Cano and Levanyuk Reply: In the preceding Comment [1] Biljakovic sees some contradictions between the theory developed in Ref. [2] and experimental data. According to Biljakovic, these contradictions consist in (i) the observation of deviations from the expected linear-in-T behavior, in experiments concerning biphenyl and bis (4-chlorophenyl) sulfone (BCPS) and (ii) the absence in some charge-density-wave systems of correlation between the specific heat over temperature, C/T, and the magnitude γ/ω_0^2 representing, following Biljakovic [not Ref. [2]], the ratio between the phason damping and the squared phason gap. A careful analysis of the situation reveals, however, that there is no clear contradiction at this level.

When it comes to the first point, one has to bear in mind that, as we explicitly mention in Ref. [2], the linear-in-T behavior is expected as a low-temperature asymptotic limit. If this limit is not fully achieved, then the corresponding behavior naturally deviates from the linear-in-T one. This is so for one damped oscillator [what Biljakovic in fact considers when addressing Eq. (2) of Ref. [2]], and we argue in Ref. [2] that this is also the case for a real incommensurate (IC) crystal, i.e., a set of damped oscillators. It is for real IC crystals that we try to estimate the temperature at which the asymptotic limit is abandoned. As we mention in Ref. [2] [see also Ref. [3]], at present there is unfortunately no reported theory nor experiments from which we can extract all the information necessary to carry out these estimates with high level of accuracy (we shall return to this point below). So it cannot be discarded the possibility that this asymptotic limit is not abandoned at a temperature ~ 1 K, but at a lower one not achieved in the corresponding experiments (what will depend on the sample as long as it is related to defects). This could be a simple reason for the different power laws observed experimentally, as we already speculate in Ref. [2] (we also comment there on other possibilities as, e.g., the phason anisotropy). The same happens with the apparent discrepancy between our estimated value of C/T and the experimental one. When making this estimate we assume, for instance, that the phason damping is the same for the whole phason branch. Biljakovic points out that, in accordance to data extracted from the region of the Kohn anomaly, this damping may diminish with increasing the phason wave vector. What Biljakovic is then offering is an explanation to our overestimation, an explanation that we already mentioned in Ref. [2].

In regards to the second point, there is no obvious correlation between an *integral magnitude* such as the specific heat and the magnitude γ/ω_0^2 referring to only one normal mode of the corresponding system. To begin with, let us mention that in the case of a gapless IC system such as byphenil, for instance, γ/ω_0^2 goes from infinity at zero wave vector to some finite value at large wave vectors. In IC systems with phason gap γ/ω_0^2 can also acquire very different values depending on the wave vector one considers (as a rule, the wave vector dispersion of the phason branch is quite significant). What is the one to be compared with C/T? Furthermore, one of the main conclusions of Ref. [2] [see also Ref. [3]] is just that, not only the modes associated with small wave vectors, but nearly all the normal modes of a crystal may contribute significantly to the linear-in-T term in the specific heat. This implies that it is necessary to have characterized the whole Brillouin zone to compute accurately this linear-in-T contribution. Although this point is mentioned by Biljakovic, it is ignored when carrying out comparisons between experimental values of C/T and the magnitude γ/ω_0^2 extracted from microwave data, i.e., data involving a very small subset of normal modes of the corresponding system (pure or doped samples, it does not matter; only normal modes of very small wave vectors are involved in these microwave experiments). The effort in making this comparison is remarkable, but nevertheless its results are not conclusive.

In closing, from the reasons exposed in Ref. [1], one cannot assert that the theory developed in Ref. [2] is in conflict with experimental data. In Ref. [2] we took into account the (previously ignored) phason damping when computing the low-temperature specific heat of IC phases showing that, without abandoning the phononic scenario, the effects of this damping are large enough to be responsible for the ''anomalies'' observed at the lowest temperatures. It is evident that one can go beyond our theory as long as it is restricted, for instance, to small concentration of defects (not necessarily the case in real experiments). But, even at this stage, it represents, in our opinion, a significant progress in the understanding of the topic.

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