Modeling the interannual variability of Maipo and Rapel river plumes off central Chile

Julio Salcedo-Castro, Antonio Olita, Freddy Saavedra, Manuel Castillo, Gonzalo S. Saldías and Raúl Cruz-Gómez

Response to reviewer 1's comments

This manuscript investigates the dynamics of the river plumes of Maipo and Rapel rivers, in central Chile, using numerical simulations. The model chosen is the CROCO, with detailed horizontal resolution (1 km) and appropriated number of vertical levels (20 levels in this sigma coordinate model). The main focus is in the interannual variability of the plumes. Considering the importance of the plumes in affecting biological communities, I believe this is an interesting topic of investigation and deserves attention. However, some work is still need before publication and my comments follow below, separated in major and minor concerns.

Major issues:

1. One of the purposes of this study is understanding the rive plumes dynamics in an interannual variability scale. However, the authors only provided 9 years of simulation which, in the best scenario, is capable of record 3 or 4 interannual events/cycles. I believe this period is very short for this purpose;

R: Thank you for your comment. Please note that despite the relatively short period of time, our simulations encompass contrasting years with El Niño/La Niña affecting the river discharges and the plumes in the coastal ocean. We agree that 9 years is a short period to capture any significant impact by the PDO in the study area (although it shows a clear interannual variability which correlates with the ENSO). However, the impact of El Niño and La Niña showed marked contrasting effects on turbid river plumes off central-southern Chile during the period of study (see Saldías et al. 2016; "Satellite-measured interannual variability of turbid river plumes off central-southern Chile: Spatial patterns and the influence of climate variability"). In fact, the years of 2005 and 2006 were very wet periods with anomalously high rainfall and peaks in river discharges. The river plumes were anomalously big during those years in response to the freshwater availability. In fact, these conditions promoted flooding events in central Chile. In contrast, periods such as 2007 and 2011 were characterized by anomalously low signatures of freshwater plumes from the satellite observations which coincided with the influence of La Niña and a lack of rainfall in central-southern Chile (Saldías et al. 2016). We included these ideas in lines 104-106 and lines 272-279)

2. Although the simulations seem reasonable, there is no comparison with any data set. For this period (2003-2011), the authors can find satellite SSS and SST, which could be compared to the simulation results and give some idea of the actual performance and limitations of the model. I strongly believe this is a necessary step to give more confidence on the results to other researchers interested in the subject.

R: We agree with the reviewer about the convenience of contrasting the model outputs with observations, ideally, remote sensing data. As SSS data are only available from 2015 on and these datasets still need validation, it is not possible to verify the model against sea surface salinity. Alternatively, we indirectly evaluated the model's performance by means of the reflectance associated with suspended sediment, considering that the sea surface salinity (SSS) is tightly correlated with this variable in river plume regions. According to previous studies (Saldías et al., 2012, 2016, Masotti et al 2016), total suspended solids are better sensed in the 645 nm band; therefore, we worked with the same band. We computed the 99^{th} percentile of the 645nm reflectance in the model domain to compare it with the total river discharge and model output (SSS and total river plume area).

Firstly, we tested the validity and consistency of MODIS imagery by comparing the 99^{th} percentile of the 645nm reflectance in the model domain against the river's discharge (Fig. 1).

