The study is quite ambitious, in that it covers such a large and data-sparse area. I think that the chemical trends in space and time are interesting and important, and worthy of publication. However, I often felt that the explanation for the causes of the trends were speculative, or at least that the authors' arguments were not explained clearly enough that I believed it. Generally I think that it is very hard to attribute chemical changes to a combination of physical and biogeochemical parameters without specific information at smaller scales. The authors cite many papers that give examples of the relevant processes, but it is hard to tell if these processes are ubiquitous across these large scales, or if there might be alternative explanations for the observed trends. We partially agree with this comment and rearranged our discussion of mechanisms; we also added a big deal of information on smaller scales. We have to admit that given the scale of this study (1500 km latitudinal transect across several physico-geographic and geo-cryological zones), presenting details on each typical site including underground water, soil and vegetation aspects will greatly overload the manuscript. We think that synthetic view as shown in Figure 12 (now Figure 6) helps to present the main findings of this work, which certainly represents a first look on this huge territory and is inevitably based on hypothetical mechanisms evidenced in other regions.

I also did not trust the flux calculations, which were based on a very small number of water samples (one to five samples) and discharge data derived from historic records or incomplete data. We decided to remove the flux calculation and relevant discussion from ouralready long manuscript as they do not add much new compared to what we learn from DOC and major element concentration in rivers.
While there is a compelling dip in fluxes in the discontinuous permafrost zone (Figures 9 and 10), I believe that a robust error analysis needs to be presented to quantify the uncertainty around the chemistry and discharge values, and extrapolation from a few point measurements to an annual solute flux budget. The error analysis and discharge evolution/uncertainties are presented below in our response to comment 3 of this reviewer.

Overall, I would suggest that the authors focus on the chemistry trends, and correlations between the different species which might provide a more robust picture of the processes responsible for chemical trends (eg. linking DOC, DIC, and pH). I think that the discussion could be shortened, with a focus on removing speculation and strengthening the argument for the coupled hydrological and biogeochemical processes that they find most important. We agree and removed a big deal of unsupported hypotheses and useless discussions and treatment (e.g., PCA analysis, flux calculation).

Major Comments
1. Parsing hydrology from biogeochemistry: The discussion includes quite a bit of speculation and relies on complex logic arguments to infer the coupled hydro/biogeo processes responsible for the observed trends. The logic of these arguments is not always clear, and there are many assumptions that are not justified. It is difficult to attribute the trends found in this paper to physical or chemical processes without some other line of evidence. We moderated our hypotheses on links between biogeochemical processes and fluxes; we also added some crucial information on smaller scales (examples, Figs S-8) on cation concentration in soil compartments of the WSL and the Ob river flood plain. Significant

2. Discharge: The methods paragraph on discharge is rather vague. In some cases it appears that discharge was estimated based on data from 1970 to 1972, or from 1973-1992 or was estimated based on discharge in other basins. The number of river discharges determined using these various methods should be quantified, and uncertainty needs to be rigorously considered for each (in order to support the flux calculations.

Perhaps the location and dates for discharge measurements could be included in Figure 1. We feel that the very limited number of element concentration over the year (3 to 4 sampling days only) do not allow us to rigorously quantify the fluxes and even the uncertainties of these fluxes cannot be properly determined. Revised analysis of discharges, their evolution since 1970th and the uncertainty on flux calculations are presented in our response to comment No 3 of the reviewer below. However, we did not include this information in the revised manuscript.

3. Fluxes: Fluxes aren't mentioned in the methods at all, instead they are included in the results, and never fully explain how point measurements are used to determine annual fluxes. It is problematic that discharge is so uncertain, and mostly based on historical data that cannot be directly related to discharge at the time that water chemistry was collected. This suggest major uncertainty in the actual discharge, and thus on the fluxes. The authors report an uncertainty of 30% on the flux measurements, but it is difficult to determine how this value was selected – it seems arbitrary. I think it is likely that this uncertainty is way too small, especially for the spring pulse, where sampling just before or after the peak can lead to incorrectly quantifying discharge and concentrations by orders of magnitude. In many cases, and especially on Arctic rivers, discharge uncertainty by itself may be much larger than 30%, especially for the spring pulse. On top of this, water samples were only collected at 1 to 5 days over the year (and for the fall period were only collected below 60 N (p.10633 line 25)). It seems highly unlikely that these few samples represent seasonal averages, and thus there is large uncertainty in seasonal chemistry, thus compounding the flux uncertainty. For this reason I think that the spring fluxes at least are likely highly inaccurate, and given that the majority of the annual flux occurs in the spring in Arctic systems, calculating annual fluxes based on this data is a real stretch. If the authors can provide many more details on how discharge was quantified, and thus a realistic estimate of uncertainty, it is possible that seasonally-averaged fluxes for the summer and winter could still be meaningful, albeit with very large error bars.

