a shift in the Si isotope values in river water is the most plausible explanation for our observations. We have tried to clarify this by changing the text in Section 5.5: "The shift coincides with a period of large scale hydroelectric and flood control projects in the major rivers draining into Bothnian Bay (Humborg et al., 2002; Humborg et al., 2006), leaving only a few rivers unregulated today. Since there is no change of bedrock (of course) and vegetation in the catchment of the Bothnian Bay, it suggests that the isotope composition of Si is constant. The shift is thus most likely to occur during the transport of DSi to the Bothnian Bay. Damming of rivers increases diatom production in the reservoirs behind the dams (Humborg et al., 2006) and leaves the remaining DSi in the river water enriched in the heavier Si isotopes, ultimately leading to the increased $\delta^{30}Si_{river\ input}$ values."
My next concern is related to the data variability; Taking into account the uncertainty of the Si isotope measurements the actual data barely vary at all. The relation between BSi content and Si isotope composition is far from being a linear correlation, and should not be addressed as such. The regression in figure 5 is rather a spurious correlation based on two distinct data point cluster. Adding error bars to figure 5 and one will immediately see that this is actually not a correlation. The authors write that the uncertainly of the calculated f values takes into account the 2SD errors of the Si isotope data. I assume the propagated uncertainties be very large, and think this should be added to the plots 6a and 7. Furthermore, the calculated temperatures in figure 6b must also have some uncertainty and it would be good to add them to the plot. Is the 5-year-moving average (dashed line) in Figure 6b based on observations from the data monitoring program or actually derived from the relationship in figure 7? Please clarify. It would be certainly very helpful to plot the data from the monitoring program against	We agree with the reviewer's comment here "BSi content and Si isotope values should not be addressed as such". After reconsidering this part, we have removed the original Fig. 5, i.e. the plot of BSi against δ^{30} Si values, because it is true that there is no obvious correlation and that this does not have any influence on our conclusion. We have done Monto Carlo analysis to examine the error in f values derived from δ^{30} Si values and found that the first quartile and third quartile for each f value derived from $\delta^{30}Si_{BSi}$ was calculated and shown in updated Fig.6, which gives us 50% f values in the range of f ± 0.05 in average. We have tried to reformulate this part in Section 5.4 as follows: "The error of the calculated f values due to the uncertainty of the measured $\delta^{30}Si_{BSi}$ is examined by Monto Carlo analysis. 10,000 random $\delta^{30}Si_{BSi}$ values were generated… producing 10,000 f values calculated by $\delta^{30}Si_{BSi}$. The first quartile and third quartile for each f value was calculated and shown in Fig.6 as error bars, which mean that 50% of f values fall in this range defined by error bars.… It should also be noted

calculated temperature data derived from the relation in figure 7. As is, it looks like the dashed line is simply the average of the monitoring program data, if so, where is the point in plotting it?	that the calculated f values are independent of the α-value, i.e. the absolute number of f values will change with the α-value but the variations will be unchanged." We have complied summer temperature data collected from 1820 to 2000. The data are plotted vs. the calculated f values in Fig. 6. Therefore, no temperature data are derived from the relationship in Fig. 7 and all of the calculated 5-year moving average values are based on the field observations.
Actually I think the authors could tremendously enhance this manuscript by adding some more modeling. One very interesting model could be a simple Si isotope transient model (box-model), for example similar to De La Rocha et al 2005, but only for the Bothnian Bay and obviously not for the whole ocean. Section 2 describes the sampling site and it already contains most data required for such a box-model approach. Such a Si cycle model could then be used to test some forcing of the diatom productivity due to variations of air temperature and the resulting Si isotope fractionation. This would be a very strong way of testing the sensitivity of the Bothnian Bay system to temperature variations, and could in my opinion add a lot of value to this study.	We are working on developing a box model using Si isotopes. However, we will write this as a separate story in another manuscript.
Other comments	
Section1: The Si isotope fractionation by diatoms is stable between 12-22 degC (De La Rocha et al. 1997), but some of the samples analysed here fall beneath that range. Are there any isotope effects that the reader must be aware of?	We are not sure of any Si isotope effects beyond this temperature range. Generally speaking, lower temperature gives high isotope fractionation. This means that in our case that the variations of the calculated f values might be larger i.e. we might underestimate the variations in diatom production because some samples are obtained below 12 ^{o}C but no samples above 22 ^{o}C.
Section 2: The decrease of DSi fluxes due to hydraulic engineering is mentioned. Later in the manuscript (section 4.3) the authors say that the BSi content increased since the 1950s, probably due to an anthropogenic enhanced input of nutrients. I assume they mean macro-nutrients, such as P and N? This is a bit contradictory and maybe one additional sentence could be added.	We have rephrased Section 5.3 as follows: "The BSi content of the sediment core is approximately 3.5% … It increases to a maximum 7.8%...This is due to increased diatom production probably caused by anthropogenic nutrient enrichment in the Baltic Sea, especially nitrogen and phosphorus.".
Section 3: What is the uncertainty of the Pb activity data? Should be added.	Added error bars in Fig. 3A show uncertainty of Pb activity.
Section 3.2: I'm aware that MilliQ-e water is supposed to be 18.2m, but some readers might not know that. MilliQ-e is not an official term for high quality deionized water, and the authors should use something along the line of "deionized water (18.2m, MilliQ-e, Milli Pore)". Please change!	We have changed 'MilliQ-e water' to deionized water.
Section 3.3: Silicon concentration data are given in mg/L, earlier in the manuscript they are given in molar units, please be consistent throughout the	We have changed all Si concentration units to mg/L.

