Detailed anser to Referee #2

Comments and questions from the reviewer are in red.

Page 1

Line: Titel / Ref. 2: Title: "the impact" - perhaps nice to have a title that is descriptive in the way it tells what the assumed impact is

The formation and melting of fast ice has several effects on the distribution of the DOM. It does not make sense to list them in the title. However, we agree with the reviewer. If we write "the impact" we should name it. To avoid this misunderstanding we suggest the following title: "On the impact of land ice on the distribution of terrestrial dissolved organic matter in the Siberian Arctic shelf seas."

Line: 17 / delete "their"

We will delete "their"

Line: 17-19 / sampling for oxygen isotopes is not described

We will include a sentence explaining that oceanographic data (T,S) were also collected and samples were taken on most of the expeditions for the determination of oxygen isotopes.

Line 18: / delete "concentration", and add "(CDOM) absorption" - assuming you measured the absorption of CDOM, not fluoresence?

In fact, we also measured the in-situ fluorescence of the DOM (WET-Labs Fluorometer). However, these data are not part of this study. We will change the sentence according to the reviewer's suggestion.

Line 19: / You make the assumption that all the DOM is of terrestrial origin, but there must be some marine production, or release from sediments?, thus the more generic term of DOM should be used, unless you have solid evidence this is all terrigenous material...

Why we, and almost all authors of studies from the Laptev Sea (citations in the discussion section), come to the conclusion that the terrestrial DOM dominates the DOM pool is explained in lines 252-258. The clearest indication in this context is the pronounced correlation between DOC concentration and aCDOM(350) absorption and the close proximity to the Lena River. This correlation is even higher for a CDOM absorbance of 254 nm (lignin band) (this can be verified by published data set). Given the available data and the studies published to date, we believe the use of the term "tDOM"is justified. Of course, there will also be a proportionally small amount of marine DOM in the samples. One possible reason why the marine DOM proportion is so low is discussed in lines 252-258. We only found a distinct marine CDOM signal in an ice core sample with a high proportion of chlorophyll a (line 237-238). We are not aware of data on high DOM inputs from marine sediments of the Laptev and East Siberian Seas that would contradict the scientific conclusions of our study. DOM introduced by erosion of permafrost coasts is, in our understanding, also terrestrial DOM. High inputs of tDOM from coastal erosion should also be evident at some distance from estuaries in nearshore DOM maxima in the East Siberian and Laptev Seas. This is not shown by our data set and therefore was not discussed in the context of this study.

Line 21 / 211 km^3 average over the years?? and Line 22 / 245 km^3 average over the years?? which period exactly (give months).. for both landfast ice melt and river water..

Unfortunately, we have not noticed that a numerical error has crept into the abstract. The volume of meltwater is 245 km^3 per year (June to mid-August). The annual river input during the freshet in 2014 (May 21 to June 19) is 211 km^3. The calculation of the meltwater volume refers to the data published by Selyushenok et al. (2015) and is described in more detail in chapter 4.2 (line 311 to 314). We will change this and describe the respective observation periods in more detail.

Line 25 / the shelf is quite shallow, so what do you mean by "near-surface layer", please be more specific, as usually one assumes that the dense brine-rich water would find their way to the bottom on the shelf, no?
The comments of the reviewer are certainly correct. We will explain "near-surface layer" in more detail in the abstract to avoid misunderstandings. The entire central and eastern Laptev Sea with its high inputs of river and meltwater has a strong density stratification that persists well into winter. Even in extreme years when the winds transport the river and meltwater plume to the east, the stratification in the central Laptev Sea is not completely eroded until March. Our research group was able to demonstrate this using measurements with oceanographic moorings (Janout, M., Hölemann, J., Smirnov, A., Krumpen, T., Bauch, D., Laukert, G., and Timokhov, L.: On the variability of stratification in the freshwater influenced Laptev Sea region, Front Mar Sci, doi: 10.3389/fmars.2020.543489, 2020.). Additional citations can be found in the Discussion section. Brines that form during the strong growth of fast ice in October to March in the SE Laptev Sea are therefore not transported in bottom waters across the inner and central shelf but in the surface mixed layer (0-10 m) and within the pycnocline (10-25 m). We will specify this in more detail in the abstract. On the outer shelf and in the area of the continental slope, the DOM-rich brines from the SE Laptev Sea can be mixed into greater water depths by the supply of further DOM-poor but denser brines and by mixing processes at the shelf edge (Schulz et al, 2021), but the DOM concentration will be diluted. We will try to illustrate this even better in the discussion (chapter 4.4).

Schulz, K., Janout, M., Lenn, Y.-D., Ruiz-Castillo, E., Polyakov, I., Mohrholz, V., Tippenhauer, S., Reeve, K. A., Hölemann, J., Rabe, B., and Vredenborg, M.: On the Along-Slope Heat Loss of the Boundary Current in the Eastern Arctic Ocean, Journal of Geophysical Research: Oceans, 126, e2020JC016375, https://doi.org/10.1029/2020JC016375, 2021.

Line 27-29 (last sentence of abstract) / Feels like hand waiving, and unless there is something more substantial to support this vague statement, delete it. If you can describe what is potentially changed, it would have more substance.
In the conclusions we have presented this in more detail and certainly better: (line 433-440) „Further changes of the ice regime and the timing of spring freshet will certainly have an impact on the dynamics of tDOM in the AO. In addition, the decline of Arctic sea ice and the associated longer ice-free season will lead to changes in wind forcing in the shelf systems of the Arctic and to an increased input of solar radiation into the water column. This will significantly change freshwater transport pathways and water column stratification in the LS and ESS. Because stratification controls where and at which depth the tDOM-rich brine leaves the shelf, changes in shelf stratification also impact the future transport pathways of tDOM in the AO. Furthermore, the increased input of solar radiation causes a rise in water temperature in summer (Timmermans et al., 2020b) and could thus potentially intensify the photochemical processes in the surface mixed layer."
We will use this argumentation (including the observed changes of the ice regime) in an abbreviated form in the abstract.

Line 32 / I would find that work by e.g. Stroeve and coworkers is more appropriate citation here.. e.g. Stroeve, J., & Notz, D. (2018). Changing state of Arctic sea ice across all seasons. Environmental Research Letters, 13(10), 103001. https://doi.org/10.1088/1748-9326/aade56
We agree with the reviewer and will cite the proposed study in the revised version of the paper.

Line 34 / Impressive number in Pg of C, but without context it is useless .. please provide context for this amount of carbon.
Honestly, I don't understand this comment. Is the context not clearly described?: "Due to the accelerated degradation of terrestrial permafrost, an estimated **1035 ±150 Pg of organic carbon stored in the upper three meters of circumpolar permafrost soils (Hugelius et al., 2014) can be** either mineralised and released as gaseous emissions into the atmosphere or **mobilized as terrestrial**

dissolved organic matter (tDOM) into the hydrosphere (Plaza et al., 2019). The release of soil carbon into the hydrosphere in combination with an increasing freshwater discharge from Arctic rivers (Rawlins et al. 2010; Haine et al., 2015; McClelland et al., 2006) **might thus increase the flux of tDOM into the ocean** (Frey and Smith 2005; Guo et al. 2007; Prokushkin et al., 2011; Tank et al., 2016)."

Page 2

Line 40 / replace "in the high" with "at high"
We will do this in the revised version of the paper.

Line 48 (and paragraph above): What about the ESS - you describe both LS and ESS in the abstract, but here focus on LS only. Aren't there also riverine fluxes of DOM to the ESS as well? How large are they compared to LS/Lena? And what about diffuse input, not carried by the largest rivers? Which fraction of the water shed (area wise) does the larger rivers cover?
We have limited the data to the 3 largest rivers. But we will add the two larger rivers (Indigirka and Kolyma) that flow into the ESS and add the data for the catchments area. Kolyma and Indigirka have a combined catchment area 0.93 10^6 km^2 (Lena 2.4 10^6 km^2) and a combined average annual river water input of ~155 km^3. The Kolyma contributes 0.67 Tg C annually (approx. 10% of the Lena contribution) to the East Siberian Sea (Stedmon et al., 2011). Although we stated (line 361-365):" **During our sampling in the ESS in 2019**, winds from the east (ERA5; Copernicus Climate Change Service, 2017) pushed the inflow from the Pacific Ocean westward far onto the ESS shelf. At the same time, southeasterly winds over the LS might have blocked the Lena ROFI from extending into the western ESS (Anderson et al., 2011; Janout et al., 2020). In addition, summer **2019 was characterized by anomalously low river discharge into the LS and ESS (ArcticGRO; Shiklomanov et al., 2020)**."

Unfortunately, we have no data on which to assess annual diffuse DOM inputs along the coasts of the East Siberian and Laptev Seas. Sampling of these inaccessible marine areas north of Siberia with sea-going research vessels did not allow for sampling in marine areas shallower than 10 to 15 m. Therefore, the nearshore area could not be sampled. However, the distribution of DOM we observed showed no evidence of local inputs of DOM away from the mouths of major rivers.

Line 54-55 / Also Granskog et al. 2012 indicated loss in Fram Strait
We will include the Fram Strait and the associated citation (Granskog et al., 2012).

Line: 54-55 / And for Hudson Bay another study on estimating loss of (t)CDOM was also presented in: Granskog, M., et al. (2009). Coastal conduit in southwestern Hudson Bay (Canada) in summer: Rapid transit of freshwater and significant loss of colored dissolved organic matter. Journal of Geophysical Research, 114(C8), C08012. https:// doi.org/10.1029/2009JC005270
We will include Granskog et al. (2009) in the revised version of the paper.

Is there differences in the ice season length on Laptev versus Hudson Bay, that could indicate different potential for photochemistry?
Unfortunately, Hudson Bay was not part of our study. It was also not our goal to compare the Laptev Sea and the East Siberian Sea with all Arctic seas. We can only refer to previously published studies and satellite data of ice cover (Univ. Bremen etc.) to answer the question. The ice season in Hudson Bay is only slightly shorter than in the fast ice areas in the eastern Laptev Sea and the western East Siberian Sea. The seasonal period with 50% ice cover is about one month shorter in Hudson Bay. However, in terms of photochemistry, two other factors could make a crucial difference. First, the study area in Hudson Bay (Granskog et al., 2009) is south of the Arctic Circle between 55° N and 61° N, whereas our study area is north of the Arctic Circle between 70° N and 78° N. Since Granskog's study is based on post-summer data (October), we would suggest that the significant differences in solar radiation between the two marine areas could be a major factor. Second, salinities in the

eastern Laptev Sea south of 75° N in the surface mixed layer (0-10 m) are below 25 in all years studied with a_{CDOM} (350) above 6 m^{-1}. This leads to strong absorption of solar radiation already in the uppermost 2 m of the water column and could further reduce photochemical processes in the deeper water layers.
Another key difference between the marine areas is also mentioned in Granskog et al (2009): "Clearly, in considering any process related to freshwater in Hudson Bay, both RW (river water) and SIM (sea-ice meltwater) need to be considered. The distribution, location of entry, and timing of these two freshwater sources differ." This is not the case in the SE Laptev Sea. Here, about a third of the annual river discharge and more than 40% of the annual DOC input flows into the SE Laptev Sea where considerable amounts of fast ice are melting at the same time. The description of this process and its impact on the distribution of tDOM is one of the main topics of our study.
In the Granskog study, the river water fraction is at most 25%. In our study from the Laptev Sea it is much higher. The Laptev Sea is a major ice-formation and export shelf sea with an open boundary to the Nansen and Amundsen Basin and thus probably more dynamic. I am sure that there are many other differences that could also explain the different potential for photochemical processes. However, this is not the subject of our paper.

Line 62 / delete "a" before tDOM
We will.

Line 66 / But in fact all the studies you cite here elude towards a loss of tDOM, isn't that consistent?
Yes, this sentence is not well formulated and misleading. In contrast to the studies on the Arctic marginal seas, all publications cited here postulate a degradation. Whereby the residence time and degradation rates of DOM varies between studies. We will rephrase this sentence in the revised paper to make this clearer.

Page 3

Lines 65-71 / Here also refer to work by Belanger et al (2006) and the CDOM losses have been estimated in the FramStrait (Granskog et al 2012), and the work of Manizza and co-workers approached this from a modelling perspective, for completeness also discuss these results in the paragraph lines 65-71.
To complete the overview of current studies, we will include and cite the proposed citations in the discussion. However, since the main topic of our study is not to discuss the degradation of DOM, but to show that with the large amounts of fast-ice meltwater an important mixing component exists that needs to be better considered in future studies of DOM degradation in the LS and ESS, the proposed citations are not of direct relevance to the results of our study.

Line 74 / You mean conservative mixing? Why do you not simply say so. This is an awkward way of telling that. And
We have tried to avoid using the term "conservative mixing" here because it already implies an interpretation of the observation. Since we do not want to appear awkward, we can rephrase the sentence to : " ...strong negative relationship of tDOM with salinity..."

Line 77 / depends on the what the sea ice melts into..and Line 77 / and here it is more appropriate to say "DOM" since offshore there is also marine DOM in the water column..
That is, of course, correct. We can rephrase the sentence: „Melting of DOM-poor sea ice is important because meltwater can dilute higher DOM concentrations in the ambient water mass (Amon, 2004; Mathis et al., 2005; Granskog et al., 2015; Logvinova et al., 2016)".

Line 79 / what do you mean by thin, and does it melt in place or is it mobile?
We are referring here to the drifting pack ice north of the fast ice. We will rephrase the sentence to make this clear. Because ice north of the fast ice is continuously exported to the transpolar ice drift system and new ice is continuously formed in the polynyas between the fast ice and the pack ice, the

thickness of mobile pack ice in the Laptev Sea is predominantly thinner than 50 cm at the end of winter (Itkin & Krumpen, 2017; citation in text). That the retreat of the pack ice in spring is predominantly controlled by the prevailing atmospheric conditions in April and May was shown in a study published in 2016 (Janout et al., 2016).

Janout, M. A., Hölemann, J., Waite, A. M., Krumpen, T., von Appen, W. J., and Martynov, F.: Sea-ice retreat controls timing of summer plankton blooms in the Eastern Arctic Ocean, Geophys Res Lett, 43, 12493-12501, 10.1002/2016gl071232, 2016.

Line 81 /There are in fact a number of experimental studies that directly look at the fractionation of DOM during sea ice formation.. and the latter point to a minor preferential retention of DOM in ice vs. salt..
We have cited Müller et al. (2013) and Gianelli et al. (2001) in chapter 4.2 (line 299). However, we will also cite Müller's studies here. Following the suggestions of reviewer 1, we will describe the results of these studies in more detail in Chapter 4.2.

Line 82 / replace "also explain" with "may explain" - before you have solid proof it is not another process.
We will.

Line 84 / landfast sea-ice and Or do you mean sea ice in general, or the ice melting in place?
We mean landfast sea-ice. We will add this.

Line 87 / replace "studied" with "understood"
We will.

Page 4

Lines 102-103 / The two sentences could be combined to: "Six ice cores from three different sites (Fig. 1) in March-April 2012 were analysed (Ti12, Table 1).
Yes that sounds better. We will combine the two sentences in the revised version of the paper.

Lines 102-103 / Should the campaign be "Ti12_ice" for the ice cores??
Yes. We chose an extra campaign label for the ice cores because the O18 data from the ice cores, unlike the Ti12 water samples, have not yet been published in PANGAEA.

Figure 1 / where all stations visited every year of sampling?, if not, please somehow indicate in the figure which where visited in which year (or expedition).
No, not all stations were sampled again every year. We can point this out again in the caption. We have tried to mark the individual expeditions with different colors. But this makes the figure completely confusing. However, each record of the different expeditions has a doi at PANGAEA. When one opens the doi, a map of the sample locations appears. One can zoom into this map. If you drag the cursor over the individual stations a pop-up window opens that gives the metadata of that station. We will point out this possibility in the caption.

Figure 1 / Bathymetry perhaps draw e.. the 50 m or 100 m isobath for clarity. And what is the source for the bathymetry?
The bathymetry is based on IBCAO Version 3.0 (we will mention this in the caption). Inserting the 100 m line that also marks the shelf edge is a good idea. We will insert the 100 m depth line.

Figure 1 / Does the 245 km^3 also include the ice in the ESS? (all that is shown on the map)? If not, please show where you draw the border between LS and ESS in your budgets. And is this the average over a period of years, please clarify in the caption.

The figure explicitly states that the 245 km³ refers only to the fast ice in the Laptev Sea. The basis of the calculation is the publication of Selyuzhenok et al. (2015). The volume of fast ice in the East Siberian Sea is subject to strong annual variations. This is explained in more detail in the discussion (line 373). It is also described there (line 373) that we have drawn the boundary (according to the general definitions) between the Laptev and East Siberian Sea at 140°E. We will also highlight this in the figure.

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Table 1: / "Campaign/expedition" rather than "Name"
We will change the column label in the table.

Line 118 / Section 2.2, how were oxygen isotope samples collected, and volume large enough to make robust salinity measurement from same sample?
Oxygen isotope samples were collected in 100ml glass bottles. About 80ml of the volume was taken for salinity measurements. Accordingly analysis with an AutoSal 8400A salinometer (Fa. Guildline) were made with a precision of ± 0.003 and an accuracy greater than ± 0.005. This is already stated within the manuscript.