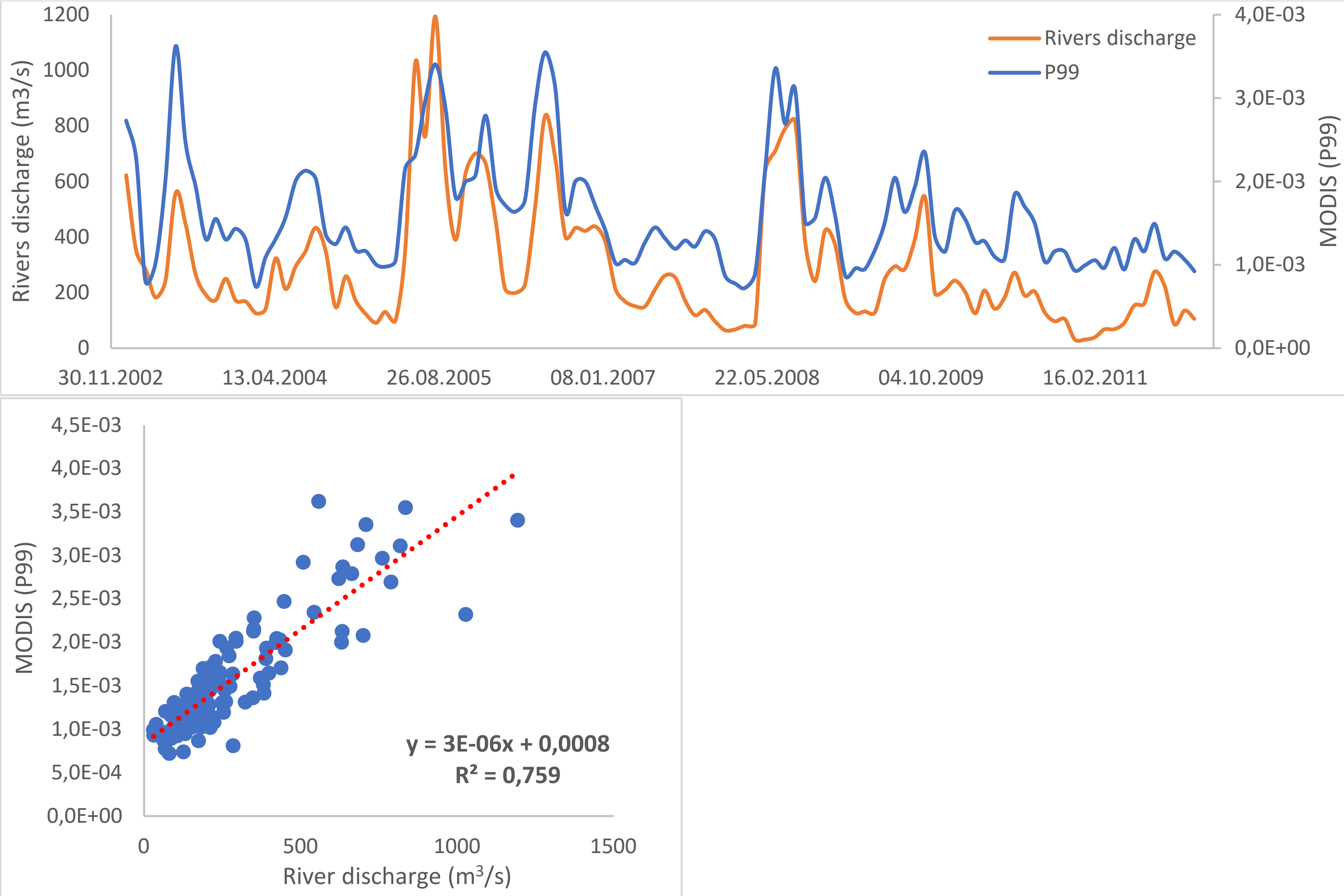

Figure 1: Time series (upper panel) and scatterplot (lower panel) of rivers discharge and plume reflectance (percentile 99) at 645 nm from MODIS imagery.

There is a good correlation between the rivers discharge and plume reflectance (percentile 99) at 645 nm from MODIS imagery, which supports the applicability of MODIS imagery to evaluate the plume. This consistency is also observed when comparing the mean plume sea surface salinity versus satellite reflectance (MODIS, 645nm), confirming the inverse relationship between the salinity and the plume extension (Fig. 2)