text.	
Section 3.4: Were any other reference materials analyzed, such as Diatomite, or some USGS rock standards? There are now quite a few reference material data published and I think the authors could considerably strengthen their work by presenting standard data. In fact, it is my opinion that data-sets without standard data are not to be published.	We did measure other standard materials i.e. IRMM-18 and Big Batch. The results were presented and discussed in the publication: Sun, X., Andersson, P., Land, M., Humborg, C., and Mörth, C.-M., 2010, Stable silicon isotope analysis on nanomole quantities using MC-ICP-MS with a hexapole gas-collision cell: JAAS, 25, 156-162. We have added one sentence in Section 4.1 to make this more clear: "the internal reproducibility was tested by measuring IRMM-18 and Big Batch".
Section 4.4: Somewhere in section 4.4 the authors discuss the relation between f values and cold weather periods: "The largest amount of remaining ... corresponds to a period with very cold summer". Is that so? How can the authors know that? Please add references here!	During the summer in the Bothnian Bay, diatom production is the major reason for regulating the concentration of DSi in the water column. This has been well examined and discussed by a study using Redfield ratio changing and trend analysis (Danielsson, Papush, L., and Rahm, L., 2008, Alterations in nutrient limitations -- Scenarios of a changing Baltic Sea: Journal of Marine Systems, v. 73, p. 263-283). The relation between f values and hot/cold summers are concluded from our observations (Fig. 6). We have rephrased the text in Section 5.4 and added the reference to support as follows: "An explanation for the diatom-temperature relationship is that diatom bloom is controlled by ML (mixed layer), i.e. hot summers with high water temperature result in shallow ML and cold summers with low water temperature results in deep ML. Wasmund et al. (1998) showed that diatom blooms were triggered by the reduction in the ML depth. Water temperature is in the studied area positively correlated with air temperature (Fig.8). Fig.8A shows daily air temperature between 1948 and 1963 plotted vs. daily water temperature giving a linear correlation coefficient of R^2=0.78. The correlation is better if average summer temperatures is used, R^2=0.93 (Fig.8B). This means that the air temperature indirectly controls the ML depth and light penetration in the deep ML in cold summers are restricted, limiting diatom production (Wasmund et al., 1998). Therefore, the net diatom production in hot summers is higher than that in cold summers."
Section 4.5: "This isotope shift is also consistent with what is expected to occur ...". As I wrote above, is there any evidence for that? This section is in general rather weak and does require considerable improvement.	As we mentioned above, damming may lead to trapping and sinking of BSi behind dams which ultimately causes the reduction of the Si riverine input to the Bothnian Bay. This has been shown by Humborg, C., Pastuszak, M., Aigars, J., Siegmund, H., Mörth, C.M., and Ittekkot, V., 2006, Decreased Silica Land-sea Fluxes through Damming in the Baltic Sea Catchment-Significance of Particle Trapping and Hydrological Alterations:

	Biogeochemistry, v. 77, p. 265-281. There is of course no change in bedrock composition and vegetation in the catchment of the Bothnian Bay, indicating that the source of Si remains the same (i.e. no change in Si isotope values).
Closing remarks: In general I think that the scope of this study is actually very intriguing and of hot topic. However, the data set and the modeling is simply not convincing and fails to win me over. It also seems that only samples from one site in the Bay were samples. The implications of this work could be very important and therefore I think it would be mandatory to confirm the observed trends in other locations from within the Bay. For example, De La Rocha et al (1998) present data for a few sites from the Southern Ocean, and so does Brzezinski et al (2002) etc. I understand that sampling sea sediments is logistically quite challenging, but I also assume that sampling the shallow waters of the Bothnian Bay is less of an enterprise than say the deep Southern Ocean.	We only have one sediment core from the Bothnian Bay, so there is nothing we could do about this right now. The Bothnian Bay is relatively a small area. As a result of a relatively homogenous bedrock and vegetation around the Bothnian Bay as well as the rather 'homogenous' bottom topography, it makes us believe that the sediment core analysed is representative.
What about O-isotope data? With the diatoms being already separated, why not taking some splits to get O-isotopes analysed? This might add weight to this study.	Due to instrument and resource limitation, we cannot do O-isotope analysis for the moment.