The evaluation of discharge uncertainty included the comparison of an ungauged river with similar river of the region which was monitored by Russian Hydrological Survey and for which the daily and monthly discharges were available. The actual discharges for the period of sampling (2013-2014) are available only for 5 moderate size rivers of the southern, non-permafrost zone as described below. The non-systematic uncertainties of mean multi-annual monthly discharge and mean annual discharges are within 15-20% although for values of discharge on a given month, these uncertainties increase to several tens %.

In case of the RHS gauging station location which was different from our sampling point of this river, we used interpolation of the discharge taking into account the watershed area change along the main course of the river (Methodical, 2007; Svod pravil, 2004). In the absence of the gauging station at the river, we used either analogous river approach or mean values for the area-normalized discharge in the region, given rather homogeneous geographical setting of the WSL (see runoff distribution in Fig. 1). The uncertainties of gauged stations discharges are within 10-15%. However, if the analogy between the known river and the sampled river is not sufficient, which was mostly the case for small rivers, the uncertainties rise to 30-50% and they cannot by quantified given the lack of direct measurements on small rivers. The exceptions are small and medium rivers of palsa and polygonal bogs of the permafrost zone, which were thoroughly monitored during hydrological study of the State Hydrological Institute (1973-1992) for which we used empirical formulas accounting for hydrological parameters of these watersheds (Novikov et al., 2009).

The evaluation of systematic uncertainty on discharge due to well-known trend in Siberian river discharge evolution over past decades was possible only for several rivers of the southern, permafrost-free zone. For the northern part of western Siberia, recent discharges of gauged rivers are not yet available.

The Table below summarizes the increase of annual and selected monthly discharge in 2013-2014 relative the previous 30-50 years, also illustrated in **Figure 1**.

River (Table 1 of the manuscript)	Observations period	Annual	February	May	August	October
Bolshoi Tatosh (RJ-7)	1973-2014*	2.87 m^3s^{-1}	0.736 m^3s^{-1}	13.6 m^3s^{-1}	1.55 m^3s^{-1}	1.49 m^3s^{-1}
	1990-2014	+7%	+9%	+3%	+19%	+19%
	2013	+89%	+11%	+138%	+10%	+44%
	2014	+38%	+26%	-3%	+24%	+31%
Vasyugan (RF63)	1960-2013*	88.1	14.9	415	55.5	52.4
	1990-2013	+2%	+11%	+5%	-11%	+4%
	2013	+28%	-23%	+45%	-34%	-47%
Parabel (RJ-15)	1957-2013*	80.7	21.7	318	60.0	50.0
	1990-2013	+14%	+11%	+20%	+1%	+15%
	2013	+45%	-22%	+17%	-16%	+73%
Bakchar (BL-2)	1974-2014*	5.34	0.214	30.2	2.59	2.80
	1990-2013	+12%	+17%	+4%	+31%	+23%
	2013	+37%	+8%	+23%	+5%	+26%
	2014	+31%	115%	-18%	-95%	-92%
Chaya (RJ-10)	1953-2013*	79.7	23.1	358	48.2	45.5
	1990-2013	+8%	+20%	+8%	+3%	+15%
	2013	+92%	-29%	+76%	-5%	+137%

* The upper line represents the mean absolute value change over the full period of observation
The 2nd line of each river represents the relative change in % for the period 1990-2014 or 1990-2013 relative to the full period of observation, in %.
The 3rd and 4th line for each river represent the excess of 2013 and 2014 relative to the average of full period of observation, in %.

