Dear Prof Kantha,
Please find the revised version of the manuscript OS-12-1669 by Brando et al.

All general and specific comments by the reviewers were addressed. Please find enclosed to the letter a table with a point-to-point response to the reviewer's comments, and a track-change version of the manuscript. The manuscript has been carefully edited.

Following the reviewers comments, more details were added throughout the manuscript:

- Three new paragraphs were added to the introduction:
  - One to detail the Horner -Devine et al (2015) plume dynamical regions
  - One to describe the main circulation patterns in NAS and the role of wind- and wave-driven resuspension on the turbidity dynamics in NAS
  - One to describe the manuscript structure
- A new figure (Figure 3) detailing the circulation patterns prior to and at the acquisition date of the Landsat-8 image was added with corresponding text to strengthen sections 3.1.
- Figures 4 and 5 (now 5 and 6) were redrawn to improve readability.
- In Sections 3.3 and 3.4 the SST and T fields were commented in relation to published literature for NAS.
- Figure 5c (now Figure 7) was improved by including also SSS.
- A new section was added (3.5) to describe the relations between SST, T and SSS in Figures 5,6 and 7.
- In section 3.6 (previously 3.5) a paragraph was added referring to the broad classification of the plumes carried out by Syvitski et al. (2005).

During the revision of the manuscript we decided to not add another Landsat 8 image from a "normal" condition. To select a "normal condition" we briefly analyzed all L8 images of NAS, but they all show different patterns, depending on wind, wave and flow regimes. This work will be described separately in a separate manuscript. In our view, adding a new figure, as suggested by both reviewers, would entail justifying the selection of an appropriate date for the comparison and then describing all associated meteo-marine conditions. This would lengthen considerably the manuscript and change its focus.

We wish to thank reviewers for the insightful comments.
Regards,
Vittorio Brando

Reviewer 1
Specific comments

R1_S1	- It'd be interesting to show another L8 image (either RGB or T or SST) to show a "normal" or non flooded situation to highlight the difference in the properties found in the two cases.	A new figure with a new another L8image was not be added to the manuscript. In our view, adding a new figure would entail justifying the selection of an appropriate date for the comparison, describing all associated meteo-marine conditions and it would lengthen the manuscript. The results of this work were discussed in reference to published work on NAS SST and turbidity dynamics in sections 3.3 and 3.4. See also response to R2_G2
R1_S2	- Is there any reference in the literature about this near-shore trapped warm waters (NTTW)? Have they been previously described? What do the authors think or hypothesize about the origin of these waters?	We changed the acronym in the manuscript to NTWW to better reflect "near-shore trapped warm water". We are not aware of any other studies. based on remote sensing or modelling, describing similar structures in the Po Delta area or in NAS. Thus, in the first paragraph of the conclusions we added a statement "To our knowledge, this study provided the first evidence of NTWWs in NAS." As we are lacking detailed information on the vertical structure of this observed phenomenon we are not ready to speculate on the underlying processes. Hence, we added to the conclusions a recommendations for "dedicated field and numerical investigations are needed to characterize the temporal evolution of the spatial and vertical

		structure of the SSS, SST and T fields of river plumes interacting with the receiving waters and among them in varying discharge conditions. This would also enable to define the processes leading to the occurrence of NTWWs."
R1_S3	- In order to show more clearly the relation between the three variables analyzed (T, SST and SS) information of SSS for each site could be included in Figure 5c in different colors, while the shape could be used to identify their location	The figure was improved as suggested in the revised version (now figure 7). A new section (3.5) describing the dilution pathways and the relationships between SSS, SST and T was added to the manuscript
R1_S4	- In Section 3.5, it is mentioned that the two ROFIs identified are "Plume F" type, following Horner-Devine et al. (2015) classification. And it's stated that the borders can be identified by the 36 isohaline corresponding to the 5 FNU and 18◦C isotherm. From figures 4 and 5 this seems to be the case for the cyclonic coastal current, but not for the western rivers, where the 36 isohaline does not correspond with the 18◦C isotherm or 5 FNU isoline, but is located further offshore. Thus both SST and T seems not to be good indicators of the region of freshwater influence. Include some discussion in the text about this.	To enable a thorough description of the ROFI boundaries in section 3.4 Figure 5 b(now figure 6) was extended to show the outer boundaries of the Po river plume. In section 3.4 we clarified the use of the 3 and 5 FNU isolines to delineate the surface expression of the plumes in the T field from the adjacent ocean waters (Fig. 6). Also we added a new section (3.5) describing the dilution pathways and the relationships between SSS, SST and T, and a revised detailed text in section 3.6 (previous 3.5) addressing this comment.
R1_S5	- Using in situ data and simulations Dogliotti et al. (2015) showed that uncertainty for turbidity estimation is expected to be low (typically less than 6% using simulations and ∼13% from in situ data). Thus, the high variability in the composition of the region is not expected to affect the algorithm accuracy, but the relationship between T and suspended matter	A statement on the accuracy was added to section 2.1. The statement of the future work on the conclusions was revised according to this comment: "The observed optical complexity of NAS due to the variability in composition of the particulate matter may affect accuracy of the Dogliotti et al.

	concentration for each river. Indeed, future work is needed to characterize the optical properties including side-scattering (turbidity), i.e. to validate the cited algorithm.	(2015) algorithm for T retrievals, and the relationship between T and suspended matter concentration for each river. Hence future work is needed to characterize optical properties of particulate and dissolved matter delivered by each river in flood and non-flood conditions. This will also enable the validation of the Dogliotti et al. (2015) algorithm and the parameterization of other OCR algorithms (e.g. Melin et al., 2011;Vantrepotte et al., 2012;Brando et al., 2012) to accurately retrieve chlorophyll and suspended matter concentrations in these complex coastal waters.”

