vant. As one would predict, we did not observe giant quantum oscillations in our samples nor did we report them. The relevant theory seems to be that of Spector[6] for the tilt effect. The peaks we reported are in agreement with the predictions of this theory.

Finally, it should be pointed out that the purpose of the original comment was *not* to contest the fact that Henrich first observed nonextremal Fermi-surface cross sections *by means of* quantum oscillations, but rather to point out his erroneous statement concerning the lack of previous unambiguous nonextremal measurements. We have no desire to fault his excellent data.

[1]Victor E. Henrich, Phys. Rev. Letters 26, 891 (1971).

[2]John W. Dooley and Norman Tepley, Phys. Rev. 187, 781 (1969).

[3]R. N. Bhargava, Phys. Rev. 156, 785 (1967).

[4]V. E. Henrich, following paper, Phys. Rev. B 6, 3151(1972).

[5]V. L. Gurevich, V. G. Skobov, and Y. A. Firsov, Zh. Eksperim. i Teor. Fiz. 40, 786 (1961) [Sov. Phys. JETP 13, 552 (1961)].

[6]H. N. Spector, Phys. Rev. 120, 1261 (1960); 125, 1192 (1962).

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Comment on "Observation of Nonextremal Fermi-Surface Orbits in Bulk Bismuth"–Author's Reply*

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This paper points out the uncertainties inherent in comparing Dooley and Tepley's ultrasonic Fermi-surface measurements with separate de Haas–van Alphen data and in trying to interpret the differences as due to nonextremal Fermi-surface orbits. Calculations based on the theory of Gurevich *et al.* are not consistent with Dooley and Tepley's interpretation.

In the preceding paper,[1] Tepley calls attention to an erroneous statement in a footnote to a previous paper by Henrich.[2] He correctly points out that, when the experimental quantum oscillation periods measured ultrasonically by Dooley and Tepley[3] are compared directly to the de Haas–van Alphen periods of Bhargava,[4] the electron and hole effective masses do not enter explicitly; they are derived at a later stage in the de Haas–van Alphen analysis.[4] This does not, however, prove that the differences between the two data are due to nonextremal Fermi-surface areas. We do not believe that the departures observed[3] are sufficiently outside of the experimental uncertainty inherent in comparing the two data to warrant identification as nonextremal areas. If they are, however, and if the electron relaxation time they quote is correct, then the theory of giant quantum oscillations[5] requires revision.

The largest difference observed by Dooley and Tepley[3] between the Fermi-surface areas measured using the ultrasonic tilt effect and the extremal ones from the de Haas–van Alphen data of Bhargava[4] is about 7% (see Fig. 2 of Ref. 3). Unfortunately, the orientation used there—sound wave vector $\vec{q}$ 8° from the binary axis in the binary-trigonal plane, and magnetic field $\vec{H}$ in the binary-trigonal plane—is very sensitive to misalignment of either $\vec{q}$ or $\vec{H}$. When $\vec{H}$ is rotated in the binary-trigonal plane near the normal to $\vec{q}$, the extremal Fermi-surface area for the ellipsoid they consider changes by 5.5% per degree of misorientation. If $\vec{H}$ is rotated toward the bisectrix axis, the rate of change is 3.2% per degree. While the relative orientation of $\vec{q}$ and $\vec{H}$ can be determined to within a few tenths of a degree by means of the tilt effect, the orientation of $\vec{q}$ or $\vec{H}$ relative to the crystal axes—very important if two different experiments are to be compared—is usually somewhat less accurate (of the order of 1°). It should also be noted that any rotation of $\vec{H}$ in the plane normal to $\vec{q}$ (toward the bisectrix axis in this configuration) is difficult to detect. It cannot be seen by the tilt effect and can only be determined if enough runs are made with the crystal orientation changed by known amounts to see the Fermi-surface symmetry in that direction. Dooley and Tepley report no such measurements,[3,6] and they only claim to be within 2° of the binary-trigonal plane in Ref. 6. Thus, their areas could be more than 6% different than Bhargava's near $\nu = 0°$. To these sources of (systematic) error must be added the uncertainties in the de Haas–van Alphen periods quoted by Bhargava. They range from 0.6