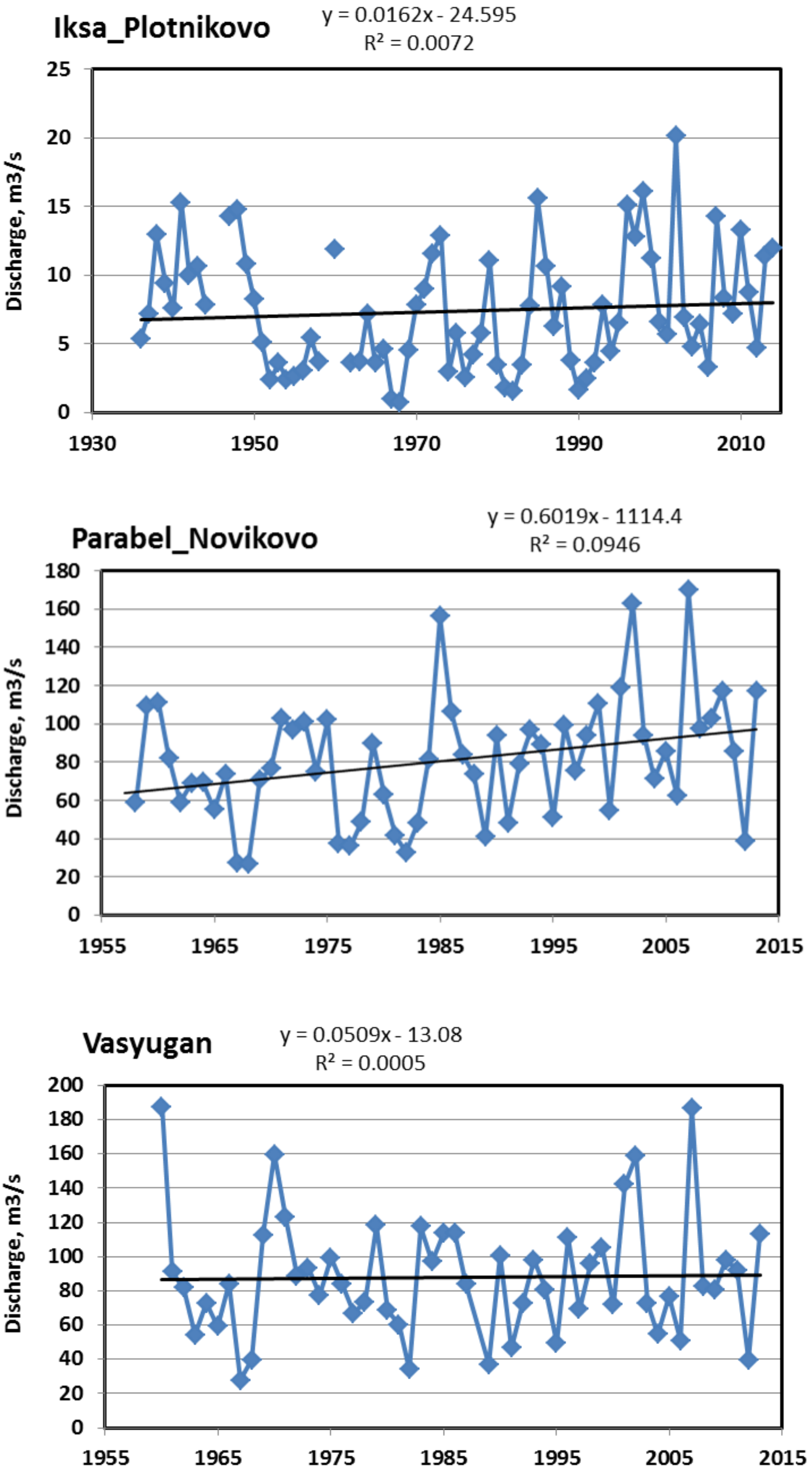

Figure 1. Evolution of the annual discharge of southern (permafrost-free) rivers of the WSL

