

Interactive comment on “Snow characteristics, distribution and disappearance in a subtropical volcano (Teide, Canary Islands)” by R. Martín Moreno

Anonymous Referee #1

Received and published: 13 January 2011

This paper about Teide has two faces, in my opinion. On the one hand it is quite interesting to read the manuscript and get to know this field site, but on the other hand the paper seems more like a report and does not meet the standards of a scientific article. I am sorry to say this as I like the idea to study the snow cover of this subtropical site, but the material presented does not further our understanding of processes that control extent and temporal variability of a subtropical snow pack. It is a pure description of possible processes which are known from other places, and thus fails to generate new insight. I therefore recommend rejection of the paper. I have clustered the major problems into three groups, followed by minor comments.

C1

(1) Deficient referencing style and absence of analyses

A number of statements are made without support from references or analyses by the author. For example:

(+) in 3(i) the relevant weather systems are described, but a reader who is not familiar with the synoptic situation of south-western Europe has no chance to assess whether this is true or not. A typical case is the statement "The majority of snows are produced during the invasions of polar air in the cold season; the duration of these invasions is quite variable, with an average of 5 or more days during the winter months." - - The author must provide a reference or show this to us. As the author has apparently access to local weather data, he could have taken air temperature at different atmospheric levels from a reanalysis product and compared it to measured local precipitation, which would illustrate these cold air intrusions.

(+) Section 3, claims about melt characteristics right before section 3.1: again no reference or analysis.

(+) page 82, the author states: "After a snowfall, the totality of the stratovolcano is covered in snow, which quickly begins to disappear from the base in a regular ascending pattern due to the logical accentuation of the ablation processes at lower altitudes, varying again by orientation." Another one where we have no chance to assess the reliability of a statement. Here the author could have analysed several images after a snowfall event and – by help of a digital terrain model – determined the sequence of snow vs. elevation patterns, which would give insight into both ablation cycles and vertical characteristics.

(+) page 86, author states: "The frequency and size of the snow penitentes in the Teide is decreasing due to the current global warming." Please don't make such statements unless you present evidence! While there is no doubt that atmosphere and oceans warm globally, we must be very careful in attributing local processes to global temperature. I am sure that small perturbations to the moisture and wind patterns over Teide

C2

are more influential on Penitentes. Global climate change also implies CHANGES IN DYNAMICS, not only warming. My request also extends to the last sentence of the paper: "Finally, the decrease in both extension and duration of the snow cover on Teide due to the current global warming is remarkable." Again, there is no evidence for that from the material in this paper. And thus, in a responsible way, such a statement should not be made because – as we all know – these statements are soaked up quickly by the press and are misinterpreted.

(2) Lack of theoretical process understanding/terminology

(+) page 78: "Most precipitation occurs in the winter, not only because of the evident decrease in temperature, ..." - Lower air temperatures do not necessarily mean higher precipitation. Do you mean the solid fraction of precipitation?

(+) page 78: "positive temperatures" - Are you referring to temperatures above melting point? Temperature on the Kelvin and Fahrenheit scales is/can also be positive.

(+) page 79, reference to Zerolo (1878): How is this relevant for the present manuscript? Our measurements around the world on high mountains show that air temperatures in the summit zone on sunny days are still clearly lower than those of the low- or mid-elevation zones, and certainly not "like that of the hot zone".

(+) page 79: "high radiation" is not the most important driver of sublimation. At the very first vertical gradients in humidity and sufficiently high wind speed (to generate turbulence) must be established.

(+) page 79: the terms sublimation and evaporation are mixed and equated.

(3) The goals defined are not achieved

In section 1 three goals are defined, but only goal 1 (description of snow characteristics) is touched in the manuscript, goals 2 and 3 (understand and explain snow cover dynamics) are not fulfilled.

C3

Minor comments:

(+) page 76, review of previous work: Snow cover and glaciers have also been studied on tropical volcanoes in Africa.

(+) page 79, line 6: From which station do these numbers come from (55 and 18%)?

(+) page 79: Sublimation can even account for 100% of snow ablation at high and dry sites (e.g., see work on Illimani by French scientists from LGGE)

(+) page 79, line 27: are you talking of air or snow temperature?

I want to close this review by saying again that I appreciate the author's effort to study the snow cover on Teide, but the current implementation does not warrant publication in a high-profile journal like *The Cryosphere*. The author should publish this as a field report in a less formal platform, or put substantially more work into it (especially quantitative analyses that support the statements made) for publication in the peer-review literature.

Interactive comment on *The Cryosphere Discuss.*, 5, 75, 2011.

C4