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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Cloudiness and snow cover in Alpine areas from MODIS products" *by* P. Da Ronco and C. De Michele

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We thank the Referee for comments and suggestions. These will improve the quality of our contribution.

General comments

Referee comment: **The paper presents an innovative approach for estimating snow cover in cloud obscured pixels of a remote sensing product, the MODIS daily Snow Covered Area. The strategy is based on a stepwise procedure taking advantage of spatial and temporal filtering techniques, already know in the**

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literature. The innovation stems in exploiting the complementaries of different methods, by their combined use. In my opinion, this aspect, ..., should be emphasised and clarified in the text.

The paper shall be restructured: The state of the art, as well as its discussion, must be reported in only one section (currently it is partly described in the "Introduction" and partly in the "Cloud removal procedure" section). The introduction must state clearly the objectives and contain an outline of the paper contents. The "Case study" section is quite puzzling: it contains the study area description; a brief account on materials; the preprocessing of snow cover products."

Author response: We agree that aspects related to the combined use of different methods must be emphasized in the Abstract and in the Introduction. Note however that, in Section "Cloud removal procedure" we have provided a brief state-of-art and clearly stated: "*We integrate and improve steps proposed by different authors in order to maximize the performance of the procedure when applied to the Po basin*". Anyhow, we will clarify this issue in the revised version of the manuscript.

About paper structure: 1) the state-of-art will be added to the Introduction and within the same section we will emphasize the novelty of the combined use. 2) Subsection 2.1 and 2.2 ("MODIS snow-covered area products" and "Data pre-processing") will be aggregated and shifted to a new Section "Methods", which will contain even the cloud removal procedure. The validation methodology will be added to this section. This adjustment will lead to a compact Section "Case study", where we will provide only the description of the basin.

Referee comment: **In my opinion, this aspect, together with the reasons for the sequence order, should be emphasised and clarified in the text**.

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Author response: About the reasons for the sequence order of the procedure: we tried to explain our goals in the Abstract (original manuscript): "*While conceiving the new method, our first target was to preserve the daily temporal resolution of the product. Regional snow and land lines were estimated for detecting snow cover dependence on elevation. In cases when there were not enough information on the same day ...*". Again, before step 4, we claimed: "*Until this point the target of maintaining snow maps with daily temporal resolution is kept. Step 1 and 3 bring down cloudiness using information from the same daily map, while step 2 uses data from close days but only when ground condition can be assumed constant over time*".

In the revised version of the manuscript, the "Cloud removal procedure" section will be organized as follows. We report firstly the steps of the procedure, secondly the motivation of the order, and thirdly the detailed description of each step.

The procedure is composed by 5 steps:

- Step 1 combines Aqua and Terra images of the same day;
- Step 2 is a conservative temporal filter (+2/-2 days);
- Step 3 uses the regional snow and land lines;
- Step 4 consists in a six day backwards temporal filter;
- Step 5 is a yearly seasonal macro-cycle.

The sequence order has been designed so that moving from step 1 to step 5, the temporal window of the data interested in the procedure is progressively increased, from one day to the entire year. The first three steps, as well as their order, are similar to those included in Gafurov and Bárdossy, 2009 and Paudel and Andersen, 2011.

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Step 3 follows step 2 because the latter provides a greater amount of cloudless areas on which we can estimate regional snow/land lines.
Step 4 has been justified by the good performance provided by the 7-day backwards temporal filter used in Parajka and Blöschl, 2008 for Austria. However, when clouds cover the same pixel for more than a week, it seems more likely that ground conditions are those typical of the season than those observed 7 or more days before. That is why we have introduced the seasonal filter at this point. As it is stated in the text, step 5 was validated even individually. It provided good results but it did not reach the accuracy of the 7-days filter. This also confirms the order between step 4 and step 5.

Referee comment: **Please state that you are only considering techniques referred to the MODIS sensor, there is a huge literature in remote sensing regarding the cloud obstruction issue.** Author response: We thought that it was clear having reported this information in the title "Cloudiness and snow cover in Alpine areas from MODIS products". However, we will stress this also throughout the manuscript.

Referee comment: **Though the paper is clear enough as a whole, sometimes the style in written English and the improper use of lexicon make it difficult to make out the actual meaning; both aspects must be revised.**
Author response: We appreciate that the Referee considers the manuscript enough clear as a whole. Some sentences will be reconstructed in a simpler and more readable structure to improve written English. The technical language will be revised as suggested.