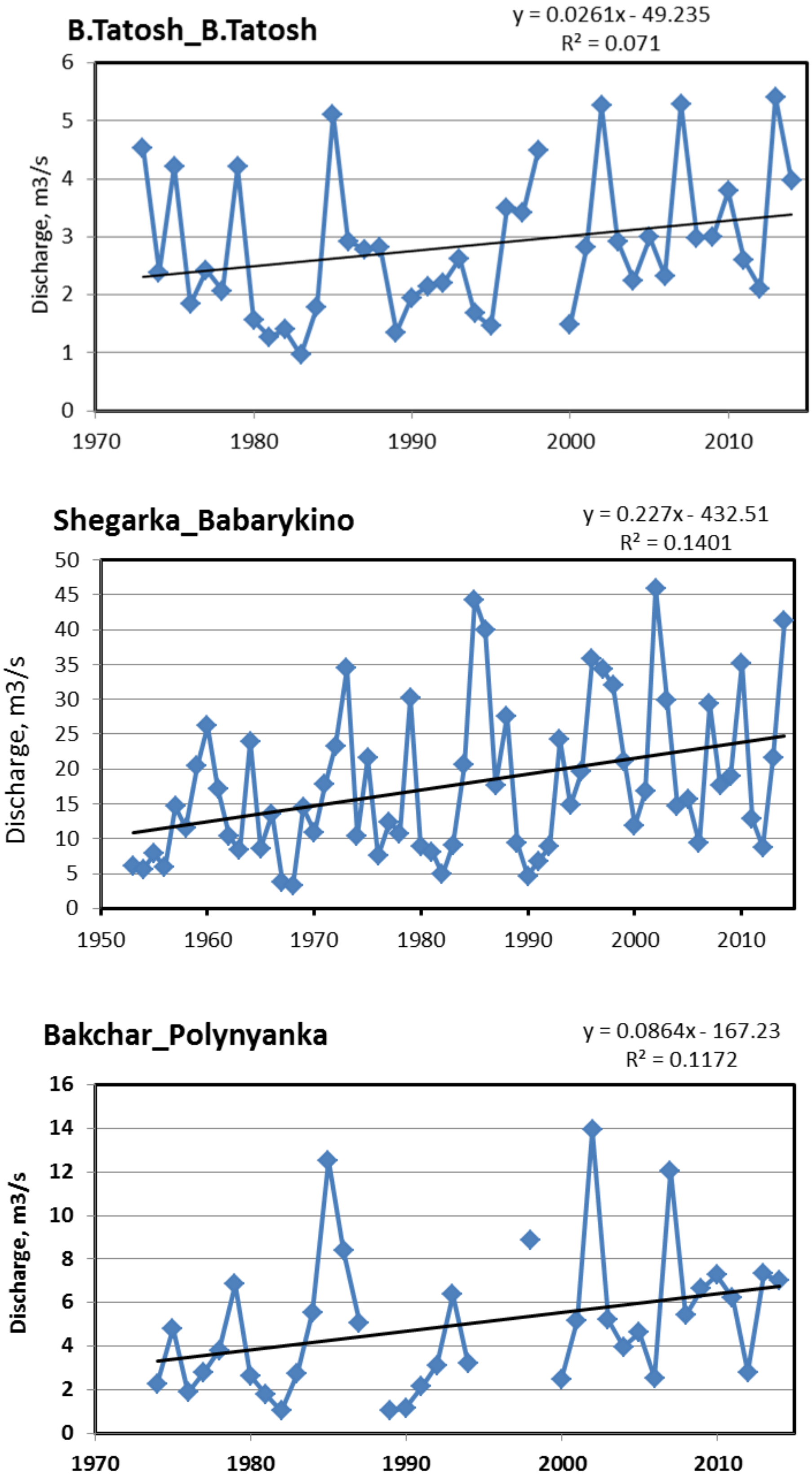

Figure 1, continued.

The ratio between the discharge in 2014 and the average value for 1995-2014 ranges from 1.75 (Shegarka) to 1.2 for other known rivers of the southern part of the territory.
For individual months, this ratio is subjected to much larger variations: February, from 2.24 (Iksa) to 1.1 (B. Tatosh); May, from 1.29 (Shegarka) to 0.84 (Iksa); August, from 0.92 (B. Tatosh) to 0.33 (Shegarka) and 0.09 (Iksa); October, from 0.21 (Iksa) to 0.30 (Shegarka) and 1.02 (B. Tatosh).
As such, the realistic uncertainties of flux calculation even in southern most studied zone of the WSL, when using mean multiannual values of past years may vary from 10 to 80% and cannot be rigorously constrained without thorough hydrological measurements.

The evaluation of the systematic uncertainty on water discharge in northern, permafrost-affected rivers is more difficult as only large rivers were properly monitored. The change of flood period discharge in the north does not seem to evolve over past decade (although the data are quite scarce) whereas the information on baseflow flux is insufficient (Zakharova E. A., Kouraev A. V., Kolmakova M. V., Mognard N. M., Zemtsov V. A., Kirpotin S. N., 2009. The modern hydrological regime of the northern part of Western Siberia from in situ and satellite observations. International Journal of Environmental Studies, 66/4, 447-463. DOI:10.1080/00207230902823578) as shown in Fig. 2:

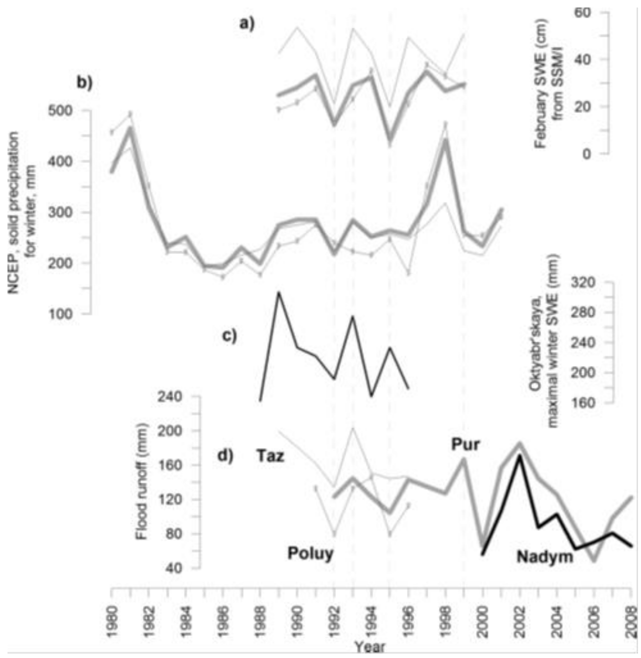

Figure 2. Inter-annual variability of (a) mean February SWE from SSM/I, (b) NCEP winter precipitation, (c) maximal winter SWE (mm) from forest transects at station Oktyabr'skaya and (d) flood flow (mm) for rivers Poluy, Nadym, Pur and Taz (Zakharova et al., 2009)

It is important to note that the discharge may also decrease in the north due to the snow meltwater lost (Fig. 3).