George Swann

Comments	Answers/Actions
1) The authors suggest that air temperature is the main driver of changes in the silicon isotope record, presumably through reductions in seasonal sea-ice cover. If air temperature is the main driver, why do changes in the actual δ^{30}Si core record (Fig. 4b) not co-vary with changes in air temperature (Fig 6b)?	Yes, δ^{30}Si values does vary with changes in air temperature. We think, however, that it is better to compare variations between f values and air temperature. This is a more straightforward way to evaluate diatom production.
2) The authors state in Section 4.1 that the analytical error on their δ^{29}Si measurements is up to 0.2‰ (2σ). Since the data are then converted to δ 30Si (δ30Si = δ 29Si *1.96), we can therefore presumably conclude that the error on the reported δ 30Si data is up to 0.39‰ . Given an observed range in the δ30Si core record of c. 0.4-0.5‰ this would imply that all measurements are within analytical error of one another. Maybe I'm missing something obvious, but there is nothing else in the methodology (Section 4.1) to suggest so. In fairness the authors do state that errors for δ29Si are " better than 0.2‰ " so perhaps the authors just need to provide more detail here on what the errors are for each sample.	In Fig.4, we now have used 95% confidence intervals of δ^{29}Si values multiplied with 1.96 to get δ^{30}Si values. Seven measurements of each sample were done during analysis together with standards in between giving in total 60 measurements for a complete sample/standard cycle. The detailed measurement process was documented in Sun, X., Andersson, P., Land, M., Humborg, C., and Mörth, C.-M., 2010, Stable silicon isotope analysis on nanomole quantities using MC-ICP-MS with a hexapole gas-collision cell: JAAS, v. 25, p. 156-162. In principle, errors cannot be handled as simple multiplication since the error in this case is distributed. From the error estimation we have done

	in this way it is still obvious that several peaks statistically differ significantly from each other.
3) Have the authors looked at what diatom species are in the analysed samples? These will presumably have changed over the analysed interval in response to environmental changes and may allow the authors to further strengthened their arguments/interpretations.	We have not identified the diatom species in this sediment core. In another study, an identification of diatom species was done at each centimetre of the sediments in Gulf of Bothnian (Olli, K., Clarke, A., Danielsson, Å., Aigars, J., Conley, D.J., and Tamminen, T., 2008, Diatom stratigraphy and long-term dissolved silica concentrations in the Baltic Sea: Journal of Marine Systems, v. 73, p. 284-299). The diatom assemblages were in that study dominated by planktonic taxa, i.e. high numbers of Skeletonema spp. and Chaetoceros vegetative cells (weakly silicified taxa) indicate good preservation conditions. Pauliella taeniata (a cold-water species) and T. levanderi were the most common taxa. C. choctawhatcheeana was present in the sediments as well.

Anonymous Referee #2

Comments	Answers/Actions
Major comments	
1. It would be better to describe advantages of this method (Si isotope analyses) over the previous studies. For example, in the previous studies, lacustrine BSi flux was also used to reconstruct air temperature. Is the estimate of air temperature from the Si isotope more accurate than that from BSi flux?	We have added a new part to discuss the advantages and limitations of this method in the revised text as follows: "This method can also be used for other diatom dominated closed aquatic systems, such as lakes. In comparison to other BSi-based method for reconstructing air temperature, the advantage of this method would be to reconstruct relative diatom production (f values) and temperature at the same time in a high-resolution and more accurate way. The measured sedimentary BSi is a balance of diatom production and dissolution. High diatom production induced by high temperature would lead to high diatom dissolution, which might increase uncertainty when reconstructing the temperature using sedimentary BSi. The Si isotope-based method is to reconstruct relative diatom production by calculating how much DSi is transformed into BSi since the Si isotope values of diatoms does not change with its dissolution and sedimentation. Certainly, this Si isotope-based method also has some limitations. It is really dependent on the isotope fractionation factor (α in Eq.3), which might differ in different areas. In another word, we need a reliable α before applying this method. We would like to show in this study how this idea and method could be applied although we were not able to measure the exact α-value for the Bothnian Bay. Meanwhile, the validation using the temperature data between 1980 and 2000