Technical corrections

R1_D1 Page 1672, line 22:	During this combines flood event a total of ~15 km3 of freshwater entered NAS, ~10 km3 of which entered the basin by 19 November 2014.	done
R1_D2 Page 1673, line 5:	change the future to the past tense in order to use the same tense in the whole paper, i.e. In this study we combined SST and turbidity maps derived from L8 imagery...	done
R1_D3 Page 1673, line 9:	change "plume" with "plumes’ " in ... interpretation of the plumes’ dynamic and their interaction...	done
R1_D4 Page 1673, line 17:	Define VNIR and SWIR acronyms	done
R1_D5	Define SNR	done

Page 1673, line 18:		
R1_D6 Page 1674, line 5:	Please, give more details regarding the atmospheric correction. Which bands were used: NIR (VR 2014) or SWIR (VR 2015)? In case of the later, epsilon was calculated on a pixel basis or it was calculated for the whole scene or a sub-scene?	As recommended in the manual of ACOLITE (v. 20150701), in the case of moderately to extremely turbid waters (turbidities > ~30 FNU), we applied the SWIR atmospheric correction (Vanhellemont and Ruddick, 2015), with a per-pixel variable aerosol epsilon. These details were added to section 2.1
R1_D7 Page 1674, line 10:	did the authors mean top-of-atmosphere brightness temperature?:	Yes, corrected
R1_D8 Page 1674, line 11	add wavelength of L8 band 10 for clarity (10.9 um)	done
R1_D9 Page 1674, line 21:	define ARPA	done
R1_D10 Page 1675, line 2:	add "the" in: ...followed by the effect of increasing...	done
R1_D11 Page 1675, line 7:	change of with than ("...smaller than 0.3 m.")	done
R1_D12 Page 1676, line 13:	associate the spectra from the center of the basin with the name given in Fig. 3. e.g. "the spectra for the center of the basin (indicated as open waters in Fig. 3a) have a peak at 443 and 482 nm, typical of blue waters..."	done

R1_D12 Page 1676, line 16:	add a comma (,) after "... a 562 nm peak, typical of green waters..."	done
R1_D13 Page 1676, line 25:	change "similarly" to "similar to values found at rho(655)"	done
R1_D14 Page 1677, line 5:	add "the" in "The yellow /brown shades of the other rivers..."	done
R1_D15 Page 1678, line 18:	Add FNU after 10-30 range	done
R1_D16 Page 1679, line 3-4:	add "than" after higher	done
R1_D16 Page 1681, line 21:	Add a comma (,) after "Moreover,"	done

Rev2

R2_G1	However, in order to make the analysis clearer, and the manuscript suitable for publications, the authors should provide a better introduction regarding what a “river plume characterization” means. This will give to the reader a better understanding of the main goal of this work and its potential.	A new paragraph was added at the end of the introduction to clarify the aims of this work.
R2_G2	The authors focus on the 19 November 2014 flood	A new figure with a new another L8image was not

	event. I believe that such an analysis needs to be complemented by a comparison with satellite, optical measurements during a steady state condition (either a climatologic pattern or a low water discharge state) in order to actually recognize the role of river outflow momentum during the flood.	be added to the manuscript. In our view, adding a new figure would entail justifying the selection of an appropriate date for the comparison, describing all associated meteo-marine conditions and it would lengthen the manuscript. Further than the particulate matter associated with freshwater discharge the turbidity dynamics of the NAS is controlled by wind and wave driven resuspension. A new paragraph was introduced in the introduction to describe these processes. The results of this work were discussed in reference to published work on NAS SST and turbidity dynamics in sections 3.3 and 3.4.

R2_S1	The authors state that they "characterize river plumes in the NAS" but it is not clear what such a characterization means until the discussions. The manuscript will be much clearer if the authors specify, from the beginning, that the goal is to provide an optical/lithological as well as spatial characterization of those plumes. For this purpose, they should briefly summarize, in Introduction, the scheme proposed by Horner-Devine et al. (2015). In this way the reader will follow the analysis and the discussions (Section 3.5, in particular) in a better way.	The scheme proposed by Horner-Devine et al. (2015) was summarized in the introduction and in the new section 3.5. We strengthened the final paragraph of the introduction to describe the aims of the this work.
R2_S2	Something missing in Introduction is a brief comment regarding other satellite sensors that are often used for coastal and river plume waters. The authors should	To clarify the transition from OCR sensor to Landsat, two new sentence were added here: "These studies were based on data acquired from

