**Biogeosciences
Discussions**

Interactive comment on "New products for a better characterisation of smoke plume and gas/aerosol dispersion from boreal eurasian forest fires: the ALANIS Smoke Plume project" *by* J.-P. Muller et al.

Anonymous Referee #1

Review of 'New products for a better characterization of smoke plume and gas/aerosol dispersion from boreal Eurasian forest ïňĄres: the ALANIS Smoke Plume project' by J.-P. Muller and co-workers.

The authors present their work related to aerosol and trace gas emissions in Eurasia. It is rather short and preliminary. In fact, the paper reads more like a proposal, leaving the reader wondering what the added value is for the literature at the current stage. The goals of the work are clear and the introduction is well written. There is plenty of scope for interesting research, but unfortunately not much is presented in the paper and I cannot advise anything else than rejecting the paper.

Specific comments

Abstract: please refer to the funding agency and project in the acknowledgments, not in the first sentence of the abstract. In addition, it is known that MERIS and AATSR (both not abbreviated) are European missions, no need to add this. And the 'new burned area' product referred to in the abstract is not presented in the rest of the paper.

2.1: The authors mention they use CO observations from IASI. This is relatively new and interesting, but it does require a comparison to other, more established, products (e.g., MOPITT) or calibration with the ground-based network. Have the authors done this, or has it been done in the papers they reference? Is the retrieval impacted by aerosols? More details would be very welcome.

2.2: Several approaches are presented (M6, GotchaS, MINX), please clarify (quantitatively) what the difference is and especially what the added value of M6 is. Referencing other studies that have focused on plume heights (e.g., the work of Freitas in Brazil comes to mind) and explaining the added value of this work is necessary.

2.3: This section is particularly confusing. First, the authors state that traditional spectral classification does not work here due to persistent smoke cover. Therefore they revert to MODIS thermal anomalies (no reference given, no MOD number). But then they state that in the project traditional spectral classification will be used, even though they just said that this does not work? And to make it even more confusing, in figure 4 I do see burned area (from what source?) and in the text I read that the MODIS approach only has an error of 20% while the burned area outlines in figure 4 are much larger than the area covered by the thermal anomalies.

In addition, the conversion of burned area to emissions is worrisome; no data is given and the assumption that all available biomass burns is just wrong, please dive into literature to see that this may be the case for fine fuels, but certainly not for standing fuels. And from other papers I learned that most of these fires were in peat areas, how have you accounted for this?

2.4: This section contains no new information, and 'will be' is all over the place where I would have expected 'we did'. Please detail what the added value is of the zoom function of TM5, or how the added information on injection height improves the agreement with IASI, or how it influences air quality at the surface compared to fixed or no injection height at all to give this paper added value.

Conclusions: the authors state that the release of new products will represent high added value for the community. Again, when they can show that this indeed is the case I feel the work is ready for publication, now it is just a bold statement.

In summary, I felt that I have been reading a proposal instead of new and exciting science. The text is confusing in several sections and work from others is not properly acknowledged. Please re-write the paper after the science has been done. In my sense that embodies more than 'major revisions' so I have advised to reject the paper.

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Interactive comment on Biogeosciences Discuss., 8, 9747, 2011.