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Interactive comment on "Screening the ESA ATSR-2 World Fire Atlas (1997–2002)" *by* B. W. Mota et al.

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General Comments

This is an interesting and timely paper. The ATSR World Fire Atlas is probably the most widely used fire data set yet produced, especially within the atmospheric modeling community, and this paper will consequently be of considerable interest to readers of ACP. I recommend publication after a few relatively minor revisions.

Specific Comments

Page 4648: Is using a 1-km bare land mask reasonable given the geolocation errors

mentioned on page 4646? Would it be better to exclude the problematic 2001 data from the analysis?

Page 4650, line 5: Linear artifacts could easily be explained by, say, corrupted telemetry, but it's not clear how acquisition or processing errors could cause triangular clusters of fire pixels. A more detailed analysis of these unusual clusters is warranted. Prescribed fires are one possibility, or could these perhaps arise from the geolocation errors mentioned earlier in the manuscript?

Page 4655: I have a minor quibble with the characterization of gas flares as commission errors. While they should be filtered out of the WFA data set for most applications, they are most certainly fires and the detection algorithm should not be penalized for detecting them. I would mention these separately in an additional sentence, e.g. "In addition to these sources of false alarms, gas flares are detected repeatedly and comprise..."

Page 4655, line 5: "The commission errors found do not occur randomly but, on the contrary, are highly clustered in space and in time." Isn't this result partly a reflection of the screening procedures that were used? By construction, the gas flare screening, the nighttime light screening, the volcano screening, and the visual-cluster screening identify commission errors clustered in space and time. Of all the procedures that were used, only the bare-land screening could identify non-clustered false alarms, and these were only in areas of bare land. It seems somewhat circular, therefore, to state that most of the false alarms found occurred in clusters.

This point is also relevant to an earlier statement on page 4644, where the authors write "Our goal is to classify and remove from the WFA dataset all observations other than vegetation fires." I think this statement needs to be qualified since there is no

ACPD

5, S2094–S2097, 2005

Interactive Comment

practical way to achieve this goal. I'll illustrate this with a hypothetical example. Suppose I inserted a number of false fire pixels into the 1996-2001 WFA data set, randomly distributed over the land surface. What fraction of these would the authors' screening procedures identify? An order of magnitude estimate is the ratio:

$$\frac{\text{total area of non-vegetated land surface}}{\text{total area of land surface}} \sim 30\% \quad (1)$$

Thus ∼70% of these hypothetical false alarms would remain. Since the screening procedures have already removed the obvious false alarms at this point, one would have to resort to a painstaking analysis (complete with tens of thousands of high resolution satellite images) to identify the false alarms that sneaked by. Even with such an enormous effort, some ambiguous cases would remain.

In reality, of course, false alarms *are* frequently clustered and *do* often occur over bare land, and the screening procedures employed by the authors are emminently reasonable for finding these. However, their screening procedures are not adequate for identifying all types of false alarms, such as those resembling shot noise which can occur more or less anywhere. I suggest, therefore, that the authors modify the manuscript to make it clear that their screening procedure – indeed, *any* practical screening procedure for a data set of this size – can only be reasonably expected to remove the relatively obvious false alarms. I would add, however, that these cases probably represent the majority of false alarms, or at least the most persistent ones, so the filtered data set is still a big improvement over the original unfiltered data.

Technical Corrections

Page 4643, line 3: A pedantic point, but I think that Dwyer et al. analyzed only the first twelve months of the 21-month AVHRR data set produced by Stroppiana et al.

Page 4646, line 24 and elsewhere: There should be no degree symbol in K.

ACPD

5, S2094–S2097, 2005

Page 4649, line 2: Delete redundant "all".

Figure 5: Specify time period covered by the maps (presumably 1997-2002).

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 4641, 2005.