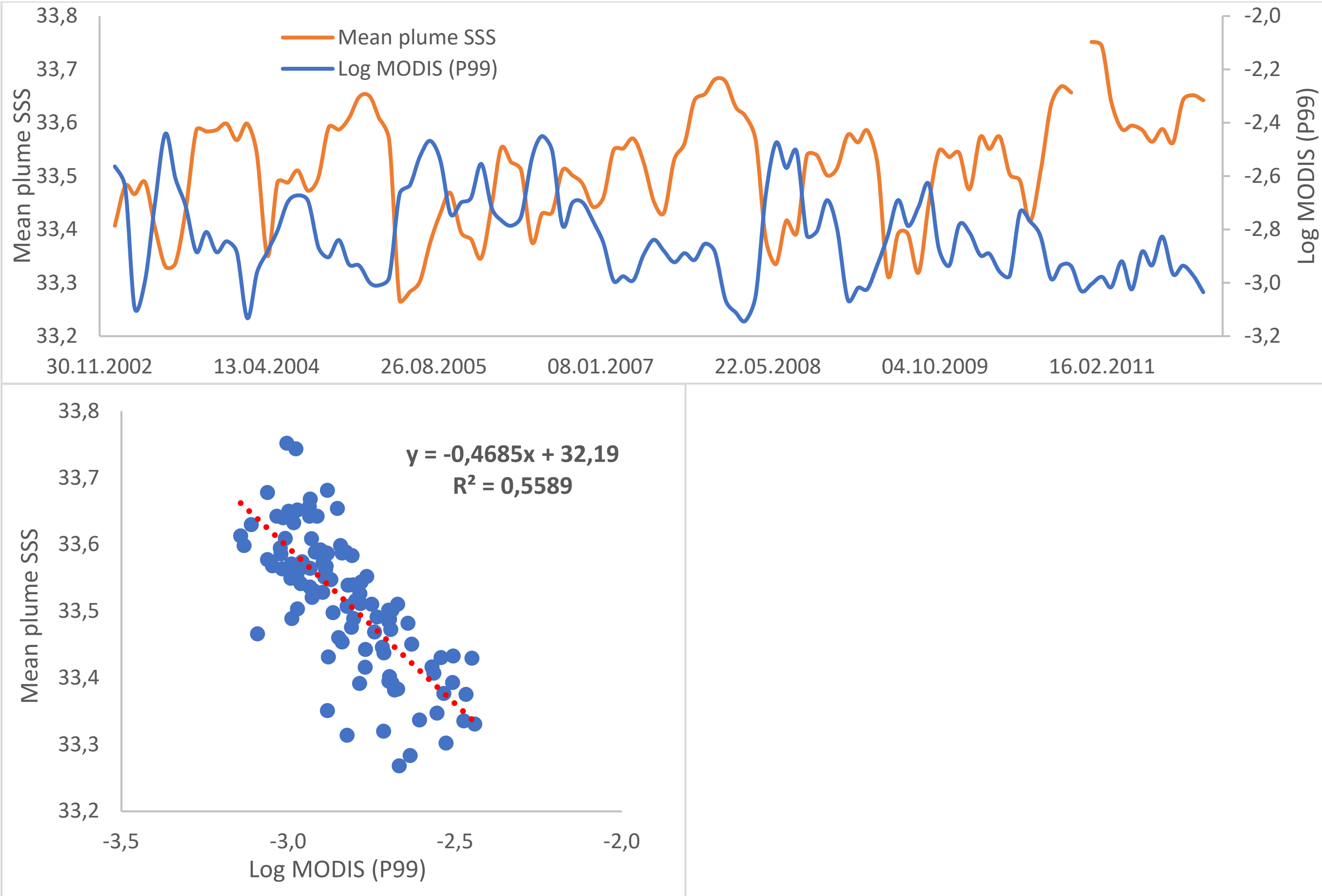

Figure 2: Time series (upper panel) and scatterplot (lower panel) of mean SSS and plume reflectance (logarithm of percentile 99) at 645 nm estimated from MODIS imagery.

Next, we compared the plume extension estimated from the model versus the plume reflectance (percentile 99) at 645 nm from MODIS imagery. It's worth noticing that the plume estimated from the model corresponds to the area delimited by the isohaline of 33.8, as explained in the manuscript. This comparison is shown in Fig. 3, where both series have a good agreement.

