



Interactive
Comment

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.

Anonymous Referee #3

Received and published: 31 August 2014

The paper is overall well organized and its scope is well framed. I invite the authors to perform further editorial checks prior to the submission of the final version. The following includes few additional remarks and comments that could be addressed before the publication of the paper. Abstract: Please rephrase to remove “(i.e. ..)” Introduction 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 in order to reach the regional scale those studies should have analyzed a group of sin-

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



gle 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. 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. Section 4: Methodology I guess there is no need to include a literature review here; those cited studies should be included in the introduction Page 4110 line 10: “generally speaking values ..” please rephrase .. Page 4110 line 22: did the authors performed the pre-processing (calibration and geolocation); why they did not use L1B or L2 data Page 4113 line 5: why eight values were picked to assess ALICE; how these values/ranges were selected? Page 4114 line 15: “at whatever” please replace with “regardless of “ 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)? It is worth comparing the sensitivity of the ALICE index using day and night time observation separately Satellite estimates are based on a snapshot that is taken at the satellite overpass time (1:30 AM?PM for AQUA); the faring 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. 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;

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper