We thank the reviewers for their constructive criticism on our manuscript. We have responded to all comments made by the reviewers as the text below shows. The answers are structured as follows: (1) comment of the referee in **black**, (2) answer in **red** and (3) rewritten text parts in **blue**.

Anonymous Referee #2

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In the reviewed paper authors propose several hypotheses, assumptions, simplifications and finally - conclusions.

Generally, the paper deals with three distinct issues: 1) direct observation of transient tracers (CFC-12 and SF6) distribution in summer 2012; 2) use of IG-TTD method for comparison of theoretically calculated and observed tracers ages and for anthropogenic carbon calculation based only on CFC-12; 3) transport of DIC and previously calculated anthropogenic offset in the upper/intermediate layer across the Fram Strait.

While the first part is rather trivial, the second is the most developed/discussed, the third is controversial and only briefly analyzed in the manuscript. Nevertheless, presentation of any new observations concerning the ocean uptake capability for anthropogenic gases is valuable for the scientific community, especially considering the fact that the ice-covered polar regions are still under-sampled. The theories introduced in the manuscript, even though arguable, are interesting. Thus, in my opinion, the submitted paper should be published after some corrections and additional explanations – this means a major revisions.

Below are some specific comments which may help re-think the presented material:

Title

The title is misleading – it suggests broader look, as well as longer time perspective.

Title changed to "Transient tracer distribution in the Fram Strait in 2012 and inferred anthropogenic carbon content and transports"

Abstract

Page 2190 - Current velocity measurements along the same section - it should be mentioned the measurements were performed in the previous years – mean flow.

We now make it clear in that sentence that the mean velocities from 2002 to 2010 are used.

"Mean current velocity measurements along the same section from 2002-2010 were used to estimate the net flux of DIC and anthropogenic carbon by the boundary currents through the Fram Strait above 840m."

Introduction

Page 2191 - The elevated heat flux of warm Atlantic Water into the Arctic Ocean – where and when? Higher temperature or volume of AW?

Text added.

"The temperature of the Atlantic Water flowing into the Arctic Ocean in the Fram Strait has warmed since 1997 (Beszczynska-Möller et al., 2012), which increased the heat flux into the Arctic."

Page 2192 - a short meridional section along the fast ice edge in 2012 – how valuable is this section for the overall analysis and discussion? The data from several (1-2?) stations appear only in Figure 2.

This section shows no differences in the horizontal distribution so that the zonal range of the fast ice section is already described by the corresponding longitude range of the zonal section. This particular section is not used for the analysis but nevertheless we introduce the full data set at this point. Additional text added.

Section 2.1

"The meridional section along the fast ice edge was only sampled for CFC-12 and SF_6 which shows no differences in the horizontal tracer distributions compared to the corresponding longitude range of the zonal section. Therefore we have only used the zonal section for the following analysis."

Data and Methods

Water transport data

Page 2194 - gross assumption applied by authors is that the mean velocity field calculated for the years 2002-2010 represents particular situation in summer 2012. This is based on the authors' statement of small interannual changes and no trend in the flow. Previous studies indicated a non-steady situation, which is also confirmed by a recent work (Hansen et al., 2015). This study is based on a combination of the in-situ data (moorings and CTD stations) and satellite altimetry, and it shows a distinct trend in volume, heat and salt transports in the AW layer across the Faroe Shetland Channel (FSC) – the main AW entrance into the Nordic Seas. The mentioned paper also points out high interannual and seasonal variabilities. Since the AW transport across the FSC has increased in the recent period (9_8% in the last 2 decades), transport through the Fram Strait/BSO is also most likely to increase. Even though it is not clear how this additional volume has been distributed (FS, BSO and recirculation in the Norwegian and Greenland Seas), it is rather hard to claim that there is no trend in fluxes in the Fram Strait at all.