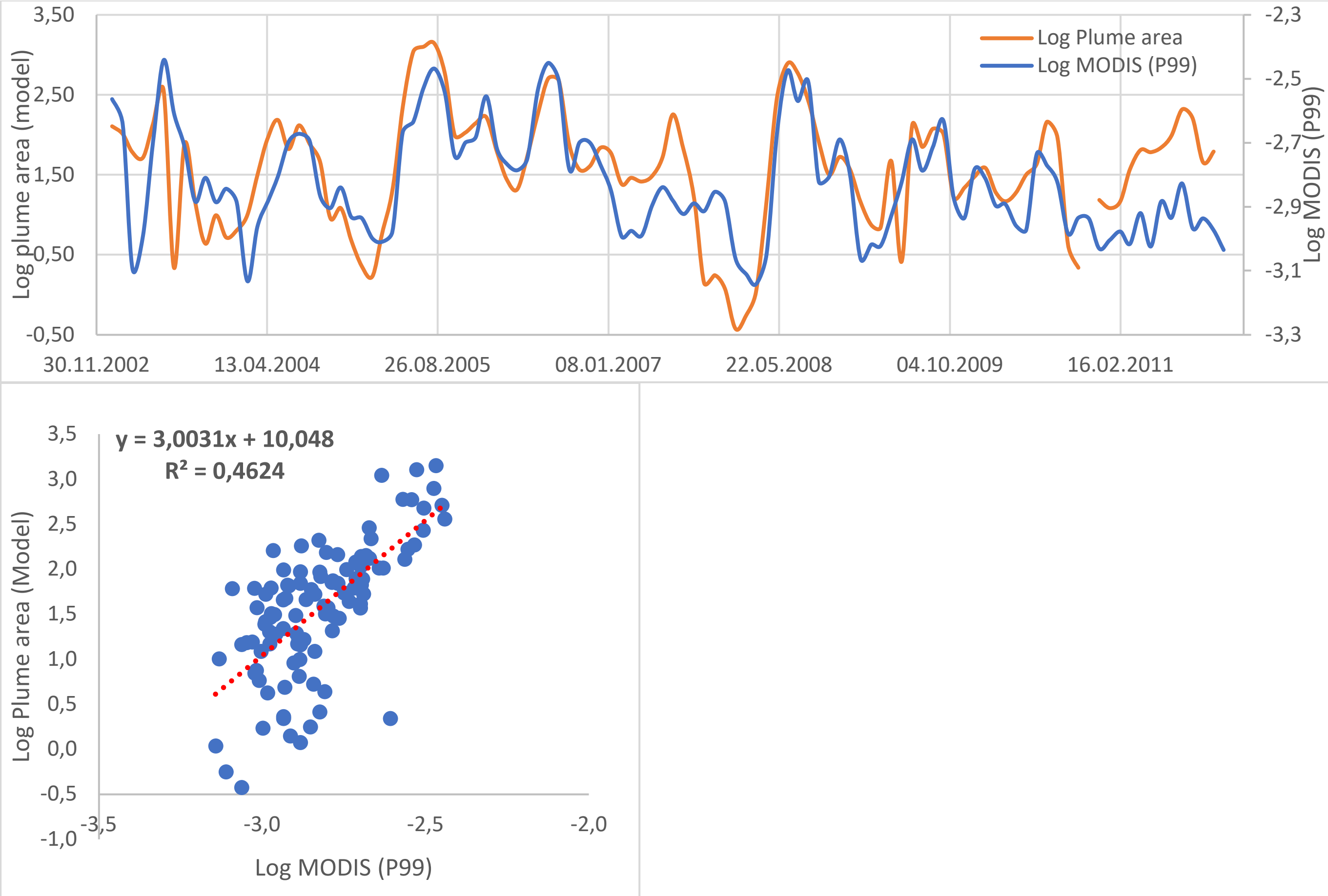

Figure 3: Time series (upper panel) and scatterplot (lower panel) of plume area (logarithm) and plume reflectance (logarithm of percentile 99) at 645 nm estimated from MODIS imagery.

Finally, by using the TSS/discharge relationships described in Tapia (2020), we could estimate the monthly mean flux of total suspended solids and compare it with the mean SSS of the plume. As observed in Fig. 4, these results are consistent, which verifies the acceptable representativeness of the model.

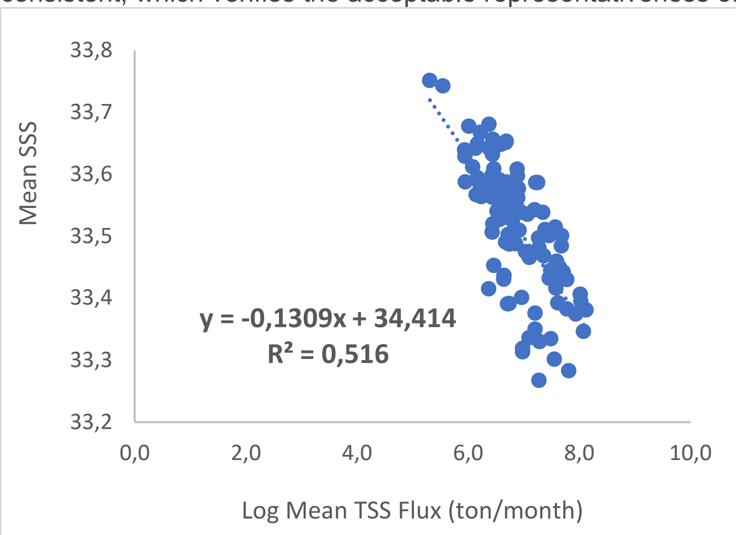

Figure 4: Time series (upper panel) and scatterplot (lower panel) of plume area estimated from model and MODIS imagery.

In conclusion, the model captures reasonably well the temporal and spatial variability of the area. Even though these are qualitative evaluations, they show that the model results are consistent with river and satellite observations. We included these ideas in an additional paragraph (lines 146-158).

References

Tapia, Paulina. 2020. Sediment flux in the land-ocean coupling in Central Chile: Maipo River as a study case. Honors Thesis submitted as fulfilment of Bachelor Degree in Geography, University of Playa Ancha. Valparaíso, Chile. 84 pp.

Minor issues:

1. Line 3 (Abstract): “In the central part” instead of “In the central parto”;

R: Corrected (Line 3)

2. Line 23 (Abstract): “Study” seems more appropriated then “work”;

R: Corrected (Line 24)

3. Figure 1: The resolution of it is very coarse and should be improved;

R: Corrected.

4. Equations (1) to (6): Every term in the equation should be clearly stated, even when it looks obvious. Some of the terms are not described;

R: Corrected (Lines 115-128).

5. Line 124: Reference is given as “(e.g. Olita et al. (2011a))” and should be, I believe, “(e.g. Olita et al., 2011a)”. Please, check if are other references with the same problem;

R: Corrected (Line 136)

6. Line 127: Considered instead of considering?;

R: Corrected (Line 139)

7. Figure 3: Units should be mentioned, preferably in the color bar;

R: Corrected.

8. Figure 3: Why logarithmic scale? Is this really the case? It does not look like and, if so, I’d suggested using linear scale;

R: Corrected.

9. Figure 3 (legend): Avoid terms such as “logically associated to”. Everything either needs a reference or be proven;

R: Corrected (That sentence was removed).

10. Lines 140 to 145: The authors give a limit to salinity values to define the river plume, which is fine. I believe, however, that a better description of the SSS in the region should be part of the Introduction, so the reader can better understand why that limit was chosen.

R: Corrected (Lines 63-65 and lines 168-175).

