

## ***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 #2**

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General comment This manuscript presents InSAR data showing deformation over the Kokubu urban area, Japan. The manuscript aims to show the potential of InSAR to study deformation processes over urban areas. The authors need to provide more convincing evidence regarding the observed deformation signal. Moreover, the deformation signal lacks the spatial resolution needed to constrain the inferred deformation source. I suggest that the authors consider including in their analysis other InSAR and GPS data that are already published, or improve the spatial resolution of their dataset doing a persistent scatters analysis. The manuscript is generally not well written, and possibly an English-speaking person should assist improving the writing. The legibility of the figures should be improved.

Major comments 1) The authors need to provide more convincing evidence showing

that the observed deformation signal is real and that is not an atmospheric artifact. If the authors have formed some interferograms, using completely independent image pairs, as they state on page 154 lines 16-17, then they should show these results. All unwrapped interferograms shown in Fig.1B are constructed using a common master image, therefore an atmospheric artifact cannot be excluded. The same applies to all interferograms used for the modeling, and mentioned in Table 2, which all have the same slave image. On page 154 lines 12-14 the authors write that the observed signal is unlikely to be an atmospheric artifact, because height variations over their study are weak. This is partly true, but atmospheric artifacts are not only related to high topography and this argument cannot be used to rule out atmospheric contributions in the interferograms. Most important, on page 156 lines 20-26, the authors decide to discard one interferogram showing rapid subsidence, because of 'contamination by local atmospheric heterogeneities'. Are the interferograms affected by atmospheric artifacts or not? The authors should clarify this point better in section 2. I also suggest that the authors provide a figure with several panels showing the recorded interferograms, with at least two independent ones. 2) The authors say that the interferograms have generally low coherence; hence they decide to focus on a small area, 10 by 10 km. However, in the modeling they use a source that causes a deformation signal over a way bigger area (in figure 3 a scale would help). It appears that the deformation source cannot be well constrained, because of the low spatial resolution. Moreover, in fig. 3 the authors show both the recorded interferogram and the best-fit model over the whole Aira caldera (Fig. 3A and 3B), but they show the residual interferogram only over the Kokubu area (Fig. 3C), which is a very small fraction of the total area. How does their model fit northwest (Kagoshima city) and southwest of the caldera, where coherence is kept? The authors should show the residual over the whole area, as a potential reader may lose credibility in their study and think that they are showing the residual only where it fits. I suggest that the authors include more InSAR and GPS data in their analysis in order to improve the spatial resolution. As the authors write in the conclusion section, observations by JERS, GPS and leveling data are already

published and should be included in this study. Another valid alternative to obtain coherence over a larger area is to perform a persistent scatter analysis, as the authors mention in the conclusion section. 3) The authors use uncertainties of 1 mm for all the interferograms, while in the literature much higher values are reported. Massonet and Feigl (1998) and Pedersen, et al., (2003) (see references below) report 10 mm of uncertainty. The authors should argue for using such a low value and provide some reference where similar low uncertainties are used. References: Massonet, D., Feigl, K.L., 1998. Radar interferometry and its application to changes in the earth's surface, *Rev. of Geophys.*, 36, 441-500. Pedersen, R., Jónsson, S., Árnadóttir, T., Sigmundsson, F., Feigl, K.L., 2003. Fault slip distribution of two June 2000 Mw 6.5 earthquakes in South Iceland estimated from joint inversion of InSAR and GPS measurements, *EPSL*, 213, 487-502.

Other comments Section 1. The introduction is completely lacking information regarding the volcanic activity and the seismicity of the study area. These aspects are important and should be added. Section 2. page 154 line 3, the authors should not use the phrase 'Ĕ. reveals a persistent interferometric phase signal located onĔ.' to describe the observed signal. It is confusing as persistent scatter is a special method not used here. The phrase should be corrected to 'Ĕreveals a deformation signal consistent with (add) onĔ..'. Section 3. Page 155 line9, the authors should summarize the technique proposed by Usai (1999) and used in this manuscript. Page 155 lines 9-12. Including some GPS points (Murakiami et al., 2001; Okuyama et al., 2001) would help to correct for the uncertainty in identifying the fringe corresponding to zero displacement rather than fixing an arbitrary point. Alternatively, the authors should include in their modeling an additional offset parameter to account for this uncertainty. Page 155 lines 20-30. This paragraph needs major improvement mainly with respect to the writing. References should be given on the method that is introduced in this paragraph. On line 25 the authors should say what is a 'phase value closure' and why it is expected to be zero, as not all potential readers are used to the method. Including a formula and explaining the parameters would help. On line 28, 'Then the network of 31 interferograms' should

be corrected to ‘Then the series of 31 interferograms’.

Section 4. On page 157 lines 11-13, the authors decide to fix the depth of the source, but they don’t argue why. If the source is supposed to be stable in space why are they fixing the depth and not the location? On page 157 lines 14, the authors refer to Table 2 for the rms, however, in the text the authors should say how much in % of deformation their model can explain, or provide the rms for a null model.

Section 5. This part would gain significance through some more detailed comments of the results. A possible comparison with observations at other volcanoes in the area and how this fits with similar observations made at calderas worldwide should be added.

Figure 1A, Add the outline of the Aira caldera and a bar with a km scale. Figure 1B Add a bar with a km scale Figure 3 Add a bar with a km scale and in fig. 3C show the same area as in 3a and 3b.

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