We thank the reviewer for his/her points here, but we disagree with the conclusions. While there may have been a trend at the Faroe Bank Channel (at the other side of the Nordic Seas), no statistically significant interannual trends in the volume transport through the Fram Strait between 1997 and 2010 were observed (Beszczynska-Möller et al, 2012). These observations are based on the extensive mooring data set in the Fram Strait and in our opinion they are hence more insightful about what is happening in Fram Strait than the observations in the Faroe Bank Channel and the speculations about how those changes may or may not be then divided up between the Barents Sea inflow, the Fram Strait inflow and the recirculations in the Fram Strait

and the Nordic Seas. There has, however, been a significant change in the temperature of the inflowing Atlantic Water in the Fram Strait.
One aim of our manuscript was to estimate transports of anthropogenic carbon through the Fram Strait. Transports are the product of concentrations times velocities integrated over an area. The assumptions which we make (and which in our opinion is not "gross") is that the trace gas concentrations change relatively slowly between years with no significant seasonal changes. Hence, we can take the concentrations from summer 2012 to be informative about other seasons and years within some range from 2012. On the other hand, we know that velocities change strongly between seasons, but not significantly between years. It follows that the measured (2002-2010) long term average volume transport is representative of the volume transport through Fram Strait in the late 2000s / early 2010s. Likewise, the measured Cant concentrations in summer 2012 are representative for the Cant concentrations in the late 2000s / early 2010s. The product of the two is then our estimate of the Cant transport through the Fram Strait in the late 2000s / early 2010s.

Section 2.4
"We then proceed to estimate transports of anthropogenic carbon through Fram Strait. Transports are the product of concentrations times velocities integrated over an area. We assume that the trace gas concentrations change relatively slowly between years and that there are no significant seasonal changes. Hence, we can take the concentrations from summer 2012 to be informative about other seasons and years within some range from 2012. On the other hand, it is known that velocities change strongly between seasons (and on shorter time scales), but on average not significantly between years in the Fram Strait (Beszczynska-Möller et al, 2012). It follows that the measured (2002-2010) long term average volume transport is representative of the volume transport through Fram Strait in the late 2000s/early 2010s. Likewise, the measured Cant concentrations in summer 2012 are representative for the Cant concentrations in the late 2000s/early 2010s. The product of the two is then our estimate of the Cant transport through the Fram Strait in the late 2000s/early 2010s."

The second assumption states that the net transport beneath the upper/intermediate layer (the depth of the FBC sill being a part of the Greenland-Scotland-Ridge is 840 not 750 m) is equal to zero. Previous studies take into consideration various net values in the deep flow in the Fram Strait - from net southward transport (Schlichtholz and Houssais, 1999; Beszczynska-Möller et al., 2012, Marnela et al., 2013) to balanced exchange (Tsubouchi et al., 2012; Von Appen et al., 2015). The authors also point out that the moored instruments at the 78°50'N section do not resolve the mesoscale features and the local bathymetry very well.

We agree with these points raised by the reviewer, but both tracers show a homogenous zonal distribution below 1500 m so that any net flux would not change the inventory of anthropogenic carbon below this depth. Net fluxes in the depth range between 840 m and 1500 m might indeed contribute to either the Arctic or the Nordic Seas reservoir but this is still an enclosed basin-basin interaction.

Section 2.2
"The estimate of the volume transport across the Fram Strait below 840m from the moorings is more complicated. The method of Beszczynska-Möller et al. (2012) which was developed to study the fluxes in the West Spitsbergen Current predicts a

net southward transport of 3,2Sv below 840m. This is unrealistic given that there are no connections between the Nordic Seas and the Arctic Ocean below the sill depth of the Greenland-Scotland Ridge (840m) other than the Fram Strait. No vertical displacements of isopycnals in these two basins are observed that would suggest a non-zero net transport across the Fram Strait below 840m (von Appen et al.,2015a). The large net transport inferred by Beszczynska- Möller et al. (2012) is due to the insufficient horizontal resolution of the mooring array to explecitly resolve the westward flow of the recirculation and the mesoscale eddies. For these reasons we assume a net flux of 0Sv across the Fram Strait for the deep waters below 840m."

Another explanation of the variable southward deep flow through the Fram Strait is discussed in connection to extreme air-sea exchange in the Barents Sea in strong winters (Moat et al., 2014). This study underlines the importance of the variable surface conditions in only one of many marginal seas affecting the deep circulation in the Fram Strait.

Moat et al (2014) discuss velocity anomalies in Fram Strait of less than 0.5cm/s as a result of processes in the Barents Sea. Those are tiny numbers compared to the total flows through the Fram Strait (both in the upper and lower water columns). Therefore, the differences between years resulting from the processes discussed there are small compared to the long-term average volume transports. Furthermore, it is not clear how those processes should be integrated into our analysis where the snapshot of measured Cant has to be interpreted on a larger time horizon to be insightful.

TTD method

Page 2195 – The D/G ratio equal to 1 seems to be used in many water mass productive regions, but is it best for the Fram Strait (strong advection)? In fact, the Section 3.3 answers this question.