11. Line 165: The first two sentences are repetitive. Please, rephrase them;

R: Corrected (Lines 197-199).

12. Line 167: 66.7, 9.1, 5.5 and 4.9 are percentages, correct?

R: Corrected (Line 199).

13. Figure 10: The resolution of it is very coarse and should be improved;

R: Corrected.

14. Line 169: “system variability varies” sounds weird. Please, rephrase it;

R: Corrected (Line 201).

15. Lines 178 to 181: I believe you could be more assertive, since you are dealing with numerical simulations and are capable of analyzing all the data without a gap;

R: Corrected. Those sentences were rewritten (Lines 208-213).

16. Line 225: “correlated to ENSO”;

R: Corrected (Line 280).

17. Lines 244 and 245: everything after the comma is quite confusing. Please, rephrase it;

R: Corrected (Lines 298-299)

18. Line 250: “Have so impact”?;

R: Corrected (Line 304)

19. Line 252: “This” instead of “thus”?;

R: Corrected (Line 306).

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Response to reviewer 2's comments

The paper "Modeling the interannual variability of Maipo and Rapel rive plumes off central Chile" uses a high-resolution Croco simulation approach to observe and describe the effect of wind regimes on the dynamics of two fluvial plumes off the coast of Chile. The biological implications and the influence of flow dynamics on processes such as primary production, sediment transport or stratification highlight the need for a better understanding of these types of systems. However, the Reviewer considers that the article needs improvements in the methodology as well in the simulations before being considered for publication in the journal Ocean science.

1. Regarding the modelling approach, a 9 years period simulation is not enough to discuss the variability of the plume at the interannual time scale. The Reviewer believes that the time span of the simulation needs to be extended further so that conclusions/hypothesis such as those presented in the Conclusion section can be self-sustained ("No correlation was found between the plumes characteristics and El Nino-Southern Oscillation (ENSO) and Pacific Interdecadal Oscillation (PDO) "). 9 years is just right at the border to capture any ENSO oscillation and is not enough to find any correlation with PDO.

R: Thank you for your comment. Please note that despite the relatively short period of time, our simulations encompass contrasting years with El Niño/La Niña affecting the river discharges and the plumes in the coastal ocean. We agree that 9 years is a short period to capture any significant impact by the PDO in the study area (although it shows a clear interannual variability which correlates with the ENSO). However, the impact of El Niño and La Niña showed marked contrasting effects on turbid river plumes off central-southern Chile during the period of study (see Saldías et al. 2016; "Satellite-measured interannual variability of turbid river plumes off central-southern Chile: Spatial patterns and the influence of climate variability"). In fact, the years of 2005 and 2006 were very wet periods with anomalously high rainfall and peaks in river discharges. The river plumes were anomalously big during those years in response to the freshwater availability. In fact, these conditions promoted flooding events in central Chile. In contrast, periods such as 2007 and 2011 were characterized by anomalously low signatures of freshwater plumes from the satellite observations which coincided with the influence of La Niña and a lack of rainfall in central-southern Chile (Saldías et al. 2016). We included these ideas in lines 104-106 and lines 272-279)

2. The Reviewer agrees with the authors about the robustness of CROCO as a numerical tool for the aim of this study; however, some validations need to be performed before using the model to discern and conclude about the river's plume dynamics. The validation can be performed in average basis, that is, by comparing satellite-derived geostrophic currents and SST through monthly averaging along the full time-span (multiannual monthly analysis). This procedure will allow the authors to aware future readers about the limitations of this methodology when analysing similar systems somewhere else.

R: We agree with the reviewer about the convenience of contrasting the model outputs with observations, ideally, remote sensing data. As SSS data are only available from 2015 on and these datasets still need validation, it is not possible to verify the model against sea surface salinity. Alternatively, we indirectly evaluated the model's performance by means of the reflectance associated with suspended sediment, considering that the sea surface salinity (SSS) is tightly correlated with this variable in river plume regions. According to previous studies (Saldías et al., 2012, 2016, Masotti et al 2016), total suspended solids are better sensed in the 645 nm band; therefore, we worked with the same band. We computed the 99th percentile of the 645nm reflectance in the model domain to compare it with the total river discharge and model output (SSS and total river plume area).