Line 124 / "containers" rather than "boxes"?
We will use the term "containers"

Line 126-127 / -what samples were drawn from the ice cores, DOC, CDOM, O18, salinity?
Samples for salinity, DOC, CDOM, Nutrients, Chlorophyll a, particulate matter and O18 were drawn from the ice cores. Only Salinity, doc and CDOM were used in this study.

Line 127 / Which unit of salinity is used for the data in this paper?
Our measurements are based on conductivity, temperature and pressure. We used the practical salinity scale to describe salinity (PSS 78).

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Line 150 / what is the effect of microbial degradation on this slope? and also Granskog (2012) indicated that slopes of shorter wavelengths are sensitive to removal.
We will expand the description and describe the effect of microbial degradation on the slope as well. To comply with the reviewer's request, we will also include the citation from Granskog (2012).

Line 160 / Since the choice of end-member is probably quite important in such a system, what are the uncertainties in the fraction calculated using this mass-balance equation?
The errors in fractions are discussed in Bauch et al., 2013 (as cited in the manuscript): The analytical errors from d18O and salinity measurements add up to approximately +-0.3% for each of the fractions. The additional systematic error depends on the exact choice of end-member values. When end-member values are varied within the estimated uncertainties (see Bauch et al., 2013), both fractions are shifted by up to ~1% in absolute values, but results are qualitatively always conserved even when extreme variations in end-member values are tested (Bauch et al.,2011).

Line 168 / Please elaborate on the choice of end-members, since the resulting fractions are sensitive to the choice of end-member, especially in such a region with clearly several different end members.
And: Do you have a measured sea ice end-member value for the landfast sea ice?
The endmembers are important, but well known for the study area (see Bauch et al., 2010, 2013). The only exception is the sea-ice endmember that is based on an assumption on the signiture of the source water, since sea ice and underlying water can move independently from each other. The choice of endmember for sea-ice meltwater is discussed in detail in Bauch et al., 2010: Within the direct vicinity of the Lena River, the summer surface layer is strongly influenced by summer discharge

of the Lena River, so the low d18O summer surface signature in this area is not a useful end-member for the sea ice formed during winter. Therefore, the average surface value from the winter polynya region of −7 in d18O is applied as source water for sea ice formation to all stations with a surface d18O lower than −7‰. The differences in calculated sea ice meltwater and river water fractions in the southern Laptev Sea, when a constant polynya value is used instead of each station's surface signature, are generally small (see Fig. 6 within Bauch et al., 2010), and calculated fractions remain stable relative to each other. We do have measurements of land-fast ice, but these measurements cannot be used directly as endmember for sea-ice meltwater as they represent a mixture of sea-water and river water contained within the ice.

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Line 173/ month/date? Do you have the hydrograph of the Lena River, to indicate when you sampled relative to the freshet in 2014?

We will specify the month and date of the freshet in more detail. The hydrograph is shown in figure 3 in chapter 3.2. The citation from Juhls et al. (2020) cited in Chapter 3.2 describes in great detail the evolution of the hydrograph of the Lena River and the variability in the timing of the freshet over the past decades. We will include a reference to chapter 3.2 when discussing DOC concentrations in the course of the freshet in chapter 3.1.

Juhls, B., Stedmon, C. A., Morgenstern, A., Meyer, H., Hölemann, J., Heim, B., Povazhnyi, V., and Overduin, P. P.: Identifying Drivers of Seasonality in Lena River Biogeochemistry and Dissolved Organic Matter Fluxes, Frontiers in Environmental Science, 8, 10.3389/fenvs.2020.00053, 2020.

Line 176–177 / 50 ug/L at salinity of 20? Seems quite low at such low salinity. Sea-ice melt?

Please note: All DOC data are presented as µmol l^{-1} **NOT** µg l^{-1}! The lowest values (50 µmol l^{-1}) were measured in the Atlantic Intermediate Water (continental slope at 300 m water depth), which has a river water content of less than 2 percent. Our data do not show a significant input of sea-ice meltwater at river water fractions (fr) below 5 %. We show only the CDOM values in our manuscript because DOC was not measured in all samples and the DOC data only show information that does not provide additional information related to our hypotheses (see Figure A). It may be possible to publish additional graphs in a supplement to the article. A figure showing the ratio of $a_{CDOM}(350)$ to river water fraction will be included in the manuscript.

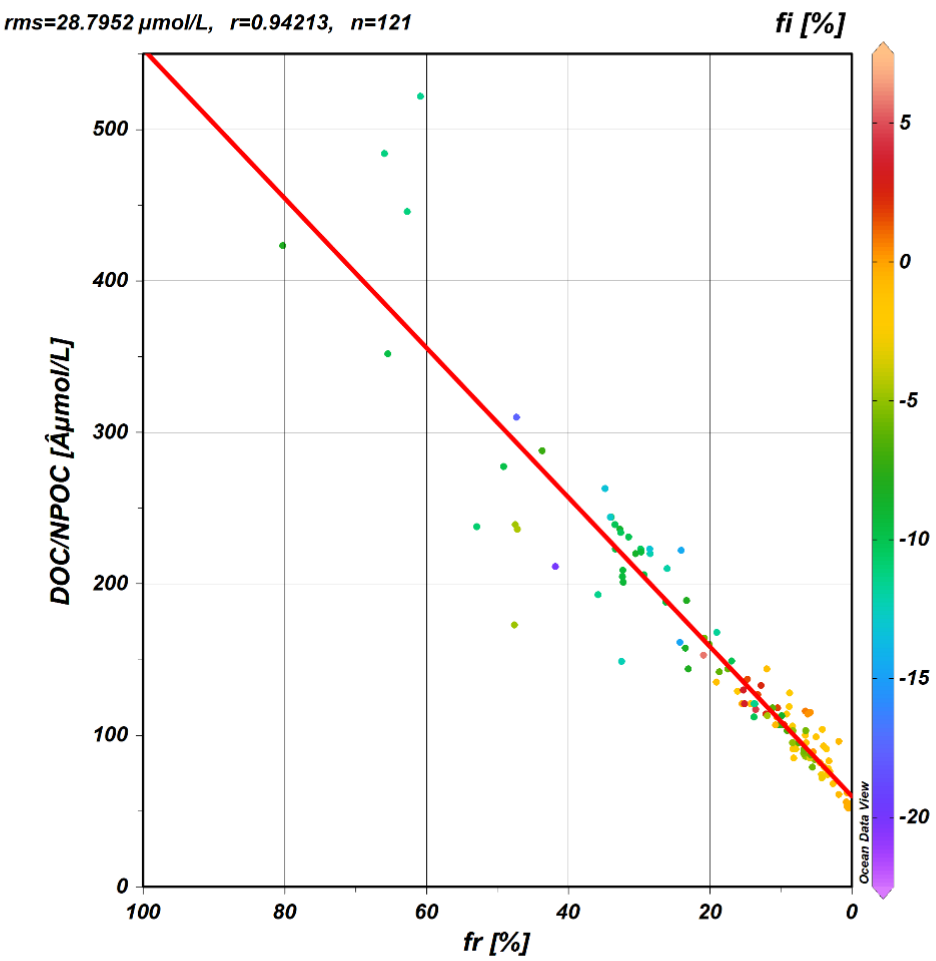

Fig A: DOC (µmol/l), river water fraction (fr), and **sea-ice** meltwater fraction (fi = fsim in the manuscript). Fi does not represent the whole land-fast ice meltwater fraction because the land-fast ice contains a high proportion of river water!. Fitted curve =least square line. ODV. Data set from the published doi's (Table 1)

Line 179 / How well does the methods applied by Fichot and Benner - and - Goncalves-Araujo et al apply: Fichot, C. G., & Benner, R. (2011). A novel method to estimate DOC concentrations from CDOM absorption coefficients in coastal waters. Geophysical Research Letters, 38, L03610. https://doi.org/10.1029/2010GL046152
Gonçalves-Araujo, R., et al. (2020). A decade of annual Arctic DOC export with Polar Surface Water in the East Greenland Current. Geophysical Research Letters, 47, e2020GL089686. https://doi.org/10.1029/2020GL089686 and Line 181 / impressive R2, but what about RMSE, especially at lower DOC concentrations..
This study does not focus on using CDOM as a parameter to predict DOC concentration (also shown by the fact that we use DOC as x and CDOM as y). Here, we simply describe their relationship. Due to that reason, we initially did not include more statistical parameters for the performance of the non-linear fit model. However, we agree that an absolute error parameter is helpful. We will include the RMSE (= 0.80 m^{-1}) into the figure (Figure B).
The use of other CDOM parameters (e.g. S275-295) and fit models (amongst many others Fichot and Benner - and - Goncalves-Araujo et al. (2020) to predict DOC is not the scope of this paper. In Juhls et al., 2019 it is reported that S275-295 results in slightly weaker relationships to DOC, compared to a single aCDOM wavelength.