It can be expected that the boundary currents passing the Fram Strait show higher mean velocities and are probably better described by a higher advective share (i.e a lower Delta/Gamma ratio) at this particular point. However, this only accounts for the narrow Fram Strait. In contrast, the TTD model describes constant mixing processes affecting the water mass from its source, i.e. where the water parcel lost contact with the atmosphere, to the point of measurement which is the Fram Strait section in our case. The water mass characteristics are not based on the particular measurement region but are rather constantly affected along the flow pathway, e.g. in the Nordic Seas or Arctic Ocean. It can be discussed if the assumption on constant mixing parameters (IG-TTD) should be replaced by a more variable / dynamic model with changing mixing conditions along the flow pathway. To our knowledge there are no such models available.

Section 3.3

"Based on the raw field data, and on assumptions implemented in the IG-TTD (like constant mixing processes along the flow pathway as well as constant saturation of the gases at the surface before entering deeper layers), the IG-TTD or linear combinations of the IG-TTD can only partly describe the ventilation pattern of water masses in the Fram Strait."

Page 2195 - similarly as for the CFCs input functions - it was recently described (Fang et al., 2014) that around the year 2000 there was a reversal in the global SF6 emission trend, from decreasing to increasing, which was probably caused by increasing emissions in the East Asian countries. This additional amount shall be detectable in the AO surface water through the Pacific inflow.

The atmospheric SF_6 concentration in the northern/southern hemisphere as well as the global mean is still increasing and never showed a decreasing trend such as it can be observed for the CFCs. See https://agage.mit.edu/ for more information. The reason for this is the long atmospheric life-time of SF_6 and the dispersive nature of the release to the atmosphere that tend to dampen annual variations in emission. The ocean tracer application of SF_6 is only dependent on the atmospheric concentration, which is well known.

Furthermore, the changing sea ice cover of the Arctic Ocean needs to be mentioned as a potential source of anthropogenic gases content in the Polar Water of the Pacific origin – a recent study (Ballinger and Sheridan, 2015) describes changes in the western Arctic freeze-up pattern suggesting the changing ocean–atmosphere heat exchanges connected with prolonged melt period as a cause. This is also most likely to apply to the Siberian shelf seas.

The gas-exchange in the Arctic Ocean is a very complex system since it involves melting and freezing processes, different types of ice cover, ice thickness etc. which is rarely described in the literature. Note that we provide an example of the model output of Shao et el. 2013 which show the extreme variability of tracer surface saturations in the Arctic Ocean in different regions. This model includes assumptions on ice cover but the real conditions probably highly deviate from the model output as we have shown for observed data in the Southern Ocean. There is a great potential for future investigations but there are no corresponding constraints available which can be used for our analysis. It is also not clear to what extent CO_2 and CFCs can equilibrate through ice cover; it is likely that the dis-equilibrium of CO_2 is larger than for CFCs due to the longer equilibrium time. This is certainly a source of uncertainty.

Section 3.6

"Furthermore it is unclear to what extent the time period and type of sea ice coverage as well as the sea ice formation and melting processes bias the pCO_2 and tracer saturations at high latitudes."

Results and discussion

Water masses in Fram Strait

Page 2197 – "Note that this water mass classification is not based on an optimum multi parameter analysis and only serves as an indication for this specific purpose" – what is the reason for this explanation? Would OMP analysis provide more accurate classification? If yes, why not using it with so many data collected? This would be an additional work but perhaps a little more certainty would be beneficial for this paper. If not, consider this sentence redundant.

For our purpose the clear definitions of water masses are sufficient to show the specific tracer characteristics of the different water masses. An OMP would provide the water type mixing ratios of different source regions which would be indeed interesting but these information would not improve the results of our analysis. Sentence removed.

Page 2197 – "the warm Polar Surface Water, defined by a potential temperature (Θ) >0, which comprises sea ice melt water due to interaction with warm Atlantic Water" – and due to solar radiation because it is summer.

Text added.

Section 3.1
"…,which comprises sea ice melt water due to interaction with warm Atlantic Water and due to solar radiation;…"

Page 2197 – "Return Atlantic Water which derives from sinking Atlantic Water due to cooling in the Arctic Ocean" – in the applied classification (Rudels, 2005) the RAWoriginates in the West Spitsbergen Current – it is the water that recirculates in the northern Greenland Sea, not the water making a long loop in the Arctic Ocean and coming back – this one is called the Arctic Atlantic Water (AAW). This is also inconsistent in Section 2.2 (Page 2194).