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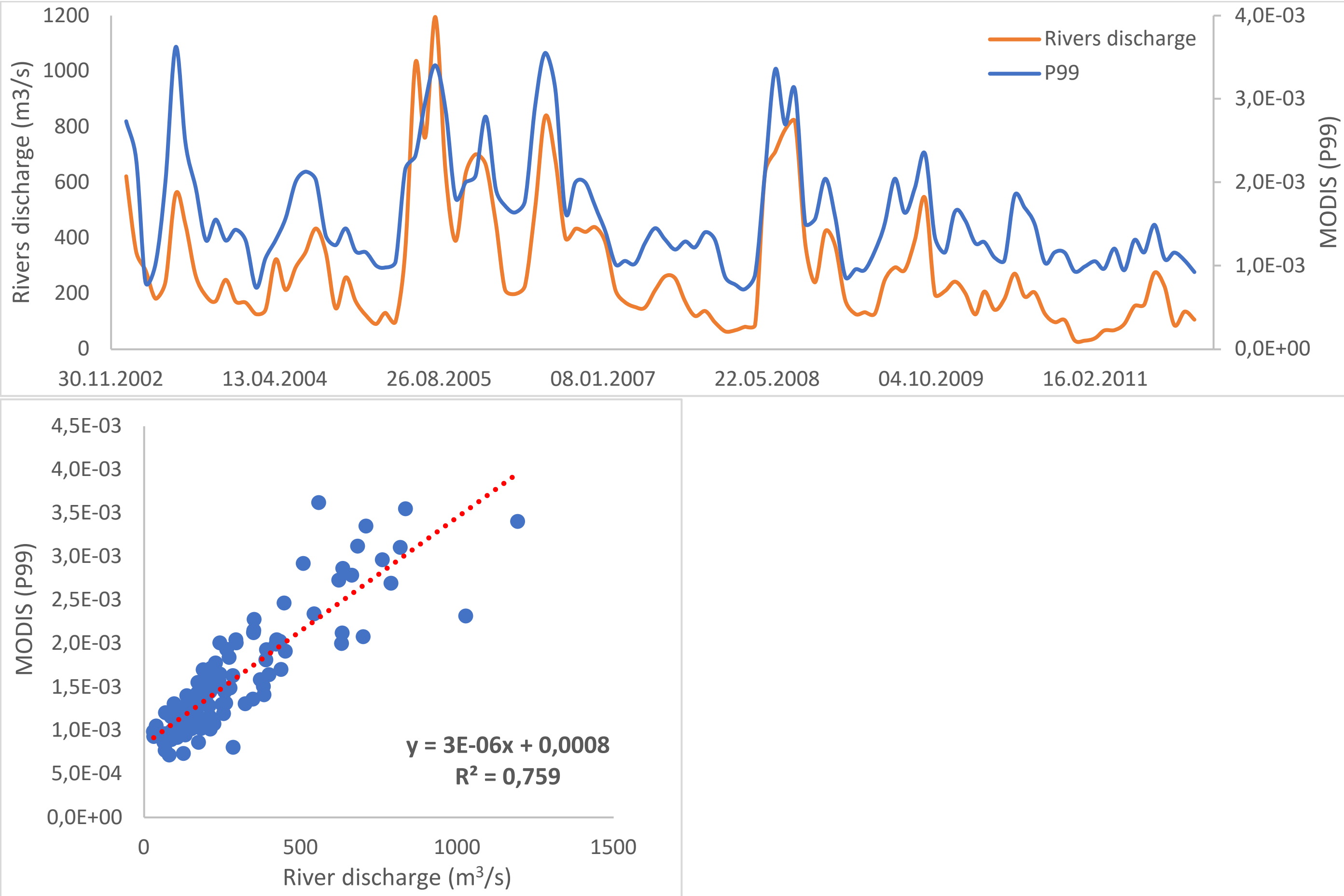

