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eEarth Discussions

Interactive comment on "Inflation of Aira Caldera (Japan) detected over Kokubu urban area using SAR interferometry ERS data" *by* D. Remy et al.

Anonymous Referee #1

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Review of "Inflation of Aira Caldera (Japan) detected over Kokubu urban area using SAR interferometry ERS data" by D. Remy1,*, S. Bonvalot1,2,*, M. Murakami3, P. Briole2, and S. Okuyama4

This paper presents surface displacement observations made between June 1995 and November 1998 at Aira Caldera, Japan. The authors use interferometric synthetic aperture radar (InSAR) from the European Space Agency's ERS-1, and ERS-2 satellites to measure the uplift of the Aira Caldera floor. From these data, they determine a model for the source of displacements using a simple but reliable procedure. The paper is interesting and concise. However, I believe that it deserves some complementary efforts to make it ready for a wide international readership.

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It appears clearly from the text and the figures that the conditions at Aira - Sakurajima are not totally ideal for InSAR studies with C-band data. While the authors point out the global low coherence of the interferograms, they did not provide any explanation for this low coherence. I guess this is because of the dense vegetation cover? I guess also that the Aira - Sakurajima area is representative of the surface conditions not only in the Japanese archipelago but also in several other inter-tropical volcanic areas (e.g. Indonesia, Philippines, Central America,Ě). Therefore this work can serve as good case study of the limitations of C-band interferometry (ERS, ASAR, RADARSAT) in this type of context. I would appreciate a short discussion on this issue and also on the respective limitations and benefits of C-band and L-band data (since it seems that JERS data exist and have been used in published works on the same target for the same period).

The low level of coherence make more difficult than usual the extraction of useful geodetic information and it is the merit of the authors to demonstrate that even in these difficult conditions relevant interpretation could be made. However, as the interferometric signal described by the authors is subtle (less than 1 fringe in the better case) and very partially imaged, the interpretation proposed by the authors need solid arguments to be totally convincing. This is why I don't understand why the authors renounce to the use of all the coherent areas in their study. In figure 3A, the displayed interferogram shows coherence not only on the Kokubu and Kagoshima cities, but also on the Sakurajima Island (that is close to the supposed source of the displacements) and to the South of the island. It is likely that these areas, coherent on an almost two years -spanning interferogram, will also be coherent on most of the shorter interferograms. I would appreciate some explanations addressing this issue. A figure with a selection of 3 or 4 interferograms spanning different periods would help the reader to get a more objective idea of the validity of the author's assumptions.

The authors propose a model of the displacement source from the ERS data. They mention other works made from GPS, JERS and precise levelling data. I think that

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a critical review of the interpretations made from each data set is missing as well as a more detailed discussion on the information provided by geodetic data in term of volcanic structures and processes.

Below I make (pose) a few specific suggestions (questions) that can help the authors to improve the clarity of the manuscript. Although being not myself native English speaker, it seems to me that some expressions or formulations do not sound like academic English! I think the style and spelling of the text could be significantly improved if the authors give it to read to a native English speaker.

1) Table 1 : What is the meaning of the perpendicular baseline listed here? Is it calculated with respect to the first image? I would prefer a baseline/date representation of the interferometric database (see for example Figure 1 of Beauducel et al., 2000, JGR).

2) 22/153 : Figure 3 is mentioned before Figure 2 and Figure 1B.

3) 3-4/154 : “a persistent interferometric phase signal located on Kokubu city whereas the phase on Kagoshima city remains generally flat” : ambiguous formulation. What do you mean by persistent phase signal and flat phase?

4) /154 : It seems to me that the description of the signal can be formulated in a clearer and more concise manner (something like “the Kokubu area exhibits a time dependent but perpendicular baseline independent phase pattern, while the Kagoshima area exhibits a constant phase”). On the other hand I would appreciate more details on the interferometric processing (what is the criteria for selecting the 31 interferograms to be calculated from the 36 potential interferograms, how do you remove the topographic fringes from the interferograms, Ě.

5) 7/155 : “time series of interferograms” improper expression. I suggest “time series of range change maps”. You need also to explain somewhere that the “displacement maps” in figure 1 are range changes in the line of sight of the satellite (as I guess this

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is the case).

6) 18/155 : “a 200 m squared box” : in pixels ? What is the horizontal resolution of your interferograms?

7) 20/155 : “By this process” : I suggest “By this procedure”

8) 26/155 : “This analysis enhances the consistency of our dataset” improper expression. I suggest “This analysis demonstrates the consistency of our dataset”.

9) 27-28/155 : How many triplet of interferograms do you use to calculate this statistics ?

10) I recommend to move 21-28/155 (In the first stage, Ělower than 4mm.) to 6/155.

11) 28/155 - 1/156 : partial redundancy with 6-9/155.

12) Figure 2A : “The amplitudes and the standard deviation (vertical error bars) of the maximum uplift values are taken from a 10Œ50 pixelsĚ" : I suggest “The amplitudes and the standard deviation (vertical error bars) of the maximum uplift values are calculated in a 10Œ50 pixelsĚ”. Justify why you choose to calculate this amplitude in a rectangular window rather than in a square window.

13) Figure 2B : It seems more logical to me to display the range displacements as a function of distance rather than distance as a function of range displacements.

14) 5-8/156 : This sentence is a little bit too long and difficult to understand. The figure 2B is mentioned before the figure 2A.

15) 14/156 : “images orbits” is repeated two times.

16) 15/156 : The orbit number refers to a particular SAR image. Therefore the term “spanning” is improper.

17) 22-23/156 : “We believeĚ” : beyond their beliefs, do the authors have more serious arguments to support this interpretation ? If they have, then why do they mention

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after (25/156) an “uplift-subsidence sequence” or an “inflation-deflation process” (27-28/156). Moreover, “decrease in the uplift” (22/156) is improper as the data rather show a subsidence, even if it is an apparent one.

18) 28/156 : “already identifiedĚ” give a reference.

19) 5/157 : “Such an approach is facilitated by the low topography in the study area that makes possible to use the elastic half space 5 assumption. ” ambiguous formulation. Say that the low topography in the area allows neglecting the effect of topography on the displacement pattern (Cf. Cayol et al., 1998) and then using a simple analytical formulation (Mogi, 1958).

20) 10/157 : What do you mean by “associated variation” ?

21) 11/157 : Please give more detail of what these “4 parameters” actually are. Mention also more explicitly which formulation do you use. I guess it is the Mogi one ?

22) Figure 1 : Show, if possible the structural boundaries of the caldera.

23) Figure 3 : Show with some symbol the location of the best fit model.

Interactive comment on eEarth Discuss., 1, 151, 2006.