Specific comments

Referee comments: **Though the overall procedure is scientifically sounding to me, anyway I have some comments and some major concerns. The descriptions**

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of the steps of the cloud removal procedure are quite cursory and should be detailed, decision rules must be clearly stated at each step. Some more rules/tables describing all possible cases or value combinations, together with a figure depicting the complete workflow may be of help. For instance for step 2 the temporal filtering final decision is not clear for all possible incongruities, and in step 4 rules are not even sketched. In step 5, since the flag depends on the threshold, a preliminary data analysis is needed to account for the choices at different elevations. In step 3 you consider 4 different aspect classes during the calculation of the regional snow and land lines. I agree that aspect strongly controls the snowmelt locally, but you claimed the approach to be regional, and you use the whole basin, which is characterized by very different landscapes, in all the other steps. Don't you think that the aspect subdivision turns your approach to a different level of detail as regards meteo and morphologic conditions?

Author response: We will improve the description of step 2 and 4 in the revised version of the manuscript. In the original manuscript, we wrote a concise description of these steps since they were already used in literature (eg Parajka and Blöschl, 2008 and Gafurov and Bárdossy, 2009). Conversely, we paid more attention to step 3 and 5, where we introduced some innovative solutions. Regarding step 2, we will add a table or a diagram showing all possible situations and related decision rules. About step 4, the basic idea is that of using the most recent observation within the previous 6 days for each pixel. This concept was explained as: "*We introduce a six day backwards temporal filter on step 4. A cloud pixel is classified to snow or land depending on the last time it was observed within the previous period*."
A further analysis will be provided on step 3. As the Referee suggested, the advantage of considering four aspect classes should be better demonstrated. There is a firm way to understand if the "signal" of aspect exists even at the regional scale, where other meteo or morphologic variables may have the upper hand. We will provide a picture showing the altitude of the regional snow lines across the melting season, for clear-sky days. North and south classes will be considered and we are going to use maps by

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step 2 (before the application of step 3). If the south class presents a regional snow line systematically higher than that of north class, it means that exposure provides a perceptible difference even at the regional scale. Thus, it may be concluded that a regional snow line drawn per aspect classes is more representative of snow ditribution over altitudes. Consequently, the procedure may just take an advantage from our subdivision.

Referee comments: **My first major concern regards the stepwise procedure. Given that the initial products (which actually have some inaccuracies) are good, at every step some errors could be introduced, especially if there are very few cloud-free pixels; on these errors the next step is applied. Even if the results seem accurate, maybe some sensitivity analyses could help to better understand critical conditions at each step**.

Author response: Please note that tables, like Tab.7 and Tab.8, were produced for each validation day (25 days in total). These are not reported in the manuscript for brevity (We will add these tables as additional material in the revised version of the manuscript). In particular, Table 8 shows contribution, individual performance, and error type of each step for 3 March 2012 and 10 December 2004. On the basis of such 25 tables, in the section "Results" we discuss the performance of each step in terms of accuracy. If the Referee was discussing the problem of error propagation, a key point should be considered. Step 2 uses output maps from step 1, and step 3 considers maps from step 2. Here, errors introduced by a step may be used by the others. However we already discussed the high level of accuracy provided by such interventions, which emerges both from our validation and from other studies. Regarding step 4 and 5, note that they remove remaining clouds using maps from step 1 (combination of Aqua and Terra data). This ensures that the information used for the classification comes from observations (either from Terra or from Aqua) and not from assumptions of previous steps. This is clearly stated in the procedure description for

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step 5: "*We used output maps by step 1, since we prefer that these two time frames come from snow actually observed from a satellite*." However, we agree that for step 4 it is stated only in the results: "*While the first 3 steps proceed in series (i.e. the output map of each is the input of the following), step 4 uses the most recent observation for each pixel (produced by step 1), as long as it falls within the previous week*.". We are going to add this details within the section "Methods".
It should even be considered that a separated and individual validation of step 5 was carried out. We provided this result since step 5 is the least involved in the procedure (it classifies only few pixel) and we thought that an individual assessment was essential. Overall, we believe that our detailed analysis on the performances is an adequate tool for understanding the accuracy of the output maps and the error sources.

Referee comments: **My second major concern regards validation strategy: even if the final aim is not to evaluate the MOD101A and MYD101A accuracy, I do not think the use of synthetic images, as the ones described in the paper, could provide an independent observation for accuracy assessment. In fact the likelihood of synthetic images to natural dynamic conditions (several subsequent days of cloud coverage) seems not to be arranged. The time dependency of filtering techniques could take advantage of it, if the artificial imagery is not correctly designed**.