Lines 181-186 / What was the DOC and CDOM in the water that the ice grew into, i.e. what was the "fractionation" at freezing .. the same as for salinity? Are salinity normalized values the same for ice and water?

The fast ice grows throughout the winter, but the thermodynamic growth should be strongest in October, November, and December, when the fast ice is still thin. The measurements in the water under the ice made during the Ti12 winter campaign in March and April 2014 therefore represent rather the sea water that will mix with the meltwater (0-10 m, Mean Salinity 20, Mean DOC 308 µmol l^{-1}, SD 104 µmol l^{-1}, Mean $a_{CDOM}(350)$ 5.5 m^{-1}, SD 1.6 $^{m-1}$, n=19). Since the Siberian Laptev Sea is closed to navigation from mid-October to July and measurement campaigns with helicopters are only possible from March onwards, there are absolutely no oceanographic and marine chemical measurements from these areas during this period. Therefore, for the estimation of DOM concentrations from which the fast ice grows (line 384), we took the average salinity (9) and the average DOC concentration (475 µmol l^{-1}) of the surface mixed layer (0-10 m) from the SE Laptev Sea. These values were recorded during our measurement campaigns in August/September. We did not calculate the fractionation due to lack of continuous measurements in the water body. For the calculation of the budgets the measured values from the fast ice and the water of the SE Laptev Sea were taken. This is certainly only a first approximation, but in our opinion a plausible assumption.

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Figure 2 / The fit is made for the DOC range that is "huge" when marine environments are considered, and despite the high R, are the deviations from this at low DOC significant? What is the RMSE, as it is an absolute measure of the goodness of fit, rather than R? Add a "zoom in" for the range 0-200 ug/L DOC, or perhaps 0-400 to include the ice samples.

Please note: DOC concentrations are presented as µmol l^{-1}.

In the revised version we we will include the zoom inset and the RMSE into the figure (Figure B)

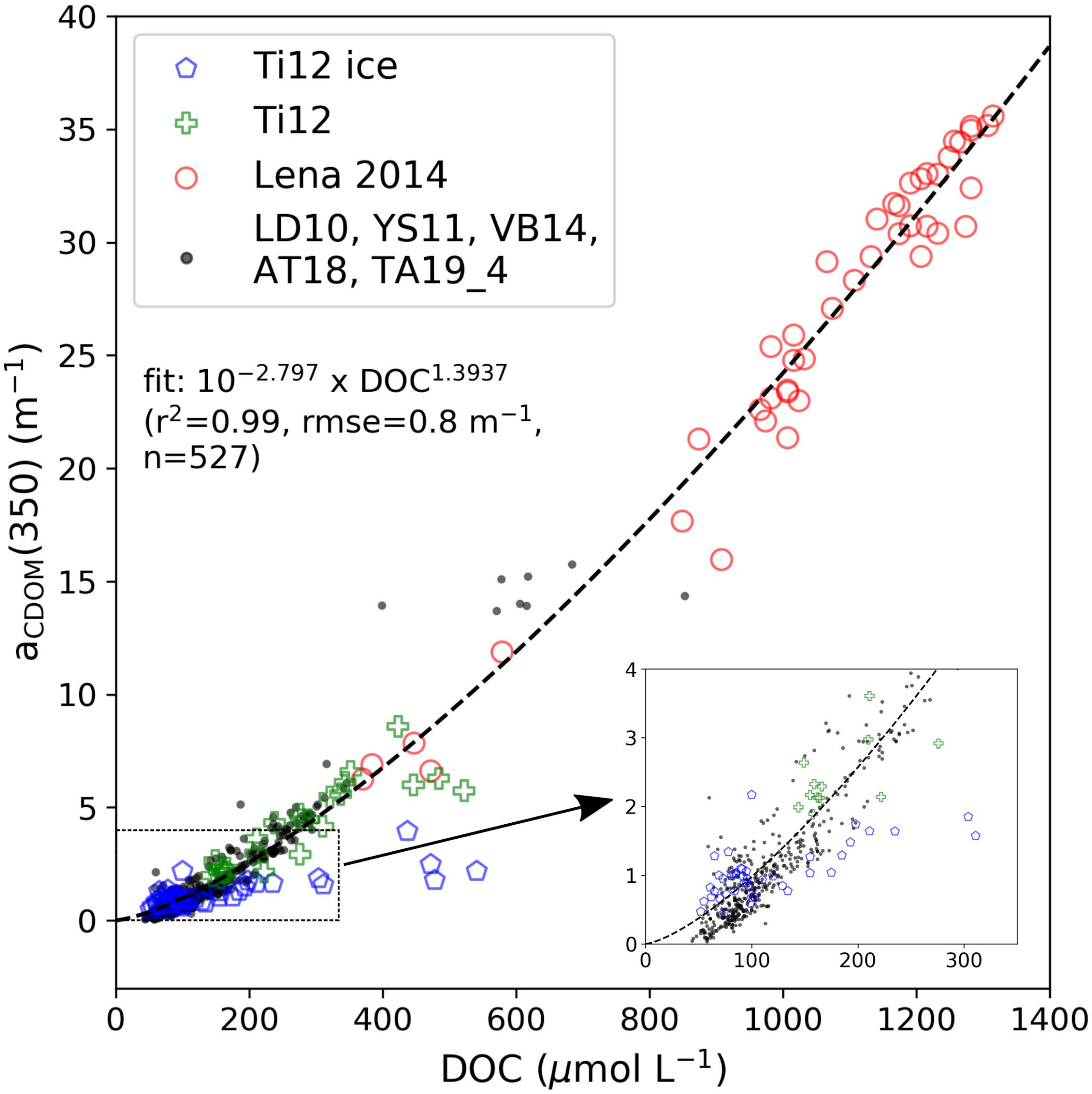

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Figure 3. / Add a panel for S275-295, does this change with season/freshet..
We do not think that an additional panel is helpful since it does not provide any additional information within the scope of this paper" (Figure C). In order to provide the numbers for potential comparisons, we will add the range of S275-295 in the text. Additionally, we provide the complete spectral CDOM dataset, allowing a calculation of CDOM slopes.