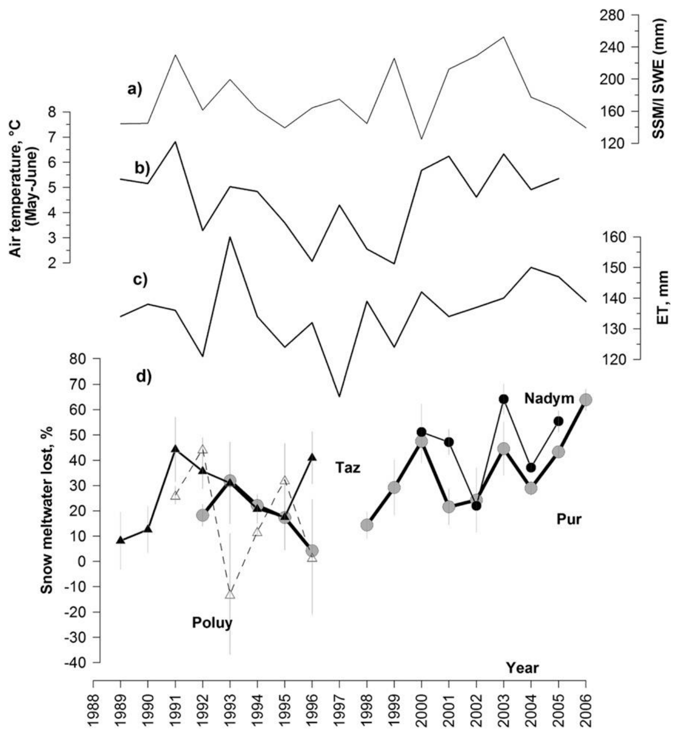

Figure 3. Yearly values for 1989–2006 of the (a) SSM/I SWE (mm) in March; (b) air temperature (°C) in May–June, average value for all PNPT watershed, NCEP reanalysis data from ArcticRIMS (2009); (c) ET (mm) from ISBA-TRIP simulations; and (d) portion of snowmelt water lost (in % to the total amount of water stored in snow cover for each year). Thick black line with circles: the Pur, black line with circles: the Nadym, black line with triangles: the Taz, and dotted line with diamonds: the Poluy. Vertical gray lines: error bars [Zakharova, E. A., A. V. Kouraev, S. Biancamaria, M. V. Kolmakova, N. M. Mognard, V. A. Zemtsov, S. N. Kirpotin, B. Decharme, 2011: Snow Cover and Spring Flood Flow in the Northern Part of Western Siberia (the Poluy, Nadym, Pur, and Taz Rivers). J. Hydrometeor, 12, 1498–1511]

The most probable error of river discharge evaluation is 50% for small rivers and 30% for large rivers. However, in individual cases, this error may be 10 times higher due to significant difference between the mean multi-annual discharge for the data of sampling and the actual discharge.

Taken together, the poor knowledge of actual and modelled river water discharges across the gradient considered in this work, and clearly insufficient seasonal resolution of collected hydrochemical data preclude us of presenting the fluxes of river dissolved components in revised version. Now we are preparing flux calculation paper based on historical and current Russian Hydrological Survey observations and our measurements in 2013, 2014 and 2015 with sufficient seasonal resolution in southern, permafrost-free zone. The work on fluxes in northern rivers is under progress.

4. Objectives: Objective 2 intends to relate chemistry to active layer depth, peat thickness, and permafrost coverage. However, these were not determined in this study and no data is provided from other studies. I don't think that this objective can be properly tested without this data. And how was permafrost extent determined? Likely based on latitude.
We have to note that western Siberia is among the best studied areas in terms of geo-cryological information, mostly linked to extensive engineering work since 1950th, on oil and gas industry in this region. The extend of permafrost coverage in WSL, which is not a simple function of latitude, is known from available results of decades of extensive geo-cryological research, now added to revised section (Baulin et al., 1967; Baulin, 1985; Gruzdov and Trofimov, 1980; Tyrtikov et al., 1979, Liss et al., 2001; Romanosvky et al., 1983; Fotiev, 1989, 1991). This information is based on thorough drilling network, soil and ground temperature measurements and modeling of the 0°C isotherm in the ground. Detailed maps of the active layer depth, peat versus silt coverage and ground freezing temperature are presented below (Supplement to response). However, we do not have quantitative estimation of permafrost coverage and active layer depth for each sampling river. This is especially true for small rivers (< 1000 km² watershed), representing ¾ of all sampled rivers. However, because the evaluation of the permafrost type distribution effect on river water chemistry is still possible thanks to significant geographical coverage achieved in this work, in response to this comment, we reformulated the 2nd objective as following: "The second objective was to assess the effect of the permafrost coverage on DOC, DIC and its isotopic composition in rivers during different seasons." (L 110-112)

So correlating latitude to permafrost extent (Figures 6 and 7) doesn't seem all that meaningful or useful. Especially since you didn't quantify permafrost extent. This is standard representation of river water chemistry in western Siberia, first proposed and justified in works of Karen Frey and co-authors. In addition to summer-period observations of former authors, our study includes different and contrasting seasons. Figures 6 and 7 represent synthetic view of river water chemical composition dependence on latitude and permafrost extend. However, following 1st and 2nd reviewer recommendations, we moved Figures 6 and 7 from revised version to the Supplement.

<u>Minor comments</u>

10622-4: Samples were not collected in Autumn over the 1500 km latitudinal gradient according to the text (p. 10633, line 25). We corrected the text accordingly (L33, 158-159)

10622-11: I'm not sure which trend "The trend of inorganic components was " refers to. We referred to the northward decrease of concentration; corrected accordingly (L39-40)

10622-15-17: This logic is not totally clear. We revised as following: "Because the degree of the groundwater feeding is different between large and small rivers, we hypothesize that, in addition to groundwater feeding of the river, there was a significant role of surface and shallow subsurface flow linked to plant litter degradation and peat leaching". L 46-49

10622-22: Maybe change 'until' to 'below'. Fixed.

