**Climate
of the Past
Discussions**

Interactive comment on "Strength of forest-albedo feedback in mid-Holocene climate simulations" *by* J. Otto et al.

Anonymous Referee #2

Received and published: 20 May 2011

In this paper, the authors claim that they aim to quantify the pure contribution of the atmosphere-vegetation interaction to the mid-Holocene. The title of the paper reflects this goal. However, the paper contains at least three main points, which are complementary. They are (1) the comparison between the climate at 6ka BP and at present with AV simulations (I assume that it is what the authors call Δ(AV), although I couldn't find the definition of this term), (2) the pure contribution of the vegetation-climate interaction (ΔV), and (3) the impact of a weak vs strong snow masking effect. According to me (and lines 14-16 on page 812) this point (3) is the major contribution of the paper. I would like to suggest to re-organise the paper along that line.

General comment

1. The discussion in the paper is not well organised in the sense that it is not al-

ways clear which simulations are compared and discussed, and the purpose of the comparison. For example, page 817 line 8 to page 818 line 4, the discussion refers to simulations 'compared to the pre-industrial' or 'anomaly' from pre-industrial values. However, the figures that support the discussion give the pure contribution (ΔV) of the vegetation-climate interaction. This is not coherent.

2. More generally speaking, it is important that the authors check that for all the comparison they describe they clearly mention what they are comparing. They have performed 8 simulations and it is sometimes very hard to identify which ones they are comparing.

3. The simulation protocol needs a more in depth explanation. The basic equations for the method should be presented. The use of several references (0k simulations) must be explained. A reference to Otto et al (2009a and b) is not sufficient. It must be explained why the use of several references circumvent the problem of non-linearity in the model. The authors mention that they consider the last 240 years of all experiments for analysis. I assume that the results presented here (tables and figures) are an average over those 240 years. It is not explicitly written. For my personal curiosity I would like to know the difference between 0k(AV) and 0k(V) on average over those 240 years.

4. The Stein and Alpert method is most of the time used with two factors. However, here the authors are using it with one factor only. What is the difference between this factor separation method and a more 'basic' sensitivity test or sensitivity analysis?

Specific comments

1. P822 l. 7 : "Commonly, EMICs distinguish only between two vegetation types (trees and grass) per grid box with a resolution of 10° in latitude and 51° in longitude (Ganopolski et al., 1998)". This sentence is fundamentally not correct because, commonly, EMIC resolution is NOT 10° by 51°. This is maybe true for CLIMBER but not for others. Moreover, the surrounding sentences let the reader assume that the whole

grid box of an EMIC is covered either by grass or by forest. I do not know whether it is the case for CLIMBER but it is not for other EMICs.

2. Caption of figure 1 is misleading. Indeed the figures display the pure contribution of vegetation. This is not mentioned.

3. Figure 4. The authors claim in the paper that they will use an astronomically based calendar. However, they display their results on a time calendar. This is not consistent! Moreover, this presentation is facing the problem that they wanted to avoid, e.g. spring is not defined for the same time interval at 6ka BP and at present.

4. Figure 4 (minor) the caption in the figures themselves indicate ΔV while in the figure caption it is only V. If the time series in these figures are mean over 240 years, it might be interesting to show the standard deviation around that mean.

Interactive comment on Clim. Past Discuss., 7, 809, 2011.