There is some inconsistency about the abbreviation of the Arctic Atlantic Water (AAW) which is also called the Return Atlantic Water (RAAW) but should not be mixed up with the expression of Recirculating Atlantic Water (RAW). We removed the expression "Return Atlantic Water – RAAW" from the manuscript and stick to Arctic Atlantic Water for this water mass.

Page 2197 – "the deep water masses are upper Polar Deep Water (uPDW), Canadian Basin Deep Water (CBDW) and Eurasian Basin Deep Water (EBDW) and the Nordic Seas Deep Water" – does the classification from 2005 still applies to these water masses? Are they within the range? The _S diagram would be helpful.

Von Appen et al (2015, Deep Sea Research) presents a TS diagram of Eurasian Basin Deep Water (EBDW) and Greenland Sea Deep Water (GSDW) and it shows that both water masses have warmed over the past two decades. By using a constant water mass definition (where the temperatures are not allowed to change over time), Langehaug and Falck (2012, Progress in Oceanography) incorrectly concluded that GSDW had disappeared from the deep Fram Strait. Hence, we do not distinguish between GSDW and EBDW here, but rather use a broader definition for those deep water masses. Such an analysis has not been done for the other deep water masses and we think that the current manuscript would not be the appropriate location to do so.

Transient tracer and DIC distributions

Page 2199 – "Both tracer maxima probably correspond to extensive ventilation events" – when and where? Obviously, this is more like guessing but indicate how little we still know about regions/periods favorable for the Greenland Sea convection.

We do not know where, when and how exactly this ventilation event occurred but we can see the signal in the tracer data. Text rewritten.

Section 3.2
"Both tracer maxima probably correspond to recently ventilated water which mainly affected the Arctic Intermediate Water and partly the Atlantic Water in the transition zone of both water masses. The Arctic Intermediate Water in the Fram Strait thus consists of recently ventilated areas and less ventilated areas which is also indicated by the large range of transient tracer concentrations."

Page 2199 – "0.2ppt of SF6" – is it not too close to the method accuracy?

This partial pressure of SF_6 is close to the detection limit of the used analytical system (0.1 fmol/kg) but not to the methods accuracy of ~ 0.06 fmol/kg.

Page 2200 – "two branches of tracer age relationships" – it is a misfortunate expression, since branches are associated with the water pathways. Perhaps "sets" would be better.

We now use "set" instead of branch for the tracer age structures.

Page 2200 – "show a transition to the upper branch" – perhaps it is better to say that two sets merge or have an intersection area.

Text rewritten.

Section 3.3
"Note that the Arctic Atlantic Water and upper Polar Deep Water merge with the upper set for a SF_6 tracer age larger than about 25 years."

Page 2200 – "However, the upper branch does not correspond to the unity ratio and, moreover, it is outside the validity area of the IG-TTD" – does this mean that only the results below 20 years from "the lower branch" can really be used in the validation process? Well, yes, the answer is on the next page.

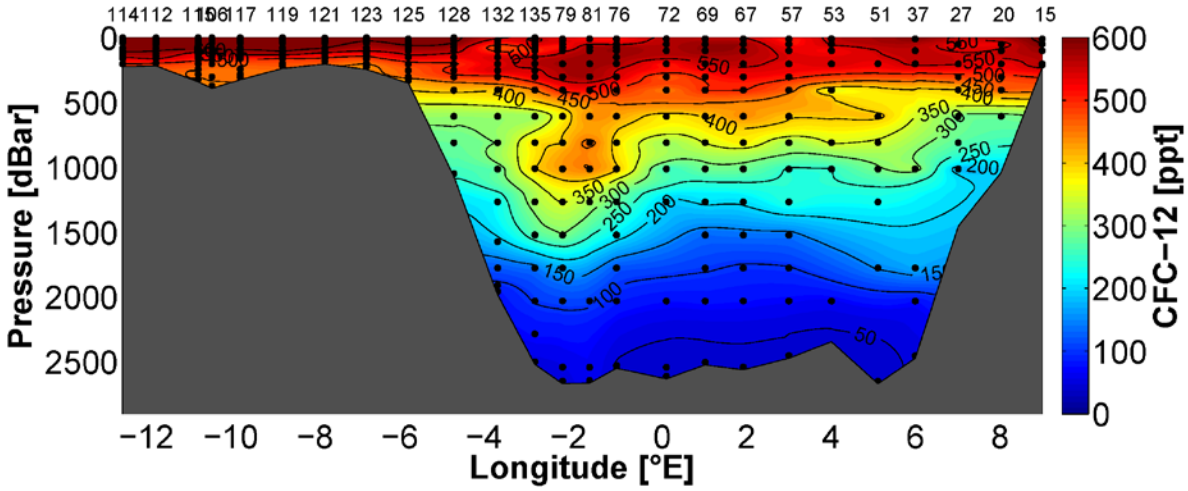
The model is only valid for the minor part of the tracer data when applied as measured. We explain in detail that saturation effects are possible limiting factors of the IG-TTD and not complex ventilation pattern as usually suggested in other literature. A saturation correction of the tracer data can enable the application of the IG-TTD at high latitudes.

