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REPLY TO COMMENT

Reply to the comment on 'Inhomogeneities and birefringence in quartz'

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Abstract. The ferroelasticity effect of the incommensurate phase of quartz is discussed. It is pointed out that the effect discussed in the paper by Saint-Grégoire *et al* (Saint-Grégoire P, Snoeck E, Roucau C, Luk'yanchuk I and Janovec V 1996 *JETP Lett.* **64** 410) is negligibly small, and cannot explain the anomalous observations near the $\alpha \leftrightarrow \beta$ transition in quartz.

In the paper by Saint-Grégoire *et al* [2] the authors claim that they found a 'novel type of IC phase in quartz', namely, the elongated-triangle phase (the irregular triple-k IC structure, or ELT phase in their terms). Elongated triangles, however, had been observed already by Van Landuyt *et al* 1986 [3] and by Yamamoto *et al* in 1988 [4] by means of electron microscopy. The theoretical possibility of an irregular triple-k IC structure was also pointed out in the paper by Aslanyan *et al* [5]. In the paper by Saint-Grégoire *et al* [2] it is not shown that the regular triple-k structure should *necessarily* transform into the irregular (i.e., elongated-triangular) structure, but they mention that under some *special assumptions* the irregular triple-k structure may be more favourable than the regular triple-k structure.

In the paper [2] the authors also claim that the irregular triple-k IC structure in quartz is ferroelastic. This statement, however, was not sufficiently justified by the authors. Their only argument was the following: the symmetry of the irregular IC structure is very low, and hence it should be ferroelastic. We think that in order to claim that the crystal is ferroelastic it is necessary to introduce the strains appearing below the transition and to estimate them to show whether the effect is observable, or negligibly small. In the paper [2] the strains were not introduced and were not estimated. In the preceding 'Comment ...' by Saint-Grégoire and Luk'yanchuk [1] the strains u_{xy} and $u_{xx} - u_{yy}$ derived in our papers [6, 15] are introduced, but they are not estimated, although the authors claim that they should be of the order of 10^{-5}. We note that for such a large-scale inhomogeneous structure, as is observed in quartz (in the lower-temperature range the sizes of triangles exceed 1 μm [4]), one may expect only a negligibly small effect of ferroelasticity. In other words, a great number of ferroelectrics with low-symmetry-shape ferroelectric domains should also be considered as ferroelastics, as follows from the arguments of the authors [2]. (For example, the polarization vector in the ferroelectric crystal of $LiNbO_3$ is along the C_3 axis, but the shape of ferroelectric domains is, generally, of symmetry lower than C_3. The strains which always exist in the ferroelectric domain walls in such a case do not compensate each other, and the crystal acquires integral

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non-zero strain. However, such an effect is negligibly small and cannot be used as a justification for treating $LiNbO_3$ as a ferroelastic.) The smallness of the ferroelasticity effect for the large-scale inhomogeneities in quartz was pointed out and estimated in our papers [6, 15] for the sinusoidal IC structure.

However, the authors of [1] claim that the ferroelasticity effect should be sufficiently larger in the soliton-like IC structure, where the sign of the order parameter η alternates in different ELT, as they treat the observed triangular pattern in quartz. First we note that birefringence (which is a manifestation of ferroelasticity) is a bulk effect, and the thin domain walls separating ELT in the soliton-like IC phase cannot give any birefringence. It should also be noted that according to the x-ray diffraction studies by Gouhara and Kato [8, 9] (this result was re-discussed in our paper [10], on the basis of the analysis of neutron diffraction by Dolino *et al* [11]), the observed long-period IC modulation in quartz is almost completely an acoustic modulation. The optical mode atomic displacements are so small that they *do not contribute* to the IC diffraction satellites. So, it is obvious that the optical displacements should also contribute only negligibly to the electron diffraction structure factors in the TEM observations in the IC phase, and the long-period triangular pattern observed in TEM is a manifestation of the spatial alternation of the signs of the acoustic displacements u_x, u_y and u_z, while η is very small and the alternation of its sign does not contribute to the TEM images. In other words, a soliton-like long-period IC phase with alternating Dauphiné twins cannot be used as a model for the explanation of the ferroelasticity effect in quartz, as is attempted by Saint-Grégoire *et al* [1, 2]. We also note that an almost completely acoustic character of the long-period IC modulation follows directly from our model for the transition in quartz [12]. Such unique character of the IC modulation should be considered as confirmation of our model.

Our model for the IC transition in quartz and its comparison with experimental observations are discussed in references [6, 7, 10, 12–15].

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