	indicates that the α-value of 0.9989 used in our study could represent the Si isotope fractionation factor for the Bothnian Bay. For other areas, the form of Eq.4 might change. In addition, this method is valid as long as the Rayleigh model for a closed system could be applied…”
2. I think that the Bothnia Bay would be a suitable place to examine this method and ideas. It would be useful to discuss the application of this method to other oceans. In Abstract, you mentioned that this method can be applied to other diatom dominated aquatic systems. But, I couldn't find such a discussion in the text.	We have added the discussion of the application of this method in the revised text combined with the discussion of Comment 1 above as follows: “This method can also be used for other diatom dominated closed aquatic systems, such as lakes. …… Caution is needed when applying this method because changes in nutrient concentrations and other environmental conditions may also alter diatom Si uptake, e.g. mixing of water bodies with different isotope compositions . Another thing is that this area also shows seasonality, i.e. temperature controlled ecological systems, especially remote area, such as Arctic and Antarctic region and Alpine area, where diatom production is highly dependent on light availability.”
3. You assumed that the increase of air temperature (between May - September) melted the sea ice, resulting in the enhanced light availability in the water column. Therefore, the diatom production could increase. But I think winter cooling (or air temperature during winter) and annual wind patterns, as well as air temperature during summer, could contribute to summer sea ice distributions. Probably, we need evidence to agree with your assumption. In addition, you also mentioned that the cold temperatures likely limited diatom production by preventing the formation of stratified water column. Please show a threshold value of the stratification or air temperature that limited the diatom production. Which is the determinant factor for the diatom production, light availability or water column stratification?	We add Fig. 8 to show that the summer air and water temperatures are highly correlated and in the range of 3 – 20 ^{o}C. This means that most of the sea ice can melt during summer, which indicates in our study, that the influence of sea ice could be neglected. Meanwhile, light penetration is the most important factor affecting diatom production in summers of the Bothnian Bay. We think that the stratification influence the light penetration in the water column, therefore we add Fig. 9 plotting pycnocline depth against surface temperature to clarify that surface temperatures dominated by air temperature have influence on the depth of the mixed layer (above pycnocline) where diatom production mainly occurs. High surface temperatures lead to shallow mixed layer, where light can fully penetrate. In contrast, low surface temperature increases the deep mixed layer which could cause light limitation to diatom production. This has been shown by Wasmund N., Nausch G., Matthäus W. 1998. Phytoplankton spring blooms in the southern Baltic Sea—spatio-temporal development and long-term trends. J. Plankton Research, 20, 6, 1099-1117. We have tried to discuss in our revised text in Section 5.4 as follows: “An explanation for the diatom-temperature relationship is that diatom bloom is controlled by the mixed layer (ML)…. Wasmund et al. (1998) showed that diatom blooms were triggered by the reduction in the ML depth. Water temperature is in the studied area positively correlated with air temperature (Fig.8)….. This means that the air temperature indirectly controls the ML depth and that light penetration in the deep ML in cold summers is restricted, thus

	limiting diatom production……” In summary, the determinant factor for diatom production is light penetration which is strongly affected by air/water temperature which influences the mixed layer depth.
4. The value of _30Si increased after 1950 (Fig. 4b), which was explained by the increased diatom production caused by anthropogenic nutrient enrichment and the increase of _30Si river input. Please add a quantitative discussion of their contributions to the increase of _30Si.	We do not have enough data to conclude about this, especially the Si isotope values in the rivers draining into the Bothnian Bay. Although it is our hypothesis that increased δ^{30}Si values after 1950 is caused by increased diatom production behind dams, which ultimately increase the δ^{30}Si values in the river input to the Bothnian Bay, we have reasons to say that “the shift of Si isotope values coincides with a period of damming of the major rivers, leaving only a few rivers unregulated today. Meanwhile, no change of bedrock and vegetation in the catchment of the Bothnian Bay have occurred suggests that the isotope composition of ‘initial’ Si is constant. Hence, the shift is most likely to occur during the transport of DSi to the Bothnian Bay. Damming of rivers increases diatom production in the reservoirs behind the dams and leave the remaining DSi in the river water enriched in the heavier Si isotopes, ultimately leading to the increased $\delta_{30}Si_{river\ input}$ values.” To validate our hypothesis, more river samples should be taken and Si isotope values should be measured in the future.
Other comments	
Page 3773, Line11: What are the ongoing environmental changes?	In comparison to paleoclimatic and paleoceanographic studies, ongoing environmental changes here means those processes which are affected and altered by the current change of environment conditions, such as diatom-inferred Si untilization in the oceans, the formation of modern sediments tracked by opal preservation, eutrophication caused by excessive nutrient emission to water bodies and the change of global Si cycle etc.. This also includes the changes occurring recently and having influence on the ecosystem from now to the future, such as in our study, damming on rivers which is possibly causing the increased Si isotope values in the Bothnian Bay and ultimately leading to the ecological shift in the system.
Page 3773, Line 17: Please define DSi here.	We have defined DSi in the revised text.
Page 3777, Line 18: Not 4M Cl, but 4M HCl.	We have corrected HCl in the revised text.
Page 3778, Line 26: I can’t understand the reason why changes in detritus sedimentation rate were minor in comparison to diatom flux. Can you show the data that the diatom flux was larger than the detritus sedimentation rate?	The field data of diatom flux in the Bothnian Bay is still lacking. However, here we mean that the nearly linear relation shown in Fig.3 suggests minor variability of the sedimentation rate in the past 200 years. This implies that the sedimentation rate does not control the variability of BSi content in sediments. In particular, there is a large increase of BSi content in the past 100 years in the Bothnian Bay. Meanwhile, Si

	isotope values of diatoms do not vary during sedimentation, hence, the variability of sedimentation rate will not cause large uncertainty to our study.
Page 3779, Line 11: Do you mean low nutrient at the bottom? That is, low remineralisation at the bottom?	Low nutrient concentrations lead to low remineralisation rate at the water bottom, which could help the preservation of diatoms in the sediments. We have rephrased this sentence to make it more clear as follows "Although no diatom dissolution rate estimates exists for the Bothnian Bay, the excellent preservation of diatoms and low mineralization rate caused by low nutrient concentrations at the water bottom combined with a shallow water depth of 112 m and rapid sedimentation rate imply that dissolution of BSi are less than 20%."
Page 3780, Lines 1, 4, 9: Not Eq. 2, but Eq. 3.	We have corrected equation numbers in the revised text.
Page 3782, Lines 5, 10, 12, 13: Not Eq. 3, but Eq. 4.	We have corrected equation numbers in the revised text.
Page 3782, Line 9: Eq. (4) was derived from the data between 1896 and 1955. However, you compared the f values calculated from Eq. (4) with that obtained from the field observations between 1980 and 2000. Can Eq. (4) be extended to the years after 1955?	We are sorry for the confusion with the equations and data. In the revised version of the manuscript, we retrieve the summer temperature data from 1820 to 2000, they are plotted against the calculated f values in updated Fig. 6. Now Eq.4 is derived using data from 1824 to 1955 from the relation between f values and temperature, so there is, although a shift of Si isotope values in river input, it does not affect this f - temperature relation, i.e. this relationship between f and temperature is independent on Si isotope values. Eq.4 is supposed to be valid for all the years as long as we have f values. Si isotope values are only used to reconstruct diatom production (represented by f values in this study).

