Review of the manuscript: "High resolution satellite turbidity and sea surface temperature observations of river plume interactions during a significant flood event" by V. E. Brando, F. Braga, L. Zaggia, C. Giardino, M. Bresciani, D. Bellafiore, C. Ferrarin, F. Maicu, A. Benetazzo, D. Bonaldo, F. M. Falcieri, A. Coluccelli, A. Russo, and S. Carniel

Recommendation: The manuscript will be suitable for publication after some minor to major revisions regarding the introductory part and the discussions

Summary: The authors provide a detailed description of river plumes spatial and optic characteristics in the Italian sector of the North Adriatic Sea (NAS); the analysis includes fresh water inputs from lagoons and wetlands as well. Satellite measurements (from Landsat 8 Sea Surface Temperature and Ocean Colour sensors) allow for the recognition of the "fine structure" of these rivers and coastal plumes while numerical modelling output for Sea Surface Salinity is paired to remote sensing SST and Turbidity in order to give some insights regarding the fresh water zone extension. A spectral analysis, finally, provides some insights regarding the lithological structure of the study plumes.

General Comments: The manuscript represents an interesting and pioneering approach for the optical characterization of the main river and wetland inputs that form the West Adriatic coastal plume. I believe that the approach presented by Brando et al. might have a large impact on coastal geomorphological studies since it allows for a satellite-based analysis of river plumes spatial extension and for the lithological characterization of their sediments. 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.

The authors focus on the 19 November 2014 flood 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. Detailed comments regarding those and other points are below.

Specific Comments:

- 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.
- 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 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.
- 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.
- 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.
- 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.
- 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.

Minor Comments

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).

Line 20: Rephrase as "by advection and mixing processes".

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].

Line 23-24: Rephrase as "importance of these processes".

Page 2, Line 13: add Bignami et al. [Journal of Geophysical Research: Oceans 112 (2007) 1978–2012].

Line 27: same as Line 13.

Page 3, Line 3: I think, before this paragraph, authors should provide a 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).

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.

Line 10: Please, indicate here the spatial resolution of the model.

Page 5, Line 19: a dedicated section on the general circulation of the NAS will make this sentence more robust.

Page 5, Line 24-Page 6, Line 3: Where are these data from? Please, specify.

Page 6, Line 13: I would rephrase as “the spectra for the offshore part of the basin”.

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.

Line 5: isn’t it this true for the Brenta, Livenza, and Sile too?

Figure 3: The plot legend is in common for the two panels. I would suggest moving it in the middle of them.

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).