Elimination of the supersolid phase through crystal annealing
Authors: Ann Sophie C.Rittner and John D.Reppy.
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Superfluidity of grain boundaries and supersolid behavior
Authors: S.Sasaki,R.Ishiguro,F.Caupin,H.J.Maris and S.Balibar
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Recommended and a Commentary by A.J. Leggett, University of Illinois, Urbana.

Few experiments in low-temperature physics have generated as much excitement and controversy in the last few years as those conducted on solid ^4He by E.Kim and M.W.H.Chan (hereafter KC)[1,2]. These experiments are,conceptually speaking, the analog of the famous 1946 experiment of Andronikashvili[3] on liquid ^4He, although the geometry is somewhat different. In Andronikashvili's case, a pile of closely spaced discs was suspended in the liquid from a torsion thread, and the period of oscillation measured as a function of temperature; from the decrease of the period below the lambda-temperature it was inferred that a fraction of the liquid,the "superfluid fraction" $\rho_s(T)$,which tends to 100% as T falls to zero,was coming out of rotation with the discs and staying at rest in the laboratory. In the KC experiment, *solid* ^4He, either in the pores of Vycor glass or in bulk, was contained in an annular cylinder suspended on a torsion thread, and the period of oscillation (and damping) again measured as a function of temperature and pressure. From the results, and various control experiments, the authors infer that a fraction of the ^4He comes out of equilibrium with the rotating container below about $200mK$; this "supersolid" fraction is temperature and pressure-dependent, reaching a maximum of about 1.5% at 55 bar in the low temperature limit. These results have prompted a lively debate among theorists, in particular over the question whether the behavior seen is characteristic of pure bulk crystalline ^4He or rather associated with surfaces, grain boundaries, dislocations or some other "extrinsic" feature(s); they have also motivated a number of experiments, both published and in progress, which look for different possible manifestations of superfluid-like behavior in solid ^4He. One point which is worth making explicitly is that in both the Andronikashvili and the KC experiments the maximum angular velocity of rotation of the discs/cylinder was always greater than the characteristic quantum unit of angular velocity $\omega_c = \hbar/mR^2$,where m is the mass of the He atom and R the mean disc/cylinder radius. Under these conditions it is straightforward to show that the behavior seen by Andronikashvili in the liquid (in contrast to that seen by Hess and Fairbank[4] 20 years later) could not possibly have been the true thermodynamic equilibrium behavior but must rather be evidence for a nonequilibrium state which is metastable at least over the oscillation period; and it appears very likely that the same remark applies to at least part of the KC data on the solid (in which it should be noted that a sufficiently large angular velocity suppresses the "supersolid" effect).

The two experiments which are the subject of this comment are rather different in nature.The experiment of Rittner and Reppy (RR) is a torsional-oscillator one similar to that of KC; however, they used both a solid-cylinder geometry and one in which the horizontal cross-section of the sample is approximately square. In both cases, when the solid ^4He was formed rapidly from the liquid, the oscillation period and damping behaved in a way similar to that seen by KC; RR argue that this rules out slippage at the solid-container interface as the origin of the "supersolid" behavior, since in the square geometry no such effect should occur. The most significant result reported by RR, however, is that by annealing the ^4He crystal for a few hours at a temperature above 1 K they could drive away the "supersolid" behavior entirely. This would appear to suggest rather strongly that this behavior is associated with some kind of imperfection which is absent in well-annealed crystals (though see below).

The second experiment, by Sasaki et al.(SICMB) is of a different nature; the authors studied the approach to hydrostatic equilibrium, if and when it occurs, of ^4He condensed inside and outside a glass tube, open at the bottom, at pressures close to melting, so that both liquid and solid phases occur in both regions. The raw data, taken at 50mK, is the visually observed motion of the liquid-solid interfaces, and from this they infer a rate of mass transport out of the bottom end of the tube; since this region is occupied exclusively by the (denser) crystalline solid phase, they infer that any mass transport is occurring through the bulk solid. In addition to monitoring the motion of the interface, they could also detect visually the presence of grain boundaries in the solid, since when these meet the interface visible cusps are formed. In ten of their crystals, each with none or few cusps, SICMB saw no observable relaxation of the solid-liquid interface, thus putting a very stringent upper limit on any mass transport through the solid. However, in three crystals they saw relaxation. In one case, containing one cusp, the relaxation was slow and stopped when the cusp disappeared at a nonzero height difference of the interfaces; in another, containing many

cusps, relaxation was much faster and continued until the interface heights coincided.The most significant aspect of the relaxation is that it is *linear in time*, indicating a mass flow rate which which is independent of the pressure difference between inside and outside - a behavior which in the liquid phase is a signature of superfluid flow at the (pressure-head-independent) critical velocity v_c. The authors interpret their data (and by extension that of KC) in terms of mass transport occurring along one or more grain boundaries which intersect the open bottom of the tube, and on this assumption calculate a critical velocity (about a few m/sec) which is at least comparable to that seen in atomic-thickness liquid film creep.

A number of difficulties arise in reconciling these two experiments with those of KC: (1)in more recent work[5], KC find,contrary to RR, that annealing, so far from driving away the "supersolid" effect, actually enhances its reproducibility (2) if the explanation of the KC data is as suggested by SICMB, it is difficult[5] to understand why the supersolid fraction shows so weak a dependence on pressure and geometry (Vycor versus bulk), since the number of grain boundaries might prima facie be expected to vary by orders of magnitude (3) the critical velocity needed by SICMB to fit their own data is 3-4 orders of magnitude greater than the velocity of the cylinder walls at which the supersolid effect tends to zero in the KC experiments, and this despite the fact that the SICMB experiment is effectively dc while the KC one is at 1 kHz (and therefore would be expected to show more, not less, effects of metastability). Indeed, it is not even clear whether in these three groups of experiments we are dealing with a single phenomenon or two possibly unrelated ones; clearly a direct determination of the defect density in the torsional-oscillator experiments, if it is possible, should go a long way towards resolving this question. All in all, at the time of writing I think the only statement which we can make with any confidence about solid ^4He is that we do not understand it nearly as well as we thought we did three years ago!

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[5]E.Kim and M.H.W.Chan, http:ArXive.org/cond-mat/0605680.