Necessary revised figures

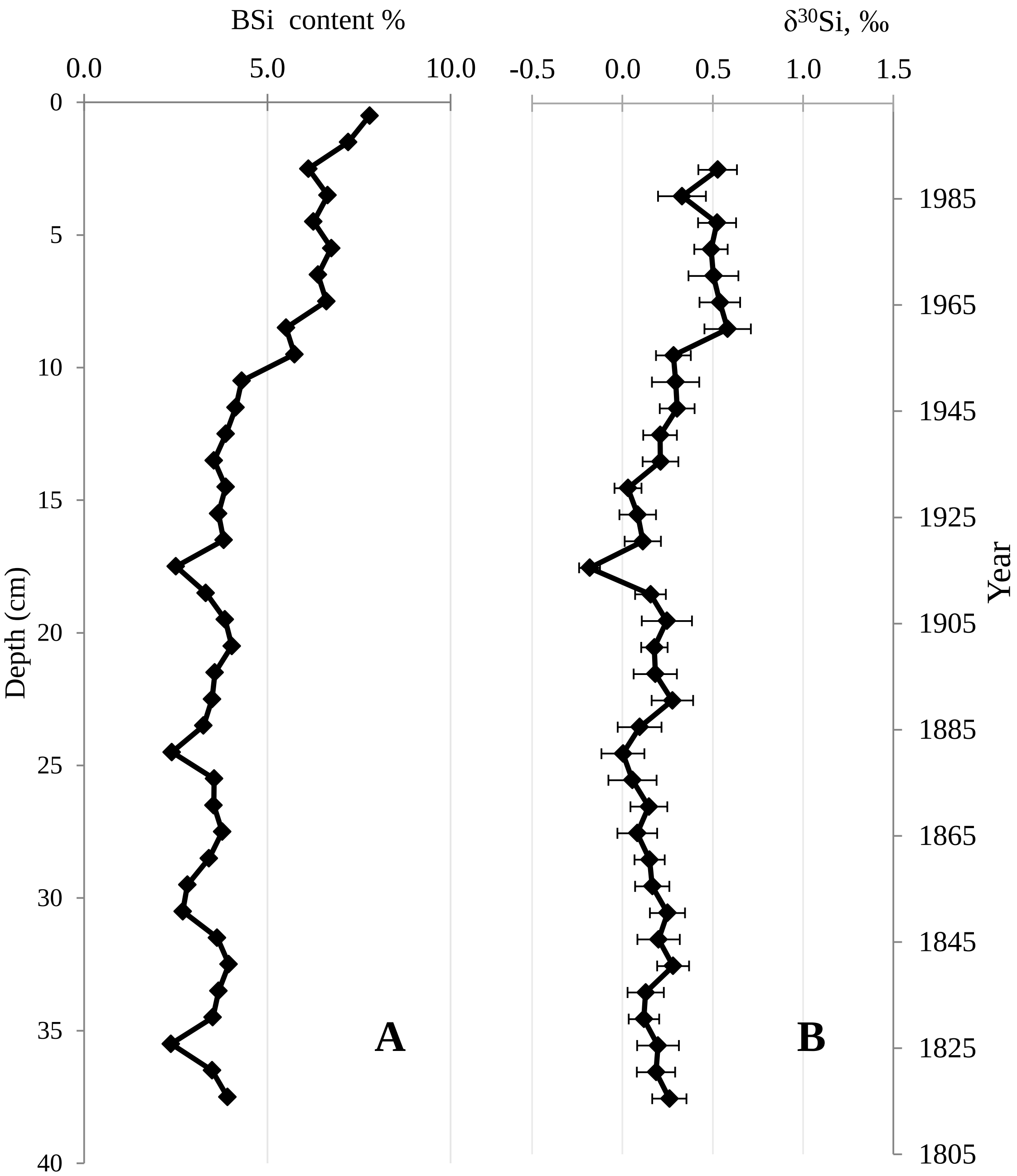

Fig 4 Sediment profiles of Bothnian Bay sediments, A. BSi content (%); B. δ^{30}Si in BSi. The error bars in B represent 95% confidence interval of δ^{29}Si multiplying 1.96 for δ^{30}Si.