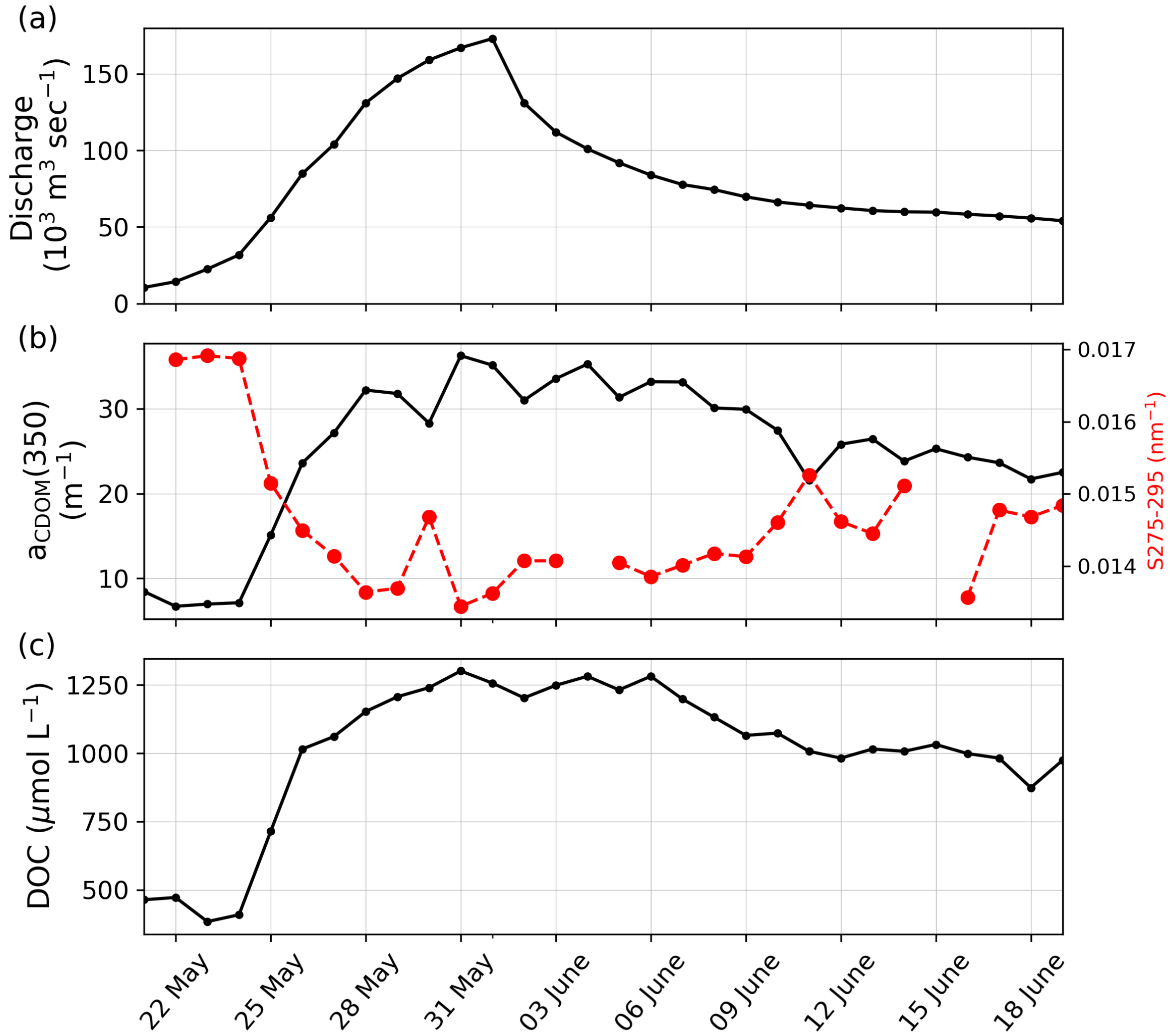

Fig B. S275-295 (nm^{-1}, raw data) during the observational period (Lena 2014)

For a detailed description of S275-295 and its full seasonal variability in the Lena River, we refer to the publication by one of the co-authors of this study: Juhls et al. (2020).

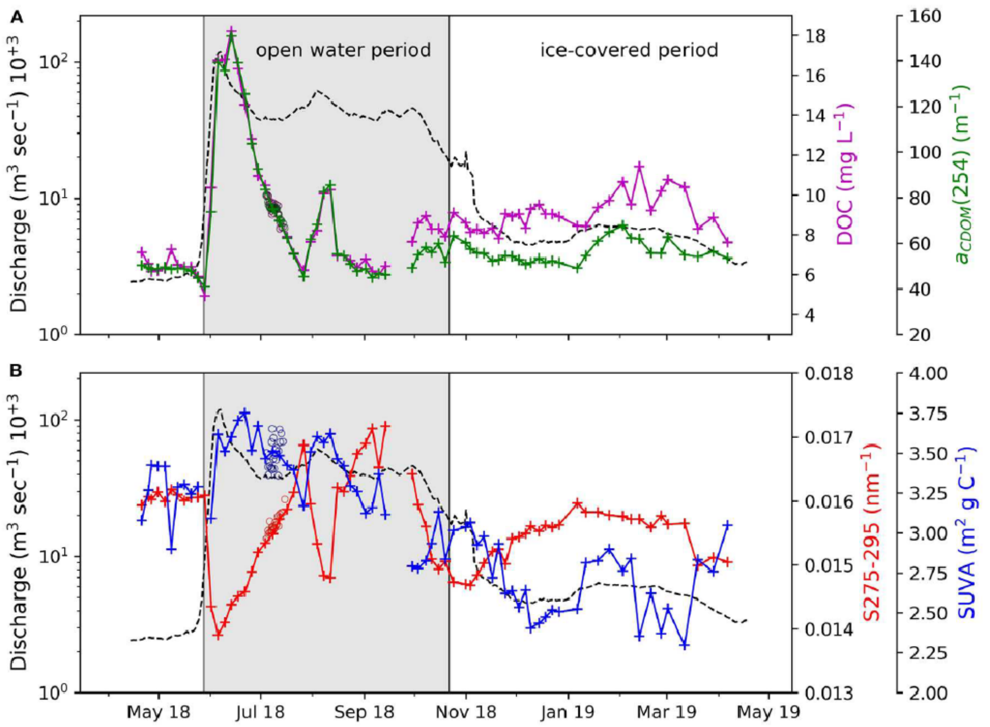

FIGURE 5 | Seasonal variation of DOC and $a_{CDOM}(254)$ **(A)** and S275-295 and SUVA **(B)**. Black dashed line shows the discharge. Circles in respective colors show high frequency measurements from July 2018.

From: Juhls, B., Stedmon, C. A., Morgenstern, A., Meyer, H., Hölemann, J., Heim, B., Povazhnyi, V., and Overduin, P. P.: Identifying Drivers of Seasonality in Lena River Biogeochemistry and Dissolved Organic Matter Fluxes, Frontiers in Environmental Science, 8, 10.3389/fenvs.2020.00053, 2020.

Figure 3. / Also, a second y-axis with accumulated flux could be added to each panel (except S275-295)..

The accumulated fluxes are given in the text. Another illustration would not give any further information (Figure D). We possibly may add this figure to a supplement.

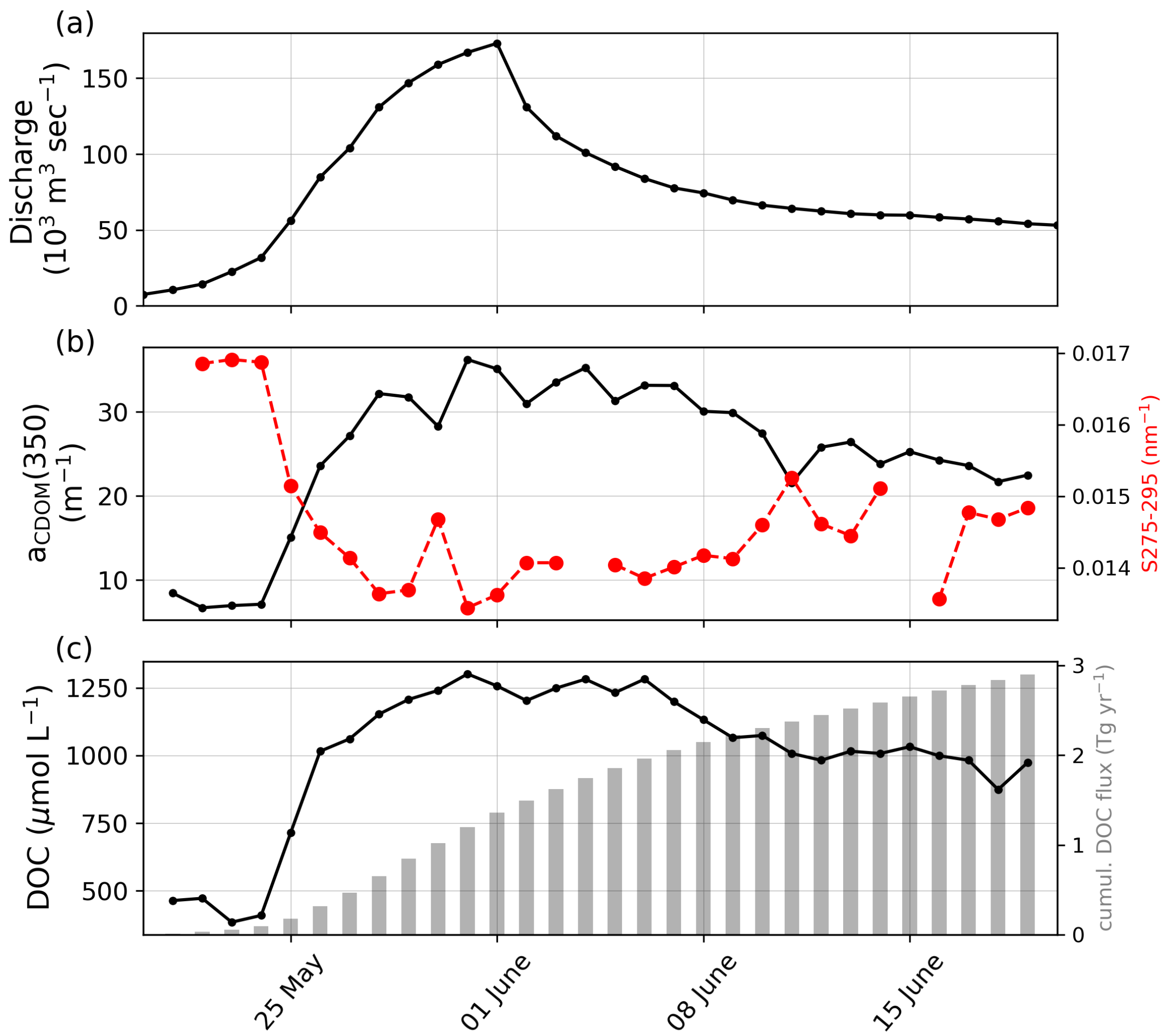

Figure D: Figure with cumulative flux added

When recalculating the DOC flux during the observational period in 2014 (2.35 Tg), we found that we made a calculation error, which was very difficult for the reviewers to detect and was therefore not objected. The actual entry in the observation period is 2.83 Tg DOC. However, the flow-averaged value of aCDOM(350), which is the basis for our calculation of mixing with meltwater, is correct. In the revised version of the paper we will change all text that refers to the DOC flux.

