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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "A satellite-based analysis of the Val d'Agri (South of Italy) Oil Center gas flaring emissions" *by* M. Faruolo et al.

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Authors: we wish to thank the reviewer for his/her suggestions and comments. They gave us the opportunity to improve the overall quality of the paper.

Abstract: Please rephrase to remove "(i.e. ..)" Authors: done.

The introduction is too long. The content of the section, from the beginning of the introduction to the second page line 23; is too general and could be shortened. The content of the introduction could rearranged to link its different sections and paragraphs The motive of the study as expressed in the first paragraph of page 4106 is not clear enough; the authors mentioned that none of the previous studies has focused on a single emission source and that most of them were conducted at a regional scale. I guess

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in order to reach the regional scale those studies should have analyzed a group of single source gas flaring. So I think the justification needs to be rephrased. I guess what the authors should emphasize on here is the usefulness of these maps of gas flaring to initialize and assess atmospheric dispersion model. Plants manager usually have access to the values of flaring volumes and they do require satellite based estimates. However, satellite data could be useful where data from the plants are scarce or non-available or, as mentioned above, to fore and assess atmospheric dispersion models. Authors: as suggested by the reviewer, the introduction was shortened in the first part. The main aim of the paper was better explained too. The difference between COVA and the other plants already investigated is the very low flaring emissions of COVA compared to the others. Obviously, as suggested by the reviewer, the achieved results could be used to initialize and assess atmospheric dispersion model. This concept has been added in the revised manuscript.

Section 2: I guess it should be renamed “study domain” or Study Case “COVA” The section is long and I guess it could be shortened and then merged with 3.2 Section 3.1: MODIS data. it not mentioned in this section how the authors handled the cloud issue; which cloud mask did they use and how did they account for the impact of the cloud presence on the flaring data. Authors: following the reviewer’s comment, the title of section 2 was renamed. All details described in this section are useful for better understanding the COVA features. Section 3 refers to the data (satellite and ground-based, respectively) used during the analysis, so that in our opinion it should be separated from section 2 . In section 3.1 we mention the standard approach used to identify cloudy pixels (i.e. Cuomo et al., 2004). Such an approach, named One Channel Algorithm (OCA), is based on RST approach too and identifies clouds on a statistical basis. Considering all available images in our archive and applying the OCA approach, almost the 50% of COVA pixels were classified as cloudy.

Section 4: Methodology I guess there is no need to include a literature review here; those cited studies should be included in the introduction. Authors: the reviewer is

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right saying that we have already cited these works in the introduction, but we just need to remind, at the beginning of this section, the specific literature tests for hot spot detection based on MODIS sensor to better highlight the one we proposed.

Page 4110 line 10: “generally speaking values ..” please rephrase. Authors: done.

Page 4110 line 22: did the authors performed the pre-processing (calibration and geolocation); why they did not use L1B or L2 data. Authors: the reviewer is right. At our institute there is a Direct Broadcast Satellite Station, which allows us to receive raw (Level 0) data, which are then automatically processed (calibrated and geo-located) to produce L1B MODIS data which are then stored in our archive.

Page 4113 line 5: why eight values were picked to assess ALICE; how these values/ranges were selected? Authors: as explained in the text, the ALICE index is a standardized variable characterized by a Gaussian behavior (i.e. with mean equal to zero and standard deviation equal to 1). Signal anomalies with a different probability of occurrence can be then detected by selecting a different statistically significant level. Considering that the COVA is a stable source, persistent in space and time, ALICE is not expected to reach very high levels (i.e. >3) in case of normal operation of the flare system. Therefore, from this point of view, the COVA represents a significantly different hot source compared to previous RST applications and levels lower than 2, but higher than 1, were considered to take into account its peculiarity. The maximum ALICE level was fixed at 4.5 because no anomalies were detected at higher levels. So that, to summarize the achieved results we decided to divide the whole range (1-4.5) in 8 intervals, with a decreasing probability of anomalies occurrence.

Page 4114 line 15: “at whatever” please replace with “regardless of “ Authors: done.

Section 4.2: Is Kaufman’s formal expandable to the study area? Why the authors did not relate FRP to ALICEs values (i.e. the magnitude of the anomaly)? Authors: the Kaufman formula can be obviously applied to all pixels of the scene and to all the detected ALICE values, but it won’t provide the information we are looking for. RST

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was used to identify thermal anomalies of COVA process in order to compute the FRP, which value quantify the radiative energy emitted by the COVA during the flaring activity.

It is worth comparing the sensitivity of the ALICE index using day and night time observation separately. Authors: being this work the first one focused on COVA, we use only night time (both Terra and Aqua) acquisitions, as all literatures papers suggest. Anyway, as stated in the conclusion, the capability of the proposed RST configuration in daytime conditions to detect and characterize COVA activity will be also tested in future.

Satellite estimates are based on a snapshot that is taken at the satellite overpass time (1:30 AM?PM for AQUA); the flaring could vary before and after the image acquisition; so the integral based on the snapshot as proposed in page 4118 line 5 is not solid. Authors: the reviewer is right in assessing that one snapshot observation cannot be representative of a daily emission and, exactly for this reason, satellite information is integrated over a longer time scale (annual). Historically, satellite based analyses for this kind of application are focused on annual estimates of gas flaring volumes, because the main aim of these studies was to identify multi-annual trend in gas flaring emission in the framework of the GGFR initiative. This was also due to the above mentioned limitation of satellite sensors to provide reliable information at shorter time scales as well as to the unavailability of the validation datasets (handled only by governments and/or petroleum companies and generally related to the annual amount of flared gas). For these reasons, following literature papers (Elvidge et al., 2009, 2011), also in this work data were aggregated on a annual temporal scale.

I guess the authors should include a discussion part in which they may mention the source of uncertainties and errors e.g. cloud effect; nigh vs day overpasses etc and their potential impact. Authors: following the suggestion of the reviewer, in section 4.3 the limits of the proposed approach (e.g. temporal sampling, spatial resolution, noise contributions due to clouds, residual false positives/negatives, missing data) were better highlighted.

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 4101, 2014.