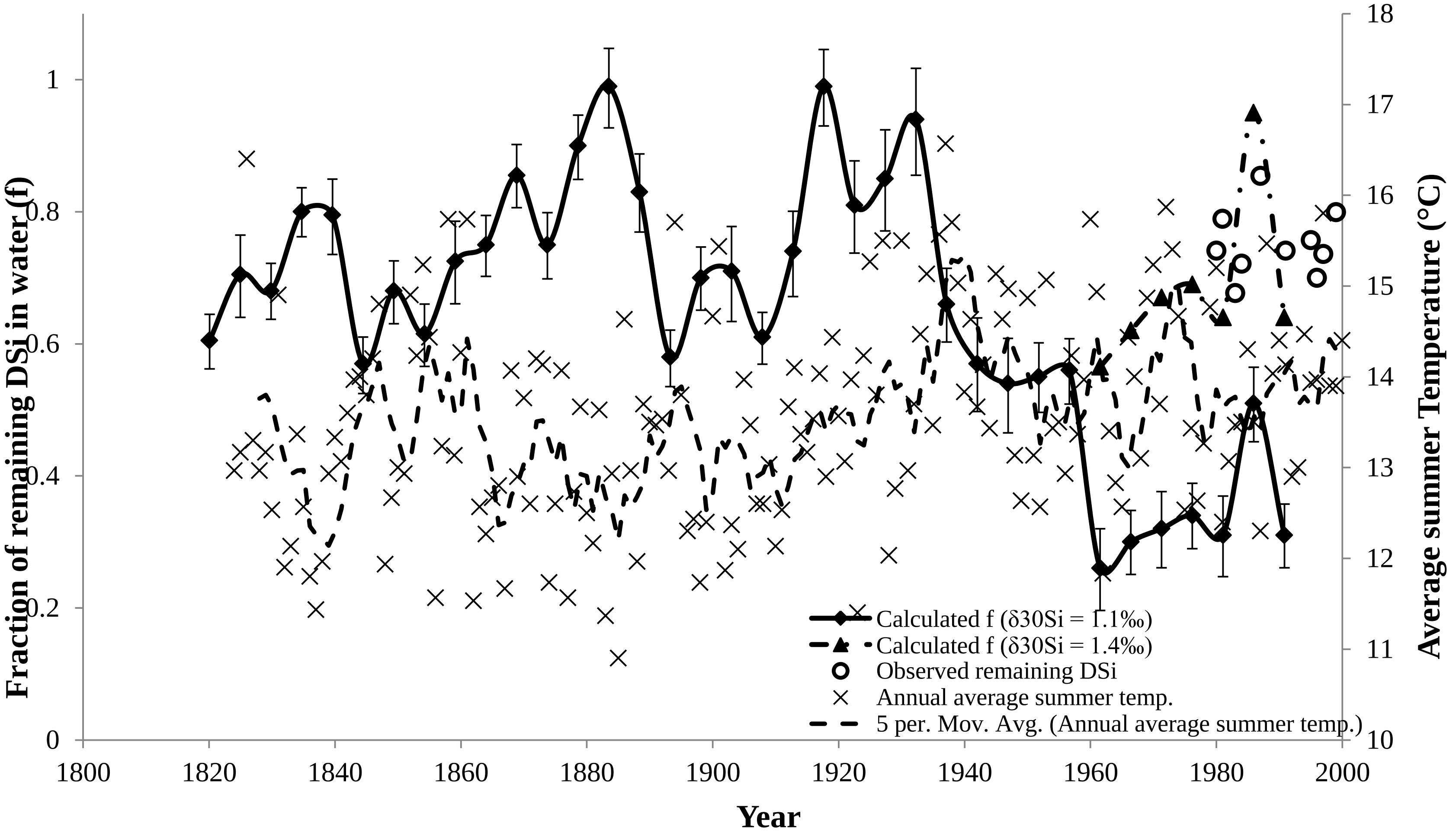

Fig.6 Fraction of the remaining DSi (f) in the water column of the Bothnian Bay calculated using a Rayleigh distillation equation (Eq.3) and f values derived from DSi observations during the summers between 1980 and 2000 are plotted together with average summer air temperature through years. The inferior and superior error bars on f values are the first quartile and third quartile for each f value. The δ^{30}Si river input value is -1.1‰ from the pristine boreal Kalix River (Engström et al., 2010) before 1950. For the time period after 1950 the data are adjusted to +1.4‰. Each point is measured and calculated from 1 cm sediment slices, which correspond to ca. 5 years of deposition

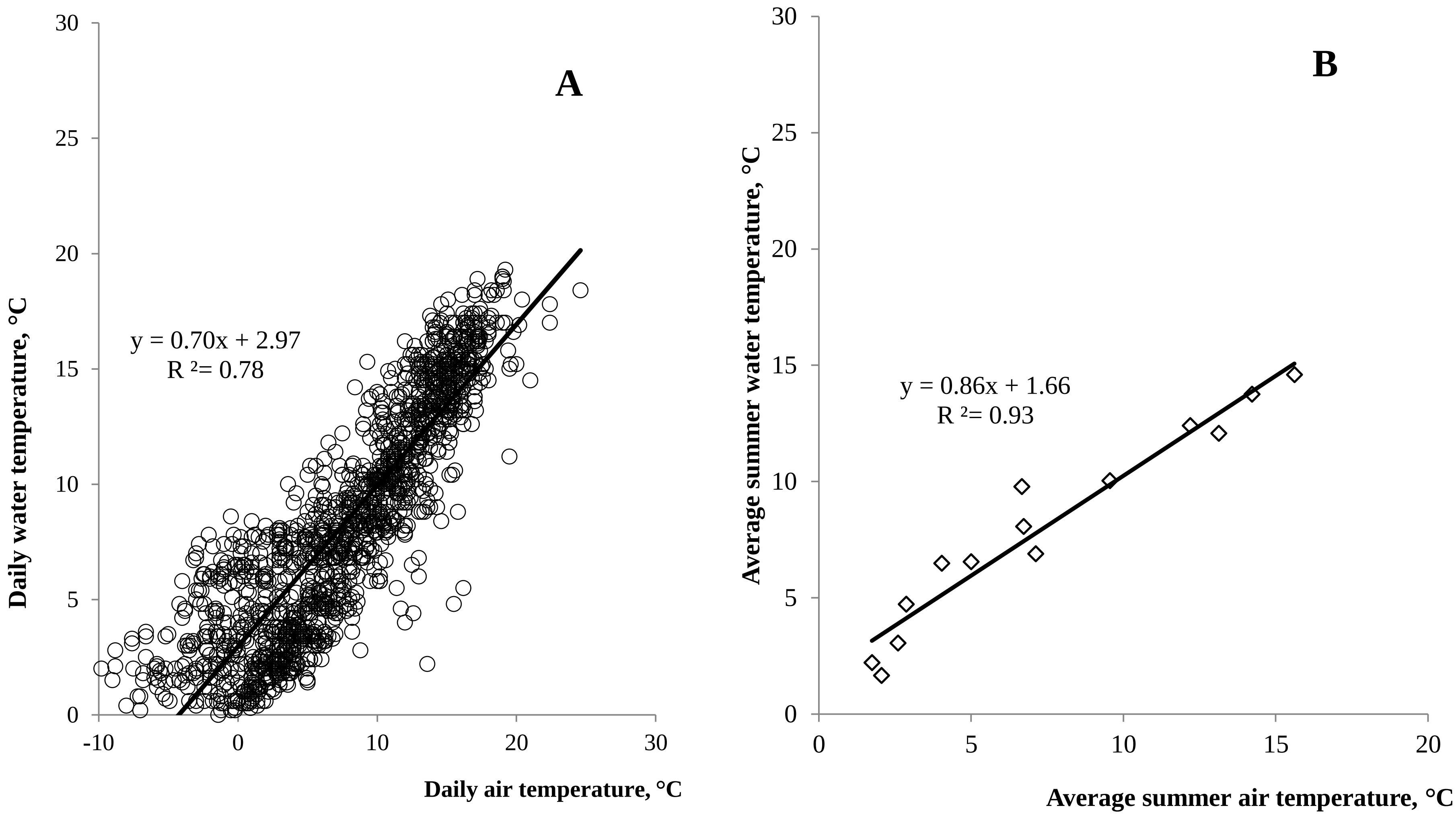

Fig. 8A Plot of daily air temperature against daily water temperature between 1948 and 1963. B. Plot of average summer air temperature against average summer water temperature between 1948 and 1963. The linear regression lines are shown for both figures.