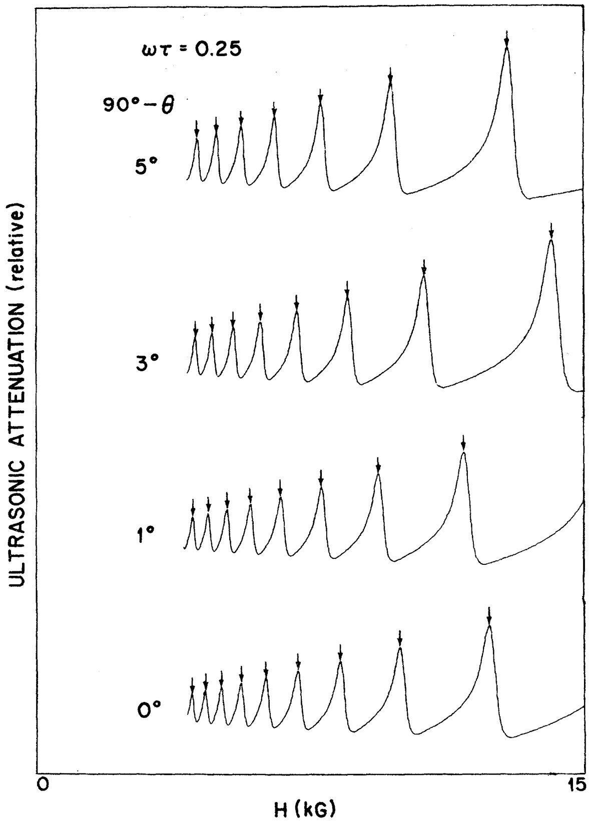

FIG. 1. Predicted ultrasonic attenuation vs magnetic field for the experimental configuration of Ref. 3, using $\omega\tau = 0.25$. θ is the angle between $\vec{q}$ and $\vec{H}$. Arrows indicate location of peaks for *extremal* areas.

to 5.6% (see Table II of Ref. 4). An indication that the above considerations can give rise to substantial differences between the two experiments can be seen in Fig. 3 of Ref. 3, where the extremal Fermi-surface areas measured from ultrasonic quantum oscillations are as much as 6% different from the de Haas–van Alphen results (excluding the region near the trigonal axis).

Thus, we do not believe that the measurements of Dooley and Tepley constitute proof of the existence of nonextremal Fermi-surface orbits. Two things should be done to make the results more definitive. An orientation should be chosen in which small misalignments will not give rise to large changes in Fermi-surface area. Also, the nonextremal areas should be compared with extremal areas measured on the same sample, as in Ref. 2, rather than comparing with a different experiment on different material.

If nonextremal areas were in fact seen by Dooley and Tepley, it is a significant result for the theory of giant quantum oscillations. In Ref. 2, there was no independent measurement of the electron relaxation time τ. It was inferred for the orientation used from the line shape of the giant quantum oscillations and the simultaneous existence of both extremal and nonextremal areas. This yields a value of $\omega\tau$ near 7, where ω is the acoustic angular frequency. Dooley and Tepley, on the other hand, have measured $\omega\tau$ by several independent methods[6] and quote $\omega\tau = 0.25$ for the conditions of Ref. 3. Using the theory of Gurevich *et al.*[5] in a calculation similar to that in Ref. 2, we have simulated the conditions of Dooley and Tepley's experiment, taking $\omega\tau = 0.25$, and we find that this value is too low to allow observation of nonextremal orbits. Our computer programs are not written to plot the exact tilt effect curves they measured, where $\vec{H}$ is held fixed and the angle between $\vec{q}$ and $\vec{H}$ is varied, but we have computed attenuation curves for several fixed angles while varying $\vec{H}$. The periods deduced in these two cases are the same, as are the conditions for observability of nonextremal areas. The attenuation curves are shown in Fig. 1 for four values of $(90° - \theta)$, where θ is the angle between $\vec{q}$ and $\vec{H}$. In the terminology of Ref. 3, $(90° - \theta) = \nu$. The arrows above each curve indicate the location of the attenuation peaks for *extremal* Fermi-surface orbits at these angles. [These can be obtained either by letting $\omega\tau \to 0$ or by changing the direction of $\vec{q}$ so that it is nearly parallel to $\vec{H}$. We have computed both cases, and the periods are the same within computational error (about 0.5%).] The maximum difference between the extremal areas and those for $\omega\tau = 0.25$ is 0.8%. In order to clearly see nonextremal orbits, $\omega\tau$ would have to be about an order of magnitude larger than Dooley and Tepley's value, and then two periods would probably be observed, one due to extremal and one to nonextremal orbits, as in Ref. 2.