Line 199 / 211 km3: But this must vary from year to year, and this data is available from Arctic GRO for the years with observations, consider at least reporting these values..

The discharge measurements of the Lena River (Kyusur) were provided through ArcticGRO. This is mentioned and cited in line 194. The annual variation of the Lena River discharge is presented in the publications of Janout et al. (2020) and Juhls et al. (2020) and many other studies. The variability in the discharge and timing of the freshet are discussed in detail in Juhls et al. (2020). We took the year 2014 in which we made our measurements as a case study. Just as in the study by Juhls et al. (2020), several measurements were taken during the freshet each week. These measurements in the Lena delta are currently being continued and form a time series that gives a much better temporal resolution than the rather sporadic DOM measurements taken in the middle reaches of the Lena River by ArcticGro. However, the time series from the Lena Delta are subject to future studies and are not part of this publication.

Line 202 / where is the value of 14 m^{-1} taken from?

This value is an average of all July, August, and September data from the Lena Delta (Polar Station Samoylov) derived from the data DOI cited in Juhls et al. (2020) (Data DOI https://doi.org/10.1594/PANGAEA.913196). This is cited in line 264 (Actually: 14.4 m^{-1} with a standard deviation of 3.4 m^{-1}). We will also include the citation in the text in line 202.
If we take the data published at ArcticGro (July to October) from the middle course of the Lena we get a mean value of aCDOM(350) 13 m^{-1}. We decided to take the value of the high resolution time series near the mouth of the river Lena.

Line 203 / how far does the "full freshet" extend, and by how much (%) would the water and CDOM/DOC fluxes increase?
The reviewer's question has led us to delete the text part "…indicate that the observation period did not cover the entire spring freshet. Taking into account the whole period of the spring freshet would lead to an even higher tDOM flux, as reported in Juhls et al. (2020).".

With an outflow of 50,000 m^3 sec^{-1} at the end of the observation period, the Lena has reached a value that is within the range of discharges observed in July to November (ArcticGro). The $a_{CDOM}(350)$ absorption is still higher than the average value for the summer months (data-doi, Juhls), but is within the seasonal variability of CDOM absorption measured in 2018 (Juhls et al. 2020). Therefore, specifying a date for the end of the freshet in 2014 that is after June 19 would be somehow arbitrary.

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Line 206-7 / Using an a350 end-member of 14 m^{-1} seems low, when you show in Figure 3 values of 26 m-1 for the period of observations in Lena River, no?
In the text it is always described that the value of 14 m^{-1} is the absorption of the river water after the freshet in (post-freshet July, August and September). We will emphasize this again in the text with the term "post-freshet".

Figure 4 / In this system you also have another mixing line, with sea-ice melt as end-member, right? This should also be indicated with at least with apparent location, low S (2-5?) and 5-fold lower CDOM than the parent water (at most 14/5?). And: That said, you have a system where salinity alone cannot be used to deduce the exact behavior or cause of the deviations. I would expect to add a panel to this figure where "salinity" is replaced with "fraction of river water(meteoric)".
The reviewer's comment is absolutely correct. However, we assume that the majority of the readers are not experts in the field of DOM in the Arctic. Therefore, we have separated the chapters describing the results into river, sea water (LS and ESS) and fast ice to ensure that the description of the data is not too confusing. The discussion section then examines the data in synopsis. Following the reviewer's suggestion, we will show a figure with the river water fraction (like Figure E) and could also include the “mixing line” between the fast-ice melt water and the sea water in Figures 6 and 8. The problem in this context is that in the Laptev Sea, the melt water from the fast ice mixes with the river discharge from the freshet of Lena River. This is one of the main results of this study! This mixing line would thus run parallel to the y-axis in direct proximity to the axis .

Figure 4 / Are the samples with S<5 also included in the fit? And the "drop" of values from S<5 to S>5, how is that explained?
Yes they are included. We do not want to go too much into the interpretation of the data when presenting the measurements. We cannot see a "drop" in the sample cluster because the sample with the lowest salinity does not show the highest value. A possible reason for this sample cluster is discussed in the discussion section in lines 264-267.

Figure 4 Legend / Instead of "Theoretical mixing line" - this is the River:Marine mixing line. Since there are more than 2 end-members here (+processes we cannot account for in such a salinity-property plot).

We will insert a river-water fraction vs. CDOM figure and insert there the theoretical mixing line between the post-freshet river water and the sea water. We call it "theoretical" because it is clear from the data shown that there is more than one freshwater component (i.e. post-freshet, freshet, fast-ice meltwater). For this reason, we do not call our regression line that was created based on the measurements a mixing line but "only" a linear fit. This fit cannot be an "actual" mixing line because we prove that there are three dominant freshwater sources with different DOM characteristics. This will be examined in more detail in the discussion section.

Figure 4: / Especially the end-member at 0 salinity might vary a lot from campaign to campaign (which you only show in Fig 8!)... thus I think one should indicate this uncertainty in the mixing line(s) and also here show the zero salinity values now only shown in Fig 8.
The uncertainties of the different “mixing lines” are given in the text (Results and Discussion). The variability of the river endmember during the post-freshet period is described in line 265. We believe that the figure shows what the 0 salinity values are. “Exact” values are given in Figure 8. We do not want to overload the figures with too much information.

Fig 4 / And I believe the Fsim fraction could be shown better with a different colormap. E.g. in matplotlib a colormap type Diverging (coolwarm), see https://matplotlib.org/3.1.0/tutorials/colors/colormaps.html#diverging . Would perhaps be better, than the "rainbow" type used now.
We will check if a different color scheme will improve the image.

Line 214/ insert. .. fit "to the data in the Laptev Sea" (solid line). ?
We will.

Line 216 / insert. .. fit "to the data in the Laptev Sea" (solid line). ?
We will.

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Figure 5 / Again, make certain what the mixing line is for (See comment on fig 4). Also here, indicate the plausible marine to sea-ice melt mixing line.
See answer on comment on figure 4

Figure 5 / Consider that you show the station in ESS in Fig 1. with their own color, and then use same color in this plot for ESS data points.
We can mark the stations in the East Siberian Sea in red in Figure 1. However, we believe that it will be clear to the reader that the samples we indicate as coming from the East Siberian Sea actually come from the East Siberian Sea (see also Tabel 1). The station details (meta data) can be found in the map shown under the associated PANGAEA data doi.

Figure 5 / What are the different sizes of the back dots representing? Or are they clusters of samples?
This is an error in the illustration. The black dots should all be the same size. They are not clusters. We will correct this.

Generic comment / Results Section. I would have expected a salinity-DOC plot, just out of curiosity to see also how DOC relates to salinity. Same also for Fr-DOC if there is enough data available. And colored with e.g. Fsim
For the answer to this question, we refer to the answer to the question on page 7 line 176-177. We again point out that the entire data set is published and thus the reader can satisfy his curiosity.

Figure 4, 5, 6 / Figure 4, 5, 6 should be merged into one figure with multiple panels, also including plots Fr-property. And add S/Fr-DOC plots as well.

We already do this in Figure 8 in the discussion section. A fr-property plot will be inserted in the results section (similar to Figure E).