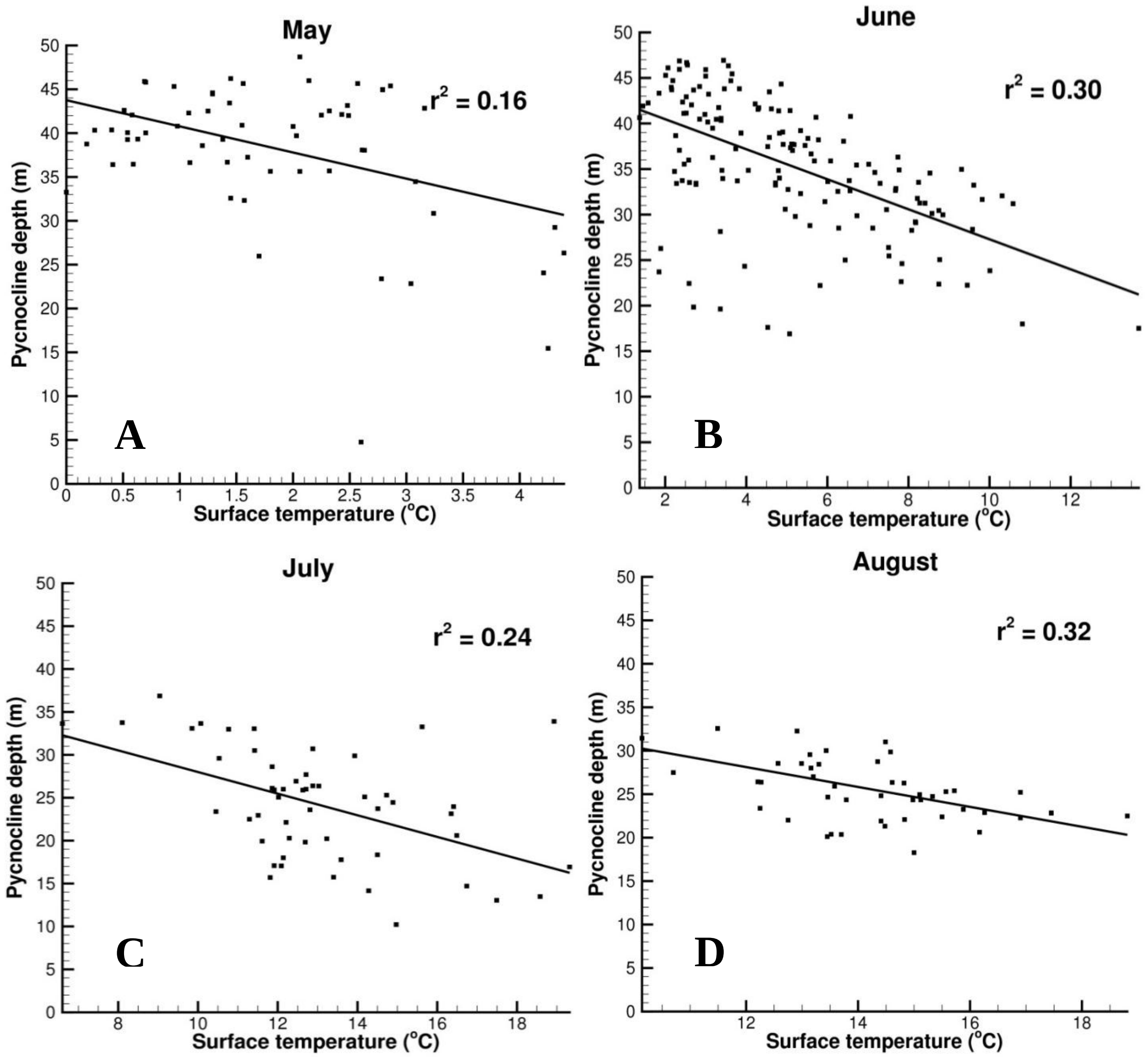

Fig. 9 Plot of pycnocline depth against water surface temperature for May (A), June (B), July (C) and August (D). An F-test is done with p value of better than 8.5×10^{-7} for the four figures, indicating there is equal variance between pycnocline depth and surface temperature.