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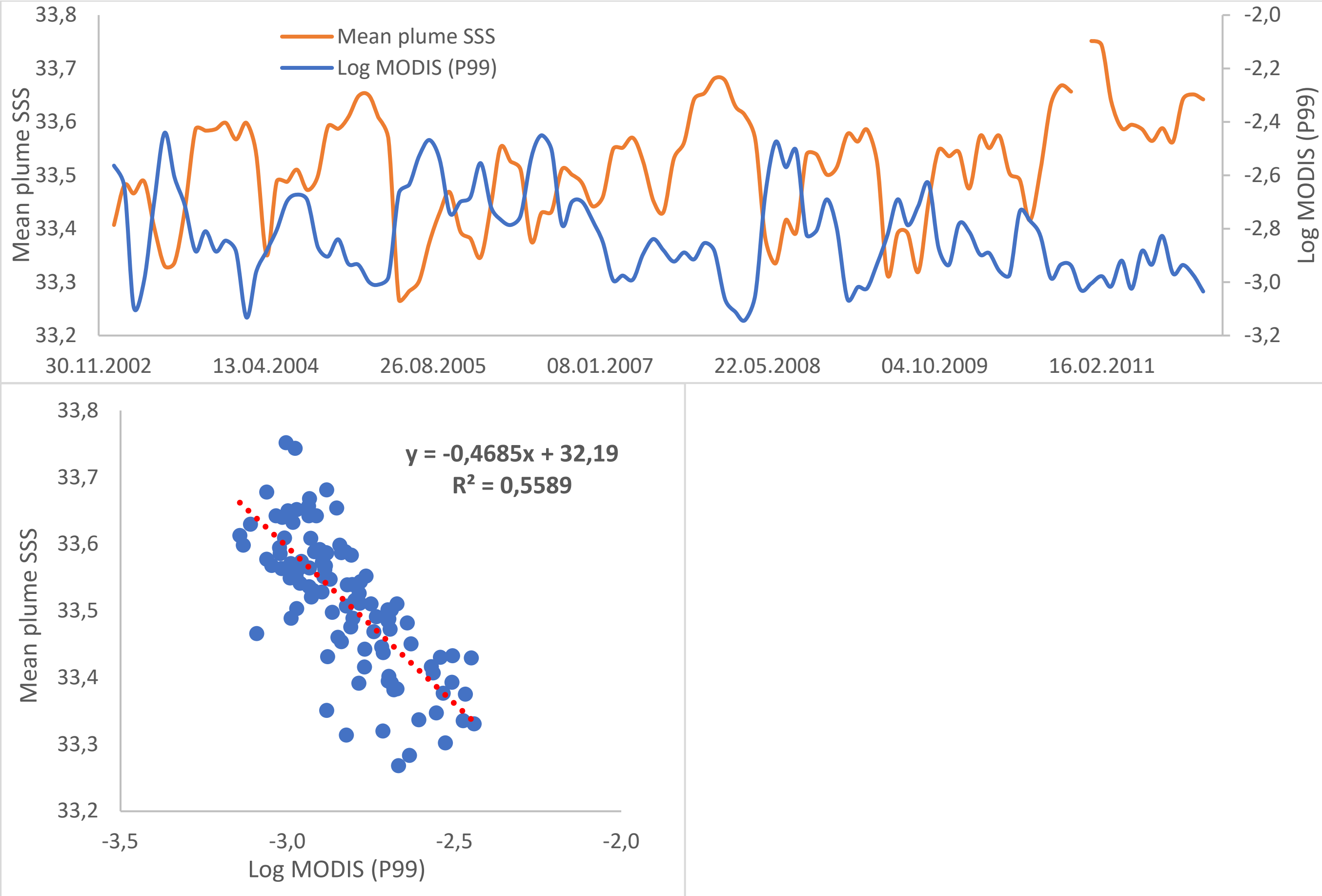

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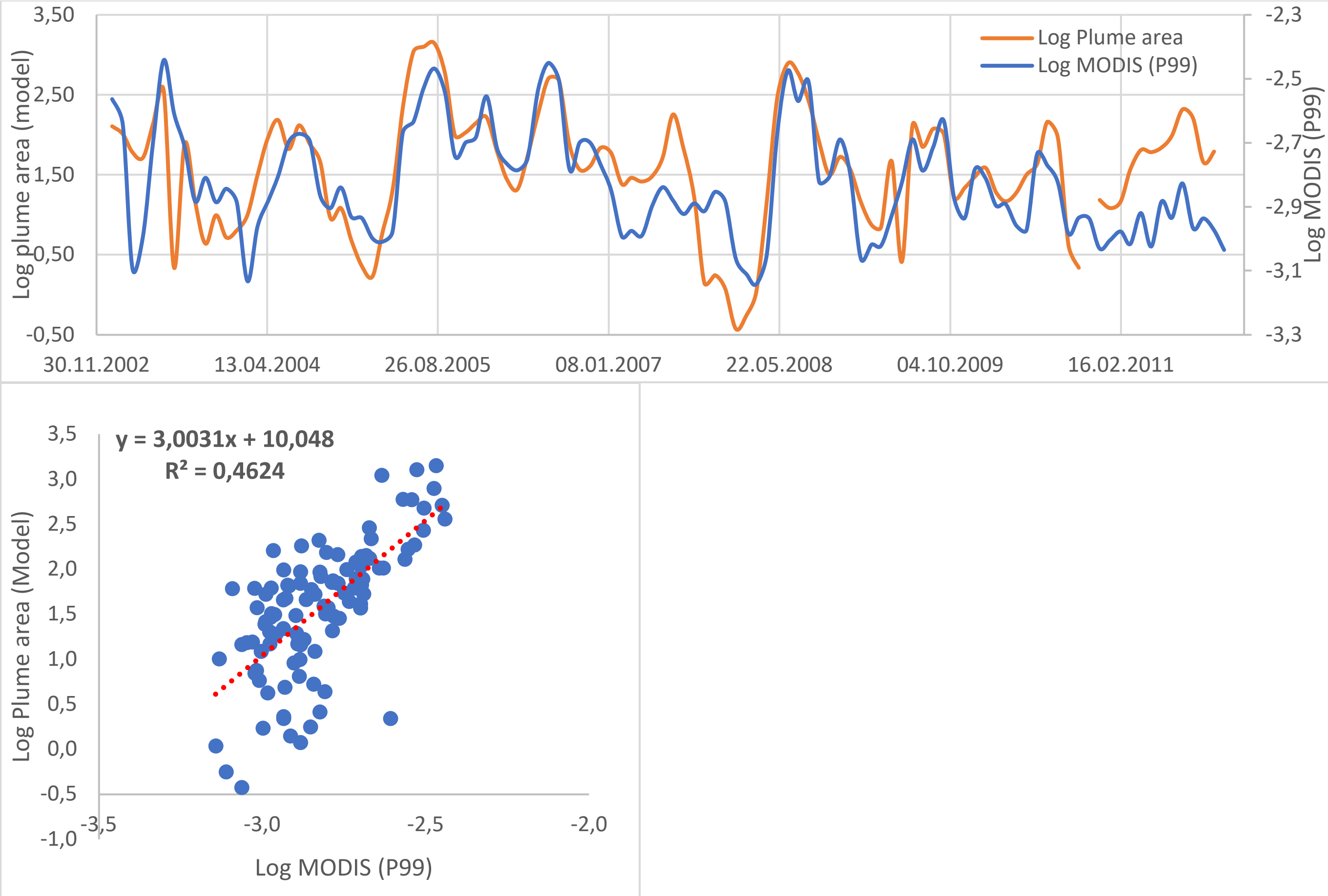