There thus exists an inconsistency between the observation of nonextremal areas with that small an $\omega\tau$ and the theory of Gurevich *et al.* Some problems with the theory were already cited in Ref. 2, where the disappearance of all oscillations when $(90° - \theta) = 0°$ could not be predicted. If nonextremal orbits in bismuth have been observed when $\omega\tau = 0.25$, then the theory needs serious revision. We hope that Dooley and Tepley will publish additional data on their Fermi-surface measurements as well as any more information they may have on electron relaxation times. It could prove valuable in completing the picture of giant quantum oscillations.

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[3]J. W. Dooley and N. Tepley, Phys. Rev. 187, 781 (1969).
[4]R. N. Bhargava, Phys. Rev. 156, 785 (1967).
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[6]J. W. Dooley and N. Tepley, Phys. Rev. 181, 1001 (1969).

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Comment on "Acoustic Measurements of Electron Relaxation Times"

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Recent papers by Phua and Peverley and by Dooley and Tepley have discussed acoustic means for measuring electron relaxation times. This comment points out that the copper data in the former paper clearly display the effect described and predicted in the latter. The method of Dooley and Tepley is not sensitive to the geometric-decay term considered by Phua and Peverley.

For a free-electron model in which $\omega_c\tau < 1$ (ω_c is the cyclotron frequency and τ is the mean time between collisions for electrons), the amplitude of acoustic-geometric-resonance oscillations obeys[1,2]

$$A_n \sim e^{-\pi/\omega_c\tau}/n^{1/2},$$

where n refers to the nth oscillation. This form arises from an approximation to the Cohen–Harrison–Harrison (CHH) formulation[3] of magnetoacoustic attenuation.

In a recent paper, we pointed out by inspection of the exact CHH expressions that, whatever the detailed nature of the oscillations, the decay of the oscillation amplitude in the regime $\omega_c\tau < 1$ should be different from that in the regime $\omega_c\tau > 1$.[4] We examined data from Cu, Bi, and from a theoretical calculation by Beattie.[5] These showed distinct deviations from exponential behavior in all cases, with good agreement between theory and our ex-

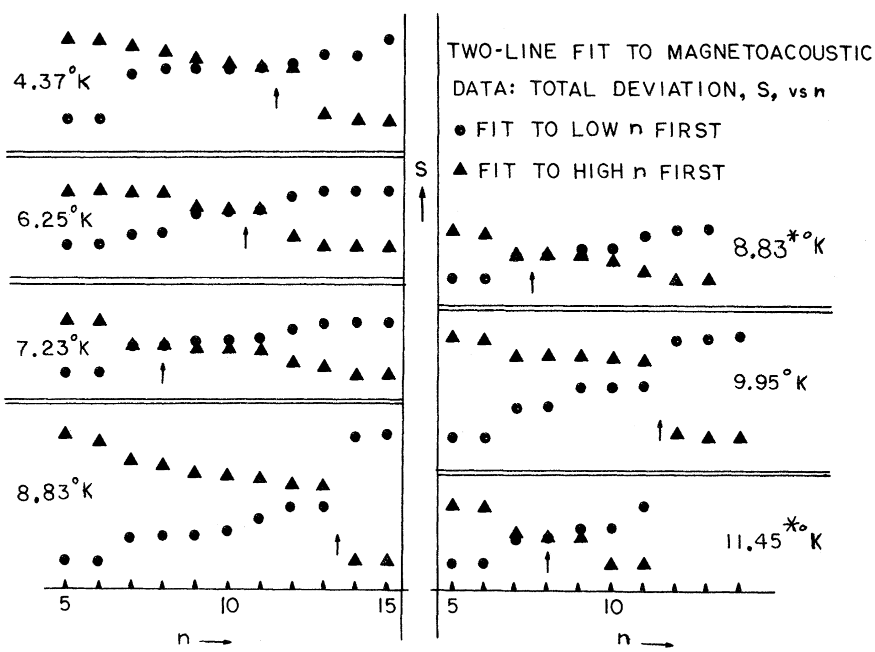

FIG. 1. Total deviation of a (single) straight-line fit as successive points are included in the fit. For circles, n indicates the highest index point used in the fit. For triangles, n indicates the lowest point used in the fit.