	provide a sentence that explains why the preferred the L8 with respect to other satellites (e.g., MODIS). I believe that their goal was to recognize the "fine structure" of the plumes and thus they preferred to go for a high spatial resolution approach. All this need to be stated.	MODIS, SeaWiFS or AVHRR sensors with a 1-4 km spatial resolution and 1-3 days revisit time. (Dickey et al 2003)." "Historically Landsat data has been used in coastal and inland waters to map both particulate matter and surface temperature at finer spatial resolution (~30m and ~100m respectively) (Hellweger et al., 2004;Fisher and Mustard, 2004)."
R2_S3	I would suggest to add an additional section that provides a better introduction of the NAS circulation and the role that river inputs have on it.	In the introduction we expanded the description of NAS providing more details on the general circulation, detailing the effects of winds and river discharge.
R2_S4	The analysis does not include any in situ data for calibration and validation of the satellite measurements. A comment on this (in the methodologies) would be appreciated.	A statement on the accuracy was added to section 2.1. Please see also response to R1_S5
R2_S5	It is not clear to me what the SSS (provided by the numerical model) adds. I see two main issues here: i) spatial resolution of the model is much lower than the satellite one; ii) while for the Po River input the numerical simulation considers the actual (daily or hourly) water discharge, for the other rivers the authors consider monthly climatological estimates (Page 1674; Line 28). Both issues weaken some of the discussions in Section 3.3. My suggestion is to restrict the SSS analysis to the Po River plume only, where the SSS and SST fields are more coherent and allow for a better discussion.	We feel that showing that current resolution and that the (lack of) available near real-time inputs of the operational model have some limits in describing such an extreme event may be useful for the local scientific community. Hence, in section 3.3 we clarified that the mesoscale patterns are well captured, while difference arise at submesocale and fine scale due to differences in resolution and the lack of near real-time data for freshwater discharge. Also in the revised version we strengthened section 3.1 by adding a new figure describing the modeled circulation patterns.

R2_S6	As I mentioned in the general comment, I was expecting to see a comparison between the river plume patterns during a high water discharge event and a low stage state. In this way, the authors can really quantify the role of riverine outflows in forming the bulges and delivering sediments.	Please, see response to R2_G2

Minor Comments

R2_D1 Page (167)1, Line 5:	I would not write that the SSS field "support" the interpretation but rather may add some additional information (for the Po River plume only, see Specific comments).	As we are now also using the model outputs to describe the circulation patterns, we believe that it is appropriate to use the term "support"
R2_D2 Line 20:	Rephrase as "by advection and mixing processes".	Done
R2_D3 Line 23:	I think the authors should include those two references: Geyer et al. [Continental Shelf Research 24 (2004) 927–949], Nof and Pichevin [Journal of Physical oceanography 31 (2001), 3045-3058].	Done, thanks for the suggestion
R2_D4 Line 23-24:	Rephrase as "importance of these processes".	done
R2_D5 Page 2, Line 13:	add Bignami et al. [Journal of Geophysical Research: Oceans 112 (2007) 1978–2012].	Done, thanks for the suggestion.
R2_D6 Line 27:	same as Line 13.	Done
R2_D7	I think, before this paragraph, authors should provide a	Please see response to R2_S2

Page 3, Line 3:	comment regarding other satellites that are often used for coastal and river plume waters (e.g., MODIS) as well as a sentence that explains why the preferred the L8 with respect to other satellites (see specific comments).	
R2_D8 Line 8-10:	As I mentioned I do not believe that the COAWST model "support" the interpretation, but rather it may add some additional information (this is true for the Po River mouths only; see specific comments). Please, rephrase this sentence.	Please see response to R2_D1
R2_D9 Line 10:	Please, indicate here the spatial resolution of the model.	To simplify the sentence, the resolution of the satellite was removed
R2_D10 Page 5, Line 19:	a dedicated section on the general circulation of the NAS will make this sentence more robust.	Done, please see response to R2_S3
R2_D11 Page 5, Line 24-	Where are these data from? Please, specify	This data were from the model outputs. We strengthened section 3.1 by referring to a new figure (Fig 3) describing the modeled circulation patterns.
R2_D12 . Page 6, Line 13:	I would rephrase as "the spectra for the offshore part of the basin".	done
R2_D13 Page 7, Line 1-13:	Maybe I am wrong, but I believe that all this part would be much more quantitative if the authors provide a plot for the ratios 865/655 and 655/562 (and a consequent discussions based on it.	We tried to plot the suggested ratios but we found the figure very difficult to interpret and describe. Hence we will not include this plot in the revised version.
R2_D14 Line 5:	isn't it this true for the Brenta, Livenza, and Sile too?	Two sentences were added to section 3.2, one for the Brenta river an one for the rivers draining the floodplain and agricultural soils

R2_D15 Figure 3:	The plot legend is in common for the two panels. I would suggest moving it in the middle of them.	The proposed change was not be implemented.
R2_D16 Figure 4:	The colorbar is missing here. Moreover, I would suggest including a metric scale (as the authors did for Figure 1). Finally, there is an error on the isohaline 37 (which is marked as 36).	The figure was redrawn.