**Reply to ‘Sensitivity of Santorini eruption model predictions to input conditions’**

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We thank Walker et al. [1] for their interest in our paper ‘Eruptive activity of the Santorini Volcano controlled by sea-level rise and fall’ [2]. Earlier they denied [3] our main conclusion that most of Santorini’s eruptions for the considered period occurred during low sea level. We replied [4] to [3], and now Walker et al. [1] accept our conclusion. They have also [1] reproduced our numerical model, thereby supporting its mathematical correctness, but have concerns with some of the boundary, geometric, and loading conditions. In the new numerical models Walker et al. [1] provide, however, the loading conditions are incorrect, the suggested geometry is inappropriate, and the boundary conditions are less realistic than in our model, resulting in their ‘sensitivity analysis’ being irrelevant to our study. They have also been unable to show quantitatively the significance of any loading other than sea-level change. Their model with an added ‘mantle analogue’ uses unjustified viscosity, assumes constant sea level, and has therefore no relevance to the volcanotectonic processes that our model explains through sea-level changes. Our observational and numerical results [2] thus stand.

In criticising the boundary conditions applied in our model Walker et al. [1] state ‘In reality, no part of the crust is in fixed position’, implying that there is no valid rationale for fixing the ends of the model. But they use fixed crustal position conditions themselves in their new models (Fig. 1c, Supplementary (S) Fig. d). They apply the ‘clamped edge’ boundary conditions (denoted as ‘fixed boundary’, Figs 1C, Sd), generating artificial stress concentrations seen in the lower corners of the figures. Everyone familiar with modelling of this kind knows that in order to avoid rigid body rotation/translation the model must be fastened somewhere. To minimise the artificial stress effect, and in contrasts with what Walker et al. [1] state, our model does not use fixed (clamped) edges/boundaries but rather the roller boundary conditions. The latter allow easier horizontal crustal/lithosphere displacement during loading and bending (doming). We consider this as more realistic boundary conditions, particularly given the melt accumulation assumed at the base of the lithosphere (where the excess magmatic pressure is generated during lowering of the sea level) and the general plate movements in the area. Their criticism of our model as to the crust being in ‘fixed position’ is thus without value.

Walker et al. [1] state that the predicted stresses in our model depend ‘strongly’ on ‘model dimensions and boundary conditions.’ All models of this kind depend on dimensions and boundary conditions - and, additionally, on mechanical properties, layering, loading, and other factors [5-8] For laminated materials, the effective flexural rigidity *De* is a function of the properties of the mechanical layers and their contacts and, in particular, of their Young’s moduli [8]. *De* is generally much lower than the flexural rigidity for homogeneous, isotropic plates, so that for the same loading and plate dimensions, the maximum deflection, hence the stresses, would be greater – results that also apply to layered crustal segments[9-12]. Additionally, the variation in the value of Young’s modulus between layers, which may be by several orders of magnitude, has great effects on the stress distributions in the layers. It is the job of the modellers to use their knowledge and understanding of the relevant geology and physics to come up with the most appropriate and realistic dimensions, boundary conditions, mechanical properties, and loading.

We have already shown above that our boundary conditions [2] are more appropriate than those by Walker et al. [1]. As for the dimensions, the South Aegean Volcanic Arc is about 400 km long and up to 40 km wide and may be divided into five volcanic fields/systems [13]. The lateral cross-sectional areas of deep-seated reservoirs are normally larger than the surface areas of the associated volcanic fields/systems[14]. This fact and the central location of the Christiana–Santorini–Kolumbo volcanic field/system within the arc suggest that the area of the melt-accumulation zone and pressure change beneath the field/system may be about 100 × 100 km and affecting an elastic crustal segment of the same size (as used in our model). (The 20 km thickness is primarily estimated from seismic data). Shallow crustal magma chambers tend to form above the centre of the deeper zone of melt accumulation, which is controlled by Darcy’s law[14], rather than above its edges.

Walker et al. [1] list 10 factors that may contribute ‘loading conditions in addition to sea level’. We have discussed many of these factors in other papers and books15-19, but we regard them as beyond the scope of a paper whose stated aim is to establish the effect of sea-level change, in isolation, on volcanic activity. To attempt to use 11 factors or variables to explain a single observed correlation when one factor (sea-level change) demonstrably explains much of the data has no explanatory power and is in flat contradiction with Occam’s razor [20-22], which in the present context implies that models to explain observations should be made as simple (and thus as testable) as possible. Walker et al. [1] apparently think that a 40-50 m column of rock, because of its higher density than sea-water, will generate much greater deflection than 110 m sea-level change, but fail to realise that a tall but comparatively narrow load (e.g. a volcano) commonly induces less deflection and associated stress changes than a much thinner but broader (more widely distributed) load (e.g. sea-level change). Also, Walker et al. have now had 22 months [3] to demonstrate quantitatively the effects of these 10 factors, but have failed to do so.

Walker et al. [1] state that ‘bending of the plate will be subdued or removed by the viscous lower crust or mantle’. Studies in Iceland show that (primarily the central) parts of the country subsided (during glaciation) and rose (during deglaciation) by up to about 500 m[23]. This is the country whose mantle viscosity is used in the model by Walker et al. [1]. Even in the past decades we see rise in parts of Iceland due to the retreat (slow melting) of glaciers[23]. Additionally, magma reservoirs are routinely being detected through slight doming (inflation) and subsidence (deflation)[24-27]. Walker et al. [1] do not provide any evidence at all for their statement (the burden of proof must be theirs), and observations from the country from which they get the mantle viscosity for their model show that their statement is incorrect.

Their new axisymmetric models have several flaws that make them irrelevant to the processed modelled by us [2]. First, there is a stress shadow (compressive stress) around the shallow chamber (Fig. 1d). We have made many models on doming (up-bending) of crustal segment hosting a shallow sill-like chamber and all show tensile stresses in the chamber roof[17]. We have remade their new models and the results show that the stress shadow cannot be due to doming but can be due to excess pressure in the chamber. The latter, however, is not the loading in our model [2] and thus not relevant. Additionally, they now state that the viscosity from the Icelandic mantle, which they use in their new models (Fig. Sd) for Santorini (without any justification), applies only ‘at constant sea level’; our paper [2] is about changing sea level. Additionally, they say in the text that there is no ‘need to fix the edges’ in this model. And yet in Fig. Sd they write ‘fixed boundary’. Whichever it is, a model that assumes constant sea level obviously has no relevance at all to our model on the volcanotectonic effects of sea-level rise and fall at Santorini.

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