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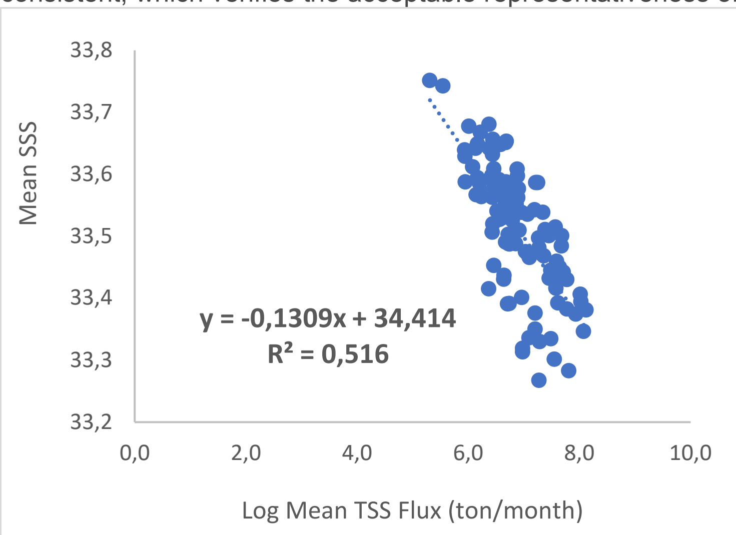

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3. The isolation of the plumes was performed by using a threshold methodology on the isohaline (33.8). The Reviewer agrees that there is not a universal method to separate fresh water from the surrounding sea one. However, recent studies have proposed to delimit the plume boundary considering a contour of water 1% fresher than the oceanic water (Please, see papers https://doi.org/10.1016/j.ecss.2022.108077 and https://doi.org/10.1175/JPO-D-20-0228.1). Is this 33.8 value close to the 1% threshold? if not, does the 1% threshold value provides a different plume distribution?
R: A practical reason for choosing 33.8 as a limit was that the plumes kept a maximum regular shape within the domain when using this value. However, the main reason to follow the 33.8 threshold is that this is an adequate limit for the Subantarctic Waters, characteristic surface water mass in this region. Consequently, all waters fresher than that must be from plumes and not from a regional water mass. In a similar approach, this limit coincided with the values defined by other authors that have studied river plumes in Chile (33.8-33.9). So, we prefer to stick to this limit and not try the 1% criterion, which is still an arbitrary delimitation. We provided a context and clarified this in lines 63-65 and lines 168-175).

4. The combination EOF and wavelet is an interesting way to subtract the main features of the dynamics in a complex system (here the river's plumes). To complement the analysis of the EOF results, the Reviewer would suggest to include the surface sea currents or wind fields associated with each mode, so that future readers will be able to see the plume response as well as the circulation patterns together with the wind forcing characteristics (The Reviewer suggests to follow doi:10.1029/2004JC002786 by Liu and Weisberg). In addition, an important aspect to highlight is the time scale observed on the wavelet results in figures 12, 13 and 14. 1000 months is a large time scale to be captured with 9 years of simulation, is this a typo or the true value? Please, double -check this result as well as the percentage of each of the 4 modes obtained with the EOFs.
R: We agreed with the referee and performed EOF analysis of the meridional and zonal velocities at the surface. We observed no significant patterns, easy to be interpreted, for the zonal component, while the meridional one showed some signal that can be linked with salinity footprint and therefore it is interesting for the aim of the paper. In virtue of these observations, plots of meridional flow analysis have been added to the MS while the zonal component was not shown. We appreciated also the suggestion of the referee that point on a different approach as the one adopted by Liu and Weisberg, 2005 (which used SOM, Self Organizing Maps approach instead of EOF), but we consider a good approach, and in some way similar, the one adopted in the present paper with a combined use of EOF and wavelet for the analysis of the time series. We also corrected the time scale (days vs months) in Fig. 12, 13 and 14. We included this information in lines 216-233).