10623:3-18: Much of this paragraph seems reasonable, but speculative and not related to the results from this study. I would suggest removing or at least greatly abbreviating this paragraph. We agree and shortened this paragraph in 2 sentences via removing mostly speculative predictions, partially moving them to Discussion (L 49-54)

10625-13: Year for 'Frey et al.'? 2007a, b – fixed.

10625-17 – Is river size really the only influence on groundwater discharge? It seems like regional topography and sedimentology should also play an important role. This is true. We added the following explicatory sentence: "This is especially true in the WSL, exhibiting highly homogeneous, extremely flat topography and similar lithological cover (peat, sand and silt) (L 107 - L 110).

10625-17 – This assumes that a talik exists in all cases. Here we intended to say that ".. it can be suggested that the impact of groundwaters via taliks will be mostly visible on large rivers, as it is also known from the geocryological studies of the WSL (Fotiev, 1989, 1991). Corrected in L 104-106

10625-18-19 – unclear how the previous statement leads to this one. We added necessary explanation in L 105-107

10626-15: What's a 'flood zone'? Corrected to "second largest flooding territory". L131

10629-23: What were you comparing with the ANOVA? The ANOVA was used to reveal the differences between different permafrost zones. It was carried out using Dunn's method because each sampling period contained different number of rivers. Added in L 201-202.

10629-26&27: 'elemental' should be changed/removed, because some of these are compounds. Corrected to "dissolved component" and "DOC and major element"

10630-9: By 'normed' do you mean 'normalized'? Yes, normalized. The PCA analysis was removed from revised version.

10630-14-20: This information should be in the methods rather than results. Agree and moved accordingly.

10631-14-16: It is unclear if the ranges refer to the seasons or the three factors. And the major PCA factors should add up to a large value (towards 100%) to be meaningful – here the three factors only add up to 12% at best, suggesting that they are telling you very little about the variance in the data. This makes me question the analysis and its utility. We basically agree with this comment. Given low number of variables for this analysis and low capacity of 3 factors to explain the variability, we removed the PCA part from the revised text.

10631-18: "the first factor which is presumably latitude" It is unclear exactly what Table S2 is showing (the legend provides little guidance), but it appears to be the correlation coefficients between various factors and the principal components. If my guess is correct, then it is not a good assumption that the first factor is latitude - Latitude appears to correlate well to the first component in the spring and winter, but not in the summer. Furthermore, many other variables including calcium, DIC, and Mg have higher correlations that are consistent across seasons. We agree. However, we did notice that the latitude exhibits the lowest impact on inorganic components in summer (section 3). The pattern of these elements is indeed highly consistent across seasons. We removed this § from revised version as it provides relatively little insight on environmental factors controlling DOC and major element concentration as a function of season and latitude.

10632-5-8: This information should be in the methods rather than results. We agree and moved it to Methods (end of section 2.4, L 208-215).

10632-22-25 and 26-27: This information should be in the discussion. We agree and moved this text to the Discussion (now L 430-437 in section 4.3).

10633-1-9: This information should be in the methods. At this point, there is no mention of flux determinations at all in the methods. We removed the part on fluxes from revised manuscript. See our response to general comment No 2 and 3 of this reviewer.

10633-7: It's possible that the contributing area changes in time, but the watershed area should not change. We agree. This source of uncertainty is especially important for spring period fluxes on extremely flat territory of western Siberia.

10633-24: It seems important to highlight that October samples were only collected in 12 of the 96 rivers, all below 60 N. This has major implications for the results and conclusions of this study. We added an explicatory sentence in the "Method and site description" (beginning of section 2.2, L 157-159). We do not agree with the reviewer that "…October sampling has major implications for the results and conclusions", since the relative contribution of the October period to the total annual flux does not exceed 10% which is smaller than the uncertainties of flux calculation (min 30%, see response to general comment 3). In any case, we removed the flux part from revised manuscript and we clearly stated that October concentration data were used only for general treatment of the permafrost impact on riverine component concentrations (L 226-228).

10633-14-29: There are no results here. This is a mix of methods and discussion. We did not move this information in the Methods because we deleted the fluxes part. We agree with the opinion of both reviewers that the uncertainties associated with our calculations of fluxes are too high to provide new insights compared to what can be learned from concentration pattern.

10633-10634: "The dominant factor controlling the uncertainty of seasonal flux was the standard deviation of the average value of individual rivers". To what does 'value' refer? Also, I think that a much more thorough and transparent uncertainty analysis is necessary to support the reported fluxes. In response to this comment, we provide such an analysis in response to comment No 3 of this reviewer (see above). However, we do not present the fluxes in revised manuscript.

20 – The second and third factors each explain less than 5% of the variability? That is really small. Are they actually important? We consider that 5% variability is too low for explanation power of factorial analysis. As such, we removed the PCA part from revised version.