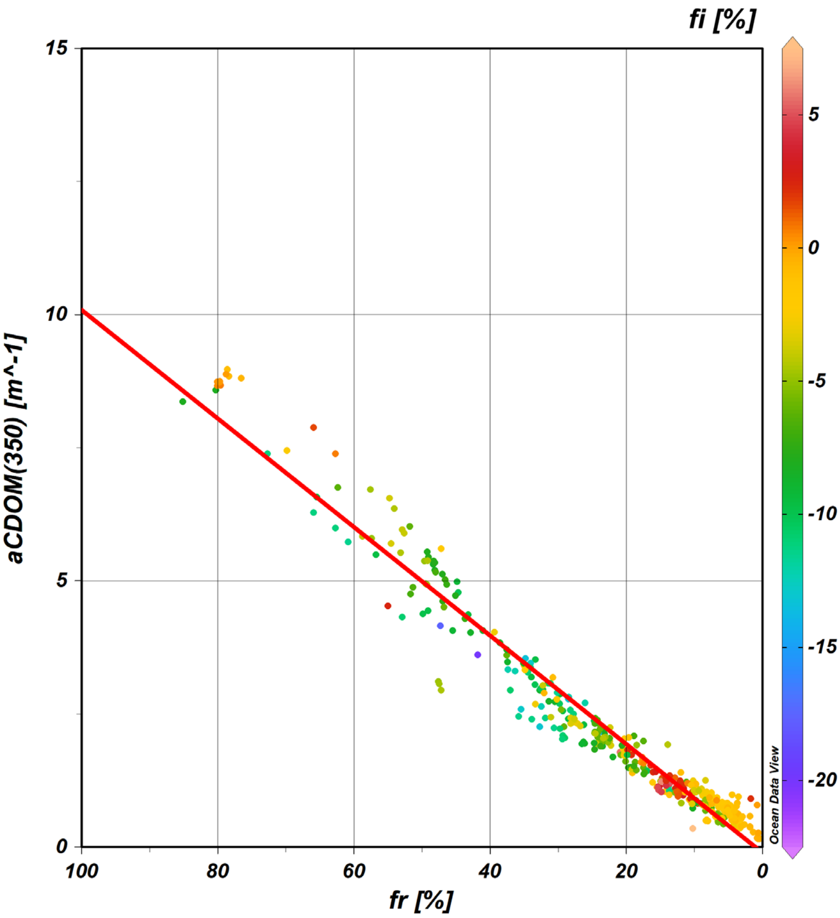

Figure E: Ratio of river water fraction (fr), a_{CDOM}(350), and sea ice meltwater/brine fraction (fi). We will also outline the samples from 2011. (please note: this is not the final version of the figure that will be inserted)

Line 235-236 / Here you have the data to also indicate the potential sea-ice end member in Figs 4 and 5. And mixingline from marine to sea-ice melt. What was the DOC concentration in the sea ice?
The median of the DOC concentration in the ice is 96.2 µmol l^{-1}. We will add this in the Text. This information is actually important because it is the basis for calculating how much DOC is bound in the fast ice of the LS.

Line 235-236 / Also, the comparison to a350 at same salinity is valid in terms of the effect as end-members. But the ice was likely grown into water with a salinity of >20?
No, the ice has probably not grown in water with a salinity of > 20. The ice is formed from surface water with salinities well below 20. For example: In October 1995, the hydrography of the LS was recorded during the onset of sea ice formation (the only existing measurements from this important period, however, without DOM measurements). Salinities in the SE Laptev Sea at this time were well below 15 (see also Janout et al. (2020), Figure 13).

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Figure 6 / Your sea ice data in Fig 6 shows what you could use as a mixing line for sea-ice melt in the Figures 4&5, thus I would introduce the sea ice a350 data already in Figure 4, and use this to add the mixing line. This in turn explains (potentially) the data from the ESS. At least I would show them in the same figure, but different panels, this would make it much easier to compare and review.
We will plot the mixing line in this figure because it shows the data from the Laptev and East Siberian Sea , and for the first time the CDOM data from the fast ice. We will also highlight the data from the ESS again graphically.

Line 244-45 / this was also shown to indicate CDOM loss (Granskog, 2012).
Yes. We will add this in the text.

Page 14

Line 261 / But with salinity alone, you cannot really say this, because you have evident contributions from sea-ice melt (or brine) which could also alter the situation, and thus change your salinity-property mixing line. Thus as suggested above also including the Fr-property plots would allow to make full use of the isotope data collected (cf. Granskog, 2012, analysis in Hudson Bay).
Yes, we will show a fr-property plot and rephrase that sentence. Our data also suggest degradation of DOM on the shelves. In this study, the authors also do not postulate that the tDOM-rich river water in the LS mixes conservatively with the sea water. Instead, we attempt to make clear that the mixing line (fit) is the result of the mixing of multiple freshwater inputs with extremely different DOM concentrations and the sea water.

Line 261 / Why is the situation in the Hudson Bay so different? (Granskog, 2012), with evident loss of CDOM?
Hopefully, this has already been clarified, at least to some extent, by answering the question in lines 54-55.

Line 268 / This apparent conservative mixing is indicated in the (deleted)
We will delete the reference to conservative mixing because the sentence is misleading and rephrase the sentence.

Page 15

Figure 7 / Since you have at least CDOM data, add a panel with S275-295 into this figure.
What are the black bars at two of the station on the top of figure?
The relationship between $a_{CDOM}(350)$, S275-295 and salinity is shown in Figure 6. A supplemental profile with the slope values really does not contain any additional information that would support or contradict the hypotheses of our study. We could show a figure in the supplement.
The black bars indicate the location of the directional changes in the profile (Figure 1). We will explain this in the caption.

Line 280-282 / could this also be explained rapid loss at very early stages of the more labile material during freshet?
This is a quote from Alling et al. (2010). You should ask this question to the authors of this study.

Line 289 / do you mean microbial degradation is a plausible loss term?
Yes. Why would the referee exclude possible microbial degradation?

Line 290-291 / How does this relate to what was observed by Belanger et al.? And: What does the S275-290 data tell you? You show it above, but do not fully explore what the data tells in terms of signs of photodegradation. I would have expected some discussion on the Slope data since it is shown in the Results part.

Belanger et a.l (2006) studied the degradation of tDOM in the Mackenzie River plume to describe the influence of a seasonally steadily decreasing sea ice cover. During the study period, the sea area was ice-free in large areas. Quote from Belanger : "Consequently, the plume was exposed to UV radiation for longer periods of time thus allowing more tDOC to be photochemically mineralized." The authors of this study are explicitly talking about the freshet of the Lena River at this point. Here, the extremely CDOM-rich (and therefore not very transparent) river plume flows beneath the 2-m-thick, snow-covered, completely closed ice sheet of the fast ice. How can there be significant photochemical degradation of the tDOM? Unfortunately, we have no DOM samples taken under the fast ice during the freshet.
The "initial" S275-295 signal coming out of the river during the year is already extremely variable (Juhls et al., 2020). Most studies do not take this into account. Therefore, it is extremely difficult to attribute changes in S275-295 to processes/degradation on the shelves unless we "normalize" it to the river signal. Most photodegradation likely occurs when organic matter is still on land surfaces before it is transported to lakes and rivers and finally to the ocean.

However, to show the rate of degradation of tDOM is not the purpose of this study. Instead, a hypothesis is presented that may help to better understand the observed distribution of DOM concentration in the Laptev and East Siberian Seas. We do not claim that no degradation is occurring. Instead, we can plausibly show that the observed tDOM distribution cannot be explained by degradation of tDOM on the shelves alone. Nevertheless, we will comply with the reviewer's request and explain the meaning of the slope values in more detail in the text.

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Section 4.2. / Please split this one up in a few paragraphs.
We have divided the sections describing the influence of melting and ice formation into three short paragraphs. The authors of this study do not see why these short paragraphs should be further subdivided in any meaningful way.

line 299 / “during sea ice formation” added.
Accepted

line 308 / cross-out
Accepted.

line 314 / “landfast” added
Accepted

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Figure 8 / again, since this is a multiple source system, I think adding the sea-ice melt mixing line is also valuable to show. You also have some data to indicate that end-member.
And as before indicate the uncertainty in the mixing lines with e.g. shading. I would drop the Linear fit, since it is a fit to the data, that might include factors affecting the CDOM, and as such is not "the" mixing line but a "results" of processes acting on the CDOM on the shelf.
We will implement the suggestions of the reviewer. That there are uncertainties in the mixing lines due to the variable concentrations that are described in the text. We will check whether a representation by gray shaded areas does not make the already very complex figure too confusing.

Line 333-334 / I thought the landfast ice start growing early, so it is fairly thick after the winter? You mention 2.0 m thick landfast ice, so I am bit confused here. Please clarify.
At this point we describe the relatively young drifting pack ice north of the fast ice that covers most of the Laptev Sea north of the 20 m depth contour (middle and outer shelf). We will present this

more clearly in the text. We will include the citation of the study by Itkin and Krumpen (2017) that describes why this is so.

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Lines 334-337: Given the complexity of the system the simple property-property plots (figs 4-5,8) are hardly able to explain the situation. Here the approach should rather be on individual profiles, where the "parent" water mass (i.e. end-members) for each one can be more reliably determined, and such the relative contribution of e.g. ice melt can be deduced. E.g. the approach by Granskog et al. (2009) in Hudson Bay was based on "individual" end-members since these vary much in space in such coastal systems with sea ice.
And: Are there data for the isotopic values of the landfast sea ice?
We will present and discuss the O18 data in more detail and use a plots of the river water fraction (meteoric) to explain the situation.