Section 3.3
"Nevertheless, by comparing the shape of the two field data sets with the shape of the black line in Fig. 5, it is noted that both sets show similar characteristics as the unity ratio or, generally, as IG-TTD based tracer age relationships. This opens up the possibility to use the IG-TTD the other way around,…"

Saturations and excess SF6

Page 2202 – “These new boundary conditions are then applied to the measured tracer concentrations and the IG-TTD” – it would be good to see these corrected data.

The offset does not change the distribution pattern. The figure below shows the saturation corrected CFC-12 section. If necessary we could include this figure into the manuscript but we don’t think that this is a must-have for the manuscript.



Page 2202 – “The SF6 excess is estimated using the corrected CFC-12 concentrations and the IG-TTD (Δ/Γ = 1.0)” – I still think that this ratio may be too low.

See explanation above

Page 2203 – “This indicates that probably an additional source of excess SF6 exists” – try to find an answer in references, it must be connected with the AW inflow.

We discuss the only two sources of excess SF_6 that we can think of; from the tracer release experiment and from supersaturation from bubbles injected to the water. We cannot think of any other source of SF_6.

Page 2203 – “the generally elevated tracer concentrations of CFC-12 and SF6 in the same area” – this contradicts with the conclusion from the Southern Ocean experiment on solubility.

No it does not. Both tracer distributions show generally elevated concentrations, i.e. indicate that this water parcel was recently ventilated by both tracers and not only by SF_6. The concentration ratio between both tracers can then be explained by bubble effects.

Generally, the theory of SF6 excess source for is interesting, yet perhaps more study on gases solubility would be required. The previous paper (Alvarez and Gourcuff, 2010) indicates the difference between Cant and CFCs solubility affecting the gases concentration and transports. Could it be the case?

This is two different problems that the reviewer point out here: 1) difference in saturation between the transient tracers SF_6 and CFC12 that makes it more difficult to characterize the transit time distribution, and 2) the difference between the solubility of anthropogenic carbon and transient tracers. It is well known that the equilibrium process for CO_2 (i.e. anthropogenic carbon) is about 10 times slower than for CFCs which leads to issues in areas where the water mass in in contact with the atmosphere for a short time only, so that an uptake of transient tracers will be accompanied by a small uptake of anthropogenic carbon. We have focused our discussion here on point 1, the potential difference in solubility of SF_6 and CFC12 that are commonly assumed to be close to equal. This is thus a new aspect, and not directly related to the discussion in Alvarez and Gourcuff (2010).

Anthropogenic carbon and mean age

Page 2204 – “show the highest mean current velocities in Fram Strait (see Sect. 3.7 below)” – there is no information about the mean currents velocity in Section 3.7 and nowhere in the manuscript (only transports).

Thanks for pointing this out. We removed the reference from Section 3.7 and substituted it by a reference from Beszczynska-Möller et al (2012).

Sensitivities on anthropogenic carbon

Page 2207 – “The mean flux of deep water layers below 750m was taken to be 0Sv and therefore not considered for this estimate” – this assumption means that only the upper/intermediate transport is considered in that manuscript, not the whole FS.

This is true. We now mention this explicitly in the abstract and at the end of the introduction. The reason for this is two-fold: 1) we are not able to estimate a net transport based on the mooring data that we have, and 2) the zonal distribution of Cant is very similar at deeper depths so that a net north/south transport would not mean much in terms of Cant transport.

Section 3.7

“Furthermore, any net flux below 1500m would not change the anthropogenic carbon inventory of the Nordic Seas or the Arctic Ocean due to the homogeneous distribution of anthropogenic carbon at these depths. The depth range between 840m and 1500m might contribute to either the Arctic or the Nordic Seas reservoir but it is still an enclosed basin-basin interaction.”