10646-11: Isn't the highest discharge in the spring? What do you mean by seasonal discharge? The spring (May-June) in the south corresponds to maximal discharge but this is not the case for the northern rivers of the permafrost zone, where ice can persist until August-September. In northern rivers, the highest discharge may be July-August depending on the year.

10647-11: 'Reactive' is not the appropriate word. Maybe 'active', or simply '

the mineral layer is 'frozen'. Corrected as 'frozen'.

10647-25: This seems rather speculative. Warming will also increase peat growth, leading to greater insulation of frozen boundaries. We agree and present it as hypothetical short-term scenario. The peat growth and formation that may insulate the frozen minerals will take thousands of years, compared to first hundred years changing scenario. (L 526)

10648-1-2: Generally, clay is impermeable and should not result in greater infiltration. Agree and modified the sentence as "involvement of upper clay horizon and sand/silts in water pathways within the soil profile" (L527-528). In fact, water migration on the surface of the unfrozen clay and iron-hydroxide coated sand may be sufficient to deplete the solution in DOC via adsorption.

10648-7: I agree that sorption will be greater for DOC, but water moving across this mineral boundary may also leach and transport other solutes. We agree. For this reason we stated that "To which degree this change of water hydrological pathways in the soil column may affect the other dissolved components cannot be predicted. However, this effect for inorganic solutes is expected to be lower than that of DOC, given much lower affinity of HCO3, cations and Si to clay surfaces and the lack of unweathered (primary) silicate rocks underneath peat soil column." In revised version, we alerted the reader about the possibility of leaching of inorganic solutes from the mineral layers and provided a pertinent example from the Yukon River watershed (L 534 - L 538).

10648-14-25: I'm not sure that this paragraph is needed. It doesn't tie well to the rest of the story and while it's interesting, it's unclear why we're being told this. The CO_2 consumption from the atmosphere and its transport to the ocean in the form of bicarbonate is equally important for global C cycle as the organic carbon transport. However, we agree with the reviewer and deleted this paragraph.

10648-28: What about Walvoord references from 2007 and 2012? We overlooked these important references on the groundwater feeding increase in the Yukon River basin and we thank reviewer for pointing them out. Added in revised version accordingly (L545-546)

10649-1: But won't ET also increase with the potential to balance increasing precipitation and runoff? Yes, but this effect will be most visible in forested, permafrost-free zone. The northern sites covered by tundra and forest-tundra vegetation (moss lichen, dwarf shrubs) are unlikely to greatly enhance their evapotranspiration.

10649-2-3: How does your data support this statement? The increase of the water runoff due to climate warming is a very common phenomenon. In our response to reviewer's comment No 3, we presented the increase of water discharge for southern rivers. We do not possess the temporal evolution of the discharge data in the north, but we make an analogy with other arctic and subarctic sites. Our main message in this sentence is that any possible modification in the annual runoff will be within the uncertainty of flux evaluation and thus can be neglected. We removed this sentence from revised version.

10649-5: Vegetation productivity is also likely to affect C export. Why isn't this considered? Here, we share the opinion of Lal et al (1996) (Soil Processes and the Carbon Cycle, R. Lal, J.M. Kimble, R.F. Follett, B.A. Stewart) that the effect of climate change on DOC is uncertain: DOC production will likely to be increased because of higher net primary production and temperatures, but changes in export from soils and catchments will be primarily dependent on variations in runoff, reflecting changes in precipitation and evapotranspiration. We think that speculating on the link between DOC and vegetation in the WSL is beyond the scope of this work and will greatly overload the "hypothetical" part of the paper.

Table 1 – What do the codes under the months represent? And how was the annual runoff calculated? The codes represent the identification of the samples shown in Fig. 1; we explained it accordingly. The annual runoff was calculated following the approach of Frey et al (2007b) as we stated now in revised version. For southern rivers of the region, in the permafrost-free zone, the annual runoff was taken from available data of RHS in 2013-2014 and calculated for ungauged rivers using analogous approach (Supplement 1 of revised version)

Figure 1:
It is very difficult to distinguish winter and spring sampling markers. We greatly revised this figure to better distinguish sampling mark of different seasons for the same river. Note that, because the same river was sampled several times exactly in the same place, viewing all sampling points over 1500 km profile is possible only with high screen resolution (at least 200 %).
What is a runoff contour line, how is it calculated, and how was it used in this study? This isn't mentioned in the text. The runoff contour lines are based on results of Russian Hydrological Survey gauged river monitoring in the region as summarized and compiled in Nikitin and Zemtsov (1986). We added an explicatory sentence in revised text (L 213 - L 214)
It is hard to distinguish the rivers and their names. We labelled major rivers of the region in the revised Figure 1b. Indeed, the river names (except 4th rectangle of southern region) were absent in the first version of the ms.
Where is the Kara Sea basin from which you used data? All sampled rivers of western Siberian Lowland belong to the Kara Sea basin, now stated in L 157-158.
1b- Again, the rivers are not well labeled, difficult to distinguish, and it's unclear how this relates to Figure 1a. The four rectangles in Fig 1 a are detailed in Fig 1 b. We explained it in revised figure caption.