Due to the fact that sampling in the Laptev Sea is practically impossible in the period between mid-October and mid-March, and due to the very complex oceanographic processes, we do not consider the approach of defining different endmembers on the basis of single profiles to be useful for our study.

(please note also the our answer for line 168). We do have measurements of the land fast "sea-ice", but these measurements cannot be used directly as endmember ("parent" water mass) for sea-ice meltwater as they represent a mixture of sea-water and river water contained within the ice. In addition to the analysis of the O18 isotope data, our approach is therefore based on direct measurements of the DOM characteristics in the fast ice and the calculation of the volumes and process which led to the mixing of the different "parent" water masses (freshet, post-freshet and melt water of the fast ice) from direct biogeochemical and oceanographic measurements, and satellite observations. We concluded that the mixing of two of the three freshwater components (freshet and meltwater) results in a freshwater component that corresponds in its DOM characteristics to the third freshwater component (post-freshet river water). This "peculiarity" explains the **seemingly** conservative mixing behaviour of the DOM in the Laptev Sea.

Lines 342-344 / Again, it would have been helpful if there was data on Fr and the deviation from the river water mixing line could be deduced, salinity alone makes it rather difficult to discern what actually causes this deviation.
We will show the river water fraction in the results and also highlight the 2011 samples in the figure to discuss that here. These results do not change the message of this section. We emphasize again that the fast ice includes a significant amount of river water that can exceed 90 percent in regions close to the Delta (Eicken et al., 2005). As described in our study, the meltwater from the fast ice that mixes with sea water with a high brine content north of the Lena Delta cannot be quantified by the O18 signature of the sea water.

Eicken, H., Dmitrenko, I., Tyshko, K., Darovskikh, A., Dierking, W., Blahak, U., Groves, J., and Kassens, H.: Zonation of the Laptev Sea landfast ice cover and its importance in a frozen estuary, Global Planet Change, 48, 55-83, 10.1016/j.gloplacha.2004.12.005, 2005.

Line 261 / What about sea ice conditions during this expedition? (pack ice gone long before cruise?).
Good point. The sea ice condition should be described here too. During sampling in the last week of September in 2019, the ESS was ice-free. During the first week of July 2019, the ESS was still almost completely covered with fast ice and drifting pack ice. The last ice fields on the shelf melted during the last week of July. (www.meereisportal.de)

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Line 376 / is it all landfast ice, or could it also be drift (Pack) ice that is melting in the region?
Yes, it can be both. We will change this in the text accordingly.

Line 383 / must if be landfast ice, or in cases further offshore also pack ice? The pack ice could have even lower CDOM than the landfast ice? E.g. see Kowalczuk et al. Kowalczuk, P., et al. (2017). Bio-optical properties of Arctic drift ice and surface waters north of Svalbard from winter to spring. Journal of Geophysical Research: Oceans, 122(6), 4634–4660. https://doi.org/10.1002/2016JC012589
This sentence refers to the oxygen isotope signal of the meltwater in general. We will write "sea ice" Of course, the meltwater of the sea-ice proportion (positive sea-ice meltwater value) can also play a significant role. However, this O18 signal can also be masked on shelves with high winter ice export and a resulting high proportion of brine in the water (negative sea-ice meltwater value).

Lines 384-386 / Do not quite follow how you derive the amount of DOC "removed" from sea ice, or rather moved with the brine. Please elaborate.
We agree with the reviewer. We should explain this in a little more detail. As a baseline for the water mass from which the fast ice forms, we have calculated the average surface salinity and DOC concentration (0-5 m) of the LS east of the Lena Delta (south of 73.4°N and east of 125.0°N). The basis is the data published in this study. The DOC concentration is the median of the concentrations measured in the fast ice (96.2 µmol l^{-1}). The difference (i.e., the DOC missing in the ice) is approximately 379 µmol l^{-1}. This value was multiplied by the total volume of the fast ice (273 km^3; Kotchetov et al., 1994; Barreis and Görgen, 2005; Selyuzhenok et al., 2005). We will explain this in more detail in the text. This budget is certainly only a first approximation.

LIne 388-389 / what do the actual studies of Giannelli and Müller tell about the change in DOM composition at ice growth?
We will present this briefly in the revised version of the paper.

Lines 390-395 / at what salinity are these brine-rich waters on the shelf? From an oceanographic point of view, these do not then contribute to the formation of Arctic halocline in the Nansen or Amundsen basin?
The seasonal development of density stratification in the Laptev Sea is described in more detail in Janout et al. (2020). Due to the erosion of the density stratification at the end of winter and the mixing with denser water masses (from the western shelf) in the area of the shelf edge, the tDOM-rich brines from the southeastern Laptev Sea are probably diluted and mixed down to greater water depths and can thus contribute to the formation of the Arctic halocline. That this takes place is very plausible but has not been observed so far.

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Lines 407-8 / at what depth? Do they feed to the halocline observed all the way in Fram Strait? And could CDOM be used to indicate where the brine-rich waters originate from?
Due to the possible erosion of the density stratification at the end of winter in the mid-shelf region (water depth > 40 m; Janout et al. 2020) and the mixing of the water column at the shelf edge, the CDOM-rich brines transported northward during winter from the inner shelf of the eastern LS could leave the shelf at depths of up to 100 m (shelf break). The actual water depth depends on the strength of stratification, which in turn is controlled by the position of the Lena River plume and the intensity of ice formation on the shelf. Whether brine and tDOM enriched water masses are then

also dense enough in the eastern LS to sink further (like the brine-enriched bottom waters observed in the western Laptev Sea, Janout et al. 2017) could not yet be substantiated with observations.

The formation of the Arctic halocline is an important and interesting research topic that our group has also been working on for more than 20 years. In this context, we refer to the published studies (and studies cited in this study) by Bauch et al. (2009, JGR), Bauch et al. (2011; Prog in Oceanogr.), Bauch et al (2014), Janout et al. (2017) and Janout et al (2018). In addition, the publications of Igor Dmitrenko and Thomas Krumpen who were members of our working group for a long time are also of interest. These papers provided an important basis for the hypotheses presented in this study. In this context, we are surprised that a reviewer who repeatedly points out that the degradation of tDOM is an important process proposes to use tDOM like a conservative tracer for water mass formation (Arctic Halocline) on the Siberian shelves. We believe that this is a very difficult and challenging approach that would require a much better availability of data from the Siberian Arctic (especially winter data).

Line 413 / outer shelf?
No. The large polynyas of the western Laptev Sea run along the entire coast of the Taymyr Peninsula (from the inner to the outer shelf) and the northern edge of the fast ice approximately along the 20-meter depth contour. There are also large polynyas off the Severnaya Zemlya archipelago. However, the dense water formed there drains directly into the oceanic basins.

Lines 418-420 / Does this mean the winter brine formation never reaches the bottom on the shelf?
Never? I would definitely not say that. We just haven't observed it. The seasonal development of density stratification in the Laptev Sea is described in more detail in Janout et al. (2020).
The bottom water (> 40 m water depth) of the Laptev Sea has salinities above 33. The CDOM maximum in Fram Strait described in Granskog et al. (2012) shows salinities between 32 and 33 which is interpreted in Granskog et al. (2012) as an indication of formation on the Siberian shelves. Thus, our observations are consistent with the measurements in Fram Strait.

Line 428 / and these multiple sources are?
Okay, that should be clear to the reader by now. But the reviewer is right, we should name them.

Page 21

Line 431-433 / are all these changes relative to 1940s? Please be specific which period the rates of change are given for in each case.
That's right. We forgot to name the observational period for Selyuzhenok. The observations refer to the period from 1999 to 2013.

Line 437 / But arguably you also first need a source of brine, thus sea ice formation in the future must also play an integral role? IF there is stratification sea ice can more easily form - but will it ever penetrate the stratification?
Exactly! This was so obvious to us that we forgot to mention it. We will change that.
The strong density stratification in the eastern Laptev Sea ensures that the warm but denser (compared to the eastern LS) surface water from the western Laptev Sea is forced under the river water plume in the east, where it can still be detected well into the winter, affecting ice formation and bottom water temperatures in winter (Janout et al., 2016; Janout et al., 2020). These issues will certainly be with us for a long time in the face of climate change.

Janout, M., Hölemann, J., Juhls, B., Krumpen, T., Rabe, B., Bauch, D., Wegner, C., Kassens, H., Timokhov, L. (2016) Episodic warming of near-bottom waters under the Arctic sea ice on the central Laptev Sea shelf. Geophysical Research Letters, 43(1): pp. 264-272.10.1002/2015gl066565