Page 2207 – “we cannot with great confidence decide whether more anthropogenic carbon is transported into or out of the Arctic region through the Fram Strait” – this is the weak point of the manuscript, though perhaps it could lead to some additional studies (better sampling coverage and current measurements).

We agree that it is disappointing that we are not able to provide a clear consensus of the net Cant transport. Although there are difficulties in accurately determine the Cant concentration (that we also discuss in the manuscript), the by far largest source of uncertainty is the transport estimates. This uncertainty in transport estimates is known by the operators of the current array and a new design of the array has been implemented to remedy this weakness. In that respect, the high uncertainty in transport reported here has contributed to the need of the re-design.

Uncertainties

Page 2207 – “is supposed to be limited by complex water mass mixing and ventilation patterns” – this contradicts the theory of small mixing impact on the differences in the tracers age relationships.

Text rewritten

Section 3.8
“We showed that neither the IG-TTD nor linear combinations of the model can describe the tracer age relationships between CFC-12 and SF6 in the Fram Strait. This means that either the models are not suitable to describe the prevailing ventilation pattern or that there are other reasons which lead to the specific concentration ratios. Here we focused on the second case which incorporates the assumptions that the tracer age relationships are related to different saturation states of the transient tracers and, furthermore, that the simple IG-TTD model can describe the ventilation processes of all water masses in the Fram Strait. The uncertainties of our approach thus correspond to the chosen shape of the IG-TTD, i.e. the unity ratio of $\Delta/\Gamma = 1.0$, and the uncertainties of the measurement precision of the transient tracers and apparent transient tracers (see section 3.6 above). Further uncertainties are related to processes which influence the gas exchange and thus the boundary conditions of the tracers. This includes the important but yet rarely investigated impact of sea ice cover, sea ice formation and sea ice melting processes as well as bubble effects during heavy wind conditions.”

Page 2207 – “the IG-TTD model is valid for all water masses in the Fram Strait” – it was showed that the model is valid for some water masses, not for all of them.

See answer above

Page 2208 – “recommend the use of data from the subsurface layer” – recommend using the data. Which data? Salinity?

As it is described in the foregoing sentence it is the salinity/alkalinity data from the subsurface layer and not from the surface. Sentence rewritten for a better understanding.

Section 3.8
“The determination of the preformed alkalinity highly depends on the used method. Here we used the linear relationship between surface alkalinity and salinity which is a commonly used method. However, other authors recommend the use of alkalinity /

salinity data from the subsurface layer (Vazquez-Rodriguez et al., 2012) or the surface temperature and salinity dependencies (Lee et al., 2006)."

Conclusions

This part is more of a summary than actual conclusions. There is no new information which has not been already mentioned in the previous sections.

We added a new text part which provides more information about the essence of the manuscript.

Section 4
"The theory of saturations effects on transient tracers requires more targeted experiments and data acquisition from high latitudes to get proven or rejected. However, this approach should not contradict the assumptions on complex ventilation pattern but should rather contribute to a better understanding and analysis of the dynamic processes in polar ocean regions. Estimates on carbon transport are very important to predict future changes of the global carbon cycle and their impact on the global climate which requires the continuous improvement and, even more important, the critical questioning of existing scientific methods."

Figures

Fig 5. and Fig. 6 are practically the same - is Fig 6 really necessary in that paper? Perhaps distinguishing between the data marks would be enough?

These figures are practically not the same since Fig. 5 points out the age ratio in general separated by water masses. Figure 6 is the basis for our assumption that the IG-TTD is valid and that the data needs corrections for saturation effects.

Fig. 7 – provides little information. In my opinion it should be more detailed (or maybe merged with subplots from Fig. 8) or removed.

We thank the reviewer for this suggestion and have now merged the two figures into one.

A few technical corrections:

Page 2194 - Section 3.8, not 3.6
corrected

Page 2197 – von Appen et al., 2015 – already published
corrected

Page 2197 – A typo in the surname Beszczynska-Möller (check in the whole paper)
corrected

Page 2199 – “at _ 200m” – should be _ not _ (also in some other cases in the paper, but not everywhere)
corrected

Page 2206 – “100 % corresponds to a anthropogenic carbon” – delete the indefinite article.
corrected

“the” Fram Strait – correct in the whole paper
corrected