Author response: We agree with Referee that the best possible assessment requires both types of validation. However, such a double assessment has not yet been used in literature. Depending on the amounts of data available, authors used one method or the other. Acquisition and validation of in-situ data for the Po basin are managed by different local administrations and has not been possible to obtain measurements for an adequate number of locations within the broad domain considered. We argue that both strategies have advantages and disadvantages. For instance, one of the major discussed issue is whether a point value can be considered representative of snow

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coverage within a 500 m - cell. Consequently, appropriate snow depth thresholds for in-situ data must be chosen. This influences the results of the validation (e.g. Parajka and Blöschl, 2008; Gao et al. 2010). Wang et al, 2009 stated: "*As the comparison of in-situ point observation versus areal mapping of MODIS snow cover image is concerned, the in situ snow depth is a critical factor for the accuracy of MODIS snow mapping ... Because of the challenge in mapping fractional or patchy snow, snow mapping comparison across different MODIS snow cover products in this study is based on the snow data with snow depth* $\geq$ *4 cm, although other threshold values of snow depth, such as 1 cm (Maurer et al., 2003) and 2.54 cm (Simic et al., 2004; Tekeli et al., 2005), were used in literature*." Then, a validation carried out through snow-depth values is highly influenced by the number of stations available (and their altitudinal distribution), as well as by the temporal continuity and reliability of the observations. On the other side, masking clear-sky images with wide clouds, the number of pixels over which the test is performed is certainly higher. In Italy, few stations exists for altitudes over 2500 m a.s.l. where snow stands until the late spring and summer. Personally, we think that this method is reliable for validating cloud removal procedures. In contrast, ground-based observations are essential for studies which focus on the assessment of the source MODIS images.

We agree with Referee that the procedure could take advantage by the possible no-likelihood of artificially-masked maps to time series of clouds that can occur (e.g. several days of continuous cloudiness). For example, if one masks a clear-sky image which is included within several continuous days of clear weather, basically step 2 (which is also the most accurate) closes that procedure. As consequence, 1) the time series of clouds is not likely 2) the reliability of the validation is lost, since only two steps are involved. To overcome this limitation our 25 testing days were chosen within time frames of cloudy days alternated with clear-sky days. This allows to involve all the steps in the procedure. Such results can be seen from Table 8 for two days.

It must also be considered that our validation methodology has already been used in at least two recent studies (Gafurov and Bárdossy, 2009 and Paudel and Andersen,

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2011). Moreover, we provided results of a comparative study, which investigates even a 7-day backwards temporal filter and the methodology by Gafurov and Bárdossy, 2009. Results clearly highlight an improvement. We don't think a true reason exists why our procedure should take an advantage by the type of validation and the others not.

Overall, Referees (1 & 2) do not appear completely convinced by the validation. Thus, two main actions are scheduled for the revised version of the manuscript. First, we will increase the number of days for validation, saving our methodology. We will use clear-sky maps extracted from years that have not yet been considered for the validation (2003, 2007, 2008 and 2010). However, in order to meet the suggestion by Referee (1) we are going to introduce artificial clouds to several continuous days of clear weather. This measure should help in simulating an atmospheric disturbance that lasts several days covering large part of the domain. We will refer to this additional test by the name "artificial multi-temporal mask".
Second, we will provide an additional table/plot, showing the contribution of each step for the whole sample of validation and the average contributions. This will help to emphasize that all the steps are tested, as well as they intervene in the application of the procedure to those 10 years of images (Fig. 6).

Minor scientific and Technical corrections
Referee comments: **Please always cite the data sources and characteristics: the DEM (is it aggregated at 500m? its vertical accuracy?), the glacier map (creator, reference scale,...)**.

Author response: Additional details will be provided for the DEM and the glacier maps.

Referee comments: **As regards glacier the sentences are not clear to me: is the problem solved or is it a pending issue? Are you sure it is relevant for the overall procedure? Maybe it is not an issue: Alpine glaciers are generally very small as**

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compared to the spatial resolution of the MODIS products, is their surface so relevant as compared to classification errors of the satellite products?

Author response: The picture showing glaciers was included in the case study section: this would only help in understanding the study domain in term of snow cover duration and distribution, as it is discussed in that section. We did not intend to discuss the issue of glacier monitoring. This aspect will be clarified in the revised version of the manuscript.

Referee comments: **MYD and MOD products contain a quality tag for each pixel; there is no needed to make assumptions about their quality. As regards quality, I do not think it consistent to use pixels tagged as "missing data" and "no decision", since they have stated problems of signal or its interpretation.**

Author response: Regarding "missing data" and "no decision", such cells are indicated with "0" and "1" in the SCA product. They are processed by the procedure in the same way it considers clouds, as it is stated in the text. Thus, the snow presence in these cells is investigated in the procedure.

Referee comments: **Please avoid to use the term "cloudiness" in favour of cloud-cover or cloud obstruction.** Author response: In the revised version we will use "cloud-cover".

Referee comments: **I advise to revise the remote sensing terminology, please refer to the one adopted in the MODIS snow cover product ATBD, also regarding the product legend. (The algorithm is called SNOMAP please fix in the text). Please avoid using the term satellite crossing (which is usually referred to the Equator) in favour of overpass.**"
Author response: In the revised version we will revise the remote sensing terminology,

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as required.
In the product legend (MODIS Snow Products User Guide to Collection 5) the name "no snow" is used. We changed it to land and other pixel classes (sea, lake, lake ice) are named "others", similarly to what was done in several previous contribution.

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