Figures 2 – 4: Why is autumn not included here? The latitudinal coverage of October is too short and does not add much useful information. **Is it worth even mentioning Autumn in the text if it can't be used in the analysis?** It was used for statistical treatment and for assessing the permafrost impact. We added an explicatory sentence in revised text (L 226-228) 262)
Figures 9-11: I don't believe these values at all. And the methods don't mention quantifying an annual load. How can you have any confidence in this based on the fact that 1) the rivers were only sampled 1 to 5 times over the year and 2) the discharge data is often from 20 to 40 years earlier than the chemistry data? Given significant uncertainties associated with current flux estimations, we removed these figures and relevant text from revised version. See our detailed response to comment No 3.

Figure 12: Are these two cross sections really representative of the entire domain? They are very detailed, but how representative are they of all of the variability in the system? Yes, these two cross sections are highly representative for two most contrasting cases of flux formation, corresponding to coniferous taiga in the permafrost-free zone and frozen bog peatlands of continuous permafrost zone, both located at the very flat watershed divide. We added this explanation in revised text (L 482 - L 488). We would like to underline extremely homogeneous physico-geographical setting of western Siberia Lowland. The purpose of this

figure was to illustrate two most contrasting sites; the other sites of discontinuous and sporadic permafrost zone will represent intermediate cases.

We are grateful to reviewer No 1 for his/her very constructive comments.

Supplement to Response to reviewer No 1

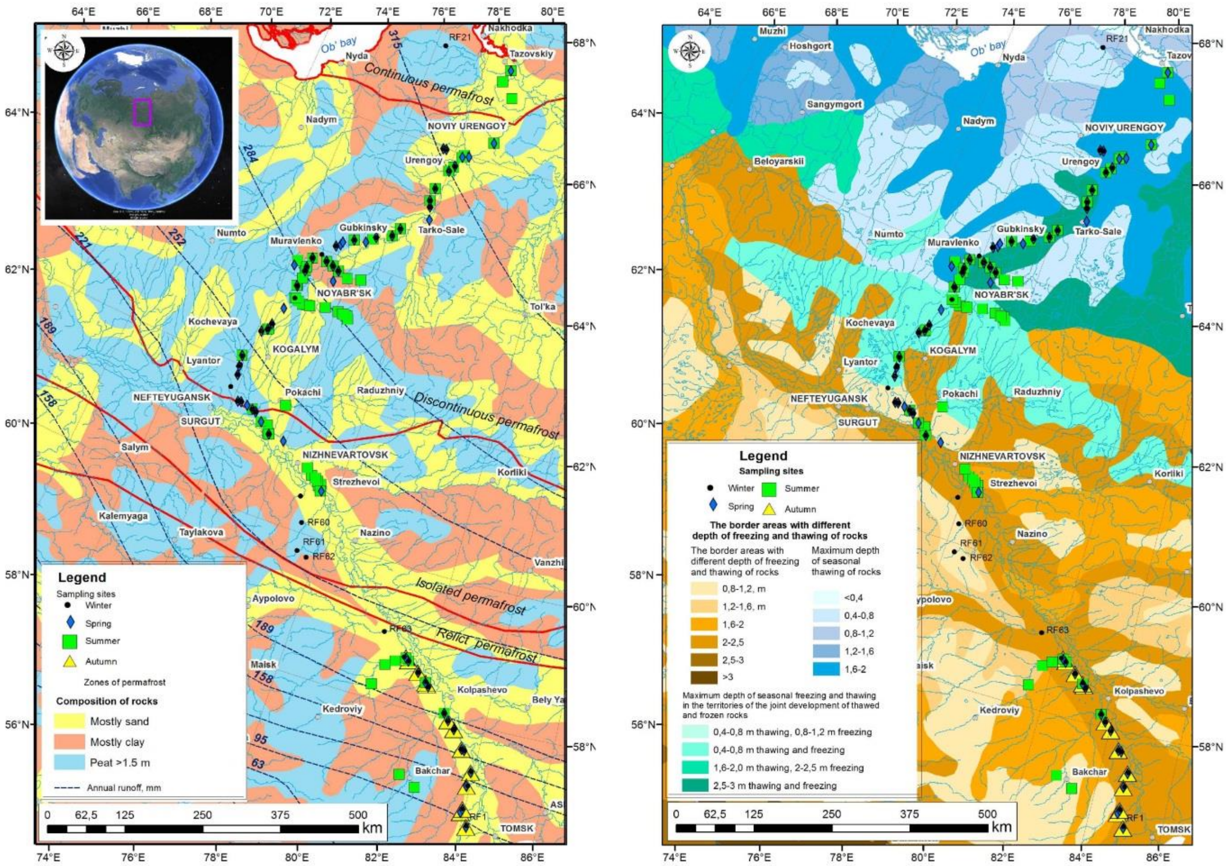

Figure S1. Detailed lithological and geocryological map of the region. Please use 200% zoom to view these maps.